Toward commensurable psychological research on social distance in everyday moral decision-making : A threefold psychosocial and dynamic investigation

Aurore Gaboriaud

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THÈSE

Pour obtenir le grade de

DOCTEUR DE L'UNIVERSITÉ GRENOBLE ALPES

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Spécialité : PCN - Sciences cognitives, psychologie et neurocognition
Unité de recherche : Laboratoire interuniversitaire de Psychologie/Personnalité, Cognition, Changement social

Vers une recherche commensurable en psychologie sur la distance sociale dans les décisions morales du quotidien : Une analyse dynamique et psychosociale en trois niveaux

Toward commensurable psychological research on social distance in everyday moral decision-making: A threefold psychosocial and dynamic investigation

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Résumé

L'objectif de cette thèse est d'étudier l'impact de la distance sociale (c'est-à-dire la relation plus ou moins intime qu'entretiennent les individus entre eux) sur la prise de décision dans des contextes moraux du quotidien. Pour ce faire, nous avons tout d'abord dressé un inventaire des différents termes et opérationnalisations utilisés dans la littérature, ainsi qu'une définition synthétique de la distance sociale en tant que continuum. Nous avons ensuite examiné, selon un triple cadre analytique—aux niveaux intra-individuel, interindividuel et intergroupe—les biais inhérents à la distance sociale (e.g., auto-favoritisme) dans le contexte de la cognition sociale et morale. Afin de cartographier l'étendue de la recherche existante de manière plus systématique, nous avons également entrepris une revue de la portée sur cette question (appliquée uniquement aux situations morales). Les résultats préliminaires de ce travail ainsi que la revue de la littérature précédente ont permis de mettre en évidence deux tendances dans les résultats—l'une dans le sens d'un favoritisme de soi ou de ses proches, l'autre dans le sens d'un favoritisme envers des personnes distantes—et d'identifier des modérateurs potentiels de ces relations. Tout au long de cette thèse, nous montrons que la distance sociale exerce des influences complexes et parfois ambiguës sur les prises de décision morale du quotidien. Ainsi, nous défendons l'idée qu'elle nécessite d'être étudiée au sein d'un cadre intégratif à plusieurs niveaux d'analyse et par le biais d'une approche dynamique. Pour étayer cette affirmation, nous avons d'abord analysé aux trois niveaux d'analyse décrits l'influence de la distance sociale lorsqu'elle est manipulée en direction de l'agent de la situation morale en jeu (Exp.1 à 4). Nous avons ensuite abordé cette même question lorsque la distance sociale est manipulée en faveur de la victime de la situation (Exp.3 à 6), avant d'analyser l'impact de certains modérateurs potentiellement importants de la distance sociale (Exp.1-2-3 & 5-6). Enfin, nous avons étudié comment les techniques d'investigation dynamiques (en particulier le mouse-tracking) peuvent être utiles pour étudier une telle question de recherche (Exp.1 & 7-8). Dans l'ensemble, ces résultats nous invitent à considérer les effets de la distance sociale sous un angle intégratif et à contribuer ainsi à une science psychologique cumulative et commensurable.

Mots-clés : distance sociale, moralité du quotidien, prise de décision, revue de la portée, mouse-tracking, science psychologique commensurable

Abstract

The aim of this dissertation is to study the impact of social distance (i.e., the more or less intimate relationship between individuals) on decision making in everyday moral contexts. To this end, we first provided an integrative inventory of the different terms and operationalizations used in literature as well as a synthetic definition of social distance as a continuum. We then reviewed under a threefold analytical framework—at intraindividual, interindividual, and intergroup levels—the biases inherent to social distance (e.g., self-favoritism) in the context of social and moral cognition. In order to map the scope of existing research in a more systematic way, we also undertook a scoping review on this question (applied solely to moral situations). Preliminary results of this work, along with the state-of-the-art review, allowed to highlight two tendencies of findings—one in direction of self or close ones' favoritism; the other in direction of distant others' favoritism—and to identify potential moderators of these relations. Throughout this dissertation, we argue that social distance exerts complex and often ambiguous influences on everyday moral decision-making. Therefore, we claim that this question should be studied within an integrative multilevel framework and through a dynamic approach. In support of this claim, we first analyzed at all three levels of analysis the influence of social distance when manipulated toward the agent of the moral situation at stake (Exp.1 to 4). We then tackled this same question when social distance is manipulated toward the victim of the situation (Exp.3 to 6), before analyzing the impact of some potentially important moderators of social distance (Exp.1-2-3 & 5-6). Finally, we investigated how dynamic methods (in particular mouse-tracking) could be useful to study such a research question (Exp.1 & 7-8). Taken together, these findings enjoin to consider social distance effects under a unified lens and contribute this way to a cumulative and commensurable psychological science.

Keywords: social distance, everyday morality, decision making, scoping review, mouse-tracking, commensurable psychological science

Forewords

This dissertation work dealing with the impact of social distance on moral decision-making was initiated as part of a master research project, then pursued as a PhD project. This research results from the collaboration between Aurore Gaboriaud, Jean-Charles Quinton, and Annique Smeding. It was mainly funded by a doctoral grant from the Ministry of Higher Education and Research, obtained in 2021 at the Health, Cognition, and Environment Engineering Doctoral School (ED-ISCE in French) of University Grenoble Alpes (UGA). This work was carried out at the LIP/PC2S and LJK laboratories of UGA, with their support.

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In line with open science practices, this doctoral work has been conducted with a commitment to transparency and reproducibility, including the preregistration of hypotheses and online sharing of materials and data. Moreover, all procedures have been approved prior to experimentation from ethical committee. The present manuscript was written following APA 7th edition norms. Also note that ChatGPT has been occasionally used during the writing of this manuscript to assist in clarifying sentence structure and/or to enhance readability, as well as for code generation, while ensuring that all content and analyses remained under the author's full supervision and responsibility.

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Toward commensurable psychological research on social distance in everyday moral decision-making: A threefold psychosocial and dynamic investigation

Préambule

Imaginez un instant, vous retrouvez l'un de vos meilleurs amis autour d'un café. Il vous raconte entre deux conversations la frayeur qu'il a eu le matin même, alors qu'il prenait la route pour venir assister à l'un de ses cours de biochimie. Se remémorant les détails de l'accident et empreint d'une émotion qu'il tentait tant bien que mal d'inhiber, il vous raconte comment un cycliste lui est passé devant sans qu'il n'ait le temps de freiner suffisamment… Il faisait encore à peine jour, et ce dernier ne portait aucun éclairage ni dispositif auto-réfléchissant. Il vous avoue par la suite avoir percuté le cycliste, qui—bien qu'inconscient—« ne semblait pas être blessé tant que ça ». N'ayant vu aucune trace de sang, ni aucun témoin des faits, votre ami avait préféré partir pour éviter d'éventuelles représailles et problèmes d'assurance. Vous, choqué.e par ce propos, et connaissant visiblement mieux la loi que lui, lui faites remarquer qu'il s'agit là d'un délit de fuite, et qu'il peut encourir jusqu'à 7 ans de prison et 100 000 euros d'amende pour cet acte. Vous lui conseillez alors de très vite retourner sur les lieux du délit et/ou de se présenter aux autorités pour indiquer sa responsabilité dans cette histoire. Pourtant, craignant les conséquences de sa première décision de fuite, votre ami préfère ne pas s'y rendre et vous demande de jurer de garder cela pour vous.

Que faites-vous ? Respectez-vous la décision de votre ami—qui sera déjà probablement rongé par la culpabilité—en le protégeant, au risque d'être vous aussi rongé.e par cette même culpabilité, ou bien préférez-vous le dénoncer, remettant ainsi en question vos 10 ans d'amitié fidèle par cette trahison ? Et quand serait-il s'il ne s'agissait pas de votre ami mais plutôt de votre frère, de votre père, ou bien de votre propre fils qui avait/aurait commis ce délit de fuite ? L'aspect accidentel de cet événement et le peu de temps pour réagir pourrait les excuser au moins partiellement, si tant est que l'auteur du délit de fuite décide finalement après réflexion de se dénoncer lui-même. Toutefois, l'incertitude quant à l'état de la victime (est-elle en vie, sévèrement blessée, ou bien souffrant uniquement de blessures superficielles ?) vient également ajouter un élément de raisonnement : les peines encourues sont effectivement différentes du point de vue de la loi, selon que l'acte en

question est un homicide involontaire ou bien une atteinte involontaire à l'intégrité physique de la personne (avec incapacité de travail sur plus ou moins de 3 mois).

Finalement, vous vous dites que, quand bien même vous jugez la décision de votre proche comme *moralement inacceptable*, ce n'est pas à vous d'aller le dénoncer mais bien à lui de le faire. Ainsi, vous gardez cette histoire pour vous, même lorsqu'un appel à témoin est diffusé sur la chaîne télévisée locale. Vous y apprenez alors que la victime de l'accident a finalement succombé à ses blessures. En réalité, cette personne souffrait déjà d'un problème au cœur, et—aggravé par le traumatisme lié au choc de l'accident—est décédée des suites d'une hémorragie interne. Tourmenté.e par ce que vous savez de la réalité de cet accident et connaissant la fin tragique de la victime de cette histoire, vous restez tenaillé.e entre le respect de la promesse faite à votre ami et votre propre conscience morale qui vous pousse à révéler la vérité. D'autant que le reportage télévisé montre l'interview des membres de la famille de la victime, bouleversés par l'événement. A leur place aussi, vous ne pourriez comprendre l'arrogance et le manque d'empathie de l'auteur de l'accident, qui s'est enfui sans même se retourner sur sa victime. Peut-on alors conclure que, comme le vieux dicton le prétend : 'Qui ne dit mot, consent' ? Rien n'est moins sûr dès lors qu'on l'on tient compte des puissants facteurs situationnels : Les situations impliquant l'humain sont souvent complexes et l'absence d'un désaccord explicite ne suppose pas nécessairement un accord.

Cet exemple illustre parfaitement le sujet de cette thèse : Ce qui créé le dilemme moral ici est surtout le fait de bien connaître la personne ayant commis le délit, et de devoir la trahir—ce qui représente aussi jusqu'à un certain point une transgression morale—pour respecter ses propres valeurs morales d'origine. On peut effectivement penser qu'au contraire, en tant que témoin objectif de la scène (c'est-à-dire ne connaissant ni l'auteur, ni la victime de cet accident), nous aurions plus facilement tous et toutes tendance à aller au secours de la victime, et reporter ce que l'on a vu de l'accident auprès de la police, obtenant par là un sentiment de satisfaction et de gratification morale du fait même d'être dans la position du 'sauveur'. Dans les deux cas pour autant, nous ne sommes pas directement impliqués dans la situation, et simplement considéré comme une tierce personne. Ce qui

change donc, et a toute son importance dans la façon dont nous allons agir en réponse à cette situation (dénoncer ou non l'auteur), est notre proximité plus ou moins marquée avec l'auteur du délit et/ou avec la victime de l'accident.

S'il va de soi que dans certaines situations, nous avons plus tendance à défendre nos proches qu'à les dénoncer—tout comme nous avons tendance à nous trouver de bonnes excuses pour de petites maladresses ou erreurs (par exemple, se dire que c'est de la faute du bus si l'on est en retard), l'inverse est également parfois vrai dans d'autres situations. Nous pouvons avoir tendance par exemple à punir ou réprimander nos propres enfants plus sévèrement que ceux des autres pour une même bêtise. L'objectif du présent travail de thèse est donc d'identifier les différents facteurs en jeu dans ce genre de situations morales qui viennent interagir avec (voire modifier) l'influence de la *distance sociale*, c'est-à-dire la relation plus ou moins proche et intime que l'on entretient avec les protagonistes impliqués.

Dans la première partie théorique de ce travail de thèse, nous allons tout d'abord retracer les usages terminologiques et définitionnels de ce concept de distance sociale depuis ses prémisses au début du XXème siècle. Au vu de l'éclatement de ses divers usages, nous proposerons un inventaire intégratif des différents termes utilisés dans la littérature scientifique en psychologie, puis proposerons une définition unificatrice de la distance sociale vue comme un continuum (**Chapitre 1**). Nous passerons ensuite en revue les biais inhérents à l'influence de la distance sociale en cognition sociale et morale (e.g., favoritisme envers soi-même vs. autrui). Pour cela, nous nous appuierons sur un cadre analytique inspiré de la classification de Doise en psychologie sociale (1982) et composé de trois niveaux : le niveau intra-individuel (soi vs. autrui), le niveau inter-individuel (un autre proche vs. distant), et le niveau inter-groupe (un membre endogroupe vs. exogroupe) (**Chapitre 2**). Enfin, pour clore cette partie théorique et afin de cartographier l'étendue de la recherche scientifique sur cette question de manière plus systématique, nous avons entrepris une revue de la portée (scoping review), appliquée tout particulièrement aux situations morales. Étant donné le vaste travail que cela représente, nous reporterons ici seulement une partie des résultats de cette vue d'ensemble (**Chapitre 3**).

Ce travail de revue de la littérature a permis de confirmer l'existence dans la littérature de deux tendances concernant l'influence de la distance sociale telles qu'introduites ci-dessus. L'une semble suggérer un plus grand favoritisme pour soi ou nos proches ; alors que l'autre semble indiquer parfois un certain défavoritisme pour soi ou nos proches, notamment en cas de transgressions morales. Le présent travail de thèse vise ainsi à montrer que l'influence de la distance sociale sur nos jugements et prises de décisions morales du quotidien est plus ambiguë et complexe que ce qu'elle pourrait paraître au premier abord. L'objectif est également d'identifier et de tester les potentiels facteurs situationnels interagissant avec la distance sociale (et pouvant peut-être réconcilier ces deux tendances contradictoires). En cela, nous insisterons sur la nécessité et pertinence d'étudier cet objet de recherche selon un cadre intégratif comportant différents niveaux d'analyse et via une approche de la cognition située et dynamique.

Comme nous le verrons plus en détails plus loin (**Chapitre 4**), la partie expérimentale de ce travail de thèse s'organise d'abord selon la cible envers laquelle est manipulée la distance sociale. Nous verrons tout d'abord quels sont ses effets lorsqu'on fait varier la distance sociale envers l'agent de la situation (i.e., celui qui commet la transgression morale), et ce au travers des trois niveaux analytiques précédemment décrits (**Chapitre 5**). Puis, nous nous intéresserons à cette même question mais lorsque la distance sociale varie envers la victime de ladite transgression (**Chapitre 6**), avant d'étudier les différents potentiels modérateurs de ces relations (**Chapitre 7**). Enfin, nous proposerons une investigation au travers d'un outil dynamique (i.e., le suivi des trajectoires de souris ou *mouse-tracking*) pour étudier plus finement les processus sous-tendant les prises de décisions morales, et nous l'appliquerons à l'analyse des effets de la distance sociale dans ce contexte (**Chapitre 8**).

Le dernier chapitre du présent manuscrit (**Chapitre 9**) sera consacré à la discussion générale de cette thèse, portant en particulier tout d'abord sur les limites et compléments des approches théoriques utilisées comme fondement de ce travail. Il s'agira également de pointer les limites de nos études expérimentales et discuter nos résultats plus généralement

au regard des précédents travaux. Enfin, nous considérerons les perspectives et pistes d'améliorations pour les futures recherches sur cette question et réinsisterons sur la nécessité de converger vers une approche intégrative pour étudier cette question de recherche, en vue de contribuer à une science psychologique cumulative et commensurable.

Preamble

Imagine meeting one of your best friends for a coffee. Between two conversations, he tells you about the fright he had that very morning, while driving to attend one of his biochemistry classes. Remembering the details of the accident, he tells you how a cyclist passed him without giving him time to brake sufficiently... It was still barely daylight, and the cyclist was not wearing any lights or reflective devices. He then confessed that he had hit the cyclist, who—although unconscious—'didn't seem to be that badly hurt'. Having seen no traces of blood, nor any witnesses to the incident, your friend preferred to leave to avoid possible reprisals and insurance problems. You, shocked by his story, and obviously knowing the law better than he does, point out that this is a hit-and-run offence, and that he could face up to 7 years in prison and a 100,000 euro fine for this act. You thus advise him to return to the crime scene as soon as possible and/or to report to the authorities to state his responsibility in the matter. However, fearing the consequences of his first decision to flee, your friend prefers not to go and asks you to swear to keep this to yourself.

What do you do? Do you respect the decision of your friend—who will probably already be wracked with guilt—by protecting him, at the risk of being wracked with guilt yourself, or do you prefer to denounce him, thus jeopardizing your 10 years of faithful friendship with this betrayal? And what if it wasn't your friend, but rather your brother, your father, or even your own son who had committed the hit-and-run? The accidental nature of the event and the short time available to react could at least partially excuse them, if the hit-and-run driver finally decided, on reflection, to denounce himself. However, the uncertainty as to the victim's condition (is she alive, severely injured, or suffering only superficial wounds?) also adds an element of reasoning: The penalties incurred are indeed different from a legal viewpoint depending on whether the act in question an involuntary homicide or involuntary bodily harm is (with a work incapacity of more or less than 3 months).

In the end, you tell yourself that, even if you find your close one's decision morally unacceptable, it's not up to you to report him, but up to him to do so. So you keep the story to yourself, even when a call for witnesses is broadcast on the local TV station. There you learn

that the accident victim has finally succumbed to his injuries. In fact, this person was already suffering from a heart condition, and—aggravated by the trauma of the accident—died of internal bleeding. Tormented by what you know about the reality of this accident, and knowing the tragic end of the victim in this story, you remain torn between keeping your promise to your friend and your own moral conscience, which urges you to reveal the truth. Especially as the TV report shows interviews with the victim's family members, who are distraught by the event. If you were in their shoes, you'd be unable to understand the arrogance and lack of empathy of the perpetrator, who fled the scene without even looking back at his victim. Can we then conclude that, as the old saying goes: 'Silence is consent'? Nothing is less certain, given the powerful situational factors involved: Situations involving humans are often complex, and the absence of explicit disagreement does not necessarily imply agreement.

This example perfectly illustrates the subject of this thesis: what creates the moral dilemma here is, above all, the fact of knowing the person who committed the crime well, and having to betray him or her—which also represents a moral transgression to a certain extent—in order to respect one's own original moral values. On the other hand, as an objective witness to the scene (i.e., knowing neither the perpetrator nor the victim of the accident), we might all be more inclined to come to the victim's aid and to report what we saw of the accident to the police, thereby gaining a sense of satisfaction and moral gratification from the very fact of being in the position of the 'savior'. In both cases, however, we are not directly involved in the situation, but simply considered as a third party. What changes, then, and has a major impact on the way we act in response to the situation (i.e., whether or not to report the perpetrator), is our greater or lesser proximity to the perpetrator and/or to the victim of the accident.

While it goes without saying that, in some situations, we are more inclined to defend our loved ones than to denounce them—just as we tend to find good excuses for clumsiness or minor mistakes (for example, telling ourselves that it's the bus's fault if we're late), the opposite is also sometimes true in other situations. We may tend, for example, to punish or

reprimand our own children more severely than those of others for the same mistake. The aim of this thesis is therefore to identify the various factors at play in such moral situations that interact with (or even modify) the influence of social distance, that is, the more or less close and intimate relationship we share with the protagonists involved.

In the first theoretical part of this dissertation, we will first trace the terminological and definitional uses of the concept of social distance since its inception at the beginning of the 20th century. In face of the fragmentation of its various uses, we will propose an integrative inventory of the different terms used in the scientific literature in psychology, then offer a unifying definition of social distance seen as a continuum (**Chapter 1**). We will then review the biases inherent to the influence of social distance in social and moral cognition (e.g., favoritism toward self vs. others). To do this, we will draw on an analytical framework inspired by Doise's classification in social psychology (1982) and composed of three levels: the intraindividual level (self vs. other), the interindividual level (close vs. distant other), and the intergroup level (ingroup vs. outgroup member) (**Chapter 2**). Finally, to close this theoretical section and to map the scope of scientific research on this issue in a more systematic way, we have undertaken a scoping review, applied specifically to moral situations. Given the vast amount of the work involved, we will report here only part of the results of this overview (**Chapter 3**).

This review of the literature has confirmed the existence of two tendencies regarding the influence of social distance as introduced above. One seems to suggest greater favoritism for oneself or one's close ones; while the other seems to indicate at times a certain *un-favoritism* for oneself or close others, particularly in the case of moral transgressions. The aim of this thesis is thus to show that the influence of social distance on our everyday moral judgments and decision-making is more ambiguous and complex than it might appear at first. The aim is also to identify and test potential situational factors interacting with social distance (and perhaps reconciling these two contradictory tendencies). To this end, we will insist on the necessity and relevance of studying this research object within an integrative framework involving different levels of analysis and through a situated and dynamic cognition approach.

As we will see in more detail later (**Chapter 4**), the experimental part of this thesis is organized according to the target toward which social distance is manipulated. First, we will look at the effects of social distance varying to the agent of the situation (i.e., the one committing the moral transgression), through the three analytical levels described above (**Chapter 5**). We then turn to the same question, but when social distance varies toward the victim of the said transgression (**Chapter 6**), before examining the various potential moderators of these relationships (**Chapter 7**). Finally, we propose the use of a dynamic tool (i.e., mouse-tracking) to study in a finer way the processes underlying moral decision-making and we apply it to the analysis of the effects of social distance in this context (**Chapter 8**).

The final chapter of this manuscript (**Chapter 9**) will be devoted to the general discussion of this thesis, focusing first and foremost on the limitations and additions of the theoretical approaches used as a basis for this work. It will also point out the limitations of our experimental studies and discuss our results more generally in the light of previous work. Finally, we will consider prospects and avenues of improvement for future research on this issue, and will re-emphasize the need to converge on an integrative approach to investigate this research question to further contribute to a cumulative and commensurable psychological science.

Part I.

THEORETICAL BACKGROUND

Chapter 1[1]

Social Distance in Social Psychology: from Plural Views to an Integrative Definition

'Most of culture lies hidden and is outside voluntary control, making up the warp and weft of human existence. Even when small fragments of culture are elevated to awareness, they are difficult to change, not only because they are so personally experienced but because people cannot act or interact at all in any meaningful way except through the medium of culture.'

Edward T. Hall, The Hidden Dimension (1966/1969, p.188)

How is Social Distance Defined and Operationalized in the Psychological Literature?

Origins of Social Distance

In social psychology, the use of the term social distance can be traced back to the early 20th century. It has first been defined by Robert E. Park as 'the grades and degrees of understanding and intimacy which characterize personal and social relations generally' (p.339). Even though it is not entirely clear which factors are at play—e.g., understanding and/or intimacy—this definition can be summed up as the degree of intimacy from one individual to another (1924). Bogardus then reused this terminology to create his *social distance scale* (1933), measuring the desire for people to get more or less relational closeness (or sympathy) with another one, and especially with other groups. Later, Lewin (1936) built upon this to analyze differences in relationships between individuals from the United States vs. from Germany. Here, social distance reflects inter-cultural differences between regions. Lewin then concludes that US people have less *social distance* in the sense that they are more willing to have contact and relational proximity with strangers (i.e., they are more accessible) than Germans (Lewin, 1936).

[1] This chapter has been currently submitted to Social and Personality Psychology Compass peer-reviewed journal, as a theoretical contribution.

Subsequent Uses of Park's (1924) Definition

Park's definition of social distance has subsequently been used all along the 20th century in psychological research, especially for intergroup studies (e.g., Sherif, 1973; Triandis & Triandis, 1962) but also in other areas, such as economics (e.g., Akerlof, 1997), anthropology or sociology (e.g., Chamboredon & Lemaire, 1970). This research often takes the perspective of Bogardus' scale (1925a; 1933)—and thus Park's definition (1924)—with only rarely new conceptualizations or definitions. The definition of social distance has therefore barely evolved since the beginning of the 20th century. Consequently, it is still not clear which factors highlighted by Park (1924) may be involved or are most central in social distance. Current research on social distance therefore often suffers from some vagueness.

More Recent Definitions

A few new definitions or frameworks for social distance have however emerged between the end of the 20th century and the beginning of the 21st, with especially a resurgence of interest for this issue these last two decades. Hoffman et al. (1996) defined social distance as 'the degree of reciprocity that subjects believe exists within a social interaction' (p.654), far from the original definition of Park (1924). Trope et al. (2007) conceptualized social distance as one of the four dimensions of psychological distance, hence taking as a definition the fact of feeling remote from the here (spatial), now (temporal), self (social), and highly probable (hypothetical) events for the distant instance. Trope et al. (2007) did not provide a more specific definition of social distance per se, but they mentioned several operationalized forms of social distance. That is, either via *similarity* as more formally suggested afterwards by Liviatan et al. (2008) or via *power* as studied by Smith and Trope (2006) and a bit later in a more systematic way by Magee and Smith (2013). As for Eyal et al. (2008), they used perspective-taking (own vs. other perspective) as an operationalization of social distance. We will develop right below in more details what psychological distance theory is and what it involves for the social dimension specifically. More recently, Jones (2021) provided a definition of social distance as a 'psychophysical measure of how close or distant an individual feels toward someone else' (p.511).

Social Distance as a Dimension of Psychological Distance

Psychological Distance Theory

The concept of psychological distance was developed in the context of construal level theory (CLT), notably by Liberman and Trope (1998) with temporal distance, then generalized to other distance dimensions by Liberman and collaborators in 2007 (see also Trope et al., 2007). According to CLT, we judge an event or an object in a higher / more abstract (vs. lower / more concrete) level depending on how psychologically close (vs. far) we are from it (e.g., Bar-Anan et al., 2006; Trope et al., 2007). Psychological distance is then defined as the 'subjective experience that something is close or far away from the self, here, and now' (Trope & Liberman, 2010, p.440). Is psychologically distant what is 'removed from direct experience' according to Trope and collaborators (2007, p.84). Following this approach, there are four subdimensions of psychological distance: *spatial* distance (sometimes referred to as *geographical* distance), *temporal* distance, *hypothetical* distance, and *social* distance (also referred to as *personal* distance) (Trope et al., 2007). We will briefly develop the social distance dimension within this framework.

Social Distance Dimension

The social dimension of psychological distance reflects the fact that an individual or event is more or less close to oneself socially speaking (in terms of relational link). With operationalizations in terms of first vs. third person perspective (Eyal et al., 2008, Study 3), similarity (Liviatan et al., 2008) or social power (Smith & Trope, 2006), this definition gets somewhat closer to the original definition of social distance (Park, 1924). One main difference is that social distance can also be manipulated toward a non-human entity within this framework (e.g., a domestic vs. foreign object, see Amit et al., 2009). Some researchers have been referring to social distance for individuals' relationships with their pets (e.g., Topolski et al., 2013; Xu et al., 2022), with robotic agents (e.g., de Melo et al., 2016; Kim & Mutlu, 2014; Li & Sung, 2021), or even relationships between specific organizations (e.g., Wang et al., 2021). These pieces of research may be very interesting, especially when studying social distance within animal societies (e.g., Brosnan et al., 2015), but—as it is not

the first focus of the present dissertation—we will limit the current work to the analysis of social distance when measured or manipulated among (and between) humans.

Various conceptualizations and operationalizations have been used to translate this notion of social distance in the literature, as suggested by Didi Alaoui and Cova (2021) in their review of psychological distance in the field of marketing. They distinguished at least six forms of social distance operationalization (applied to marketing studies): in relation to the self (i.e., is the self or another person the target), similarity (toward the target of judgment or choice), the group (i.e., ingroup or outgroup member involved), culture (i.e., interdependent or independent nature of the self), politeness (i.e., addressing consumers in a certain way), or power (i.e., relative power given to consumers in decisions). In sum, despite a full century of research on social distance since its inception (Park, 1924), a diversity of meanings and operationalizations co-exist, making an integrative understanding and definition difficult to achieve.

Psychological Distance Dimensions' Correlation and Overlap

The four subdimensions of psychological distance are supposed to be strongly and positively correlated (Fiedler et al., 2012; Trope & Liberman, 2003, 2010). Fiedler et al. (2012) found more particularly strong positive correlations between all four dimensions of psychological distance, irrespective of the item valence (positive or negative) and of its location in time (in the future or in the past). It seems indeed easy to think that a person thinking about an event that is distant in time (i.e., that should occur in many years—natural ecosystems collapsing in 2050 for instance) will also tend to perceive this event as more distant in terms of space (i.e., anywhere else but here), as less likely, and as concerning only distant unknown people. This phenomenon can be illustrated with the recent movie *Don't look up* (McKay, 2021), where two scientists discover an approaching comet that is directly targeting the earth and will destroy for sure human civilization in a 6-month delay. Despite a 100% likelihood to occur, and even if directly concerned as people by this global issue, only few people take the threat seriously despite the scientists' efforts to warn the

population. They eventually realize their own finiteness only when they see with their own eyes the comet crossing the sky and shining.

Difficulty of Studying a Single Dimension

Because of these strong correlations between subdimensions of psychological distance, it is often difficult to examine only one of them separately (Fiedler et al., 2012; Liberman & Trope, 2014), resulting in possible confounding effects. For example, applied to social distance, someone who is close to us socially (e.g., a friend, a family member) is also often closer to us spatially (e.g., in terms of living area) or temporally (e.g., in terms of more or less frequent interactions). Similarly, someone who is distant from us socially speaking (e.g., an old acquaintance or a stranger) is also hypothetically more distant (i.e., in terms of probability to meet them, or even in terms of mental representation we have of them, with less certain characteristics).

When we manipulate social distance experimentally, it is therefore often difficult to perfectly control (i.e., not making varying) the other subdimensions at the same time to achieve an all-other-things-being-equal comparison, particularly regarding hypothetical distance. This also raises the question of whether the effect of psychological distance is not a global effect (with a single explanatory factor) rather than a factor with four subdimensions. In this vein, some argue to view psychological distance as a unitary concept (as all four subdimensions are found to be positively correlated) (Fiedler et al., 2012). In fine, although sometimes considered as interchangeable in some contexts, Trope and Liberman (2010) noticed the differences of the various subdimensions of psychological distance on various aspects (such as their degree of relative primacy, their dimensionality, controllability, or valence implications), hence reaffirming the necessity to distinguish them. An example of research where subdimensions are fundamentally imbricated, thus jeopardizing examination of the sheer effect of social distance, is research on intergenerational effects (see for instance Agneman et al., 2024; Hanks, 1991; Wade-Benzoni & Tost, 2009). Such research intrinsically confounds the social (i.e., one’s offspring) and temporal (i.e., in the future) dimensions of psychological distance.

Other Works Relying on the Original Definition of Social Distance

So far, we have focused on social distance definition at its inception (Park, 1924) and in the most recent framework of psychological distance theory (Trope et al., 2007). In between, social distance seems to have been tackled as a research question, as evidenced by search results (N = 160)[2] for 'social distance' occurrences in *PsycInfo* during the 1940-1999 period. This research however mainly focused on social distance as understood by Park and Bogardus, with no new definition or theoretical conceptualization. Specifications of Park's social distance definition (1924) can be found, with notably the studies of Triandis and Triandis (1960), aiming at identifying the determinants of social distance (i.e., what counts the most in social distance variations between race, nationality, social class, religion etc.). Schmitt (1972) also suggested a reformulation of social distance through plural variables such as identity, status, cultural / physical / personal distance, integrated in a model of information exchange equity. This research completes our understanding of Park's (1924) social distance definition, but without bringing in a new integrative framework. As a diversity of definitions and operationalizations still co-exist in literature, such an integrative endeavor is nevertheless much needed.

Interdependency and Interpersonal Relationships as Social Distance

When searching the literature in psychology with keywords other than social distance (e.g., interpersonal relationships), we found a large set of studies over the same period related to social distance. For instance, there are occurrences with *close relationships* (N = 145), and *closeness* (N = 124)[3], which only reflect a different way of considering social distance. That is in this case, how people interact with their (very) close relationships compared to more distant ones. This starts with Lewin (1940/1997) who considered married couples as the smallest entity of a group (i.e., two individuals only), and therefore the most interdependent type of group. In line with Park's conceptual framework, Lewin (1940/1997)

[2] This is the number of results obtained through EBSCOhost server and when searching only in the titles of articles published in peer-reviewed journals (date of search: January 8, 2025).
[3] Occurrences in EBSCOhost server, only in the title of articles published within the 1940-1999 period in peer-reviewed journals (date of search: January 8, 2025).

defines close relationships as having the smallest social distance, that is, as including the other in one's life, in terms of overlapping areas.

Building upon this notion of interdependency, Kelley et al. (1983) offered a formalized conceptual framework for the study of interpersonal relationships and especially close ones. They define a close relationship as 'one of strong, frequent, and diverse interdependence that lasts over a considerable period of time' (p.38), hence not referring to the notion of social distance per se. This is what will be the basis for Aron and Aron's self-expansion model (1986), as a theoretical framework for close relationships seen as including others in the self (see also Aron et al., 1991). This resulted in the creation of the *Inclusion of Others in the Self* (IOS) scale by Aron et al. (1992). Although still referring to the work of Lewin (1940/1997) on overlapping areas, they do not explicitly refer to social distance. They however linked close relationships and closeness to empathy and change of perspective (actor vs. observer), but also to the ingroup / outgroup distinction (Aron et al., 1991).

Another important line of research regarding social relationships and their influence in everyday life lies in kin selection theory, starting with Hamilton (1964). It was subsequently expanded through the study of genetic relatedness influence and kinship more generally in social cognition (Altman, 2011; Lieberman & Linke, 2007). This last theorization is sometimes explicitly linked to some other-similarity and self-other overlap, notably by Tan et al. (2015), but only anecdotally to social distance. Some authors, like Fiske with his relational model theory (1991) in the field of anthropology theorized the influence and the internal organization of interpersonal relationships. Fiske (1992) described four relational models, which he also calls 'elementary forms of sociality': communal sharing (i.e., including intimate and kin relationships), authority ranking (i.e., asymmetric relations such as parents-children or teacher-student), equality matching (i.e., including non-intimate acquaintances), and market pricing (i.e., relations embodied in a system of monetary transaction). This classification can serve as a guideline to organize and elaborate on social distance influence but is not in itself sufficient to unify the diverse literatures and definitions reviewed so far.

These various literatures, evolving in parallel, may have contributed to the difficulty of coherently integrating and understanding the effects of social distance and its moderators. Indeed, beyond the fact that social distance can take plural forms once operationalized—as already traceable in work based on Bogardus' work (1925a; 1933)—research often does not refer to the same term, nor to the same conceptual framework albeit dealing with social distance. Hence the difficulties to compare findings in these various literatures which are rarely confronted as seldomly linked together. For a few exceptions, see Didi Alaoui and Cova's review (2021) and Winterich's dissertation work (2008), although both are applied to marketing research and are not exhaustive. Liberman and Trope (2014) as well as Hofmann et al. (2018) also suggested various dimensions involved in social distance research, but without offering systematic integration.

An Inventory of the Various Terminologies and Literatures Referring to Social Distance

As developed so far, when extending beyond the social distance term per se, we identified several literatures examining the influence of relational distance or closeness between one and another, and applied to different domains. Nevertheless, given this scattered literature, it remains difficult to get an integrative overview of the diverse terminologies and operationalizations of social distance. To our knowledge, there is currently no taxonomy or inventory that references the various terminologies of social distance used in social cognition in an organized way. An exception is found with a review on psychological distance effects and operationalizations in the marketing literature (Didi Alaoui & Cova, 2021), but which did not focus specifically and uniquely on social distance, and is restricted in scope. Given this gap in social cognition despite the ubiquity of social distance-related work in this domain, we intended to provide such an inventory in the current work, with social cognition being the primary domain of interest. We will first define several categories developed to organize the various terms found in literature for social distance. Then we will review what terms have been found in the social cognition literature in each of these

categories and give examples when possible. Finally, we will provide a summary map to display in a visual way the inventory (see Figure 1.1).

To develop these categories, we relied on the various literatures focusing on social distance in one way or another. However, we use these categories hereafter as a guide to organize existing knowledge, not in a rigid way, allowing for new categories or subcategories to emerge if relevant in future work. We also do not claim to be exhaustive and to cover all domains, as our focus is on social cognition. Hence, we may not have embraced all synonyms and forms of social distance which might exist in the broader scientific literature, but specifically its main acceptations in social cognition. Our aim and hope is that this inventory will be useful for future research focusing on social distance by easing access to all the relevant studies, avoiding in this way to merely focus on one specific field or theoretical framework using one particular terminology, and ignoring those being closely related. This inventory therefore directly aligns with the broader objective of building an integrative and cumulative psychological science, to support reliability and efficiency (e.g., Almaatouq et al., 2022).

On a final note, we did not include in this inventory some misuses of social distance, in the sense that social distance referred to something radically different from the original definition of Park (1924) (e.g., social distance referring to spatial distance between one and another, social distancing as a measure to protect the population from the COVID-19 disease). Notwithstanding these exclusions, we embraced some flexibility in the terms reported below[4], as some are direct synonyms of social distance whereas others are extensions of it through specific operationalizations. For instance, genetic relation is not exactly the same concept as social distance (even if we can be more or less close genetically speaking), but we included it as a specific form of social distance as often closely linked together.

[4] Also note that as the developed categories aimed to organize the literature, some terms could in theory fit into more than one category, hence a few redundancies in the description of the various categories, which do not jeopardize the usefulness of the inventory itself.

In sum, we reviewed the literature to identify all relevant terms and/or operationalizations of social distance which fall in line with (i) Park's (1924) original definition—degree of intimacy from one individual to another—or (ii) with the psychological distance theory definition—being more or less removed from the self. To establish this inventory, we meticulously referenced the terms used in each relevant new article we identified while reviewing the different literatures, and then grouped these various terms into different families according to their meaning. In what follows, we subsequently describe for each category its specificity and give a few prototypical references of the literature that use the mentioned terms.

Generic Terms

We started with this supra-ordinate category which includes all synonymous terms for social distance, that is, terms that are interchangeable with social distance (same exact meaning in the literature). We find in this supra-category, beyond the *social distance* term itself, terms such as *social closeness* (e.g., Linke, 2012; Passarelli & Buchanan, 2020; Singer, Binapfl et al., 2021), *social proximity* (e.g., Evans, Ong, et al., 2021) and *social connectedness* (e.g., Capaldi & Zelenski, 2016; Duong & Pensini, 2023; Robson, 2021). Other terms included in this category are *similarity*[5] (e.g., Liviatan et al., 2008; Noval et al., 2018; Rotemberg, 2014; Zorn et al., 2022), *familiarity* (e.g., Charness et al., 2024; Lee & Setoh, 2022; Prentice, 1990; van de Groep et al., 2020), and *social assortment* (e.g., Cohen & Haun, 2013)—even if we found only few occurrences in the literature for this term.

Terms Referring to the Self

Although social distance is viewed, in psychological distance theory, as a matter of comparison from the *self* point of view (being close or distant from the self), some terms refer explicitly to the self in literature. The most prototypical example in the literature is Aron et al.'s concept of *inclusion of other in the self* (1991, 1992), which has often been reused

[5] Alternatively, similarity could also be considered as a supra-category for social distance, social distance being only one of the possible dimensions of similarity. However, for the purpose of the present inventory, including similarity within the generic terms category was the most appropriate option.

during the last decades (for some examples, see Biella et al., 2023; Cheng et al., 2010; Fitzgerald et al., 2010; Tan et al., 2015; Wang et al., 2021). But this terminology is also sometimes referred to as *self-other merging* (e.g., Batson et al., 1997; Paladino et al., 2010), *self-other overlap* (e.g., Gutsell & Inzlicht, 2010; Tan et al., 2015), or with the notions of *oneness feeling* created by Cialdini et al. in 1997 (see also Maner et al., 2002), and *confusion of self with close others* (e.g., Mashek et al., 2003).

In this category, we also include the terms of *personal involvement* and *personal* or *relational* (vs. *impersonal/non-relational*) situations found in some research as the degree of involvement of the individual in the situation and/or to refer to the presence of close people in the situation (e.g., Losecaat Vermeer et al., 2016; Skoe et al., 2002; Skoe & Gooden, 1993; Wark & Krebs, 1996). These terms may, in our opinion, not be the best suited to reflect social distance variations as they also refer, in the moral psychology literature, to the personal (vs. impersonal) types of dilemmas, that is, where the participant is more (vs. less) emotionally engaged in the situation, hence confounding different parameters (see Greene et al., 2001)[6]. We nevertheless kept them in the inventory to reflect their rare correct uses (i.e., when authors really want to refer to social distance as it is presently understood).

Another term which is close to the first meaning of personal involvement is the notion of *perspective*, in particular when modulating the perspective of the participant between a *first-person vs. third-person perspective* or between an *actor vs. observer perspective* (also sometimes referred to as *role-taking*) as mentioned by Aron et al. (1991). Perspective-taking has been used in some research to manipulate social distance within self vs. other comparisons (e.g., Hirschfeld-Kroen et al., 2021; Park et al., 2024); or within the various roles taken in a given situation (e.g., Jones & Nisbett, 1972; Levine, 1976; Li & Han, 2010; Nadelhoffer & Feltz, 2008).

[6] The distinction between personal and impersonal dilemmas confounds in Greene et al.'s paper (2001) three different criteria: (1) bodily harm as a result of the action; (2) perpetrated toward a specific person or specific group members; (3) action committed for its own sake without being the result of deflecting another threat.

Terms Referring to Genetic Filiation

Kin selection theory or the *green-beard effect*—the favoritism and altruism we tend to have toward our relatives in order to protect our genetic lineage, firstly studied in biology and non-human species as a direct legacy of Darwin's evolution theory (Darwin, 1859; Hamilton, 1964)—and more largely *kinship* or *genetic relatedness* is another important part of the literature on social distance influence. However, because it is not linked to the social distance term per se, it can be easily overlooked, despite its relevance. We restrained this category to manipulation of social distance in terms of *genetic filiation* of an individual (e.g., parents, family, siblings, etc.), thus keeping only the stricter sense of '*kin*' or '*familiar'* and not the larger ones (when a kin or a familiar person can also be a non-genetically related peer). To give some examples, see for instance research by Rachlin and Jones (2008) or Tan et al. (2015) for *relatives* or *kinship* terms; Korchmaros and Kenny (2001), Bleske-Rechek et al. (2010), and O'Gorman et al. (2005) for *genetic relatedness*; Brandner et al. (2021), Lee and Holyoak (2020), McManus et al. (2020) for *family* related terms; and Uhlmann et al. (2012) for *blood kinship* and *blood* related terms.

Terms Referring to Intimate Filiation or Affective Dimension

This category includes *very close relationships* from the individual's point of view but not genetically related ones. We also included in this category terms that relate to *affective* (or *emotional*) *proximity*, as we think this also characterizes well close relationships (i.e., we generally feel emotionally closer to those we love). We can find in this category relationships such as *romantic* or *intimate partnership* (also found as *intimate bonds*); *friendship*, or *kinship* when it is understood in a larger sense than genetic relatives only. We can also find in the literature the '*significant others*' designation as well as the use of the *familiarity* term when specifically referring to family but non genetically related members.

A few examples of research using these terms for social distance: Bleske-Rechek et al. (2010), Fitzgerald et al. (2010), and Forbes and Stellar (2022) for *romantic* and *intimate partnership* (or *bonds*). Fisktjønmo et al. (2021), O'Gorman et al. (2005), and Tan et al. (2015) for *friendship* or *kinship* occurrences. Andersen and Cole (1990) for *significant others*

term; Prentice (1990) for *familiarity*; and Selterman and Koleva (2015), Weidman et al. (2020), and Weiss and Burgmer (2021) for *close relationship*. The term *we-ness* is sometimes specifically used to mean the closeness of an affective or romantic relationship (e.g., Cruwys et al., 2023), as for the *identity fusion* concept (e.g., Joo & Park, 2017). Moreover, another term seems to be used sometimes in Eastern research referring to the close social network of an individual with the '*guanxi'* term (not necessarily bounded to personal context, as it can also be used for business or political relations, 2024), see for example Ho and Redfern (2010) and Hwang (1998).

Finally, illustrating the affective dimension of this category, we can find terms such as *emotional closeness* (e.g., Hackman et al., 2015; Korchmaros & Kenny, 2001; Moore et al., 2023; Pollet et al., 2013), *affective proximity* (e.g., Tassy, Oullier, et al., 2013)*, affective relatedness* (e.g., Hao et al., 2015), or even *affective perspective-taking* (e.g., Will et al., 2013). We could also link to this category terms related to the literature on *interpersonal liking* (e.g., Bocian et al., 2022; Street, 2016; Zorn et al., 2022) or *affinity* related terms (e.g., Marion & Burke, 2017). Some finally consider *empathic concern* or *empathy* as a manner to measure social distance between one and another (e.g., Batson et al., 1991; Noor & Nazneen, 2017; Scaffidi Abbate et al., 2022) even if we could further discuss this choice.[7]

Terms Referring to Group Filiation

Another important dimension of social distance is the group dimension, already evoked at the beginning of social distance conceptualization by Bogardus (1925b)*,* and a bit later by Lewin (1936, 1940/1997). The group dimension is also taken into account in more recent research by Aron et al. (1991) when considering close relationships as including other in the self. Terms used to refer to the intergroup dimension of social distance are however diverse as they can reflect any dimension gathering people as part of a group and sharing a common identity. We found the terms referring to *intergroup* dimension in general, *ingroup/outgroup member* or more rarely *insider/outsider* (see for instance Bonetto et al.,

[7] In particular, we may question whether empathic concern is not more an indirect consequence of the social proximity preexisting between one and another or a determinant of this social proximity.

2023; Brewer, 1999; McKee et al., 2023; Otten & Epstude, 2006). Other less frequently used terminologies include *parochial relationship* (e.g., Bernhard et al., 2006; De Dreu et al., 2014; Lazić et al., 2021), *partisanship* or *tribalism* (e.g., Bocian et al., 2021), as well as *entitativity* (understood as the level of group unification, see Khan & Howe, 2021) or again *familiarity* (e.g., Charness et al., 2024).

We also found in this category more specific dimensions referring to subgroup categories such as *race* or *ethnicity* (e.g., Hester & Gray, 2020; Triandis & Triandis, 1960); *gender* or *age* (e.g., Hester & Gray, 2020; Prétôt et al., 2024; Xiao et al., 2022); *social class* or *social category* (e.g., Triandis & Triandis, 1960; Yang et al., 2022) or even *shared values* (as with political values or religion, see for instance Isler et al., 2021; Saroglou et al., 2011; Shackleford, 2020; Triandis & Triandis, 1960). We added to this category the terms referring to groups formed on the basis of *social identity* (e.g., Xiao et al., 2024), *identity fusion* or *we-ness feeling* (i.e., defined as more or less overlap between self and the group of membership, see Swann et al., 2010).

Terms Referring to the Interdependence Dimension

The next category is not the easiest to determine or to infer from research as sometimes abstract or non-explicitly stated. We chose the term *interdependence* to encompass terms used for social distance that reflect the reciprocal or interdependent nature of the relationship between one and another individual. When close to each other, individuals are very interdependent as stated by Lewin (1936), whereas socially distant people will be very independent toward each other. We chose to put in this category terms related to *power relationships* (see for instance Magee & Smith, 2013; Smith & Trope, 2006) even if it remains controversial to include power as a form of social distance.

We also considered as interdependence the *social role* or *status* we may endorse in everyday life in various situations (see Haidt & Baron, 1996 and Hasty & Maner, 2020 for illustrations of both terms, respectively). We also found some occurrences of research (e.g.,

Evans, Kogler, et al., 2021; Yang et al., 2020) where *synchronicity*[8] between people was measured or manipulated as a form of social distance (with synchronous people being socially closer than non-synchronous people). We could also integrate to this category the previously detailed *guanxi* term, understood in a broader sense as one's *social network* (e.g., Chou et al., 2006; Takahashi et al., 2008), or terms related to the interdependence between people, as for *we-ness* (when used in the framework of romantic relationship, see Cruwys et al., 2023) or *inclusion of other in the self* (Aron et al., 1991, 1992).

Terms Referring to Identification

Going even further in the operationalized forms that can take social distance, we created this category of *identification* (see Bohnet & Frey, 1999) for research manipulating social distance at a very minimal level, for instance by manipulating the *anonymity* of the target of judgment (e.g., Jennings, 2019) or by enhancing the *identifiability* of the target (e.g., Kogut, 2011; Ritov & Kogut, 2017; Wiss et al., 2015).

Toward a Definition of Social Distance as a Continuum

The concept of social distance is definitely more complex than anticipated, due in particular to the multitude of operationalizations that can be assigned to it. And the fact that it is referred to by a wide range of terms (see Figure 1.1) does nothing to help getting an overarching view of the research investigating its influence on relevant outcomes. For instance, depending on whether you search for the term *intimate relations* or *social distance*, you will find relevant results in both literatures, even though they rarely cite each other. This lack of consistency and integration is even more problematic when the same authors use multiple terms across their research in an undifferentiated way and without referring to a broader and integrative framework of social distance.

Social Distance as a Neutral and Bi-Directional Term

In the present work, where the aim is precisely to provide an integrative inventory of these different literatures, we purposefully chose the term *social distance* rather than other

[8] We may wonder if synchronicity is rather a consequence of social distance than a form of social distance (for more about the effects of synchronicity, notably on self-other merging, see Paladino et al., 2010).

terms used in major articles in this field (e.g., *close relationships* as in Weidman et al.'s paper, 2020 or Selterman and Kolova's work, 2015). The first reason is the importance of having a non-oriented, neutral term (in terms of proximity or remoteness). Indeed, in mathematics, a *distance* can be short or long, but remains a non-oriented vectorial object. The idea is similar here: The social distance toward a specific other can be short or long, depending on the degree of social proximity that we have with that other. The competing terms *social proximity* or *social closeness* are biased in direction of closeness, as we cannot say 'a distant proximity' or a 'distant closeness' (or it would appear as an oxymoron). The term *close relationships* often found in the literature remains itself very broad and unspecific: it can refer to both the relationship maintained with the person and the person him/herself, and already excludes the visibility of the *distant* component in the comparison. We will therefore favor the term *social distance* throughout this work; it is also more theoretically and historically anchored, as seen previously with psychological distance theory and Park's (1924) original definition, even though it is not the most frequently used term in current research.

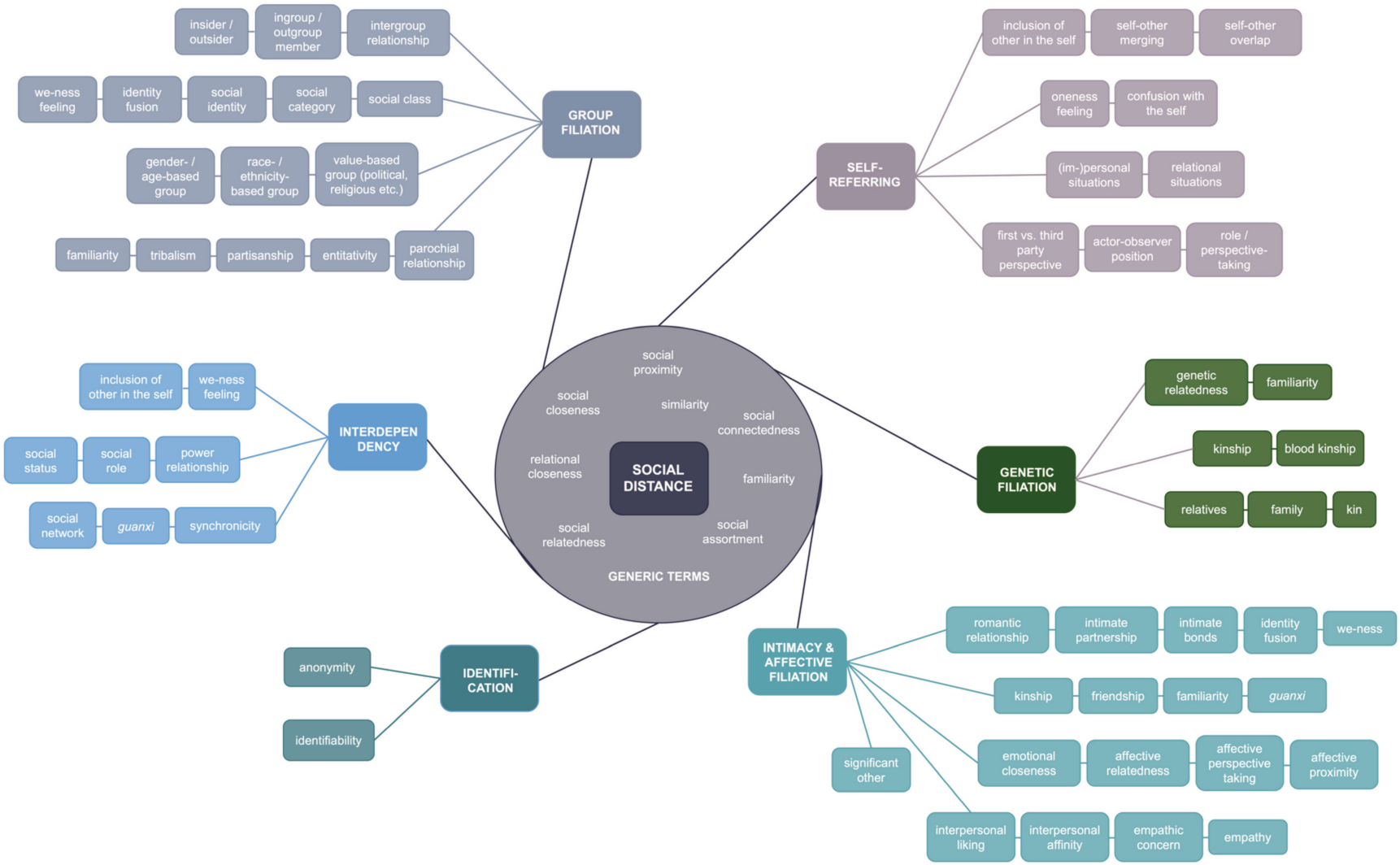
Social Distance as a Continuum Including Self as the Closest Extremity

Having previously reviewed the social distance literature and given the variety of terms and operationalizations, we presently conceive social distance as a continuous dimension and not just a binary (or dichotomous) one. The social distance concept, conceived as a continuum, allows to encompass all terminologies and operationalizations reviewed above. Hofmann et al. (2018) introduced this incentive to consider social distance as a continuum to study its effects:

> This suggests that earlier models of kin selection (e.g., Lieberman & Linke, 2007) or in-group favoritism (Bernhard et al., 2006) may be summarized more parsimoniously as typical instances of a more general, continuous social distance metric that may underlie people's tendency to favor close others—irrespective of the specific source of closeness (such as same vs. different group memberships, kin-based relationships, intimate relationships, similarity in attitudes and beliefs, and more). (p.1707)

Figure 1.1

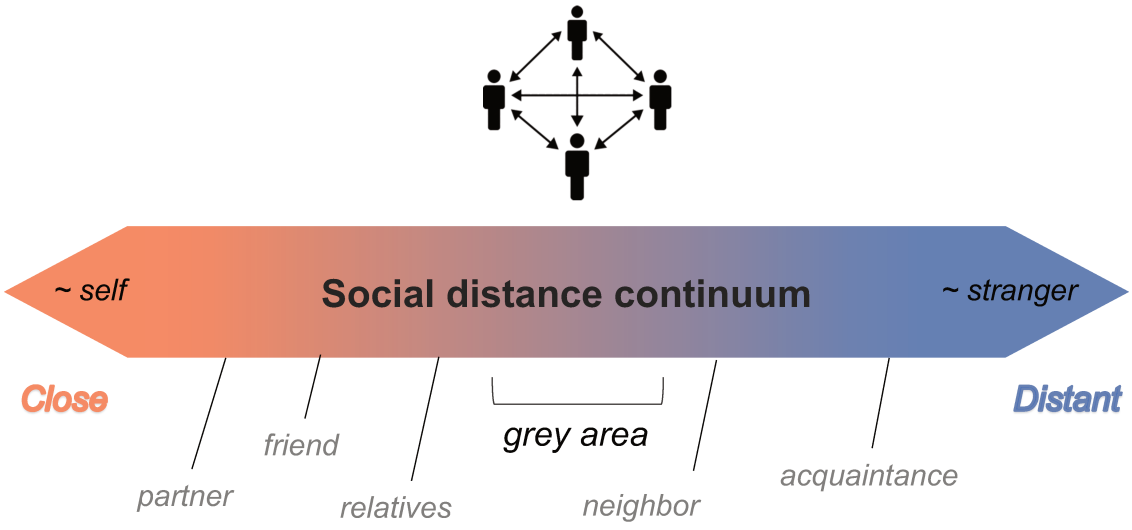
An Inventory of the Various Terminologies Referring to Social Distance in Social Cognition



As illustrated in Figure 1.2, we will consider that the self is located at the extreme-left (close) end of this continuum, whereas a pure stranger (i.e. someone we have never met) would be located at the extreme-right (distant) end of the continuum. We also integrated to this social distance schematic representation a *grey zone* in the middle of the continuum, figuring relationships toward the self that are not fully defined, or a bit vague in terms of relationship categorization, as if lying in an intermediate position between the two ends. For instance, if agent A knows agent B from a long time ago, but has not seen him/her for long time, agent B, for agent A, falls in this grey zone: not very close, but not a stranger either. The same would apply to family members we have only met on rare occasions. Despite this grey zone and the fact that we may not always be able to locate people very precisely, this schematic representation nonetheless allows to position them approximately on the continuum. In this way, it remains entirely relevant empirically to test social distance effects in terms of the relative position of all protagonists involved in a given situation.

Figure 1.2

Continuum of Social Distance from Closest to Most Distant Entities



In this continuous version of social distance, we decided to put as the closest extremity (left end) the *self*, hence considering the self as an integral part of the social distance dimension, while being at the same time the reference point of it (as conceptualized

in psychological distance theory, see Trope et al., 2007). In most cases indeed, oneself should be the socially closest entity to the self as it perfectly merges with it at 100%. Moreover, social distance is sometimes conceptualized in the literature as a mere change of perspective of one single individual—in terms of actor/observer perspective, or as a first- (or second-) vs. third-person perspective (e.g., Eyal et al., 2008). In marketing, a social distance change is sometimes described as a manipulation of the recipient (i.e., buying something for oneself or for a friend) but also as a manipulation of the role played in the situation (i.e., being the offer or the recipient of the purchase) (see Didi Alaoui & Cova, 2021; and Baskin et al., 2014 for a specific study example).

In social cognition too, several studies have contrasted the social or moral judgments of individuals depending on one's role in the situation, even if they do not always refer explicitly to it as a social distance manipulation (e.g., Berthoz et al., 2006; Eyal et al., 2008; Hirschfeld-Kroen et al., 2021; Leibbrandt & López-Pérez, 2012; Nadelhoffer & Feltz, 2008). In these cases, we consider it is still a matter of social distance because the person being responsible for or suffering from the act is varying, hence a change in the target to be judged in the situation. It is indeed straightforward to imagine that people will not judge the situation the same way depending on their role in it: Were they the wrongdoer—harming an innocent close or distant victim—, the victim of one's (close or distant) wrongdoing, or a mere independent observer of the situation?

Interindividual and Intraindividual Differences in Social Distance

Studying social distance is also complex because of its variability. We can indeed consider social distance as a dynamic dimension which can vary both *between* individuals and *for the same* individual in the course of time or depending on the situation. In other words, there are potential interindividual differences in the perception of social distance regarding the same type of target: For example, we may feel on average very close to our mother, but this may not be the case for someone who has never known their mother or who does not feel close to her overall due to past conflicts. At the intraindividual level, it is possible for the position of an agent B relative to an agent A on this continuum of social

distance to evolve over time or contexts. As an illustration, we may know very little about our next-door neighbor at first—even hate him for making too much noise—, and then become his best friend when realizing that he is a beautiful person.

In their 2018 research, Hofmann and colleagues investigated the different degrees of social distance based on the everyday moral experiences of the American and Canadian participants they surveyed. After independent raters coded the social closeness to both the victim and the agent in the reported situations, the authors presented a linear regression model of people's desire to punish the perpetrator as a function of their social closeness to him/her (e.g., partner, friend, family member, ex-partner, acquaintance, stranger). The desire to punish the agent decreased as the social proximity to this person increased. On the basis of this naïve and Westernized classification of social closeness degrees, we could deduce a certain norm about how social distance is on average perceived in Western societies (i.e., who is considered as socially close or distant). However, this normative classification cannot be generalized to all populations, cultures or relational systems: Western and Eastern societies may indeed consider social distances differently (see for instance, Marshall et al., 2022; Takahashi et al., 2008).

Specificities Related to Moral Decision-Making

For all reasons mentioned above, the question of social distance is not the easiest one to tackle in psychological research. But the domain of application in the current thesis, that is, testing the influence of social distance on *moral decision-making* does nothing to help. Even if it involves a specific target domain, morality is also a vast (and to some extent poorly delimited) field of research in psychology. Before entering into the specificities of morality for the study of social distance effects, we will briefly define social and moral cognition, and decision making as understood in the current dissertation.

Definitions of Social and Moral Cognition, and Decision Making

Social cognition is here defined as reflecting how people give meaning to the world as it currently works around them, and by which cognitive processes they move from an

event (or input) to a social behavior (or output), understood in a large sense as it could be opinions, judgments, or emotions (Ric & Muller, 2017). *Moral cognition* could then be defined as a subdomain of social cognition (Qu et al., 2022), in the sense that social cognition is here being applied to a moral context, that is, where the issue at stake is referring to some 'rules that define what is good or bad within a society' (see Qu et al., 2022, p.50). We will further detail characteristics of moral contexts in the next chapters, as our aim here is merely to provide a general definition.

Regarding the vast literature on judgment and decision making, it has, during the last decades, been more formalized and integrated compared to morality.[9] *Judgments*—'the capacity to recognize relationships, draw conclusions from evidence, and make critical evaluations of events and people' (American Psychological Association, n.d.-b) and *decisions*—'the cognitive process of choosing between two or more alternatives' (American Psychological Association, n.d.-a) will be both considered in the current work. We will however from now on refer to *decision making* as a general subcategory including both *judgments* and *decisions* (or *choice of action*) in an undistinctive way. The more specific terms will be used when relevant, and we acknowledge that judgments and decisions do not necessarily lead to the same results in moral cognition (e.g., Tassy, Deruelle, et al., 2013; Tassy, Oullier, et al., 2013).

Another complexity in moral cognition is that some researchers confound in their experiment an operationalization of social distance regarding the agent of an action (i.e., self vs. other as agent) with the manipulation of the decision-making type (i.e., judgment vs. decision) (see Du et al., 2023; Martinez et al., 2024; Saltzstein, 1994). In these works, the self as agent was considered to make a decision, whereas self as a mere observer (thus judging another agent) was considered to make a judgment. This enjoins us to consider social distance and type of decision making (judgment or decision) as two distinct factors

[9] See for instance the major framework proposed by Kahneman (2011) on the theory of the two speeds of thought. It does not mean there is no criticism about this theory of dual processes (see for instance Bago & De Neys, 2019), but most of the new theories or new frameworks proposed are still referring to the same terms and basic definitions as for the difference between a judgment and a decision, unlike the field of morality, much more disparate in terms of methods and concepts.

which should not be mixed up to avoid confounding effects. It also points at another subtlety in moral decision-making situations, namely the fact that several protagonists may be involved holding different roles.

Several Roles in Moral Situations (Agent, Victim, Recipient, Observer)

A difficulty of testing social distance effects on moral decision-making is that moral situations very often involve several protagonists, at least when they portray an everyday life situation or dilemma. Such situations often involve at least one agent (the perpetrator of the moral or immoral action at play), often (but not necessarily) one victim (the one who suffers from the bad consequences of the act, be it a harm in case of a wrongdoing or a loss in case of a prosocial act), and sometimes one recipient (the one who either benefits from a wrongdoing or is the target of the prosocial act). Additionally, moral situations also often involve a mere observer, who judges the situation from an external point of view without actively taking part in the situation[10]. Participants in moral cognition studies can be invited to endorse any of these roles, and social distance may vary in relation to any of the other protagonists.

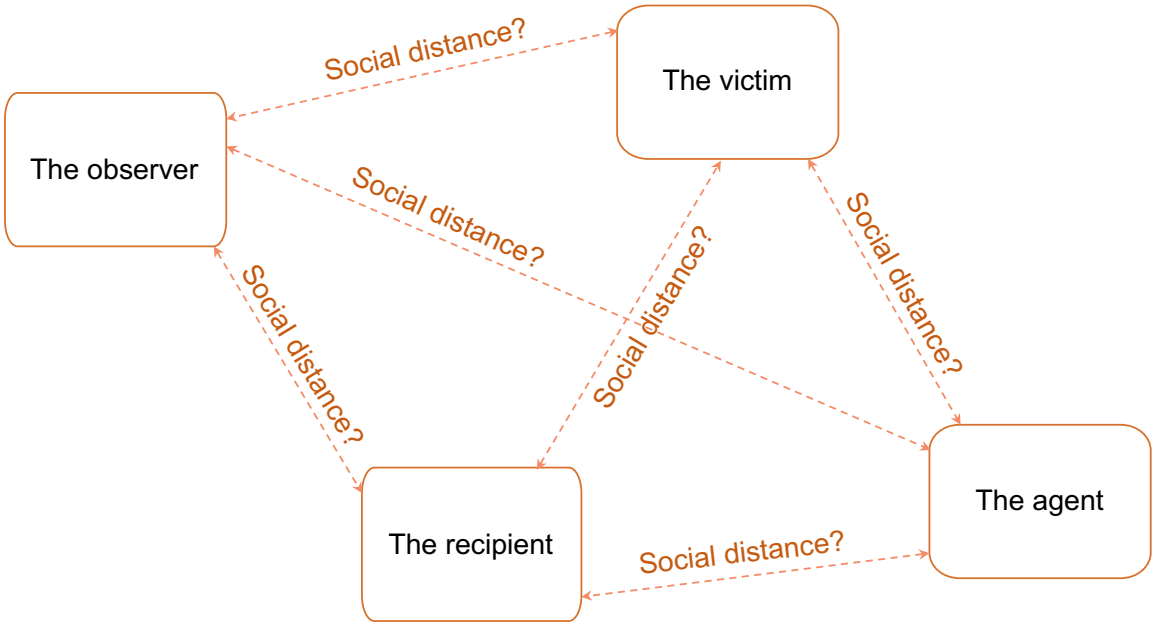
Social Distance Variations within these Roles

When examining social distance effects in moral situations, there are several configurations depending on who are the protagonists, with social distance varying: i) between the agent and the victim/recipient; ii) between the observer and the agent; iii) between the observer and the victim/recipient. Figure 1.3 illustrates this conundrum and how fluctuations in these different roles result in a wide range of configurations in terms of social distance. This further reduces the integrative aspect of how social distance is manipulated or controlled for, even when only focused on morality. This is why we first started reviewing diverse meanings and operationalizations of social distance in the present chapter, before reviewing in the next chapter evidence regarding the influence of social distance on social and moral cognition.

[10] This is notably the case in all studies asking participants to judge the situation from a third-party perspective.

Figure 1.3

Moral Situations Involving Four Possible Roles with Social Distance Fluctuations



Chapter 2

From Social Distance to Self vs. Others (Un-)Favoritism in Social and Moral Cognition: A Threefold Analytical Framework

‘Birds of the feather flock together’

[…]

‘It makes no moral difference whether the person I can help is a neighbor’s child ten yards from me or a Bengali whose name I shall never know, ten thousand miles away.’

Peter Singer (1972, pp. 231-232)

In the movie *Juror N°2* (Eastwood, 2024), the main protagonist is faced with a complex dilemma when he is called as a juror in a murder case. A man is accused of having murdered his girlfriend after a heated argument in a local bar. The woman was found dead under a bridge. The juror, soon to become a young father, realizes that he also was in the bar that same night, dealing with life's difficulties and the demon of alcoholism stalking him. *It probably wasn't a deer he'd hit over that bridge with his car that night, while driving home.* He's then torn between turning himself in and going to jail, given his history as a recovering alcoholic (though sober that night, there's nothing to prove he didn't have a drink at the bar), or wrongly accusing the innocent man of the charges against him—although his involvement with the drug cartels doesn't make him a good Samaritan. The protagonist ultimately chooses to protect his own life and that of his family at the expense of the accused's.

What is interesting is that the other jurors, unaware of this crucial information for the investigation, immediately see in the violent boyfriend an ideal culprit, despite the lack of clear evidence in this case. The fact that he used to belong to a gang involved in drug traffic already condemns him, by association. As Bègue-Shankland analyzed in a psychological analysis of the movie (2024), the jurors are prone to a robust confirmation bias here: their

knowledge about the delinquent past of the accused plays in favor of his guilt, without searching deeper evidence of his involvement in the present case. But from a slightly different angle, we could also say the accused is categorized to some extent as a member of delinquent groups, and therefore judged already more harshly from the outset than if he or she had belonged to the category of seemingly ‘respectable’ people. As for the main protagonist, he eventually decides in an egoistic way—far from his supposedly impartial role in this case—to protect his own life and family. As noticed by Bègue-Shankland (2024), this type of moral reasoning is at most part of the preconventional stage of Kohlberg’s moral development theory (1958), favoring individualism and personal needs. But this decision by our protagonist—not avoiding a consuming guilt—could also be viewed in accordance with evolutional selection theory that urges us to protect our loved- and genetically-related ones at all costs (Bègue-Shankland, 2024).

In Chapter 1, we defined social distance and reviewed various literatures examining social distance effects (albeit using various terminologies). The current chapter will focus on social distance effects pertaining to these different literatures in everyday social—and more specifically moral—cognition. Considering moral cognition to be a subdimension of social cognition, we suggest that biases triggered by social distance influence in social contexts would apply in moral situations too. It seems the case for instance in the illustrative example reported from the *Juror N°2* movie (Eastwood, 2024), with the protagonists prone to outgroup *un-favoritism* (i.e., categorization of the accused as an outgroup member and judged more harshly) and kin selection (i.e., protection of close ones) in *moral* situations.

However, we sometimes observe contradictory influences regarding social distance. Imagine in the example above how would the wife react if she learned the truth about the future father of her child. One might think that she would either continue to defend her husband to protect him at all costs or that, on the contrary—condemning the fact he had lied and not denounced himself in addition to murdering—, she would judge her husband much more severely than if it had been someone less close. Two tendencies may thus emerge, one in favor of more leniency toward self and close others, and one in favor of strangers.

Findings supporting both tendencies can be found in literature, notably because of the various roles moral situations can entail and the entanglement of social distance within it (Figure 1.3). To organize sometimes opposite effects in literature, we propose a novel integrative framework that embraces social distance effects in social and moral cognition throughout the diverse terminologies and operationalizations found. More particularly, as suggested by Hofmann and colleagues (2018), we built this analytical framework to allow tackling social distance effects spanning from the intimate and kin-based relationships (individual dimension) to group-based relationships (intergroup dimension). This was also the analytical approach taken by Ellemers et al. in a recent extensive review of moral psychology (2023).

The currently developed analytical framework comprises three levels inspired from Doise's psychosocial levels of analysis (1982). For each of these analytical levels, we review the biases induced by social distance in its plural forms, both in social and moral cognition (to show the equivalence or at least the parallelism of these two domains of application). Finally, this demonstration will be all along separated into two parts, depending on the direction of the effects of social distance. As illustrated in the above example, two opposite tendencies are found in the literature (as summarized in Figure 2.1 below): One major tendency (named Tendency 1 hereafter) comprising biases in direction of a general favoritism for oneself or close ones over distant others (e.g., self-enhancement; close ones' protection; ingroup favoritism). The second body of evidence (hereafter named Tendency 2)—even if less frequently encountered in the literature—comprises biases in direction of a certain *un-favoritism* for oneself or close others compared to distant others.

Three Analytical Levels Inspired from Doise's (1982) Framework

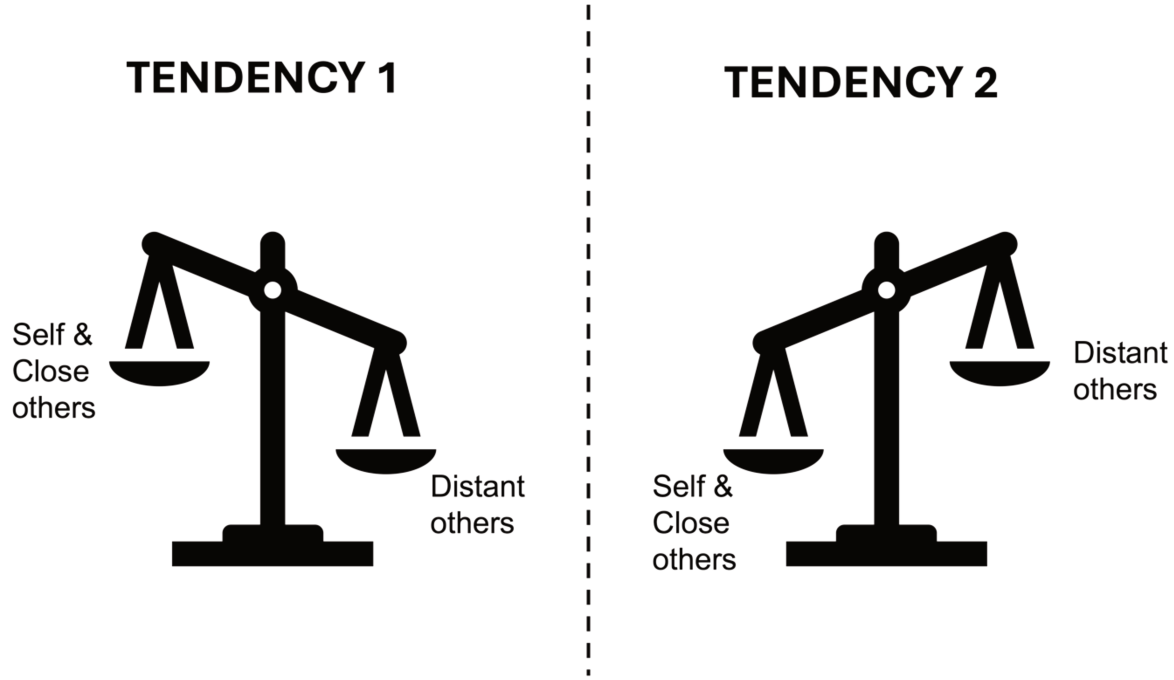
Doise's Framework (1982) in Social Psychology

Doise originally developed in 1982 a theoretical framework to study social psychology issues within four *levels of analysis*. The intraindividual level [Level I] includes research focusing on the way individuals behave and perceive their environment, without being

interested as a primary aim in the interactional aspect of these reactions. In other words, at this level of analysis, we mainly search for explanations in the individual's internal processes in reaction to their social environment (Doise & Valentim, 2015). The interindividual or situational level [Level II] corresponds to the study of the individual in interaction with their environment, thus creating a system (Doise & Valentim, 2015). It is at this level that we can consider explanations of individuals' behavior as a response to their interactions with the system (e.g., the group). The positional level [Level III] involves research tackling relationships between individuals of distinct status, for example social status, positions in society, or role in a specific situation (e.g. student-teacher; employee-employer, leader-follower etc.) (Doise & Valentim, 2015). Finally, the representational or ideological level [Level IV] corresponds to the analysis of the society belief system and normative system in which the individual is embedded.

Figure 2.1

Two Opposite Tendencies in the Literature Regarding Social Distance Effects



Note. When the scale tips to the higher side, it suggests a certain favoritism, whereas a tilt to the lower side indicates a certain disfavor.

Doise further enriched this analytical framework with propositions such as the intersocietal level (Doise, 2004), and later with the neurological (or *social brain*) level of analysis (Doise, 2011) as reviewed by Doise and Valentim in 2015. This shows the flexibility of this framework to organize empirical research. Doise and Valentim (2015) also indicate that these levels of analysis are not purely independent and communicate with each other: we can find several levels of analysis nested in a study about one single psychological phenomenon (Doise & Valentim, 2015).

Our Own Framework of Analysis Inspired from Doise

Our own framework will be based on Doise's work to examine, in an organized way, the influence of social distance on moral decision-making, as the literature largely suggests the interplay of several levels of explanations. It was indeed suggested by Lewin (1936, 1997) that both the intergroup and interindividual levels are involved in social distance. More recent research also suggested it in several works (e.g., Aron et al., 1991; Hofmann et al., 2018) without, however, undertaking an integrative work pertaining to different levels of analysis. An exception is Ellemers et al.'s review (2023), but which did not focus on the question of social distance especially. We therefore started from Doise's analytical levels, which we re-organized and adapted to create our own set of analytical levels, to better fit with the relevant literature on social distance.[11]

Table 2.1 displays Doise's analytical levels (including the more recent additional levels reviewed by Doise and Valentim in 2015), compared with our own proposition of analytical levels for the current work. We also provide a typology of studies that would be included in each level as well as prototypical examples of relevant references focusing on social distance in morality. Our first intraindividual level comprises studies dealing with a self vs. other comparison (primarily measured with a behavioral outcome). The interindividual

[11] Note that the currently proposed analytical framework is not exactly of the same nature as the one from Doise (1982). More particularly, whereas Doise's framework does not only focus on social cognition (except in his first *intraindividual* level where we search for the internal cognitive processes at play), we created the current novel framework to primarily tackle social cognition (i.e., for all levels of analysis). In other words, when we will talk about the *intergroup* level of our framework below, it means we are analyzing what happens in terms of the processes at play by an individual when confronted to a situation involving an intergroup dimension.

level involves studies analyzing the influence of social distance in terms of close vs. distant other comparison. Finally, the intergroup level of our framework reflects studies confronting judgment or decision taken about an ingroup vs. outgroup member, declined in various possible ways as standing in the subsection of this level (e.g., sharing group membership, social status, values, or cultural norms).

Table 2.1

An Integrative Framework Inspired by Doise's (1982) Analytical Levels for Studying Social Distance

Doise's analytical levels	**Our analytical levels**	**Types of studies focusing on social distance**	**Study examples in morality**
0. Neuro-logical	0. Neuro-logical	self / close vs. distant others comparison (measured via neural data)	Hirschfeld-Kroen et al. (2021); Van Bavel et al. (2008)
I. Intra-individual	**I. Intra-individual**	self vs. other comparison	Hirschfeld-Kroen et al. (2021); Weiss et al. (2018)
II. Inter-individual	**II. Inter-individual**	close vs. distant other comparison	Linke (2012); Tassy, Deruelle, et al. (2013)
	III. Inter-group	ingroup vs. outgroup member comparison (in terms of shared group membership)	McKee et al. (2023); Tang et al. (2023)
III. Positional		higher status vs. lower status individual comparison (in terms of social class or power)	Yang et al. (2022)
IV. Ideological		similar vs. dissimilar other comparison (in terms of different values)	Pereira et al. (2015); Isler et al. (2021)
V. Intersocietal		identical vs. foreign background comparison (in terms of culture or ethical background)	Endevelt et al. (2021)

Neurological Level

First, regarding the neurological level (which was added more recently by Doise and Valentim, 2015), we decided not to integrate it as a level of analysis for the current work as it has not yet been the focus of our empirical research (developed in the empirical chapters of this thesis). In addition, the focus on neural-based processes (i.e., brain activity as measured by techniques like fMRI or EEG) does not necessarily focus on final judgments or decisions as an outcome, hence less relevant for the current dissertation on moral decision-making processes. If a given study investigates both behavioral outcomes and brain activity, it will be referenced at both corresponding levels; as for Hirschfeld-Kroen et al. (2021) in Table 2.1. All the other levels are then relying as a primer intent on behavioral or self-reported data[12] (as neural data would fall in the *neurological level* of analysis). The absence of color for *neurological level* in Table 2.1 signals that this level will not be considered further in the present work. However, we consider it may be relevant for future research aiming to review social distance effects at this level of analysis, as we found several studies about this question using notably neural measures (e.g., Berg et al., 2021; Brandner et al., 2021; Gutsell & Inzlicht, 2012; Hirschfeld-Kroen et al., 2021; Molenberghs et al., 2014; Van Bavel et al., 2008; Wang et al., 2012; Wuyun et al., 2014; Zhan et al., 2018). Note that even if neurological level studies typically deal with substrate-based processes, it does not mean they could not tackle a question that relates to another level, as conceptualized here below (see for instance the review from Peng et al. in 2024 on ingroup-outgroup relationships analyzed at the neurological level).

Intraindividual Level

Our first *intraindividual* level of analysis (I) corresponds more or less to the first level of analysis from Doise's framework (1982), that is, involving research that tackles social distance from the individual point of view. This level focuses in our framework more

[12] Note that research focusing on implicit levels of analyses as with the Implicit Association Test (IAT, see Greenwald et al., 1998) are kind of at the frontier between neurological and behavioral levels, as they do not directly measure the impact of factors on decision or judgment of interest but more unconscious processes that would bias peoples' action in an automatic manner.

specifically on the processes involved at the individual level when comparing oneself with another one (i.e., when the self is directly involved in the situation vs. considered as a mere observer). To illustrate, prototypical research of this level examines social distance effects from a self vs. other perspective of comparison (e.g., Weiss et al., 2018), hereby focusing on the self vs. other discrepancy. Weiss and collaborators (2018) show in particular that moral judgments regarding oneself and another one are moderated by the level of trust in the other person. When a trustworthy coworker is involved, people rate as more acceptable the transgressions of the other compared to their owns', but when it is an untrustworthy coworker, they rate their own transgressions as more acceptable than the other ones'. This level of analysis thus also includes research taking first- vs. third-person perspective as a social distance manipulation[13] (e.g., Berthoz et al., 2006; Hirschfeld-Kroen et al., 2021) or actor-observer discrepancy (e.g., Nadelhoffer & Feltz, 2008). That is, exploring decision-making processes involved when moving from a situation where we are the actor (i.e., agent) to a situation depicted as if we were only a mere observer (i.e., thus judging another one for the same wrongdoing).

Interindividual Level

Regarding the second level of analysis from Doise's framework (*interindividual*), we re-organized it into two subcategories, that is the *interindividual level* (II) on the one hand, and the *intergroup level* (III) on the other. Intergroup dimension was already induced and nested in the interindividual level according to Doise's perspective (1982)[14]. In our proposition of analytical levels, the second level (II) then refers to studies dealing with social distance in an interactional perspective between individuals, not or to a lesser extent between members of different groups. It primarily concerns studies where one is judging either a close or a distant other (i.e., even if the self is also involved at some point as a judge

[13] We also consider research tackling second-party perspective (e.g., self as the victim of the depicted action) as part of this level of analysis.

[14] Note that our intergroup level is covering plural levels of Doise's original framework (1982). But the reason for the subdivision of the interindividual level into two different categories (i.e., interindividual and one dimension of the intergroup level) is that the intergroup dimension is already nested in the interindividual level in Doise's rationale.

of the situation, the self is not here the main point of comparison in the analysis, contrary to the intraindividual level). A prototypical study for this level could be the one from Linke (2012), where participants have to judge, as a third-party observer, transgressions committed either by a close-one (i.e., a kin or a schoolmate) or by a distant other (i.e., a foreigner) [Study 1], then suffered by the same individuals (i.e., a close-one or a distant other as the victim of the transgression) [Study 2] (see also Tassy, Deruelle et al., 2013, for another example with social distance manipulated to the victim).

Intergroup Level

The third level (III) focuses on group membership of this other individual we are in interaction with in the situation (i.e., whether we are judging an ingroup vs. an outgroup member). Our motivation to distinguish these two sub-categories (interindividual and intergroup) for the study of social distance stems from the literature on that topic at these two levels, with, on the one hand, the rich literature on the impact of close or intimate relationships on socio-moral cognitions and, on the other, the equally prolific literature on the impact of individuals' group membership on socio-moral cognitions or behaviors. For a prototypical example at this level, see notably McKee and collaborators' recent cross-cultural study (2023). The authors compare moral judgments across different combinations of ingroup and outgroup members as agent and victim (e.g., Ingroup-Ingroup with the first occurrence reflecting the agent's identity and the second one the victim's identity; Ingroup-Outgroup; Outgroup-Ingroup; Outgroup-Outgroup).

Finally, we did not integrate in our framework the other analytical levels from Doise's framework (i.e., positional, ideological, and intersocietal levels) as independent levels of analysis for the current work. We indeed considered that positional, ideological, and intersocietal levels are all subdimensions of the intergroup level, hence gathering them all under this single category. In fact, judging or interacting with a higher status vs. lower status individual is a social distance manipulation as long as the individual's own status is measured or controlled for (for an example, see Yang et al., 2022). The same rules apply for sharing ideological values (e.g., Isler et al., 2021 with religion and Pereira et al., 2015 with

political values), as well as for cultural background (e.g., Endevelt et al., 2021 for race/ethnicity) and all other dimensions found as relevant for social distance operationalization as a group filiation in Chapter 1's inventory (see Figure 1.1). If some future research aims to specifically tackle the question of social distance influence over one subdimension of this intergroup level (as social status, values, or cultural background), these presently integrated levels could be further disassociated.

Analytical Levels' Dynamic Interplay

The proposed analytical levels are conceived as a guide to organize the current reflection and work about social distance influence in moral decision-making. As mentioned previously, the retained levels in our framework were those deemed most relevant to examine social distance influence in moral decision-making, with a first- vs. third-person perspective for the *intraindividual* level; close relationships for the *interindividual* level; and ingroup vs. outgroup focused studies for the *intergroup* level. Of importance, this effort to distinguish different levels of analysis is not intended to uniquely study each level independently, but rather to:

1. Gather knowledge about our research question at each level of analysis;
2. Synthesize this knowledge at each level (i.e., what are the main tendencies, their contradictory effects, etc.);
3. Putting these different levels of analysis into perspective with one another.

In fact, a single study can fit several categories (as seen above in Table 2.1 with the example of Hirschfeld-Kroen et al., 2021) as long as it involves several levels of analysis (for this example, the neurological and the intraindividual levels). Very rarely and implicitly[15], past research has hypothesized an interaction effect between these various levels of analysis (e.g., Berg, 2021; Keefer, 2013; McKee et al., 2023), considered in the current work as different ways to operationalize and define the social distance concept. It is however of

[15] As most research in that case do not directly and/or explicitly introduce this effect as an interaction between two different ways to operationalize or conceptualize social distance (e.g., McKee et al., 2023, looked at the victim's and perpetrator's group membership impact on moral judgments, then crossing these effects with the relative perceived social closeness to both protagonists).

importance to first summarize which effects are found at each level of analysis (i.e., as aimed in the current work) before studying their entanglement. The next section therefore reviews the state of the art on biases due to social distance influence in social and moral cognition, considering these three levels of analysis as a guiding framework, while reviewing the two main tendencies observed in the literature (as illustrated in Figure 2.1).

Examining Self or Close-ones Favoritism at All Three Levels of Analysis (Tendency 1)

In social cognition—and by extension in moral cognition—, social distance is known for biasing our reasoning, perceptions, judgments, and decisions (even if not always referred to as part of the *social distance* issue per se). As seen before, we consider here social distance influence as the potential discrepancy in judgment toward oneself compared to others (Level I of analysis), or toward close others compared to distant ones (Level II and III of analysis). Most of the time, this bias goes in favor of oneself or our close ones compared to some distant others, hence in direction of Tendency 1 (cf. Figure 2.1, left panel).

This can be illustrated by many everyday situations, starting from current political controversies. In France for instance, Marine Le Pen has currently been judged for suspicion of fictitious employment and misappropriation of public funds in her political party. While she used to defend (as evidenced by interviews from the 2000s) that political parties engaging in this kind of illegal practices should be judged and severely punished by the law, she no longer sees things in the same way (having herself been charged as a politician), and is arguing for the application of an exception for her particular case. But the political area is far from being the only domain where this phenomenon is occurring. We can see the extent of this bias in everyday life, including in academics (see van Lange et al., 1997 for an illustration among social psychologists). We will then review scientific evidence of how these self-serving biases apply at all three levels of analysis depicted above.

Intraindividual Level (Self vs. Others)

In Social Cognition

Let's start with the (most intuitive) *intraindividual* level: The social psychology literature is full of evidence for this bias of favoritism toward oneself compared to others.

Starting with the literature about attribution in social psychology, Heider (1958) showed that behaviors can be attributed either to internal causes (due to individuals' traits or dispositions for instance) or to external ones (due to situational constraints). The fundamental attribution error (FAE) evidenced by Ross (1977)—also named as the *correspondence bias* by Gilbert & Malone (1995)—suggests that people often overestimate internal causes and underestimate the external causes to explain the behavior of others. But what about their own behavior? Jones and Nisbett (1972) suggested an attributional bias in the manner we interpret the causes for our own behaviors: We tend to over-attribute our behaviors to independent or contextual (i.e., external) causes, whereas an independent observer would attribute them to the individual him/herself (i.e., internal causes). This is what they called the *actor-observer asymmetry*, because we see the situation differently depending on our role in the situation. For instance, when arriving late at an appointment, we (as the one who arrives late) will gladly attribute this delay to traffic or last-minute urgency (understood as independent of our will), whereas an observer of the situation or even the person with whom we had an appointment will more likely attribute this delay to our lack of anticipation or to a lack of punctuality. However, Jones and Nisbett specified in the end of their essay (1972, p.93) that this attributional bias is guided, among other processes, by motivational stances, that is notably, to keep self-esteem high. Hence the fact that the actor-observer bias seems to occur this way especially for neutral or negative events but would probably be reversed when positive events occur (i.e., one attributing a positive issue more to oneself than to external causes compared to another observer).

To bounce off this idea of self-esteem enhancement and in direct relation with these attributional biases, there is a general tendency for human beings to evaluate themselves more positively than others (without speaking about speciesism, the tendency to overvalue the human species over other animal species). This has been evidenced in social cognition through numerous biases, such as the *self-serving bias* (SSB, see Bradley, 1978; Mezulis et al., 2004 for a meta-analysis; Zuckerman, 1979), the *better-than-average* or *above-average* effect (see Alicke et al., 1995; Alicke & Govorun, 2005; and Zell et al., 2020 for a meta-

analysis), the *self-positivity bias* (e.g., Lin et al., 2003; Zhang et al., 2013), also known as the *unrealistic optimism* (Eiser et al., 2001; Weinstein, 1980) or *self-enhancement* bias (J. D. Brown, 1986). These biases all describe the tendency to favor oneself or to perceive oneself more favorably compared to others, clearly participating this way in an asymmetry of judgment between self and others (see also Pronin, 2008).

Close to these tendencies, there is also the *bias blind spot* effect (see Pronin et al., 2002), that is, the tendency to think these cognitive biases apply more to others than to oneself (hereby confirming the tendency of self-favoritism or self-protection). But also in a more general way, all effects related to self-centered biases (see Sedikides & Strube, 1997 for a review): for instance the *egocentric(ity)* bias (see Greenwald, 1980; Zuckerman et al., 1983; and Todd & Tamir, 2024 for a recent review), the *spotlight* effect (Gilovich et al., 2000), the *beneffectance* bias (i.e., feeling responsible only for the desired outcomes, see Greenwald, 1980), and the *implicit egotism* bias (see Pelham et al., 2002). Preston summarizes these biases as such: ‘One side effect of being stuck in our own point of view is that we tend to interpret our own life events as more special than others’ (2018, p.354).

In Moral Cognition

In a similar way as for social cognition, we can find such biases also in moral cognition, as illustrated first by the *moral superiority* effect (see notably Messick et al., 1985 about fairness; Tappin & McKay, 2017). It can be defined as ‘the bias consisting to think ourselves more moral than others’ (Bègue-Shankland, 2011, p.39). This phenomenon was also described by Codol (1975) for normative rules with the concept of *superior conformity of the self*. We can also find in the literature the equivalent of the better-than-average effect for the moral domain (e.g., Han & Kim, 2022; Tappin & McKay, 2017). Klein and Epley (2016, 2017) named the tendency to believe one is more moral than others (or maybe less evil than others) as the *self-righteousness*. Stanley and De Brigard (2019) also studied the mechanisms at play to preserve this image of the *morally good self*, notably by forgetting the details of our own transgressions but not of our ‘good doings’. Finally, some researchers consider *moral hypocrisy* to be to some extent another form of this bias. Beyond the mere

discrepancy between the moral values we promote and our discording acts (or how to appear moral when we are not, see (Batson et al., 1997; Batson et al., 1999), *moral hypocrisy* can also be seen as judging one's own transgressions differently than other's identical transgressions (see Lammers et al., 2010; Monin & Merritt, 2012; Valdesolo & DeSteno, 2008; Weiss et al., 2018). This bias is sometimes referred in the literature as the *duality of virtue* (Valdesolo & DeSteno, 2008), as the *two-faced morality* (Weiss et al., 2018), or as a *double standard* (e.g., Abrams et al., 2013; Weiss & Burgmer, 2021)[16].

Interindividual Level (Close vs. Distant Other)

In Social Cognition

In the early research about self-serving biases, we already find some indicators/clues that the discrepancy of behavior attributions between self and others (i.e., actor-observer bias) goes beyond the scope of the only self (e.g., Jones & Nisbett, 1972). In other words, we would attribute more internal (vs. external) causes to successes (vs. failures) not only when reasoning on our own behaviors (compared to others'), but this would still be true when inferring about our loved ones' behaviors (compared to disliked people's behaviors). Indeed, as noticed by Jones and Nisbett in 1972:

> The tendency to infer dispositional [internal] causes is undoubtedly also enhanced when the observer dislikes the actor who performs a blameworthy act. … Again, however, the observer's bias can just as easily be reversed, as when the observer likes the perpetrator of bad acts … or dislikes the performer of good acts … . (p.93)

Hence our tendency to apply identical causal attribution processes as for ourselves to the behavior of those we like, and by extension of those we love. Whereas those we dislike (or by extension, we hate) will be submitted to the attributions we generally reserve to (distant) others compared to the self. Here is already underlying the foundations of the tendency to

[16] This should not be confounded with the duality of morality in general, that is, the fact morality is often comprising two poles, one for bright morality (the Good), the other for dark morality (the Evil). This could notwithstanding be indirectly linked to the creation of two polarized categories for 'Us' versus 'Them' (see 'Ethical Dualism', 2024).

favor close others over distant ones, as an extension of the biases we apply for oneself to specific others that are close to us (see also Pronin, 2008).

This phenomenon could be explained by what is called in the literature *self-other merging*, that is, the tendency to 'merge' with another who is close to the self. Numerous studies have evidenced this tendency to merge with close others on several dimensions, starting with mental representations. At the neural level first, representations of the self and of close others are activating some overlapping areas compared to distant others, especially in Eastern and collectivistic societies (e.g., Zhu et al., 2007 with the parents; Wang et al., 2012 and Wuyun et al., 2014 with the mother; and Zhu et al., 2016 showing similar motivational representations for friends). Cognitive mental representations for a close other are also generally more accessible than for a distant other (Andersen & Cole, 1990), sometimes even confounded with our owns (Mashek et al., 2003), and still when only sharing some familiarity (Prentice, 1990) or some similarity with this close other. For instance, Mitchell et al. (2005) showed how brain areas used for self-reflection could also be at use to infer mental states of others, as long as they are similar enough to the self.

Therefore, we can imagine how social distance influence, in terms of behavioral attribution, judgments and perceptions, could be extended from the self to other people, but only the closest ones. This phenomenon has already been demonstrated in social psychology, as with the extension for instance of the *spotlight effect* to other people that are part of our social connections (see O'Connor et al., 2024). Sedikides et al. (1998) demonstrated how self-serving bias—here blaming the partner for the failure and taking credit for success—is not occurring anymore when investigated within close relational dyads (compared with distant relational dyads)[17]. Hence suggesting that we tend to act toward the close partner more as if it was ourselves (Pronin, 2008). Alicke and collaborators also showed in 1995 that the better-than-average effect is reduced when the comparison occurs with an individuated target compared to a non-individuated one (i.e., an average student).

[17] Although the opposite result was demonstrated by Schütz (1999) among married couples.

This could be explained—beyond the reduced level of abstraction advanced by Alicke et al. (1995)—by the mere effect of identifiability of the target, one possible form of social distance as seen in Chapter 1 (in the individuated condition, people were sitting next to a real same-gendered person whom they have to evaluate on several dimensions, hence feeling supposedly socially closer to that target).

In Moral Cognition

There is a general and strong tendency as demonstrated in biology by humans (as well as non-human animals) to protect and favor their own offspring and kin, as issued first from Darwin's theory of natural selection (1859) and later formalized mathematically by Hamilton (1964), known under the *Hamilton's rule*. The direct consequence of it is that altruism is more likely to occur with kin than non-kin individuals (see 'Kin Selection', 2025). Another linked notion is the one of *homophily*, firstly developed in sociology as the tendency to bond preferentially with similar others than dissimilar ones (see 'Homophily', 2025), be it on various dimensions such as age, gender, social class etc. (e.g., Aksoy, 2015; Chen & Igarashi, 2023).

Building on Hamilton's rule, we can find some evidence for these favoritism biases toward close ones directly in the moral psychology literature. There is indeed first a *moral obligation of protection* toward close others, in particular family members, as recently evidenced by some researchers for instance in reporting a wrongdoing (e.g., Lee & Holyoak, 2020; Weidman et al., 2020), or in helping behaviors (e.g., McManus et al., 2020; McManus et al., 2021). Still in the helping literature and as an attempt of reinterpretation of Batson's empathy-altruism hypothesis (stating we are more likely to be altruistic toward people for whom we feel empathy, see Batson et al., 1991), Cialdini et al. (1997) suggested that the effect of empathic concern on helping behavior is primarily due to a certain self-other overlap and sense of oneness with the one to help, hence still participating to a self-serving behavior, extended to close others.

In relation to this, we could also mention the literature about the *mere liking effect*, that is, the fact that a mere positive attitude (as liking or loving) toward an individual will lead

us to evaluate them as more moral (Bocian et al., 2018, 2022). Some research used this term referring to other operationalizations of social distance such as *affinity* or *similarity of beliefs*, but found almost the same effects of favoring those that have similar (rather than dissimilar) attitudes (e.g., see Ross et al., cited by Pronin et al., 2004; Bocian et al., 2018). In the same idea, we could finally mention at this level of analysis the vast social discounting literature, with the robust effect of people being more prone to altruistic choices (e.g., more willing to give money) to a close other rather than a distant other as reviewed by Jones in 2021 (see also Jones & Rachlin, 2006 and Rachlin & Jones, 2008 applied to relatives).

Intergroup Level (Ingroup vs. Outgroup Member)

In Social Cognition

At the intergroup level too, these biases are involved even if often not directly linked to the previously mentioned social distance biases at intraindividual and interindividual levels. As intuitively deduced from the previous section, if favoritism biases toward oneself also apply to close others, we may think—as an extension—that a socially close ingroup member would also be favored compared to a distant outgroup one. This is a consequence of what is underlying in research suggesting a *self-ingroup overlap*—in a similar manner than the *self-(close) other overlap* (e.g., Gutsell & Inzlicht, 2012; Otten & Epstude, 2006; Smith et al., 1999; Smith & Henry, 1996). As another example of this extension of favoritism bias to ingroup members, Ross and collaborators (cited by Pronin et al., 2004) found that the *bias blind spot* also occurs for other people with the same position on US policy but not for those with furthest position on the same question.

In social psychology, this bias in direction of favoritism toward ingroup members has extensively been studied in the intergroup conflict literature, first initiated by Tajfel et al. (1971), then theorized under the social identity theory (see Tajfel, 1974; Tajfel & Turner, 2004). According to the authors, people generally identify themselves with the category they are assigned to, hereby strengthening their group membership. They then generally tend to be more positive toward and to favor other members of their own group compared to those of the outgroup, in order to preserve a high self-esteem. In a situation of intergroup conflict

(with various possible dimensions of group considered), this leads to the *ingroup favoritism* bias and to discrimination toward the outgroup. Further research later showed this bias is more guided by ingroup love than by outgroup hate, as showed by Brewer (1999) or Brown et al. (2006), see also Balliet et al. (2014) for a meta-analytical review on this question. This phenomenon of *ingroup favoritism* is also found when tackling at the neural level and/or with a minimal induction of group membership as demonstrated by the inaugural paradigm of minimal group induction from Tajfel et al. in 1971 (see also Van Bavel et al., 2008).

In Moral Cognition

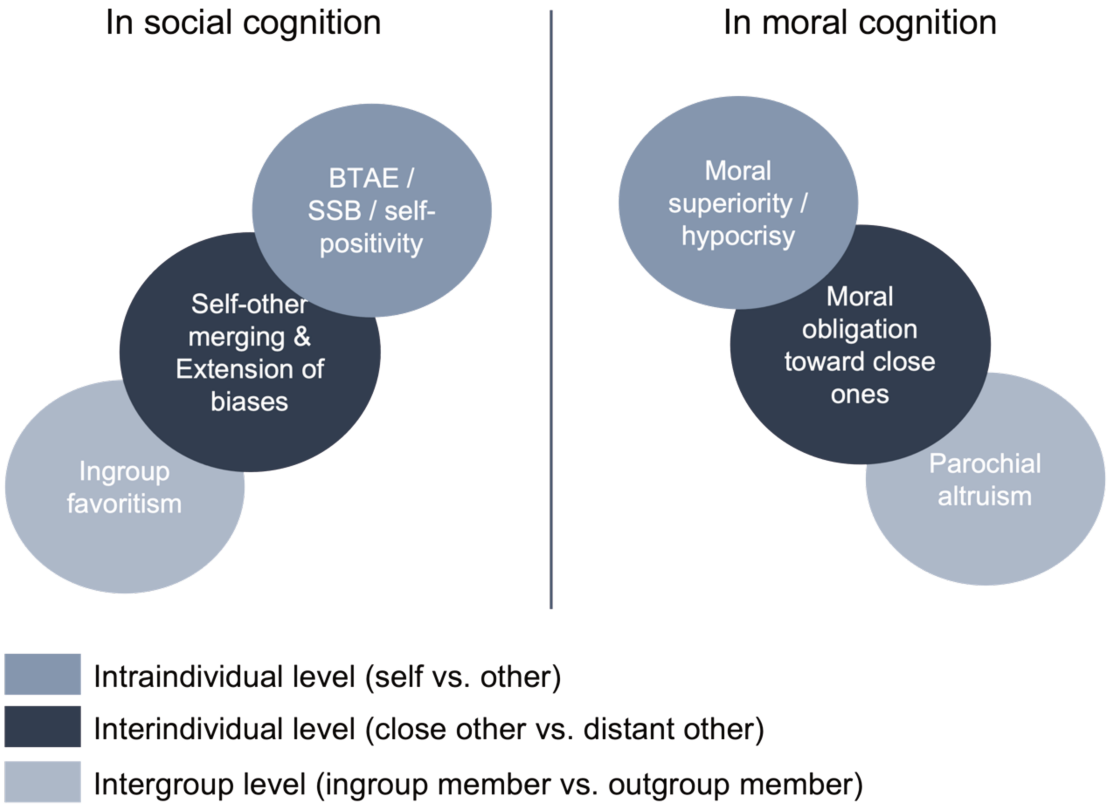
In morality, we also find the same biases as often applied to a context of intergroup conflict and discrimination (hence an almost direct link with the moral psychology literature). Liu et al. (2018) for instance found ingroup favoritism in a moral task where participants have to punish ingroup or outgroup members for a wrongdoing (see also El-Bialy et al., 2022; Endevelt et al., 2021; Gonzalez-Gadea et al., 2022; Schiller et al., 2014). McKee et al. (2023) demonstrated harsher moral judgments toward outgroup (vs. ingroup) perpetrators. We can also find consistent results with research on trolley-like dilemmas but applied to intergroup relationships (e.g., Cikara et al., 2010). Others found the same bias in tasks related to prosocial decisions or cooperation, as illustrated by Everett et al. (2015) or Lazić et al. (2021) by children. Some also describe this phenomenon as *parochial altruism* (see Boulu-Reshef & Schulhofer-Wohl, 2019 for an example). Finally, we can also find some examples in the literature that generalize the moral hypocrisy effect (as described above) at the intergroup level (see Valdesolo & DeSteno, 2007).

To end with this section, we can conclude that social distance—understood as previously defined, as a continuous dimension starting from oneself to a complete stranger—can be linked to several biases in both social and moral cognition. To sum up these findings for moral cognition specifically (as this is what will be at stake for the rest of this work), we identified the moral superiority of the self and moral hypocrisy at the intraindividual level, the protection of close ones through special moral obligations at the interindividual level, and the ingroup favoritism and parochial altruism at the intergroup level (see Figure 2.2 for a visual

summary of these biases). What is now worth to be noticed for social distance study in morality is that—although the previous biases are the most frequently encountered in terms of their direction—we can find some occurrences of biases going in the opposite direction as we will see in the next section.

Figure 2.2

Examples of Social Distance Biases in Direction of Tendency 1 in Social and Moral Cognition at the Three Levels of Analysis



Note. BTAE = better-than-average effect; SSB = self-serving bias

Examining Self or Close Others' *Un-Favoritism* at All Three Levels of Analysis (Tendency 2)

What is remarkable with social distance biases is the fact that people have in some circumstances kind of opposite moral standards when acting or judging a situation where a close one is involved. Think again to our juror movie example, we can feel quite empathetic to the main character, even if guilty of murder, it was an accident (and we know for sure it

was, as an omniscient spectator). We can yet hardly forgive him for not turning himself in when he realized he was the culprit, all the more knowing that he was condemning another innocent man to almost life sentence. We may think this because the main protagonist looks just like Mr. Nobody and could easily be our husband, friend, or neighbor. Because he is socially in a middle-class status and resembles anyone else, we may expect from him to behave in line with our moral ideals: to denounce himself for his crimes and assume the irreversible consequences of his acts.

We will thus review in this section how social distance is sometimes affecting decision making in the opposite direction than the one previously reviewed, that is, judging oneself (or close others) more harshly than (distant) others for the same transgressions (as we would in the example above judge the main protagonist harsher than if it had been a less common, outgroup individual). This is what we called the Tendency 2 of the literature (see Figure 2.1, right panel), although less frequently evidenced in the literature compared to Tendency 1, we will review research to illustrate this tendency both in social and moral cognition and following the three analytical levels as a guiding framework.

Intraindividual Level (Self vs. Other)

In Social Cognition

Contrary of the well-known self-serving biases, one may find in some cases an *other-serving bias*, as demonstrated by some researchers in specific applications. Palmeira et al. (2015) demonstrated for instance this counterintuitive *other-serving bias* in advice taking: We are more prone to attribute an internal cause (i.e., responsibility here) to another adviser for positive rather than negative outcome advice, and the contrary was found when we are ourselves the advice-giver. They however found that this bias required specific conditions such as the fact that the final decision-maker follows the advice in question. This reversal of the traditional self-serving effect has also been found in other research contexts such as in teacher-student relationships, with teachers more frequently taking responsibility for student's failures and attributing more often successes to the student's efforts or ability (e.g., Ames, 1975; Ross et al., 1974). It has also been found in competitive team contexts: Gill

(1980) found a self-serving bias when considering attributions of failure or success between teams but an *other-serving* effect when considering the same attributions within the team. In a close relational context, Sedikides et al. (1998) failed to find the expected self-serving tendency contrary to the distant relational context, even if no clear evidence of the reverse pattern was found. This at least suggests a suppression of the self-serving bias when tested among close relationships (see Schütz, 1999 for contradictory findings).

Moreover, in parallel to the well-documented better-than-average effect, some researchers also suggest a *worse-than-average* or *below-average effect*, that is, the tendency to believe one is below the average person on several skill or trait dimensions, specifically occurring in domains where success is difficult to reach (Kruger, 1999) or for rare or uncommon abilities (D. A. Moore, 2007). Zell and Alicke (2011) showed for instance that while younger adults were most of the time subject to the better-than-average effect, older adults showed a worse-than-average effect especially in domains where they are viewed deficient (i.e., athleticism, physical attractiveness). This could however depend on people's motivation to appear above average depending on their degree of personal investment or effort put in the domain at stake. For instance, someone doing a lot of strength training will have some difficulty to consider him/herself as below average about being athletic, even if it is a domain where success is *objectively* hard to reach (in terms of investment, effort etc.).

Another instance found in social cognition is the *negativity bias*, as the opposite of the positivity bias (see previous section), that is, when people interpret the reality in an unfavorable way for themselves. It has been evidenced especially among specific populations, such as depressed people (e.g., Beck, 1987; Sweeney et al., 1986). Participants from specific cultures also express less positivity bias than other cultures (e.g., Lee & Seligman, 1997 with Chinese vs. Americans), as for people showing a pessimistic explanatory style (e.g., Peterson et al., 1988). However, some further research argues more in favor of an attenuated *positivity bias* (i.e., being less inclined to it) by these populations rather than a clear *negativity bias* (see Mezulis et al., 2004).

In Moral Cognition

In the moral domain too, we observe such tendencies of disfavoring oneself compared to others, but more focusing about valuing others' life compared to one's life (e.g., self-sacrifice literature, mainly dealing with trolley-like dilemmas) or blaming oneself more than others for a same mistake with immoral consequences. In this sense, Sachdeva et al. (2015) demonstrated that in the context of the footbridge trolley dilemma (rather than the traditional switch trolley dilemma), people (especially those with a US cultural background, to a lesser extent for those with an Indian background) tend to sacrifice themselves more than others. In a similar way, Bahník et al. (2021) demonstrated a self-sacrifice tendency, but only occurring when participants are asked to judge about the wrongness of the act (hence a normative and negatively framed question) and not in a prescriptive or positively framed question. In other words, people were more likely to think it is wrong to sacrifice another life rather than themselves (but they did not particularly think so when asking what should be done). The authors even show that this bias is also sensitive to perspective taking as participants showed this tendency in their answer only in a first-person perspective (i.e., when choosing for oneself) and not in a third-party perspective (i.e., choosing for another one) (Bahník et al., 2021). Sachdeva et al. (2015) found the same perspective contingent effect for other-serving tendencies (i.e., people preferred to sacrifice themselves rather than others when they are actors of the situation but, as an observer, they did not prefer others to sacrifice themselves). It should however be noted that some research of the self-sacrificing literature found inconsistent results compared to what is described above, see notably Huebner and Hauser (2011), who found still a higher proportion of people more willingly sacrificing another life than their own (even if not statistically tested).

Weiss and Burgmer (2021) preferred to develop the notion of *moral* '*hypercrisy'* (paralleling the *moral hypocrisy* detailed in the previous section) to reflect this other-serving tendency, especially found when the moral situation at play involves close people. They found people judged their close friends more leniently they would judge themselves for the same transgression (Studies 1 & 2), the same directional effect is found with the romantic

partner (Study 3) but not with a stranger, and no matter if participants are themselves the victim of that transgression (Weiss & Burgmer, 2021). In a similar way, *hyperaltruism* is described as the tendency of being more ready to pay more to prevent harming others compared to oneself or to ask for a greater compensation to harm others than oneself (e.g., Kitcher, 1993; Volz et al., 2017). Harm aversion for others was also found as higher than harm aversion for oneself (Crockett et al., 2014, 2015; Volz et al., 2017).

Another important theoretical rationale to demonstrate this tendency toward self-blame—even if only a few occurrences were found in the psychological literature—is the *agent-regret* concept, first theorized by Williams (1981), later discussed by others (see notably Jacobson, 2013; MacKenzie, 2017; Sussman, 2018). The philosopher suggests that, in case of a non-intentional wrongdoing committed by the self, there is a specific form of *regret* directly linked to the status of *agent* of the situation (i.e., the *agent-regret*) and that is not felt as a mere observer: One will feel more regret and feel more responsible for the consequences of the act than if another one was the perpetrator (Williams, 1981). This has been experimentally tested and validated by the study of Hirschfeld-Kroen and collaborators in 2021: People judged themselves slightly harsher on a wrongness scale than when a third-party was the perpetrator and hold themselves as even more responsible than others for the same wrongdoing (even if non intentionally perpetrated). We could also mention as a demonstration of this phenomenon some instances of the *blame-taking* literature, the fact of taking the blame on oneself for another one's wrongdoing, hence protecting the other (see Gudjonsson et al., 2007; Willard et al., 2015 for examples)[18].

Interindividual Level (Close vs. Distant Other)

In Social Cognition

At the interindividual level, this Tendency 2 would mean we tend to favor a stranger rather than our close one (i.e., our best friend, our mother etc.). Although a bit counterintuitive in front of the previously mentioned specific obligation we have to protect our

[18] This seems again to depend on the relationship with the perpetrator (i.e., people take more the blame for a partner or friend than for an acquaintance, a family member, or another, see Willard et al., 2015).

close ones, we could think that—with the same rationale used for Tendency 1—biases applying to self might permeate to a certain extent to close others (due to self-other overlap or merging). That is, when we are hyperaltruistic or 'hypercrite' in the sense that we favor another compared to oneself in some situations (i.e., self-sacrifice), we may think the same bias occurs when judging two others, one close and another more distant. Few research however dealt with the analysis of this tendency in social cognition. Indeed, the previously mentioned biases such as *worse-than-average effect*, *other-serving bias*, or *negativity bias* are most of the time tackled with comparisons between oneself and another, but never or very rarely between a close and a distant other. We would expect following this tendency that one gives more responsibility for a failure to his/her close one than to the other distant person. Sedikides et al. (1998) showed that this other-serving tendency appears especially within close relationships (i.e., favoring the close other) rather than within distant ones, but never compared directly from a third-person perspective how one would attribute the respective failure or success of the dyads depending on the social distance between one and the others. This seems moreover not to be a necessary condition for this Tendency 2 as Palmeira et al. (2015) found the *other-serving bias* toward socially distant people.

In Moral Cognition

We could however argue that there is at least a tendency to look more closely at the behaviors of close others rather than those of distant others, probably due to the higher emotional and personal involvement that close ones' moral transgressions potentially trigger. For instance, if you learned that your son had committed a theft for no good reason, you would maybe blame him and punish him harsher for this transgression than you would have done for another young, because he is your son (and therefore he is supposed to be well-educated, sharing good values etc.) and this threats you own reputation or identity of a 'good parent'. However, few evidence of the literature goes in this direction, as illustrated by the Study 2 of Sachdeva et al. (2015) where both sacrificing oneself or a stranger is more frequently approved than sacrificing a close one compared to another unknown person. It

seems therefore that close ones get a *special immunity* that do not allow this extension of *other-serving biases* from self to close others.

Intergroup Level (Ingroup vs. Outgroup Member)

In Social Cognition

At intergroup level however, this *other-serving* tendency—which also reflects harsher judgments for oneself or for close others compared to distant others—is not as rare as at the interindividual level. This is indeed exactly what is depicted in the social cognition literature through the *Black Sheep Effect*. According to Marques et al. (1988), people judge ingroup members who behaved well more positively than outgroup members (i.e., extension of self-serving bias to ingroup members) but also judged ingroup members that behaved badly more negatively than outgroup members, especially if group identification is heightened. In other words, ingroup members (both those doing good and doing bad) are subject to *more extreme* judgments than outgroup members (either in direction of way more positive judgments if they're doing great or in direction of way more negative judgments in case of bad doing). The main reason advanced by the authors, in line with social identity theory (Tajfel & Turner, 2004), is that people search by this way to protect their own social identity, as their group membership is relevant to it. They therefore exclude or reject the deviant ingroup member to protect their group identity, and in this way their own identity. This hypothesis is then supposed to be in line (or at least not contradictory) with social identity theory and predictions of the Tendency 1.

In Moral Cognition

In the field of morality more specifically, we also find some occurrences of this *Black Sheep Effect* (sometimes even defined as properly occurring in the moral domain, see Tang et al., 2023), notably in research where some deviant actions are perpetrated either by an ingroup or an outgroup member (see for instance Abrams et al., 2014; Bettache et al., 2019 for harmless transgressions only; Boggio et al., 2023 for purity violations; Fousiani et al., 2019; Shinada et al., 2004; Tang et al., 2023; Yang et al., 2022). The *subjective group dynamics theory* was developed by Marques et al. in 1998 as the fact that people hold their

ingroup norms as subjectively legitimate in order to preserve their own positive group identity, via maximizing both intergroup and intragroup differentiation (i.e., differentiating between deviant and non-deviant ingroup and outgroup members).

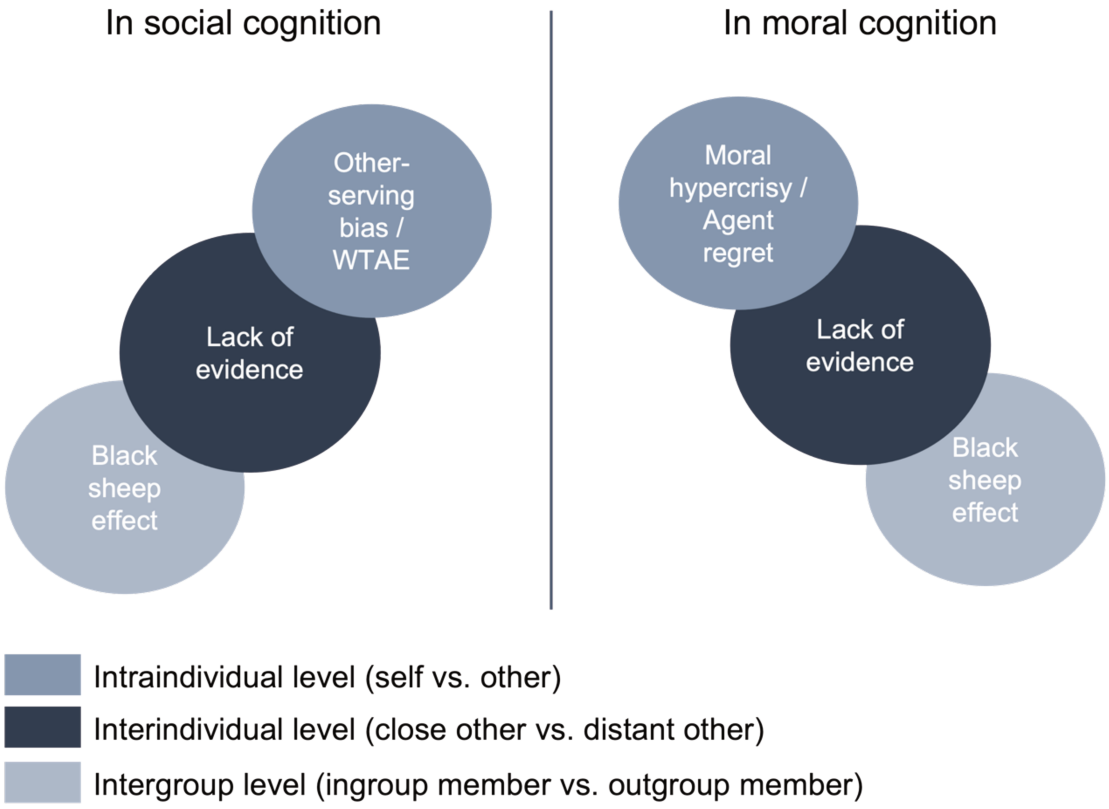
Despite substantial evidence for this tendency of more severe judgments toward ingroup deviants than outgroup ones (see references listed above), these findings are sometimes challenged by replication efforts. A recent intercultural replication and meta-analysis of this effect from Bonetto et al. (2023) found no significant difference in judgments toward ingroup vs. outgroup deviant members across six experiments using diverse dependent variables. The authors pointed to the fact that the influence of social distance on judgments differentiation between ingroup vs. outgroup deviants may exclusively occur under specific conditions, as when there is a strong identification with the ingroup. They also highlighted that this effect could be more context-sensitive than previously hypothesized (Bonetto et al., 2023). However, Tang et al. (2023) further put things into perspective as they noticed Bonetto et al.'s replication (2023) took into account a combination of moral and non-moral dimensions as dependent variables, and they considered mixed situations in terms of agent/victim identities (either an ingroup deviant agent over an outgroup victim or an outgroup deviant agent over an ingroup victim), thus potentially creating confounding effects. Taking into account only within ingroup or within outgroup deviations, Tang et al. (2023) indeed found the expected *Black Sheep Effect*, especially for morally relevant transgressions (in comparison to social transgressions).

To sum up, in a minor tendency of the literature both in social and moral cognition (Tendency 2), we also found evidence regarding social distance effects of opposite direction compared to Tendency 1 results, especially at intraindividual and intergroup levels. At the intraindividual level, we found notably in moral psychology effects such as *moral hypercrisy* or specific *agent regret* bias in reasoning, whereas the same direction of effect is mainly found at the intergroup level under the *Black Sheep Effect*. For the interindividual level nevertheless, we lack clear evidence that this other-serving tendency is extended or generalized from oneself to close others, hereby suggesting a possible immunity for specific

related persons (see Figure 2.3 for a synthetic illustration). In the following, we will synthesize this appearing tension in the literature and review the already existing attempts to resolve it.

Figure 2.3

Examples of Social Distance Biases in Direction of Tendency 2 in Social and Moral Cognition at the Three Levels of Analysis



Note. WTAE = worse-than-average effect

Tension in the Moral Literature Between these Two Tendencies

Tension and Main Attempt to Resolve it

We have documented in the previous section how social distance biases that apply in social cognition at all three levels of analysis (intraindividual, interindividual, and intergroup) could apply to moral cognition too. We also documented two tendencies in the literature: The mostly found Tendency 1 as a *favoritism bias* toward oneself or close others compared to distant people; and the opposite Tendency 2, which appears only in certain contexts as an

un-favoritism bias toward self or ingroup members (but not or to a lesser extent toward close others) compared to (distant) others. In other words, it seems that most of the time, people favor and protect themselves or their close ones over (distant) others, but in some specific cases, they favor (distant) others rather than themselves or close others. These two tendencies, although apparently contradictory in terms of direction, could in fact be explained by some common mechanisms of self-enhancement and group identity protection. As seen earlier, Tendency 1 (favoring oneself or close other over distant other) is directly in line with this self-enhancement strategy and protection of one's own positive identity. However, Tendency 2 findings (favoring others over oneself) are less straightforwardly understood as an indirect self-enhancement strategy. We detail hereafter for each level why this may nevertheless be the case.

At the intergroup level first (as the most documented in literature), it has already been theorized this way by Marques et al. (1988), as they explained apparently contradictory effects for good- vs. wrongdoings via the social identity protection hypothesis. In this way, rejecting or excluding a deviant ingroup member is a way to preserve the positive identity of the group. At the intraindividual level, other-serving tendencies are also explainable by this hypothesis as people could use it as a way to be perceived as a *supra-moral* or *hyperaltruistic* person, hence reinforcing the positive image of the self (see the positive side-effects of self-sacrifice, Bargdill et al., 2022). However, this is less defensible for the interindividual level, primarily because of the lack of evidence in the literature that would show a clear *un-favoritism* effect when judging close others compared to distant others. This is also why we chose the term 'tension' to illustrate these controversies rather than 'contradictions', because of the compatible nature of these contradictory findings in some cases.

What remains less clear if we consider social distance as a continuum that can be examined at several levels of analysis (hence suggesting inherently the self / close other / ingroup member overlap) is to find no robust evidence consistent with Tendency 2 at the interindividual level while such evidence exist for the other levels. If close others are an

extension of the self (i.e., self-other merging literature), and ingroup members an extension of our close relationships or of the self (i.e., self-ingroup merging), why this lack of evidence regarding other-serving tendencies at interindividual level, that is, judging a close one harsher than a distant one for the same transgression? It could be explained by the fact that close others somehow get a specific *immunity* in such cases, but this remains an open question to date.

Furthermore, for the same analytical level, we can document non-consistent findings, either showing a positive or negative effect of social distance or showing null results as illustrated by the example of the Black Sheep effect confronting Bonetto et al.'s replication (2023) and Tang et al.'s work (2023). This may again suggest that examining the effects of social distance without an integrative framework (in terms of levels of analysis) and without a clear overview of existing evidence for each level is complicated. It furthermore suggests that it is all the more difficult when the situations involved are moral situations, as both moral transgressions and prosocial actions may be at stake, along with different roles (agent, victim, recipient, observer).

Other Attempts to Resolve this Tension

We suggest—in accordance with Marques et al.'s Black Sheep hypothesis (1988)—that the valence of the action (negative for transgressions or positive for prosocial acts) may be a crucial criterion for the influence of social distance in morality, at least at the intergroup level. It would indeed predict findings consistent with self or close one favoritism (Tendency 1) in case of prosocial acts and the other way around (i.e., self/close-one *un-favoritism* as in Tendency 2) for moral transgressions. However, some inconsistencies emerge, as for moral transgressions both tendencies within diverse studies were observed, even when staying at the intergroup level (e.g., McKee et al., 2023 for ingroup transgressor favoritism; Tang et al., 2023 for ingroup transgressor *un-favoritism*). The valence of the moral action at play thus cannot *alone* explain these discrepancies, although it seems to play a role.

We suggest that another crucial criterion to consider—beyond the mere valence of the moral act—would be the harshness or severity of the transgression. Specifically, a minor

(e.g., harmless) case of transgression would trigger more lenient judgments for our close ones, whereas more severe transgressions (e.g., sexual assault) could induce the reverse pattern of non-protection. This would stem from the intuitive idea that we can no longer support someone—even very close—who has committed atrocities that go beyond what is bearable for us in terms of endorsed values (hence a too huge threat for one's own identity).

But again, literature is not clear about this severity hypothesis as—despite intuitive thinking—we found no study supporting this view (i.e., favoring close others for minor infractions but not for more severe ones). Other research teams have demonstrated the reverse pattern of results (e.g., Berg et al., 2021; Kouabenan et al., 2001; Weidman et al., 2020). Weidman and collaborators (2020) evidenced throughout five studies a favoritism bias toward socially close agents compared to more distant ones (i.e., people were more likely to predict they would lie to the police to protect—via not reporting—close transgressors compared to distant ones). This tendency was found as even truer (larger effect)[19] when the act in question was of high severity compared to a low severity transgression (i.e., meta-analytical interaction effect between social distance and severity) (Weidman et al., 2020).

What should further be noticed is that the type of transgression may count as much as its severity. McKee et al. (2023) analyzed ingroup favoritism effect among both physical and emotional harm. The authors found the ingroup favoritism bias (i.e., judging overall the ingroup transgressor more leniently and the transgressor more harshly when the victim is from the ingroup) was even stronger when applied to an emotional harm compared to a physical harm transgression. Although not statistically tested[20], they argue this could be due to the fact that emotional harms, as less obvious to notice than physical harms, may also be

[19] The interaction coefficient being positive, it means the difference between the desire to protect a close (vs. a distant) transgressor is larger in the condition of high severity (vs. low severity). It does not mean however that we would protect a close one at the same level for a high severity act than for a low severity act (i.e., the likelihood of protection still decreases with the severity of the act). See Weidman et al.'s work for more (2020).

[20] We did not find such an interaction test in the original paper, or in the online script.

more easily denied[21], hence a greater propensity to relativize these harms when the perpetrator was an ingroup member (or when the victim was an outgroup member).

As an extension of the type of transgression as potential moderator to explain social distance influence, some discrepancies may be explained by the nature of the transgression in terms of moral values it threatens. That is, people may decide whether they want to protect or punish their close friend for a given wrongdoing based on the act's relevance for shared moral values. If my friend cheats on his girlfriend whereas we met and became friends at a conference about gender equity, this will probably be hardly forgivable as—beyond the mere transgression—it also threatens our friendship (via the betrayal of a moral value we had in common). Related to this, Weiss et al. (2018) demonstrated that a transgression is deemed more acceptable when self-perpetrated than when perpetrated by an untrustworthy coworker (i.e., not sharing the value of trust), whereas this is not the case when perpetrated by a trustworthy other.

Toward an Integrative Threefold Approach for Social Distance Influence in Moral Decision-Making

Given these inconsistencies about social distance effects in morality and the lack of a clear picture of where they come from, the present work argues in favor of a unifying contribution integrating several levels of analysis for social distance within the previously proposed framework. However, no such integrative review currently exists. Some research proposed models that could be linked or applied to social distance influence, but they are either too general (e.g., considering social distance in a broad sense) and not specifically focused on morality or do not encompass all three levels of analysis. We could for instance briefly refer to the empathy-altruism hypothesis (Batson et al., 1991) which argues that altruistic behaviors are fundamentally motivated by the empathic concern we feel for others, and not by the personal joy it could entail. However, as long as empathy is an emotion, hence not a manipulation of social distance per se, this is quite an indirect operationalization

[21] They also put forward the argument that outgroup members' internal emotions are potentially more difficult to access, hence more lenient judgments toward the transgressor when they are the victim.

of social distance. Consistent with this, Batson, Sager, et al. (1997) demonstrated that there is no relationship between this empathy-altruism tendency and the group membership or the self-other merging measures. Cialdini et al. (1997) yet offered an alternative view showing how the empathy-altruism tendency disappears once we control for the feeling of oneness (i.e., self-other overlap), hence better suggesting a self-interest bias in these altruistic behaviors.

More specific to the moral realm, Noval and collaborators (2018) developed a model to explain why ‘we distance ourselves from those we harm’ through the motivated dissimilarity construal, that is, ‘the act of purposely and proactively distancing oneself psychologically from the victim of one’s own self-serving behavior’ (p.145). However, this is again not our first focus as the social distance is here more considered as a consequence and not as a determinant of the behavior. We have also retrieved during this literature review a quite significant number of PhD dissertations dealing with social distance influence in morality, under different perspectives (e.g., Altman, 2011; Berg, 2021; Castaneda, 2017; Dasci, 2019; Grime, 2005; Saunders, 2014). The problem remains however the same: although probably very relevant to the question we are tackling, all these works often use disparate operationalizations and definitions of social distance and of morality. There is also a little amount of them focusing on the influence of social distance (or close relationships as often mentioned this way) in the moral development of young children or adolescent. Again, this could be interesting to compare the findings from these studies (notably to check whether the late adolescents’ responses fit with those of adults or not, and more so than younger ones) but this is not the primary focus in the current work.

Our current goal—more explicitly depicted in the next chapter—is to propose such an integrative review of social distance influence in moral decision-making, and hence to identify the most relevant moderators to be integrated in future investigations. This should start by gathering and organizing the existing literature in a systematic manner within the three levels of our analytical framework presented above. This review is presented in Chapter 3.

Chapter 3[22]

A Scoping Review of Social Distance Influence in Moral Decision-Making

Introduction and Rationale

Why Doing a Scoping Review About this Research Question?

As introduced earlier, the impact of social distance in moral decision-making is not a novel question in the research domains of social and moral psychology. It has already been tackled a lot, especially these last two decades. However, as evidenced at the end of the previous chapter, we struggled to find a comprehensive overview on this question, as concepts and terminologies on this topic are often very diverse and findings not always consistent. This is certainly due to the different approaches taken to tackle this topic, which almost makes each research team working in parallel from its own perspective, with limited awareness—and hence discussions—of the other existing perspectives. Moreover, empirical studies within a single approach are often hardly comparable, given the use of very diverse methodologies and operationalizations for social distance (even diverse definitions for some).

This is not a new issue in the literature regarding various area of research (see Almaatouq et al., 2022 for a debate about the lack of commensurability in research more generally). We could even talk about kind of a fragmentation crisis of the field regarding the proliferation of constructs in the literature to designate the same concept (see notably Anvari et al., 2023; Elson et al., 2023). But we argue—as done by Almaatouq and collaborators (2022)—that research in psychology considered as a cumulative science should be integrative toward what has already been done and should sometimes provide good synthesis or overview of that point. Otherwise, research on a given topic is sometimes proliferative but also lowly commensurable (i.e., we cannot directly compare findings from

[22] We particularly thank Marie Dall'Agnol, Lucie Vernay, and Zoé Szczepaniak for their precious help and collaboration in this scoping review work.

one study to another). This is also the problem occurring with metanalytical efforts applied to a vast question (i.e., that is tacked in the literature through too diverse methodologies or concepts).

Delimitation of the Scope for this Review Endeavor

Regarding Social Distance

As previously developed in Chapter 2 (see Table 2.1), we defined a framework of analysis for the study of social distance in moral and social psychology taking a threefold psychosocial perspective (inspired from Doise's framework, 1982). This analytical framework comprises: (a) a first intraindividual level of analysis with comparison of studies manipulating or measuring social distance between self-perspective and other- (3rd party-) perspective; (b) a second interindividual level of analysis for studies comparing social distance influence manipulated toward a close one and a distant other (considering the self as a mere observer); (c) a third intergroup level of analysis when comparing social distance influence from a group membership perspective (ingroup vs. outgroup member), which can be extended to social class or other group dimensions. As such, we considered in this scoping review all terms and possible relevant operationalizations of social distance as reviewed in our inventory of Chapter 1 (Figure 1.1), using as an organizing framework the three analytical levels defined above (see Figure 3.1 below).

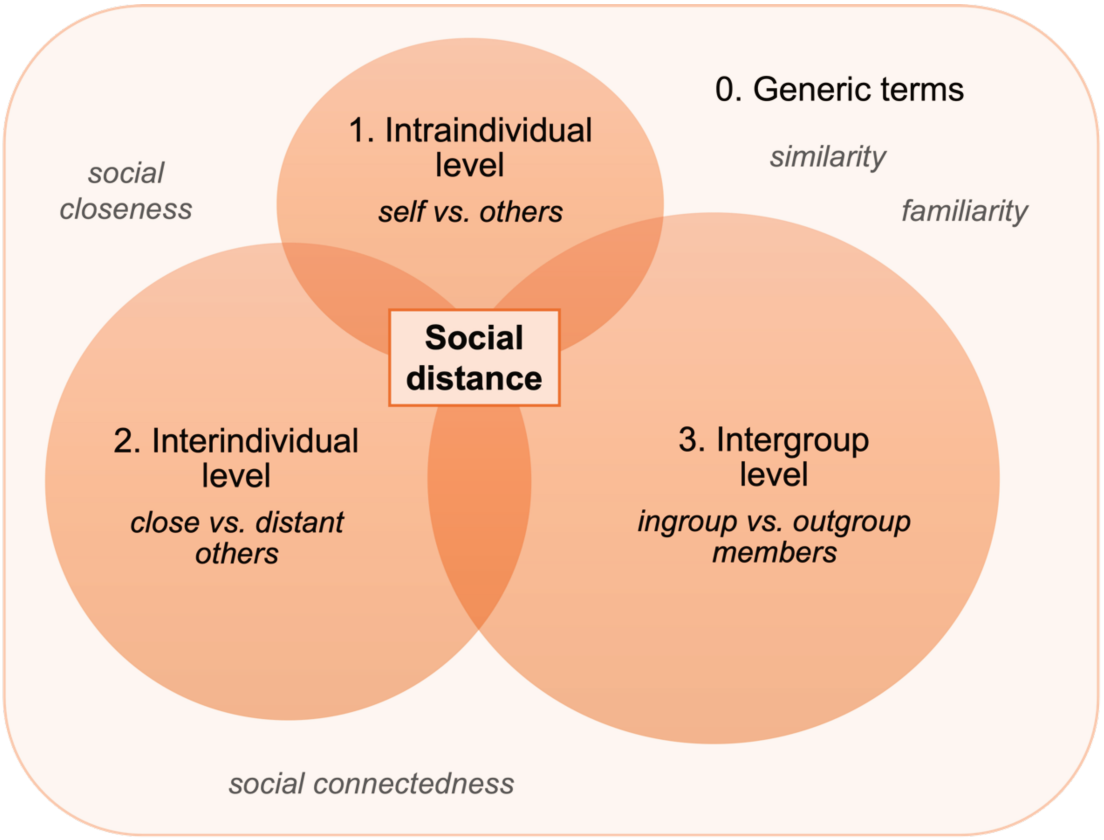
Regarding Moral Decision-Making

We can do the same effort of organization for the vast literature of moral decision-making—with less margin of flexibility as the state of the art is already well supplied. For morality, we decided to organize it via distinguishing two main dimensions identified in the literature (see Figure 3.2 for an illustration): The *bright side of morality* on the one hand which we will refer to in the following as the 'bright morality', and the *dark side of morality*—as called by Workman et al. (2020; see also Decety, 2023)—further referred as the 'dark morality'. The former comprises the moral psychology literature about prosocial and altruistic acts, starting from helping behaviors, donating to charities, but also cooperating in a task or protecting a vulnerable other. The latter refers on the contrary to moral transgressions and

antisocial acts, going from the mere unfair allocation of resources to illegal behaviors or even crime.

Figure 3.1

Social Distance Studied under a Psychosocial Threefold Analytical Framework



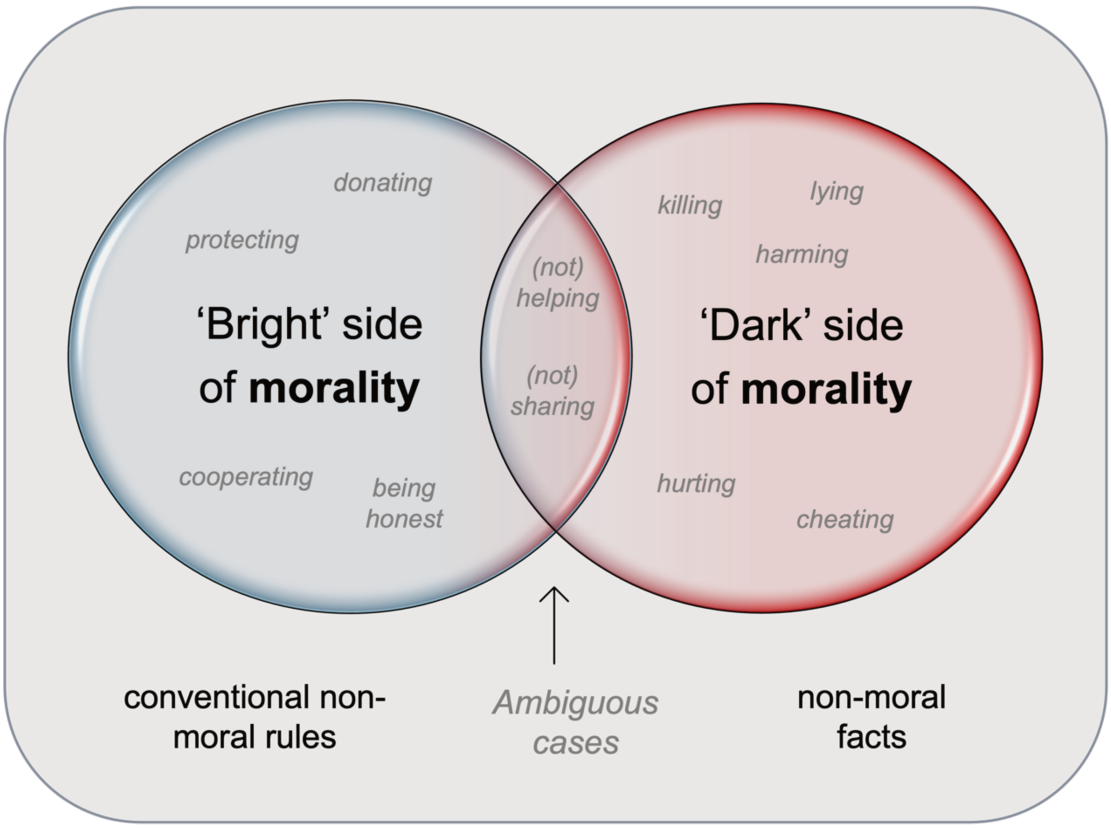
Note. A zero-level 'Generic terms' category has been added as the generic terms identified earlier do not necessarily match one analytical level more than the others. Words in grey are non-exhaustive examples of this category of terms.

We then understand morality for the current work in a larger sense than the strict *moral domain* as often conceptualized, that is, restricted to the field of norms that are not conventional but moral (see Chapter 55 of the *Atlas of Moral Psychology* for a discussion on the delimitations of the moral domain, Stich, 2018). Some authors as did Stich (2018) even argue for the non-existence of a *moral domain* per se and opt for a vision of morality as part of the bigger normative judgments' category. According to this view, we could not purely divide the normative judgments into two categories of the moral ones and the non-moral

ones. This perspective on morality inspired us to consider a large number of acts, judgments, or claims to be part of our definition of morality (e.g., both unfair and fair allocation or resources, egoistic vs. altruistic acts, charity behaviors as well as antisocial acts to give a few examples).

Figure 3.2

Morality Dichotomy under the Current Large Perspective



In a nutshell, considering these various perspectives regarding both social distance and morality conceptualizations does not necessarily yield consistent findings in terms of social distance effects in moral decision-making. This makes it quite difficult to compare findings about social distance effects and to get a good overview of the current state of the art about this research question. We therefore thought that a review conducted in a systematic way was necessary to better re-unify the various approaches on this topic, to gather and organize their results along the proposed threefold analytical framework in order to make them more commensurable. Only then should we be able to make some

recommendations on the definition to retain and on the implications for future research on this question, particularly in relation to findings on the descriptive directionality of these effects.

Previous Syntheses on Social Distance Influence in Morality

We did identify one recent systematic review on this research question from Mehdizadeh et al. (2021). Unfortunately, this review seems limited in its scope with regards to the key terms included in the literature search (i.e., taking mainly the second interindividual level of analysis and only using synonyms of 'interpersonal relationships' for social distance terms), thus covering partially one approach of the question but neither recovering nor synthesizing findings from the diverse perspectives described above. Moreover, this review has not been published in English, but in Persian, which limits access to its precise content, procedure, and, in the end, impact in the field. We also identified other systematic or synthetic reviews linked to our current question of interest but still not crossing the diverse approaches, as the one from Balliet et al. (2014) focusing on the intergroup level or the one from Jones (2021) within the interindividual level and altruistic side of morality (i.e., more specifically focusing on social discounting tasks).

As we wanted to map in this endeavor a large area of studies (crossing the various existing approaches) and as our aim was not for now to precisely estimate the size of the effect of social distance over all analytical levels, a systematic review or meta-analysis were here not the most relevant types of review. Instead, as we aimed at unifying the various approaches, organizing their findings along the threefold analytical framework, and examining (descriptively) the directionality of the effects at each level, to in the end have a stable basis for commensurability, a scoping review was the most appropriate form of systematic investigation. Indeed, according to Tricco et al. (2018), the scoping review may be a first good initiative to map the extent of knowledge about a specific research question (more so the case when diverse methodologies or terminologies are used), before wanting to get into a meta-analytical inspection of the size of a more specific effect. However, we do not exclude the possibility to conduct afterwards a meta-analysis or systematic review about

a more restricted area of the large map we want to currently build. We chose to mainly follow in the present scoping review the guidelines from Tricco et al. (2018) as well as the corresponding chapter of the JBI Manual for Evidence Synthesis (Peters et al., 2020) to provide a systematically reproducible and transparently reported review about our question of interest, also to fit the open science practices in the field of evidence syntheses.

Objectives of the Current Scoping Review

Although preliminary results are reported in the present chapter—as only a small part of the initial 8,873 outputs have been thoroughly analyzed (see the complete PRISMA flow chart in Figure 3.5 below)—, the current scoping review aims to answer the question of what is currently known about the influence of social distance in moral decision-making throughout the three levels of analysis depicted above. Importantly, we aim to identify key factors to consider when tackling this research question, as well as reviewing the overall direction for social distance effects[23], as previous research indicated ambiguous and sometimes opposite directions of results. As such, we first focus on summarizing and mapping research dealing with this question at all three levels of analysis and considering moral decision-making in a large sense (see above Figure 3.2 for our morality understanding). We also hope to identify this way contradictions in terms of definitions of concept, methods, or findings (when apparently inconsistent in terms of direction), and propose through this review a path toward the unification of knowledge on this question.

In the end, the identification of the main moderators found in the literature under the various analytical levels may allow to better understand the conditions for social distance opposite directional effects to appear. We also aim at identifying this way what are the gaps in the literature on this question (which subareas of the question or which moderators have been underexplored to date). This may furthermore give orientations about the sub-domains of this large review that would still require to further conduct a systematic review or meta-

[23] We do not intend through this scoping review to quantify the effects of social distance (as no critical appraisal and no risk of bias are usually conducted in scoping reviews), rather to get an overall idea of how many studies found effects in favor of Tendency 1 vs. Tendency 2 (as drawn in Chapter 2). This will therefore not allow to conclude about the veracity or robustness of this effect throughout the whole literature.

analysis in order to better understand more specific social distance effects. This endeavor would eventually result in general recommendations in terms of terminology unification and directions for future investigations.

Methodology of the Scoping Review

A Systematic and Preregistered Endeavor

In order to get a reliable overview on this question, we followed as closely as possible the guidelines from Tricco et al. (2018) for conducting scoping reviews in a systematic and transparently reported way. We notably used their PRISMA extension for Scoping Review (PRISMA ScR) as a checklist (Tricco et al., 2018). The goal of the current review was also to provide a reliable and easy to access overview of this question for future researchers interested in this topic to save time and resources to understand the main issues to consider when tackling this research question. In accordance with this idea and with open science practices, we provided a preregistered detailed protocol of the current scoping review following the template from Leibniz Institute for Psychology (Spitzer, 2022), available on the project page (https://osf.io/k3mjx/).

Population-Concept-Context (PCC) Criteria

Population

First of all, we did not focus for this scoping review on a specific population, as both research with adults and with children or adolescents could be informative to map the current research question. One restricting condition is however to include research dealing only with human participants (i.e., we did not include a source when it only contained simulated data, or when it only dealt with non-human individuals, such as animals or robots). Related, we only included sources where social distance was considered toward another specific human entity (e.g., we did not include research where social distance was considered toward a pet, an IA agent, or a nation or charity—without a specific human entity targeted).

Concept

As main concept for this scoping review, we considered obviously *social distance* manipulation or measurement corresponding to one of the three possible levels of analysis depicted in Figure 3.1, even when social distance per se was not the first focus or factor of interest. That is, either sources comparing several roles of the participant in the situation (first- vs. third-person perspective) or sources comparing a close vs. a distant target (be it at interindividual or intergroup levels). We also considered as to be included the sources where social distance was varying between two protagonists of the situation and the participant being considered as an independent observer (as an extension of the interindividual level). However, we did not consider as included in the current scoping review sources where social distance was only measured as a dependent variable (without being considered at least to some extent as a predictor).

Context

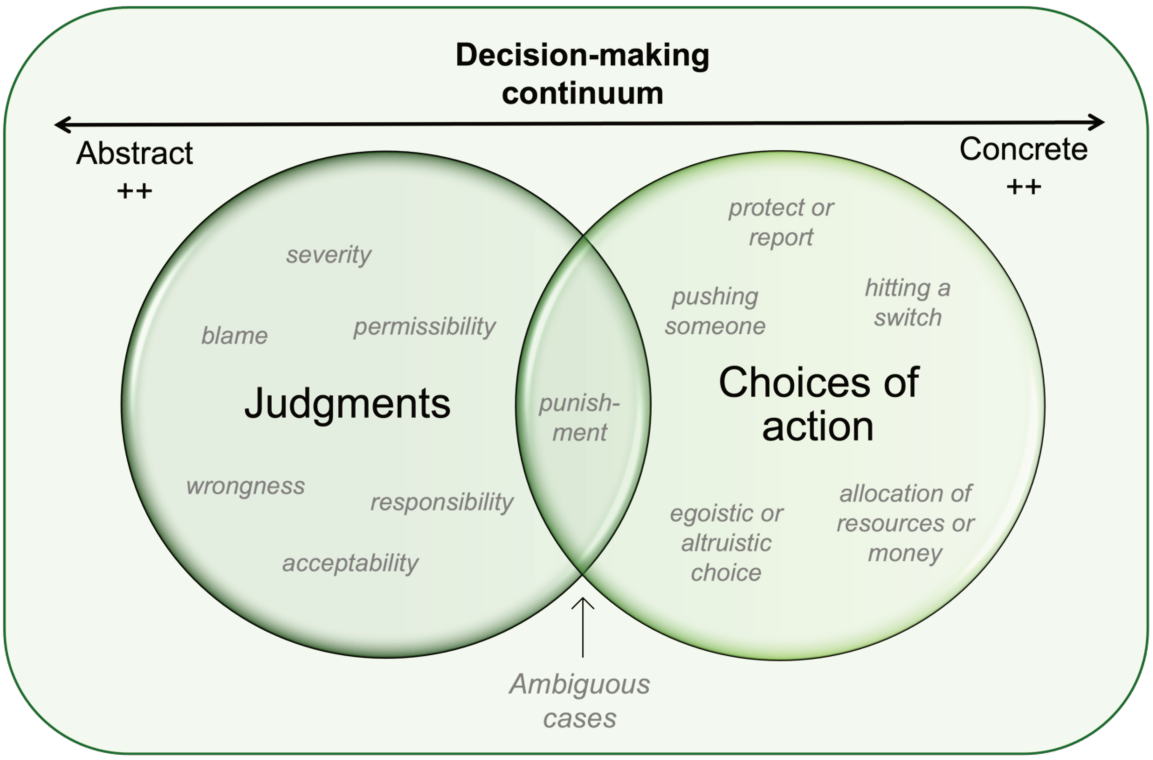
For the context of application of the current review, we consider only the sources that apply to *morality*, even if not the first and/or not the only focus or context of application of the source. As depicted above (cf. Figure 3.2), we consider morality for the current work in a large sense, including sources dealing with both ‘bright’ and ‘dark’ sides of morality. For some examples, we then included sources dealing with tasks of cooperation or allocation of resources; sources dealing with donating, helping behaviors or more generally altruistic and prosocial acts. On the other side, we did also include in this review the sources that involved moral transgressions, from the most severe ones (i.e., crimes) to the least severe ones (e.g., lying or hurting someone), but also egoistic or antisocial behaviors (e.g., social exclusion or discrimination).

The other context of application for the current scoping review is *decision making*. Indeed, we aimed at gathering all research in the literature dealing with the impact of social distance in moral situations, but more particularly when there is specific moral judgment or decision of action at stake. To this point, as described earlier in the theoretical chapters, we presently used the generic term of ‘decision making’ to refer in a large sense to the supra-

category, within which we included both *judgment* and *decision* or *choice of action* as sub-categories. On the one hand, judgment is considered as a mere positional or evaluative statement at one time and which most of the time does not trigger direct concrete implications. On the other hand, choice of action or decision is referring to a real choice that has (more or less) direct implications (see Figure 3.3 for an illustration). We could thus even consider decision making as a continuum when defined this way, with the left judgmental extremity considered as the most abstract type of decision making and the left decisional extremity considered as the most concrete type of decision making, as illustrated in Figure 3.3.

Figure 3.3

A Schematic Representation of Decision Making as a Continuum from Judgments to Choices of Action



Other Eligibility Criteria

As we wanted the current scoping review to be reproducible and transparently reported, we decided to select only the sources that could further be analyzed and checked or exploited by other researchers. We then added as a criterion for selection the language of

the document, that is, only those written in English. Regarding the method used in the retrieved sources, we did not put any restrictive criterion for inclusion as we aimed to gather all existing research on this topic, hence inclusion no matter it is a qualitative, a quantitative, or a mixed method.[24] The same criterion applies for the type and status of publication, we did not focus especially and only on experimental studies published in scientific peer-reviewed journals, but aimed to also gather book chapters, evidence syntheses, theses, unpublished manuscripts (i.e., what is called the 'grey literature'), except for retracted papers, that will obviously not be integrated in the current review. No restriction was applied either regarding the year of publication (the limit being what is computerized in the actual databases we used for the search, see next section).

Searching Strategy

For the search strategy, we followed the guidelines from the University of Montreal library (online website) and reported the complete and detailed procedure of the search in an Excel file available on the OSF project page. We chose as databases for the search to be executed the following ones: *PsycInfo, PsycArticles, Psychology and Behavioral Sciences Collections (PBSC), Web of Science (Core collection),* and *PubPsych*[25]. For grey literature specifically, we questioned *Proquest, ArXiv,* and *Semantic Scholar.* Finally, we also used *Google scholar* as a check that all important sources on the topic (even nonpublished ones) had been retrieved, taking the first 20 outcomes of each central key terms combination (then retrieved thanks to the *Publish or Perish* software, Harzing, 2016).

The search query has been built first for *PsycInfo* following general guidelines from the JBI Manual for scoping reviews (Chapter 11, Peters et al., 2020), combined with a more practical guideline from University of Montreal library resources[26]. The search query was then translated to fit the other various databases. Results for each database were lastly

[24] Note however that this criterion has been revised in the a posteriori restrictive criteria, due notably to the large number of potentially relevant outputs obtained (see 'Results' section below).
[25] PsycInfo, PsycArticles, PBSC were reached through the EBSCOhost server. Other online databases were reached via their own platform.
[26] Made available on the University of Montreal library: https://bib.umontreal.ca/evaluer-analyser-rediger/syntheses-connaissances/examen-portee

exported on the 27th of August 2024. Please find in Table 3.1 below the summary of the number of sources for each database with the date of last execution and the exclusion filters applied if any. The whole process for search query construction and search execution is detailed in the excel sheet on the OSF page of the project (section Method).

Table 3.1

Number of Sources for each Database, Date of Last Execution, and Filters Applied

Main databases	Date of execution	Number of outputs	Filters applied*
PsycInfo	08/27/2024	2624	Language = English; Population = NOT animal; Type of publ. = NOT book
PsycArticles	05/28/2024	309	Language = English; Population = NOT animal
PBSC	05/29/2024	548	Language = English
PubPsych	05/29/2024	507	Language = English
Web of Science	08/27/2024	2946	Language = English; Type of publ. = NOT book or book chapter or editorial material
Grey databases	**Date of execution**	**Number of outputs**	**Filters applied***
ProQuest	08/27/2024	723	Language = English; Type of publ. = NOT book
ArXiv	05/24/2024	166	NA
Google Scholar	05/23/2024	600**	NA
Semantic Scholar	05/23/2024	450**	NA
Total number of results		8873	*(before deduplication)*

*Filters for exclusion of specific non relevant sources (e.g., research on animals) were not available on all databases.

**Number of results for Google Scholar and Semantic Scholar were determined by taking the first most relevant sources as classified by the online search algorithm.

Note. Publ. = publication; PBSC = *Psychology and Behavioral Sciences Collections;* NA = non-available. Book (or chapters) were sometimes excluded on some databases because bringing too many results with probably low relevance as not the first focus of the current scoping review.

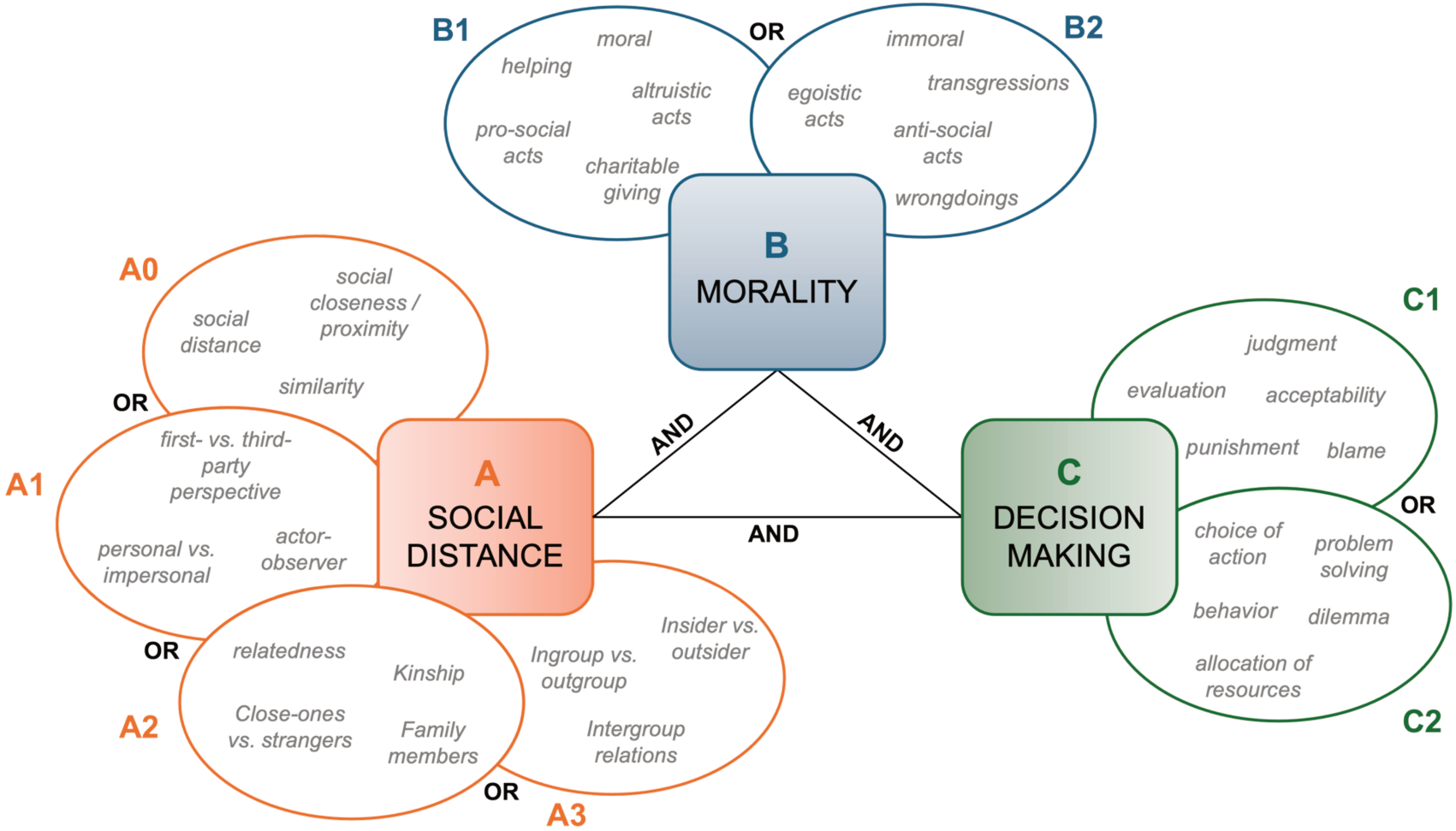
Also find in Figure 3.4 below a synthetic view of the research query construction logic and the various concepts (and subsets of these concepts) used in this review. Consistently with what has been previously explained, we listed each possible relevant synonyms for social distance (concept A) under the three levels of analysis as depicted above—A1, A2, A3—(i.e., generic terms have been added as a another subset A0), and we did the same thing for both morality (B) and decision-making (C) concepts (within two subcategories each, 'bright'—B1—and 'dark'—B2—morality, judgment—C1—and choice of action or decision—C2—respectively). Some examples for each concept subset have been added in Figure 3.4. Once these terms were listed, we combined these terms with Boolean operators in order to get at least one occurrence of each main concept (A and B and C) but under one of the subsets for each concept (e.g., B1 or B2 for morality). This gave us the following query logic:

(A1 **OR** A2 **OR** A3) **AND** (B1 **OR** B2) **AND** (C1 **OR** C2)

We then applied this logic to search for these key terms within titles, abstract, and keywords, also using controlled vocabulary (i.e., determinants or subjects or subheadings depending on the platform) when available in the sources database.

Figure 3.4

Synthetic View of the Three Main Concepts Plan for the Current Scoping Review



Note. Terms in grey are examples of key terms included for each concept and sub concept, not reported exhaustively here.

During the search, we also applied some filters[27] to already diminish the number of potentially relevant sources to screen for inclusion in the next step (see Table 3.1). We then applied when it was possible filters on the language (i.e., English only) and on the population (i.e., human only)[28]. For the two main databases (i.e., PsycInfo and Web of Science), we also decided in the end not to include the books as type of publication, to reduce the number of sources to export, as we considered that even though it could be interesting for a theoretical perspective, they were not the most directly relevant for our purpose. Finally, to search on grey literature databases, and as the main part of them do not allow to implement a complex query structure as the one described above, we slightly adapted the procedure by decomposing the whole query into three main queries corresponding to the main social distance subdimensions and by selecting only a few terms for both moral and decision-making concepts (the most central in our opinion). For each of these subqueries, we then only retrieved the first outputs of the search (see Table 3.1 for a summary)[29].

Deduplication and Screening Stages

We used the online software *Rayyan* (Ouzzani et al., 2016) to gather, deduplicate, and then screen all the sources to decide which ones are to be included in the current scoping review. We started with a quite large amount of sources collected throughout the diverse selected databases, that is, 8,873 documents. After both automatic and manual deduplication thanks to Rayyan functionalities, this number felt to 6,911 sources to be screened. In order to ensure a good inter-judge reliability, four independent judges (one main reviewer with 100% of the whole sources to screen, the three others with 1/3 of the

[27] Note that filters could not always be applied depending on the database. We executed the searches applying as much filters as possible (to optimize the number of sources to screen afterwards), but filters were not always available on all database (see Table 3.1 for the filters used per database).

[28] We more precisely filtered for 'NOT animal' as population when possible during the search, that is not searching for research dealing with animals. In fact, adding the (more direct) filter of 'human' as population was at the same time excluding sources that were not labelled as research toward human, despite clearly dealing with a human population (hence a too high number of potentially relevant sources that would have been missed this way).

[29] This is true only for Google Scholar and Semantic Scholar, on which it is not directly possible to retrieve a whole set of sources (i.e., without extracting manually one by one each reference). We used the software *Publish or Perish* (Harzing, 2007) to do this automatically.

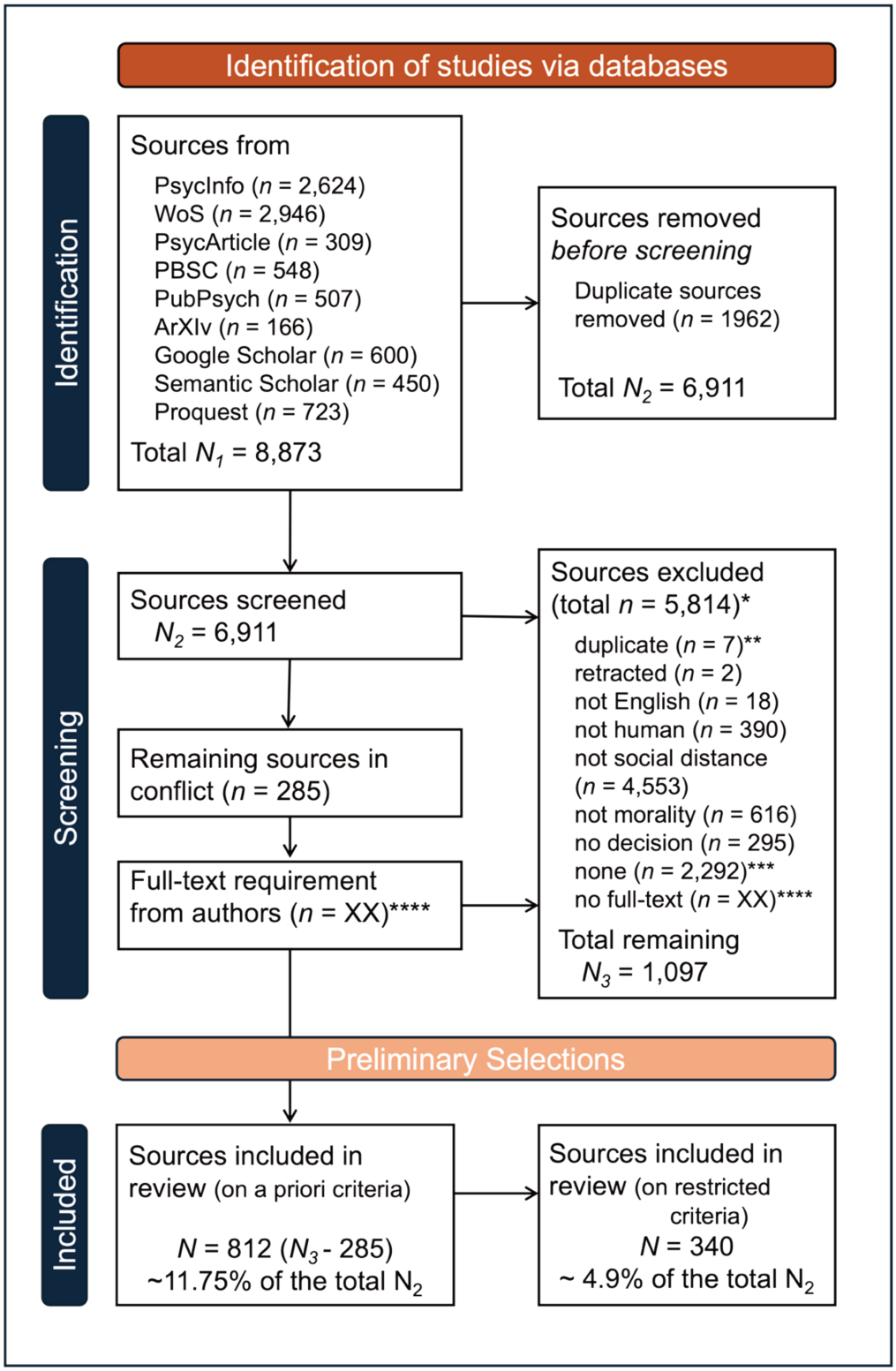
sources to screen) went over all the sources with blind mode on, allowing this way to minimize inattention errors by crossing the two independent decisions for each source. Conflicts and uncertain cases were discussed together to be resolved or handled with the help of a fifth independent reviewer of our research team (see in the protocol on the OSF page of the project for more information about how we conducted these steps).

We summarized this selection procedure in Figure 3.5 via an adapted PRISMA flowchart (Page et al., 2021) of the scoping review. For each included source, we added some labels to pre-categorize the sources within our three-level framework of social distance (and/or in 'review' or 'theoretical' labels if the case) to make the later selection of specific sources subsamples easier. For each excluded source, we indicated the reason for exclusion (e.g., 'not in morality'; 'no social distance' etc.). You can find the details of all reasons for exclusion, as well as the number of sources in each step in the flow chart in Figure 3.5. For included sources where full text was not directly available online, we planned to reach the first author via e-mail and without any answer within a three-week delay, to exclude the source.[30]

[30]As the process of selection is still ongoing, we did not already deal with these cases.

Figure 3.5

PRISMA Flow Chart for the Current Scoping Review Preliminary Selections



Note. *Number of total exclusions does not necessarily equal the sum of each reason's *n*, as for some sources, plural reasons were given depending on the independent reviewers;

**Duplicate sources at the selection process are those which were not identified beforehand (e.g., when thesis and published paper); ***The 'none' reason for exclusion was chosen by reviewers to mean that at least two criteria on main concepts (social distance, morality, decision making) were not met; ****The 'no full-text' reason for exclusion has not yet been completed (i.e., waiting for the conflicts to be solved beforehand).

Data Extraction

We planned to extract for all final sources included in the scoping review firstly some basic metadata, such as title, authors, date, journal of publication as well as basic information about the main concepts of inclusion (i.e., social distance, morality, decision making). Second, when sources are not a theoretical source or a synthetic review, we also retrieved as much as possible information about the method and how the diverse concepts were conceptualized and operationalized (i.e., what is the role of the participant in the situation, toward whom the distance is manipulated or measured, what is the judgment or decision at stake etc.), as well as the overall direction for the effect (both for main and simple effects of social distance only).

The extraction of the data from included sources was planned to be done by two independent coders, to ensure a more reliable extraction process. Conflicts and inconsistencies would again be resolved by discussion between all reviewers.[31] The extraction form (with all information to be retrieved for each source) was adjusted quite a few times since registration, as it is supposed to be an iterative process (Pollock et al., 2021). You can find the codebook with the whole set of items to be retrieved and their definition on the OSF page of the project (cf. section Method). Also notice that some items are supposed to be necessary (i.e., to be filled for each source, or NA if non available) whereas others are only facultative (i.e., necessary items indicated via an asterisk).

[31] Note that for the currently reported preliminary results below, data have been extracted by only one coder. We however plan for the final analysis and report to check—at least on part of the sources to be coded—via a second rater the reliability and consistency of information extraction.

Analytical Strategy

We used an adapted PRISMA flow chart to represent the steps of selection and exclusion using the template made available by Page et al. (2021) (for PRISMA, see Figure 3.5, currently in an intermediate process of selection). After having coded each source within the extraction form detailed above, we ran some descriptive analyses in R programming language (version 4.2.2, R Core Team, 2021), using mainly frequencies (as the aim is again not to test inferentially the observed differences in effects) (see analytical script in the OSF page). We then charted the diverse relevant findings through various types of graphs to represent our data in the most appropriate way (following general recommendations by Pollock et al., 2023). We especially analyzed plural dimensions of interest for this review:

1. First about methodological aspects, mapping the types of publication and the types of method used in the selected sources to inform the review in terms of their importance. We also wanted to represent the type of population to check how many studies are conducted among adults vs. children.
2. Second, in terms of concepts related data, we planned to map the diversity of social distance terms and manipulation, as well as a graphic visualization of how studies are distributed across the three levels of analysis presented above. We will do the same for the morality concept and decision-making concept.
3. In the same way, we wanted to compute the proportion of studies studying social distance effects when manipulated toward the victim or recipient or agent of the situation at stake, as it seemed to us to be one of the most fundamental criteria to analyze social distance effects. We also planned to get a representative view of the directions of social distance effects (either the Tendency 1 or Tendency 2) along the different levels of analysis and/or along the subcategories defined above (prosocial vs. antisocial morality; distance toward the victim or the agent etc.)
4. As a secondary aim, we also wanted to index the diversity of materials used in the studies (proportion of realistic and/or plausible situations, severity of the situations

etc.), as well as the heterogeneity in the number and variety of situations encountered.

5. Finally, we planned to analyze the moderators identified throughout all studies (while focusing notably on those that were found as significantly influencing social distance effects), as well as their respective importance. We may also explore other types of potential moderators among data presently being extracted from the sources, even if not considered as such explicitly within the sources.

Scoping Review Preliminary Findings

Rationale for a Truncated Version of this Larger Review

The scope of the current review is larger than what had been previously done in terms of synthetic and/or systematic reviews, because of the large and three-level analytical framework considered (and in order to map all existing research in an integrative way). However, we did not anticipate such a large number of potentially relevant sources to include. Indeed, following the a priori defined criteria for selection (as stated in the 'Method' section above), we gathered more than 8,000 sources during the search process. Once manually screened to decide about their relevance for the current scoping review, we obtained around 800 sources to be included[32]. This sample of 800 relevant sources then needed to be thoroughly screened to extract all necessary and detailed information (see again the complete codebook of extracted information in OSF). Given that the estimated time required to retrieve all this detailed information from 800 sources (which may each include 2 or 3 studies on average) exceeded available time but also the aims of the present dissertation, we decided to focus on a restricted sample of these 800 sources.

To get the most relevant information from this scoping review given the present dissertation's aims and empirical focus (see details in Chapters 5 to 8), we decided to restrict the review to some specific criteria. We subsequently pre-coded each of the ~800 included sources in terms of these criteria (especially population and type of morality), so that the

[32] This is an approximation as some sources still need decision to be reached (either reflecting cases of conflict between independent reviewers and/or full text not being available).

analyses of this scoping review focused on a restricted sample of this larger scoping review. These pre-coded criteria were as follows. First, regarding type of population, as our experimental studies focused on adults from the general population (and does not empirically investigate morality in children or moral development), we retained only research dealing with adult participants. Second, as our experimental studies essentially deal with social distance effects in everyday moral transgressions, we chose to restrict our focus to the *dark side* of morality (cf. Figure 3.2). Finally, as the aim of the current truncated scoping review is to identify the proportion of social distance effects in direction of Tendency 1, Tendency 2, both, or no clear direction, along with their relevant moderators, we retained only experimental, quasi-experimental, and correlational studies (but not descriptive or qualitative research, synthetic reviews, or theoretical sources). Within these restrictive criteria, we estimated the total number of sources to code and analyze to be around 330, which is still a non-negligible amount of work.

Preliminary Findings

As noticed earlier, we only have for now preliminary findings to present regarding this scoping review. Indeed, out of the estimated final sample of 340 sources to include, we only report below results from 16 different sources (or articles as only consisting of primary research). This represents overall a total of $N = 38$ studies (with only studies that are relevant to the topic within a single source being coded). You can find the complete analytical script and data extracted from the various sources on the online OSF page (section 'Analysis & Report').

Verification on Restriction Criteria

First of all, we started by checking on the retrieved information whether we were indeed sticking to the restricted criteria fixed a posteriori, in order to constrain the scope of the current report. The sources were pre-classified in terms of population (i.e., adults vs. children) and type of morality (i.e., bright vs. dark morality), then selected on that basis to be coded (and then pre-selected on that basis). Therefore, this is more about a verification step than a relevant analysis per se. However, for indicative purposes, we also reported below

the descriptive frequences on these a posteriori criteria compared to the whole database of included sources (even if not the final one, ~812 sources).

Population. First regarding the population targeted, we aimed as a primary goal to review all studies with an adult population, hence filtering all research dealing with children or adolescents (i.e., defined as less than 18 years old). On the 38 studies from which we extracted information, all were involving adult participants, hence checking this criterion. For descriptive purposes however, and as we pre-coded this criterion among all included sources, research tackling social distance influence in moral decision-making among adult participants represented a total sample of 665 out of 812 sources (~ 82%). The remaining sources were either dealing with children or adolescents (~ 14%) or with both children and adults (~ 4%).

Type of Morality. Regarding the type of morality involved, we aimed first to target research dealing with antisocial acts (or as called earlier the dark morality realm) as it is the domain of interest for the current dissertation. On the 38 studies analyzed, 35 were classified as such, while the three remaining were classified as involving both antisocial and prosocial aspects, hence also fitting the a priori fixed criterion on the type of morality involved.[33] When looking at the proportion of sources including antisocial vs. prosocial morality on the whole dataset of inclusions, we found that a majority of sources were about some prosocial dimension of morality (421 out of 812, ~ 51.8%). Sources dealing with antisocial morality uniquely represented around 32.3% of all the sources included (with 15.9% of sources involving both antisocial and prosocial morality).

Type of Method. We also aimed in a first instance to gather and analyze studies with experimental (or quasi-experimental/correlational) data. As this information was not always easy to pre-classified based on the abstract only, they were not systematically pre-classified on the whole sample, hence also a possible overestimation of the sources pre-included in the restricted sample. On the 38 studies coded (as one source may contain plural relevant

[33] We were indeed quite flexible in what is considered as purely antisocial (dark morality) vs. what is purely from prosocial (bright morality) area, due to the porosity between both concept dimensions.

studies), we obtained in the end three studies with a correlation design (at least on the dimension of social distance being of interest in the current review), with five others having a mixed design. The others were all following an experimental or quasi-experimental design. Moreover, we wanted to target studies that firstly involved the agent as the target of decision in the moral situation at play (and not the victim for instance). By descriptive frequencies, we noticed 31 studies were in this case, whereas two were involving a decision toward the recipient of the act (not directly reflecting the victim of the action) and five others could not be easily classified within this classification (i.e., no direct target of the decision).

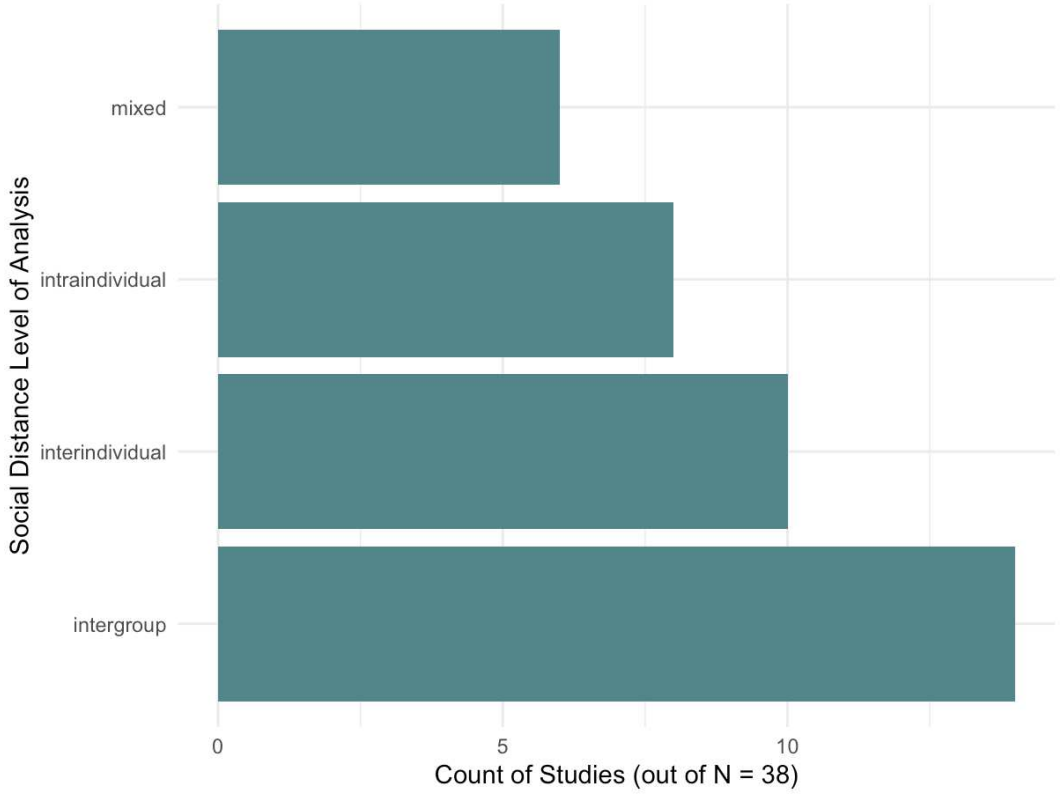
Main Findings about Social Distance Concept

Social Distance Analytical Levels. Following the same three-level framework as previously developed, we first reviewed the analytical level involved for each study. We observed via frequencies that many studies involved a manipulation of social distance at intergroup level (*N* = 14), followed by those at interindividual level (*N* = 10), then those at intraindividual level (*N* = 8). The remaining ones were studies involving at least two levels of analysis (*N* = 6), for instance both the intraindividual and intergroup levels, or the other possible combinations (see Figure 3.6 below). We can already notice descriptively that using this threefold framework of analysis to the study of social distance influence in morality is relevant, as soon as we get a good proportion of studies within each level of analysis.[34] As a descriptive complementary analysis, we looked at the proportion of each analytical level representation within the whole sample of included sources (possible because it was also an information we pre-coded during selection process). We found consistently a good proportion of sources tackling effects of social distance at the intergroup level (*N* = 358, ~ 44%, without considering those involving mixed levels of analysis). Interindividual level was the second most represented level (*N* = 210, ~ 25.9%), then followed by intraindividual level (*N* = 87, ~ 10.7%), which was however often involved in a mixed framework of analysis (i.e., often coupled with an analysis of social distance at the interindividual or intergroup level).

[34] Even though the sources that have been coded for now have not been chosen fully randomly. We indeed in priority coded those with which we were subjectively the most familiar.

Figure 3.6

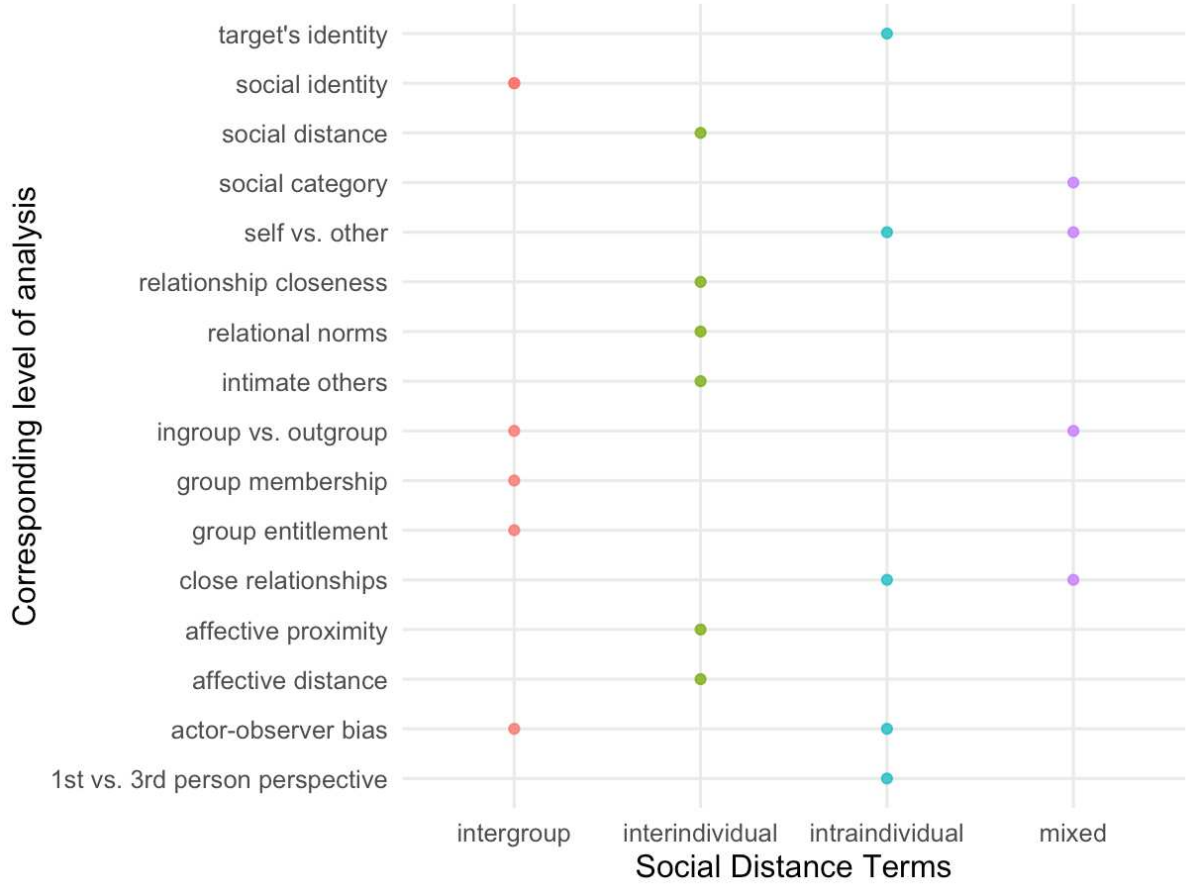
Number of Studies Depending on Levels of Analysis for Social Distance



Social Distance Terms. Regarding social distance specifically, we extracted from all sources the terms that were used to describe or refer to social distance (be it in a direct or more indirect way). We found—consistently with what has been seen earlier in Chapter 1—a large diversity of terms referring either to generic terms (as for social distance) or to dimensions that can be explicitly linked to the three levels of analysis. As such, we visually displayed in Figure 3.7 these various terms depending on the level of analysis of the study they were issued from. These results remain however quite qualitatively limited in scope (due to the limited number of sources being here represented). This will be much more robust when all sources will have been categorized.

Figure 3.7

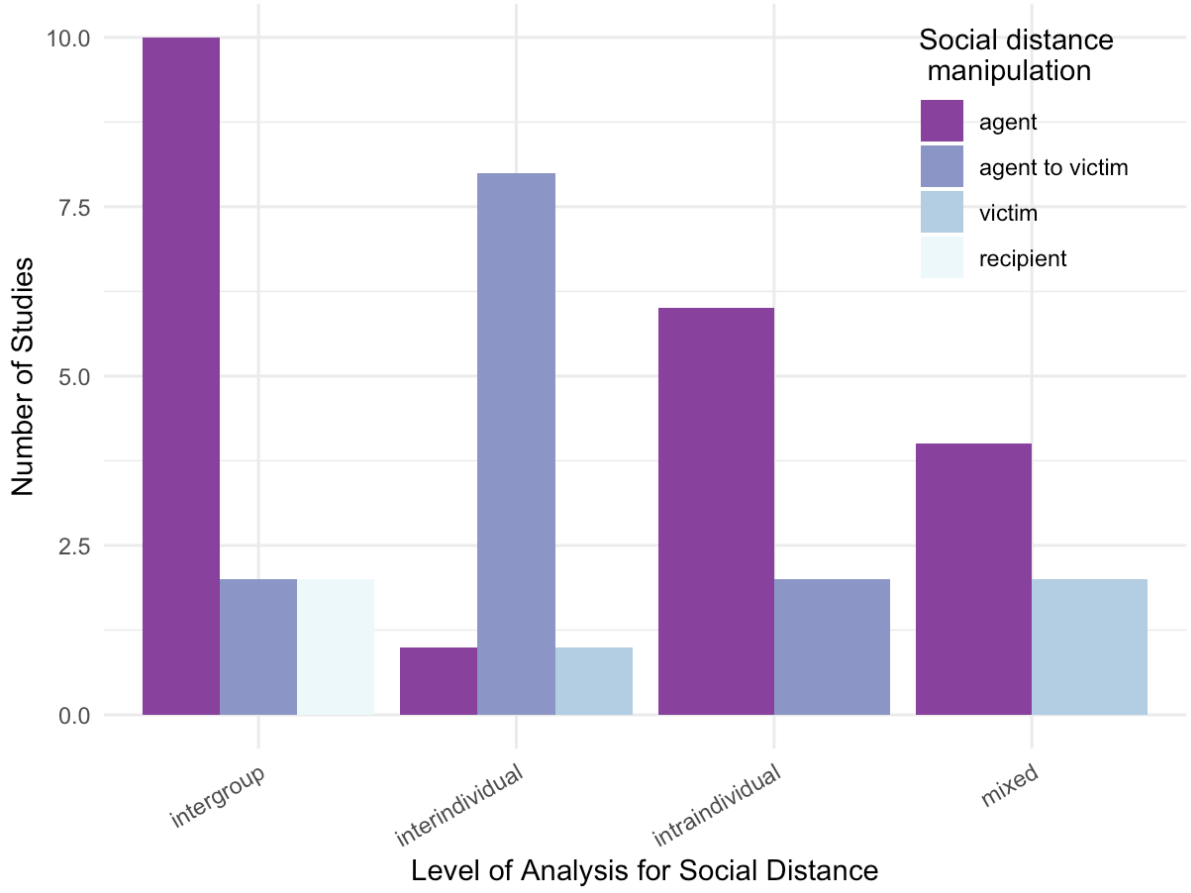
Social Distance Terms Depending on Levels of Analysis for Social Distance



Social Distance Manipulation Target. Regarding social distance manipulation, the main part of studies from this reduced sample of preliminary analyses tackled the effects of social distance when manipulated toward the agent of the moral transgression at stake (*N* = 21, ~ 55%). Another substantial series of studies (*N* = 12, ~ 32%) made social distance varying from the agent to the victim (independently from participants), with the remaining ones dealing with a manipulation of social distance toward the victim or the recipient of the wrongful act (*N* = 5, ~ 13%) (see also Figure 3.8 for how social distant targets of manipulation were displayed depending on the three levels of analysis for social distance).

Figure 3.8

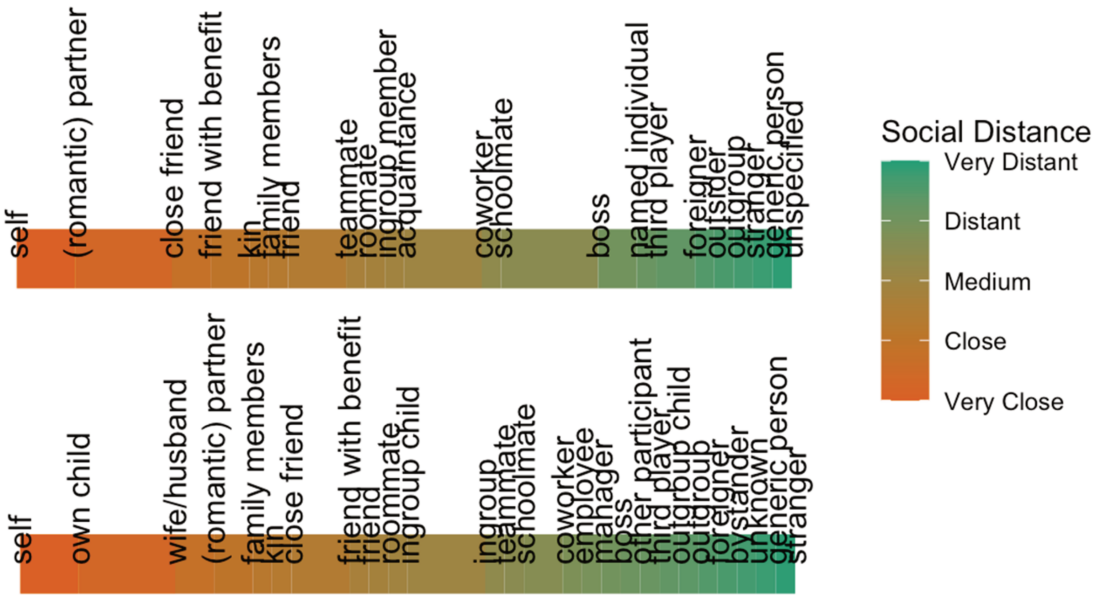
Number of Studies for each Type of Social Distance Manipulation Target Depending on Levels of Analysis



Agents' and Victims' Identities. To look more closely at how social distance was operationalized in concrete terms, we extracted from each study the identities of the agents and victims (when relevant). We grouped some identities that were very close together within only one term when relevant (as for family member, originally comprising mother, father, siblings etc.). As an illustrative purpose, we then categorized them naively from closest to the more distant category and displayed them on a visual continuum of social distance (see Figure 3.9).

Figure 3.9

Agents' (Top) and Victims' (Bottom) Identities on Social Distance Continuum



Note. On the upper continuum of the figure, the different identities of the agent are displayed, as retrieved from the various studies and classified by social distance (from 'very close' on left side to 'very distant' on right side), whereas at the bottom is represented social distance continuum toward victims' or recipients' identities.

Findings about Social Distance Effects' Direction

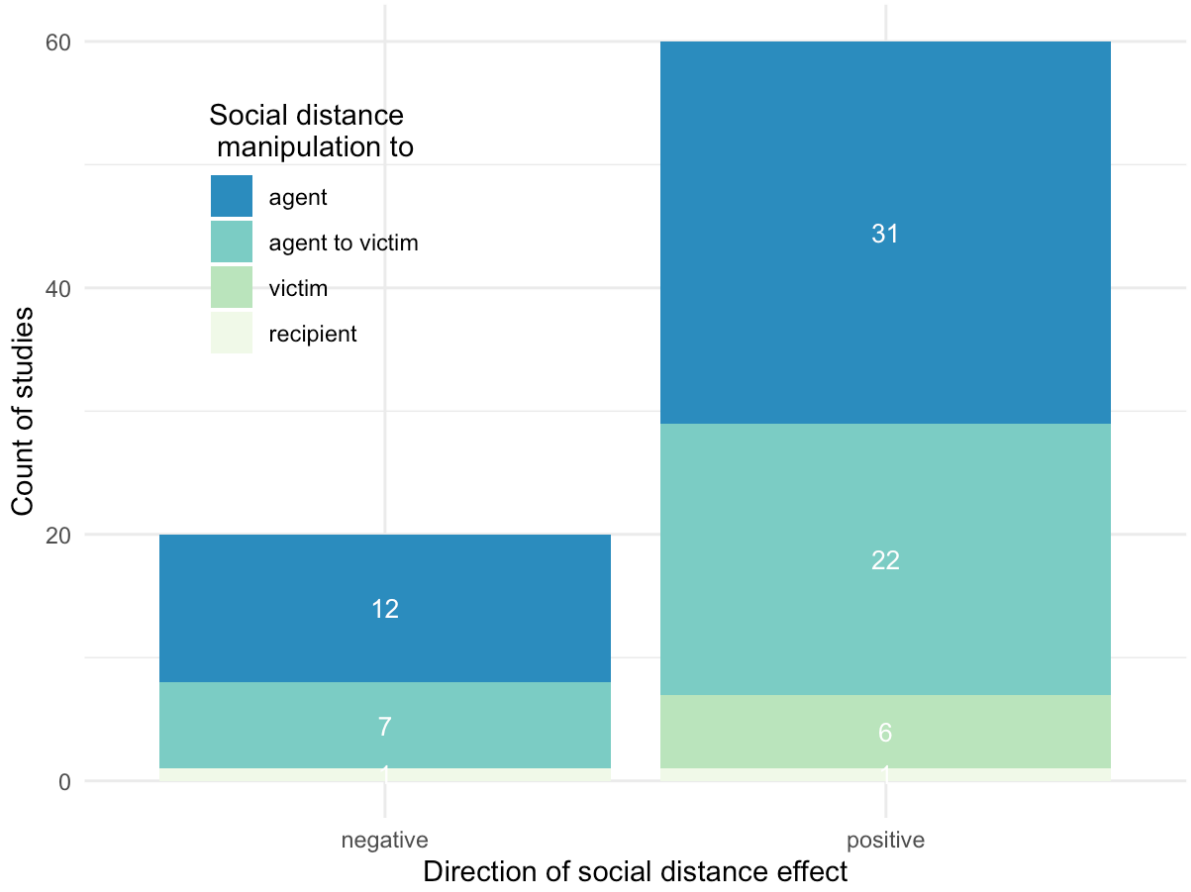
This section aims at reviewing the descriptive direction of social distance effects on this preliminary data of the scoping review. Note that the scoping review itself does not have as main purpose to assess the existence of an effect (or the relevance of an intervention), nor to quantify the size of an effect (as no critical appraisal and no risk of bias was made on the collected sources—which is necessary in a meta-analysis for instance to conclude about an effect). We however decided to still extract relevant statistics available in the source, with the goal to exploit them in the future for a potential meta-analysis on a more specific question of research (e.g., social distance effect in antisocial acts and for adults at intraindividual level only). This was also a helpful step to infer and code the direction of these effects.

Direction of social distance effects (be it main or simple effects) was first coded as positive when close ones were judged more leniently than distant others (in accordance with Tendency 1). It was on the contrary coded as negative when the reverse pattern of results appeared (Tendency 2), or as 'null' if the effect was found as statistically non-significant as reported by the authors. We however also provided—when possible—a second coding of the direction of social distance effects, in order to get a descriptive direction of it even when the effect was statistically non-significant (i.e., coding the previous null effects as either positive or negative based on means difference or effect sizes). Note also that the previous interpretations of effects' direction hold for both social distance being manipulated toward the agent and toward the victim of the situation. In such a latter case, we consistently coded as positive an effect where the act is judged more harshly when the victim is a close one (or self) compared to a distant other. Reversely, the direction was coded as negative when the act was judged more harshly when directed against a distant victim compared to a close one (or oneself).

Altogether, we found 69% of descriptively (hence not necessarily being all significant statistically) positive effects (N = 60), consistently with what has been previously described as the main tendency in literature. There were however quite a few effects coded as negative in terms of social distance direction (23%, N = 20), the remaining ones corresponding to non-available data (~ 8%, N = 7) (that is, effects where no statistics were reported, even no descriptive means). As social distance effects' direction may crucially depend on the target with whom social distance was manipulated, we observed the frequencies of effects' direction per target of social distance manipulation. We displayed in Figure 3.10 these effects' directions depending on social distance manipulation target: either social distance from the participant to the agent, or to the victim/recipient, or social distance from the agent to the victim (see Figure 3.10).

Figure 3.10

Counts of Effects' Direction (Positive vs. Negative) Depending on the Target of Social Distance Manipulation



Through this categorization, we found for effects of social distance to the agent a descriptively higher proportion of positive effects (~ 67%, *N* = 31, 2 *NA* values) compared to negative effects (~ 26%, *N* = 12), hence in adequation with the previous general tendency. Regarding effects of social distance when manipulated toward the victim or recipient of the immoral act, we found almost only positive effects (*N* = 7 in total, with 2 *NA* values and only one negative effect for recipient), consistent again with broader findings of literature. We should however acknowledge the low number of effects having currently being coded for social distance toward the victim/recipient (*N* = 8), coming mainly from three different studies (comprising multiple analyses), hence this conclusion requiring to be further amended depending on results on the whole database of included sources. Finally, when social

distance was manipulated from the agent to the victim, we found a majority of positive effects (~ 71%, *N* = 22), compared to negative ones (~ 23%, *N* = 7, with 2 *NA* values), consistent with the direction of social distance effects to the agent (see Figure 3.10).

Materials and Contextual Information

Beyond the mere effects of social distance in morality, we were also interested in what type of moral situations were mostly representative of this literature. With this aim, we also extracted from the various sources information about the materials used (especially in terms of severity, plausibility, and realism of the situations at play), but also about the experimental design and number of different situations people were generally confronted to. We finally retrieve the factors being tested as potential moderators of social distance effect, with their global significance (even if again, this was not the first aim of the current scoping review to conclude about the veracity of these influences).

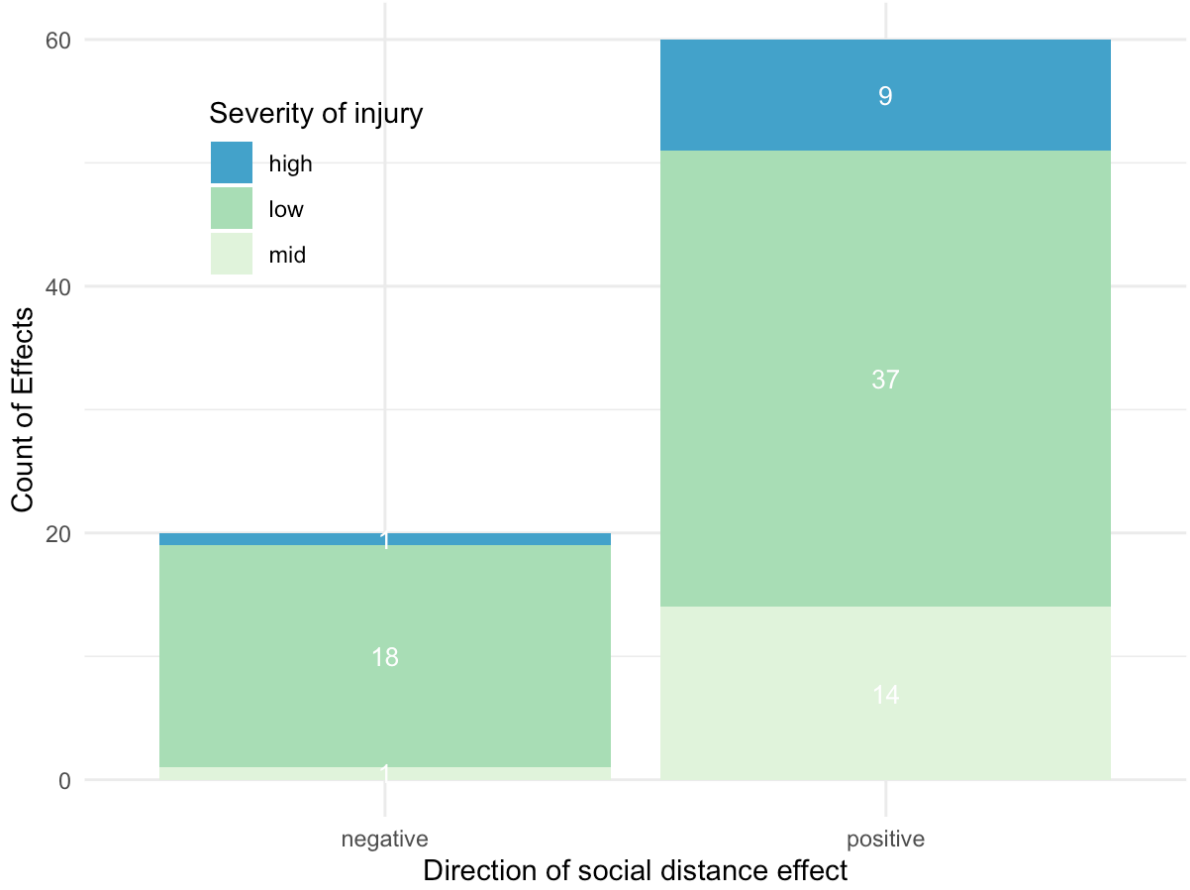
Material Severity. Regarding materials' severity first, most studies (*N* = 20) comprised moral situations with a low level of severity, that is, involving only low severe consequences for the victim. The other studies were coded as involving either mid-severe (*N* = 11) or high severe (*N* = 7) consequences. In particular, high severe situations (i.e., involving irreversible damages for the victim) mainly reflect studies using trolley like dilemmas as materials, whereas low severe situations were mainly represented by everyday-like moral scenarios.

When looking descriptively at the direction of social distance effects depending on situation's severity, we found, consistently with previous results, a higher number of studies with positive effects of social distance compared to negative effects within all levels of severity (see Figure 3.11). Although the number of studies was not identical within each severity category (low, mid, high), thereby limiting the scope of this conclusion, we did not observe an overrepresentation of positive effects for instance in low severe situations (~ 66% vs. ~ 70% and ~ 82% for mid and high severe cases, respectively). Similarly, we did not observer an overrepresentation of negative effects in high severe scenarios either (~ 9% vs. ~ 5% and ~ 32% for mid and low severe cases respectively). It seems—given these

descriptive frequencies—even going better in the opposite tendency (i.e., more negative effects for low severe situations). However, these interpretations are again biased by the limited number of data being compared, hence the necessity to confirm these tendencies on the final whole data analysis. Moreover, an absence of clear pattern in effects' direction reversal depending on levels of severity does not mean severity is not impactful for social distance study. We may indeed observe differentiated effects of social distance within low compared to high severe situations, without observing a reversal of the effect. This therefore requires further investigation.

Figure 3.11

Counts of Social Distance Effects' Direction Depending on the Severity of Injury



Material Realism and Plausibility. We then looked at the classification of studies in terms of materials' realism (i.e., how much it seems realistic from participants' point of view) and plausibility (i.e., how much the depicted situation would be likely to occur in real life). As

such, studies were coded as low on realism when involving materials where the participants could easily infer that they were not directly experiencing the situation and that their action or decision would have almost no consequence in real life. This was typically how we coded materials involving hypothetical scenarios, that have only low ecological validity compared to conflicts experienced in real life or virtual reality settings (i.e., considered as high and mid-realism respectively). As for plausibility, it was coded as high when the situation—even if low in realism—entailed an everyday-like action (i.e., that could have been easily encountered by the participant in real life, beyond the experimental setting). Prototypical examples of high plausibility materials are those involving everyday moral scenarios for instance, whereas trolley-like dilemmas would reflect prototypical instances of low plausible situations (i.e., one is rarely confronted in real life to the dilemma of deviating a trolley to save five people by killing one).

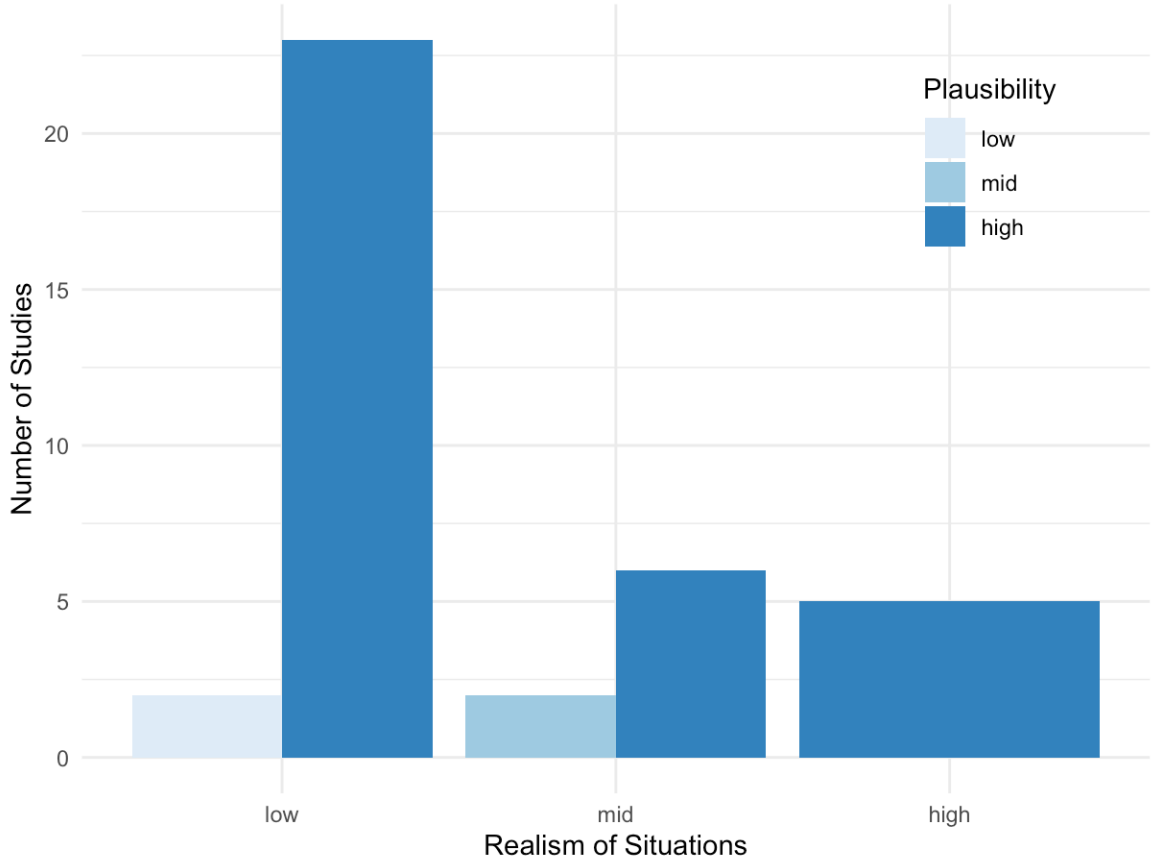
Crossing realism and plausibility criteria, we found that almost all studies involved high plausible (N = 34, ~ 89%) but low realistic (N = 25, ~ 66%) situations. These situations mostly corresponded to everyday-like situations. On the contrary, low plausible situations were mostly reflecting trolley-like dilemmas. Contrary to what we may intuitively be expected, there was not necessarily a correlation between the two criteria (see Figure 3.12). One situation could indeed be high in realism but low in plausibility and the other way around. This is why both criteria were extracted.

Number of Situations & Design. Another important aspect of the materials generally used in the sources identified as relevant is the number of situations involved, as well as the design regarding social distance distributions (both per item and per participant). First, regarding the number of situations involved, descriptive statistics on the 38 studies included in these preliminary results revealed a mean number of situations involved of 4.08 (SD = 7.13). When looking more closely, we noticed that a high number of studies involved a unique situation to judge (N = 27), whereas only a few involved 10 or more different situations (N = 5). The median of the number of situations involved across all studies was indeed 1, indicating that in most studies, only a single moral situation was considered to test

the effects of social distance (see Figure 3.13). When displaying moreover these number of situations depending on the level of analysis for social distance, we noticed at intergroup level almost only unique situational tasks, as can be noticed in Figure 3.13. This is consistent with what has been qualitatively observed during sources' information extraction, with intergroup studies often involving a unique real-life conflict situation (such as the Israeli-Palestinian conflict, or Black vs. White racial tensions).

Figure 3.12

Counts of Studies Depending on Situations' Plausibility and Realism

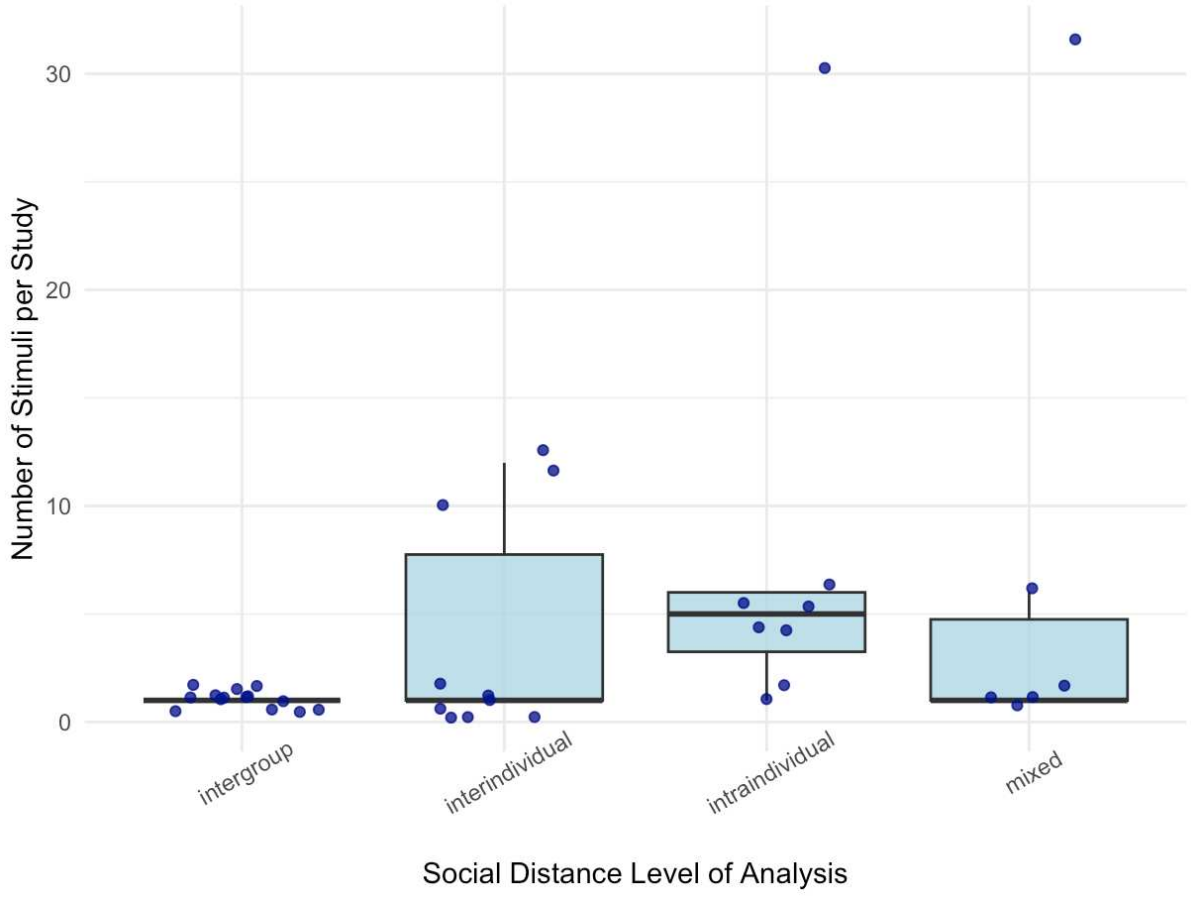


As a complementary analysis, we extracted for each study information about the design of social distance distribution, at both participant and stimulus levels. Regarding participant design, social distance various conditions were most frequently distributed between participants (N = 32) whereas only few studies opted for a within-participant design (N = 6). This is quite consistent with the common assumption that social distance may be more easily detected as a factor of interest when displayed in a within-participant design

(see discussion about that in the following chapters), even if not demonstrated per se as a necessary criterion for social distance effects to appear. Regarding distribution of social distance within the various stimuli of studies, we found a majority of within-stimulus design (*N* = 35) compared to between-stimulus design (*N* = 3), that is, when social distance conditions are displayed between stimuli (and not within the same stimulus). It should be noticed, given the mean and median of number of situations reported above, that a majority of studies was coded as within-stimulus design while containing only a unique situation participants had to judge.

Figure 3.13

Number of Situations Displayed per Study Depending on Social Distance Level of Analysis



Social Distance Effects' Moderation

Last but not least, we coded for each study the moderators (or other factors than social distance) which were tested in interaction with social distance effects. We coded them

only when some findings were reported about their interactive power with social distance, regardless of whether they were central or peripheric to the authors' hypotheses (i.e., they were all considered as moderators). We report here only a few of them, in particular those that have been found as significantly moderating the effect of social distance (even if again, we can only conclude about the potential relevance of these moderators, not about their real significance, as no quality assessment was conducted in the current review).

Relevant Moderators Identified. With these criteria and on the preliminary findings of the scoping review, we identified the presence of a harmful outcome as a significant moderator of social distance effect for at least two studies on the reduced sample of included sources (Hirschfeld-Kroen et al., 2021; Antonetti & Crisafulli, 2021). At interindividual level, the strength of the relationship with the other person was also significantly altering the influence of social distance, in particular for people with negative (vs. positive) expectations about the relationship (see Weiss & Burgmer, 2021), and/or in a distrustful (vs. trustful) relationship with the other, also varying depending on people's dispositional sensitivity to the victims (see Weiss et al., 2018). At the intergroup level, we found that group unpacking (i.e., presenting the various constitutive subgroups of a larger group) could reduce or increase the ingroup favoritism bias in intergroup conflicts (depending on which group is unpacked, see Halevy et al., 2021, for more). Moreover, at this level of analysis, social identity salience was also found to increase discrimination toward outgroup members (see Sutcliffe et al., 2020).

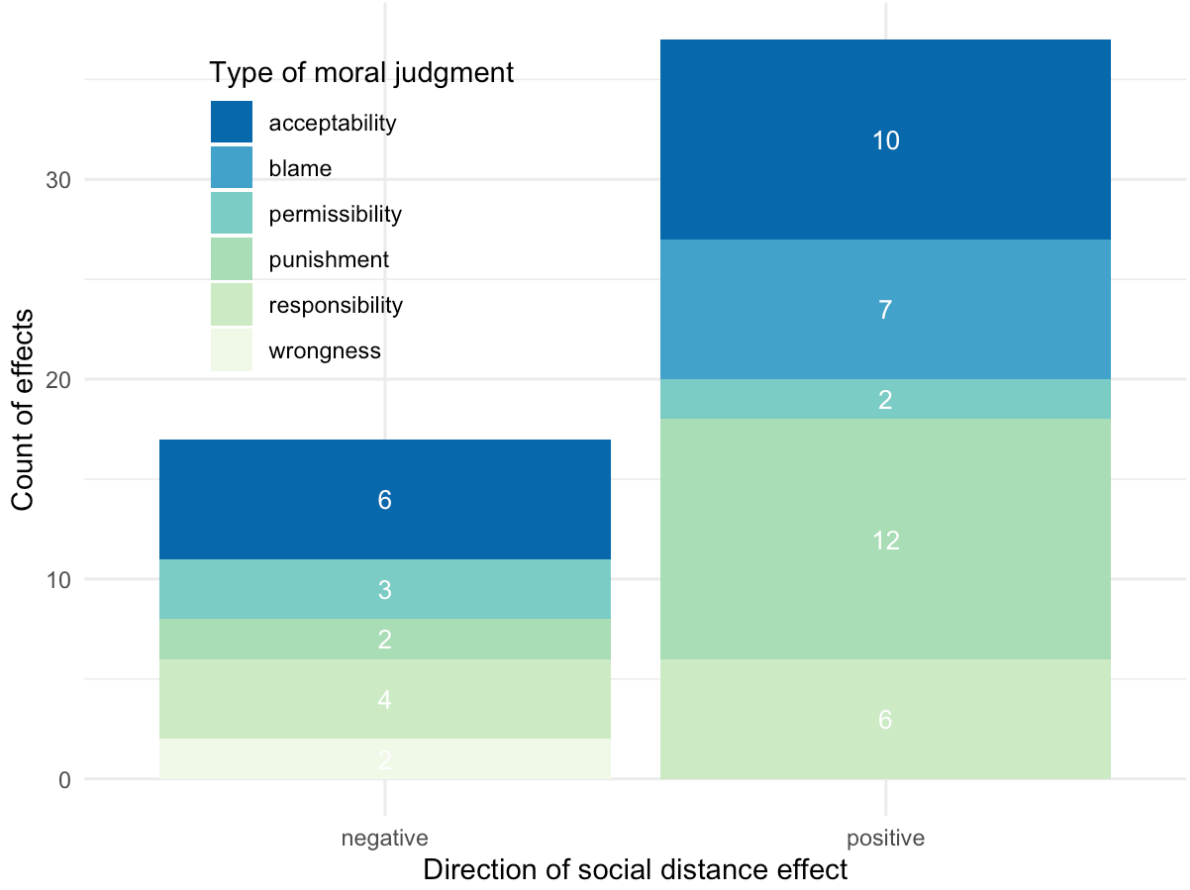
This list of moderators is again not exhaustive as the current scoping review findings are only preliminary results, but this already well synthesizes some specific orientations of the existing literature. We should also notice that participants' gender (or sex) was tested almost seven times across different studies (N = 7, throughout three different research works, and one additional as the direct operationalization of social distance between male/female protagonists). All (except for the latter case) yet revealed a non-significant moderation over social distance effects. This will be discussed in the 'General Discussion' of this dissertation (Chapter 9, p. 339).

Potential Non-Identified Moderators. Beyond the moderators explicitly referred as such in the sources already coded, we also analyzed whether other parameters such as the type of judgment or decision at stake could be a significant source of moderation for social distance effects. Many sources indeed comprised various dependent measures of moral judgments or decisions within a single study. Some of these sources directly tackled this question (see in particular Tassy, Deruelle, et al., 2013), that is whether social distance effect was more or less strong depending on the type of question involved (either a moral judgment or a choice of action). However, in the current preliminary results, we gathered only few studies tackling this question on choices of action (*N* = 2, with two additional studies comprising both types of response). Hence the difficulty to directly compare proportions of effects of social distance in one or another type of response.

Regarding moral judgments however, which were better represented in number than choices of action, there were enough studies to be analyzed (*N* = 34) to get a first idea of these influences. Studies comprised overall 12 different types of moral judgments. Note however that, as the goal is not here to get a precise idea of these effects' sizes, and because we do not necessarily expect a reverted direction of effects depending on the type of moral judgment at stake, the following results are necessarily limited in scope. As summarized in Figure 3.14 with only the most common types of moral judgment being represented, the part of positive and negative effects on acceptability judgments (representing 16 out of the 83 effects reported, ~ 19.3%) was almost identical to what was found globally (on all types of judgment). That is, more positive effects (*N* = 10) than negative ones (*N* = 6). The same pattern was found for punishment judgments (*N* = 12 and *N* = 2, respectively), and responsibility judgments (6 positive vs. 4 negative effects). However, there were only positive effects of social distance for judgments of blame (*N* = 7), and only negative effects for judgments of wrongness (*N* = 2). Finally, quite as much positive (*N* = 2) as negative (*N* = 3) effects were found on permissibility ratings. These descriptive tendencies are to be confirmed with the remaining sources, as some types of judgment were here represented only by a few studies.

Figure 3.14

Count of Social Distance Effect's Direction Depending on Type of Moral Judgment



Conclusion and Discussion

The current scoping review findings offer a preliminary overview of existing research on social distance influence in moral decision-making, via the consideration of a threefold analytical framework. Indeed, the process of searching relevant sources and their selection was made in consistency with the previously detailed analytical framework. More precisely, we considered the various terms for social distance as reviewed in the inventory of Chapter 1 (see Figure 1.1, p. 30), but classified them in supra-categories in accordance with the analytical levels described in Chapter 2 (see Table 2.1, p. 42). This aimed at systematically reviewing studies tackling this question with a large understanding of what social distance is, in accordance with the integrative and continuous definition we supported in Chapter 1.

Given that the number of relevant sources to include was huge within the a priori

fixed criteria (*N* ~ 840), we decided to restrain the analyses to the sources dealing with an adult population and tackling social distance effects within the dark morality area (i.e., moral transgressions and antisocial acts). Note however that the whole sources in the other subdomains have also been sorted and pre-classified, such that we do already have a list of included studies dealing with this question among children/adolescents and/or for prosocial acts only. We therefore invite other researchers who would participate in this integrative endeavor to join our efforts and draw on this database to conduct targeted scoping reviews or meta-analyses on specific sub-dimensions of the broader map we explored here.

Out of the 340 sources included with the restricted criteria of selection (i.e., adult participants only, in dark morality only, and not considering further the theoretical and synthetic review contributions, or qualitative works), we reported above preliminary results for a total of 16 sources, with 38 studies being coded (as one single source sometimes contained plural relevant studies). On these preliminary data, we indeed found several terms and operationalizations of social distance, as well as a fairly balanced distribution of studies within the various levels of analysis. Some research even tackled social distance within mixed levels of analysis (e.g., interindividual and intergroup levels).

Regarding social distance per se, the aim of this scoping review was not to quantify or draw definitive conclusions about its influence. We rather wanted—beyond mapping the diversity of terminologies and conceptualizations used—to identify key parameters to take into account when investigating this question. One of the critical parameters to consider, in line with the present dissertation's aims, is the target of manipulation for social distance. In other words, from whom and toward whom social distance is being varied. Within these preliminary results, we found that most studies manipulated the relationship between the participant and the agent of the wrongdoing, or between the agent and the victim of it, while only few manipulated social distance between the participant and the victim or the recipient of the act. This observation may however be biased as the studies coded for now have not been chosen randomly within the dataset of included studies (but rather by previous knowledge of the sources).

Another objective was to gather which direction of effects were found overall for each study, regarding the two tendencies of findings that have previously been described in Chapter 2. Based on these preliminary results, and consistently with our previous intuitive observations of social distance effects in the state of the art, we found most effects of social distance being tested to be positive in direction (~ 53%). This translates into a general favoritism for self (at intraindividual level), or close ones (at interindividual level), or ingroup members (at intergroup level) compared to more distant others (Tendency 1). However, a quite substantial number of studies revealed negative effects' direction (~ 23%), hereby suggesting that social distance effects are more complex than sometimes assumed. Congruent with our expectations, this pattern was observed mainly for social distance effects when manipulated toward the agent, or between the agent and the victim, whereas studies manipulating social distance to the victim uniquely did not result in any negative effect. These findings remain yet to be confirmed on the whole set of studies to be included in this scoping review.

Such variability in findings may also appear as closely tied to the type of moral material used, including the severity of the situations (in terms of consequences for the victim) as well as their realism and plausibility. Most studies involved high plausible (i.e., everyday like situation), but low realistic (i.e., hypothetical vignettes) and low severe situations. Moreover, many studies were based on a very limited number of situations that participants had to judge (i.e., most often less than five, even only one unique moral situation for almost 71% of them), hence restricting the generalizability of their findings. They were also mostly presented in a between-participant design with regard to the conditions of social distance, which raises important questions for future research design.

Finally, the scoping review allowed to identify several moderators already explored in literature in relation to social distance influence (as for the strength of the relationship with the target), with some being systematically found as non-significantly influential on our preliminary data (e.g., gender). Though still on these preliminary results and rarely stated explicitly as a moderator, the type of moral judgment at stake might have its importance for

social distance effects. As an illustrative purpose, studies asking for a judgment of acceptability triggered effects' direction in proportion to what was exposed earlier, whereas judgments of blame only triggered positive effects. This would however also to be confirmed on the whole data review. As a prelude to the following chapters of this dissertation, we will therefore shift our focus away from the already well-explored moderators (such as those explicitly identified in the present scoping review) and toward those that have been underexamined—or even largely overlooked—in the literature to date (e.g., the type of judgment and/or of decision, the materials' severity, as well as other morally salient factors).

Chapter 4

Overview of the Current Dissertation

In the theoretical introduction of the current dissertation, we reviewed the literature about social distance effects on moral decision-making. In doing so, we highlighted the fact that this question has been largely examined in literature although in a very dispatched way. Some research deals with this question at the intraindividual level (self vs. other) whereas others tackle it at the interindividual or intergroup levels (close vs. distant other; ingroup vs. outgroup member). Very few provided however an integrative framework for the diverse terminologies and methods used. This is what we did in the first theoretical part of this work, through the proposed inventory in **Chapter 1** to first review the various terminologies for social distance, then via the review of the studies tackling this question at all three levels of analysis in **Chapter 2**, and finally by providing preliminary results of the scoping review undertaken about this question in **Chapter 3**. This theoretical contribution about the state of the art was necessary to guide our empirical work, which will be detailed in **Chapters 5, 6, 7, and 8**.

Key Parameters we Chose to Focus on

Among the various parameters that appeared as important to consider in the scoping review preliminary results (**Chapter 3**), we selected several ones. We particularly chose either those considered as unavoidable for the study of social distance effects in moral decision-making and/or those still underexplored in the literature. These parameters are detailed below.

Social Distance Varying Toward Whom?

The first important parameter to consider is the role of the protagonists between whom the social distance is varying. It is indeed very different to manipulate social distance between the participant (as an observer) and the agent of a moral transgression, and to manipulate it toward the victim of the transgression, notably in terms of social distance

effects. However, it is not always something research has controlled for. In the current work, we will first report experiments where social distance has been manipulated toward the agent of the action (**Chapter 5**), then those where social distance has been manipulated toward the victim (**Chapter 6**).

Types of Moral Judgment or Decision Tasks

Another important parameter is the type of moral judgment or decision task at stake. Some authors already suggested a discrepancy between moral judgments vs. choices of action (see for instance Tassy, Deruelle et al., 2013), but even within a same category (e.g., moral judgment) we may expect some differences in terms of social distance impact, as suggested earlier in the scoping review findings. This parameter will be particularly one of the topics addressed in **Chapter 7**.

Severity of the Situation Outcome

Last but not least, the severity of the situation outcome has sometimes been considered as an important factor, even if often not considered at all in numerous research. As suggested by some authors (e.g., Weidman et al., 2020), considering the severity of the action at play may be of great importance to predict social distance effects. We could even think it may reverse its effect's direction. When the wrongdoing is a minor infraction (e.g., lying without any physical harm or a physical harm that does not threaten the victim's life), social distance toward the agent influence may not be the same as in a more severe transgression (even if accidentally perpetrated) as when the victim suffers an irreversible damage. This will also be tackled in **Chapter 7**, which focuses on the moderators of social distance, either controlled or manipulated in our experiments.

Intent, Outcome, and Causality Factors

We should add some words about the factors of intent (i.e., the belief and desire of the agent when committing the wrongdoing, was it intentional or accidental?) and outcome or causality (i.e., the consideration of the outcome of the act, is it neutral or harmful; and whether the agent was directly the cause for this outcome or not). These factors will often be taken into account in our experiments (whether they are manipulated or controlled). Yet,

these parameters did not appear in literature as main predictors for the study of social distance (see scoping review results). The reason why they are important in our work is because they are key factors in the area of moral decision-making. As such, they allowed ensuring the validity and robustness of our experimental paradigm by replicating robust effects of literature. We also analyze them as potential moderators of social distance effects in **Chapter 7**.

Specific Framework for the Current Experiments

Beyond the consideration of these parameters as important potential moderators or parameters of social distance influence, we want to further specify the specific framework of the current experimental work. These specifications do not mean these parameters will be manipulated or measured per se in our following experiments, but that we will focus more precisely on these contexts for the study of social distance on moral decision-making.

The Area of Moral Transgressions by Adults

We first focus on moral transgressions only, or what we more generally called above the 'dark morality' (i.e., when someone committed some wrongdoings, and/or acted in a wrong way). We do not therefore focus primarily on pro-social acts or 'bright morality' (i.e., when someone is giving some help or more generally should allocate some resources)[35]. We also tackle the moral cognition of adult participants only (or at the margin, of university students). We indeed did not want to orient the current dissertation toward the moral development of individuals as passing through the various stages of Kohlberg's stages of moral development (Kohlberg, 1958). Although probably interesting to tackle—as done previously by Grime (2005) as a doctoral work or by Lazić et al. (2021) as a meta-analysis about parochial cooperation among children—our focus here remains on adults' cognition.

The Agent (Wrongdoer) as Target of the Judgment or Decision

[35] Note that there is some porosity between both types of morality when considered as a continuum (see Chapter 2 for more). For instance, egoistic acts are seen as part of dark morality, but also entails often not acting altruistically (hence being at the same time part of bright morality). Similarly, tasks where participants should allocate some resources were not planned in the current experiments as not the primary focus, even if an unfair sharing could at some point result in a wrongdoing.

Independently of the manipulation of social distance toward the agent or the victim, the target of judgment will also be constrained in the current investigation to the agent (i.e., the one who perpetrated the wrong action) and never the victim. We thus aim at analyzing judgments given by participants regarding the behavior of the agent only (i.e., judging the agent's behavior). Although in some contexts, as for sexual harassment studies, people could be asked whether the victim is blameworthy or not, we do not focus on these cases in the current dissertation.

In Hypothetical but Numerous and Diverse Everyday Moral Situations

We will consider moral situations as close as possible to what people could experience in everyday life, hence the use of moral scenarios that are plausible, extracted from situations that have the potential to occur in reality. Although remaining hypothetical scenarios (as the participant does not actually experience the situations in real life, but has to imagine them), this distinguishes our work from the huge literature tackling moral decision-making with hypothetical and low plausible scenarios, such as with trolley-like (sacrificial) dilemmas. We will also use numerous and diverse moral situations or scenarios (often up to 60), which again differentiates our work from many studies in the literature that exploit only a limited number of scenarios. As observed in the preliminary results of the scoping review, studies about this question seem to often involve less than five different situations, sometimes even only one. Restricting the number of encountered situations to only a few scenarios hampers generalizability, and in the extreme case of only one scenario, we can merely conclude that the effects hold in this specific case, not in moral situations more broadly. The much larger set of situations in our experiments allows to generalize our findings to other moral situations and to moral judgment and decision making more generally.

A Socially Situated and Dynamic Phenomenon

Finally, we consider in the current work moral cognition as a socially situated phenomenon, hence being subject to the dynamic interplay between the individuals' internal states and goals and their environment (Smith & Semin, 2007). As such, social distance may

benefit from an analysis of its impact in moral contexts on the whole decision-process and not only on the final decision taken. Moreover, considering that social distance may have a more subtle influence than previously conceived, we should also consider that deliberate responses from participants in previous research may have suffered from a desirability bias, hence not reflecting their own real opinion about what they would really do or how they would really judge these situations. We will therefore rely in some of the following experiments on real-time processes-tracing methodologies to investigate social distance effects in a more dynamic way (i.e., via a mouse-tracking technique).

Toward an Integrative Threefold Approach of Social Distance Influence in Moral Decision-Making

Aim of the Current Thesis

Having reviewed the literature in social and moral psychology regarding the influence of social distance and its large diversity of methods and concepts, we did not find an integrative framework that tackled social distance influence in moral decision-making at all three levels of analysis depicted earlier (i.e., intraindividual, interindividual, and intergroup levels). This is what we aimed to do in the current dissertation, organizing under a commensurable framework, then unifying the existing literature to offer an integrative understanding of social distance effects in moral decision-making. The various empirical experiments conducted in the present work allowed to investigate these effects, while considering plural levels at the same time to directly compare findings between them in a single experiment. By doing so, we also experimentally endorsed a truly social psychological perspective on moral decision-making, which is recently emerging as a more general analytical framework (see Ellemers et al., 2023 for another illustration of the threefold analytical framework).

Thesis Claim

The research question that interests us for the current work is therefore how to unify and make commensurable the apparently confusing literature about social distance

influence in moral decision-making. That is to say first, clarifying the concept of social distance by reviewing its various forms and terminologies and determine an integrative definition of it. Then reviewing the apparently ambiguous findings in the literature within a multifold analytical framework and testing them throughout the various levels of analysis. Finally, examining the potential influence of some moderators still underexplored in previous research and developing a more indirect method to grasp social distance dynamics. Throughout this dissertation, we therefore argue that—given its complex and ambiguous role in moral decision-making—social distance should be studied within an integrative multilevel framework that enhances its commensurability, and through a dynamic approach that examines its influence throughout the entire decision process.

Overview of Experiments

Note that, in order to precisely address the objective of each empirical chapter, we sometimes refer to only one part of a single experiment (through the use of part A / part B). It is the case for Experiments 3 and 4 for instance when tackling the manipulation of social distance to the agent (part A) or to the victim (part B), but also when we refer to the analysis of different type of data within a single study, as it is the case in Experiment 1 (with part A on final responses, and part B on mouse trajectories).

To address these objectives, we start by focusing in the first empirical chapter (**Chapter 5**) on social distance manipulation toward the agent in various moral situations—that is, where the agent performed a harmful act (voluntarily or not). In this chapter, we tackle this research question at both the inter- and intraindividual levels (Experiments 1, part A and 2) and test the effects both in lab (Experiment 1) and within an online procedure (Experiment 2). We further explore these effects at the intergroup level of analysis via the induction of a minimal group paradigm, once in lab (Experiment 3, part A) and once online via a more ecological paradigm (Experiment 4, part A). This last experiment allows at the same time to confront findings about this question at both the intraindividual and intergroup levels.

Chapter 6 tackles the same question but when social distance is manipulated toward the victim of the perpetrated harm (not anymore toward the agent). That is, what does it imply to be socially close or distant from the victim of a perpetrated wrongdoing? First, at the interindividual level (Experiments 5 and 6) and then at the intergroup level (Experiment 3, part B; Experiment 4, part B), still through the same minimal group paradigm induction as before.

We then investigate the influence of some potential moderators on social distance effects in **Chapter 7**, such as when tested with regard to some key factors in moral (i.e., intent, outcome, causality) (Experiment 1). We also herein test the effects of social distance regarding the type of judgment or decision at stake, as analyzed in Experiments 2 and 5. In the same manner, we report the observed effects of social distance depending on the severity of the situations in Experiments 1, 2, and 3 in an explorative way and in Experiment 6 in a confirmative way. We then end this chapter with explorative results regarding how social distance effects may be modulated by the agent's responsibility and/or by the more or less large part of external circumstances perceived in such moral situations (Experiments 1, 2, and 3).

In **Chapter 8**, considering everyday morality as a socially situated and dynamic phenomenon as well as social distance's influence as maybe more subtle than firstly expected, we developed an innovative mouse-tracking paradigm and argued in favor of its efficiency to study moral decision-making more dynamically. We tested this novel methodological tool first on the replication of key factors' influence in moral situations (Experiments 7 and 8) and provided a first empirical test of the influence of social distance through this paradigm in Experiment 1 (part B). Table 4.1 comprises a summary of all the experiments of the current dissertation work as well as their respective characteristics.

Finally, **Chapter 9** presents the general discussion of this dissertation, integrating the main findings, addressing theoretical and methodological limitations, and outlining directions and implications for future research.

Table 4.1

Overview of the Experiments in the Current Manuscript

Study ID	Factors involved	Social distance to	Levels of analysis	Material	*N*(pp)	Response Analyzed	Lab / online	OSF project link
Exp.1	Social distance x Intent x Causality	Agent	Inter-individual	Leloup et al. (2018)	73	Part A. Final resp. Part B. Dynamic (MT)	Lab	https://osf.io/feup6/
Exp.2	Social distance x Type of judgment	Agent	Intra & inter-individual	Leloup et al. (2018)	165	Final response	Online	https://osf.io/cyb8k/
Exp.3	Social distance (MGP like)	Part A. Agent Part B. Victim	Intergroup	Leloup et al. (2018)	152	Final response	Lab	https://osf.io/4sym5/
Exp.4	Social distance (MGP like)	Part A. Agent Part B. Victim	Intra-individual & intergroup	*NA*	69	Final response	Online	https://osf.io/xd8c5/
Exp.5	Social distance x Gender	Victim	Inter-individual	Singer et al. (2019)	299	Final response	Online	https://osf.io/j49k2/
Exp.6	Social distance x Severity	Victim	Inter-individual	Singer et al. (2019)	149	Final response	Online	https://osf.io/kr9ef/

Exp.7	Intent x Outcome/ Causality	*NA*	*NA*	Leloup et al. (2018)	78	Part A. Final resp. Part B. Dynamic (MT)	Lab	https://osf.io/3uyps/
Exp.8	Intent x Outcome / Causality x Type of judgment	*NA*	*NA*	Leloup et al. (2018)	144	Part A. Final resp. Part B. Dynamic (MT)	Lab	https://osf.io/675gm/

Note. Exp. = Experiment, MGP = minimal group paradigm, pp = participant, MT = mouse-tracking, OSF = Open science framework

PART.II

EXPERIMENTAL CONTRIBUTION

Chapter 5

Social Distance Influence in Everyday Moral Decision-Making when Manipulated Toward the Agent

To begin with the experimental part of this work, we analyzed in this chapter social distance influence in everyday moral decision-making when social distance is manipulated with respect to the agent of the situation at stake, that is, with the wrongdoer or perpetrator of the wrong action. This will be done in accordance with the analytical framework we depicted earlier in the theoretical part of this thesis, at the intraindividual, the interindividual, and the intergroup levels. However, we will during this experimental part only focus on transgressions (*dark morality*) and not on pro-social acts that could also suffer the same biases. Note also that several levels of analysis are sometimes involved at once within a single study. This is notably the case for Experiment 2 and Experiment 4 reported below, with the aim to directly confront findings at each level.

At the Intra- and Interindividual Levels

Rationale and Overview of the Experiments

At the intraindividual and interindividual levels of analysis, we consider social distance respectively either as (i) comparing oneself with others in the role of the agent (i.e., does it make a difference to judge oneself or someone else for the same wrongdoing?) or as (ii) comparing a close other with a more distant other in the same role (i.e., does it make a difference to judge a friend vs. an acquaintance or a stranger for the same wrongdoing?). As a reminder, two tendencies of results were found in the literature at both levels: One where people favor themselves or their close ones in such moral transgressions—by punishing more harshly those who are socially distant for instance (Tend.1); the other where people disfavor themselves or their close ones in case of transgression—by punishing oneself or a close one even harsher than they would do for a distant other (Tend.2).

At the intraindividual level first, Weiss et al. (2018) demonstrated that people imagining distrust within a professional relationship judged their own transgressions as morally more acceptable than those of their coworker, hence exhibiting to some extent moral hypocrisy. However, this pattern was not evidenced in the condition of imagined trustful relationship with the coworker (see Study 2, Weiss et al., 2018). The authors even showed in Study 3 that this effect was not contingent to distrust within the relationship with the coworker: When generally experiencing distrust in their own lives, people were more susceptible to show a moral hypocrisy bias in their favor (Weiss et al., 2018). Extending this idea, Weiss and Burgmer (2021) then analyzed the reversed pattern of *moral hypercrisy*, and evidenced throughout various settings how people made more lenient judgments when their close ones were the authors of various transgressions (i.e., friends in Studies 1 & 2, romantic partners in Study 3) rather than themselves. In the same idea, Hirschfeld-Kroen et al. (2021) found that people judged their own accidental wrongdoing as slightly more morally wrong than those perpetrated by a third-party, and a posteriori judged themselves as more responsible for their harmful act than they did for those of others.

At the interindividual level, we found mainly instances of research either in direction of the first major tendency of close one's favoritism (Tend.1) or an absence of social distance significant effect. Linke (2012) investigated this question especially when social distance is manipulated toward the agent of the transgression in his first study (either the participant's mother, or a classmate, or a foreign visitor). The author found a main effect of social distance such that the transgression appears as less severe when close others are involved as perpetrators compared to distant others. But he did not find such an effect on punishment or wrongness ratings (Linke, 2012). Weidman et al. (2020) found across five experiments that the likelihood to protect (vs. punish) the perpetrator of a moral transgression is significantly higher overall when involving a close one rather than a distant other. O'Gorman et al. (2005, Study 1) on the contrary found no significant influence of social distance (manipulated notably via genetic relatedness and friendship) on punishment judgments to the transgressor. The same social distance effect was however revealed as

significant on prosocial measures as the willingness to help (in Study 2, O'Gorman et al., 2005).

In front of these controversies and/or lack of consistent evidence, we aimed in this first section to test the influence of social distance toward the agent at both the intraindividual and interindividual levels of analysis. In Experiment 1 (N = 73), we first dealt with the latter case. Specifically, it compares participants' punishment judgments regarding daily-life moral transgressions (see details of the material used in the following sections) when a close one versus a mere stranger is involved as the agent of the wrongdoing. Experiment 2 (N = 165) complexifies this protocol by adding the possibility that the self (i.e., the participant him or herself) perpetrates the wrongdoing, thus investigating both the first and second analytical levels at once.

Experiment 1 (Part A)—Close vs. Distant One as Agent and Self as Observer

Rationale

We first investigated the impact of social distance within a novel mouse-tracking paradigm (i.e., registering of the mouse movements across time and space)[36] of everyday morality developed by our research team (see also Gautheron et al., 2023). Beyond the mere social distance effect, we aimed to replicate at the same time two traditionally important factors in moral judgments: the agent's intent (i.e., how much the agent intended to transgress) and the agent's causality (i.e., how much the agent is causally responsible for the harmful outcome to the victim). To do so, we adapted the identity of the transgressing agent in the moral vignettes, so that the wrongdoing was committed either by a close one (i.e., from the participant's point of view) or by an unknown identified agent. We predicted—beyond the mere replication of main and interaction effects of intent and causality—that social distance would have a significant influence on moral judgments of punishment against the agent. More precisely, we expected judgments would go either in direction of the main

[36] Note that the mouse-tracking paradigm as well as the corresponding mouse trajectory results will not be presented and detailed in the current chapter, as we focus here merely on social distance effects. We however dig into it more deeply in Chapter 8, in an explorative way (see pp. 324–327).

tendency in literature (i.e., Tendency 1: close ones being judged more leniently than strangers) or in the other direction (i.e., Tendency 2: close ones being judged more harshly).

Method

This study was preregistered via OSF, materials and analytical script are available on the OSF page of the project (see https://osf.io/feup6/). The study protocol has been validated by the local ethics committee.

Sample Size. The sample size for this study has been determined thanks to a power analysis conducted under the PANGEA software (Westfall, 2016), based on effect sizes found in the literature and aiming at a 80% statistical power. We fixed a reference sample size based on this calculation at n = 80. We however chose to run sequential analysis (as recommended by Lakens, 2014), for very uncertain effect sizes, we ran a first analysis at n = 40, then at n = 84 (the final sample we get).

Participants. Eighty-four Psychology undergraduates (78 females and 6 males, aged from 18 to 45, M = 20.83, SD = 3.71) participated in exchange for a course credit. The current experiment was entirely conducted online due to the Covid-19 crisis and subsequent face-to-face restrictions in France. We failed to correctly collect the complete data from nine participants because of technical issues due to the online experimentation (i.e., less than 60% of the whole task completed). We therefore excluded these (too) incomplete data for the final analysis, leaving a final sample for analyses of 75 participants, with 100% or at least 60% of the experiment completed (only 4 participants concerned for the latter case).

Material. We used Leloup et al.'s moral scenarios battery (2018). These vignettes, in French, figure hypothetical real-life moral transgressions, in which an agent perpetrates an immoral act either intentionnally or not, and either with harmful consequences or not. The task is basically to judge for each moral scenario how wrong is the depicted transgressive act and how much the agent should be punished for having perpetrated it. The material

originally includes 64 hypothetical moral situations but we used four of them as training items[37], leaving a final set of 60 scenarios per participant.

In a previous work, we already adapted Leloup et al.'s (2018) original material (see Gaboriaud et al., 2022, detailed in Chapter 8, pp. 286–302). Four versions of each scenario were built to manipulate the agent's intent (intentional vs. non-intentional) and the agent's causality in the outcome (caused by the agent vs. not caused by him or her). For the present study, in order to also manipulate social distance, we created eight versions of each scenario. These final eight versions for a unique scenario were obtained by creating for each of the four current distant versions (i.e., as in Leloup et al.'s article in 2018) a close variant (involving a close one instead of a stranger), hence manipulating social distance between the participant and the agent (see Figure 5.1 for an example translated in English)[38]. We thereby obtained a database of 512 (64x8) distinct stimuli, with eight versions per scenario crossing the three factors: the agent's intent (x2), the agent's causality (x2), and the agent's social distance (x2) (see entire materials on the OSF page). Other minor adaptations such as vocabulary changes (as the original database had been created for a Belgian audience) or control of gendered phrasing (to match with the original scenarios' gender in close conditions) were made (see supplementary material on OSF).

Procedure & Design. The computerized online task was completed either with the computer mouse or trackpad. Participants first get across four training scenarios to familiarize to the mouse-tracking interface, before judging the experimental 60 moral situations. We included four attention checks (i.e., one before each break, at 25%, 50%, 75%, and 100% of the study run) to ensure participants were still focusing on the ongoing task. We planned respectively online briefing and debriefing sessions with the experimenter before and after the experiment, to check for participants' discomfort.

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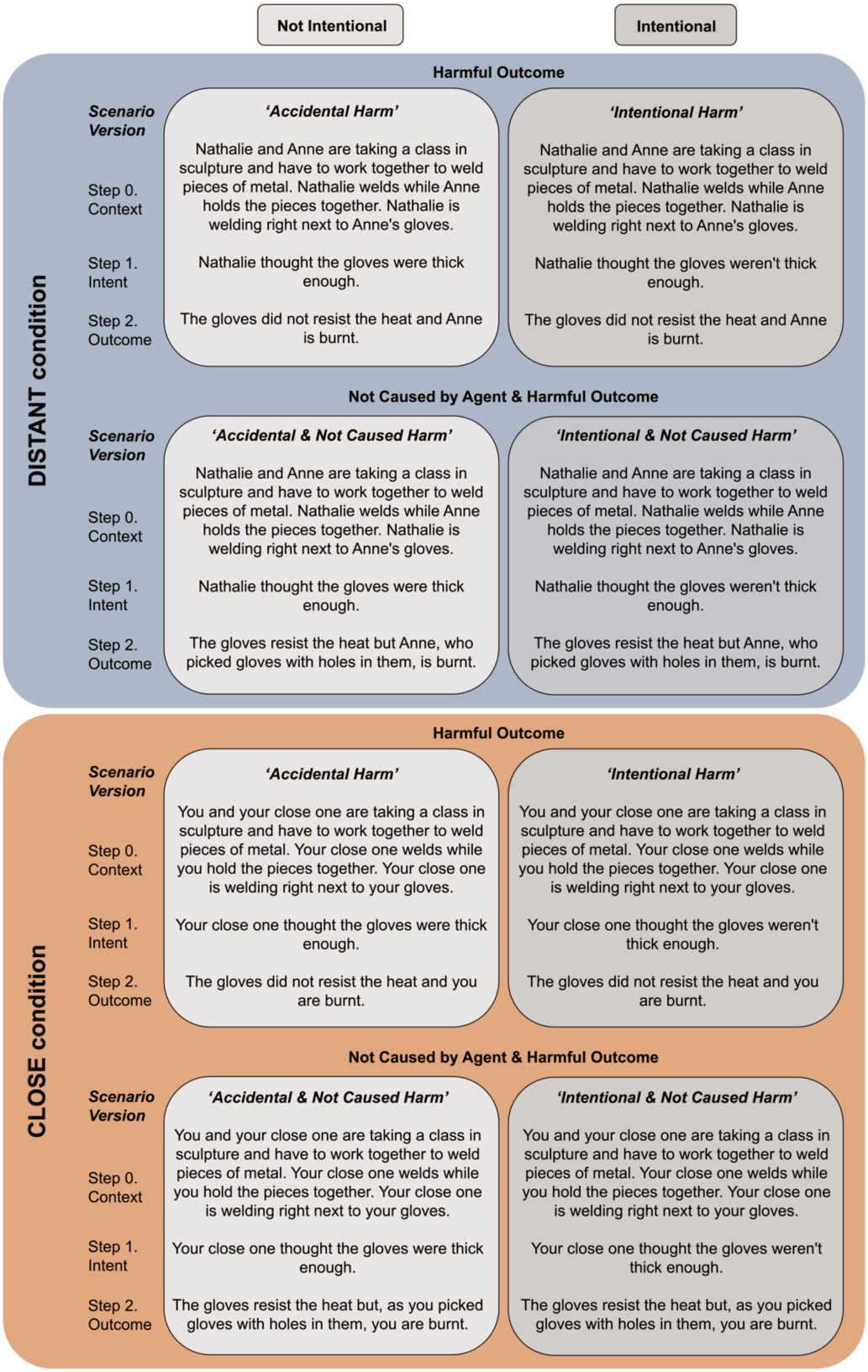
[37] In particular because of the mouse-tracking paradigm that required some training to get familiar with the experimental task.

[38] Note that the original material (Leloup et al., 2018) manipulated the agent's intent, the outcome (either neutral or harmful, currently adapted into the causality factor), and the presentation order of both features. We did not here control for the order of presentation and only chose the most intuitive version (i.e., first presenting the intent, then the outcome) as Leloup et al. (2018) reported no main effect of order even if it played a significant role in judgments of attempted harms.

Figure 5.1

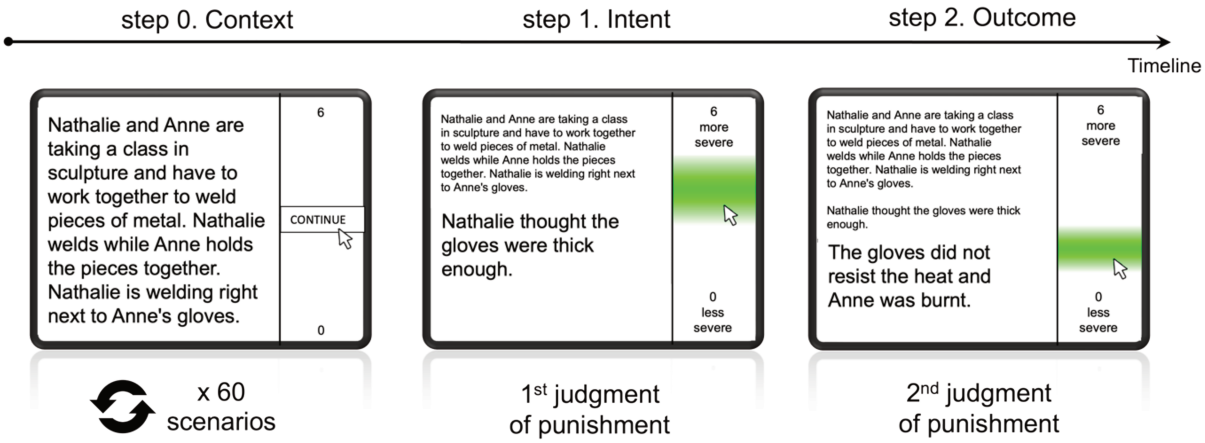
An Example of Scenario in Exp.1 in its Eight Possible Versions Crossing Social Distance(x2), Intent(x2), and Causality(x2)



Regarding the task itself, participants had to successively read and then judge scenarios involving daily-life wrongdoings by inflicting from 0 (less severe) to 6 (more severe) tokens of punishment to the agent for his or her wrongdoing. They first read the general context plus the agent's intent only and gave a first judgment of punishment (step 1); then read about the outcome of the situation and gave a second judgment (step 2) (see Figure 5.2). For reasons of brevity, only analyses about step 2 final judgments will be presented in the current chapter (see analytical code in OSF for results in step 1). This study followed a counterbalanced design—i.e., within participants and within stimuli—but not fully crossed. Participants judged each of the 60 scenarios only once under one out of the eight possible versions, but participants did not see the same scenario under the same version (i.e., 7 or 8 scenarios per condition and per participant).

Figure 5.2

Schematic Representation of the Procedure Followed in Exp.1



Note. An example was taken here to illustrate the procedure, translated in English, and presented in the distant, non-intentional, and harmful outcome condition (i.e., accidental harm).

Analytical Details. We conducted all analyses with linear mixed models (*lme4* package, Bates et al., 2014) in R (v4.1.1; R Core Team, 2021). We ran a mixed model with three fixed effects (i.e., intent, causality, and social distance), one control variable (i.e., the participant's gender), and two random variables (i.e., participant and scenario). Manipulated

variables were coded by contrasts: [Intent] non-intentional (-0.5) vs. intentional (0.5); [Causality] caused by agent (-0.5) vs. not caused by agent (0.5); and [Distance] close (-0.5) vs. distant (0.5). After having applied a procedure to reduce the random structure of the model (see Bates et al., 2014), the final model R-equation was:

$$Punishment \sim Intent * Causality * Distance + (Intent * Causality \mid Participant) + (Intent * Causality \mid Scenario)$$

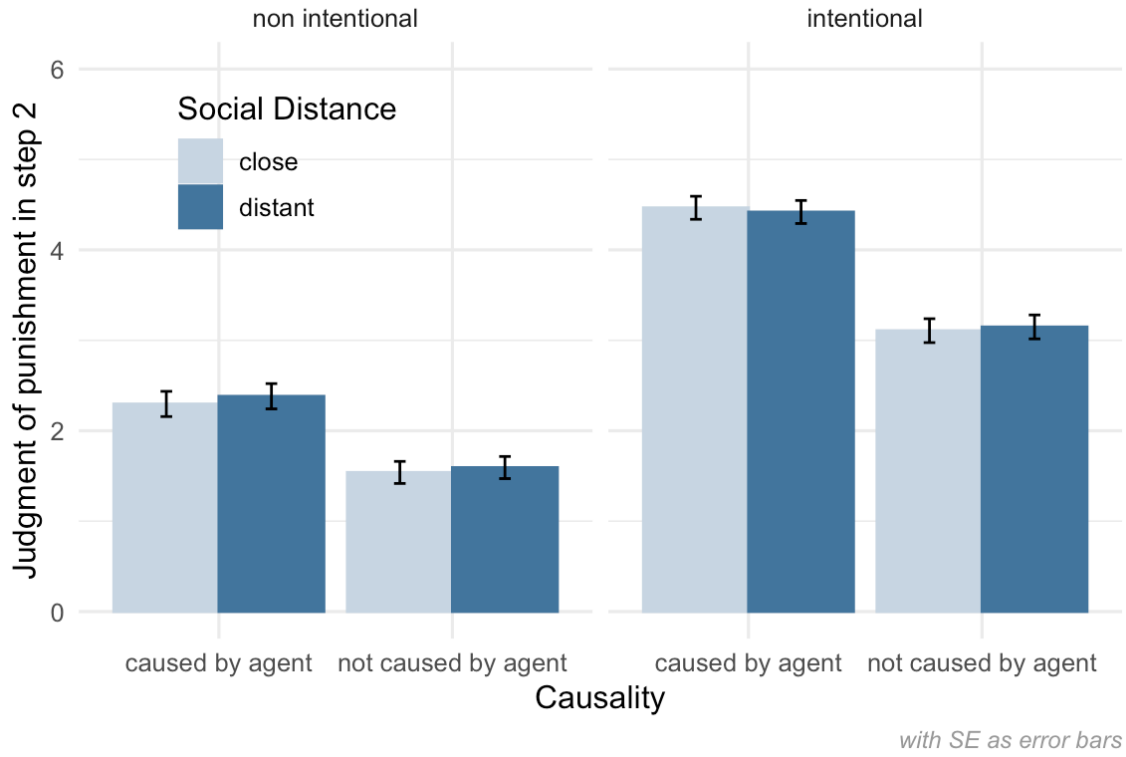
As preregistered, we excluded from analyses all missed trials and trials with a response time under 500ms or above 10s. The complete data from one participant who answered randomly (i.e., too fast) too many trials and from another participant who failed all attention checks were also excluded, leaving a total final sample of 73 participants.

Results

Through the mixed model analysis run on step 2 final judgments of punishment (i.e., when both information of intent and outcome was available to participants), we replicated the traditional effects of intent and causality: b = 1.83, SE = 0.14, t(84.89) = 12.92, p < .001, 95% CI [1.55, 2.11], d = 1.20 and b = -1.04, SE = 0.11, t(77.30) = -9.54, p < .001, [-1.36, -0.83], d = -0.68 respectively. Participants indeed punished more harshly the agent when the act was intentionally perpetrated (M = 3.78, SE = 0.10) compared to non-intentionally perpetrated (M = 1.95, SE = 0.11) and when he or she caused the outcome directly (M = 3.39, SE = 0.10) compared to when the outcome was caused by another source (M = 2.35, SE = 0.09) (see Figure 5.3). We also found as suggested by previous research (e.g., Gaboriaud et al., 2022) a significant intent-by-causality interaction, b = -0.54, SE = 0.12, t(71.23) = -4.36, p < .001, [-0.79, -0.29], d = -0.18, such that the effect of intent became weaker (-0.54) when moving from scenarios where the outcome was caused by the agent to scenarios where the agent did not directly cause the outcome (see Figure 5.3).

Figure 5.3

Final Judgments of Punishment in Exp.1 (Part A) Depending on Intention, Causality, and Social Distance (Step 2)



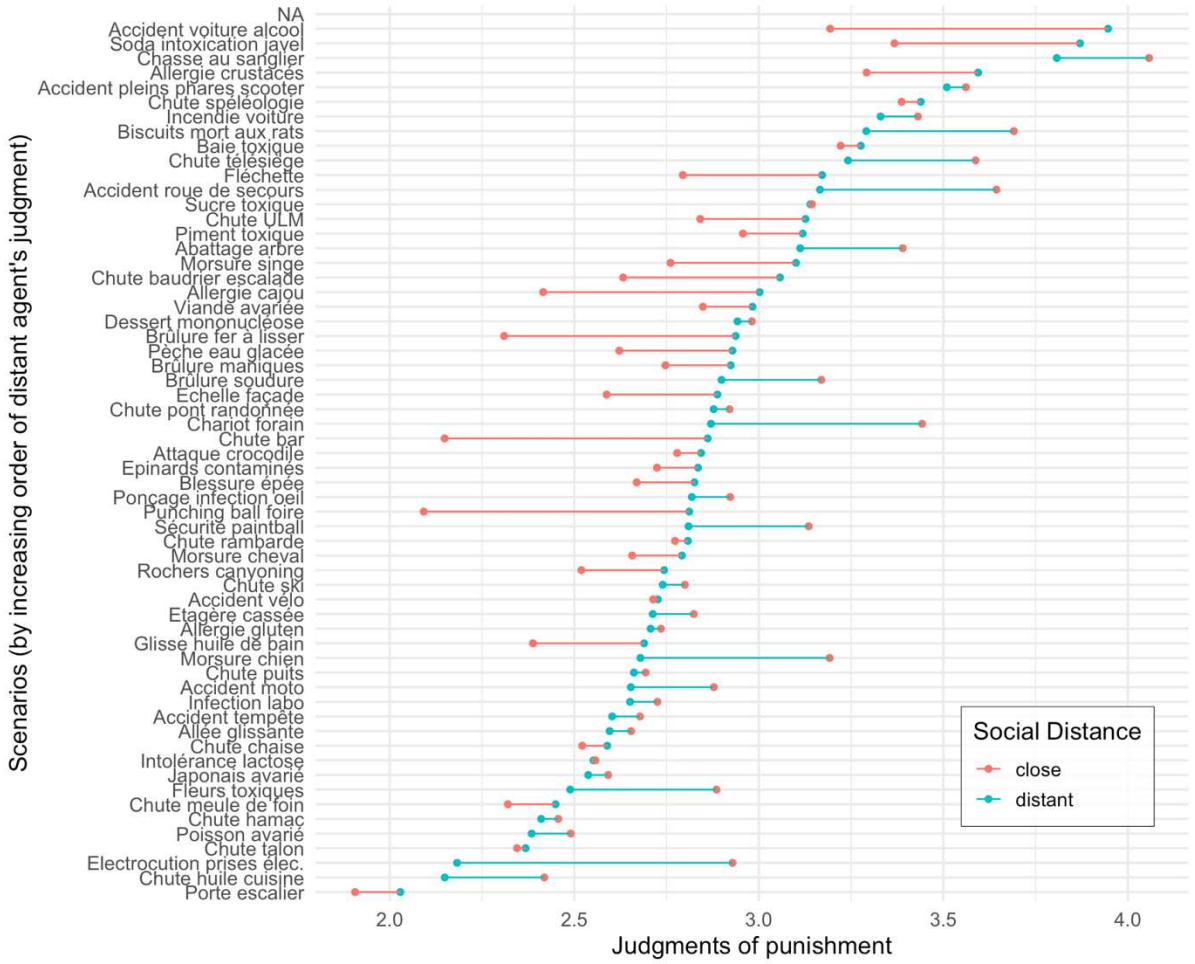
In this first experiment manipulating social distance to the agent from the participant's perspective, we found no significant evidence of a discrepancy between judgments of a close one or of a stranger as the agent, b = 0.03, SE = 0.04, t(3815.31) = 0.93, p = .35, [-0.04, 0.10], d = 0.02 (in step 2). Participants did not overall judge their close-ones (M = 2.85, SE = 0.08) as significantly more (or less) punishable than a distant other as the agent (M = 2.89, SE = 0.08) (see Figure 5.3).

When looking, yet descriptively, at the distribution of the effects of social distance across all conditions of intent and causality combined, we noticed very diverse effects, both in terms of size and direction, depending on the scenarios involved (see Figure 5.4). Some scenarios seem indeed to trigger an overall positive effect of social distance (in red lines on the figure) with harsher judgments of punishment for distant than for close agents (for almost 1 out of 2 scenarios). Others are triggering on the contrary negative effects of social distance

(in blue lines on the figure) with harsher judgments for close than for distant others (for almost 1 out of 3 scenarios). For a few ones in the end (as can be visualized in Figure 5.4), there seems to be no real difference of judgment between close and distant agents (as shown by scenarios with almost no difference between blue and red dots).

Figure 5.4

Descriptive Effects of Social Distance in Exp.1 (Step 2) on Judgments of Punishment Depending on the Various Scenarios



Note. Scenarios are displayed on the vertical axis in increasing order of punishment judgments toward the distant agent. On the horizontal axis, judgments of punishment are displayed from more lenient judgments (0) to more severe judgments (6). Here the abscissa axis has been zoomed (from 2 to 4) to better visualize the discrepancies between social distance effects. Blue lines represent 'negative effects' of social distance, that is, when the close agent is judged more harshly than the distant agent, whereas 'positive effects' of social

distance (in red lines) reflect cases where distant agents are judged harsher than close ones.

We also conducted additional exploratory analysis on how social distance effect direction for each scenario was correlated with mean judgments of punishment attributed to the close agent. As visible in Figure 5.5, no matter the condition of intent and causality, it revealed a positive relationship between both dimensions (from $r^2 = 0.33$ to 0.60) such that the more leniently participants judged their close one in the scenarios, the more negative the corresponding effect of social distance was (measured as the difference of punishment between close and distant agents). Here a negative value on social distance effect means the distant agent being judged more harshly than the close agent. Conversely, the more severely the close agent is punished—especially in cases of intentional harm—the more likely they are to be punished more harshly than the distant agent (see Figure 5.5). We found all correlation parameters to be significant ($0.57 < r < 0.77$, $5.33 < t < 9.24$, $p < .001$).

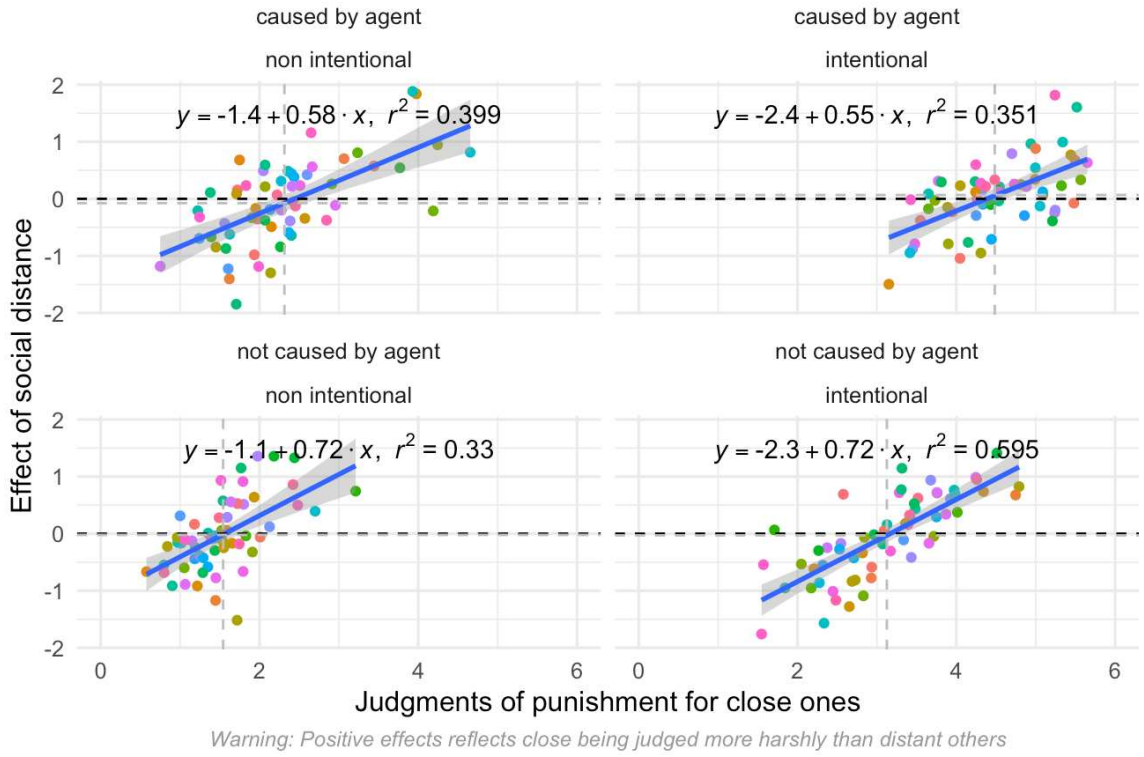
Discussion & Limitations

As a conclusion, we did not observe overall in confirmatory analysis on final responses (part A) any significant influence of social distance to the agent (i.e., close vs. distant). What is worth to be noticed in this experiment is that social distance was manipulated in a within-participant design. Each participant indeed judged in a randomly distributed way across the various moral scenarios both conditions of social distance (i.e., judging a close-one and a socially distant person). This kind of design enables strong and well-powered experiments, thus allowing more robust findings (e.g., Westfall et al., 2014), but here it could also have resulted in participants' higher awareness on what were the expectations for this study. We did not expect that participants may steer their decisions of punishment when guessing that we expected a discrepancy between close vs. distant ones. But we cannot exclude it could have played a role in the current results as a demand characteristic effect (i.e., participants who want to be 'good participants' and unconsciously change their behavior to fit the experimenters' expectancies, see for instance Orne, 2009).

Nevertheless, the hypothesis of research did not always appear as obvious to the participants during the debriefing session. We could also have expected if such a bias was at play that participants exacerbate differences rather than attenuating them.[39] But this behavior would go in the opposite direction of what is socially desirable, that is, appearing as a moral person, hence judging in an equivalent manner the stranger compared to one's close one (i.e., social desirability bias, Edwards, 1953). Even if the current design was keeping participants' anonymity in their answer, we cannot ensure that their desire to appear as impartial did not bias their judgments.

Figure 5.5

Correlational Relationship between Social Distance Effect and Mean Judgments of Close Agents on the Various Scenarios in Exp.1 (Step 2)



Note. Positive effects of social distance mean on this figure that the close agent is being judged more harshly than the distant agent (in accordance with Tend.2), whereas negative effects correspond to the reversed pattern (consistent with Tend.1).

[39] We would then have observed an even higher influence of social distance on judgments of punishment.

Another methodological limitation of this first experimental test of social distance influence in moral decision-making is the potential confound that can have occurred with participants' perspective being not constant across social distance conditions. In the *distant* condition, participants were mere observers of the situation, thus judging the situation within a third-person perspective and with both protagonists being involved in the situation as unknowns. In the *close* condition however, participants were directly involved in the situation as they were the victim of their close one's bad action (see Figure 5.1 as an illustration), thus being in a second-party perspective[40]. In the end, we are then not fully at the interindividual level of analysis purely here (i.e., where the participant and source of judgment has not an active role in the situation). This design mixed up at some point the influence of social distance at the intraindividual level too (i.e., when the self is involved vs. not involved), but regarding the victim of the situation (see further discussion about this in Chapter 6, p. 178).

Findings in the literature suggest that, when being a victim of a harmdoing, people are judging the agent harsher than when being a mere observer (see for instance Fehr & Fischbacher, 2004). This effect could have induced, in the current experiment, a confound that may have compensated the original effect of social distance. Participants in the close condition might have been torn between the desire to forgive the agent because of their social proximity with them and at the same time to punish him or her more harshly because of their status of victim in the situation. Consequently, we cannot clearly conclude through this experiment on the precise reasons why we did not observe the effect of social distance to the agent overall.

However, when looking descriptively at the differentiated effects of social distance throughout the various scenarios displayed (whatever the conditions on intent and causality), we yet noticed very diverse effects both in terms of size and direction. This suggests that more than a design issue (with people being aware of the expectancies or normative

[40] This experimental choice had been made in a first instance on purpose in order to extremize the social distance conditions (i.e., making the close condition as even closer with the participant being directly involved in it, while making the distant condition even more distant with no involvement of the participant). Despite maximizing social distance, this design also triggers many more confounds.

behavior to adopt or with confounding effects of self-statuses as observer vs. victim), one more probable explanation may lie in the variety of moral situations we employed. This study thus alerts us to the fact that both manipulating social distance toward the agent—while keeping the victim's identity constant—and considering a large variety of moral situations may be crucial to study social distance effects in a moral setting.

Experiment 2—Self vs. Close One vs. Distant Other as Agent

Rationale

In this second experiment, we reused a similar design as in Experiment 1, but with a few major changes. First, we did not use the mouse-tracking paradigm anymore, in order to remove the potential noise it could have induced in the first experiment. Second, we added the possibility that the agent perpetrating the wrong act would be the participant him/herself (i.e., participants have then to sometimes judge their own misconduct). Doing so allows to explicitly manipulate social distance both at the intraindividual and interindividual levels (comparing self vs. others as agents and close one vs. distant other as agents). We also slightly adapted the scenarios so that the victim is not varying anymore between conditions of social distance compared to Experiment 1 (i.e., always a stranger).

Finally, we asked the participants to answer two types of moral judgment (i.e., about the wrongness of the act and about the punishment the agent deserves), in order to compare findings for both types of judgment. Findings regarding these two types of judgments will not be reported in the current section as it focuses here on findings about social distance to the agent. But they will be fully reported in Chapter 7, when dealing with type of judgment as a potential moderator of social distance effects (see pp. 236–238). In this second study, we expected regarding the effects of social distance that, (a) the distant agent would be judged more harshly than close or self as agent according to the main tendency of findings in the literature (Tend.1), or less harshly than a close one or oneself as agent according to the Black Sheep hypothesis (Tend.2). Similarly, for self vs. close other comparison, (b) we expected a slight difference in direction of harsher judgments for a close other (according to Tend.1) or harsher judgments for the self (according to Tend.2).

Method

The current experiment follows quite closely that of Experiment 1 (both in terms of materials and design), but we report here below the few changes important to notice. Again, all materials, script of analysis, and data, as well as the preregistration form of the study are accessible on the OSF page of the project (https://osf.io/cyb8k/). The study protocol has also been approved by the local ethics committee.

Power Analysis. In order to determine the appropriate sample size for this study, we conducted an a priori power analysis based on the effect sizes obtained in previous studies for the main effect of social distance (despite a lot of variability in method and operationalization of social distance). We chose in the known literature to keep only studies that are similar in method to the one that is currently planned. That is, experiments with moral scenarios (where a transgressive act is perpetrated), and where social distance is manipulated between the participant and the agent of the perpetrated act (and not toward its victim). The various studies of the literature in this area reported various effect sizes for social distance, ranging from 0.14 to 0.45 in absolute value (equivalent Cohen's *d*). Based on variance partitioning coefficients (VPC) obtained in previous research with similar design (i.e., Gaboriaud et al., 2022), we ran an estimation through PANGEA software (Westfall, 2016) of the statistical power we could reach with the current study design with diverse sample sizes, considering a stimuli sample of 61 scenarios. Through this analysis and with a medium effect size of $d = 0.29$ (Linke, 2012), we found that we could reach a power of 82% with 120 participants and a power of 91% with 160 participants.

However, as the effect size estimation for social distance is again very uncertain, we chose to conduct a sequential analysis following Laken's recommendations (2014), with a maximal sample size of 168 participants (to be a multiple of 42, as participants were recruited by blocks of 42), with an intermediate analysis planned at 84 participants, using a linear spending function (with a general alpha threshold of 0.05 and intermediate alpha threshold of 0.025). For independent technical reasons, the intermediate analysis was made

on 82 participants. As *p*-values for the main effect of social distance was far above the adjusted alpha threshold (alpha of 0.025), we continued data collection until the value of N = 168 participants was reached.[41]

Participants. We recruited for the present study 170 French-speaking participants through the Wirk.io platform, who were rewarded for their participation around 8€ for a task duration of 30 minutes. Data of only 165 participants were finally analyzed as five participants were excluded based on a priori exclusion criteria (see below for more details). The final sample size for this study was then 165 participants (82 females, aged from 19 to 81, M_{age} = 45.07, SD_{age} = 12.59).

Materials. For this second experiment, we used the same materials as in Experiment 1 with vignettes from Leloup et al. (2018). For the current experiment, we chose to keep only three scenarios (rather than four as in the previous experiment) as training, to familiarize participants with the task and especially with the type of situations that could be encountered throughout the task[42]. Contrary to the first experiment, where all conditions of intent and causality could be encountered (as in Leloup et al.'s original study, 2018), the current one only displayed to the participant the *accidental harm* versions of all scenarios (i.e., where the act is non-intentionally perpetrated and triggers a harmful outcome directly caused by the agent). Controlling for the scenario version allowed us to focus on social distance effects, while minimizing the preponderant effect of intent (as seen earlier in Exp.1).

We again manipulated social distance toward the agent by creating new versions of each scenario, one involving a close other from the participant's perspective, the other involving a distant other. We however improved the operationalization of the close one and

[41] We deviated a bit from what had been preregistered (i.e., only one intermediate analysis rather than two at 84 and 126) because it seems that we were far from reaching the expected effect of social distance at N = 82.

[42] Contrary to the first experiment, this second study is not implemented via a mouse-tracking paradigm, thus less training was requested for participants before going through the task. We however used this training session as a way to show participants a diversity of severity in terms of situation outcome.

stranger identity by labelling the close one as 'Your friend'[43] and labelling the stranger as 'Mr. or Mrs. X' (thus involving even more closeness with the close one, and even more distance with the stranger, knowing nothing from them outside their gender). We also allowed for the participant to be in some scenario themselves the agent of the situation, in order to compare judgments toward oneself vs. another who is close vs. another who is distant. Finally, the victim in the scenarios was controlled for, so that it always remains an unknown person from the participant point of view, in direct improvement of Exp.1's design confound.[44]

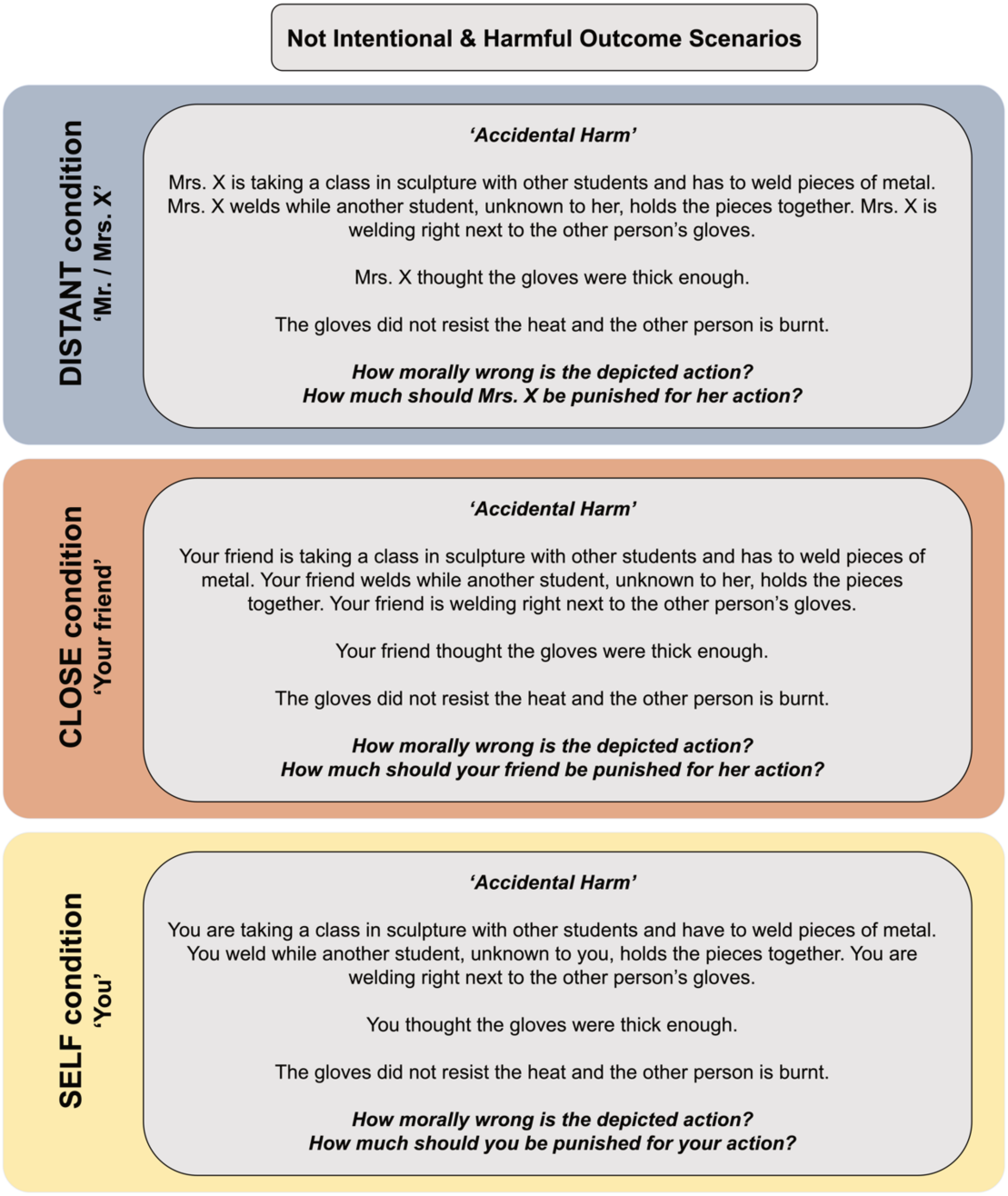
We also kept from Exp.1 the minor changes in terms of scenarios phrasing, to adapt it to the French culture. We determined the gender of the various involved protagonists (i.e., the participant's friend, M. or Ms X, and the unknown victim—except for the participant) in accordance with the gender of the protagonists in the original scenarios from Leloup et al. (2018). In the original material, half of the scenarios (x32) involved male protagonists whereas the other half (x32) involved female protagonists, so was the case in the current study (independently of the participant's gender). Again, a complete example of a scenario translated in English is given in Figure 5.6 within the three possible versions of social distance. The total amount of stimuli for this study (all versions of all scenarios included) is then 192 (i.e., 64x3) distinct stimuli, hence 183 without the training ones (see the entire adapted material in the OSF project page).

[43] We asked participants in the beginning of the experiment to think about two of their closest friends (i.e., one female, one male) and to imagine the corresponding person at each occurrence of 'Your friend' in the scenarios.

[44] Some scenarios have gone through an important adaptation so that it would be believable to have an unknown victim in it. Some scenarios originally involved a situation between friends, colleagues, or roommates, we adapted them by assuming the person was a new acquaintance, colleague, or roommate etc.

Figure 5.6

A Scenario Example in Exp.2 in the Three Conditions of Social Distance



Procedure & Design. Participants were recruited online on the Wirk.io platform and then redirected to the online questionnaire supported on the Limesurvey software. They first had to give the name of their closest friends (one female, one male) and were asked to think about them each time their friend would be involved in the depicted situation. They then started the experiment by judging three training scenarios (i.e., one in each condition of

social distance) and should afterwards rate on a continuous scale the *perceived social proximity* with the involved protagonists from *not close at all* (0) to *very close* (6) (see the Appendix in the OSF page for more). Participants could then read and answer one by one the 61 remaining moral scenarios, without time constraint (contrarily to Leloup et al.'s procedure, 2018). For this experiment, we asked participants to give two types of moral judgment for each situation, one about the wrongness of the act, the other about the punishment that the agent would deserve for their action. To allow for more nuanced responses compared to Likert scales (see Gautheron et al., 2023), responses were given on continuous scales (i.e., sliders) ranging from *not wrong at all* (0) to *totally wrong* (6) for wrongness judgments and from *not punished at all* (0) to *very strongly punished* (6) for punishment judgments.

We chose not to counterbalance the order of the two types of judgments (i.e., wrongness, then punishment judgments), even if some asymmetrical effects have been demonstrated depending on judgment presentation order (Leloup et al., 2018). What has been found is that punishment judgments are slightly harsher in the punishment-wrongness order compared to the wrongness-punishment order, especially for accidental harm scenarios (Leloup et al., 2018, Experiment 2). But it should not induce further variability as we did not manipulate intent and outcome in the current experiment, contrary to the original study. Three attention check items were also distributed across the whole task to ensure that the participant still focused on the ongoing task (one every 15 items). In the middle of the task (i.e., after the second attention check item), we also added a reminder of the friends' name that participants gave in the beginning of the study.

The current study followed a variant of a counter-balanced design—i.e., within participants and within stimuli—but not fully crossed to avoid block effects, with two fixed factors—social distance (self, close, distant) and type of judgment (wrongness, punishment), one control variable (i.e., the participant's gender), and two random factors (i.e., participant and scenario). The social distance factor was compounded with three modalities depending on the agent's identity from the participant's point of view: the self (i.e., you) vs. a close other

(i.e., your friend) vs. a distant other (i.e., Mr. or Mrs. X) whereas the type of judgment and participant's gender only had two modalities each. Each participant had to give two moral judgments per scenario (one of wrongness and one of punishment). As an additional dependent variable, we registered the total response time and response time by scenarios to ensure that participant did not take too short time to answer our survey. These data were, however, not analyzed in the current study as our hypotheses did not rely on them.

The 2(type of judgment)-by-3(social distance) [=6] conditions were thus presented in a partially counter-balanced way: Each participant answered both types of judgment for each scenario, but each of the 61 scenarios was randomly presented only once in one out of the three conditions of social distance. We pseudo-randomized the distribution of the scenarios within the social distance conditions, so that each participant treated approximately 20 scenarios in every social distance condition in a random order and so that each scenario was not treated by each participant under the same condition (see Method section on OSF for more).

Analytical Strategy. We used linear mixed models (LMM) to analyze the current data in order to generalize the results not only to other participants but also to other moral stimuli (Judd et al., 2012). Indeed, it seems to be a good strategy when we have data with large inter-participants and inter-stimuli variabilities, as it is often the case when dealing with moral materials. *R* language (version 4.2.2, R Core Team, 2021) and *lme4* package (Bates et al., 2014) were used as well as some other associated packages. The maximal model random structure (including all factors and random terms) was reduced following the procedure of Matuschek et al. (2017) in order to obtain a parsimonious model that fits well with the data and whose random structure is supported by the data. The final reduced model to run the analyses was: $\text{Judgment} \sim \text{DST} * \text{TYPE} * \text{GENDER} + (\text{DST} + \text{TYPE} \mid \text{P}) + (\text{DST} * \text{GENDER} + \text{TYPE} \mid \text{S})$.

We coded social distance as a Helmert contrast, to test (a) on the one hand the effect of social distance for judgments toward the self or a close friend vs. a stranger (Dst1: *self* = -1/3 & *close* = -1/3 vs. *distant* = + 2/3) and (b) on the other hand, the effect of social distance

for judgments toward the self vs. a close friend (Dst2: *self* = - 0.5 vs. *close* = + 0.5; *distant* = 0). Type of judgment and participant's gender were both binary coded (Type: *wrongness* = - 0.5 vs. *punishment* = + 0.5 ; Gender: *male* = - 0.5 vs. *female* = + 0.5). The dependent variable (DV) for this study was the moral judgments given by participants, on a continuous 0-6 range (with a two decimals precision). Through the LMM reported above, fixed effects were thus estimated for both contrasts of social distance (i.e., Dst1 and Dst2), for the type of judgments, and for participants' gender as well as their respective interactions, while only some of these effects were kept as random effects, in addition to all correlations between random estimates for both participants [P] and stimuli [S].

By model comparison approach and degrees of freedom estimation (e.g., *lmerTest*, *emmeans* and *car* packages), the *p*-values associated with the different effects of interest were calculated. The alpha threshold was set at 5% to determine the significance of the effects. However, according to the procedure described by Lakens (2014) for sequential analysis, we computed through the R-package *GroupSeq* the alpha threshold for the first intermediate analysis (alpha = 0.025) and then, corrected the final *p*-value of the effect of interest (i.e., Dst1) according to the computed upper bound value. Effect sizes are reported as Cohen's *d*, adapted to linear mixed models (Judd et al., 2017).

As preregistered and mentioned above, we a priori excluded from the analysis one participant who took less than 5 minutes to answer the whole questionnaire (whereas it should have taken around 30 minutes overall). Another four participants were also excluded for having not successfully answered at least two out of three attention check items, leaving a final sample of 165 participants. Missed trials were also filtered (when participant answered one scenario in less than 2 seconds, which seems unrealistic given the scenario format[45]). No clear outlier was found both on the scenario and participant distributions of random effects, thus keeping a stimuli sample of 61 scenarios and a participant sample of 165 participants.

[45] This could be due to handling errors; we did not therefore remove the complete data for these participants but only the trials missed.

Results

As a reminder, we expected in this experiment to find an effect of social distance between distant other vs. close one or oneself as agent, either in direction of more severe judgments for the former (Tend.1) or more severe judgments for the latter (Tend.2). Table 5.1 provides a summary of the descriptive means and standard deviation for moral judgments toward the agent depending on the social distance and type of judgment conditions.

Table 5.1

Descriptive Statistics for Moral Judgments to the Agent Depending on the Type of Judgment and Social Distance Conditions

Agent Condition	**Wrongness judgments** *M* (*SD*)	**Punishment judgments** *M* (*SD*)
Self	2.39 (1.96)	2.47 (1.93)
Close	2.26 (1.90)	2.32 (1.90)
Distant	2.40 (1.94)	2.48 (1.91)

Social Distance Perception. First of all, we checked whether social distance was perceived by participants consistently with our manipulation, that is to say, whether the specific friends declared by the participants were rated as socially closer to them than Mr. and Mrs. X. In order to do that, we graphically (see Figure 5.7), then statistically[46] checked the ratings regarding the perceived proximity of the various protagonists (i.e., just after the training items, see above in the 'Procedure' section). We found a main effect of social distance (on social proximity perception), b = -3.18, SE = 0.16, $t(163)$ = -20.03, $p < .001$, 95% CI [-3.49,-2.87], d = -2.17, such that participants indeed rated close protagonists (i.e., their friend—either male or female) as closer to them (M = 4.66, SE = 0.10, with higher

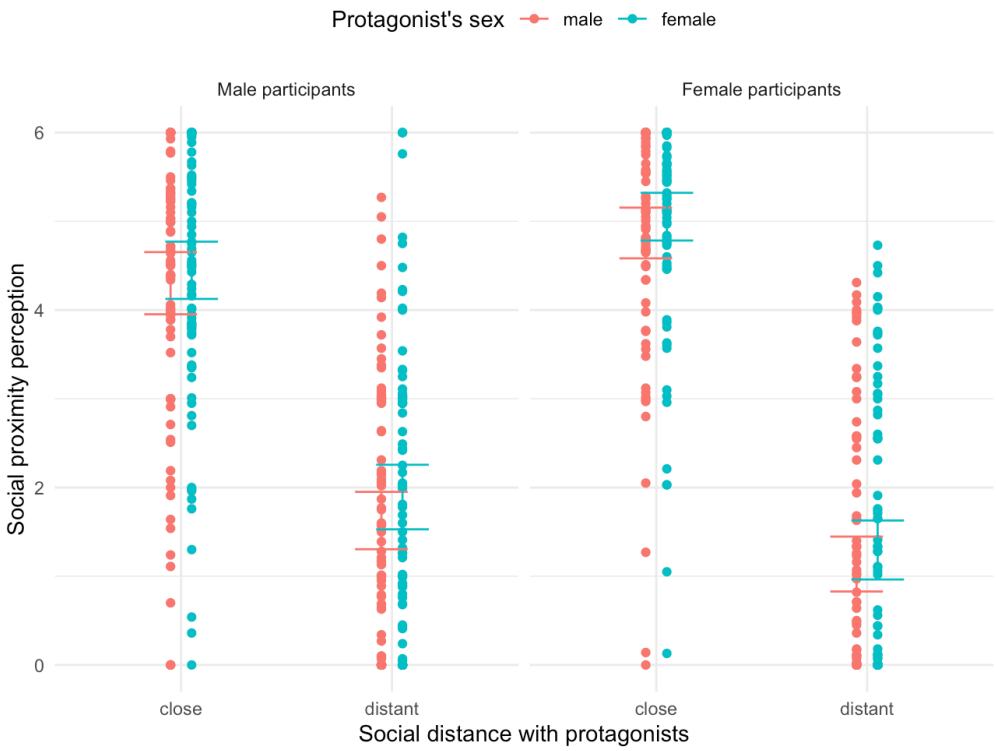
[46] To do this, we used a mixed model analysis with participant only as random factor and with social distance, protagonist's sex, and participants' gender as fixed effects [Dst_percep ~ dst*sex*gender + (dst + sex | pp)].

ratings indicating higher proximity) than strangers (i.e., Mr. or Mrs. X) (M = 1.49, SE = 0.11, see Figure 5.7).

We also noticed that male protagonists (M = 2.98, SE = 0.08) were judged as slightly less close than female protagonists (M = 3.17, SE = 0.08) whatever the participant's gender and the effective manipulated social distance with them was: b = 0.19, SE = 0.07, $t(163)$ = 2.65, p = .0089, [0.05,0.33], d = 0.13. Additionally, an unexpected interaction effect between social distance (manipulated) and participants' gender on the perceived proximity with the protagonists was revealed, b = 1.13, SE = 0.32, $t(163)$ = 3.56, p < .001, [0.50,1.76], d = 0.38. Judging close protagonists as socially closer than distant protagonists was even stronger for female participants (b = -3.74) than for male participants (b = -2.62), thus giving more polarized judgments for female participants (cf. right side of Figure 5.7).

Figure 5.7

Social Proximity Perception Depending on the Effective Manipulation of Social Distance, the Sex of the Protagonist, and Participants' Gender

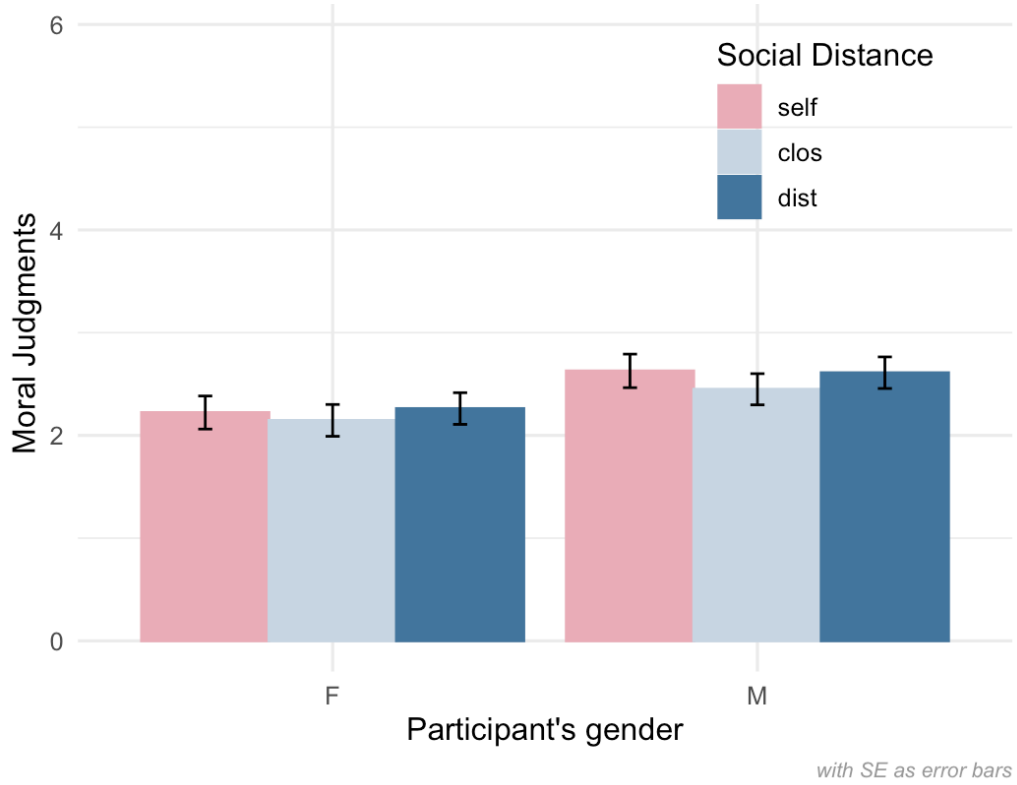


Note. Social proximity as perceived by participants is measured on a 0-6 range continuous scale from (0) *Not close at all* to (6) *Very close*.

Confirmatory Results. On final moral judgments, we found as expected a main effect of social distance on the first contrast (i.e., Dst1: comparing judgments toward a distant agent vs. a close one or the self as agent), *b* = 0.07, *SE* = 0.03, *t*(88.7) = 2.20, *p* = .03, 95% CI [0.007,0.14], *d* = 0.039 (*p* = .048 after correction for one intermediate analysis). This effect—even if quite low in effect size—goes in the expected direction, with moral judgments toward distant agents (*M* = 2.44, *SE* = 0.13) being more severe than toward close or self as agents (*M* = 2.36, *SE* = 0.13) (see Figure 5.8). Moreover, the difference between moral judgments toward a close agent vs. the self as agent is also significant (Dst2 contrast), *b* = -0.13, *SE* = 0.04, *t*(111) = -3.00, *p* = .003, [-0.21,-0.04], *d* = -0.07 (*p* = .027 after correction). Quite surprisingly, because we hypothesized this direction according to the only minor tendency of the literature (Tend.2), we found that participants were judging themselves (*M* = 2.43, *SE* = 0.14) more severely than their close friends (*M* = 2.30, *SE* = 0.13) when in the role of the perpetrator of a wrong behavior (see Figure 5.8).

Figure 5.8

Results in Exp.2 about Social Distance Effects on Moral Judgments Depending on Participants' Gender



Note. Moral judgments were measured on a 0-6 range continuous scale from (0) *Not severe at all* to (6) *Very severe* judgments.

Exploratory Results About Gender. Even if we had no a priori hypothesis on the effect of gender, we detected a main effect of the participants' gender on moral judgments, with $b = 0.35$, $SE = 0.17$, $t(165) = 2.13$, $p = .03$, $d = 0.18$, so that male participants ($M = 2.56$, $SE = 0.15$) judged globally the involved agent more harshly than female participants did ($M = 2.21$, $SE = 0.15$) (see Figure 5.8, right vs. left panels). This effect was not qualified by any interaction effect. However, when looking exploratorily at the pairwise comparisons, we noticed that the effect of social distance about self vs. close friend judgments (Dst2) tended to be stronger for males ($b = -0.18$) than for female participants ($b = -0.08$). This suggests that the main effect of social distance comparing judgments toward the self vs. a close friend as agents (Dst2) was probably mainly driven by the effect of this factor among males.

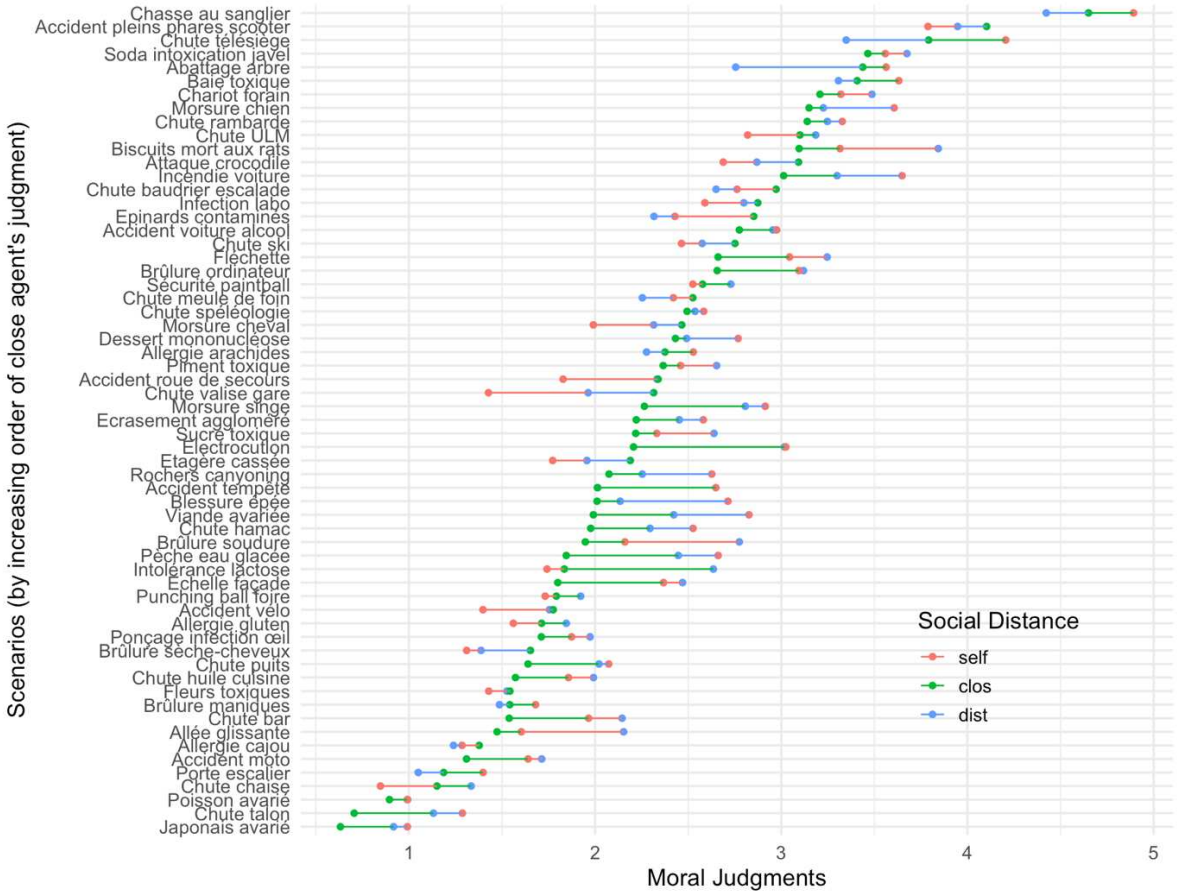
Exploratory Results About Scenarios. As done for Experiment 1, we also displayed the visual differentiated effects of social distance depending on the various scenarios (as we reused the same moral battery than in Exp.1, adapted from Leloup et al.'s study 2018). Over all conditions of judgment type (wrongness vs. punishment) and participants' gender (male vs. female), this exploratory analysis revealed again very diverse effects of social distance (be it between self and close one, or between these conditions and the distant agent). Figure 5.9 displays social distance effects depending on scenarios in increasing order of the close agent judgment. Also note that, compared to the previous experiment, all scenarios were presented in the current study under the versions of non-intentionally perpetrated harm but caused directly by the agent's action.

As it can be visualized, a few scenarios only (~6/61 times) seem to trigger an influence of social distance in direction of harsher moral judgments for distant agents than for close ones and then for close ones than for self as agents (i.e., red then green line pattern, in accordance with Tend.1 predictions). The reversed pattern is also only rarely encountered across the various scenarios (~7/61 times), that is, where the self as agent is

judged harsher than a close one, then close one being judged harsher than a distant other (i.e., blue then green line pattern). In the end, the most frequent pattern (i.e., green then blue or red lines), consistently with confirmatory findings reported above, corresponds to cases where close ones were judged overall less harshly than self or distant others as agents (~30/61 times), with the other cases mostly corresponding to almost null effects.

Figure 5.9

Descriptive Effects of Social Distance in Exp.2 on Moral Judgments Depending on the Various Scenarios



Note. Scenarios are displayed on the vertical axis in increasing order of punishment judgments toward the close agent (i.e., your friend). On the horizontal axis, judgments of punishment are displayed from more lenient judgments (0) to more severe judgments (6)

Discussion & Limitations

Experiment 2 results let us think that—for the same wrongdoing—people judge themselves harsher than they would do for a close other, but that when judging themselves or a close other compared to a stranger as the perpetrator, they would judge the socially distant stranger more severely. Quite surprisingly, both tendencies described in the introduction section of this dissertation are represented here: On the one hand, the first result is consistent with the 'hypercrisy' hypothesis and the agent-regret model that goes with it (i.e., Tend.2); on the other hand, the second result is consistent with the predominant 'hypocrisy' hypothesis and self/close one protection (i.e., Tend.1). In summary, it seems from this Experiment 2 findings that a U-curve pattern appears when comparing self vs. a close one vs. a distant other, with, in case of everyday moral transgression, a certain favoritism for close agents (compared to both a distant other and oneself as agents).

However, we should notice that the current significant effect of social distance to the agent is overall very low in size (d = 0.04). It may stem from the large number and diversity of scenarios that were currently used (as for Exp.1) compared to what is usually done in the moral psychology literature. As found in the scoping review findings, many studies investigating this question rely only on a limited number of situations, sometimes even only one (see Chapter 3, pp. 99–100). As visualized by the descriptive distribution of social distance effects across the diverse scenarios (Figure 5.9), the effect of social distance to the agent in this study was sometimes quite large (almost one point of difference on the 0-6 range scale). But these highly distance-sensitive cases may have been compensated by the other scenarios triggering only very weak (even null for some) effects of social distance. The general pattern of social distance effects may also have been weakened due to the lack of consistency in its direction (i.e., sometimes self > close < distant, but sometimes also self < close < distant, or self > close > distant).

Moreover, we should recognize the current experiment lacks some external validity, although hardly avoidable in moral psychology experimental investigations. The participants were indeed aware that the currently presented situations were only hypothetical, and thus,

that their judgments would not have concrete consequences in their own real life. To limit this bias, we asked participants to give two of their closest friends' names and to remind that when we would refer to 'their friend' during the experiment, they had to think about the situation as if one of the previously named friend was involved. However, to strengthen this association, we could have used replacement texts in the experiment code to 'paste' directly the given names within the scenarios, thus personalizing them for each participant. Beyond the fact it would have require some new adaptations in material (to fit with specific names), we do not think yet this would have really influenced the results.

Regarding the exploratory findings about participants' gender, we found that male participants judged the agent overall slightly harsher than females, and that—although not significant—the difference of judgment between self and close others as agent was mainly driven by males' responses (vs. females). This may suggest, pending further confirmatory results, that males are more sensitive to social distance biases than females, at least when manipulated toward the agent of the situation. As not the first focus of this dissertation, we further refine this possibility in the final 'General Discussion' chapter (see Chapter 9, p. 339).

At the Intraindividual and Intergroup Levels

Rationale and Overview of the Experiments

We aimed in this section to tackle the effects of social distance at the intergroup level (compared to the effects previously found at the intra- and interindividual levels), to integrate and reconcile the disparate effects of social distance at this level of analysis. In particular, two tendencies were found in the literature: One favoring the ingroup members over outgroup members in case of transgression (Tend.1), the other favoring the outgroup members—or at least disfavoring the ingroup deviant members in line with the Black Sheep effect (Tend.2). Even if both tendencies are not specifically antagonist when found for different valences of moral situations (either prosocial acts or transgressions, see Marques et al.'s theoretical rationale, 1988), it becomes conflictual as soon as we find both patterns within one type of moral situations (e.g., in moral transgressions).

McKee and collaborators (2023) found in a large-extent cross-cultural study what they called an *insider agent effect,* that is, the fact that moral transgressions committed by an ingroup member are considered as less morally wrong than the same transgressions committed by an outgroup member. Earlier, Schiller et al. (2014) found quite the same effect by demonstrating that outgroup individuals were punished harsher than unspecified individuals (i.e., who do not share explicitly a group membership), and ingroup individuals less punished overall than unspecified individuals. These examples then support the first tendency of literature, evidencing a general ingroup favoritism bias, even (or especially) in case of moral transgression.

However, as evidenced in the scoping review preliminary results (Chapter 3, pp. 87–103), we found a non-negligible number of research highlighting the reversed effect of social distance, that is, showing harsher judgments for ingroup compared to outgroup transgressors (in line with Tend.2). This is the case notably in Tang et al.'s recent research (2023), demonstrating across a series of studies that ingroup transgressors were judged more harshly by participants than outgroup transgressors, especially in case of harmful moral transgressions, and not or to a lesser extent for weak or non-moral acts. The authors also argue this bias appears especially when the transgression is relationally framed, that is, directly threatening the group social cohesion, in accordance with the Black Sheep theory (2023, Study 4). Bettache et al. (2019) also found this ingroup *un-favoritism* bias by participants strongly identifying with their religious group, but found this was only true in case of harmless moral transgressions and not for harmful ones, in contrast to Tang et al.'s findings (2023).

Again, in front of these disparities of findings across the literature at intergroup level, we aimed through two novel experiments making these contradictions clearer. We first built in Experiment 3 (N = 152) a replication of the previous Experiments 1 and 2, but at the intergroup level (i.e., judging an ingroup vs. an outgroup member as the agent), using the same hypothetical scenarios database. Experiment 4 (N = 69) then investigates the impact of the same factor (social distance toward the agent) but integrating the self as a possible

agent in addition to an ingroup or outgroup member (i.e., intraindividual and intergroup levels combined). This last experiment was furthermore implemented within an online cooperative game to enhance the ecological validity of our experimental design (i.e., not with hypothetical scenarios anymore but within a real-like multiplayer card game).

Experiment 3[47] (Part A)—Ingroup vs. Outgroup Member as Agent and Self as Observer

Rationale

In Experiment 3, we aimed to replicate findings from Experiment 2 regarding the effects of social distance toward the agent but within a manipulation of social distance at the intergroup level. We then reused the materials and design from our previous studies, but adapting the moral situations to make participants believe they are judging an ingroup vs. an outgroup member. To induce group membership, we beforehand implemented a minimal-group paradigm (MGP). This empirical endeavor was purposed to help us disentangle the contradictory effects of social distance at the intergroup level, by directly comparing within a moral transgressive context, judgments given to close (ingroup) and distant (outgroup) agents.

In the current experiment, we also manipulated social distance toward the victim of the moral situation, to investigate the potential interference and confounding effects of social distance toward both the agent and the victim. This factor influence will however not be analyzed in the current chapter, which relates to social distance effects toward the agent. The corresponding analyses and findings for social distance to the victim effects are reported in Chapter 6 (pp. 211–218). Regarding the agent's social distance effect, we expected—based on our previous findings and predominant effects in the literature—that the close (ingroup) agent would be less harshly judged than the distant (outgroup) agent.

Method

The current experiment re-uses Experiments 1 and 2 materials and design, but integrated within a minimal group paradigm, in order to induce group membership from the

[47] We especially thank Chloé Bernard as well as Zoé Szczepaniak, who provided a precious help for the data collection of this experiment.

participant's perspective. All materials, script of analysis, data, and preregistration form are available in the OSF page of the project (https://osf.io/4sym5/). The current study was also approved by the local ethical committee.

Power Analysis. In order to estimate how many participants we needed to recruit for this study, we conducted a power analysis based on the previous effect sizes obtained for social distance in our previous similar studies (with separated manipulation of social distance toward the agent and toward the victim respectively).

The estimated Cohen's *d* was of 0.04 for the effect of social distance when manipulated between the participant and the agent in our previous study (between conditions of self or close one vs. a stranger, see above results from Exp.2). It was, in the contrary, estimated to *d* = 0.27 when manipulated between the participant and the victim of the situation on judgments of wrongness (with a different material but a similar analytical procedure). We had however no previous estimation of the size of the interaction effect between social distance manipulated toward the agent and toward the victim.

We used the online application PANGEA (version 0.2, Westfall, 2016) to estimate the statistical power for the current study design within diverse sample sizes, considering a base of 60 scenarios and the variance partitioning coefficients found in the above-mentioned previous studies (VPCs). We estimated we could reach the minimal effect size of interest (SESOI) *d* = 0.04 for social distance to the agent with *N* = 60 (with a power of only 11%), and with *N* = 800 reaching a power of 25%. With the effect size found in previous findings for social distance between the participant and the victim, we could reach a power of 99% as soon as *N* = 60. We also ran an analysis of power with an intermediate effect size *d* = 0.13 (considering that the current study manipulates social distance toward both the agent and the victim of the situation).

Given also the limited funds to reward participants, we fixed a maximal reference sample size at *N* = 220 to reach 95% of statistical power with such an intermediate effect size. As for previous experiment, we conducted sequential analyses (Lakens, 2014), with intermediate analyses processed at around 73 and 146 participants (with a general alpha

threshold of 0.05 and intermediate alpha threshold of 0.022). Although at the second intermediate analysis the agent's social distance effect size was beyond the a priori fixed SESOI in absolute value (i.e., d = -0.07), we decided to stop data collection at that point, both for reasons of limited funds and because we detected some unexpected dependencies in our experimental design (see 'Analytical Strategy' below).

Participants. We recruited 161 French-speaking participants (106 females, 55 males, aged from 18 to 73, M = 28.59, SD = 13.05) on the local university campus[48]. As the data collection was during the summer, we had access mainly to academic staff (and not only to university students). Participants were rewarded for their participation 10€ for forty-five minutes to one hour task long (briefing and debriefing sessions included). Participants provided informed consent to participate. The research was conducted in accordance with ethical guidelines of the American Psychological Association and was approved by the local ethics committee.

Materials. We again used the French scenarios database from Leloup et al. (2018), in order to be easily comparable with previous experiments. As for Experiment 2, we chose to keep only the *accidental harm* version of each scenario (i.e., non-intentionally perpetrated transgression but with harmful consequences) and the *intent-outcome* order of presentation (as the most intuitive). As a dependent variable, we only kept judgments of punishment as we did not want to further complexify the study design. The main change in materials compared to previous experiments is at the level of the agent's and victim's identities, which were manipulated here in terms of social distance at the intergroup level. Indeed, the agent and the victim for each scenario could either be a member of the ingroup or a member of the outgroup from the participant's perspective.

The group of membership was induced through a minimal-group-like paradigm. To do this, we chose to rely on the material created by Gregg et al. (2006). They used two fictitious social groups: the 'Luupites' and the 'Niffites' with their four respective members.

[48] Second intermediate analysis was planned at N = 146 participants, we yet welcomed some additional participants in order to prevent further exclusions during data analysis and to check the counterbalancing design of our diverse conditions.

Luupites members have all two consecutive vowels in their name and ends with 'lup' (e.g., Maasolup). Whereas *Niffites* members' names have all two consecutive consonants and end with 'nif' (e.g., Cellanif). We added one supplementary member for each group, in order to have a possible counterbalanced proportion of Luupites' et Niffites' names in the agent and victim positions.[49]

As for minor changes, we kept from previous experiments the minor changes made in vocabulary. You can find the whole materials (with placeholders X for agent's and Y for victim's names) in the Method section of the OSF page. We also provide in Figure 5.10 below a complete example of scenario translated in English with the four possible versions of social distance manipulated toward both the agent and the victim (Ingroup agent & Ingroup victim; Outgroup agent & Outgroup victim; Outgroup agent & Ingroup victim; and Ingroup agent & Outgroup victim).

Procedure & Design. Participants recruited for this experiment were welcomed to the laboratory to pass the experiment in individual cubicles. Before and after the experiment, participants go respectively through a briefing and debriefing session with the experimenters. The developed computerized protocol was built as follows in order of appearance for participants (see also Figure 5.11 below for a visual representation of the procedure).

Participants first went through a questionnaire about their personality traits in 10 items (BFI-10—French version validated by Courtois et al., 2020) (see step 1 in Figure 5.11). We then made participants believe their responses were automatically analyzed and that they had been categorized into two groups (supposedly along with other participants) based on their responses to the personality test[50]. After that, we introduced them visually the two groups (the *Luupites* and the *Niffites*), as well as their respective members' names, which have been supposedly anonymized, to keep participants' future response not influenced (cf.

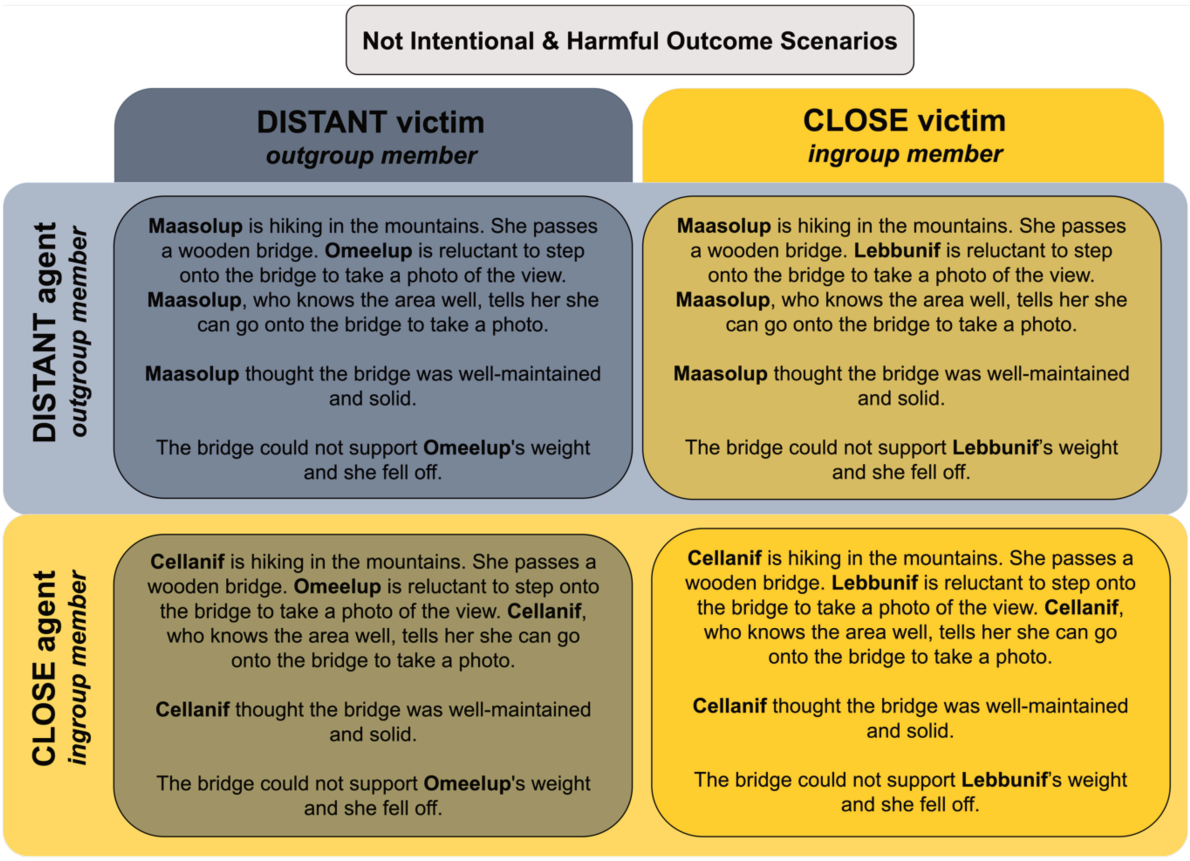
[49] These fifth members' names respect the rules depicted by Gregg and colleagues (2006) and were developed for another line of research (from lab collaborators in an unpublished work). Final Luupites members' names were Maasolup, Neenolup, Omeelup, Wenaalup and Tuuralup. Final Niffites members' names were Cellanif, Eskannif, Lebbunif, Zallunif and Otrannif.

[50] We more specifically argue that one dimension of personality has been specifically retained to build up these groups.

step 2 of Figure 5.11). We rendered explicit the link existing between a member's name and its group (i.e., the common syllable).

Figure 5.10

An Example of Scenario in Exp.3 in the Four Possible Conditions of Social Distance



Note. We consider here that the participant's group of membership is the *Niffites* family, hence ingroup (close) member being a *Niffite*. Note however that participants were either members of the *Niffites* or of the *Luupites* family (counterbalanced between participants).

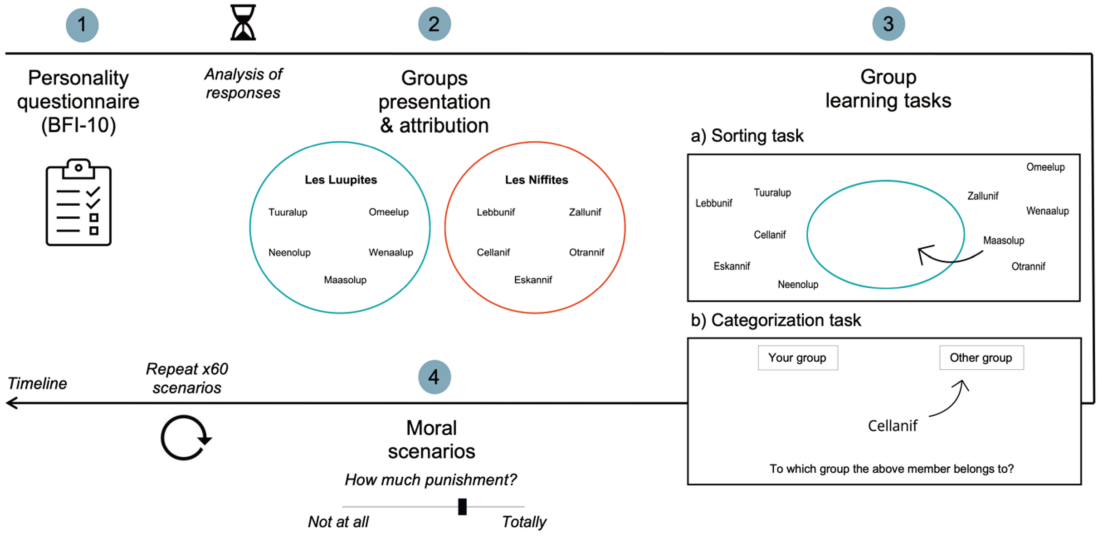
Then, participants were trained to recognize which members were part of their group and which ones were part of the other group during two tasks (step 3): (a) A first sorting task where participants should sort the various members inside or outside a circle depending on their group membership as explained during the instructions; (b) A second categorization task where participants see one by one all members' names and have to classify them either in their 'ingroup' or in the 'outgroup', hence requiring them to remind their own group of membership. Participants went then through the moral task (step 4), firstly with four training

scenarios and then with the 60 remaining scenarios. For each situation, they had to judge how much the agent has to be punished for the committed wrongdoing on a scale from (0) *Not at all* to (100) *Totally*. The protagonists (i.e., the agent and the victim) are members that have been previously depicted as either an ingroup member or an outgroup member (i.e., a Luupite's or Niffite's name). Participants finally self-reported how they thought they were influenced by agents' and victims' membership in their judgments as well as to indicate some demographics (i.e., gender and age).

We also checked the attention of the participants during this last part of the study by adding four attention check items (i.e., one after each 15 trials). At the same occasion, we consolidated participants' memory of their own membership with, after each attention check, two trials where participants were asked to recall which group a particular member belongs to (i.e., their own group or the other one). Participants were at the end of the experiment debriefed especially regarding the real purpose of the study as well as the cover story that had been used[51] (i.e., no real impact of the answers they gave in the first part on personality dimension, and thus no real group affiliation), then they were rewarded for their participation

Figure 5.11

Schematic Representation of the Procedure Followed in Exp.3



[51] Note that we used a funnel debriefing technique to check whether the cover story had been believed by the participant before revealing the deception. Especially, they were firstly asked 'What do you think was the personality dimension in the questionnaire used for attributing group membership?'.

This study followed a variant of a counter-balanced design (i.e., within-participant and within-stimulus design, but not fully crossed to avoid block effects). The varying agent's and victim's identities (i.e., part of participants' ingroup vs. outgroup) were thus manipulated within participants and within scenarios. In other words, participant saw the two possible agent's identities crossed by the two possible victim's identities (= 4 conditions of social distance) throughout the 60 randomly presented scenarios. Each scenario was also treated under all four social distance conditions among all participants (i.e., within-scenario design). However, one scenario was uniquely treated once by each participant (thus not a fully counterbalanced design). Moreover, the participant's group of membership was also counterbalanced between participants, such that the participant was either categorized in the 'Luupites' or in the 'Niffites' group in the beginning of the experiment (supposedly depending on their response in the personality test, but in fact, within a 50/50 distribution).

Analytical Strategy. The participant sample for this study was *N* = 161. We however checked, as preregistered, whether some participants data were too low in reliability to be considered, via the various attention and manipulation check items displayed throughout the study. In accordance with what was preregistered, we found no more than two mistakes on the categorization task over all participants, thus no data to exclude on this criterion. We also checked the seriousness of answers on the personality test at the beginning of the experiment[52], revealing no systematically identical responses.

We computed the number of successful attention checks items. We had preregistered to exclude participants who failed more than one attention check item (out of four). However, we a posteriori revised this criterion as several participants failed these items, despite their intuitive and easy aspect. When taking the strict preregistered criterion of at least one failed attention item, 15 participants' data would have been to exclude, and still 4 participants' data to exclude when considering the minimal criterion of four failed attention items. As a compromise for the following analyses, we considered only data from

[52] Participants answering with the same response for all personality questions would have been suspected of not having responded seriously, thus weakening the manipulation of social distance—as the affiliation to one group was supposedly made based on these answers.

participants who succeeded at least two out of the four attention items, hence 7 participants' data excluded on this criterion.

Similarly, several mistakes were made by participants on manipulation check items (where participants had to recall their group membership or those of other members). Five participants failed all manipulation checks, whereas 5 others only missed one out of the four manipulation checks. Deviating from preregistration (as removing these 10 participants would quite significantly reduce the final sample), we decided to exclude only those who failed all manipulation items (N = 5). Combined with the criteria on attention items as defined above, there were in total 9 participants we did not keep for main analyses below (i.e., with either attention check, or manipulation check, or both criteria not met). The following reported results are then the output of analyses on the reduced sample (N = 152) as they appeared also to be the most conservative ones. However, for analyses robustness check, we also performed the whole sample analyses (N = 161) and reported it in supplemental material (available on OSF).

For these analyses (both on reduced and full sample), we used linear mixed models with two random variables in order to generalize the results not only to other participants but also to other moral scenarios (Judd et al., 2012). R language (R Core Team, 2021) and lme4 package were used (Bates et al., 2014) as well as some other associated packages. We named and coded the two fixed factors (i.e., social distance toward the agent [dst.perp] and toward the victim [dst.vict] respectively) by contrasts: (-0.5) for close—when the involved protagonist is an ingroup member—vs. (+0.5) for distant / outgroup member. We started from the maximal model and reduced its random structure following recommendations of Matuschek et al. (2017), thus obtaining the final reduced model:

$$Judgments\ of\ punishment \sim dst.perp * dst.vict * ingroup + (1 \mid participant) + (ingroup{:}\ dst.perp + dst.perp{:}\ ingroup{:}\ dst.vict \mid scenario)$$

As can be noticed, we also added in the model, compared to what was preregistered, the participants' group of membership as a control variable (i.e., [ingroup] factor), although not supposed to have any effect on moral judgments but only to ensure this variation did not explain some variance in

the results. By models' comparison approach and degrees of freedom estimation (e.g., *lmerTest*, *emmeans* and *car* packages), the *p*-values associated with the different effects of interest were calculated. The alpha threshold was set at 5% to determine the significance of the effects.

Results

As a reminder, we expected, in this experiment at the intergroup level, that participants would judge the agent more harshly when it is a member of the outgroup compared to when an ingroup member is involved as the agent of the wrong action. Before reporting findings about this effect, we should mention the results about how social distance was perceived, both via the manipulation check items distributed throughout the experiment and via the self-reported item about social distance impact in participants' responses.

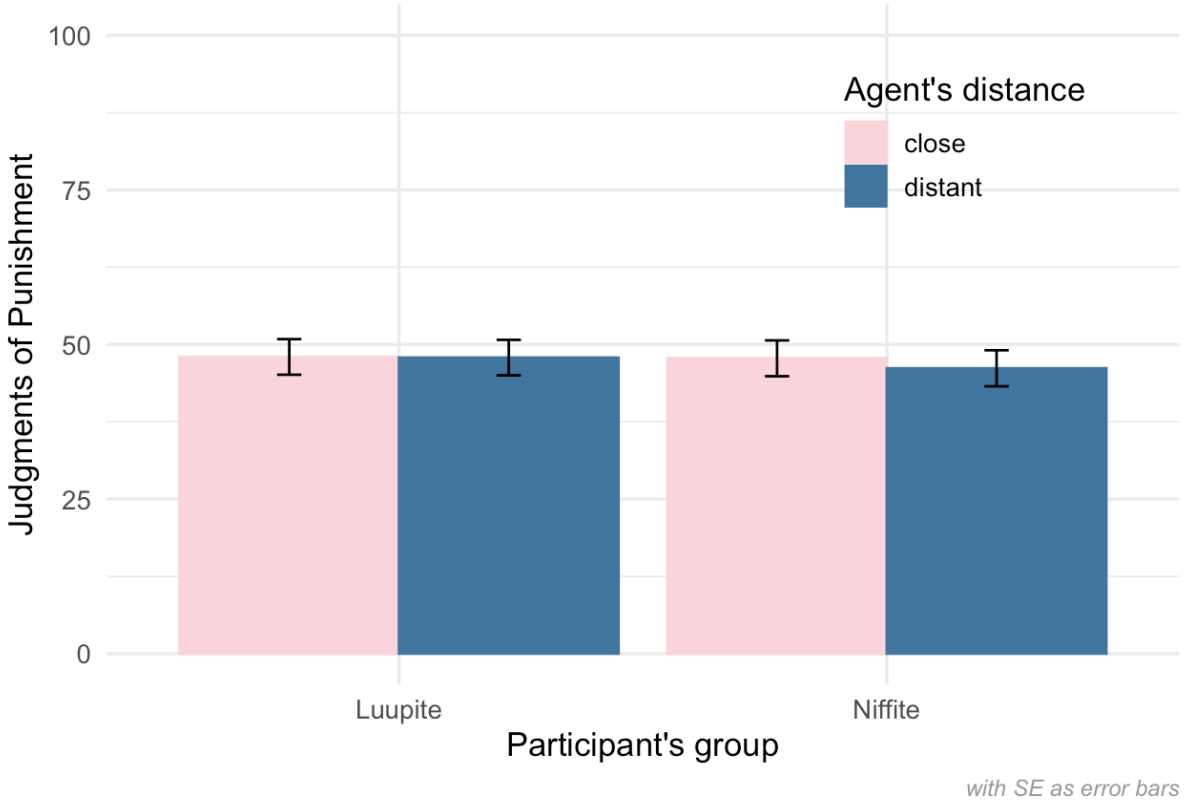
First, we could, thanks to the manipulation check items, see whether participants understood and remembered their own group membership all along the experiment. As seen before, only a few participants failed these manipulation checks (i.e., items where participants should classify sorted members' name either as ingroup or as outgroup members). However, when asked at the end of the task whether they subjectively thought they took into account protagonists' membership in the different moral situations (on a 0-100 ranged scale from *not at all* to *very much*), most participants reported not having taken this information into account in their judgments (M = 15.10, SD = 25.95, with most participants judging at the (0) '*not at all'* slider extremity).

Final confirmatory analysis on moral judgments throughout the whole set of scenarios and on the filtered sample of participants (N = 152) revealed no main effect of social distance to the agent, b = -0.86, SE = 0.52, t(8796.40) = -1.64, p = .10, 95% CI [-1.89,0.17], d = -0.025. Participants did not judge significantly differently agents committing a wrongdoing when they were part of their own group (close agents: M = 47.88, SD = 2.54) compared to the other group (distant agents: M = 47.01, SD = 2.54) (see Figure 5.12). The other effects of social distance (notably toward the victim and interaction effects) will be presented later in Chapter 6 (see pp. 211–218). No other main or interaction effects

regarding social distance to the agent was detected in this analysis (also see in supplemental material the analysis conducted on the whole sample of participants).

Figure 5.12

Judgments of Punishment in Exp.3 (Part A) Depending on Social Distance to the Agent and on Participants' Group of Membership



Exploratory Results. Even if exploratory and not preregistered, we also ran analysis taking into account, in addition to the other factors, participants' gender (as previously found in other experiments as potentially relevant to explain some of the variability, see especially in Chapter 6, pp. 179–209 & 217). Moreover, prior verification about counterbalanced conditions at first intermediate analysis (N = 76) revealed some dependencies between participant, scenario, and the distance to the victim (see our analytical script on OSF for more details). In sum, a first set of participants (the 60 first ones) went through the experiment with victim's social distance conditions always nested under the same scenarios for the same participants (hence no variability and no comparison possible within a single scenario). The following set of participants (the 60 following) were counterbalanced in terms

of social distance to the victim conditions within the various scenarios.[53] Due to these methodological issues, we created another factor (‘*reversed’*) to control for the dependencies in design within our exploratory model[54], coded as (0) for participants going through the 1st to the 60th experimental run, and (1) for participants going through 61st to 120th experimental run and so on. We thus adapted the previously reduced model via integrating both participants’ gender and the *reversed* variable as additional fixed factors and starting from zero correlation parameters model to reduce this new model.

With this exploratory model controlling for the variance explained by both participants’ gender and reversed conditions (i.e., unwilling dependencies in design), we detected a significant main effect of social distance toward the agent, but in the opposite direction to what was expected (*b* = -1.30, *SE* = 0.56, *t*(7763.50) = -2.31, *p* = .02, [-2.40,-0.20], *d* = -0.04). When taking gender and reversed factor as part of the model, participants in fact seemed to have judged slightly more harshly close (ingroup) agents (*M* = 47.38, *SD* = 2.58) than distant (outgroup) perpetrators (*M* = 46.08, *SD* = 2.58) (see Figure 5.13). Moreover, several other interaction effects involving social distance toward the agent appeared when controlling for these factors. In particular, we found—even if not expected, yet consistent with other findings (see Szczepaniak et al., 2024, reported in Chapter 6, Exp.5, pp. 179–193)—a marginally non-significant interaction between social distance to the agent and participants’ gender, *b* = -2.18, *SE* = 1.13, *t*(8529.33) = -1.92, *p* = .05, [-4.40,0.04], *d* = -0.03. When decomposing it, though still in an exploratory vein, we found that the effect of social distance to the agent was mostly driven by male participants (*b* = -2.39, *p* = .010) and not by female participants (*b* = -0.21, *p* = .74) (see Figure 5.13). As the other interaction effects found as significant in this analysis are involving social distance to the victim, we will report them in Chapter 6 (see pp. 211–218), dealing more specifically with this question.

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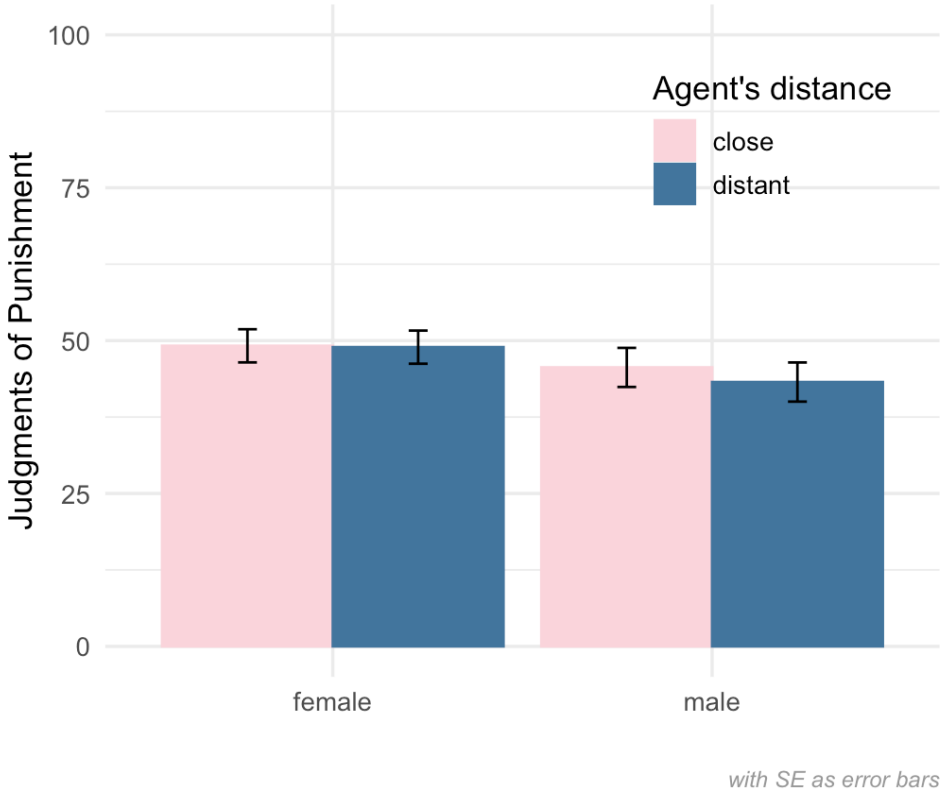
[53] It was mostly problematic for estimations of effects at first intermediate analysis (*N* = 76), hence not taking scenarios as a random variable for these prior analyses and thus continuing data collection until reaching at least (60x2=)120 participants.

[54] For more information about this methodological issue, see Quinton et al.’s communication (2024).

Figure 5.13

Judgments of Punishment in Exp.3 (Part A) Depending on Social Distance to the Agent and on Participants' Gender



Discussion & Limitations

In this experiment, we tackled social distance effects at the intergroup level, through an induction of group membership via a minimal group paradigm. We exploratorily found a small significant effect of social distance toward the agent (equivalent Cohen's *d* = -0.04), but only when including participants' gender and reversed factor (to control for the unvoluntary dependencies in design). In other words, within this exploratory analysis, participants overall judged ingroup perpetrators slightly more harshly than outgroup perpetrators. This tendency goes in the opposite direction compared to what was preregistered (i.e., a general ingroup favoritism bias, with more lenient judgments to close others). The current results yet are in line with the minor tendency in the literature as reviewed earlier in line with the Black Sheep effect (i.e., judging deviant close or ingroup members more harshly than outgroup ones for the same transgression).

This effect could however be much more nuanced than previously demonstrated in literature, as suggested by the small effect size we obtained in the current experiment with a battery of 60 everyday moral scenarios. The overestimation of this bias in literature could again stem from the reduced number of scenarios used in previous research dealing with moral decision-making, hence limiting the generalizability of this effect to various moral situations. On the contrary, the current analyses were conducted with mixed models comprising two random variables (i.e., one for participants, the other for scenarios), thus allowing to generalize the current findings to both other participants and other moral situations.

We also found, during exploratory analysis, a main effect of participants' gender on moral judgments, with males judging the moral situations overall more harshly. And regarding social distance simple effects, it was significant only for males and not females. This result seems quite consistent with Experiment 2's finding, suggesting that males would be more sensitive than females to social distance biases (at least when manipulated toward the agent). The fact they overall provided stronger punishments than females seems quite consistent with literature showing a greater tendency to punish rather than rehabilitate crime authors (e.g., Hurwitz & Smithey, 1998). However, this may not be always true depending on judgments' type, as Chu and Grühn (2018) found, for instance, harsher judgments (of moral acceptability) for female judges than for male judges.

The discrepancy of effect size estimation between our current findings and those found in previous literature could also come from the limitations of our own research. In particular, as reported earlier, quite a few participants in this experiment failed the various attention check (N = 7) or manipulation check (N = 5) items. We may have chosen too complicated manipulation checks, as they required at the same time to remember one's own group and to be able to infer a Luupite's or Niffite's group of membership based only on their names (see details on OSF for more). But this also prevented participants to forget throughout the experiment their own group of membership, while strengthening the membership of the various protagonists involved in the moral situations at play. As for the

attention checks, they were not supposed to create some difficulty for participants (i.e., asking them to put the cursor at the extreme right or extreme left of the slider). The fact this experiment was run in a lab environment should also have limited failures (compared to an online study), as participants could ask questions at any moment during the task to the experimenters (which yet very rarely occurred).

The induction of group membership may also have been insufficiently strong, passing from the minimal group paradigm to the actual content of the scenarios. Even if explained in the instructions and despite our efforts to strengthen participants' membership, the only clue they had to infer the agent's membership in the various situations was their name (either a *Luupite* or a *Niffite* name). Due to this maybe too indirect (or too minimal) manipulation of the agent's group membership, participants may have then treated the scenarios always as if an unknown person was the agent of the transgression, thus not taking that much into account the agent's identity in relation to their own membership. At least, it is what most participants self-reported at the end of the experiment when we asked them how they subjectively thought they took the agent's identity into account (i.e., with most of them reporting no influence of this criterion on their judgments). However, the use of a minimal group paradigm was in the current experiment a deliberate choice, made with the aim to see whether social distance to the agent may have an influence beyond participants' desire to remain fair and objective in their decisions.

Experiment 4[55] (Part A)—Self vs. Ingroup vs. Outgroup Member as Agent

Rationale

As for the Experiment 3 above, we wanted in this study to analyze the effects of social distance at the intergroup level (judging an ingroup vs. an outgroup member). However, as we were also interested in the direct comparison of these judgments at the intergroup level with judgments of oneself being the agent (i.e., intraindividual level), we integrated this possibility in the current Experiment 4 (on the contrary of Exp.3 where the

[55] This study was conducted in collaboration with Pr. Liane Young's Morality Lab (Boston College, US) and thanks to the precious help of Thomas Mazzuchi to implement the online task.

participant was always a mere observer of the situation). We furthermore adapted the previous paradigm to test these effects of social distance within a more ecological paradigm, that is, participants judging others' or own's behavior in an online multiplayer cooperative game. We were inspired by the original study from Hirschfeld-Kroen et al. (2021), where the authors developed a card game with participants either being agent or observer of the harmful or neutral outcome that happened. We offered in the current experiment kind of a conceptual replication and extension of this study (as integrating both the intraindividual and intergroup levels), although changing the general framework (in lab vs. online study).

Hypotheses for this experiment were first to find a main effect of outcome, with harmful outcome cases being judged more harshly than neutral outcome ones (see details about the game below). Regarding social distance specifically, we expected in line with our previous experiments to observe in harmful outcome situations especially (compared to neutral outcome) an effect of social distance to the agent. This would be either in direction of harsher moral judgments toward the self compared to close (ingroup) and distant (outgroup) members as agents (in line with Tend.2), or in the other way around (in line with Tend.1). A second prediction that should be found for harmful outcome cases especially, is that close (ingroup) members should be judged more leniently than the distant (outgroup) members.

Method

This experiment was conceptualized and conducted in collaboration with the *Morality Lab* of Boston College, where had been run the original study we were inspired by (i.e., Hirschfeld-Kroen et al., 2021). The whole materials, including preregistration form, data, and supplementary files for this experiment are available on the OSF page of this project (see https://osf.io/xd8c5/). Also, as conceptualized during an internship in Boston (US), this study protocol was approved by the Boston College Institutional Review Board (IRB) on September,19, 2023.

Power Analysis. We determined the required sample size for this study through a power analysis based on the previous effect size found in literature for social distance (d = 0.14, see Hirschfeld-Kroen et al., 2021). According to our calculations (see OSF page for

more in Power analysis section), we might have reached a statistical power of 80% with N = 210 participants. We thus fixed the reference sample size at 210. However, following Laken's recommendation (2014) when there is not a lot of certainty about the effect size (as it is still the case for social distance effects), and due to potential limitations to reach such a sample size within a reasonable time, we plan to conduct a sequential analysis, it means, conducting an intermediate analysis at a first stop of 70 participants and a second intermediate one at N = 140. We chose to fix a smallest effect size of interest (SESOI) as the criterion for stopping data collection of Cohen's d = 0.04 for the social distance to the agent, based on the lower bound of 95%CI found in Hirschfeld-Kroen et al.'s (2021) original study. In the end, we hardly reached a sample of 69 participants within several months of remote data collection. In addition, the qualitative comments at the end of the experiment suggested that many participants did not really either believe to the cover story or understand the goal of the task[56]. For these reasons, we decided not to pursue data collection although estimate effect size of social distance to the agent was higher than the predetermined threshold (d = -0.19).

Participants. Participants (N = 69, 37 females, 31 males, 1 unspecified, M_{age} = 19.52, SD_{age} = 1.97) were recruited from the local university campus (Boston College undergraduates) via the SONA platform and were rewarded through it with a 0.5 course credit if they fully completed the online experiment.

Materials. This experiment was built with the first idea of creating an experimental task where participants would be either agent or observer of the situation, this way judging either themselves or another close or distant one for the same wrongdoing. Hirschfeld-Kroen et al. (2021) roughly did so in their experiment where people were invited to play a card game in laboratory and within a fMRI device either under the first-person perspective (i.e., they were themselves making the card choice) or under the third-person perspective (i.e., another one was making the choice, and they only had to observe this choice and its

[56] Even if the whole instructions for the task were explicitly displayed all along the experiment, due to its complexity and/or issues in the task's meaning, many participants reported having misunderstood the game logic and aim (see 'Method' section below for more information about the task).

outcome). As a consequence of the card choice was displayed either a harmful outcome (i.e., automatic administering of a noise blast through headphones) or a neutral one (no noise blast) to the victim, a supposedly *passive* player, being in fact a confederate. The objective for the participant was to find the hidden relationship between the card choice and the outcome (either harmful or neutral), while there was in fact no such relationship other than random.[57]

With the same idea and due to ethical limitations[58], we implemented thanks to the *jsPsych* library (de Leeuw, 2015) a variant of this card game in an online multiplayer paradigm. In this cooperative game, players (the participant as well as five other supposedly real players) had to choose between two cards across several rounds (51 in total) in order to find the hidden relationship between the card number and the choice outcome. Here too, the outcome was either 'bad' (one penalty point attributed to the victim) or 'neutral' (no penalty point). The agent (the one who had to choose between the two cards) was designated at the beginning of each round, whereas the victim (the one who receives the potential penalty point) was randomly designated after the card choice. The overall aim for players was, as in a cooperative game, to find, in few rounds as possible, the hidden relationship between cards' number and outcome valence, in order to minimize the number of penalty points attributed to the victims[59]. After each round, participants had to rate how moral was the agent's behavior in that round depending on the outcome for the victim as well as their identities (see Figure 5.14 for a visual of the game).

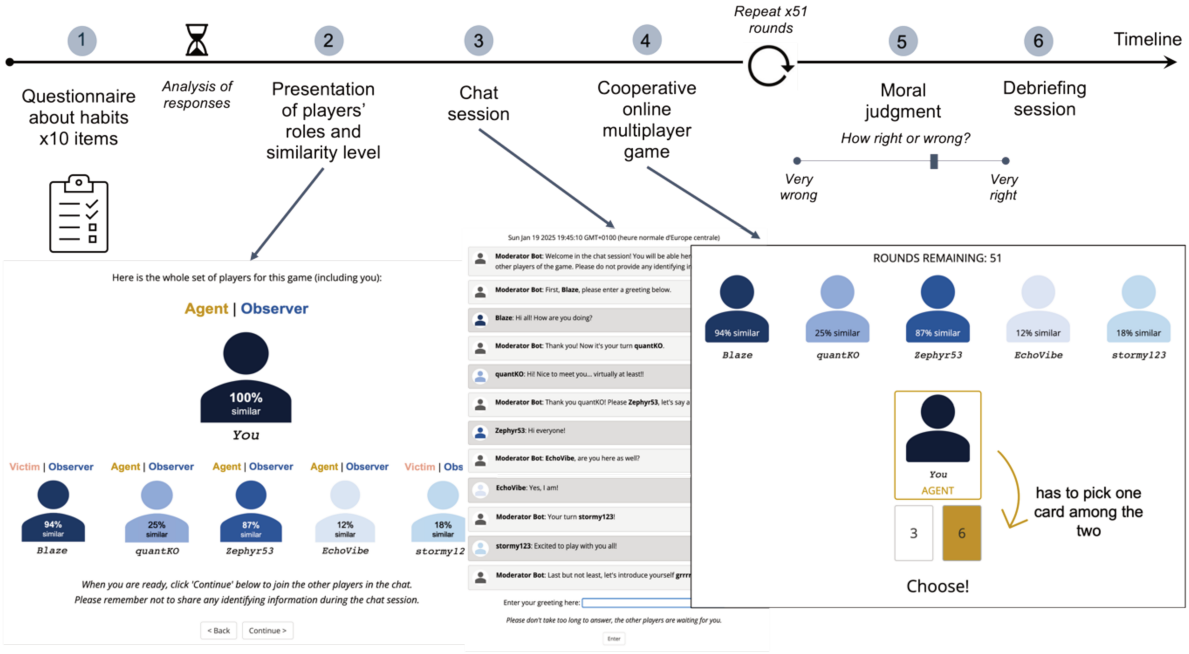
[57] The 'victim' was also only a fictious one as this role was played by a confederate which did not really receive the noise blast, and from who the experimenters recorded in video their facial expressions to translate either a harmful or neutral reaction.

[58] Not possible within a restricted time delay to build an experiment that would have been validated by the IRB committee when involving potential harm to other individuals, even fictitious. We were then constrained to change the experimental paradigm for it not to involve a real potential harm.

[59] We made the harmful consequence more real for the participants by pretending in the game instructions that a victim who would have exceeded 10 penalties during the game would have the choice in the end between performing a boring 20-minute-long task (in order to get their entire 0.5 SONA credit) or skipping the supplementary task but inflicting this way a loss in reward for all participants (only 0.25 SONA credit).

Figure 5.14

Schematic Representation of the Online Card Game Procedure Followed in Exp.4



The main difference and novelty compared to Hirschfeld-Kroen et al.'s study (2021)[60] is that we implemented this game at the intergroup level, that is, manipulating social distance between the participant and the players through their group membership. More precisely, we matched participants to a certain degree of similarity with the other players and let them believe these matches were based on their responses to the questionnaire they had to fill at the start of the experiment. This questionnaire was compounded with ten various questions about participants' habits and characteristics (e.g., age, gender, class, sport, dorm, favorite library etc.). In fact, we did not analyze their responses on this questionnaire and the other players' levels of similarity with the participant were completely a priori determined (as much as the other players were not real participants but fully created to this purpose). Doing so allowed us to control for the group membership of each player: Two players (*Blaze* and *Zephyr53*)[61] shared a high level of similarity with the participant (94% and 87% respectively),

[60] Beyond the fact we did not use fMRI technique in the current experiment compared to the original study, but only relied on behavioral data.

[61] Note that the handles for the other players have been generated by an IA assistant (ChatGPT, GPT-4-turbo).

hence reflecting the ingroup members, whereas the other three players (*quantKO*, *EchoVibe*, and *stormy123*) corresponded to the outgroup members (with 25%, 12%, and 18% of similarity shared, respectively) (see Figure 5.14 for a synthetic view of the various players' identity and level of similarity).

Protocol & Design. Participants joined the experiment after having registered to participate on the SONA platform. They could access the experiment only once via a link made available after registration and were indicated recommended times to go through the experiment[62]. After giving consent and reading main instructions about the task, participants went through the questionnaire about their demographics, habits, and preferences, hereby providing the age and gender information for descriptives (see step 1 in Figure 5.14)[63]. We then made them believe we were automatically analyzing their responses to match their profile with other participants running the experiment at the same time. After a few seconds delay, we asked participants to choose a handle, and presented them the two roles that have been (supposedly) randomly selected for them. In reality, as can be seen in step 2 of Figure 5.14, the participant was always designated for the roles of either agent or observer (as required by the experiment factorial design). We then introduced them one by one their partners for the game, their handle, their roles, as well as their degree of similarity (supposedly based on previous answers on the questionnaire). A forum session was also implemented (see step 3, Figure 5.14) where the participant could interact with the others to strengthen the idea that the other players were real people playing at the same time. Then, the participant went through the online cooperative game during 51 rounds in total (with three rounds being only attention check rounds) and had to rate after each round how moral was the agent's choice. This moral judgment about the agent's card choice—knowing that the agent could be the participant or another ingroup (high similarity) or outgroup (low similarity) player—was given on a continuous slider from *very morally wrong* (0) to *very*

[62] This time constraint (from 9:00AM to 7:00PM) was in fact part of the cover story, to make believable the fact that at least five other participants were taking part of the experiment at the same time.

[63] Note that the other personal pieces of information were not analyzed and are not made available on the OSF page (except age and gender).

morally right (100) (see steps 4 and 5, Figure 5.14). In the end, participants went through a funnel debriefing session (i.e., asking more and more specific debriefing questions to understand how much they believed to the cover story) and answered three manipulation check items[64] (step 6 in Figure 5.14).

This study followed a within-participant design where each participant had to rate the wrongness of the card choice after each of the 48 rounds (plus three attention check rounds, hence a total of 51 rounds). Overall, we made varying in a randomized way throughout the game rounds the agent's identity at the intraindividual and intergroup levels (either the participant him/herself or an ingroup or outgroup member as the agent). We also varied in a systematic way the victim's identity (so that it could only be either an ingroup or an outgroup member, but never the participant him/herself)[65], as well as the outcome of the situation (either neutral—the right card has been chosen—or harmful—the wrong card has been chosen, resulting in one penalty point added to the designated victim score). This experimental design thus comprised a total of (3x2x2=) 12 conditions, distributed randomly across the 48 test rounds (hence four rounds within each condition per participant).

Analytical Strategy. In order to test our hypotheses, we used linear mixed model analysis, with three fixed factors and one random variable for participants, thus allowing the current results to be generalized only to other participants. It seemed indeed useless trying to generalize to other situations for the current experiment as the various rounds (x48) were identical in terms of procedure (only the factors of interest varying). Compared to previous experiments where a series of various scenarios were displayed and could then bring additional variability, the current design, even though comprising several 'rounds', is not supposed to add such between situations variability. After applying the procedure to reduce the random structure of the model (see Matuschek et al., 2017), the obtained reduced model

[64] One item was supposed to check the social distance induction via the diverse levels of similarity toward the other players as presented before the game. The two other items asked participants to self-report how much they think they subjectively took into account the identities of both the agent and the victim during the game. These items were answered on a Likert scale from (0) Not at all to (5) Very much (see supplemental material for the full questions asked).

[65] Even if manipulated in the current experiment, we will not currently report the effects of social distance toward the victim, as it is the focus of the following chapter (see Chapter 6, pp. 220–222).

was: $Moral\ Judgments \sim Agent * Victim * Outcome + [\ ctr.Agent2 + (Victim + ctr.Agent1) * Outcome + ctr.Agent1{:}Victim{:}Outcome \mid Participant\ ]$

The three fixed factors were coded as contrasts with the outcome and social distance to the victim being binary coded (Outcome: *neutral* = -0.5 vs. *harmful* = +0.5; Victim: *ingroup* = -0.5 vs. *outgroup* = +0.5). We coded social distance to the agent as a Helmert contrast, to test (a) on the one hand the effect of social distance for judgments toward the self vs. toward an ingroup vs. outgroup other (Agent1: *self* = -2/3 vs. *ingroup* = +1/3 and *outgroup* = +1/3) and (b) on the other hand, the effect of social distance for judgments toward an ingroup vs. outgroup member (Agent2: *self* = 0, *ingroup* = -0.5 vs. *outgroup* = +0.5). The dependent variable for this study was the moral judgments given by the participants, on a continuous range (with a two decimals precision) starting from (0) *very wrong* to (100) *very right*.[66]

For the main analysis, we originally planned—as stated in the preregistration form—to exclude participants who took less than 10 minutes to answer the whole study (whereas it is supposed to take around 30 minutes) as well as participants who failed the three attention check trials spread along the main game (with a margin of error for answering the attention check items of 15% on the slider). We also planned to check for robustness of the mixed model analysis by removing observations that are deviant regarding the distribution of random effects. For the sake of analytical robustness, we also preregistered to compare changes in the model parameters when taking into account or not the participants who responded to at least one of the cover story questions by 'yes' during the debrief session (i.e., those who reported they did not believe to the cover story of real other players and/or to the one of the existing relationship between the card and the outcome).

On the final sample of 69 participants (as we stopped data collection at first stop of the sequential analysis), 42 were having at least one failed attention check and 22 of them

[66] Note that we also performed additional exploratory analyses available in supplementary materials (as indicated in the preregistration form), notably including participants' gender as a factor in the reduced model (as previous research suggested it could bring some variance in the pattern of results, see notably Chapter 6 for more).

had 3 out of 3 failed attention check[67]. In front of this large number of participants, we decided to deviate from preregistered analyses and to keep in a first instance the whole sample. We then rerun the same analyses without the 22 participants who failed all attention check items to compare the parameters estimation. As it did not change that much the findings (only larger effect sizes with the restricted sample), we kept the most conservative findings, that is, results from the whole sample analyses (see supplemental material on OSF for the analyses on N = 47 participants).

On the same logic, regarding the criterion of cover story check items, more than half of the participants' sample reported they did not believe to the deception of the hidden relationship to find between the card choice and the outcome (56%) and almost three quarters of them did not believe that other players were real participants playing at the same time (72%). This could however be due to a desirability bias or demand characteristic effect as these questions were asked in the end of the funnel debriefing session.[68] In front of the high proportion of participants who answered 'yes' at least at one out of the two cover story questions and due to the poor reliability of these self-reported measures to really attest participants' belief in the task, we decided to deviate from preregistration and not to take this criterion as an exclusion reason. Results below are then reported for the whole sample analysis (N = 69 participants).

In a similar way, regarding manipulation checks, most participants reported at the end of the experiment not feeling especially closer to the players with high similarity rates compared to players low in similarity (M = 1.33, SD = 1.11[69]). They also perceived the

[67] We acknowledge this represents an important part of participants (with at least one attention check failed). This may be due to the complexity of the game, hence people's disengagement to the task when they did not understand fully the game's rules or purpose.

[68] We notably noticed several participants' responses that were not fully consistent between what was reported as a first instance in the debrief session to the questions of: 'Did you identify what was the relationship between the card choice and the outcome during the game? (you can answer 'yes' even if you're not sure)', and then 'If yes, what do you think the relationship between the card choice and the outcome (either no penalty or +1 penalty given) was?' with what they answered in the final questions of cover story believability ('Did you notice that the relationship between the card choice and the outcome during the game was fake (no real relationship other than random)?' for main cover story.

[69] On a 1-5 range Likert scale, with lower values corresponding to 'Not at all' for the question 'How close you feel from (…)?'.

consideration of social distance toward the agents and the victims during the game as very low (M = 1.07, SD = 1.18 and M = 1.13, SD = 1.24, respectively). Hence, a possible low-powerful distance induction in the current experiment, at least as perceived and explicitly self-reported by participants. Results below should then be taken with caution.

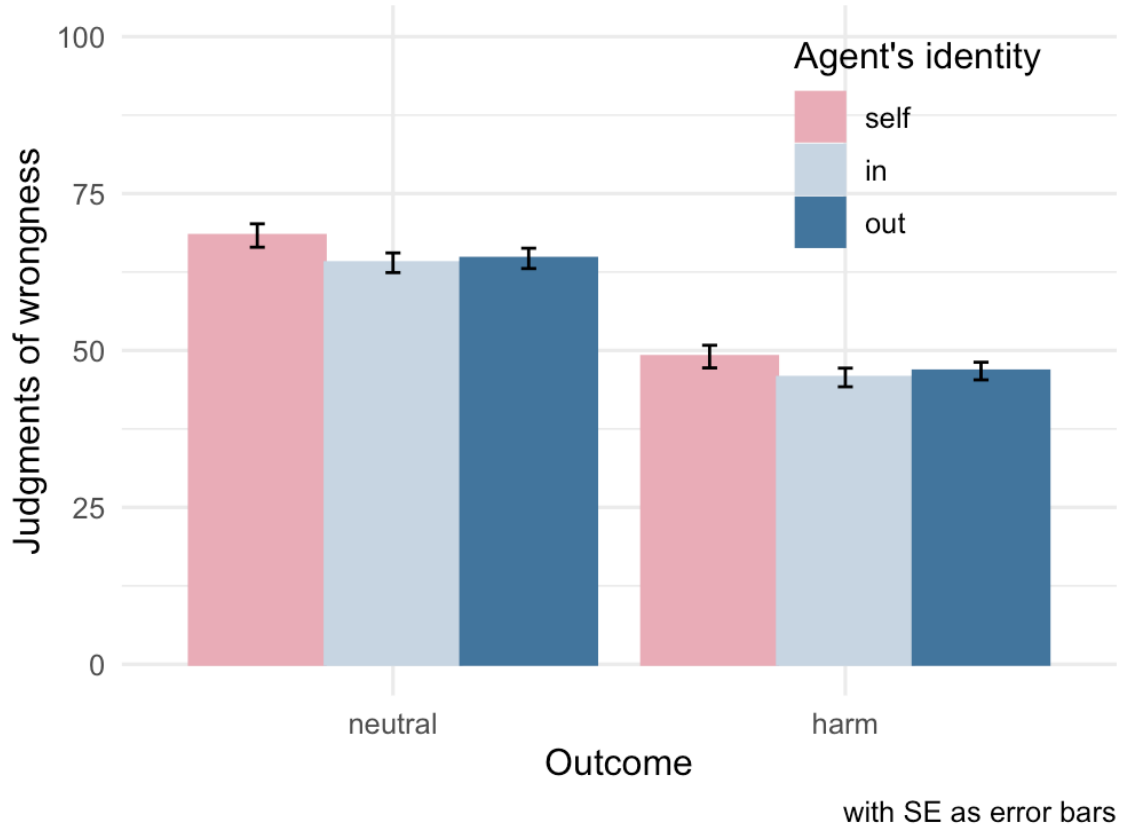
Results

As a reminder for this experiment, we expected to find—beyond the mere effect of outcome—an interaction effect between social distance and outcome, such that people would tend to judge differently self vs. others as agent, and ingroup vs. outgroup members as agent, and this to be true especially in harmful outcome cases (and not or to a lesser extent for neutral outcome). Mixed model analysis performed on the whole sample of participants (N = 69) revealed a main effect of outcome (b = -18.50, SE = 2.14, t(67.97) = -8.65, p < .001, 95% CI [-22.77,-14.23], d = -1.04). Participants judged the agent more harshly in case of harmful outcome (M = 47.15, SE = 1.45) compared to neutral outcome cases (M = 65.65, SE = 1.60), with lower scores on the scale indicating harsher judgments. It also revealed a main effect of social distance to the agent when considered between self vs. others (Agent1), b = -3.40, SE = 0.77, t(68.26) = -4.40, p < .001, [-6.60,-2.48], d = -0.19. Participants judged rounds where they were themselves in the position of the agent slightly more leniently (M = 58.67, SE = 1.43) than when another one (ingroup or outgroup member) was the agent (M = 55.26, SE = 0.98). Contrarily to hypotheses, this effect was however not especially more true in harmful (b = -3.75, p = .01) than in neutral outcome rounds (b = -5.32, p < .001) (i.e., no significant interaction between social distance to the agent and outcome), b = 1.18, SE = 1.25, t(75.78) = 0.94, p = .35, [-6.60,-2.48], d = 0.03. Also, contrary to expectations, there was no main effect for social distance toward the agent when compared between ingroup and outgroup members (Agent 2), b = 0.86, SE = 0.63, t(95.53) = 1.37, p = .17, [-0.29,1.58], d = 0.05. Participants did not judge significantly harsher ingroup agents (M = 54.83, SE = 1.04) compared to outgroup agents (M = 55.69, SE = 1.02). And this effect was not moderated by the outcome, hence not truer in harmful cases (b = 0.76, p

= .21) than in neutral cases (b = 0.53, p = .39). No other main or interaction effect was detected as significant (see Figure 5.15).

Figure 5.15

Judgments of Wrongness in Exp.4 (Part A) Depending on Outcome and on Agent's Identity



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more morally right (vs. wrong) judgments. The agent is either the participant ('self' condition in pink), or an ingroup member ('in' condition in light blue), or an outgroup member ('out' condition in dark blue).

Discussion & Limitations

In this experiment, we implemented an online multiplayer game to test the effects of social distance toward both the agents and the victims on moral judgments. Through this paradigm and focusing in the current chapter on effects toward the agent's identity only, we failed to reproduce some of the expected effects. In particular, we identified only main effects of outcome (i.e., harmful cases judged more harshly than neutral ones) and of agents' social distance when compared between self and others. Participants judged their own behavior during the game play roughly more leniently than when others were the agent, hence

arguing in favor of a moral superiority effect (i.e., judging oneself as more moral than others). What is astonishing however is that this effect was not moderated by the outcome of card choice (neutral vs. harmful), that is, people judged their own behavior more favorably no matter what the consequence of the card choice was. In addition, although expected, we found no significant difference between ingroup and outgroup members as agents.

We think main limitations to this study are (a) a lack of understanding and/or clarity in the aim of the cooperative game play, and (b) a poor induction of social distance through other players' similarity rate based on participants' own responses to the first questionnaire. Indeed, as shown by the many failed attention check items, as well as the final questions during debriefing, participants often did not understand the rules of the game and its goal, or when they did understand it, they did not really find it relevant to judge the agent's behavior in terms of morality. It was a risk we took by implementing this game (that participants would not find out the relevance of the task in front of the complexity of the game), but that was constrained by the experimental design we wanted to replicate from original study of Hirschfeld-Kroen et al. (2021), while respecting the ethical constraints depicted above. We should further consider, in future replications of the current study, to use an already conceptualized game play as a dictator-like dilemma or a prisoner game, adapted so that the recipients and agents are either oneself, or ingroup, or outgroup members.

As for social distance perception, manipulation checks suggest that participants did not really perceive the supposedly close (ingroup) other players (i.e., players introduced as having a high rate of similarity with them) as truly socially closer. We aimed at inducing social distance through a minimal group like paradigm (as for Exp.3 above) but using a less minimal criterion than before (i.e., the proximity on a 10 items personality questionnaire in Exp.3). We then used an induction through similarity on a 10 items questionnaire, dealing with everyday life habits and characteristics for undergraduates (e.g., favorite library, dorm, class etc.). However, this may have been still too minimal for participants to really feel closer to highly similar players. Another explanation of this poor perception of social distance could be that participants did not believe to the cover story of real players (i.e., other students)

playing at the same time—as suggested by the final debriefing questions and despite our efforts to make it believable (e.g., via the forum/chat session, and cover story building). To palliate these drawbacks, we might also replicate this study in a lab environment for participants being welcomed in groups of 5/6 participants and/or making them believe they are playing against each other (while also adapting the game itself to make it easier to understand).

General Discussion & Conclusion

To sum up and conclude about this chapter, we presently investigated through four preregistered experiments how social distance to the agent influences everyday moral judgments throughout the various levels of analysis as depicted earlier in the theoretical framework. At the interindividual level first, we found in Experiment 1 no significant main effect of social distance to the agent overall (either a close one or a distant other in the role of the agent). But this effect became significant in Experiment 2, once controlling for the potential confounding effect with victim's social distance and integrating the intraindividual level (i.e., self as a potential agent), with slightly harsher moral judgments toward distant agents. At the intergroup level in Experiment 3, we found in exploratory analysis consistent results, that is, rating the distant (outgroup) agent more harshly than the close (ingroup) agent, especially driven by male participants' judgments. Finally, in Experiment 4, mixing the intraindividual and intergroup levels, we found harsher judgments for (close or distant) others compared to oneself, even if not especially truer for harmful (vs. neutral) situations. In the latter experiment, no difference was found yet between judgments of an ingroup vs. outgroup member as agent.

This series of studies thus gives only mitigated evidence in favor of the main tendency in literature regarding social distance effects when manipulated toward the agent, that is, favoring oneself over others, or close ones over distant others (Tendency 1). Especially, Exp.2, 3, and 4 suggest—even if often with low effect sizes—that people tend to judge the socially distant protagonists more harshly than oneself and/or close ones in case of wrongdoings. However, when looking at the descriptive pattern of results in Experiment 2,

we observed a gap between participants' judgments of their own vs. their close ones' wrongdoings. Although not statistically tested (as not part of the hypotheses and contrasts not determined as such), people seemed to judge their own transgressions as harsh as they judged those perpetrated by a socially distant other (compared to a close one). This pattern for self vs. others' judgments (i.e., at intraindividual level) would then be more consistent with the agent-regret model, previously defined as attributing more blame to oneself in case of accidental wrongdoings than to others (Tendency 2). But the fact that people still judged the distant agent more harshly than the close agent goes against what would have been expected in our previous theoretical rationale of self-close ones merging and extension of biases applying for oneself to close others.

In Experiment 3, we found in exploratory findings that people tend to slightly judge the ingroup agent more harshly than the outgroup agent. This pattern was found as especially true for males (and less for female participants), which interrogates the role of people's gender in predicting their respective judgments toward close and distant agents. As not the first focus of the current dissertation, this question will be more deeply discussed in final discussion (see Chapter 9, p. 339), while being also controlled for as a factor of analysis in the next chapter about social distance to the victim influence (see especially Exp.5 and 6, in Chapter 6).

In Experiment 4, even if, again, it would need replication to attest these effects' robustness and significance, we observed an apparently similar pattern of responses to that of Exp.2 (U-shape), but given the judgments' scale, actually completely opposite in direction compared to Experiment 2 findings. More specifically, people judged in this study their own choices as less wrong than those of close or distant others, thus being in accordance with self-favoritism bias (Tendency 1). When looking more closely though, the difference between ingroup and outgroup members' judgments was directed toward harsher judgments for close ones (even though not significant), hence better supporting Tendency 2 results or the Black Sheep hypothesis (i.e., harsher judgments for ingroup deviant members than outgroup deviant ones). This pattern was observed both for neutral and harmful outcome cases, which

quite nuances this conclusion, as players in the neutral conditions were not really ‘deviant’ members.

In the end, we could conclude, based on these findings on social distance to the agent effects, that Tendency 1 seems to be supported at interindividual and intergroup levels. However, mixed findings were found at intraindividual level (either in Tendency 1 direction in Exp.4 or in Tendency 2 direction in Exp.2) and do not allow therefore to draw strong conclusions. We should also acknowledge that the current findings should be taken with caution, especially regarding Exp.4 results, given the reported low believability of participants to the cover story, their overall low scores on attention and manipulation check items, as well as their general poor understanding of the task. We may therefore improve the online task used to test these effects in future replication, also to enhance believability and keep the task more understandable for participants (as with dictator game, prisoner dilemma etc.). Experiments 1 to 3 results—although probably more reliable in terms of internal validity—suffer from the opposite drawbacks. Participants were indeed asked to answer hypothetical moral situations with diverse (close or distant) protagonists while they obviously knew the situations did (or will) not happen in real life. This lack of ecological validity may then have hindered the expected effects of social distance, although this is a common limitation in moral psychology studies, including a good proportion of those tackling the question of social distance influence.

Beyond these limitations, we should acknowledge the force of the experimental design we used in this series of studies, with social distance being always manipulated within participants and within scenarios, to enhance statistical power. Moreover, the use of a large database of situations (adapted from Leloup et al., 2018) in Exp.1 to 3 and the analysis conducted with mixed models comprising two random variables (i.e., participant and scenario) allow the generalization of the current findings to both other participants and other diverse moral situations (e.g., Judd et al., 2012). This is of particular importance in moral decision-making—even if rarely considered as an issue in moral psychology research—as often involving a large part of variability both at the participants and scenarios levels.

The counterpart of within-participant design is that it also provides much more indices of comparison for participants who could thus more easily infer from the diverse encountered situations what is currently being investigated (all the more when social distance is operationalized within the same modalities for each close vs. distant protagonist). We will discuss that point further in the next chapters (see especially Chapter 9, pp. 352–353), seeing why a between-participant design may in our experiments have not strengthened that much the effects.

Another point to watch for as much as possible in future research on this issue is the consideration of both the social distance to the agent and to the victim in a single experiment. As it has been seen in Exp.1, when not controlling distances both toward the agent and the victim, a potential confounding effect of social distance may occur (here, due to the variation of victims' identity between both conditions of agent's social distance). It is therefore of crucial importance to at least control for (or fix) social distance to the victim when studying the effect of social distance to the agent from the participant's point of view, as well as (when possible) controlling for the relationship between the agent and the victim themselves. Pending further investigations on this side of social distance influence (i.e., toward the agent) on moral decision-making, we will focus in the next chapter on the effects of social distance to the victim or recipient of a (im)moral action.

Chapter 6

Social Distance Influence in Everyday Moral Decision-Making when Manipulated Toward the Victim

To continue with the experimental part of the current dissertation, we report in this chapter how social distance influences everyday moral cognition when manipulated with respect to the victim of the situation. In other words, we analyzed and systematically tested how being socially closer (vs. more distant) to the victim of a moral transgression makes us judge the wrongdoer more (vs. less) harshly. Here again, we used as a framework of analysis and to guide our experimentation the three levels of analysis depicted earlier. Regarding the manipulation of social distance to the victim, it means at the intraindividual level being oneself vs. another one the victim of the involved transgression. At the interindividual and intergroup levels, it entails analyzing the differences in judgments when a close—or ingroup member—vs. a distant—or outgroup member—is involved as the victim of the transgression.

At the Intraindividual[70] and Interindividual Levels

Rationale and Overview of the Experiments

Our aim in the current section was to study the effect of social distance when manipulated toward the victim of the situation, especially focusing first on the intraindividual (self vs. other as the victim) and the interindividual (close one vs. distant other as the victim) levels of analysis and within the realm of *dark morality* as defined in the previous chapters. Contrary to the previous hypotheses regarding social distance effects toward the agent, the literature seems overall convergent about the effects of social distance toward the victim. Indeed, research at both levels of analysis almost evenly suggests that the closer someone

[70] Note that the intraindividual level was not properly tested within the current detailed experiments, as making the participant being either the bystander or the victim in a situation involving moral transgressions entails both ethical issues and practical constraints, as noticed in the discussion.

is to the victim of a moral wrongdoing, the more immoral the transgression appears and the harsher the agent is judged.

At the interindividual level, for instance, many trolley-like situations highlight less consequentialist decisions (i.e., sacrificing one life to save five others) in cases where the life at stake is a close other (e.g., a genetically related other or a romantic partner) (see Bleske-Rechek et al., 2010). Similarly, Zhan et al. (2018) found that participants chose the altruistic action choice less frequently (i.e., helping the victim at one's own expense) when confronted with moral dilemmas involving a distant other (i.e., a stranger) compared to situations involving a close other (i.e., a friend). However, Singer et al.'s replication attempt (2019) did not provide the same results when tested through a novel set of dilemmas tackling everyday morality. The authors found no main effect of social distance to the victim on the proportion of altruistic decisions, that is, people were not significantly more altruistic in their decisions when involving a close one compared to a distant other (Singer et al., 2019, Studies 1 & 3).

At the intraindividual level, we found many studies tackling this question via comparing victims' and agents' perceptions of the same transgressive act (e.g., Baumeister et al., 1990; Chaikin & Darley, 1973; Ent & Parton, 2020; Kearns & Fincham, 2005). But they often do not compare these views to a mere observer perspective, hence confounding the effects of social distance to the victim and to the agent of the situation (i.e., one is judging the situation either as the victim or as the agent, but rarely as an independent observer). We found a few exceptions, as with Fehr and Fischbacher (2004), who demonstrated that people punish norm violation (in an economic game where cooperation is expected) much more harshly when involved as a second-party (i.e., being directly affected by the consequence of the violation with economic payoff reduced) than when involved as a third-party (i.e., with economic payoff not affected by the norm violation). Even if harm to others was found to be more highly valued by individuals than harm to themselves (Crockett et al., 2014), it does not directly mean we would judge a third-party harmful transgression committed toward another innocent victim more harshly than we would do when we, ourselves, suffered from the consequences of the act.

We should, however, notice that research dealing with the effects of social closeness toward the victim often does not control at the same time the identity of the agent. Hence no control for potential confounding effects between social distance toward the agent and the victim of the situation. Some controlled it without explicitly explaining their rationale (as it is the case in the examples reported above for the interindividual level with the self always being the agent, see ; Zhan et al., 2018). Some others, as McKee et al. (2023), did manipulate it explicitly, although not directly testing the interaction between both factors of social distance (toward the agent and the victim)[71]. We will detail these findings at the intergroup level more specifically in the next section.

Coming back to our aim in the current section, we thus wanted to replicate the findings previously exhibited regarding social distance effects toward the victim, first testing it at the intraindividual and interindividual levels. To do so, we offered a replication and extension of Singer et al.'s work (2019) in Experiment 5 (N = 299), manipulating the social distance between participants (in the role of the agent) and the recipient of their either altruistic or egoistic choice. In Experiment 6 (N = 149), we reused the same material but this time focusing on egoistic decisions. People had to judge how immoral these choices of action were (presented as their own) depending on the victim's identity (a close one or a more distant one). In both studies, we predicted that people would act more altruistically and judge more morally wrong to act egoistically when a close (vs. distant) recipient or victim is involved.

As can be noticed, participants' role in these two experiments was always the agent (i.e., the one making the egoistic or altruistic choice), hence not directly investigating these effects at the intraindividual level. This would indeed have required manipulating the role of the participant, sometimes being the victim of the transgression, sometimes being a mere observer of it. This is what we (non-purposedly) did in Experiment 1, while trying to

[71] The authors did however test the interaction between the agent's (or victim's) identity and the relative level of closeness (measured with the IOS) to the agent compared to that from the victim (e.g., is the participant feeling closer to the agent or to the victim in the situation). They found a significant interaction effect, with the 'insider agent' ['insider victim'] effect being even stronger along with increasing social closeness to the agent [the victim].

maximize the closeness of the relationship between the agent and the victim in close scenarios. Findings of Exp.1 did not reveal any significant influence of social distance on moral judgments of punishment. Therefore, what has earlier been interpreted as an absence of difference between judging a close one or a stranger as the agent for the same wrongdoing could also be translated into no difference of moral judgments toward the agent when being oneself the victim of the harmful act, compared to being a mere observer of it. We cannot, however, draw strong conclusions about this result, first because it was not preregistered and/or designed for this purpose. But also given this confound between social distances to the agent and to the victim which were not systematically manipulated (i.e., only two conditions: close agent and self as victim vs. distant agent and self as observer). Again, this highlights the crucial importance to consider all intertwined relationships when designing such an experiment on social distance influence.

Experiment 5[72]—Close vs. Distant Other as Victim with Self as Agent

Rationale

As previously argued, social distance to the victim was found to modulate altruistic versus egoistic decision processes by resolving conflicts more efficiently in cases where close others are involved (Zhan et al., 2018). Yet, it did not significantly influence altruistic (vs. egoistic) choices when using another material involving moral situations that are more likely to occur in real life (see Singer et al., 2019). Using the same material—the Everyday Moral Conflict Situations (EMCS)—in another research, the authors found on a male-only sample that participants acted more altruistically when a close (vs. a distant) other was involved as the recipient (or potential victim) of the act (Singer, Binapfl, et al., 2021). They, however, later did not find this social distance effect with a mixed sample including female and male participants (Singer, Sommer, et al., 2021). This could suggest that there might be differences in social distance effects that are linked to participants' gender.

[72] This work has been published as: Szczepaniak, Z., Gaboriaud, A., Quinton, J.-C., & Smeding, A. (2024). The Effects of Social Distance and Gender on Moral Decisions and Judgments. *Social Psychology*, *55*(1), 25–36. https://doi.org/10.1027/1864-9335/a000537

In an endeavor to clarify these findings, we first performed a reanalysis of the data from Singer et al.'s original study (2019, Studies 1 & 3) to further rely on their developed material and operationalization of social distance (and thus allowing us to conduct an accurate a priori power analysis) for our own experiment.

Despite difficulties in understanding what had exactly been done as analyses in their work[73], we performed a reanalysis of their data, which were freely available online (see 'Reanalysis' of the OSF page project). We started by strictly reproducing their results regarding social distance, which required combining participants from study 1 and study 3 data (N = 150) and retaining 40 scenarios. We conducted a Welch t-test at the scenario level (aggregating over participants). Following this procedure, we found the exact same results as reported in their manuscript, which signaled a non-significant effect of social distance on behavioral choices ($t(35.04) = -0.37$, $p = 0.72$, $d = 0.12$).

During the reanalysis of their study 3 data, relying on the 40 finally retained scenarios, we found a significant interaction effect between social distance and participants' gender on behavioral choices, and which had not been reported by the authors, $b = -0.10$, $SE = 0.03$, $t(98) = -2.86$, $p = .005$, 95% CI [-0.16, -0.03], $d = -0.30$[74]. Male participants had a percentage of altruistic responses significantly higher for scenarios with a close one (M = 0.64, SD = 0.19) than for scenarios with a distant one (M = 0.56, SD = 0.16), whereas this effect was not found for female participants. This result is, however, in line with findings of Singer, Binapfl, et al. (2021). Although not derived from a strong rationale in the literature that would justify such discrepancies between male and female participants, we further considered this factor when testing the effects of social distance in the current study, also in line with what previous exploratory results suggested (see Chapter 5, pp. 141; 158; 173).

[73] The authors (Singer et al., 2019) did not detail which items were used for analyses (i.e., study 1 contained 60 scenarios and study 3 contained 40 scenarios). Also, they analyzed the data of study 1 and study 3 together which, given the differences between the two studies, made it difficult to draw clear conclusions. Finally, it was not systematically clear whether the authors performed an analysis of variance (ANOVA) per item or per participant.

[74] Here reported using an ANOVA F1 equivalent analysis, which was confirmed using ANOVA F2 and binomial GLMM (see 'Reanalysis' on the OSF page).

In the first part of the current preregistered study (part A), we aimed at replicating the main effect of social distance toward the victim on behavioral choices, which may be particularly true for male participants, as previously found with our reanalysis of Singer et al.'s data (2019, study 3). We expected the number of altruistic responses to be higher when the scenario involves a socially close other as the recipient or the victim of the act, compared to when it involves a socially distant other, and that this effect would be stronger for male than for female participants.

Beyond this replication aim, the second purpose of the present study (part B) was to test whether such social distance toward the victim effects would also be found on moral judgments (and not on altruistic vs. egoistic choices anymore).[75] To do this, we asked participants to go through the 40 moral situations again and to rate how moral the depicted (either altruistic or egoistic) behavior was. We hypothesized that altruistic behaviors would be judged as more morally right than egoistic behaviors. In addition, we predicted that the effect of social distance on moral judgments would be stronger for egoistic behaviors than for altruistic behaviors, with participants judging egoistic scenarios as more morally wrong when involving a distant other (vs. a close one). This hypothesis was derived from and refines a finding by Singer et al. (2019, study 2) signaling an effect of social distance on egoistic judgments only[76]. Finally, we also exploratorily tested how part A responses could have influenced part B responses (given the within-participant design), by first measuring the correlation between both, then by adding to the analyses in part B the factor corresponding to participants' behavioral choices in part A.

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[75] Note that the aim of this second part was originally framed in the preregistration form as well as in the published article as a manner to check the overlap between the two dimensions of altruism and morality, as often assumed as implicitly equivalent in previous research. We presently framed this aim in a quite different way to better fit with the current question of research, while still not changing the validity and relevance of our preregistered hypotheses. Presently, the comparison between altruism and morality measurement are framed as a way to tackle the difference between behavioral choices (altruistic vs. egoistic choice of action) and judgments (moral judgment of wrongness/rightness) (see Chapter 7, pp. 239–243 for more).

[76] Participants had to rate how much altruistic (vs. egoistic) an altruistic (or egoistic) act appeared to them. Beyond the main effect of the type of act to judge (altruistic vs. egoistic), the authors found an effect of social distance such that participants rated egoistic acts as even more egoistic when committed against a distant other compared to a close other ($t(37.89) = 2.11$, $p = .04$, $d = 0.69$ as reported by Singer et al., 2019, study 2).

Method

In this experiment that replicates Singer et al.'s study (2019), we manipulated social distance toward the victim of the moral situation, while the agent—the participant him/herself—remains constantly the agent across the various scenarios. All materials, preregistered form, script of analysis, and data are freely available on the OSF page of the project (https://osf.io/vxs96/).

Sample Size. To determine the sample size for this study, we ran a power analysis using G*Power (Faul et al., 2009, version 3.1.9.7) based on our reanalysis of Singer et al. (2019) (see 'Reanalysis' on OSF). We retained the smallest effect size of interest (SESOI), which was the main effect of social distance on behavioral choices: d = 0.17 (when including both social distance and gender in the analyses). With a power of 80%, 274 participants were required. We rounded up the required sample size to 300 participants in case we would have to a posteriori exclude participants. Preregistered exclusion criteria can be found on the project OSF page.

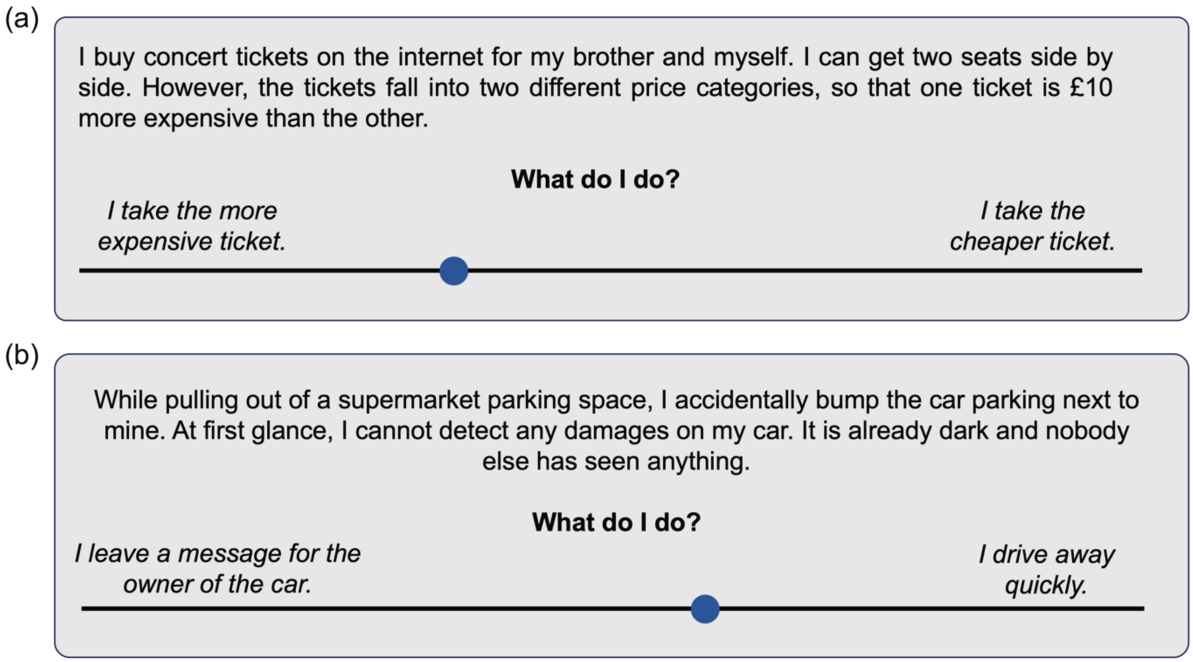
Participants. We recruited 300 participants via the Prolific platform. As the questionnaire was originally in English, we ensured that their first language was English, that they were currently living in the United Kingdom, and that they had the British nationality. An equal number of female and male participants was recruited to test the effect of gender. Participants were compensated £3.13 for study completion. The final sample was composed of 299 participants (149 females, one unreported, M_{age} = 39.93, SD_{age} = 13.77), as one participant's data were excluded for having failed the three attention checks. The study was conducted in accordance with the ethical standards of the ethical board and the ethical principles of the American Psychological Association and the Declaration of Helsinki.

Material. We used the EMCS created by Singer et al. (2019), comprising 40 everyday moral situations, with 50% of them including a distant other as the potential victim and the other 50% involving a close one as the potential victim. We say 'potential' victim as in their original material, after each scenario, the question 'What do I do?' is asked, and participants are invited to choose between an altruistic and an egoistic behavior (see

examples in Singer et al., 2019). Hence, if the egoistic option is chosen by participants, the potential victim becomes a real victim, as the egoistic act makes them suffer from a lack of help or support for instance, otherwise, they can be considered as a beneficiary of the situation (in case of altruistic choice). We will further refer to this role as the 'recipient', as a more neutral term that can either reflect the victim or the beneficiary depending on the situation. In the current study, for psychometric and statistical reasons (see Gautheron et al., 2023), we replaced the dichotomous response mode with a continuous response scale, to allow for more nuanced responses. The original behavioral choices served as the endpoints of the scale and were counterbalanced in terms of right/left order across trials. Participants could thus respond at the endpoints or anywhere in between (see Figure 6.1 for an example of scenarios).

Figure 6.1

Example of EMCS Items (a) with a Close Protagonist and (b) with a Distant Protagonist for Choices of Action (Part A)

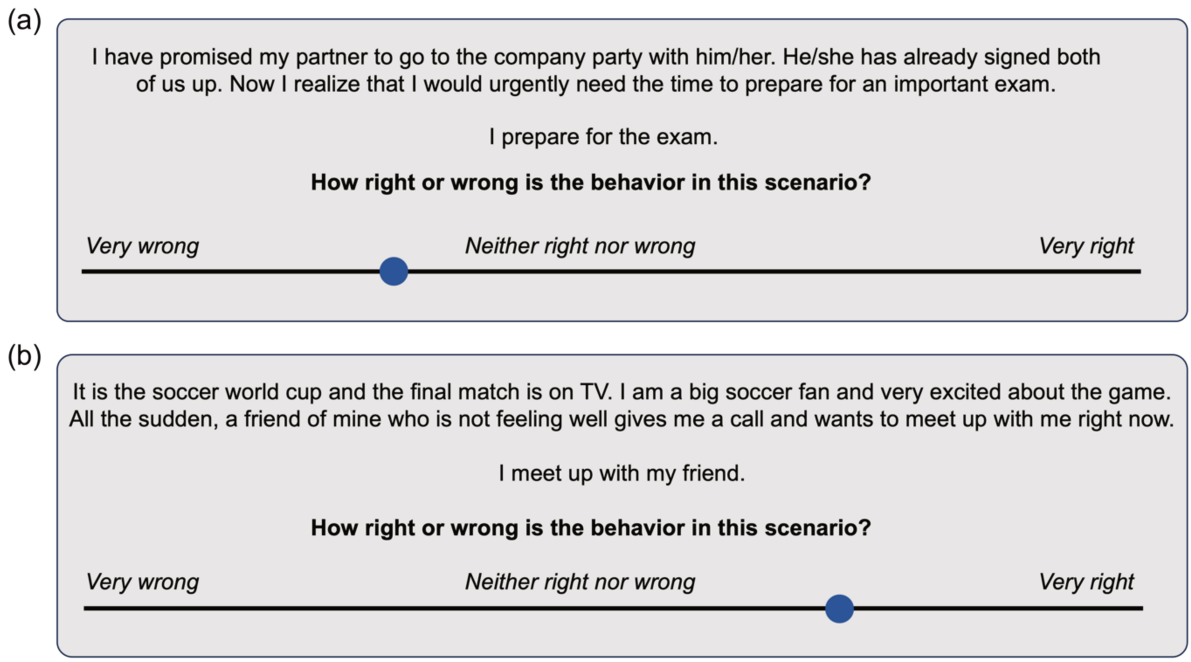


For the second part of this experiment (as summed up below in the procedure section), we presented to participants for each scenario of the EMCS an adapted version, so that the egoistic or altruistic behavior was automatically associated in advance in a

counterbalanced manner. Then, participants had then to provide in this part a moral judgment about each situation (i.e., 20 altruistic and 20 egoistic scenarios, varying across participants) (see Singer et al., 2019, study 2 for a similar procedure on altruism/egoism ratings). The question asked was 'How right or wrong is the behavior in this scenario?' and participants provided their moral judgments on a scale from (1) *very wrong* to (7) *very right*. Participants did not see the endpoint numbers on the scale during the task (see Figure 6.2 for an illustration).

Figure 6.2

Example of EMCS Items (a) with a Close Protagonist and (b) with a Distant Protagonist for Moral Judgments (Part B)

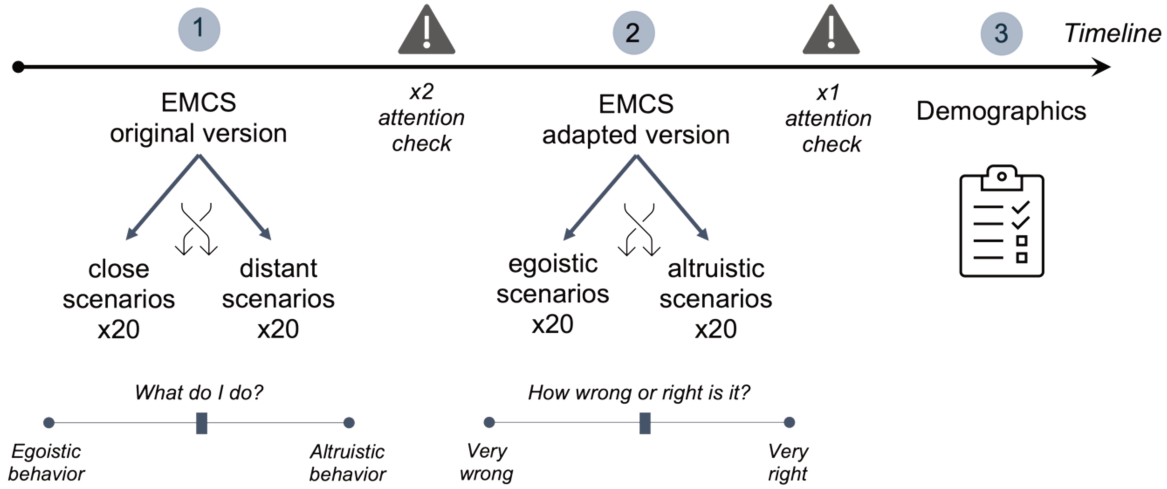


Procedure & Design. The questionnaire was presented as follows: Participants gave their informed consent, read the instructions, and first (part A) completed the EMCS scale as in Singer et al. (2019, study 3), using a slider to answer (see Figure 6.1). At the onset of each trial, the slider was placed in the middle of the scale. All participants completed the 40 scenarios, and the order of item presentation was randomized for each participant. Participants subsequently provided in part B moral judgments for the 20 altruistic and 20 egoistic scenarios, which were randomized across participants, again using a slider

(see Figure 6.2). Participants finally provided their age and gender. The questionnaire took about 25 minutes to complete. Three attention check items were included in the questionnaire (cf. 'Attention checks items' on OSF). The entire survey, as presented to participants, can be found on the project OSF page (see also Figure 6.3 for a visual overview of this experiment procedure). For behavioral choices (part A), gender was a between-participant independent variable, and social distance was a within-participant and *between-scenario* independent variable. For moral judgments (part B), the scenario version (i.e., 20 altruistic and 20 egoistic scenarios) was added as a third independent within-participant and within-scenario variable.

Figure 6.3

Schematic Representation of the Procedure Followed in Experiment 5



Analytical Details. To avoid ambiguity regarding the level of statistical analysis—participant or item level (as in our reanalysis of Singer et al's data, 2019), we preregistered an analysis of variance per participant (ANOVA F1) and per item (ANOVA F2). For clarity and brevity reasons, we report below only the results of the participant-level analyses, with the per-item level analyses reported on the project OSF page (along with the [G]LMM analyses). Analyses were performed with R programming language (v4.1.1; R Core Team, 2021), notably with the *lme4* (Bates et al., 2014), *DHARMa* (Hartig, 2015/2024), and *emmeans* (Lenth et al., 2025) packages. Results of the per participant analyses using mixed

effects models were reported in the main text. For behavioral choices (part A), as we counterbalanced the altruistic/egoistic choices position, we reversed half of the items, resulting in all items ranging from (0) *egoistic* to (100) *altruistic*. Moral judgments (part B) were coded with a continuous scale ranging from (1) *very wrong* to (7) *very right*. We recoded gender, social distance, and the scenario version variables with the following contrasts: female (-0.5) vs. male (0.5), close (-0.5) vs. distant (0.5), and egoistic (-0.5) vs. altruistic (0.5). All relevant variables and their interactions were entered into the models for parts A and B. The alpha threshold was set at .05.

Results

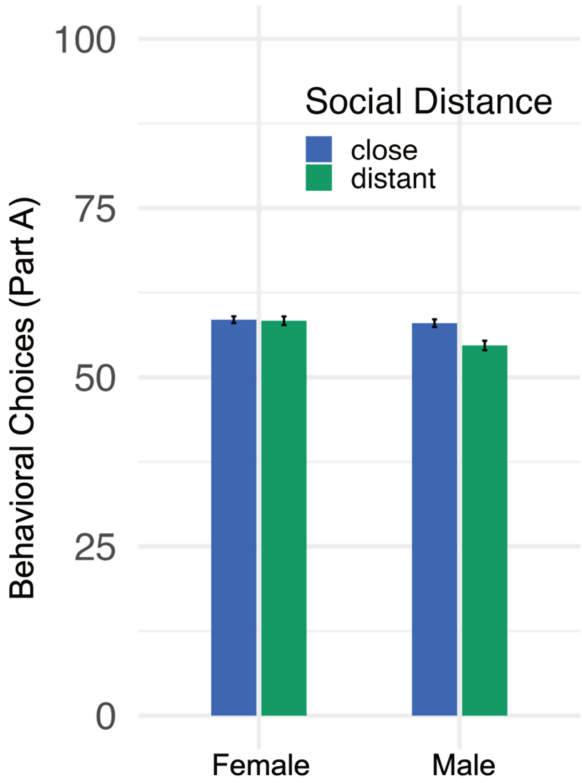
As a reminder, we hypothesized, for part A, a main effect of social distance on behavioral choices, such that participants would make more altruistic choices in proportion when a close (vs. a distant) other is involved in the scenario as the recipient. We also predicted it would be especially true for men, as previous research suggested. In part B, we predicted that scenarios where an egoistic (vs. altruistic) choice was made would be judged as more morally wrong (vs. right) and that social distance effect would be particularly true for egoistic scenarios (i.e., judging egoistic decisions involving distant others as more morally wrong compared to close others). Finally, exploratorily linking part A and part B, participants who chose more altruistic behaviors in part A would judge egoistic (vs. altruistic) behaviors as more morally wrong (vs. right) in part B compared to participants who chose less altruistic behaviors in a first instance.

Behavioral Choices (Part A). We found a significant effect of social distance on behavioral choices, with participants reporting more altruistic behavioral choices when a close one was involved than when a distant other was involved, b = -1.72, SE = 0.74, t(296) = -2.32, p = .021, 95% CI [-3.18, -0.26], d = 0.12. The main effect of gender was not significant, b = -2.08, SE = 1.53, t(296) = -1.36, p = .175, [-5.09, 0.93], d = -0.14. As expected, these main effects were qualified by a significant interaction effect between social distance and gender, b = -3.14, SE = 1.48, t(296) = -2.11, p = .035, [-6.06, -0.22], d = -0.11 (see Figure 6.4). Precisely, the effect of social distance on behavioral choices was

significant for males, *b* = -3.29, *SE* = 1.05, *t*(296) = -3.14, *p* = .002, [-5.36, -1.23]. When the protagonist in the scenario was a close one, males reported more altruistic choices (*M* = 58.00, *SD* = 13.71) in comparison to when it was a distant one (*M* = 54.71, *SD* = 17.01). For females, this difference was not significant, *b* = -0.15, *SE* = 1.05, *t*(296) = -0.15, *p* = .88, [-2.22, 1.91]. Women in our sample made almost an identical amount of altruistic choices for close (*M* = 58.51, *SD* = 12.05) and for distant recipients (*M* = 58.35, *SD* = 15.46).

Figure 6.4

Behavioral Choices (Part A) Depending on Participant's Gender and Social Distance



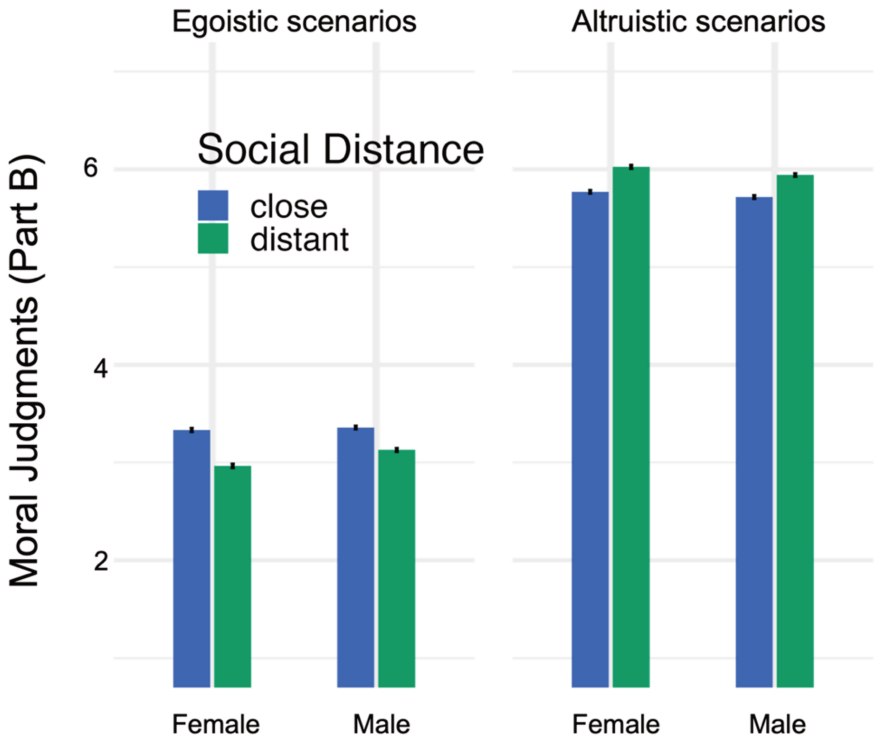
Note. Error bars represent standard errors of the means at the participant level; higher (vs. lower) values correspond to more altruistic (vs. egoistic) choices.

Moral Judgments (Part B). On data relying on the second part of this experiment, we found a significant and large effect of the scenario version (i.e., egoistic or altruistic scenario) on moral judgments, *b* = 2.67, *SE* = .06, *t*(296) = 41.89, *p* < .001, 95% CI [2.54, 2.79], *d* = 3.80, with participants judging egoistic scenarios more morally wrong (*M* = 3.20,

SD = 0.73) than altruistic scenarios (*M* = 5.86, *SD* = 0.67). As expected, this effect was qualified by a significant interaction effect between the scenario version and social distance on moral judgments, *b* = 0.54, *SE* = .05, *t*(592) = 11.65, *p* < .001, [0.45, 0.63], *d* = 0.38. The effect of social distance on moral judgments for egoistic scenarios was significant, *b* = -0.30, *SE* = 0.03, *t*(592) = -9.12, *p* < .001, [-0.36, -0.23], with participants judging egoistic scenarios as less morally wrong when the protagonist in the scenario was a close one (*M* = 3.35, *SD* = 0.70) than when it was a distant one (*M* = 3.05, *SD* = 0.75) (see Figure 6.5). The effect of social distance on moral judgments for altruistic scenarios was also significant but reverted, *b* = 0.24, *SE* = 0.03, *t*(592) = 7.35, *p* < .001, [0.18, 0.30], with participants judging altruistic scenarios more morally right when the protagonist involved was a distant one (*M* = 5.98, *SD* = 0.68) than when it was a close one (*M* = 5.74, *SD* = 0.66).

Figure 6.5

Moral Judgments (Part B) Depending on Participants' Gender, Social Distance, and the Scenario Version



Note. Error bars represent standard errors of the means at the participant level; higher values correspond to more morally right judgments.

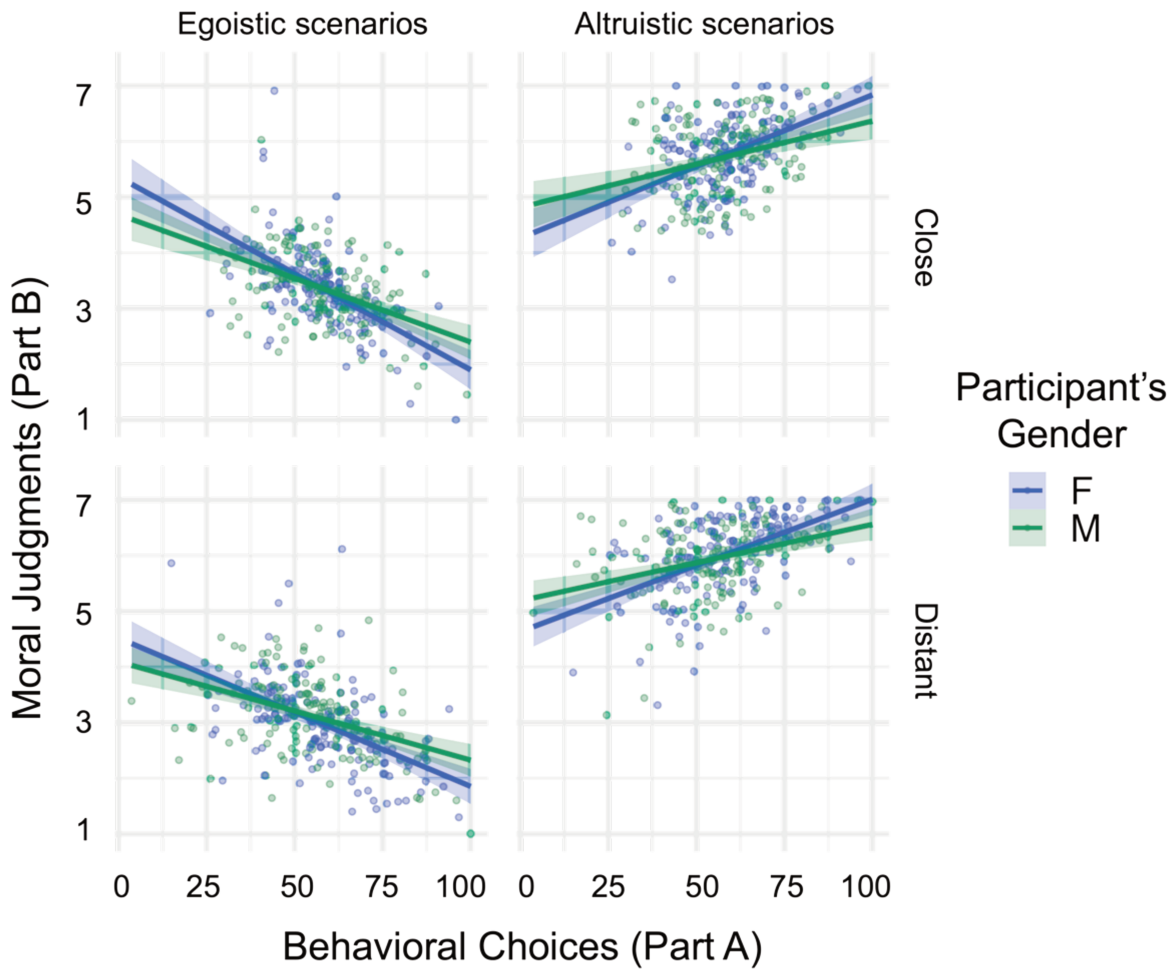
Finally, the exploratory analyses revealed that the three-way interaction effect between the scenario version, social distance, and gender was not significant, b = -0.17, SE = 0.09, t(592) = -1.81, p = .071, [-0.35, 0.01], d = -0.06. For altruistic scenarios, the two-way interaction between social distance and gender was not significant (b = -0.03, SE = 0.07, t(592) = -0.45, p = .65). It was yet significant for egoistic scenarios (b = 0.14, SE = 0.07, t(592) = 2.11, p = .035), with social distance to the victim effect being stronger for females (b = -0.37, p < .001) than for males (b = -0.23, p < .001). No other significant main or interaction effects were found ($|t_s| \leq 1.27$, $p_s \geq 0.20$).

Behavioral Choices (Part A) and Moral Judgments (Part B). Regarding the link between participants' behavioral choices in part A with their moral judgments in part B of the current study, we found there was a significant correlation when relying on Pearson's correlations across participants. As expected, the more altruistic responses participants gave in part A, the more they judged as morally wrong the egoistic scenarios in part B, r = -.49, t(296) = -9.76, p < .001, 95% CI [-.57, -.40]. Symmetrically, the more altruistic responses participants gave in part A, the more they judged as morally right the altruistic scenarios in part B, r = .42, t(296) = 8.03, p < .001, [.32, .51].

To refine the results for part B and for the reported correlations, we performed additional and exploratory linear mixed model analyses, including all variables of interest and their interactions. Beyond the exact confirmation of results reported for part B (i.e., main effect of the scenario version on moral judgments, and its moderation by social distance), we also found a significant interaction effect between the scenario version (in part B) and behavioral choices (in part A), b = 0.04, SE = 0.003, t(863.97) = 15.83, p < .001, [0.04, 0.05], d = 0.03. The decomposition of this interaction while controlling for the other factors confirmed the zero-order correlations, with a significant negative relationship between behavioral choices (part A) and judgments for egoistic scenarios (part B), b = -0.02, SE = 0.002, t(823.07) = -13.59, p < .001, [-0.03, -0.02], and a significant positive relationship for altruistic scenarios, b = 0.02, SE = 0.002, t(802.83) = 10.49, p < .001, [0.01, 0.02] (see Figure 6.6).

Figure 6.6

Relationship between Behavioral Choices (Part A) and Moral Judgments (Part B) Depending on Participant's Gender, Scenario Version, and Social Distance



Note. Error bands represent estimated conditional standard errors of the regression; points represent the associated participants' mean responses; higher values correspond to more altruistic behavioral choices on the abscissa, and more morally right judgments on the ordinates.

This interaction was furthermore qualified by a three-way interaction effect with gender, b = -0.01, SE = 0.005, t(863.97) = -2.84, p = .005, [-0.03,-0.005], with a significant gender-by-behavioral choices interaction for egoistic scenarios, b = 0.008, SE = 0.004, t(823.07) = 2.40, p = .02, [0.002, 0.02], and a reversed yet non-significant interaction for altruistic scenarios, b = -0.007, SE = 0.003, t(802.83) = -1.92, p = .06, [-0.01, 0.0002] (see

Figure 6.6). The simple effects of behavioral choices were all significant, and their amplitudes were larger for female participants than for male participants for both egoistic and altruistic scenarios ($|b_s| \geq 0.01$, $SE \leq 0.003$, $|t_s| \geq 6.32$, $p_s \leq .001$). All other interaction effects were not significant ($|t_s| \leq 1.35$, $p_s \geq .17$).

Discussion & Limitations

In this study, our first objective was to examine how moral decision-making in everyday settings is influenced by social distance toward the victim. To do this, we performed a reanalysis and conceptually replicated previous findings of Singer et al. (2019). We found, as expected, a gender-by-social distance interaction effect on the EMCS scale, with participants—especially males, but not females—choosing more altruistic behaviors when the involved protagonist was a close (vs. a distant) other. This result is consistent with Jones' literature review (2021) on social discounting effects (i.e., altruistic behaviors decreasing with social distance), and confirms Brañas-Garza et al.'s claim (2010) that social distance is a determinant of altruistic behavior.

Regarding gender differences, although other findings in the literature reported gender differences in moral decision making (e.g., Friesdorf et al., 2015; Youssef et al., 2012), only our reanalysis and integration of Singer et al. (2019) and Singer, Binapfl, et al. (2021) pointed to the possibility of a gender-by-social distance interaction effect. The present research thus provides integrated and sound empirical evidence, but it did not investigate possible explanatory mechanisms. As it is not the first focus of the current question of interest, findings about social distance effects' moderation by gender will be further discussed in Chapter 9, and several paths of possible interpretations for these tendencies will be explored.

A second objective of the current experiment was to extend Singer et al.'s findings (2019) by investigating—beyond mere behavioral choices—the impact of social distance on moral judgments. Some research suggests that judgments and choice of action do not provide the same pattern of results within moral decision-making (e.g., Tassy, Deruelle, et al., 2013; Tassy, Oullier, et al., 2013). We therefore tested how participants' gender, social

distance, and scenario version (i.e., altruistic vs. egoistic) influenced participants' judgments of moral rightness/wrongness in everyday moral settings. Using an adapted version of the EMCS, we observed that egoistic scenarios were perceived as much more morally wrong than altruistic scenarios and vice versa. Moreover, this effect also depended on social distance, with participants judging egoistic behaviors against distant others as more morally wrong than those perpetrated against close ones. They also judged altruistic behavior in favor of a distant other (vs. a close one) as more (vs. less) morally right. Participants were thus a bit more lenient when judging egoistic behaviors committed toward a close one, and judged as a bit more praiseworthy altruistic behaviors in favor of distant others.

This asymmetrical tendency seems somewhat consistent with other research, at least for the altruistic side. In line with a specific moral obligation to family, McManus et al. (2020) demonstrated that participants judged agents who helped strangers more positively than agents who helped kin (but this effect reversed when they helped strangers instead of kin). However, when dealing with antisocial or egoistic acts, the authors found that agents who failed to help strangers were judged less harshly than those neglecting kin. This is somewhat contradictory to our results, as we found that participants, in the opposite way, judged egoistic behaviors against a close one as less morally wrong than when committed against a distant other. This could stem from the nature of the moral scenarios per se, and from the severity of the situation at stake. In low-severity situations, we could indeed think that people easily blame others more harshly for acting egoistically against an unknown other rather than a close one (e.g., not giving up your seat to an elderly person vs. to a friend in public transport). More severe behaviors may yet trigger the reverse pattern, with harsher judgments for egoistic acts directed against one's close one compared to those directed against a stranger (e.g., saving your life first in a shipwreck rather than an unknown other's life vs. that of your own child). This parameter of severity will be further explored as a moderator of social distance effects in Chapter 7 (pp. 244–260).

In order to generalize our findings on one hand to items and on the other hand to both items and participants, we conducted an ANOVA per item and a LMM (i.e., with two

random factors) reported on the project OSF page. Given the restricted number of items in the EMCS (i.e., 20 socially close and 20 socially distant items) and the social distance manipulation (i.e., between-scenario variable), these analyses lacked statistical power. Therefore, in future research, the EMCS should be adapted so that social distance could be manipulated within items (as done originally in Zhan et al.'s research, 2018). In addition, to increase the item sample size and make the EMCS less oriented toward norm transgression, several new scenarios could be created following the many other dimensions possible to consider in morality according to the moral foundation theory (e.g., authority/subversion; purity/sanctity, see Graham et al., 2013).

In the same methodological vein, we adapted the EMCS with a continuous measure to better nuance participants' behavioral choices. This took us slightly away from an exact replication of Singer et al. (2019). However, when dichotomizing the present continuous choices, results were the same (see 'Analysis (Qualtrics survey)' on the OSF page). Benefits related to continuous scales may outweigh possible drawbacks, which does not prevent future research from specifically investigating how continuous (vs. dichotomous) response modes may influence moral decision-making (see Gautheron et al., 2023 for an innovative empirical test of this question).

Finally, according to literature, culture can influence moral decision making (e.g., Graham et al., 2016; Haidt, 2001; Yates & de Oliveira, 2016) and more specifically altruistic behaviors (e.g., Jones, 2021; Strombach et al., 2014). In the current study, our sample was of only British people, whereas the original material was created on a German sample (Singer et al., 2019). Even if there are differences between British and German cultures, they share a lot of similar occidental values (considered as individualistic societies), which probably minimizes differences, resulting in consistent findings. However, as Strombach et al. (2014) indicated that Western and Eastern cultures differ in altruistic behaviors, future studies may replicate the present research on a sample from an Eastern culture (see also the multi-lab replication study by Bago et al. in 2022 focusing on moral trolley-like dilemmas specifically across US, Western, Eastern, and Southern cultural clusters).

Experiment 6[77]—Close vs. Distant Other as Victim with Self as Agent

Rationale

In the previous experiment, we demonstrated that social distance toward the recipient of an altruistic or egoistic act has an impact on how much altruistically people tend to behave and on how moral they judge the situation to be. As noted above, one main limitation of this experiment was the restricted number of scenarios within each social distance condition (i.e., 20 socially close and 20 socially distant, each being distinct), thereby constraining the statistical power to detect the effects of interest within a between-scenario manipulation of social distance. To pursue our investigation of social distance effects when manipulated toward the victim of the situation, we further tested in the current experiment the effects analyzed in Experiment 5, but with a more powerful design. To do so, we built in the current study an adapted version of the Everyday Moral Conflict Situations (EMCS), originally built by Singer et al. (2019), comprising one socially close and one socially distant version *for each scenario*. This allowed to compare social distance effects as a within-scenario variable.

Moreover, as the focus of the experimental part until now (all along Chapter 5) was on the *dark side* of morality (e.g., antisocial acts, moral transgressions), we further wanted for the current experiment to focus again specifically on the egoistic scenarios of the EMCS (contrarily to what has been done in Experiment 5). As a necessary consequence, we focused in the current experiment on moral judgments of wrongness rather than on behavioral choices (as participants could no longer choose whether to act in an altruistic or egoistic manner when presented only with the egoistic version of the scenario).

Finally, one hypothesis raised in Experiment 5 discussion to explain the divergent effects of social distance within egoistic acts (compared to literature findings) was about the severity of the situation at play. As a reminder, we found in Experiment 5 that people judged egoistic behavior committed against a close one as less morally wrong than those

[77] This experiment has been firstly reported in the master dissertation of Zoé Szczepaniak: Szczepaniak, Z. (2023). The Effect of Social Distance, Severity, and the Participant's Sex on Moral Judgments in Everyday Life Scenarios, supervised by Gaboriaud, A., Quinton, J.-C., and Smeding, A.

perpetrated against a distant other. Whereas findings of literature seem to suggest the opposite pattern, with people being harsher in their judgments toward an agent who failed to help a distant other compared to an agent failing to help a close one (McManus et al., 2020). As found by Weidman et al. (2020), the severity of the situation moderated the effects of social distance, at least when manipulated toward the agent of the situation. The authors indeed found the effect of higher protection for a close agent compared to a distant agent to be even stronger (higher gap) in high-severity situations compared to low-severity situations. We then decided to also adapt the currently used material to manipulate the severity of the situation at play. Findings regarding the severity of the situation and its potential interaction effect with social distance will, however, not be detailed in the current section, as it is the focus of Chapter 7 dealing with the moderators of social distance (see especially pp. 257–260).

In the current study, participants mainly had to judge how morally right or wrong were the depicted situations (i.e., egoistic behaviors of the EMCS), depending on the victim's identity (i.e., either a close one or a distant other), and depending on the severity of the situation in terms of consequences for the victim (i.e., either low or high in severity). They afterwards had to rate, as a manipulation check, each of the scenarios seen in the first part in terms of their perceived social distance—via the Inclusion of Other in the Self scale (IOS, Aron et al., 1992)—and perceived severity.

Regarding social distance main effect, we expected that people would judge their own egoistic acts committed against a close one as less morally wrong than those perpetrated against a distant other (in direct replication of the effect previously found in Experiment 5, part B). But this would be especially true for low severity scenarios, and not for more severe situations where we would expect the reversed pattern (i.e., harsher judgments for egoistic acts perpetrated against a close one compared to a distant other). We also hypothesized that people would overall judge their low-severity egoistic acts as less morally wrong than their high-severity egoistic acts. Finally, to replicate the previous experiment, we also considered participants' gender in our analysis, predicting a stronger

effect of social distance to the victim (for egoistic acts perpetrated against close or distant others) for males than for females[78]. In an exploratory vein, we examined the interaction between social distance, severity, and participants' gender, too.

Regarding manipulation checks, we tested whether participants well perceived close protagonists as socially closer than distant protagonists, and high-severity scenarios as indeed more severe than low-severity situations. As an exploratory deepening, we also checked how these subjective ratings of social distance toward the victim and severity of the scenarios predicted participants' moral judgments, in interaction with participants' gender.

Method

To conduct this study, we then roughly used the same materials and paradigm as in Experiment 5 above, slightly adapted so that each egoistic scenario has a close vs. distant victim version, and a low vs. high severe outcome version. All the materials, preregistration form, analytic code, and data can again be found in the OSF page of the project (https://osf.io/kr9ef/). As for the previous experiment, the current protocol was approved by the local ethical committee.

Sample Size. To determine the required sample size for this study, we conducted a power analysis via G*Power (Faul et al., 2009). Based on our reanalysis of Weidman et al.'s data (2020) who manipulated both social distance and severity, we found an equivalent Cohen's *d* of 0.13 for the interaction between social distance and severity. By setting the alpha level at .05 and SESOI for this interaction effect at *d* = 0.13, we needed a sample size of 324 participants to reach a statistical power of 80% (cf. "PowerAnalysis_GPower" on the OSF page). Note, however, that social distance in Weidman et al.'s study (2020) is manipulated between the participant and the agent. In their research, participants were asked whether they would protect or punish the perpetrator of an immoral misdeed depending on their social closeness to him/her (Weidman et al., 2020). Our current

[78] This was what had been preregistered in relation to findings of Exp.5-part A. However, with regard to Exp.5-part B results on moral judgments specifically (hence closer to the current experiment), we could have expected the reversed pattern. For egoistic scenarios and on moral judgments, we indeed previously found that the effect of social distance to the victim was stronger by females than males (see interaction between gender and social distance for egoistic scenarios only).

estimation of the effect size for this effect could therefore be biased, as we target in the present experiment the social distance toward innocent victims of the situation.

Due to poor certainty about this effect size, we conducted as recommended by Lakens (2014) sequential analyses. Based on an a priori maximal sample size of 350 participants (in accordance with the previous estimation, but also considering limited financial funds to run this study), we fixed a first stop at $N = 150$. As at 150 participants, the estimated effect size for the interaction between social distance and severity was below 0.13 (in Cohen's *d* equivalent), we stopped data collection at that point (i.e., otherwise, we would have needed more than 350 participants[79] to find a significant effect)[80].

Participants. Participants ($N = 151$) were recruited via on online experimentation platform. Participants were above 18 years old, and as the questionnaire was in English, we requested the participants' first language to be English, that they were living in the United Kingdom, and had the British nationality. Those criteria were chosen to minimize the introduction of new sources of variability compared to our previous study (see Exp.5 above). Also, an equal number (on average) of female and male participants was recruited to include gender as a factor in our analytical model. Participants were compensated £3.94 for study completion. Our final sample was composed of 149 participants (76 females, $M_{age} = 39.30$, $SD_{age} = 12.99$, with age ranging from 18 to 74) as we had to exclude two participants for not answering correctly the three attention checks[81] (see 'Analytical Details' section below and the OSF "Data Analytic Plan" component for more).

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[79] Considering a margin of on average 20 participants, in case of exclusions (see first estimation of sample size required at $N = 324$).

[80] This first stop also allowed us to get the variance partitioning coefficients (i.e., VPC) to run a proper power analysis with PANGEA (Westfall, 2016) in order know whether we had sufficient power to conduct mixed model analysis with two random factors (i.e., participant and scenario). See supplementary file on PANGEA analyses on OSF for more.

[81] Although preregistered with three attention checks (and criterion for exclusion being two or three failed attention checks), there were two attention items in part A in distinct places during the questionnaire (hence countered as two distinct attention items). Whereas for part B, the two attention items were displayed at the same time only once in the experiment, requiring one answer on the severity scale and another one on social closeness scale (in order not to disturb participants in the way information were visually positioned in the screen). These third attention check items in part B were considered as correct when both were successfully answered.

Material. In the purpose of this study, we built an adapted version of the Everyday Moral Conflict Situations (EMCS) from Singer et al.'s study (2019). The same material was already used in Exp.5 above, with only adapting the mode of response as continuous rather than dichotomic (part A) or discrete (part B). One main criticism yet (see 'Discussion' section of Exp.5 above) was that the original material contains 40 scenarios of everyday moral situations, with 20 scenarios involving a socially close person and the other 20 involving a socially distant other as the recipient of the either altruistic or egoistic action. This between-item distribution of the conditions of social distance did not allow to control for the variability of the scenarios themselves and entailed a reduced power in design.

To remedy these limitations, we offered in the current study an adaptation of the original EMCS where we varied the social distance with the recipient (close vs. distant) *within each scenario* (and not between scenarios anymore). We also chose for the current experiment to focus on moral judgments of egoistic behaviors, thus in direct replication of Exp.5-part B results (at least for egoistic acts). This choice allows us to more directly confront findings regarding social distance toward the victim in the current experiment with those about social distance toward the agent as previously investigated in Experiments 1 to 4 (focusing on moral judgments about everyday moral transgressions)[82]. Finally, as a complementary investigation, we manipulated in the current experiment the severity of the harm for the victim, hence creating for each scenario a new version with more (or less) severe consequences for the victim, depending on the original scenario severity (see Figure 6.7 for an illustration of the four possible versions for one scenario). The whole EMCS adaptation is available online in the OSF page (section 'Materials').

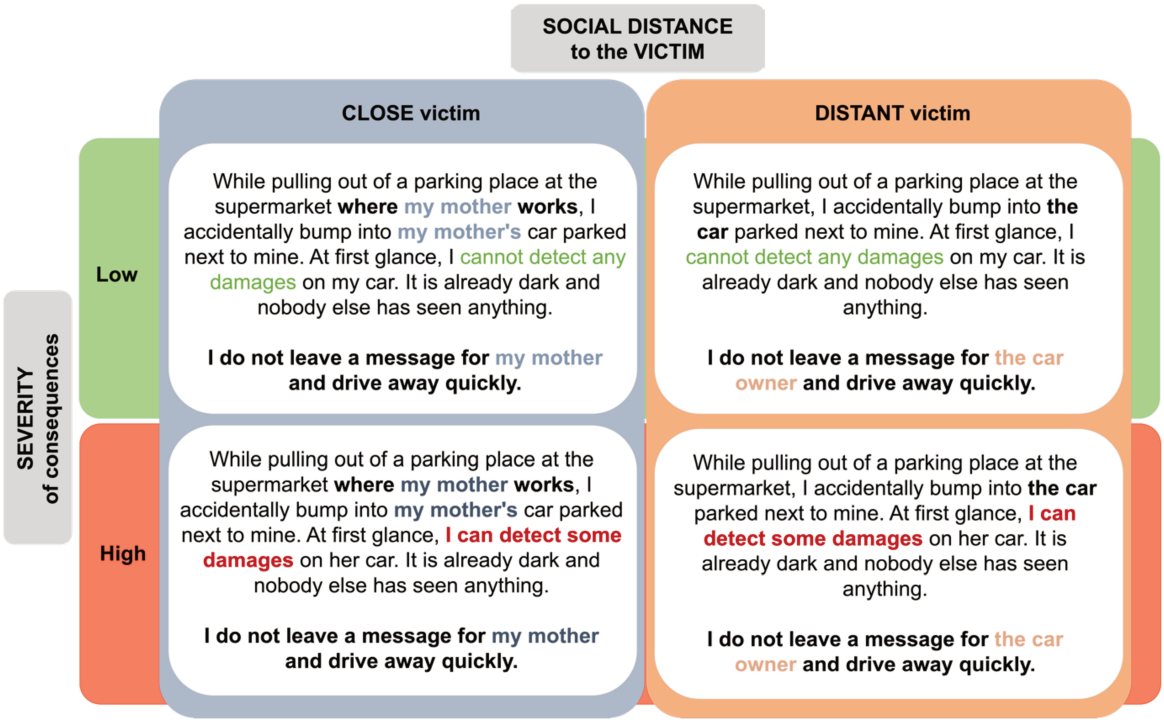
Procedure & Design. The study was posted online on the Prolific platform and took an average of 23 minutes to complete. After giving consent and receiving main instructions about the task, participants had to morally judge the 40 scenarios of the adapted EMCS in one of the 4 (social distance x2; severity x2) possible versions (see Figure 6.7) and

[82] Note that the current aim is not to explicitly test the difference in findings between both sets of studies. However, we can descriptively observe for each the directions of social distance effects and confront these findings regarding the main tendencies in literature.

displayed in a randomized order between participants. They had to answer the question 'How right or wrong is your behavior in this situation?' on a continuous scale from (1) *very wrong* to (7) *very right*. Participants as for Experiment 5 could not see the numbers of the slider extremity. The 40 scenarios were also counterbalanced within conditions of social distance and severity so that each scenario was seen an equivalent number of times in each possible version. In this first part of the study, we implemented two attention checks, one after the fifteenth scenario and one after the thirtieth scenario[83] (see Figure 6.8 for a complete procedure synthetic view, and OSF for the full questionnaire in PDF file).

Figure 6.7

An EMCS Item Adapted within Social Distance (x2) and Severity (x2) Conditions

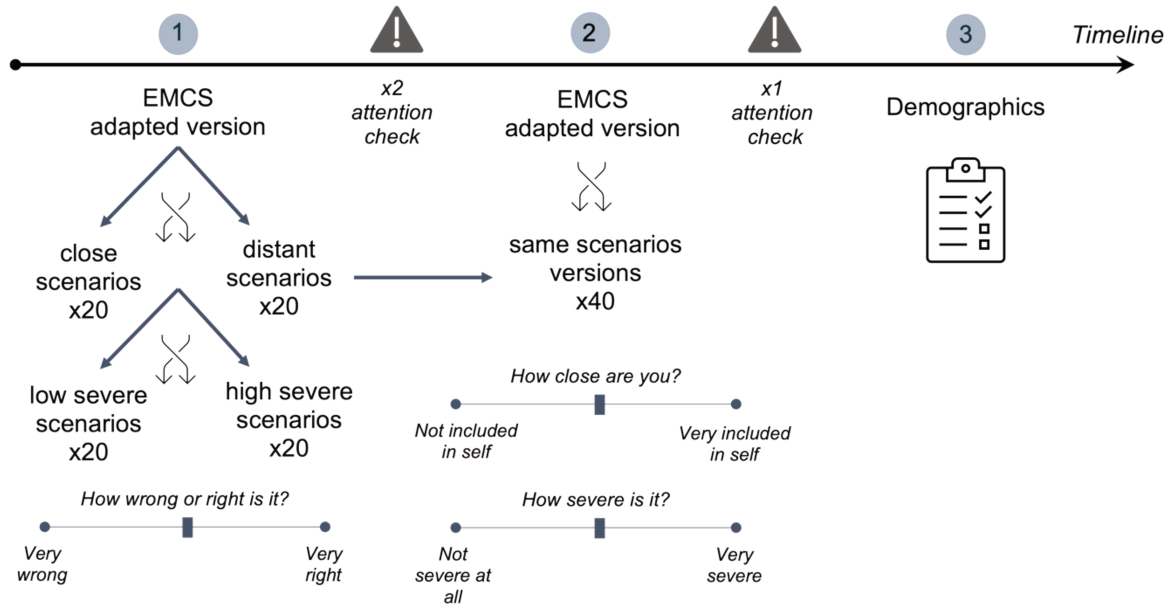


Note. The colors are used to enhance the readability of the figure: green vs. red text for changes relative to low vs. high severity respectively; blue vs. orange text for changes relative to close vs. distant victim. Bold texts represent changes made compared to the original version of the EMCS.

[83] Note that we preregistered exclusions based on failure or success on three attention checks, taking into account the two attention items from part 1, and one combined attention item in part 2. Given that there were two sliders during this part, we kept two sub-attention check items in part 2 in order not to change the visual aspect of the task from participants' point of view (although considering it correct only if both were successful).

Figure 6.8

Schematic Representation of the Procedure Followed in Experiment 6



In the second part of the study (for manipulation check purposes), participants saw again the 40 scenarios seen in the first part (in the same version they had judged earlier, so that the scenario processing is made easier as still vivid in memory). They had to rate, for each scenario, how socially close they think they were to the involved victim as measured through the Inclusion of Other in the Self scale (IOS) (Aron et al., 1992) on a continuous scale ranging from (1) *other not included in the self* to (7) *other included in the self*. They also rated the severity of the harm for the victim in the scenario (i.e., 'How severe is the action portrayed in the scenario, independently of who is the victim?') on a continuous scale from (1) *not severe at all* to (7) *very severe*. During this second part, we implemented only one attention check after the twentieth scenario. Finally, participants had to fill in some demographic data (i.e., age and sex) (see again the OSF complete questionnaire for more).

In terms of design, the first part followed a counterbalanced design (but not fully crossed to avoid block effects) with two independent variables (i.e., social distance to the victim and the severity of the scenario), one control variable (i.e., participants' gender), and one dependent variable (i.e., participants' moral judgments about their own hypothetical

egoistic acts). Each participant answered the 40 scenarios of the adapted EMCS counterbalanced in such a way that each participant randomly judged half of the scenarios involving a socially close one and the other half involving a socially distant other, crossed with half of the scenarios portraying a more severe action and the other half portraying a less severe action. As for the second part of the study, it followed the same design but with two dependent variables: participants' rating of their social closeness to the victim in each scenario and their perception of the severity of the harm. Each participant judged the same 40 scenarios seen in the first part in terms of social distance and severity version.

Analytical Details. To run the analyses, we used R programming language (v4.1.1; R Core Team, 2021), and some associated packages as *lmerTest* (Kuznetsova et al., 2017). The R script and anonymized data are available on our OSF page (cf. the "Data and Analysis Script" component). We planned to run a linear mixed model comprising two random variables (participant and scenario), as we wanted to generalize our results to both other participants and other scenarios (Judd et al., 2012)[84]. Three fixed factors were included in the model, all being coded through contrasts. For social distance, (-0.5) close vs. (+0.5) distant; for severity, (-0.5) low vs. (+0.5) high severity; and for gender, (-0.5) female vs. (+0.5) male participants. After having followed the reduction procedure from Matuschek et al. (2017) starting from the maximal model, we obtained the following reduced model:

$$\text{Moral Judgments} \sim \text{Distance} * \text{Severity} * \text{Gender} + (\text{Distance} \mid \text{Participant}) + (\text{Distance} + \text{Severity} + \text{Severity: Gender} \mid \text{Scenario})$$

With the first batch of participants (N = 149) and with the reduced model reported above, we found an effect size for the interaction effect between social distance and severity of d = 0.01, hence far below the a priori fixed SESOI of d = 0.13. We then stopped data collection at that point and performed the following analyses on the sample of 149 participants (as two participants were excluded for all attention checks failed). Finally, we used the Satterthwaite approximation for the estimation of degrees of freedom, the alpha

[84] We also ran analysis of variance per participants only (ANOVA F1) as a first instance but did not report them here (as mixed model with two random variables allows for more generalizability). You can find these complementary analyses in supplemental material on the OSF page.

threshold was set at 0.05 to determine the significance of our effects, and effect sizes were calculated as Cohen's *d*.

For the second part, we reduced again the random structure starting from maximal model comprising the same fixed factors and random variables as above. But we ran this model on participants' perception of social closeness as the dependent variable (to check for the manipulation of social distance), then on their perception of severity in another separate analysis. In an exploratory vein, we also performed an analysis on moral judgments given in the first part, with the same reduced model as reported above but considering participants' subjective ratings of social closeness and severity as independent variables rather than the manipulated variables.

Results

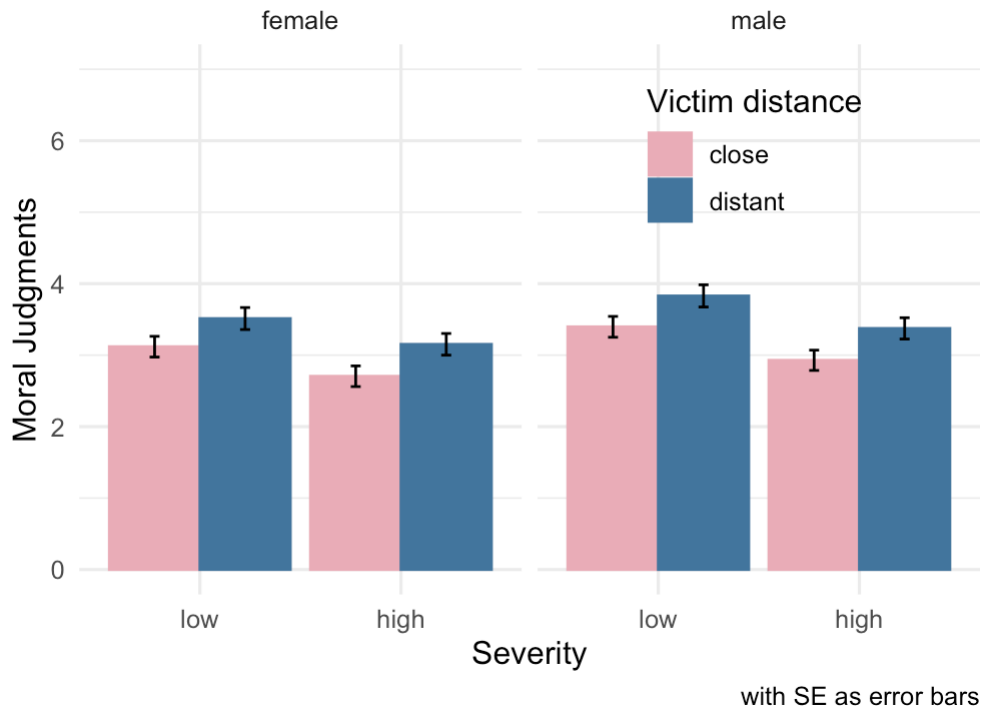
As a reminder, we expected in the main part of this study to find main effects of social distance to the victim and severity, as well as an interaction effect between both variables, with effects of social distance being reversed in direction depending on the severity condition. We also wanted to check whether social distance effect was again stronger for males than for females, as previously found in Singer et al.'s data (2019) reanalysis and in our data results from Experiment 5.

Confirmatory Results. As anticipated, analysis in mixed model revealed a main effect of social distance toward the victim on moral judgments, $b = 0.43$, $SE = 0.08$, $t(43.70) = 5.28$, $p < .001$, 95% CI [0.27,0.59], $d = 0.27$. Participants indeed judged their own egoistic actions against a close one as more morally wrong ($M = 3.04$, $SE = 0.12$) than their actions against a distant one ($M = 3.47$, $SE = 0.13$)—with the judgment scale going from (1) *very wrong* to (7) *very right*. A main effect of the situation severity was also found, with participants judging the high severity situations as more morally wrong ($M = 3.04$, $SE = 0.12$) than low severity ones ($M = 3.46$, $SE = 0.13$), $b = -0.42$, $SE = 0.07$, $t(38.36) = -5.84$, $p < .001$, [-0.57,-0.28], $d = -0.27$. These main effects were however not qualified by the expected interaction effect, $b = 0.03$, $SE = 0.06$, $t(5530.15) = 0.53$, $p = .59$, [-0.09,0.16], $d = 0.01$. In other words, the fact of judging situations involving a close one more harshly compared to

those involving a distant other was not significantly stronger in low (b = 0.41) or high (b = 0.45) severity situations (see Figure 6.9 for an illustration).

Figure 6.9

Moral Judgments Depending on Social Distance, Situations' Severity, and Participants' Gender



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more morally right (vs. wrong) judgments.

Gender Effects. Regarding effects depending on gender, we did not replicate the expected interaction effect between social distance and gender, b = 0.02, SE = 0.08, t(145.45) = 0.25, p = .81, [-0.13,0.17], d = 0.006 (i.e., no significant difference between how males and females considered situations involving a close one compared to those involving a distant other; b = 0.42 for females and b = 0.44 for males). Although not preregistered, analyses revealed a significant main effect of gender (b = 0.26, SE = 0.12, t(147.47) = 2.26, p = .03, [0.03,0.49], d = 0.16) with male participants judging their own egoistic acts on average slightly less harshly (M = 3.38, SE = 0.13) than female participants (M = 3.12, SE = 0.13), on the scale ranging from *very wrong* to *very right* (see Figure 6.9). The severity-by-

gender interaction as well as the three-way interaction between social distance, severity, and gender were not significant (b = -0.07, SE = 0.07, t(37.90) = -1.04, p = .31, [-0.22,0.07], d = -0.02, and b = -0.04, SE = 0.13, t(5540.67) = -0.31, p = .75, [-0.29,0.21], d = -0.006 respectively).

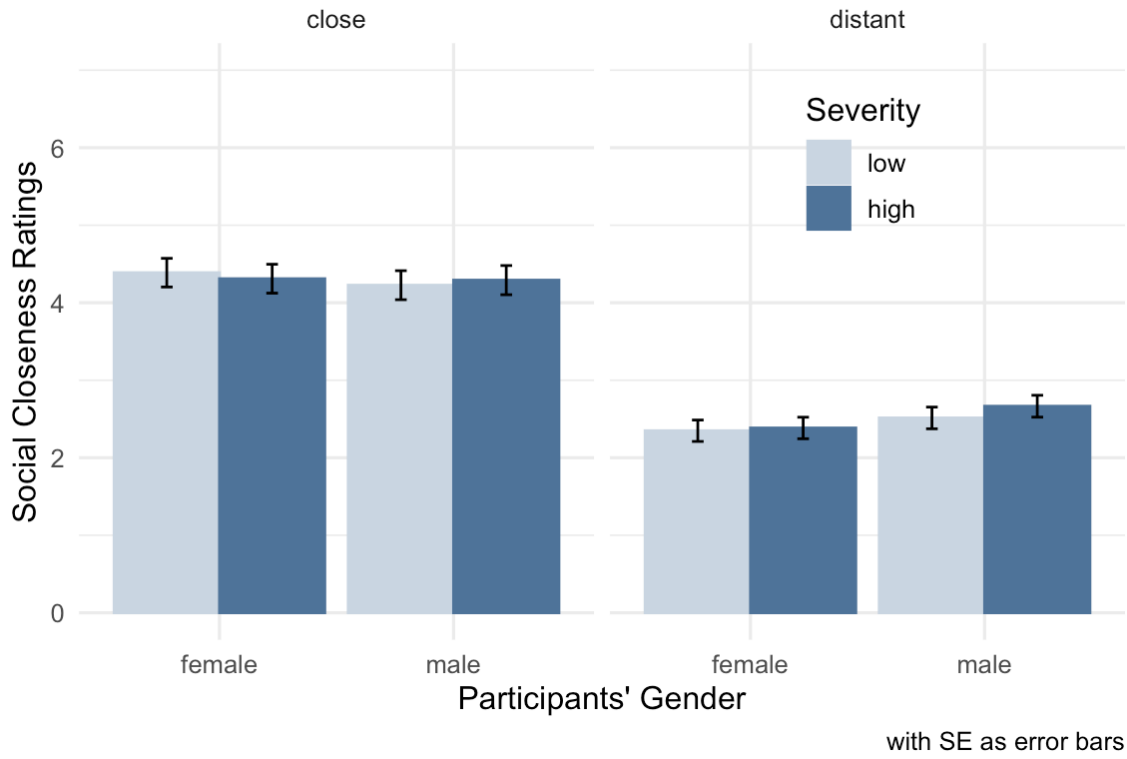
Manipulation Checks. In the second part of this study, we asked participants to again go through the whole scenario battery (in the same social distance-by-severity versions as previously encountered). In order to check whether our manipulation of social distance and severity worked as expected, we tested the same combination of fixed factors via new reduced mixed models[85] but on the ratings of perceived closeness first (with expectation of a main effect of social distance), and on ratings of perceived severity secondly (expecting a main effect of severity).

Running these analyses, we first found on ratings of perceived closeness—ranging from (1) *other not included in the self* to (7) *other included in the self*—a main and large effect of social distance, b = -1.83, SE = 0.17, t(97.24) = -11.06, p < .001, [-2.15,-1.50], d = -1.12. Participants indeed judged the protagonists involved in close scenarios as significantly more included in themselves (M = 4.30, SE = 0.16) than those involved in distant scenarios (M = 2.48, SE = 0.11) (see Figure 6.10). What is worth noting is that—although not predicted—we observed a significant severity-by-gender interaction effect on perceived social closeness ratings (b = 0.13, SE = 0.06, t(5558.82) = 2.31, p = .02, [0.02,0.24], d = 0.04). In addition to social distance manipulation itself, the severity of the scenario seemed to have an impact on how people rated the social closeness toward the involved protagonists, but mostly by males (b = 0.11, p = .007) and not by females (b = -0.02, p = -0.54). Male participants tend in our sample to judge protagonists of high-severity situations as socially closer to them (i.e., more included in their self) (M = 3.48, SE = 0.13) compared to protagonists involved in low-severity situations (M = 3.37, SE = 0.13) (see Figure 6.10). No other main or interaction effect was found on ratings of perceived social closeness.

[85] We started from same maximal model than in the first part, then again reducing the random structure of the model for each new dependent variable for the effects to be tested. Final complete reduced models can be found in supplemental material in the OSF page of the project.

Figure 6.10

Ratings of Perceived Social Closeness Depending on (Manipulated) Social Distance, Severity, and Participants' Gender



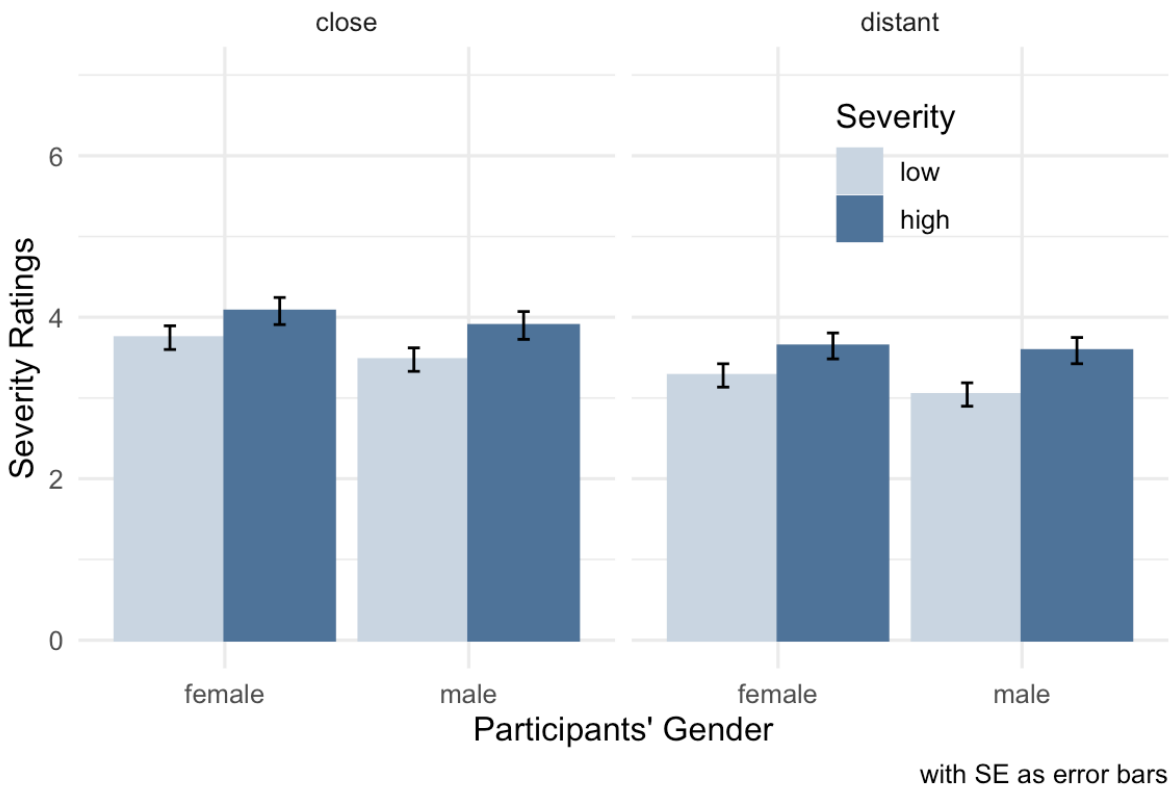
Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to the victim being perceived as socially closer (i.e., more included in the self) (vs. more distant) from participants' point of view.

For ratings of the perceived severity—ranging from (1) *not severe at all* to (7) *very severe*, we found as expected that the factor of severity had a significant effect, $b = 0.42$, $SE = 0.08$, $t(38.14) = 5.44$, $p < .001$, [0.26,0.57], $d = 0.25$. Participants perceived the high-severity situations (as manipulated) as indeed more severe ($M = 3.80$, $SE = 0.14$) in terms of the consequences for the victim than low-severity situations ($M = 3.39$, $SE = 0.12$). However, we also noticed a main effect of social distance on perceived severity ratings, although not explicitly hypothesized, $b = -0.41$, $SE = 0.08$, $t(53.57) = -5.13$, $p < .001$, [-0.57,-0.25], $d = -0.25$. This effect went in the direction of more severe ratings for scenarios involving close others as victims of the act ($M = 3.80$, $SE = 0.13$) compared to situations involving distant

others (*M* = 3.39, *SE* = 0.13). Finally, we also found a non-preregistered interaction effect between severity (as manipulated factor) and participants' gender on severity ratings, *b* = 0.14, *SE* = 0.06, *t*(5526.63) = 2.09, *p* = .04, [0.009,0.26], *d* = 0.04. As for social closeness perception, male participants perceived in a stronger way the manipulation of the situation severity (*b* = 0.48, *p* < .001) than females (*b* = 0.35, *p* < 0.001). No other main or interaction effects were found as significant on severity ratings (see Figure 6.11).

Figure 6.11

Ratings of Perceived Severity Depending on Social Distance, (Manipulated) Severity, and Participants' Gender



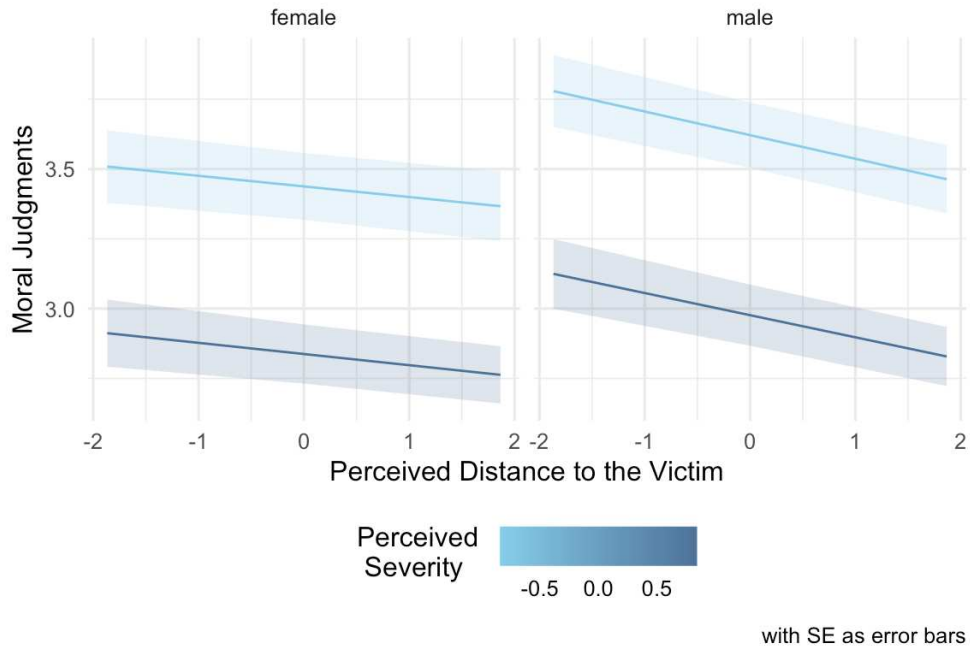
Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more severe (vs. less severe) ratings as perceived by the participants.

Exploratory Continuous Predictors. Finally, in an exploratory vein, we used these social closeness and severity ratings of participants as predictors in the mixed model analysis of the first part. We wanted this way to test whether the effects of social distance

and severity on moral judgments would still be significant considering how people subjectively perceived these dimensions. We thus transformed the previous dependent variables of perceived social closeness to the victim and of perceived severity as continuous and centered predictors and integrated them into the first model on moral judgments[86]. These exploratory analyses revealed again a main effect of perceived social distance going in the same direction than before[87], even though being considerably less robust than previously found with manipulated social distance (b = -0.06, SE = 0.02, t(81.64) = -3.97, p < .001, [-0.09,-0.03], d = -0.04). Moreover, the effect of severity was still significant and consistent in direction, b = -0.37, SE = 0.02, t(112.83) = -16.09, p < .001, [-0.42,-0.32], d = -0.27, although a bit lower in effect size (see Figure 6.12). No other main or interaction effect was significant.

Figure 6.12

Moral Judgments Depending on Perceived Social Distance to the Victim, Perceived Severity, and Participants' Gender



[86] Starting from the maximal model (including all correlations and possible components for random variables), we again reduced the model. The final reduced model can be found in the R script of the supplementary analyses on the OSF page.

[87] Although the sign of the parameter estimation for this effect is currently negative (vs. positive earlier), it remains the same direction of effect (i.e., judgments as more wrong when a close victim is involved compared to a distant victim) as the perceived distance is reverse-coded (high values = closer) compared to the manipulated factor of social distance (with +0.5 = distant condition).

Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more morally right (vs. wrong) judgments (originally going from 1 to 7, hence a zoomed view in the current graph). Higher (vs. lower) values on the abscissa axis (X) correspond to victims perceived as socially closer (vs. more distant) from participants' point of view. Dark blue line (vs. light blue line) corresponds to high (vs. low) severe situations, as perceived by participants.

Discussion & Limitations

In the current experiment, we aimed to replicate the effects of social distance toward the victim on everyday moral decision-making through the use of the EMCS (Singer et al., 2019). More specifically, we adapted this material to test the effect of social distance to the victim within antisocial acts and asked participants to give moral judgments about these situations. As argued earlier in front of previous findings, we also thought the differentiated effects of social distance in the context of egoistic or antisocial acts, sometimes found as opposite in direction, may stem from the severity of the situation at play. We thus also considered this manipulated factor in the current experiment.

We found a significant influence of social distance to the victim, although in the opposite direction to what we expected. Participants indeed judged their own egoistic actions against a close one as harsher than those perpetrated against a distant other, whereas the reverse pattern was found in Experiment 5 on moral judgments (part B). Current results are then more in line with literature predictions (see for instance McManus et al., 2020). We yet found a significant effect of severity. Consistently with expectations, people rated as more morally wrong the more severe acts compared to the less severe ones. However, no interaction effect between severity and social distance was found. In other words, the severity of the situations did not moderate the effect of social distance in our current sample of participants. We also wanted to replicate the previously observed social distance-by-participants' gender interaction effect for exploratory purposes. We did not find such an interaction, but a main effect of gender, such that males rated their own egoistic acts slightly less harshly than did female participants.

We may explain these discrepancies with previous findings by the change of the dependent variable (from a behavioral choice in Exp.5-part A, to moral judgments in Exp.6), but this does not resolve the inconsistency with the second part of Experiment 5, dealing with moral judgments. It could also come from the change in design as we currently have adapted the EMCS to fit with a within-participant and within-scenario counterbalanced design (although not fully counterbalanced). The present design should, however, be more powerful than the one of Experiment 5 (with a between-scenario distribution of social distance conditions), increasing the reliability of the present results. Another main change of this experiment was the introduction of the factor of severity (within-scenario manipulation). However, we do not think it could have hindered the effect of social distance.

We should also notice that the current experiment used mixed models with two random variables (participant and scenario), hence promoting the generalizability of the currently significant effects to both other participants and other scenarios (Judd et al., 2012). This is of crucial importance, especially in morality, where there is a large variability, be it within participants or within scenarios.

Regarding manipulation checks, we found the expected main effects of social distance on the perceived level of social closeness to the victim and of severity on the perceived level of severity. Although not expected, we also found an interaction effect between severity and gender on social closeness ratings, such that males' ratings of their closeness to the involved victims seemed to be influenced by the severity of the situation (with higher closeness ratings for more severe situations). Similarly, we found a main effect of social distance on severity ratings, with higher perception of severity in cases where close (vs. distant) others were involved. This reinforces the idea that social distance perception is to some extent influenced by the severity of the situation (and vice versa), at least for some participants. This may however also come from the fact that both questions of perceived social closeness and perceived severity were asked at once, hence allowing participants to match and/or adjust their answers between both.

Furthermore, the estimated effect size for the severity manipulation check (always asked after the social closeness rating) remains quite low (d = 0.25) compared to what would have been expected for such manipulation checks (as for social closeness, d = -1.12). A possible explanation, as suggested by the almost similar means and effect sizes for social distance and severity effects on severity ratings, is that participants misunderstood the severity question as a moral judgment question (as was the case in the first part)[88]. They indeed gave almost the same importance when rating the severity of the situation (in terms of how they perceived it) to the actual severity of the scenario as to the social distance toward the victim.

At the Intergroup Level

Rationale and Overview of the Experiments

In the last two sections, throughout Experiments 5 and 6, we tested the effects of social distance to the victim of one's everyday-like wrongdoing, focusing on the intraindividual (i.e., to keep distance to the agent constant) and interindividual levels. In the current section, we investigated the same question but at the intergroup level, that is, focusing on the situations where the victim is either an ingroup member or an outgroup member. As for the previously tackled levels, direction of the effects for social distance to the victim in literature is quite consistent, with ingroup victims being much more likely to receive help than those from the outgroup (e.g., Dovidio et al., 1997; Levine et al., 2002). More specific to the realm of moral transgressions, McKee et al. (2023) also demonstrated what they called the 'insider victim effect', that is, harmful acts perpetrated against ingroup victims being judged more harshly than those perpetrated against outgroup victims. We therefore expected to find the same tendency of results in Experiments 3 and 4, which follow quite a similar design compared to that of McKee et al. (2023) (i.e., manipulating both the victim's and agent's identities). As these experiments were already detailed in terms of rationale and

[88] Even though we explicitly asked them in the instructions of this second part to rate 'the severity of the action for the victim' by focusing on 'the severity of the action itself, no matter who is the person who suffers the consequence of it' (see supplementary material with full questionnaire).

method in Chapter 5, we will provide below only a brief reminder of the main hypotheses and design, then report findings specifically related to the effects of social distance to the victim.

Experiment 3[89] (Part B)—Ingroup vs. Outgroup Member as Victim and Self as Observer

Rationale

Experiment 3 aimed at analyzing the influence of social distance toward both the agent and the victim of everyday-like transgressions on moral judgments at the intergroup level. As the effects of social distance toward the agent have already been described in Chapter 5 (Experiment 3, part A, see p. 155), we will focus here on the effect of social distance to the victim (and associated analyses). Regarding this effect, we expected—consistently with previous results from well-powered design in Experiment 6[90]—that participants (in the position of a third-party observer) would judge moral transgressions committed against a close (ingroup) victim more harshly than those committed against a distant (outgroup) victim.

Method

To test this effect, we created a minimal group-like paradigm where participants (N = 161, 106 females, M_{age} = 28.59, SD_{age} = 13.05) were attributed to a specific group based on a minimal criterion (Luupites vs. Niffites). Please refer to Chapter 5 (pp. 147–155) for more details about sample size, participants, materials, procedure, and design (cf. OSF page: https://osf.io/4sym5/).

Analytical Details

As briefly mentioned in Chapter 5 for this experiment analytical procedure section, we found unexpected dependencies in the way conditions of social distance to the victim were distributed within scenarios and participants. This created a bias in the estimation of effects for social distance to the victim that could be controlled for by creating a new

[89] We especially thank Chloé Bernard as well as Zoé Szczepaniak, who provided a precious help for the data collection of this experiment.

[90] Note that in the real temporality, Experiments 5 and 6 were run before Experiments 3 and 4. We chose to number them in order of apparition in the dissertation manuscript to make the reading and understanding easier.

'reversed' factor. In order to fit with what had been preregistered, we first ran the analyses with the originally planned model (i.e., two random variables, participant and scenario, plus three fixed effects with social distance both to the agent and to the victim as well as the participants' group of membership). Then, in an exploratory way, we ran the same model but taking into account this *reversed* factor as well as participants' sex (as previously found in other experiments as potentially relevant to explain some of the variability, see especially previous findings of Experiments 5 and 6). As a reminder, social distance to the victim was contrast-coded with (-0.5) for close/ingroup victim and (+0.5) for distant/outgroup victim.

Results

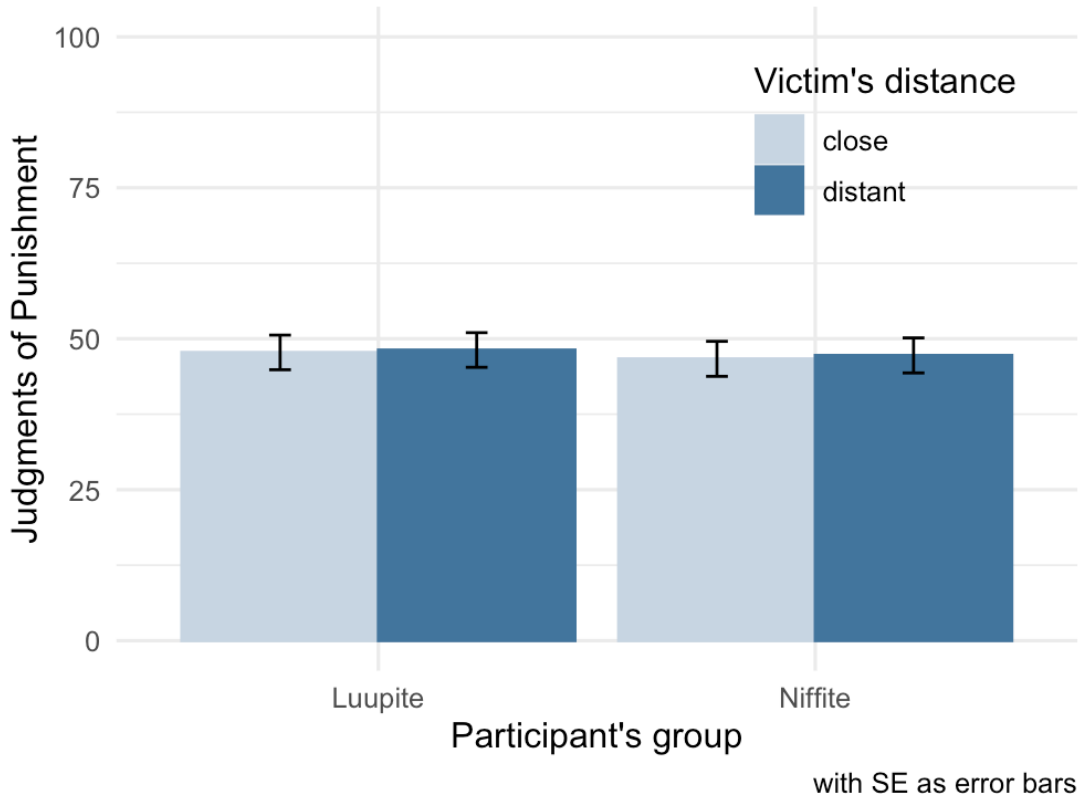
As a reminder, we expected in this experiment at the intergroup level that participants would judge the agent more harshly when the transgression had been committed against a (close) ingroup member compared to a (distant) outgroup member. We also predicted an interaction effect between social distance to the victim and social distance to the agent. The victim's social distance effect would be even stronger for an ingroup agent compared to situations involving an outgroup agent[91]. We will report the results concerning both hypotheses in the current chapter, as it tackles the effects of social distance to the victim (with potential moderation by the agent's social distance).

Confirmatory Results. Final confirmatory analyses (on $N = 152$) based on the reduced model as previously detailed in Chapter 5 revealed no main effect of social distance to the victim, $b = 0.49$, $SE = 0.52$, $t(8796.69) = 0.93$, $p = .35$, 95% CI [-0.54,1.51], $d = 0.014$. Participants did not judge significantly differently agents committing wrongdoing against a close (ingroup) victim ($M = 47.20$, $SD = 2.54$) compared to a distant (outgroup) victim ($M = 47.69$, $SD = 2.54$) (see Figure 6.13). No other main or interaction effects regarding social distance to the victim were detected within this analysis. In particular, the predicted interaction effect between both social distances (i.e., to the agent and to the victim) was non-significant, $b = 0.01$, $SE = 1.05$, $t(8794.02) = 0.01$, $p = .99$, [-2.04,2.07], $d = 0.0002$.

[91] Note that this prediction was mentioned in the preregistration in the other way around (i.e., regarding social distance to the agent effect's moderation by the victim), that is, predicting the social distance to the agent effect to be less strong when toward a close victim compared to a distant victim.

Figure 6.13

Confirmatory Results in Exp.3 (Part B) about Social Distance Effects Depending on the Victim's Identity and on Participants' Group of Membership



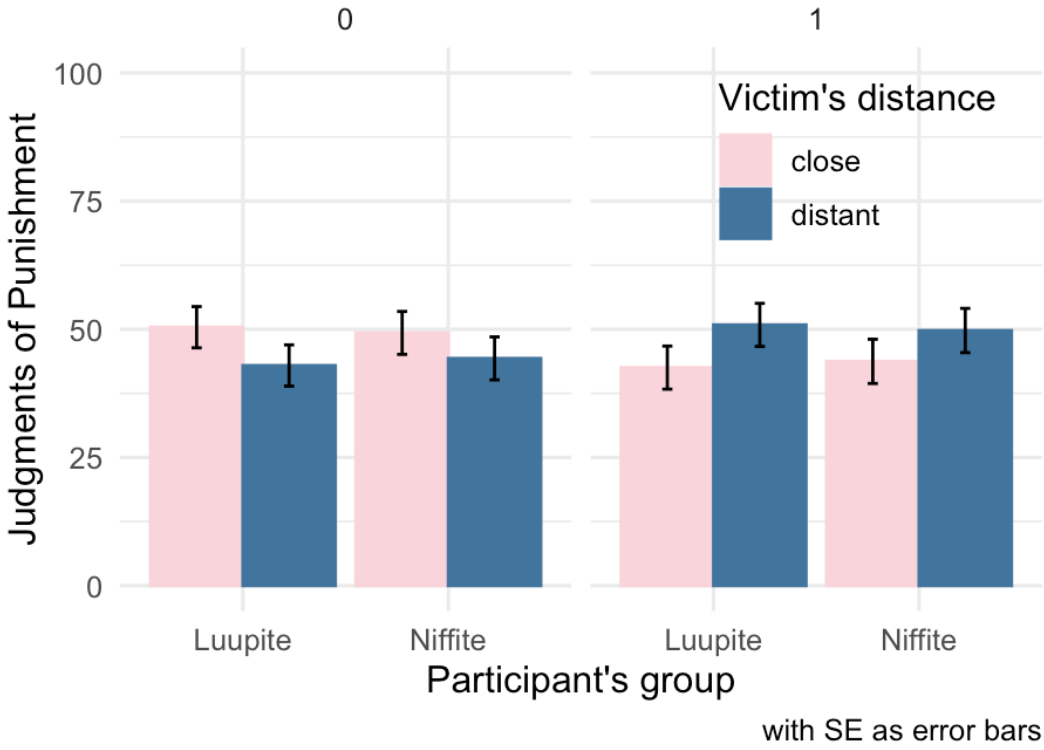
Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to harsher (vs. more lenient) judgments of punishment.

Exploratory Results. With the exploratory model controlling for the variance explained by both participants' sex and reversed conditions (i.e., to control for unwilling dependencies in design), we detected no significant main effect of social distance toward the victim (b = 0.48, SE = 0.56, t(8713.19) = 0.86, p = .39, 95% CI [-0.62,1.57], d = 0.014). However, this exploratory analysis revealed a significant three-way interaction effect between social distance to the victim and both controlled factors (i.e., participants' 'group of membership' and 'reversed' factor), b = -4.84, SE = 2.23, t(8713.19) = -2.17, p = .03, [-9.21,-0.46], d = 0.20. When decomposing this three-way interaction, we found that the simple effects of social distance to the victim within the various group-by-reversed conditions

were—although not significant—in the opposite directions. More particularly, in the first non-reversed condition (reversed = 0) participants roughly judged the agent more harshly for wrongdoings committed against an ingroup (close) other compared to when committed against an outgroup (distant) other (*b* = -7.48 for Luupite and *b* = -4.97 for Niffite participants). In the *reversed* condition however (reversed = 1), the same effect went in direction of more severe judgments of punishment toward the agent when the victim was a distant (outgroup) rather than a close (ingroup) other (*b* = 8.35 for Luupite and *b* = 6.02 for Niffite participants) (see Figure 6.14).

Figure 6.14

Exploratory Results in Exp.3 (Part B) about Social Distance Effects Depending on the Victim's Identity, Participants' Group of Membership, and Reversed Condition



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to harsher (vs. more lenient) judgments of punishment. Top numbers correspond to the condition of the *reversed* factor: (0) for participants going through the experiment under the first or third set of participants (e.g., 1st to 60th participants); (1) for those going through the reversed order of conditions (e.g., 61st to 120th participants) (see supplemental material for more).

When taking sex and reversed factors as part of the model, the previously predicted interaction effect between social distance to the agent and social distance to the victim was still not significant, *b* = 0.47, *SE* = 1.12, *t*(8166.37) = 0.42, *p* = .67, [-1.72,2.67], *d* = 0.007. However, as can be seen in Figure 6.15, we found that this interaction effect was moderated by the group-by-reversed conditions, *b* = -12.74, *SE* = 4.94, *t*(69.19) = -2.58, *p* = .01, [-22.59,-2.88], *d* = -0.05. More specifically, the interaction effect between both social distance targets (victim-by-agent) was found—even if non-significant—in the opposite direction depending on the group-by-reversed conditions. It was positive for the *Niffite* (as group of membership) and *non-reversed* conditions as well as for the *Luupite* and *reversed* conditions (*b* = 3.73, *p* = .11 and *b* = 3.58, *p* = .12 respectively), meaning a stronger effect of victim's social distance in case of a close agent (vs. distant agent). Whereas this two-way interaction was negative in sign for the other combinations (*b* = -3.32, *p* = .12 for *Luupite* and *non-reversed* and *b* = -2.10, *p* = .39 for *Niffite* and *reversed*). This means that the effect of social distance to the victim in the case of a distant agent was stronger than in the case of a close agent (see Figure 6.15). This result will be further discussed below.

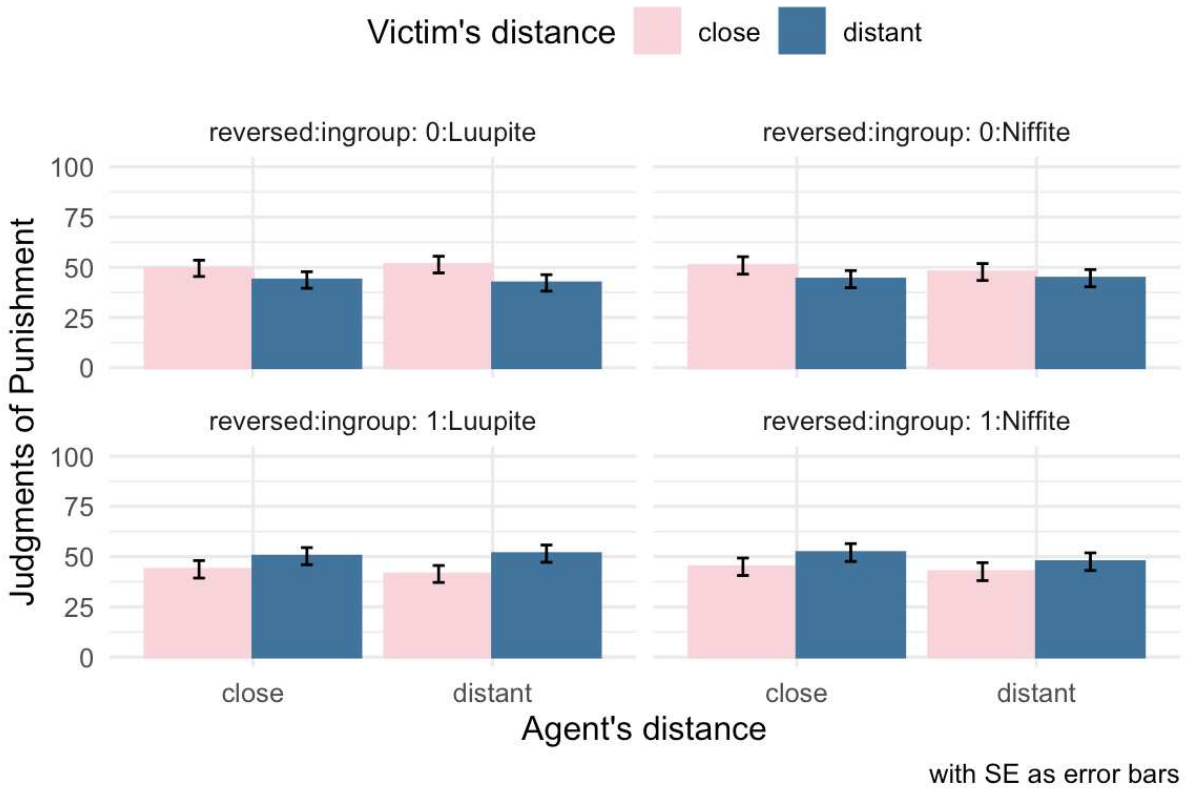
Finally, we found the highest-order interaction as significant, that is, with social distance to the agent [dst.perp], social distance to the victim [dst.vict], participants' group of membership [ingroup], *reversed* condition factor [reversed], and participants' sex [sex], *b* = -18.69, *SE* = 8.93, *t*(8706.33) = -2.09, *p* = .04, [-36.20,-1.19], *d* = -0.03. When decomposing this five-way interaction effect to visualize the lower-order interaction (without sex) for males and females separately, we found this result was mainly driven by males' answers. The four-way interaction effect was indeed significant for males (*b* = -22.08, *p* = .004) and not for females (*b* = -3.39, *p* = .55). More precisely, when decomposing depending on the reversed-by-sex condition, the three-way interaction between social distance to the agent, to the victim, and group of membership was significant for males who were in the non-reversed counterbalancing condition (*b* = 14.89, *p* = .003). When qualifying even more precisely this simple interaction effect, we found this to be especially true for males being in the non-reversed condition (*reversed* = 0) and who were assigned to the Luupite group, *b* = -8.40,

SE = 3.11, *t*(1374.52) = -2.70, *p* = .007, [-14.50,-2.30]. The sign of this effect being (consistently) negative, it means that the effect of social distance to the victim (i.e., judging more harshly the agent when against a close compared to a distant victim) was even stronger in the case of a distant agent (*b* = -14.65, *p* = .004) compared to a close agent (*b* = -6.25, *p* = .20) (see Figure 6.16, middle graph). No other (simple) interaction effect was significant during this decomposition of the maximal interaction.

Figure 6.15

Exploratory Results in Experiment 3 about Social Distance Effects Depending on the Agent's and Victim's Identity and on Participants' Group of Membership and Reversed Condition

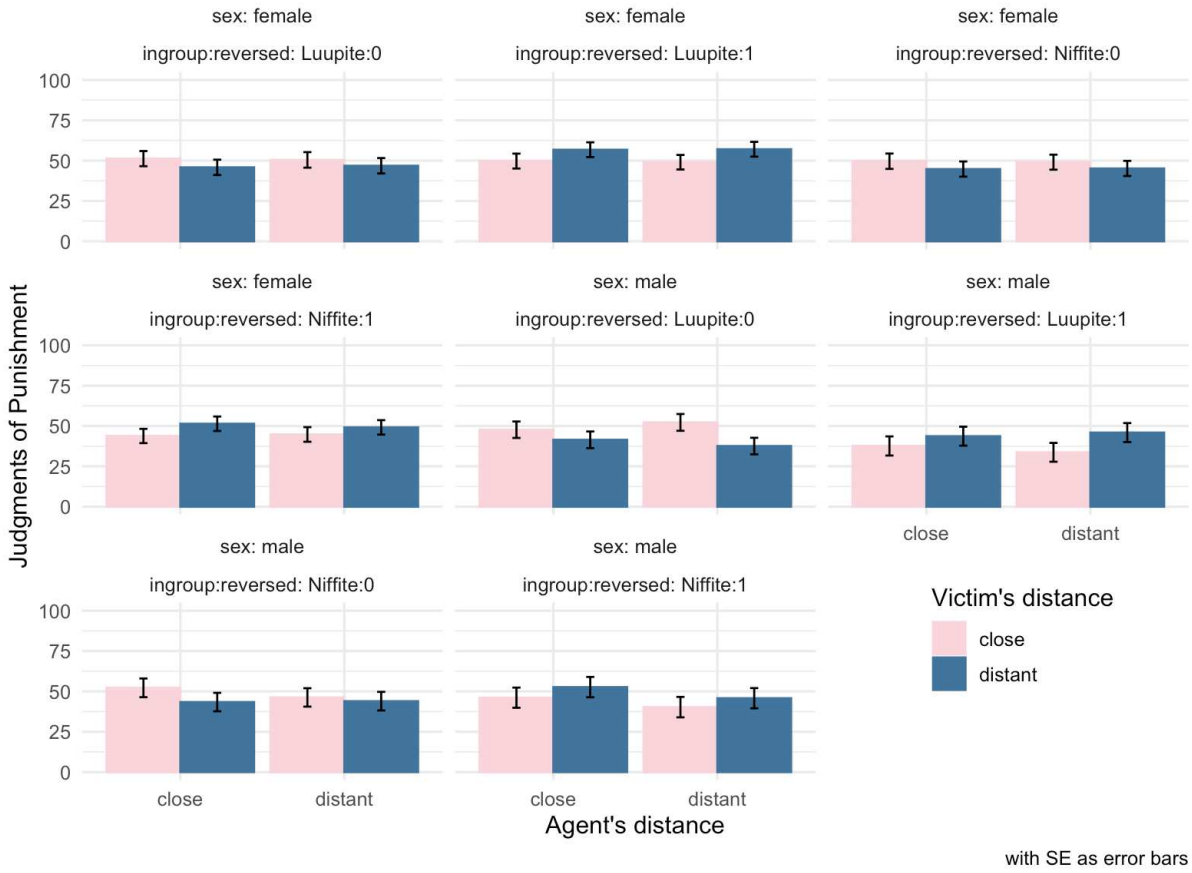
Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to harsher (vs. more



lenient) judgments of punishment. Top numbers correspond to the condition of the *reversed* factor: (0) for participants in the non-reversed condition, (1) for participants in the reversed condition.

Figure 6.16

Exploratory Results in Exp.3 about Social Distance Effects Depending on the Agent's and Victim's Identity, on Participants' Group and Sex, and on the Reversed Condition



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to harsher (vs. more lenient) judgments of punishment. Top numbers correspond to the condition of the *reversed* factor: (0) for participants in the non-reversed condition, (1) for participants in the reversed condition.

Discussion & Limitations

To sum up, when looking at the effects of social distance to the victim in Experiment 3, we did not find the predicted main and interaction effects through the confirmatory analysis. Yet, with an exploratory model controlling for dependencies in design ('reversed' factor) and participants' sex, we did observe a significant effect of social distance to the victim that varied in direction depending on the reversed-by-group of membership condition.

In the first (non-)reversed condition, participants overall judged the agent harsher when the victim was a close (vs. a distant) other, whereas in the second reversed condition, the agent was judged more harshly when committing a wrongdoing against a distant (vs. a close) other. The asymmetry of this effect is quite surprising and could be due to random contingencies in the manner that close and distant victim scenario versions were distributed among the (non)reversed conditions. This lets us think that the effect of social distance to the victim may be sensitive to some parameters inherent to the scenarios used, which would have been involuntarily distributed within the conditions of social distance to the victim in such a way that artificially amplified its effect in one direction or the other. It could also have happened only by chance. We will further explore this path in Chapter 7, by testing first the severity of the scenarios as an explicative moderator of social distance effects, then the responsibility as another possible explanatory factor.

In a similar way, the social distance to the agent-by-social distance to the victim interaction effect was descriptively (i.e., non-significantly) changing direction depending on ingroup-by-reversed conditions in the exploratory analysis. We do not know precisely to what this could be due (beyond the previously reported change of sign depending on reversed conditions), as neither the participants' group membership (i.e., Luupite or Niffite) nor the reversed factor were supposed to explain some variability in this experiment. Further exploration and replication would then be necessary to conclude about the direction of this interaction effect. We also found the maximal interaction of the exploratory model to be significant, indicating via decomposition that the interaction effect between social distance to the agent and to the victim was mainly driven by males, who were assigned to the Luupite group and to the first (non-)reversed condition. For these participants especially (even if the same pattern can be found—sometimes opposite in direction—in the other conditions), the effect of social distance to the victim (i.e., harsher judgments when a close victim was involved) was stronger when confronted to a distant rather than a close agent (see Figure 6.16, middle graph).

In front of these mixed and complex findings, even more so that they were found in exploratory analysis, and due to the current design dependencies, we cannot draw strong conclusions about these results. This would need further replication, correcting this dependency in design, as well as improving participants' identification with the various protagonists at play concerning their own membership. However, beyond these limitations, we must acknowledge that the effect size for the effect of social distance to the victim, depending on the group of membership and reversed conditions, seems quite consistent with previous literature (d = 0.20)[92]. We have yet to discover what may have impacted that much the direction of this effect in the scenarios displayed.

Experiment 4[93] (Part B)—Ingroup vs. Outgroup Member as Victim and Self as Agent

Rationale

As for the previous section, we analyzed in the following lines the non-exploited effects of Experiment 4 (see Chapter 5, pp. 169–170), that is, the effect of social distance toward the victim at the intergroup level. As a reminder, in this study, we implemented a multiplayer online game and induced a supposedly higher or lower degree of similarity between the various players (participants included). We manipulated this way the social distance toward the agent (i.e., either the participant him/herself or another close—ingroup—or distant—outgroup—as the agent); the social distance toward the victim (i.e., either an ingroup or outgroup member as victim), as well as the outcome of the card choice (i.e., harmful vs. neutral).

Regarding the effect of social distance toward the victim at core interest for the current chapter, we hypothesized that people would give harsher moral judgments when a close (ingroup) other is involved as the victim compared to a distant (outgroup) other, and that this would be particularly true in case of harmful outcome (and not or to a lesser extent in neutral outcome cases). Also, we exploratorily planned to check for a potential interaction

[92] This corresponds actually to the estimated effect size for the three-way interaction between social distance to the victim, reversed and group of membership conditions, hence a simple effect of social distance that may be even larger in effect size.

[93] This study was conducted in collaboration with Pr. Liane Young's Morality Lab (Boston College, USA) and thanks to the precious help of Thomas Mazzuchi to implement the online task.

effect between the agent's and victim's social distance, particularly in harmful outcome cases. That is, expecting the effect of social distance toward the victim (as reported above) to be even stronger in cases where the agent was a distant other (outgroup) or the self, compared to cases where it is a close one (ingroup). This hypothesis stems from both Exp.3 findings (part B, see above) for distant vs. close victim comparison and from the previous pattern observed for social distance to the agent effect in Exp.3-part A (see Chapter 5, pp. 155–158).

Method

To test these predictions, we implemented an online multiplayer cooperative game where people assumed they had to find out what the relationship was between the card choice and the following outcome (see Figure 5.10 for a reminder of the whole procedure). Participants (N = 69, 37 females, 31 males, 1 unreported, M_{age} = 19.52, SD_{age} = 1.97) were then asked to judge after each round how right or wrong was the agent's behavior depending on their level of similarity with them, depending on the outcome, and the victim's identity (ingroup vs. outgroup) (see Chapter 5, pp. 147–155 for more details about sample size, participants, materials, procedure, design, and analytical details; see also the OSF page of the project).

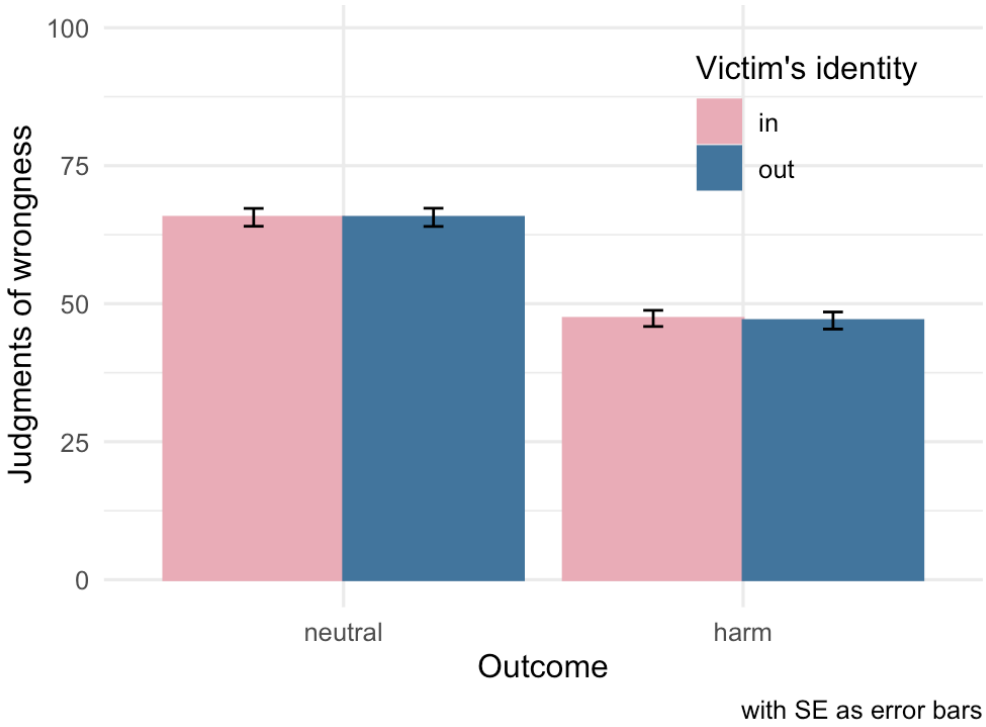
Results

In this experiment, we found no significant main or interaction effect of social distance toward the victim. More particularly, participants did not judge significantly differently situations where an ingroup member was targeted as the potential recipient of the penalty point (M = 56.50, SE = 1.12) compared to situations where it was an outgroup member (M = 56.30, SE = 1.12), b = -0.19, SE = 0.52, t(81.11) = -0.38, p = .71, 95% CI [-1.22,0.83], d = -0.01. This was even not significant in the harmful cases compared to neutral ones as we would have expected, with the interaction effect between social distance to the victim and outcome being non-significant (b = -0.38, SE = 1.02, t(78.17) = -0.38, p = .71, [-2.41,-1.64], d = -0.01). Model parameter estimations suggest a slightly (non-significantly)

stronger effect of social distance toward the victim in harmful cases (b = -0.39, p = .64) than in neutral ones (b = -0.001, p = 1.00) (see Figure 6.17).

Figure 6.17

Results in Exp.4 (Part B) about Social Distance to the Victim Effects Depending on Outcome



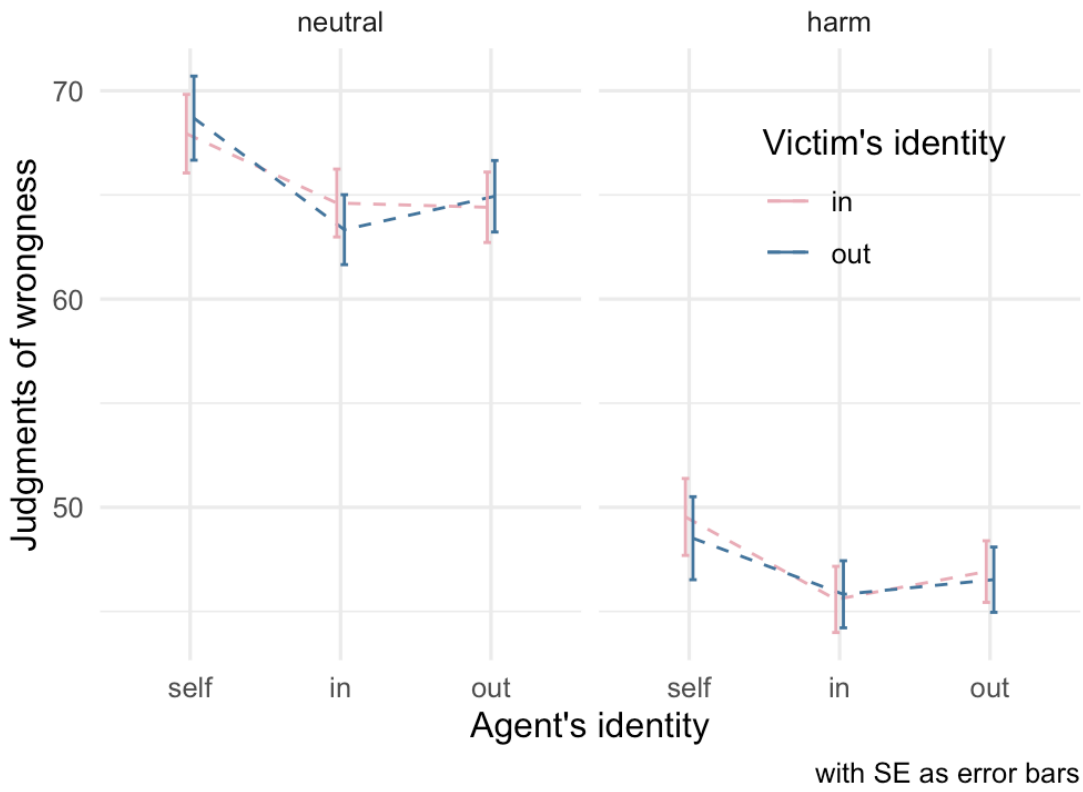
Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more morally right (vs. wrong) judgments. Victim identities are either an ingroup member ('in' condition in pink) or an outgroup member ('out' condition in blue).

Regarding the exploratory hypothesis of a possible interaction effect between social distance to the victim and social distance to the agent, we did not find evidence that would support this exploratory path, with interaction effects being non-significant (b = -0.09, SE = 0.91, t(2885.20) = -0.09, p = .93, d = -0.002 for comparison between self and others as agents and b = 0.59, SE = 1.05, t(2885.20) = 0.56, p = .58, d = 0.02 when considering an ingroup vs. outgroup agent). The agent's identity (self vs. ingroup vs. outgroup) did not significantly moderate the impact of social distance to the victim (either ingroup or outgroup member). This interaction effect was not even more true within harmful outcome cases only, as both three-way interaction effects between social distance to the agent, outcome, and

social distance to the victim were not significant either (*b* = 2.07, *SE* = 2.25, *t*(84.26) = 0.92, *p* = .36, *d* = 0.03 with first contrast of agent's distance, and *b* = -2.44, *SE* = 2.09, *t*(2885.20) = -1.17, *p* = .24, *d* = -0.03 for second contrast) (see Figure 6.18). No other main or interaction effect related to social distance to the victim was found.

Figure 6.18

Results in Exp.4 about Social Distance to the Victim and the Agent Effects Depending on Outcome



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more morally right (vs. wrong) judgments. Victim identities are either an ingroup member ('in' condition in pink) or an outgroup member ('out' condition in blue). Agent identities are either the 'self', or an ingroup member ('in'), or an outgroup member ('out'). Note that the ordinate axis scale was zoomed here (original range from 0 to 100) to better visualize the pattern of effects.

Discussion & Limitations

In this fourth experiment and focusing on the part related to the effects of social distance to the victim (part B), we found no main or interaction effect of social distance to the victim. Participants judged the situations involving a close one (ingroup) as the victim differently than situations involving a more distant other (outgroup) as the victim, yet not significantly. Therefore, the findings did not support our initial hypotheses. Moreover, the preregistered interaction effect hypothesized between social distance to the victim and social distance to the agent was not significant either, despite a descriptive pattern (when zooming in on the scale as in Figure 6.18 above) suggesting some slight differences in some conditions. It was not even more true for harmful than for neutral outcome situations as expected, as the largest effect of social distance to the victim seems descriptively to be in the neutral outcome and ingroup agent condition (see Figure 6.18).

As discussed earlier in Chapter 5 about this experiment, we think the current lack of support for our hypotheses may be mainly due to the current manipulation of social distance and to the implemented online game which was probably too complex in terms of rules (i.e., not sufficiently clear from participants' point of view). We could therefore consider replicating this study design while correcting these drawbacks in order to get a stronger source of evidence supporting or not our hypotheses (e.g., via the use of a simpler online game, for instance, a cyberball-like paradigm, though less prototypical of a moral task).

General Discussion & Conclusion

In the current chapter, we investigated throughout two novel preregistered experiments (5 & 6), in addition to Experiments 3 and 4 (parts B), how social distance toward the victim could influence everyday moral decisions. We started at the interindividual level via two novel experiments reusing the EMCS material from Singer et al. (2019), manipulating social distance to the recipient of either an altruistic or an egoistic act. In Experiment 5, aiming at replicating and extending Singer et al.'s previous findings (2019), we found as expected that people tend to act more altruistically toward close others than

toward distant others. In the other way around (regarding dark morality that particularly interests us in the present dissertation), people made slightly more frequent egoistic (and less altruistic) choices when the recipient was a distant other (compared to a close one), hence this finding being consistent with the main tendency in the literature. On moral judgments however, we found quite inconsistent findings with egoistic acts being judged as more immoral when committed against a distant other compared to a close one, and altruistic scenarios being judged more moral when performed toward a distant other (vs. a close one). In Experiment 6, the egoistic versions of these moral scenarios (also adapted to manipulate the severity of the situation) were judged overall as more morally wrong when perpetrated against a close one compared to a distant other, hence in direct contradiction with what had been previously found in the second part of Experiment 5 (i.e., on moral judgments), though more consistent with the first part and with findings of the literature.

Regarding the exploratory findings about the effect of gender and its impact on social distance to the victim influence, we found males to be more sensitive to this factor for altruistic behaviors in Experiment 5 (part A), but then not anymore on moral judgments (part B) where females were more sensitive than males to this social distance bias. Experiment 6 did not resolve this tension as we found no evidence of a social distance-by-gender interaction effect on moral judgments, although males judged overall their own egoistic acts (no matter against a close or distant other) as less immoral than females did. Even if not the first focus of this dissertation thesis, gender seems to be an important potential moderator of social distance effects and should therefore be further explored as such. We also found earlier in the scoping review preliminary results (see p. 102) that this factor has been tackled quite a few times already in the literature, without yet demonstrating a robust moderation tendency with regard to social distance influence. We will come back to the possible interpretative paths for the potential differentiated effect of social distance among males and females in the general discussion section (see Chapter 9, p. 339).

At the intergroup level in Experiment 3 (part B), we found in confirmatory analyses no evidence supporting an effect of social distance to the victim, as people were not

significantly harsher in their judgments when the act was perpetrated against a close (ingroup) or a distant (outgroup) other. However, when reanalyzing in an exploratory vein taking into account a factor to control for involuntary dependencies in design, we detected simple effects of social distance to the victim, which were moreover opposite in direction depending on conditions. For participants seeing a certain combination of the various conditions within the scenarios' distribution, situations involving a close victim were judged more harshly than those involving a distant victim (consistently with Tendency 1 of the state of the art), whereas for others it was the opposite tendency with harsher judgments for scenarios involving distant victims. We will further explore in the following chapter (Chapter 7) what could have triggered these differentiated effects of social distance to the victim (and/or whether it only happened by chance, even if hard to believe in front of such a strong reversed effect). In Experiment 4 (part B), however, we again did not find any significant difference between close or distant recipients of either harmful or neutral choices within the online multiplayer game (supposedly triggering more ecological reactions than the hypothetical scenarios proposed until then).

As these two experiments at intergroup level manipulated social distance both to the agent and to the recipient/victim of the situation at play (either within hypothetical moral scenarios in Exp.3 or within an online game play in Exp.4), it also allowed to test for the interaction between both effects of social distance. Although not significant in confirmatory analyses of Exp.3, exploratory analyses revealed this interaction effect to be significant only in very specific cases (only for males in the reversed condition and affiliated with the 'Luupite' group of membership). For these participants specifically, the effect of social distance to the victim (close victims being judged more severely than distant victims) was stronger when the agent was a distant other compared to when it was a close one. In Experiment 4, we again did not replicate such an interaction effect, with social distance to the victim not being moderated by social distance to the agent. These ambiguous and exploratory findings again do not allow us to draw strong conclusions regarding this potential

interactive effect. It might still be worth digging into it more deeply, as it seems very rarely done in a systematic way in the literature (e.g., McKee et al., 2023 for a recent exception).

Such as for social distance to the agent, we found ambiguous findings for social distance effects when manipulated toward the victim, be it at the interindividual level (Exp.5 & 6) or at the intergroup level (Exp.3 & 4, parts B). Despite more convergence in the literature about this effect direction and its robustness (in accordance with Tendency 1, judging the agent harsher when the victim is a close one compared to a distant one), some of the current findings suggest it may not always be the case (as in Exp.5, part B and in Exp.3, part B for mixed findings). What is also surprising in the current chapter is to observe effects that are opposite in direction while keeping mostly the same material as for Exp.5 and 6 findings with adapted EMCS version. This may be due to the fact we additionally manipulated the severity of the scenarios in Exp.6 compared to Exp.5 (despite our efforts to keep the same scenarios' structure and believability, and although no interaction effect with severity was detected in the end). But social distance to the victim effects were even still not consistent within the same study (e.g., Exp.5, part A vs. part B), with only the mode of response changing (i.e., behavioral choices vs. moral judgments respectively).

Similarly, the fact that we detected no effect of social distance to the victim in Exp.4 (part B), whereas it was to some extent significant as a simple effect (per reversed-by-ingroup condition) in Exp.3 (part B), makes these findings even more complicated to disentangle. Although the reason for no detection in Exp.4 could come from the various limitations we already pointed out earlier in terms of paradigm, we may think the discrepancy in effects' directions in Exp.3 could stem from an underlying confounding variable, inherent to the scenarios and/or to the task. We will further investigate this path in the next chapter by testing some underexplored moderators of social distance effects (both to the victim and/or to the agent).

Chapter 7

Social Distance Moderators in Everyday Moral Decision-Making

In the two previous chapters, we have tested empirically how social distance to the agent and to the victim (respectively) could influence everyday moral judgments or decisions. We noticed that in everyday moral settings and taking both roles (agent and victim) into account, social distance effects are not as clear and robust to evidence than previously expected. This may stem from the fact that moral situations are often complex and do not only vary in social distance to the protagonists. The current chapter is dedicated to the analysis of potentially relevant moderators of social distance effects.

As a starting point, we consider below whether social distance effects may interact with some specific factors robustly involved in morality (e.g., Cushman, 2008; Young & Tsoi, 2013), that is, the agent's intent and causal involvement (hereafter 'causality') in the perpetrated act (Experiment 1). In Experiments 2 and 5, we examine—as suggested by some authors in the literature (e.g., Tassy, Deruelle, et al., 2013; Tassy, Oullier, et al., 2013)—the relevance of the type of moral judgment or decision at stake to explain social distance effect discrepancies. Moreover, as one major hypothesis drawn from the previous chapters concerned the severity of the consequences, we also reanalyzed Experiments' 1 to 3 findings with regard to this potential moderator and tested its influence in a confirmatory vein in Experiment 6. Finally, beyond the mere severity of the consequences and/or intent and causality, we considered the level of responsibility of the agent in the wrongdoing—understood as what cannot be attributed to external circumstances. We investigated whether this could play a role in how social distance impacts perceptions of the situation (beyond the mere agent's intent). We therefore exploratorily reanalyzed previous findings of Experiments 1 to 3 through an a priori naive classification of the scenarios in terms of the agents' level of responsibility. In sum, this chapter will focus on four different types of moderators, corresponding to the four sections that follow, as synthesized in Table 7.1.

Table 7.1

Overview of the Moderators Investigated in the Current Dissertation

Moderators	Study ID	Factors involved	Type of response	Social distance to	Material
1) Intent & Causality	Exp.1	Social distance x Intent x Causality	Punishment judgments	Agent	Leloup et al. (2018)
2) Type of decision or judgment	Exp.2	Social distance x Type of judgment	Wrongness & Blame judgments	Agent	Leloup et al. (2018)
	Exp.5	Social distance x Gender	Part A. Choices of action (alt. / ego.) Part B. Wrongness judgments	Victim	Singer et al. (2019)
3) Severity & 4) Respon-sibility	Exp.1	Social distance x Intent x Causality	Punishment judgments	Agent	Leloup et al. (2018)
	Exp.2	Social distance x Type of judgment	Wrongness & Blame judgments	Agent	Leloup et al. (2018)
	Exp.3	Social distance (MGP like)	Punishment judgments	Agent & Victim	Leloup et al. (2018)

Note. Exp. = Experiment, MGP = minimal group paradigm, alt. = altruistic, ego. = egoistic

Social Distance and Key Factors in Moral Research

Rationale

In a first instance, we aimed at testing whether social distance effects could be moderated by traditionally key factors in moral research. That is to say, those that have been already largely studied in the moral psychology literature and robustly found to influence morality throughout various paradigms and operationalizations. We specifically refer here to the agent's intent, understood as the desire and belief that the act will have the expected

harmful consequence (see Cushman, 2008). But also, to the consequence or outcome of the act itself (i.e., whether it induces no harm or on the contrary a harmful outcome for one protagonist), as well as the agent's causality in the perpetrated harm (is the agent's action the direct cause of this harm, or does it come from an independent source?).

Although very few sources directly tackled the potential moderation of social distance influence by these factors, we thought it would be relevant as a first explorative strategy to start from well-known and robust effects and test whether (or not) social distance would alter their generally strong influence. At intraindividual level, Hirschfeld-Kroen et al. (2021) compared the inference of intent and the perception of harm within two social distance perspectives (i.e., either first-person perspective—self as agent—or third-person perspective—self as mere observer). They predicted that the inference of intent would be lower and harm perception higher in first-person cases compared to third-person situations[94], thus justifying overall harsher moral judgments for oneself compared to another agent. Here is then underlying an expected double interaction effect with social distance (with intent and with outcome perception), even if not tested or mentioned this way by the authors. Extending directly from this research at the other levels of analysis, we would predict intent importance to be lower in case of self or a close one as agents compared to another (distant) one, whereas the outcome impact would be higher in case of self/close other as agent compared to a distant other as agent.

Other research also investigated the outcome effects' differentiation (or moral luck effect) depending on the role played in the situation—the perpetrator, the victim, or a bystander (Frisch et al., 2021). They however provided a theoretical rationale and predictions that are opposite in direction to what was expected by Hirschfeld-Kroen et al.'s study regarding harm perception (2021). In line with the introspection illusion (Pronin,

[94] A lower inference of intent would mean that one considers this parameter to a lower extent and a higher perception of harm would mean one would consider this parameter to a larger extent (more easily perceiving the situation as harmful). The authors justify these predictions by the fact that (a) people would be less likely to consider their own innocence compared to another one, and (b) people would be more likely to perceive harm when they directly caused it (Hirschfeld-Kroen et al., 2021).

2009)[95], they predicted the outcome effect to be stronger in a second-party perspective (being the victim of the transgression), then in the third-person perspective (as a bystander) compared to a first-person perspective (oneself as the agent). Beyond the mere outcome effect, the authors did nonetheless not find any interaction effect between outcome and social role within the situation on moral judgments (of permissibility, wrongness and blame) or on punishment judgments (see Frisch et al., 2021).

These opposite predictions illustrate again the lack of consistency in literature regarding social distance effects, even when conceptualized as a moderator of a well-known and robust effect of influence for moral situations (i.e., the moral luck or outcome effect). They more particularly reflect both tendencies of reasoning as illustrated in the theoretical part of this dissertation (see Chapter 2), either relying on Tendency 1 (see Frisch et al., 2021) or on Tendency 2 (see Hirschfeld-Kroen et al., 2021). As Experiment 1's findings regarding social distance effects per se were already reported earlier in Chapter 5 (see pp. 125–128), we only report below what relates to the interaction of social distance (close vs. distant agent) with the agent's intent and causality in the outcome. Both two-way interaction effects as well as the three-way interaction effect between social distance, intent, and causality were presented as additional exploratory and non-directional hypotheses in the preregistration form of this study (see https://osf.io/feup6/).

Experiment 1's Findings on Intent and Causality as Moderators of Social Distance Effects

Method

As a short reminder (for the complete method description of Exp.1, see Chapter 5, pp. 121–125), we manipulated in this experiment social distance to the agent (either close or distant from the participant's perspective), as well as the agent's intent (accidental vs. intentional act) and the agent's causal role in the outcome (either directly caused or not by

[95] Rationale behind the introspection illusion suggest that being in a second-party or third-person role (i.e., as for victims and bystanders) makes us judging the agent from an external perspective, then focusing on the outcome more than on the agent's own mental states whereas the opposite reasoning would remain for first-person perspective (see Pronin, 2009).

the agent) within various daily-life moral situations. Participants (N = 76) had to judge in each scenario (adapted from Leloup et al., 2018) how much they would punish the agent for the perpetrated act depending on the various intent(x2)-by-causality(x2)-by-social distance(x2) conditions. This design then allowed to test within the same mixed model analysis comprising two random variables the interaction effects hypothesized in an exploratory vein.

Results

First regarding the interaction effect between social distance and the agent's intent, we found it as non-significant, (in step 2: b = -0.07, SE = 0.07, t(3840.44) = -1.00, p = .32, 95% CI [-0.21, 0.07], d = -0.02) that is, the main effect of intent (with harsher punishment judgments for intentional than accidental acts) not significantly differing between close vs. distant agents. Similarly, no moderation of the causality effect was detected depending on the conditions of social distance (in step 2: b = 0.03, SE = 0.07, t(3823.15) = 0.39, p = .69, [-0.11, 0.17], d = 0.009). Furthermore, the three-way interaction effect between these factors was also not significant (in step 2, b = 0.12, SE = 0.14, t(3828.72) = 0.83, p = .41, [-0.16, 0.40], d = 0.02) (see Figure 5.3 of Chapter 5, p. 126).

However, when looking descriptively at the effects of social distance throughout the diverse scenarios of Leloup et al.'s battery (2018) depending on the various conditions of intent and causality, we noticed very diverse effects both in size and direction (see Figure 7.1). More particularly, some scenarios seem to trigger positive effects of social distance (i.e., in red lines, with close ones being judged less harshly than strangers) when their outcome was caused directly by the agent, whereas the reverse pattern is observed (in blue lines) when the outcome was not directly caused by them. But this is not systematically the case for all scenarios, as some situations rather trigger positive effects of social distance for not caused outcome or dependant on the intent factor, whereas others simply reveal no differences between judgments of close vs. distant agents, but only for some conditions of intent and causality (Figure 7.1). The fact that no systematic pattern appeared in terms of social distance effects' size or direction depending on intent-by-causality conditions is quite

consistent with the absence of any significant interaction effect in the mixed model analysis. It does not mean however there is no other hidden factor or explanation that would drive these differentiated effects, as observed in Figure 7.1.

Figure 7.1

Social Distance Effects in Exp.1 Depending on Scenarios and on Intent-by-Causality Conditions

Note. On horizontal axis, judgments of punishment are displayed from more lenient judgments (0) to more severe judgments (6). Here the abscissa axes have been zoomed when relevant to better visualize the discrepancies between social distance effects.

Discussion & Limitations

As a conclusion, we found in this first experiment no significant interaction effect between social distance and intent or causality factors respectively, as well as no three-way

interaction effect between them. Participants' judgments of punishment were indeed affected by the intent of the agent perpetrating the action, as well as by the causality component (i.e., whether the agent directly caused the outcome or not), but there was no difference of these influences across social distance conditions (socially close vs. distant agent).

As the manipulation of social distance in this study contains several limitations already reported earlier in Chapter 5, we may have wanted to replicate this finding with a proper manipulation of social distance (without the confound on the victim's status, which remains unclear in the current operationalization). However, as we also did not identify the mere effect of social distance in the current study, the first aim was to improve the experimental paradigm in order to detect this main effect, before wanting to draw conclusions regarding its potential interactive power with the other factors. Therefore, we did not pursue further our investigations regarding these interaction effects. However, we enthusiastically encourage other research teams to investigate this question through a well-replicating paradigm of social distance, especially at the intraindividual level of analysis (to directly clarify or reconcile Hirschfeld-Kroen et al.'s, 2021 and Frisch et al.'s, 2021 findings), and/or at the interindividual and intergroup levels as not previously explored to our knowledge.

The current null results also stand for a manipulation of social distance toward the agent of the action uniquely. Consequently, it could be relevant to also test these interaction effects when social distance is manipulated toward the victim. We may indeed assume that when the victim is closer to the self, the agent's causality in the act (whatever his or her social distance with the observer) would take a greater weight in the final decision of moral judgment, in line with what predicted Frisch et al. (2021). Again, we invite future research to investigate this issue more deeply, as it clearly remains an under-explored research topic, with to a certain extent non-consistent findings as detailed above.

On a final note regarding this question, we could also think the role taken in the situation could only move judgments either in the slightly harsher or slightly more lenient direction, without especially interacting (that is, influencing the difference between the two

conditions). As an illustration, with the inference of the agent's mental states or intent (to get an idea of how this factor weights in a decision of punishment for instance), we could think the victim of the act would judge the agent a bit harsher than a mere observer in case of vile intent as well as in case of an accidental wrongdoing (because of his/her status of victim in the situation). In a similar way, the judgments provided by the agent him/herself could be more lenient (as seen from a first-person perspective, in line with Frisch et al.'s predictions, 2021) both for the vile intent and innocent intent cases. If this type of pattern would be observed, we would have no interaction effect between intent weigh and people's role in the situation, but we would detect at least a main effect of the role taken in the situation (pending sufficient differences between conditions). These explorative paths further argue in favor of a more dynamic way to study social distance influence in relation to more classic factors of influence in morality (e.g., intent, causality). This is in fact what has been done in the current experiment. We will analyze in Chapter 8 whether there was an influence of social distance beyond final judgments, that is, over the course of time during the decision process.

Social Distance and Type of Moral Judgment or Decision

Rationale and Overview

Another important potential moderator of social distance effects, as suggested earlier in the scoping review, is the type of judgment at stake. Bahník et al. (2021) for instance showed how the self-serving bias traditionally found in sacrificial dilemmas (Tend.1) is in fact reversed—people being more likely to show an other-serving tendency (Tend.2)—when they are asked about the wrongness of the act (i.e., in a normative and negatively framed judgment) compared to a positively-framed or prescriptive judgment (i.e., whether it should be done or not). This could only seem of minor importance for the question that interests us, as research dealing with wording or framing biases are often not dealing in any way with social distance. However, if systematically found in the reversed direction for such normative and negatively framed judgments, this could help understand what the tenants of social distance influence are.

In moral psychology literature, plural authors have suggested that the type of moral judgment at stake could impact how people perceive the situation. In this sense, Malle (2021) distinguishes four types of moral judgments, classified according to their cognitive processing complexity (from lower to higher): evaluations, norm judgments, wrongness judgments, and blame judgments. Cushman's two-process model (2008) focuses for its part on information processing within four types of moral judgments: wrongness, permissibility, punishment, and blame judgments. Although sometimes overlapping (and not exhaustive of what can be found in literature), these different types of judgments have not yet been systematically tested regarding social distance effects. On the preliminary findings of the scoping review (see Chapter 3, pp. 94–96), we reported that social distance directions of effect were not all represented in the same proportions across the diverse types of moral judgments.

Moreover, in literature, the gaps in findings regarding social distance effects sometimes also come from the type of decision making itself: Do people in the task have to provide an evaluation of the situation within one of the above-mentioned moral judgment or do they have to provide a choice of action or decision with potentially more concrete implications? Tassy and collaborators (2013; 2013) tackled this question via testing the probability of utilitarian responses within trolley-like moral dilemmas depending on the type of decision (i.e., judgment vs. choice) and the social distance to the victim. Beyond the main effects of type of decision and of social distance to the victim—with less utilitarian responses for judgments than for choices and more utilitarian responses when a distant (vs. close) victim is involved respectively—, they found an interaction effect between both. This differentiation went in direction of an even stronger effect of social distance to the victim (as stated above) in case of behavioral choices compared to mere judgments of the acceptability of the act (Tassy, Deruelle, et al., 2013; Tassy, Oullier, et al., 2013).

In the same vein, Gold et al. (2015) compared judgments and choices of action within the same type of moral situations but depending on social distance to the agent (and not anymore toward the victim as done by Tassy and collaborators). That is, what difference

does it make to be in the shoes of the agent of the moral dilemma compared to those of an observer? The authors found a discrepancy between observers' and agents' judgments on ratings of the rightness of the act (i.e., main effect of social distance between self and another one as the agent) but not in the percentage of those who chose to act or said the agent should act (i.e., behavioral choice) (Gold et al., 2015). This difference regarding both effects of social distance was not directly tested by the authors but could again suggest a discrepancy in social distance effects depending on the decision-making task (judgment vs. choice of action). Even though this finding suggests the opposite tendency compared to Tassy et al.'s findings (2013; 2013) with a stronger impact of social distance to the victim for judgments of rightness than for behavioral choices.

Given these controversies, we first report below how we systematically tested in Experiment 2 the potential differentiated impact of judgment types for social distance to the agent effects (i.e., judging the wrongness of the act, then the punishment the agent would deserve). We will then in a second instance confront in an exploratory way findings of Experiment 5 regarding social distance effects on behavioral choices (part A) and on moral judgments (part B).

Experiment 2's Findings on Type of Judgment as a Moderator of Social Distance Effects

Method

As a reminder, in this second study, we asked participants to judge various moral situations on two different moral judgment dimensions, while still manipulating social distance to the agent (either oneself, or a close one, or a distant other as the agent). We asked participants to judge for each scenario both the wrongness of the transgression and then how much punishment the agent would deserve for the wrongdoing. Type of judgment (wrongness vs. punishment) was thus manipulated in a within-participant and within-scenario design, while maintaining constant order of judgments' presentation. We then compared whether participants gave different ratings across both types of moral judgments, and if social distance effect was significantly differing in one or the other condition. In an

exploratory vein, we predicted—beyond the main effect of the type of judgment based on previous research, with wrongness ratings slightly harsher than punishment ratings (Leloup et al., 2018)—that the effect of social distance would be stronger for punishment judgments than for wrongness judgments[96]. This stems from the idea that punishment may better reflect almost a moral action than a moral judgment per se (Malle, 2021), hence its supposedly higher sensitivity to social distance variations as in Tassy et al.'s studies (2013; 2013), even if not toward the same target (agent vs. victim).

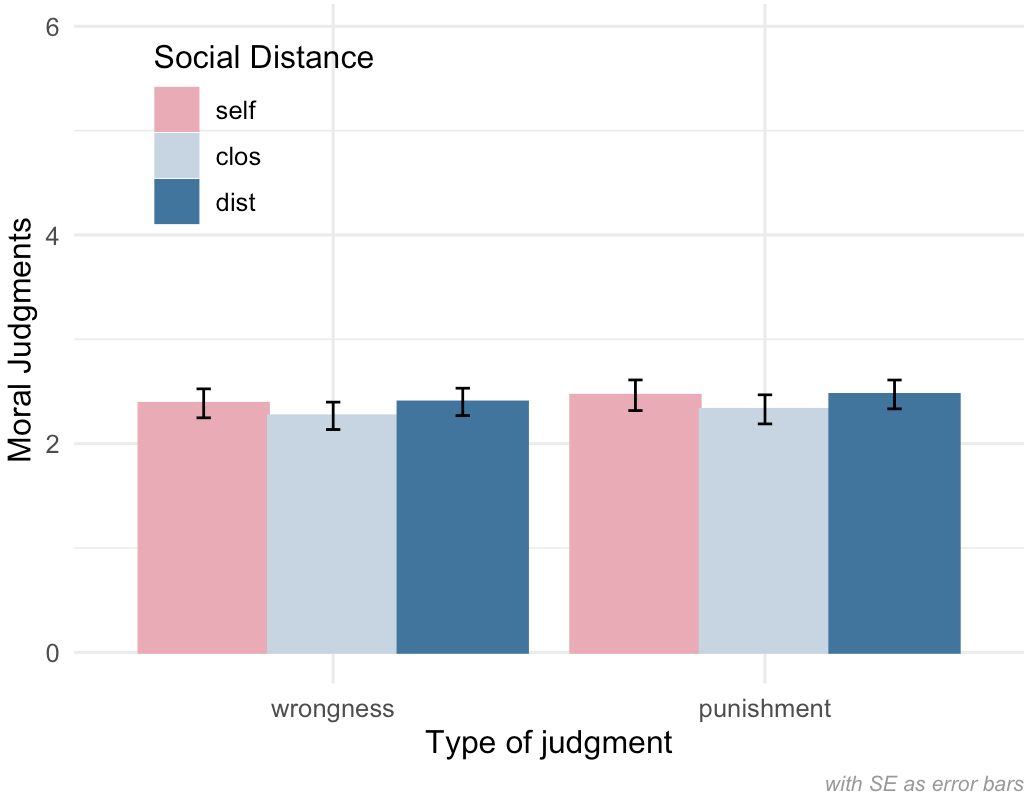
Results

As a result regarding these predictions, we did not find a significant effect of the type of judgment at stake, b = 0.07, SE = 0.08, $t(195)$ = 0.88, p = .38, 95% CI [-0.09,0.23], d = 0.04, with no significant difference between judgments of wrongness (M = 2.35, SE = 0.13) and judgments of punishment (M = 2.42, SE = 0.14). We can however descriptively notice that the difference—even not significant—between both types of judgments goes in the opposite direction compared to what had been hypothesized, that is, slightly more severe judgments of punishment compared to judgments of wrongness. As for differentiated impact of social distance as a function of type of judgment, no interaction effect between social distance (on both contrasts, Dst1 or Dst2) and type of judgment was detected ($-0.01 < b_s < 0.002$; $-0.34 < t_s < 0.05$; $.73 < p_s < .96$). In other words, participants judged themselves and distant agents as harshly for wrongness and punishment judgments (see Figure 7.2 for a graphic overview).

[96] Note that we preregistered this interaction effect between social distance and type of judgment on the second contrast of social distance only (opposing oneself vs. a close other as the agent). However, as we do not have strong evidence that would support the expectation of this interaction to be true only for one contrast of social distance (self vs. close one) and not the other (self+close vs. distant one) and as the current analyses are exploratory, we looked at both interaction effects.

Figure 7.2

Social Distance Effects in Exp.2 Depending on the Type of Moral Judgments



Note. Moral judgments are measured on a 0-6 range continuous scale from (0) Not severe at all to (6) Very severe judgments.

Discussion & Limitations

Contrary to expectations, we found in Exp.2 that social distance effect was not moderated by the type of judgment at stake. That is, participants did not judge themselves or socially close vs. distant agents differently depending on the judgment at stake (i.e., either a wrongness or a punishment judgment). The main limitation to notice in this study design is the fact people answered both judgments in a successive way (i.e., wrongness, then punishment) for each novel situation (within-participant and within-scenario distribution). They might therefore have judged the situation as morally wrong as they considered how much punishment the agent would deserve for his/her wrongdoing. Or more precisely, they may have inferred directly their estimation of punishment from their previous judgments of

wrongness. This is the risk taken when displaying multiple moral judgment questions within the same study in a within-participant design, as noticed by Malle (2021).

To overcome these drawbacks, we might have preferred to counterbalance the order of judgment to give (for some participants, wrongness then punishment; for others, punishment then wrongness) and/or only ask one type of judgment per scenario still in a within-participant and within-scenario design. However, even so, we would probably still have observed a high correlation between both judgments without being able to know whether people really distinguished the two different types of judgments. A better alternative would have been to counterbalance in a between-participant design the two types of judgments, such that participants either answered wrongness judgments or punishment judgments but never both (for them not to be able to directly compare their judgments). However, this would also have induced a lower statistical power in the potential detection of the effects and/or a higher sample size of participants required. It may also have disturbed the other effects' analysis and generalization (as main effects of social distance would not have been reported on the exact same judgment types across participants). As the current investigation (regarding type of judgment interacting with social distance) was not the first focus of the current experiment, we preferred to keep a well-powered within-participant design to preserve the main effects of interest (i.e., social distance) from more noise. It could however be worth the cost to replicate this experiment following these adaptations to further explore this question.

Experiment 5's Findings[97] on Decision Type as a Moderator of Social Distance

Method

As a reminder, in this experiment, we replicated and extended previous findings regarding the effect of social distance to the victim with the Everyday Moral Conflict Situations (EMCS) material (Singer et al., 2019). In a first part, we asked participants to choose for each of the 40 situations between two behavioral options, one altruistic, the other

[97] This work has been published as: Szczepaniak, Z., Gaboriaud, A., Quinton, J.-C., & Smeding, A. (2024). The Effects of Social Distance and Gender on Moral Decisions and Judgments. *Social Psychology*, *55*(1), 25–36. https://doi.org/10.1027/1864-9335/a000537

egoistic. Whereas in the second part of the same experiment, participants went again through the same 40 situations (framed under either their egoistic or altruistic version) but were asked about the wrongness or rightness of the performed act (see Figure 6.3 in Chapter 6 for a reminder of the whole procedure). Although not preregistered and not initially tested, this experimental design allows to directly compare in an exploratory vein the answers given in the first part (as behavioral choices) with those given in the second part (as judgments), especially regarding social distance influence.[98]

Results

In direct response to this objective, we synthesize below (even if redundant with what has already been reported earlier in Chapter 6) results regarding social distance effects on behavioral choices (in part A), then the same effects on moral judgments (in part B). We found, in part A, a main effect of social distance to the victim (d = 0.12) that was particularly true for males, who more likely chose the altruistic behavioral choices in case of a close one involved as the victim compared to a distant other. On moral judgments (part B), this effect of social distance was opposite in direction depending on the valence of the scenario, with the interaction effect being significant (d = 0.38). Participants judged egoistic scenarios involving a close one as the victim as less morally wrong (b = -0.30) and altruistic scenarios involving a distant one as more morally right (b = 0.24). We can already notice that social distance effects on moral judgments only for altruistic scenarios do not really match in direction the same effect for behavioral choices (i.e., choosing the altruistic option more often toward close ones in part A vs. judging the altruistic option more morally right when toward distant ones in part B). We will further discuss this inconsistency in the following discussion section.

[98] Note that the exploratory analysis reported in Chapter 6 (pp. 189–190) regarding the correlation between answers in part A and answers in part B was originally framed as a way to see how participants' first behavioral choices (in part A) may have influenced their perception of the same situations judged in part B. In this sense, it does not—contrary to what we might think in a first instance due to the section title—directly tackle the link and/or the difference in social distance influence between the two modes of response (behavioral choice vs. moral judgment).

Discussion & Limitations

In this experiment, even if we could not directly compare these findings in a statistical way, we descriptively notice that the effect of social distance to the victim was quite different in terms of effect size (with Cohen's *d* estimations) between the two parts of the experiment. According to these results, which would need further confirmatory replication within a single step design (i.e., directly comparing judgments vs. choices of action in a within or between participant design, see the discussion above), the effect of social distance to the victim appears to be stronger when tested on moral judgments of wrongness/rightness than on behavioral choices. This is in direct contradiction with findings from Tassy and collaborators, who found (within single experimental designs) that behavioral choices elicit a stronger effect of social distance compared to mere evaluative judgments, with social distance being manipulated toward the victim of the situation (Tassy, Deruelle, et al., 2013; Tassy, Oullier, et al., 2013).

The crucial parameter to explain this gap may be in relation to the type of moral judgments at stake. Tassy and collaborators (2013; 2013) asked participants to rate 'how acceptable' it was to choose the most utilitarian option (e.g., sacrifice one person to save 4 others), then asking them successively whether they would have done so in the situation. In our experiment above, we asked participants (in accordance with the original version of the EMCS, see Singer et al., 2019) to answer about 'what do [they] do' in this situation (in part A), then to judge how morally right or wrong was the pre-determined choice of action (in part B). It would therefore be interesting to further test whether we obtain such differentiated effects of social distance between choice of action and judgments when asking for acceptability judgments rather than wrongness/rightness judgments—even if we have low expectations given previous findings showing great similarity between both types of judgment (see Cushman, 2008; Malle, 2021).

Another important and not explicitly noticed limitation of Tassy et al.'s research is the fact that both judgments and choices of action were successively answered in a within-participant design, without counterbalancing the order of presentation of both questions

between participant (Tassy, Oullier, et al., 2013). We may therefore think that the evaluative judgment about the acceptability of the utilitarian behavior influenced their following effective choice of action (and/or depending on the way it was implemented, that people may have changed afterwards their first judgmental opinion once they had answered about what would be their own decision in that situation). In their other research however (Tassy, Deruelle, et al., 2013), controlling for the order of presentation of both types of response did not have any significant influence on final answers, with still a stronger impact of social distance to the victim in case of choices of action.

Another limitation applying to both of these studies is the binary mode of response, with participants answering via either the utilitarian option or the deontological one (Tassy, Deruelle, et al., 2013; Tassy, Oullier, et al., 2013). We can suggest that the answers would not have been so differentiated between both response modes if participants had been allowed to answer in a more nuanced (continuous) way (see Gautheron et al., 2023, for a discussion about dichotomic vs. continuous response modes in morality).

We should however notice that the current descriptive findings are consistent with research from Gold et al. (2015) who found, as reported earlier, a stronger influence of social distance on moral judgments (of permissibility or rightness) compared to choices of action (should act or not) in real-life trolley dilemmas. It is however less relevant compared to our own current design in Experiment 5 as social distance was manipulated toward the agent in Gold et al.'s research (2015), with either oneself having to directly choose whether to act or not in the situation and judge their own behavior, or another one (with self as mere observer) in the agent role. Looking closer to their descriptive results, it seems that there was a huge difference between both types of responses (judgment vs. choice) when self was involved as the agent, but no difference anymore when self was an observer.

Regarding social distance effects' direction, we noticed an inconsistency between what is found for behavioral choices (in part A) and what is found for moral judgments (in part B), especially for altruistic scenarios. We indeed found the simple effect of social distance for altruistic scenarios (in part B, on moral judgments) to be significant and positive,

meaning people were more ready to judge the act as morally right when performed toward a distant other (compared to a close one). Whereas in the first part, they were more likely to act in an altruistic manner toward a close one rather than toward a distant other. For egoistic scenarios however, it does not change the effect's direction as people were judging the act as more immoral when performed toward a distant other (compared to a close one), consistent with the tendency to act less egoistically toward a close (vs. distant) other (in part A). More than a discrepancy in the effect of social distance between behavioral choices and evaluative judgments, it seems here that we are in front of an inconsistency in results for altruistic behaviors especially (or pro-social morality more generally). This may again come from the nature of the task, with a more prescriptive task in the first part (what would/should you do in that situation?) and a more normative task in the second part (how right or wrong is it to act this way?). People may have emphasized the moral obligation (prescriptive) they have toward their close ones in the first part, whereas they overall judged within a more objective eye in the second (normative) one. Reversal of social distance effects depending on type of judgment have also been found in Gold et al.'s study (2015). They found a positive direction of social distance effect on judgments of permissibility (i.e., observers judged utilitarian choices—made by others—as more permissible than when they themselves had to make a choice) and a negative overall effect of social distance on the percentage of utilitarian action when passing from self-perspective to observer-perspective.

Finally, we could further question whether our operationalization of behavioral choices compared to moral judgments was the best to produce this effect, even if the current study was not designed per se in this endeavor. Although quite close to what was implemented by Gold et al. (2015), Tassy, Deruelle, et al. (2013), and Tassy, Oullier, et al. (2013), people may yet have inferred the current behavioral choice question not as a real choice entailing concrete consequences but more—like for judgments of punishment—as a projection in future hypothetical action. The question at stake (i.e., 'What do you do?') remains indeed a hypothetical decision and not (or to a lesser extent) a real decision as people are aware they are not really experiencing that conflict in real-life. The current

question about the potential differentiated effects of social distance between a mere evaluative judgment compared to what people would do in real life thus also deals at some point with the hypothetical nature (and/or realism) of the situation. What is underlying here is therefore the possible interactive effects between plural dimensions of psychological distance, as briefly suggested in the theoretical part of the present dissertation: The likelihood of the situation (or hypothetical distance) may also play a role in how social distance influences people's judgments and behaviors, a possibility that may be further examined in future research.

Social Distance and Severity of the Situation

Rationale and Overview

In the experimental part of this dissertation, we have suggested that the severity of the various situations may play a role in how social distance influences people's moral decision. By severity, we mean the magnitude of the bad consequences caused by the action of the agent and toward the victim of the situation. We thus speak about a high severe situation when the perpetrated act causes the death of the victim (as in trolley-like situations), or at least irreversible damages. Whereas a mild or low severe situation involves some harm (be it physical or emotional) but that could to some extent be repaired. Note that some authors speak about severity for the outcome factor referring to the mere presence of harmful consequences (vs. neutral ones) (see for an example, Frisch et al., 2021). We will however not further consider this mere outcome effect in the current section, as it is, in our mind, quite different from the question of the severity of the act.[99] Similarly, some authors differentiated whether the perpetrated act was an emotional injury or a physical harm (see for instance McKee et al., 2023). Although this factor might also play a role to determine the

[99] We could imagine a transgression that is potentially severe in terms of consequences for the victim, but happened to be unsuccessfully performed, that is, the expected consequence never happening (see notably the examples of Leloup et al.'s database, 2018 with 'attempted harm' situations). In a similar way, hurting someone can be considered as implying harmful consequences, but do not provide the same severity of consequences for the victim depending on whether it involves a psychological, emotional, or physical injury.

severity of the perpetrated act, we will not consider the type of injury in the present section when focusing on severity.

We already know that severity has some importance in morality in general (i.e., not specifically related to social distance effects). For instance, Trémolière and De Neys (2013) showed that people chose less the utilitarian (vs. deontological) option when confronted to a trolley-like dilemma involving (very) severe harm (as death) compared to less severe harm. Schwartz et al. (2022) also showed that the harm severity (in addition to the agent's intent) moderated moral judgments in direction of less acceptability, more punishment, and more blame attributed to the agent in case of a severe (vs. mild) act. The importance of this factor should however not be overestimated, as noticed by B. Trémolière (personal communication, November 4, 2024). Indeed, some authors found on the contrary no real effect of harm severity on the degree of punishment advised to the agent as well as on the intensity of punishment rates (Balafoutas et al., 2016), even when controlling for the need for help (e.g., when the agent throws a coffee cup but at the same time some books falls out of their bag).

However, this parameter may be crucial regarding social distance influence properly, as suggested by some authors (e.g., Weidman et al., 2020). As noticed in the scoping review preliminary findings (Chapter 3, pp. 97–98), we found that most studies (among those that have been presently coded) used as materials either very high severe hypothetical situations such as trolley-like dilemmas or low severe everyday-like situations as in the original EMCS. However, they rarely confront a large panel of situations in terms of severity (from low to high severe situations), hence not allowing to directly compare the potentially differentiated effects of social distance within each situation type.

In Weidman et al.'s study (2020) described in the previous chapter, the authors found social distance effects to be significantly influenced by the severity of the situation at play. The tendency to protect close agents compared to distant agents (by not reporting them to the police) was even stronger in case of a severe wrongdoing (e.g., credit card fraud) compared to a less severe transgression (e.g., download music illegally). Consistent with

this finding, we could expect more generally the effect of social distance to be stronger in case of a severe transgression compared to a mild or low severe transgression (at least when manipulated toward the agent of the situation).

In a completely different setting, within diverse work organizations, Kouabenan et al. (2001) showed a greater tendency for people to attribute accidents at work to internal rather than external causes, and to more likely protect themselves or their own group from blame (i.e., attributing the accident less to internal causes and more to external ones) compared to an outgroup agent. The authors moreover found in Study 2 an interaction effect between social distance to the victim of the accident (ingroup vs. outgroup) and the severity of the accident (mild vs. severe). When the accident happened to an outgroup colleague (i.e., a subordinate if the subject is a supervisor and vice versa), there was no difference in internal attributions between mild and severe accidents. However, when the accident target was an ingroup other, people made less (vs. more) internal attributions for a severe (vs. mild) accident (see Kouabenan et al., 2001, Study 2). In the other way around (looking at the moderated effect of social distance): Social distance influence was found as even stronger in case of a severe harm compared to a less severe harm (see especially 2001, Study 2 results).

Note however that in this research, the authors manipulated the accident victim's identity (either an ingroup or an outgroup member), but these targets were also at the same time those who performed the accidental act. Therefore, when asking participants to attribute the causes of the accident either to internal or external causes, it is more about attributing more or less blame to the target of judgment, being at the same time victim and agent of the harmful situation. Therefore, and as we have argued all along the current dissertation in favor of more clearly distinguishing the roles of the agent and victim in the situations under study, these findings—although very interesting—have only limited predictive power for our question of interest.

Given diverse findings suggesting a potential moderation of social distance effects by severity, we will synthesize in this section what has been found in re-analyses of our own

experiments regarding this question. We will first report what has been found exploratorily in Experiments 1, 2, and 3 when considering an a priori and naive classification of the severity of the various scenarios used (from Leloup et al.'s materials, 2018). Second, we will review explicitly what has been found in a confirmatory way in Experiment 6, with severity and social distance manipulated both in a counterbalanced within-participant and within-scenarios design. We will then discuss these findings in light of what has been previously found in the literature.

Exploratory Findings About Severity as a Moderator of Social Distance (Experiments 1 to 3)

Method & Results

Experiments 1 to 3 of the current dissertation dealt with the same moral materials to investigate the effects of social distance within diverse everyday moral situations (adapted from Leloup et al.'s database, 2018). Therefore, we pre-classified these vignettes (especially the 'harmful and caused by the agent' outcome versions) in terms of the severity of their consequences for the victim. This classification was made naively in an exploratory vein. As such, a scenario was classified as high severe if the consequence for the victim could induce some irreversible damages, whereas it was classified as moderately severe when the consequence was important but reversible. It was however considered as low severe if the consequence was not really damaging (as for slippering in a puddle of oil, without hurting oneself considerably). Table 7.2 below displays example of scenarios in each of these three categories taken from Leloup et al.'s (2018) materials. In the end, based on this a priori classification, there were 20 low and 20 high severe scenarios, and 24 scenarios of mid-severity. You can find the R-scripts for these supplementary exploratory analyses on the respective OSF project pages of each experiment.

Table 7.2

Examples of Scenarios Adapted from Leloup et al. (2018) Within each Category of our Naive Classification of Severity

Severity Naive Classification	Example in accidental & harmful outcome (caused by the agent)
Low severe (x20 scenarios)	Christophe is strolling through a funfair. He comes to a stand where you can test your strength by punching a punching bag. Olivier is standing at the back of the stand, chatting with a friend. Christophe puts money into the attraction and punches the punching bag. Christophe thought Olivier was far enough away from the target. The punching bag hits Olivier, who is not feeling well.
Mid severe (x24 scenarios)	Nathalie and Anne are taking a sculpture class and have to work together to weld metal pieces. Nathalie welds while Anne holds the pieces together. Nathalie welds right next to Anne's gloves. Nathalie thought the gloves were thick enough. The gloves don't resist the heat and Anne is burned.
High severe (x20 scenarios)	Alain and David are rock climbing; they bring their own equipment. Alain brings the harnesses, while David brings the ropes. Alain gives David a harness. Alain thought that the harness he had chosen for David was not damaged. During the climb, the harness breaks and David is injured.

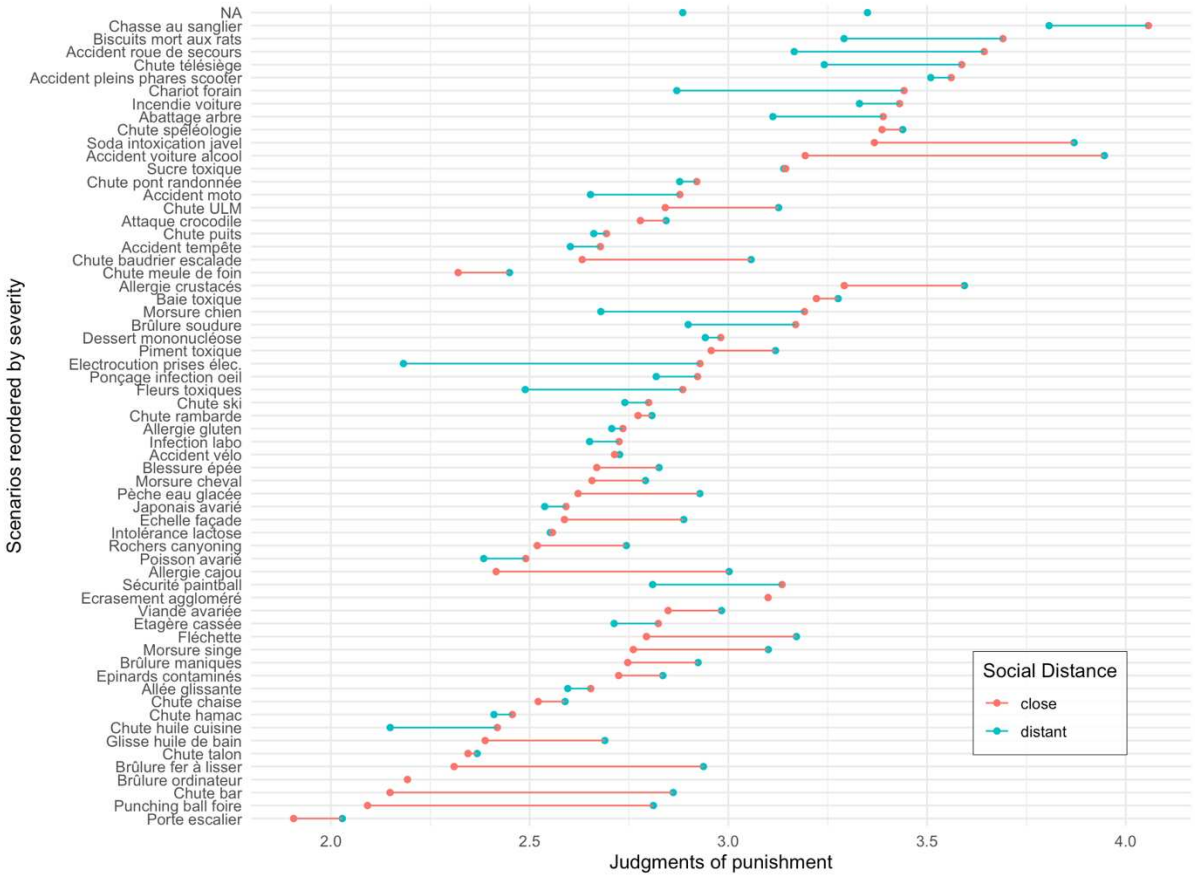
Experiment 1's Results. First, we were able, thanks to this a priori classification of the scenarios' severity, to descriptively visualize how social distance effects were displayed depending on the various scenarios. Figure 7.3 below comprises a distribution of the scenarios with their corresponding mean judgment of punishment to the agent depending on the social distance condition (close for red dots vs. distant for blue dots). Scenarios have been presently reordered to appear in severity increasing order (i.e., the least severe cases at the bottom and the most severe cases at the top), then ordered within these a priori

categories (low, mid, high severity) by increasing judgments of punishment to the close agent.

What can be noticed already on this pattern is that beyond the large diversity of social distance effects across scenarios (both in terms of size and direction), the situations' severity seems descriptively positively correlated with judgments of punishment. There seem to be indeed harsher punishments for scenarios pre-classified as high severe in terms of consequences, and on the contrary, more lenient (or moderated) judgments in general for scenarios of lower severity. It is however hard to say whether there are more 'positive' effects of social distance (i.e., distant others judged harsher than close ones, that is, red lines in Figure 7.3) for low or mid-severe cases and more 'negative' effects of social distance (i.e., close ones judged harsher than distant others, that is, blue lines) for more severe scenarios. Indeed, when displaying the effects of social distance reordered secondarily by judgments toward distant others, this pattern appears less clearly (and even seems to be reversed, see supplemental online material for more). It still suggests that the Black Sheep hypothesis (i.e., favoring distant others over close ones, in accordance with Tendency 2 of the literature) occurred more frequently than expected in our scenarios (i.e., around 13 out of 60 scenarios with a sufficiently large effect in this direction in Figure 7.3 below).

Figure 7.3

Social Distance Effects on Judgments of Punishment Depending on Scenarios' Severity in Exp.1



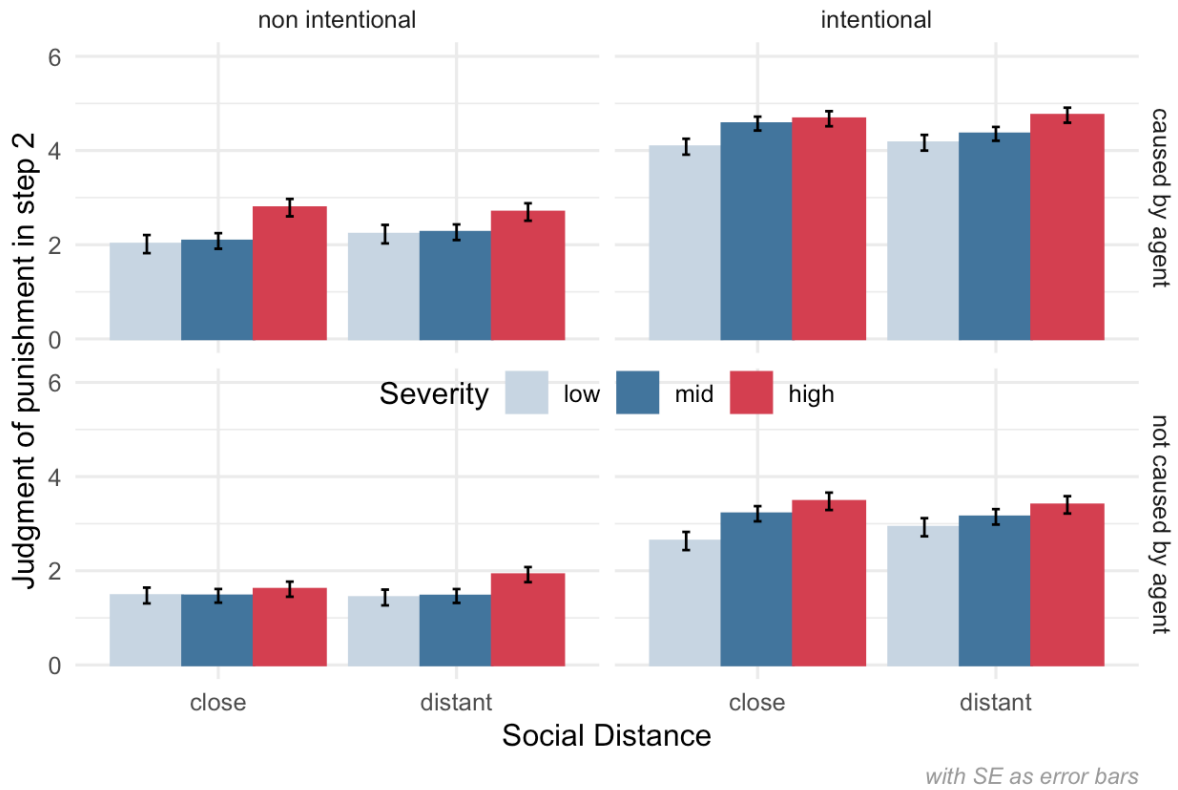
Note. Scenarios are reordered by severity from low severe situations (at the bottom vertically) to high severe situations (at the top vertically) passing through moderately severe cases (in the middle vertically). On horizontal axis, judgments of punishment are displayed from more lenient judgments (0) to more severe judgments (6). Here the abscissa axis has been zoomed (from 2 to 4) to better visualize the discrepancies between social distance effects.

When running the mixed model analysis while taking into account severity as a factor in addition to the previous factors of this experiment (i.e., intent and causality), we found a main effect of severity, both between the low vs. mid severe consequences (Sev1: b = 0.20, SE = 0.10, t(48.69) = 2.03, p = .05, 95% CI [0.002,0.40], d = 0.13) and between these

categories and scenarios classified as high severe in terms of consequences (Sev2: *b* = 0.44, *SE* = 0.09, *t*(49.53) = 4.99, *p* < .001, [0.27,0.62], *d* = 0.29). As expected, people punished the agent more harshly when the scenario involved a high severe consequence (*M* = 3.16, *SE* = 0.10) than mid- (*M* = 2.82, *SE* = 0.09) or low (*M* = 2.62, *SE* = 0.10) severe consequences (see Figure 7.4).

Figure 7.4

Judgments of Punishment in Exp.1 (Step 2) Depending on Intention, Causality, Social Distance, and Scenarios' Severity



Note. Judgments of punishment are measured on a 0-6 range continuous scale from (0) Not severe at all to (6) Very severe punishment.

This exploratory analysis also revealed a significant interaction between intent and severity (with Sev1), *b* = 0.34, *SE* = 0.16, *t*(49.90) = 2.13, *p* = .04, [0.02,0.65], *d* = 0.11. When decomposing this interaction, we found a significant simple effect of severity (between mid and low severe scenarios) for intentional scenarios (*b* = 0.37, *SE* = 0.13, *t*(48.82) = 2.76,

p = .008, [0.10,0.64]), but that was not significant for non-intentional scenarios (*b* = 0.03, *p* = .78). Contrary to what was expected regarding social distance moderation by severity, we did not detect such an interaction effect, on both contrasts of severity (*b* = -0.16, *p* = .11, [-0.36,0.04], *d* = -0.05 for Sev1, and *b* = -0.001, *p* = .99, [-0.17,0.17], d = -0.0003 for Sev2) (see Figure 7.4).

We however detected through this analysis a four-way-interaction between intent, causality, social distance, and severity (Sev2) (*b* = -0.95, *SE* = 0.35, t(3851.40) = -2.74, *p* = .0062, [-1.64,-0.27], *d* = -0.08). This interaction was qualified by a significant and negative intent-by-causality-by-social distance interaction effect for high severity scenarios (*b* = -0.55, *SE* = 0.28, *t*(3814.24) = -1.94, *p* = .05, [-1.11,0.005]) but that was not significant (and reversed in sign) for low or mid severe scenarios (*b* = 0.46, *p* = .14 and *b* = 0.34, *p* = .16 respectively). When looking at the simple effects of social distance (what currently interests us) within each combination of intent, causality and severity conditions, we found this effect to be significant only in scenarios with a non-intentionally perpetrated act, entailing a *high severe* outcome which is not directly caused by the agent (*b* = 0.31, *SE* = 0.14, *t*(3829.76) = 2.19, *p* = .03, [0.03,0.59]). For these cases especially, people seem more likely to punish socially distant agents more harshly than close ones (see Figure 7.4). It is also consistent with the idea that these conditions represent situations where nothing happens directly due to the agent's action, hence the higher tendency in those situations to favor close ones compared to distant others (i.e., the only criterion on which participants may differentiate their judgments), as shown in complementary analyses on mouse trajectories (see Chapter 8, pp. 325–327).

Moreover, when looking at these simple effects of social distance within the non-intentional but caused by the agent condition (i.e., top-left corner of Figure 7.4), we can descriptively observe that the direction of social distance effects is reversed depending on the severity of the scenarios. Even if not reaching statistical significance, people seem to punish socially distant agents slightly more harshly when confronted to a low or mid-severe outcome (*b* = 0.21, *p* = .18 and *b* = 0.19, *p* = .13 respectively). On the contrary, people tend

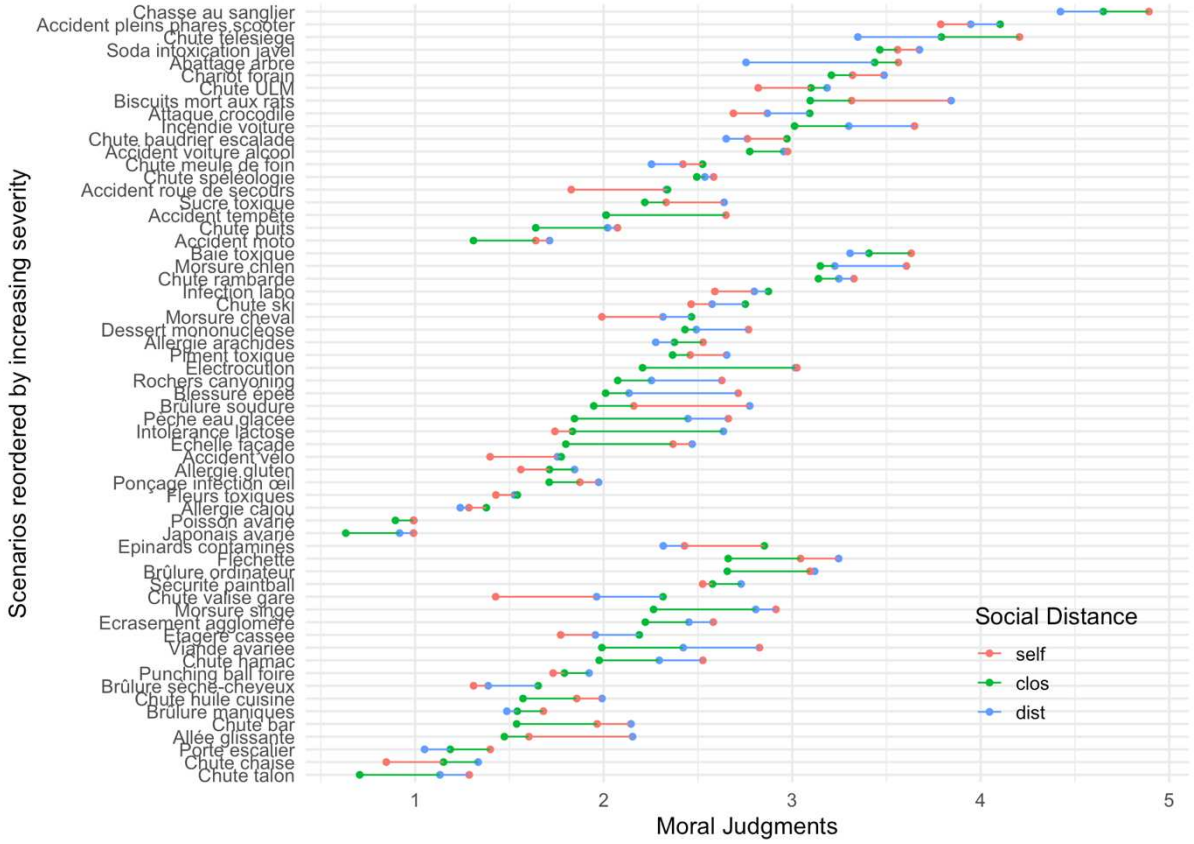
to judge their close ones as agents slightly more harshly than socially distant others in case of a high severe wrongdoing (b = -0.09, p = .52) (see Figure 7.4).

Experiment 2's Results. We reused the previous a priori classification of scenarios' severity (low, mid, high) to again visualize the effects of social distance according to these categories (see Figure 7.5). We can notice here again that effects of social distance are diverse in size and direction, and not especially more frequent in one direction (e.g., favoritism for close others compared to self and distant others) within one category of severity compared to the others. As for the previous experiment, the scenarios' severity seems to be positively correlated with the harshness of moral judgments. Indeed, the least severe scenarios are more figuring at the bottom left corner of the figure, whereas more severe scenarios appear more from the middle to the right end of the judgment scale (although not triggering very extreme judgments). However, as noticed above, it seems hard to descriptively observe a clear differentiation of social distance effects depending on the scenarios' severity.

As for the previous experiment, we statistically tested the effect of severity in an exploratory vein to check whether social distance effects were moderated by the severity of scenarios in this experiment. With the same analytical procedure as in Exp.1, we found as a result of our exploratory mixed model analysis a main effect of severity [Sev2] comparing moral judgments on scenarios with high severe consequences and those with only low or mid-severe consequences for the victim (b = 0.80, SE = 0.20, t(54.78) = 3.94, p < .001, d = 0.42). As expected, people generally judged the scenario more harshly when involving high severe consequences (M = 2.98, SE = 0.18) than low or mid severe consequences (M = 2.18, SE = 0.14)—as a priori coded within our naive classification. We did not find however a significant effect of this factor between low and mid-severity scenarios (b = 0.05, SE = 0.22, t(52.72) = 0.23, p = .817, d = 0.03), with low severe scenarios (M = 2.15, SE = 0.19) not being judged significantly more leniently than mid-severe scenarios (M = 2.20, SE = 0.16).

Figure 7.5

Social Distance Effects on Moral Judgments Depending on Scenarios' Severity in Exp.2

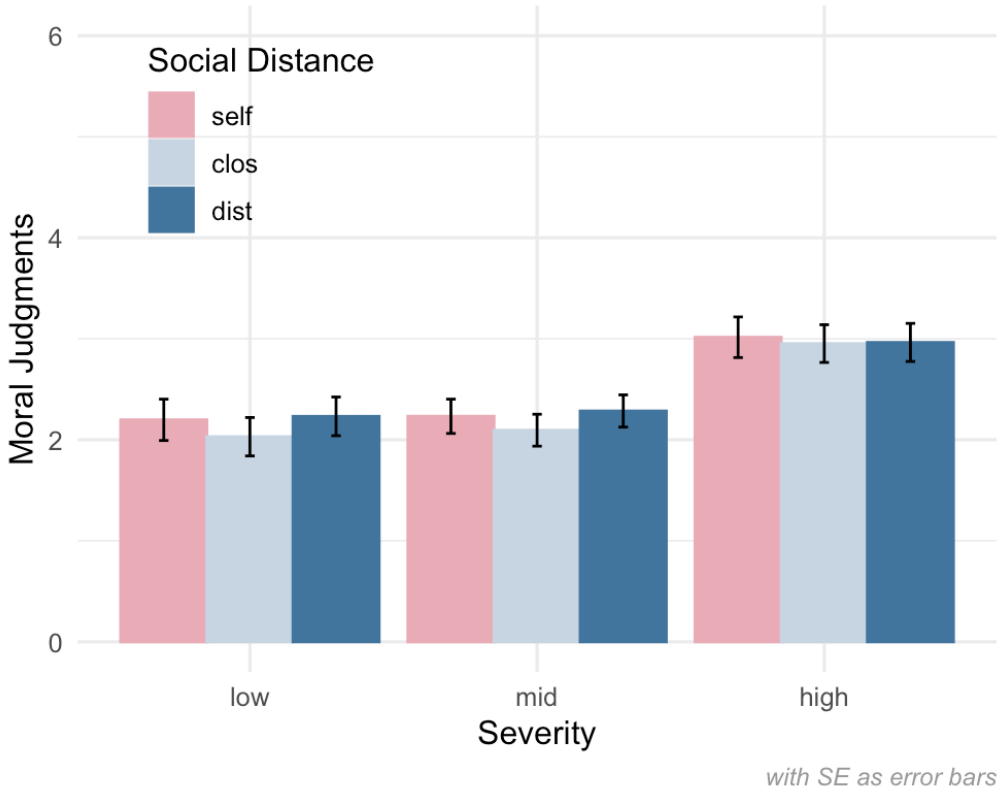


Note. Scenarios are reordered by severity from low severe situations (at the bottom vertically) to high severe situations (at the top vertically) passing through moderately severe cases (in the middle vertically). On horizontal axis, moral judgments are displayed from more lenient judgments (0) to more severe judgments (6).

This analysis also revealed an interaction effect between social distance (on Dst1, that is, comparing judgments toward a distant agent vs. a close one or the self as agent) and severity (on Sev2, that is, comparing high vs. low & mid scenarios), b = -0.14, SE = 0.06, t(53.98) = -2.15, p = .036, d = -0.04. In particular, when looking descriptively (see Figure 7.6), it seems that social distance had less influence within high severity scenarios (almost no difference between self, close and distant other as agents), and more influence in low and mid-severe scenarios.

Figure 7.6

Moral Judgments in Exp.2 Depending on Social Distance and Scenarios' Severity



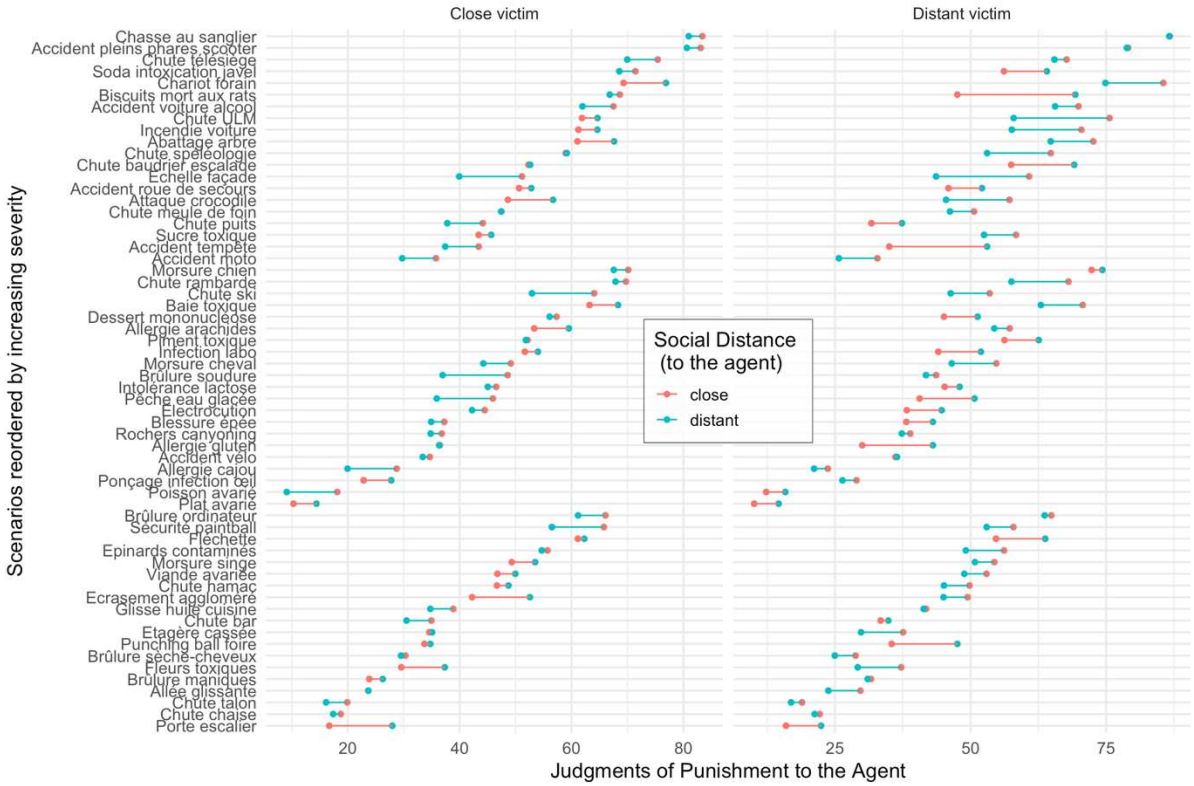
Note. Moral judgments are measured on a 0-6 range continuous scale from (0) Not severe at all to (6) Very severe.

Experiment 3's Results. Descriptively, when displaying the effects of social distance depending on the scenarios' severity (as a priori classified within the same three levels), we obtained once again apparently very diverse effects of social distance to the agent. It was less the case for social distance effects to the victim which seem less influential for all types of situations, consistently with its non-significant influence in confirmatory results (see pp. 212–213, Chapter 6). As illustrated in Figure 7.7, social distance to the agent seems to be more influential when a distant victim is involved compared to a close one as the victim. Moreover, within this condition (distant victim), social distance to the agent effects seem to be stronger in more severe scenarios (top of the figure) compared to low and mid-severe ones. Regarding social distance effects' direction (still relative to the agent and in distant victim condition, see right side of the figure), we here

again notice both directions of effects (Tendencies 1 and 2) depending on scenarios, with almost as many cases in each direction.

Figure 7.7

Social Distance to the Agent Effects on Judgments of Punishment Depending on Scenarios' Severity and on Victim's Distance in Exp.3



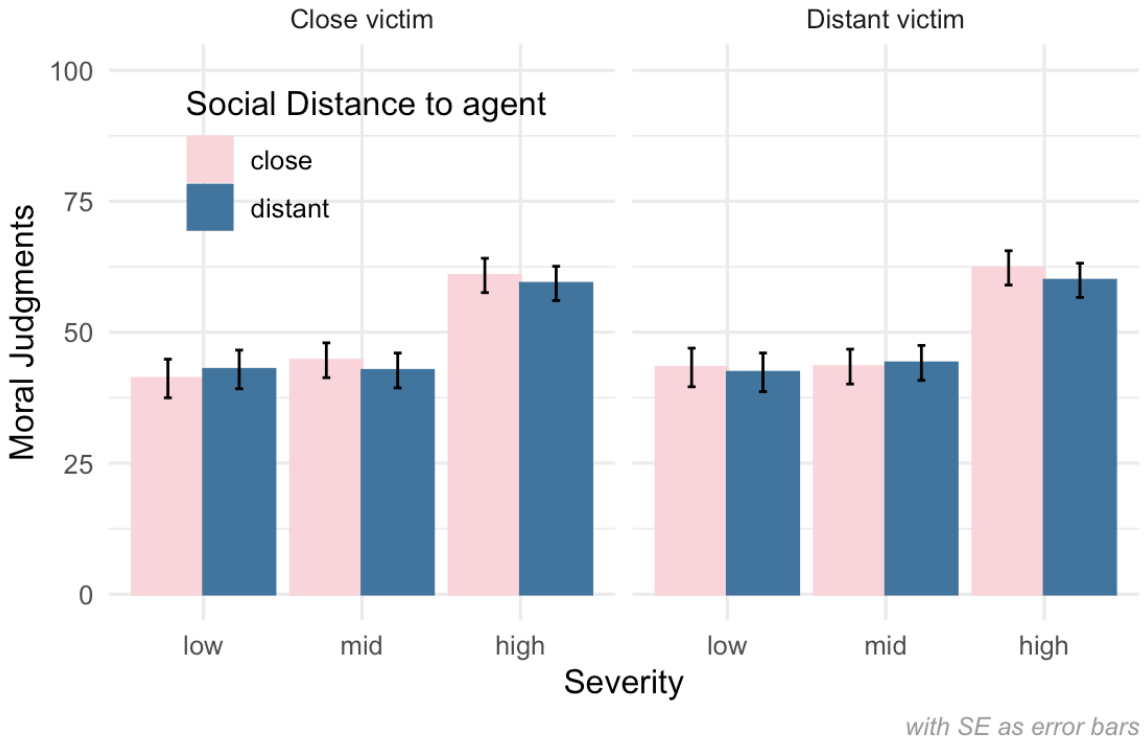
Note. Scenarios are reordered by severity from low severe situations (at the bottom vertically) to high severe situations (at the top vertically) passing through moderately severe cases (in the middle vertically). On horizontal axis, moral judgments are displayed from more lenient judgments (0) to more severe judgments (100).

When running mixed model analysis on these data but including severity as a factor (both in fixed and random terms), we found a main effect of severity on the second Helmert contrast [Sev2], b = 17.51, SE = 3.63, $t(22.68)$ = 4.82, $p < .001$, 95% CI [10.00,25.03], d = 0.52 (see Figure 7.8). People indeed did punish the agent more harshly in high severe situations (M = 60.59, SE = 3.14) compared to those where the consequences for the victim are low or mid severe (M = 43.07, SE = 2.58). There was however no significant differences

in judgments between these last two conditions (b = 1.31, SE = 4.45, t(19.61) = 0.29, p = .77, [-7.99,10.61], d = 0.04). No other effect related to severity reached statistical significance. In particular, we detected no significant interaction effect between severity and social distance (be it toward the agent or to the victim, -1.83 < b_s < 0.57 ; -1.45 < t_s < 0.45 ; p_s > .15).

Figure 7.8

Judgments of Punishment in Exp.3 Depending on Social Distance to the Agent and to the Victim and on Scenarios' Severity



Note. Judgments of punishment were measured on a 0-100 range continuous scale from (0) Not severe at all to (6) Very severe.

Confirmatory Findings on Severity as a Moderator of Social Distance in Experiment 6

Method

To test in a confirmatory vein the impact of severity on social distance effect, we manipulated in Experiment 6 both factors in a within-stimuli and within-participant design. As a reminder, participants had to judge as morally right (6) or wrong (0) the 40 situations from

the EMCS (Singer et al., 2019), adapted so that they appear under the egoistic version (see Chapter 6, pp. 196–202, for more details about the study method). Social distance was manipulated in this experiment toward the victim of the situation. Through this design, we expected regarding the severity factor that more severe situations would be judged more harshly than less severe situations. Also, with regard to previous findings, the effect of social distance was expected to interact with the situations' severity, such that people would judge less severe transgressions committed against distant others as harsher than against close others, but the reversed pattern would be found for more severe cases (i.e., harsher judgments when against a close one compared to a distant victim).

Results

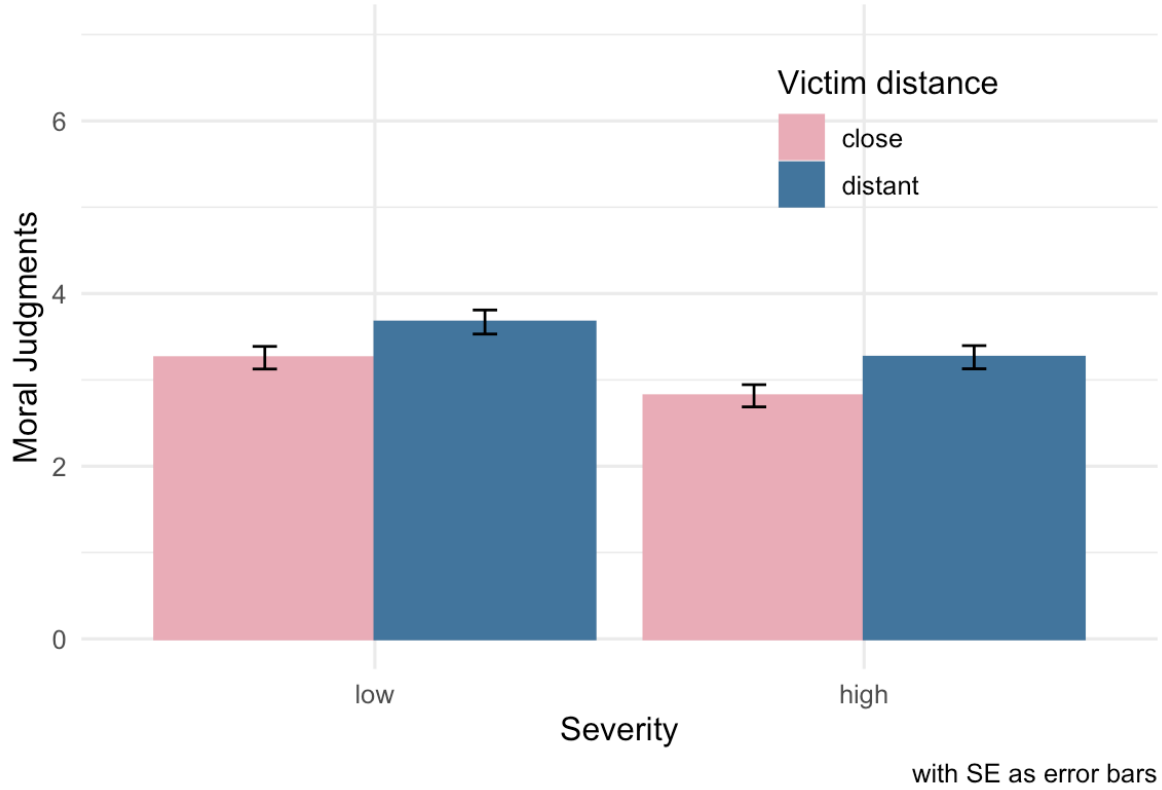
As a result, we found as expected a main effect of severity, b = -0.42, SE = 0.07, $t(38.36)$ = -5.84, $p < .001$, 95% CI [-0.57,-0.28], d = -0.27, with more severe situations judged as more morally wrong (M = 3.04, SE = 0.12) than low severity ones (M = 3.46, SE = 0.13). As reported earlier, we however did not find the interaction effect with social distance, b = 0.03, SE = 0.06, $t(5530.15)$ = 0.53, p = .59, [-0.09,0.16], d = 0.01. In other words, social distance effect was not significantly moderated by the level of severity (b = 0.41 for low severe situations; b = 0.45 for high severe situations) (see Figure 7.9).

Discussion & Limitations

In Experiments 1 to 3, with our a priori naive classification of the severity of the consequences for the victim in each scenario, we did find as expected that the more severe the outcome was, the harsher the moral judgment. Although expected, we did not find a significant moderation effect of social distance by the situation's severity in Experiment 1. Descriptively however, and for accidental harm especially, it seems that social distance had opposite directions of effect depending on the severity of the situation. Although not significant, people tend to judge their close ones more leniently compared to a distant other in case of low (or mid-) severe wrongdoing whereas they tend to judge them more harshly in case of severe wrongdoing.

Figure 7.9

Moral Judgments in Exp.6 Depending on Social Distance and Situations' Severity



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more morally right (vs. wrong) judgments.

Experiment 2 exploratory results also indicate—based on the same a priori classification of scenarios' severity—a dependency between social distance and severity. The two-way interaction was significant when running analyses (especially between high and low/mid-severe scenarios and between distant and close/self as agents). We descriptively noticed a discrepancy between close and distant agent conditions in low and mid-severe situations, whereas this difference seemed to disappear in high-severe situations. This finding is not consistent with previous research, as Weidman et al. (2020) for instance rather found a stronger effect of social distance in high severe situations compared to less severe ones. Similarly, exploratory findings for Experiment 3 indicated a main effect of severity especially between low/mid severe situations and high severe situations. There

was however again no interaction effect with social distance, be it to the victim or to the agent of the situation.

In Experiment 6, where the severity of the situation was experimentally manipulated along with social distance to the victim, we again did not find—beyond the main effect of severity on moral judgments—any significant interaction effect between severity and social distance (d = 0.01). That is, social distance effect was not significantly different across low vs. high severity conditions, contrarily to what was suggested by previous research. The manipulation check in the second part of the experiment further indicated that participants indeed perceived high severity situations as more severe than low severe situations (b = 0.42, $p < .001$). Differences were not very important compared to what is usually expected for manipulation checks (i.e., when directly asking the question that should correspond to the direct manipulation of the variable), here suggested by the Cohen's d estimated at 0.25. This may, however, stem from the everyday nature of the moral scenarios used (i.e., EMCS), as explained here after.

In the end, what could have been problematic in the current experiment to detect such interactive effects between social distance and severity may be the fact that severity was manipulated to a low degree due to material constraints. We indeed chose the current materials (be it the one of Leloup et al., 2018 or that of Singer et al., 2019) for their everyday-like nature. This has the advantage to be easier for participants to project themselves in the various situations in a more plausible way. However, it also makes it more difficult to modulate severity in such situations. It indeed appears as less frequent, hence less plausible, that such everyday-like situations involve the death of the victim and/or some irremediable damages. Therefore, the current (even when manipulated) more severe versions of the scenarios may have been still very low in terms of the consequences' severity (i.e., rarely engaging instances of irremediable harm).

We should also in future research look more carefully about the different types of severity that the situation could involve. Even if we presently only wanted to measure (in Exp.1 to 3) or manipulate (in Exp.6) severity in terms of the seriousness of consequences for

the victim, it may sometimes also mix at some point severity of consequences for the agent him/herself. That is, either in terms of the action itself (even if harmless) or in terms of consequences for third parties (as when animals are hurt in an accident). Therefore, the severity of the transgression is sometimes more complicated than anticipated to control for.

Moreover, we should notice that our severity classification of Leloup et al.'s battery of scenarios (2018) was established naively by ourselves and not based on a reliable evaluation from a representative sample of participants. Therefore, we cannot ensure that some scenarios may not have been misclassified. In addition, we used the same severity classification for each scenario across the various experiments' re-analyses whereas some changes have been applied between each experiments' materials. Starting with the scenario versions exposed to participants: in Exp.1, all versions were exposed (non-intentional and caused by agent vs. intentional and not caused etc.), whereas in Exp.2 and Exp.3, only non-intentionally perpetrated harms were displayed to participants. To extend and strengthen this analysis of the situations' severity impact on social distance influence, we plan to create a more rigorous and valid classification of the severity of these scenarios from Leloup et al. (2018) (ongoing study).

Social Distance and Agent's Moral Responsibility

Rationale and Overview

To end this chapter, we aimed at investigating whether the agent's moral responsibility could also be of major interest for social distance effect moderation. Although confounded sometimes with the previously tackled factors of the agent's intent and/or their effective causal role in producing the outcome, what we will focus on in the current section is the part of moral responsibility that cannot be attributed to some external cause or mitigating circumstances (i.e., entirely due to the agent's action). Let's take an example to make this clearer: Think about a situation where one would invite his friend at a specific restaurant, although knowing that this restaurant had a history of hygiene issues. When the friend gets sick during the night from the tainted meat he had just eaten, who is the person to blame?

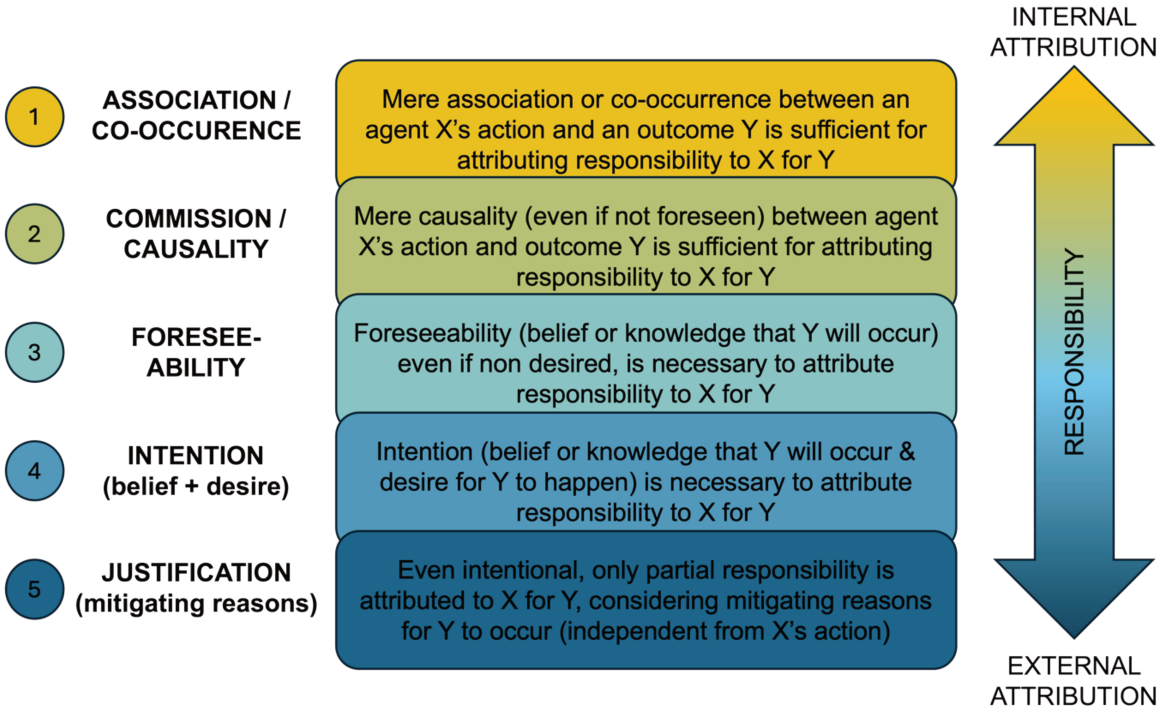
Surely, the agent (i.e., the one who invited his friend) would be to blame at some point as he knew of the restaurant's lack of hygiene and could have avoided this bad outcome. However, we could also attribute the blame in a non-negligeable way to the restaurant manager and their chefs, hence attenuating a bit the agent's responsibility given these external factors (on which he does not have much power).

Heider (1958) conceptualized this idea via the introduction of a 5-stages-taxonomy of personal responsibility (not especially applied to moral issues), with varying degrees of responsibility attributed to the agent depending on the parameters of the situation. As reproduced in Figure 7.10 below, this taxonomy aims at reflecting in decreasing order the conditions where we tend to attribute the most responsibility to the agent (i.e., internal causes) to those where we attribute him/her the least responsibility (i.e., hence responsibility being more attributed to external factors, as environmental ones). In the first level of responsibility, we then attribute the whole responsibility for an event to the agent even if we do not have any evidence directly linking causally their action to the produced outcome (as for descendants of nazis being held responsible for their slaughters). On the contrary, in the last level of responsibility, we give a huge importance to external circumstances that may attenuate the agent's personal responsibility, even though they directly intentionally perpetrated the wrongdoing.

What is to be understood in here is that Heider's levels of responsibility (1958) reflect how people reason about a single situation to attribute more or less responsibility to the agent (vs. to the environment). In other words, it reflects how much people choose, consciously or not, to ignore or give more weight to certain factors according to what suits them. However, it may not reflect the reality of what happened in the situation, as coined by Ross (1977) through the fundamental attribution error (i.e., people overestimating internal causes for others' wrong behaviors despite powerful external causes). The aim of the current section is then—based on an a priori naive classification of the agent's level of responsibility in the various moral situations—to exploratorily reanalyze Experiments' 1 to 3 findings, in order to detect a potential moderation of social distance effects.

Figure 7.10

Reproduction of Heider's (1958) Five Levels of Attribution for Personal Responsibility



Exploratory Findings on Responsibility as a Moderator of Social Distance (Experiments 1 to 3)

Method

Following the same logic than previously done for severity, we a priori classified naively the 64 scenarios from Leloup et al.'s battery (2018) in terms of the perceived agent's responsibility in the consequences occurring. As such, a scenario (in the intentional and caused by the agent version) was classified as 'high' in responsibility when its consequences were almost entirely due to the action of the agent and could not (or only to a very limited extent) be explained by other external factors (i.e., external circumstances). On the contrary, a scenario was considered as 'low' in responsibility (or 'mid' depending on the situation) when the agent—even if the action was performed intentionally and with a harmful outcome directly caused by him or her—is only partially responsible for it. To build on the above example, going to a restaurant with hygiene concerns and being poisoned by a plate of tainted meat could be considered as an example of low responsibility: Even if the agent

knew the restaurant had some hygiene issues, it was not him directly who served the tainted dish. Table 7.3 below gives an example of scenario for each of these three categories of responsibility (low, mid, high) from Leloup et al.'s adapted materials (2018). As can be seen in the table, a few more scenarios were classified in the low responsibility category (N = 26) compared to the mid (N = 16) or high (N = 22) responsibility categories. You can find the R-scripts for these supplementary exploratory analyses on the respective OSF project pages of each experiment.

Table 7.3

Examples of Scenarios Adapted from Leloup et al. (2018) Within each Category of our Naive Classification of Responsibility

Responsibility Naive Classification	**Example in accidental & harmful outcome (caused directly by the agent)**
Low responsibility (x26 scenarios)	Cecile and Sabrina visit a chemistry laboratory. As Cecile serves them coffee, Sabrina asks for sugar in hers. Next to the coffee machine is a box containing white powder. Cecile puts the powder in Sabrina's coffee. Cecile thought the powder was not a toxic substance. The powder was not sugar and Sabrina does not feel well.
Mid responsibility (x16 scenarios)	Claude takes his car to meet Bruno, who is on a scooter. They pass each other on a poorly lit country road. Claude decides to leave his headlights on when he meets Bruno. Claude thinks his headlights are well adjusted and don't dazzle him. The headlights dazzle Bruno, who has an accident.
High responsibility (x22 scenarios)	One Saturday evening, Sylvie and Fabienne are playing darts in a café. Sylvie is approached by an acquaintance with whom she is chatting not far from the dartboard. Impatient to play in turn, Fabienne throws her dart. Fabienne thought Sylvie was far enough away from the dart's trajectory. Sylvie is in the dart's path and is injured.

Exploratory Analyses

Below, we again successively present the exploratory findings regarding the impact of the agent's responsibility on moral judgments in light of social distance influence for Experiments 1, 2, and 3 based on our own a priori and naive classification. The following inferential analyses were run with mixed models. Again, we reused the reduced model from main confirmatory analyses but adding the factors of interest as fixed factors[100]. The factor of responsibility was then compounded with three modalities, coded as a Helmert contrast: [Resp1] (-0.5) low vs. (+0.5) mild and (0) high; then [Resp2] (-1/3) low and (-1/3) mid vs. (2/3) high. For each study, beyond the main effects of responsibility on moral judgments—with situations where the agent's responsibility is high being judged more harshly than mid, and then than low responsible ones—, we expected that social distance effect would be stronger for high (vs. mid/low) responsible scenarios.

Experiment 1's Results. As for severity, we first displayed visually the distribution of the effects of social distance among all intent-by-causality conditions depending on the a priori classification of scenarios in terms of the perceived agent's level of responsibility in each (see Figure 7.11). Scenarios in Figure 7.11 have been reordered to appear in responsibility increasing order (i.e., the least responsible cases at the bottom and the most responsible cases at the top), then ordered within these a priori categories (low, mid, high responsibility) by increasing judgments of punishment to the close agent (in red dots).

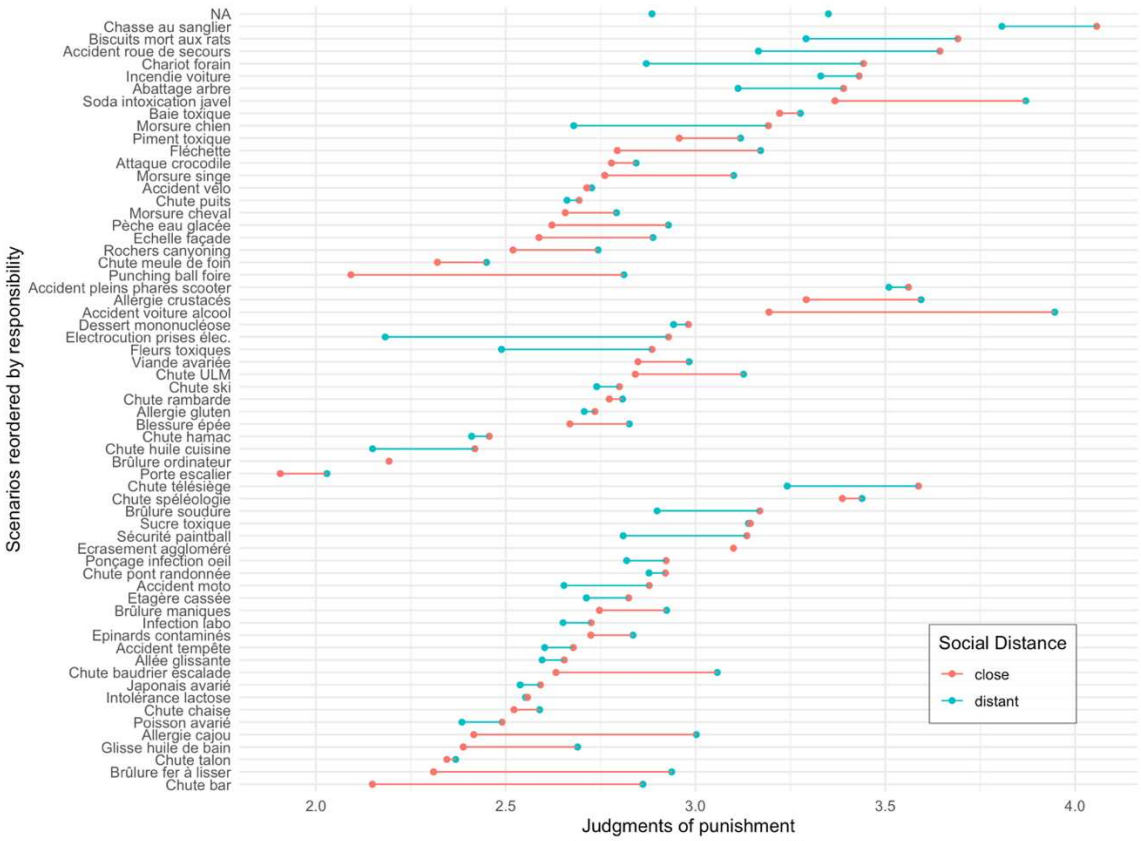
Visually, it seems first that there are very diverse effects of social distance (both in directions and size) across the three levels of responsibility. When looking closer, it seems to indicate quite a non-negligible number of negative effects for social distance (i.e., when close one is judged harsher than distant one for the same wrongdoing, that is, in blue lines in the figure) for about 14 out of the 60 scenarios. They seem moreover to be bigger in size

[100] Note that because we also tested exploratorily for each of these experiments the influence of the scenarios' severity as a moderator of social distance effect, the mixed model of analyses was actually comprising both factors and their interaction in a single exploratory model for each study. Also note that we have simplified these exploratory models for some experimental data re-analyses (i.e., Exp.2 and Exp.3 in particular), as the complete models (starting from previous reduced models) were too complex and failed to converge.

for high responsibility scenarios (top of the figure) than for situations with only a low responsibility of the agent (bottom of the figure). It is hard yet to deduce it with a high level of confidence as this tendency quite disappears visually when displaying these effects according to distant agents' judgments (see supplemental online material for more). Positive effects of social distance on the contrary (i.e., in red lines, with close ones being punished less harshly than distant others) seem present in all categories.

Figure 7.11

Social Distance Effects on Judgments of Punishment Depending on the Level of Agent's Responsibility in Scenarios in Exp.1



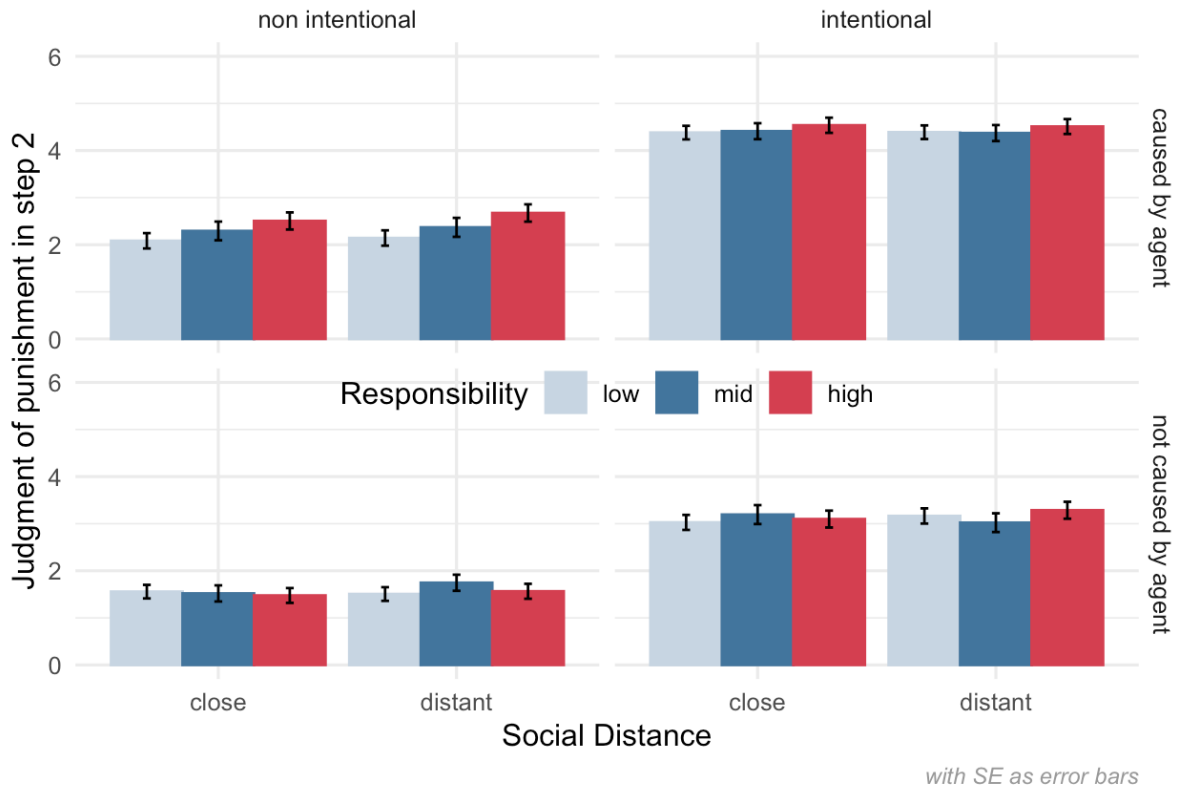
Note. Scenarios are reordered by the level of responsibility of the agent from low (at the bottom vertically) to high responsibility (at the top vertically) passing through medium responsibility (in the middle vertically). On horizontal axis, judgments of punishment are displayed from more lenient judgments (0) to more severe judgments (6). Here the abscissa axis has been zoomed (from 2 to 4) to better visualize the discrepancies between social distance effects.

When running the mixed model of analysis while taking into account responsibility as a factor in addition to the previous factors of this experiment (i.e., intent and causality), we found no main effect of responsibility on judgments of punishment, both between the low vs. mid conditions (Resp1: b = 0.08, SE = 0.10, t(50.00) = 0.83, p = .41, 95% CI [-0.12,0.29], d = 0.06) and between these categories and scenarios classified as high responsibility of the agent (Resp2: b = 0.13, SE = 0.09, t(51.35) = 1.49, p = .14, [-0.05,0.31], d = 0.09). Contrarily to what was expected, people did not punish the agent significantly less harshly when the scenario involved other potential sources of responsibility (i.e., low responsibility cases, M = 2.78, SE = 0.09 and mid responsibility, M = 2.87, SE = 0.11) compared to when it involved almost no other possible sources of responsibility in the scenario environment (i.e., high responsibility cases, M = 2.96, SE = 0.10) (see Figure 7.12).

We however detected a significant interaction effect between causality and responsibility (Resp2), b = -0.24, SE = 0.11, t(53.62) = -2.16, p = .03, [-0.46,-0.02], d = -0.08. This interaction effect was qualified by a significant simple effect of responsibility (between high and low/mid-responsibility levels) in the scenarios with an outcome directly caused by the agent (b = 0.25, SE = 0.11, t(51.34) = 2.31, p = .02, [0.03,0.47]), whereas the simple effect of responsibility (Resp2) in scenarios with an outcome not directly caused by the agent was not significant (b = 0.01, p = .89) (see Figure 7.12). This makes sense, as people do not need to adjust their reasoning about the agent's punishment according to their level of responsibility when they did not directly cause the harmful outcome in the situation. No other main or interaction effect was detected in relation with the factor of responsibility. In particular, no moderation effect was found with social distance, be it with the first contrast of responsibility—Resp1 opposing the low and mid conditions—or the second—Resp2 opposing low/mid and high responsibility conditions (b = -0.02, p = .88 and b = 0.07, p = .38 respectively).

Figure 7.12

Judgments of Punishment in Exp.1 (Step 2) Depending on Intention, Causality, Social Distance, and Level of Responsibility in Scenarios

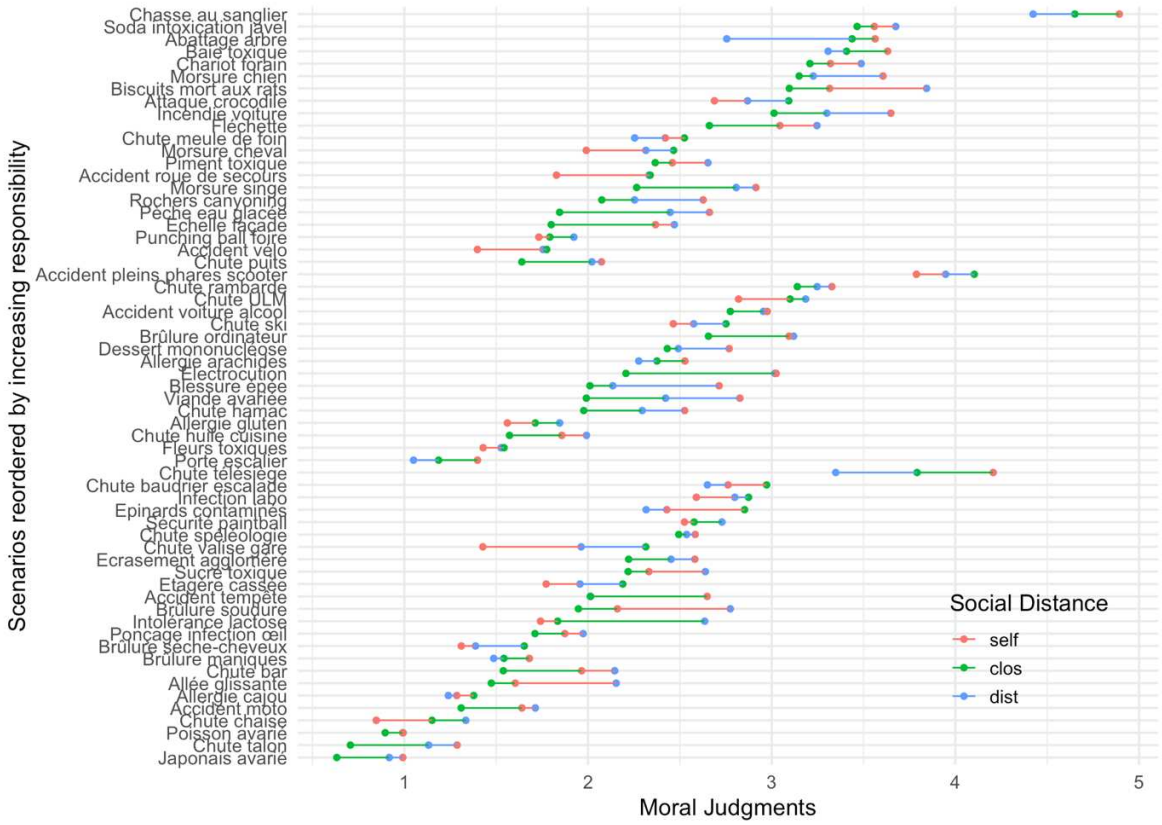


Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more severe (vs. less severe) judgments of punishment on the 0–6 range.

Experiment 2's Results. Visually, when displaying the effects of social distance depending on the agent's level of responsibility within the various scenarios in Exp.2, we could not deduce a clear logic about how these diverse effects were distributed among the situations (see Figure 7.13). In particular, beyond the mere effect of responsibility itself (with visually harsher moral judgments for cases when the agent was highly responsible), we observed for each responsibility level (i.e., low, mid, high) effects of social distance in both directions: favoring oneself or a close one over a stranger (consistent with Tend.1); and favoring distant other over self or close one, even if less frequent (consistent with Tend.2).

Figure 7.13

Social Distance Effects on Moral Judgments Depending on Level of Agent's Responsibility in Scenarios in Exp.2



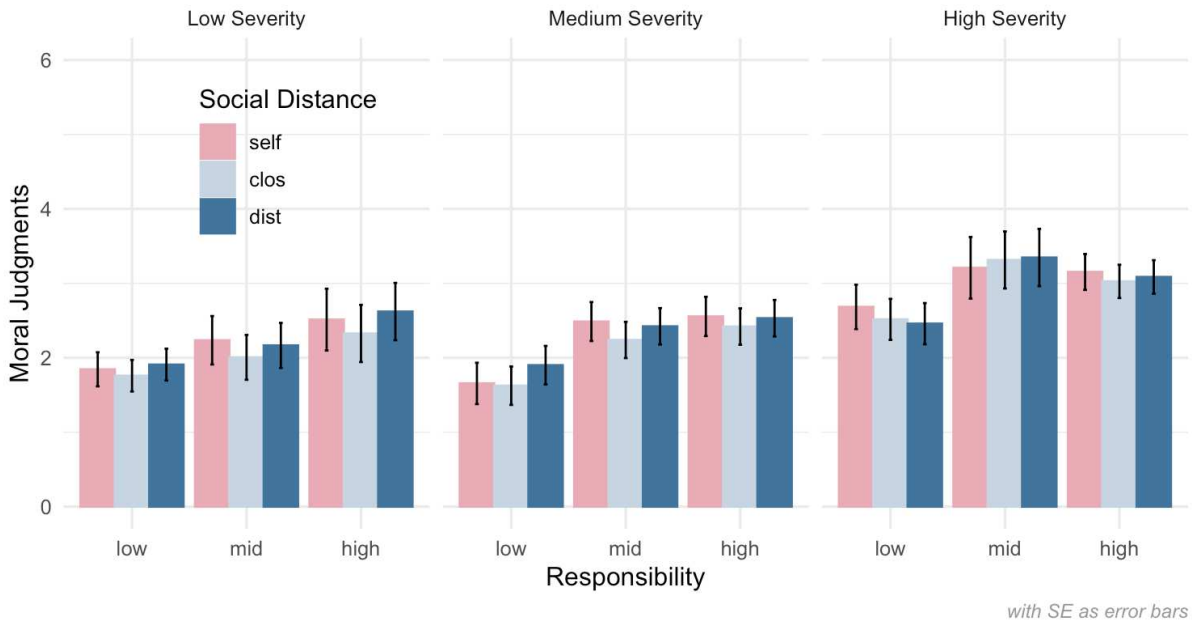
Note. Scenarios are reordered by the level of responsibility of the agent from low (at the bottom vertically) to high responsibility (at the top vertically) passing through medium responsibility (in the middle vertically), then within these categories by increasing judgments toward the close agent. On horizontal axis, moral judgments are displayed from more lenient judgments (0) to more severe judgments (6).

When running the mixed model analysis on these data including responsibility as a fixed factor, we found a main effect of responsibility [Resp1] comparing conditions of low and mid-levels of agent's responsibility (b = 0.56, SE = 0.22, t(52.85) = 2.52, p = .015, 95% CI [0.11,1.01], d = 0.30). As expected, people generally judged the scenario less harshly when the agent's responsibility in the act was low (M = 2.04, SE = 0.16) compared to when they were moderately responsible for the bad outcome (M = 2.60, SE = 0.19)—as a priori coded

within our own naive classification. We did not find however a significant effect of this factor between low/mid-responsibility and high responsibility situations, *b* = 0.37, *SE* = 0.20, *t*(53.52) = 1.88, *p* = .066, *d* = 0.20, even if marginal means were consistent in direction with what we expected for this effect (for high responsibility, *M* = 2.69, *SE* = 0.18 ; for low/mid responsibility, *M* = 2.32, *SE* = 0.14) (see Figure 7.14).

Figure 7.14

Moral Judgments in Exp.2 Depending on Social Distance, Scenarios' Severity, and Level of Responsibility



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more severe (vs. less severe) moral judgments on the 0–6 range scale.

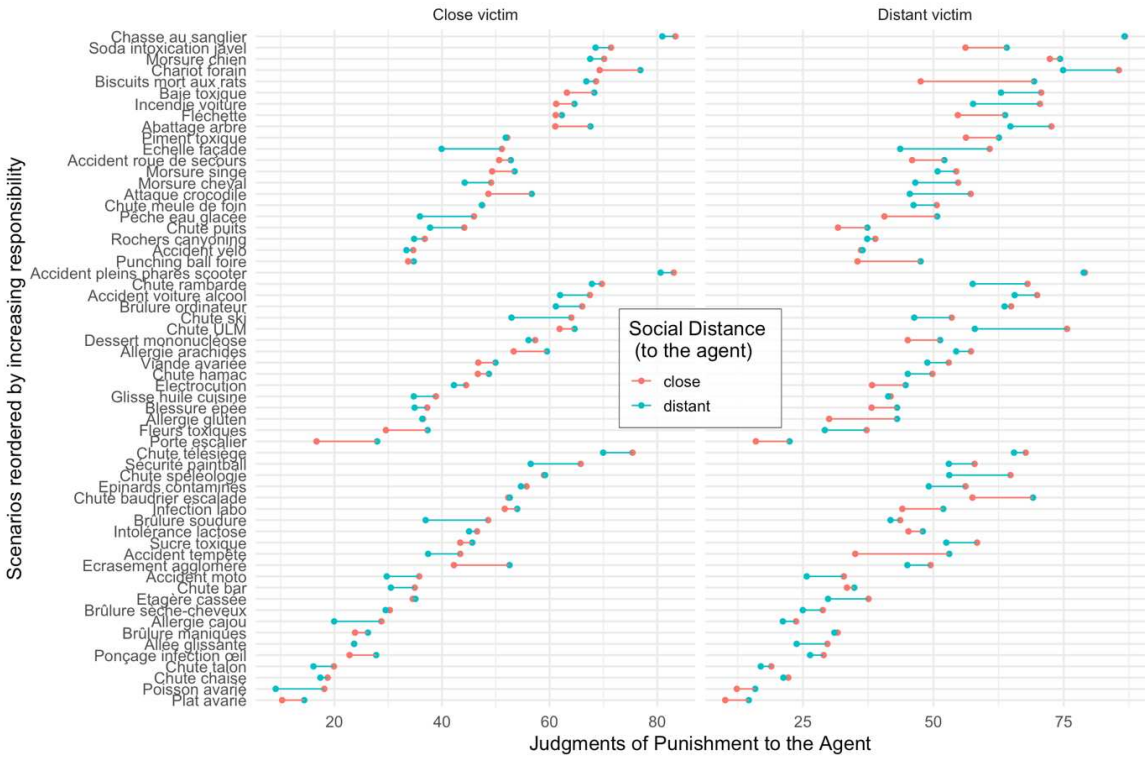
Contrary to expectations, no two-way interaction effect was detected between responsibility and social distance (even crossing both contrasts of each factor). However, there was a significant three-way interaction effect between social distance, severity [Sev2] (as also included in the model, see previous section), and responsibility [Resp1], on both contrasts of social distance [Dst1 & Dst2] (*b* = 0.36, *SE* = 0.17, *t*(52.02) = 2.15, *p* = .036, *d* = 0.05; and *b* = 0.45, *SE* = 0.21, *t*(50.77) = 2.19, *p* = .034, *d* = 0.06 respectively). In terms of direction, we can notice that the U-pattern of social distance effects described earlier in

Chapter 5 (p. 141) seems to appear mainly in the low and mid conditions of severity (except for low responsibility cases where it appears less clearly, see Figure 7.14). In high severe scenarios however, a clear discrepancy appears between low and mid/high conditions of responsibility (Figure 7.14). In situations where the consequence for the victim is high severe but where the agent has only low responsibility on the occurring consequence, the effect of social distance seems to be fully consistent with the Black Sheep hypothesis (i.e., Tendency 2 of literature), that is, judging oneself harsher than a close one and then, than a distant other for the accidental harm.

Experiment 3's Results. Applying to Experiment 3 data the same analysis about responsibility influence, we noticed visually the same tendencies than for severity (see Figure 7.15), with social distance effects to the agent being descriptively more influential when the victim is distant (rather than close), and within this latter condition, seemingly stronger effects when responsibility of the agent is high.

Figure 7.15

Social Distance to the Agent Effects on Judgments of Punishment Depending on Level of Agent's Responsibility and Social Distance to the Victim in Exp.3

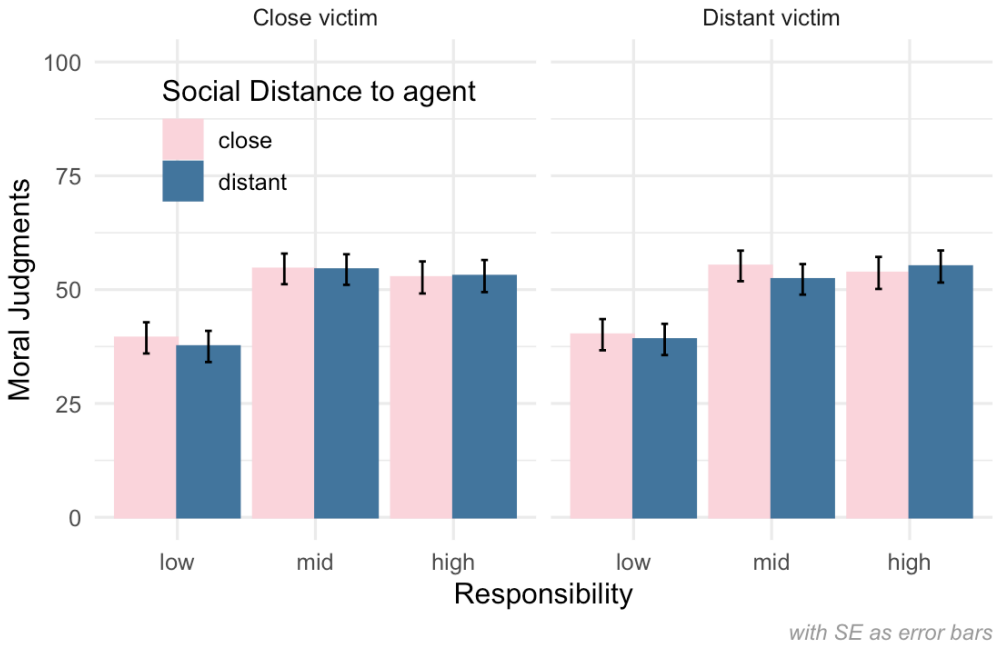


Note. Scenarios are reordered by the level of responsibility of the agent from low (at the bottom vertically) to high responsibility (at the top vertically) passing through medium responsibility (in the middle vertically). On horizontal axis, moral judgments are displayed from more lenient (0) to more severe judgments of punishment (100).

When running the mixed model analysis to check these tendencies statistically, we found a main effect of responsibility on the first Helmert contrast [Resp1], b = 15.09, SE = 4.28, t(26.07) = 3.52, p = .0016, 95% CI [6.28,23.89], d = 0.45, but not on the second one [Resp2], b = 7.04, SE = 3.79, t(14.41) = 1.85, p = .08, [-1.08,15.15], d = 0.21. Participants judged scenarios where the agent was only low responsible for the harmful outcome (M = 39.02, SE = 3.34) as significantly less harsh than those where the agent was more responsible (M = 54.11, SE = 3.23) (see Figure 7.16). No other effect involving responsibility (on both contrasts) was found as significant.

Figure 7.16

Moral Judgments of Punishment in Exp.3 Depending on Social Distance and Level of Agent's Responsibility



Note. Higher (vs. lower) values on the ordinate axis (Y) correspond to more severe (vs. less severe) judgments of punishment on the 0–100 range.

Discussion & Limitations

Using the naive classification we established for the agent's level of responsibility in each scenario, we overall found in Experiments 1 to 3 that people tended to judge situations more leniently when the agent was only low responsible for the harmful outcome. This pattern, although somewhat less consistently observed than for severity, suggests that participants took into account external circumstances that could at least partially explain or mitigate the agent's action. Beyond this main effect however, there was not much differentiated influence of social distance depending on the various levels of responsibility. In particular, we did not find social distance effects to be stronger in case of high responsibility (i.e., when few external parameters at play).

In Experiment 1, with intent and causality being manipulated in a within-participant design, we did not detect any interaction effect between the level of responsibility for the perpetrated act and social distance influence. In other words, the fact that the agent has only low responsibility for the perpetrated act (because of the presence of other external uncontrollable causes) was not influential regarding how participants judged close vs. distant agents. It could be due to the involvement of intent and causality factors within this experiment, which may have hindered the importance of responsibility factor (as seen, responsibility yet interacted with causality).

In Experiment 2, when only accidental harm scenarios were involved, we found this main effect of responsibility, especially between low and mid-responsibility cases. No direct interaction was however found with social distance. This exploratory analysis yet revealed an interaction effect between social distance, severity, and responsibility. The U-pattern of results obtained in main analyses with more severe judgments for self and distant agents compared to close agents was found quite systematically for low and mid severe cases, whereas it was not the case for high severity scenarios. In severe outcome situations and for low responsibility of the agent especially, judgments became more lenient with increasing distance to the agent (in accordance with Tendency 2).

In Experiment 3 finally, we again did not find a differentiated effect of social distance—be it toward the agent of the situation or toward the victim of it—depending on the various levels of responsibility in the scenarios. Only a main difference between low and mid-responsibility scenarios in terms of judgments of punishment was found.

In the end, as it was the case for severity, we did not find through this exploratory analysis significant evidence of a moderation of social distance effects by the level of agent's responsibility in the situation. One main limitation of these conclusions is, again, that the classification of responsibility levels was made by ourselves on an a priori and naive basis. We may therefore want to replicate these investigations or at least reanalyze the current results within a more valid classification of responsibility levels for these scenarios (i.e., a posteriori established according to evaluations from an independent participants' sample). A study with that aim is currently ongoing, asking participants to rate the part of responsibility that is not due to the agent directly but to other potential external influences (emphasizing less the moral responsibility aspect of our investigation).

Conclusions About Moderators of Social Distance

To sum it up, this chapter investigating social distance potential moderation effects did not reveal consistent evidence for any of the moderators presently being tested. In Exp.1 in particular, the traditional key factors of intent and causality were not influenced by social distance on moral judgments of punishment. Despite a considerable variability in social distance effects across scenarios depending on the various intent-by-causality conditions, there was no systematic and consistent pattern resurfacing.

Regarding the type of judgment or decision at stake, results from Exp.2 remain largely inconclusive (i.e., no significant differences between wrongness and punishment judgments). In Exp.5 however, via the comparison of part A (on choices of action) and part B (on moral judgments) effect sizes, we noticed a quite stronger effect of social distance on wrongness judgments compared to choices of action. This goes against common findings of the literature, which more often report stronger effects of social distance for choices

compared to mere evaluations (e.g., Tassy, Deruelle, et al., 2013), though this discrepancy may also reflect differences in the nature of measures between both experimental parts.

As for severity (i.e., the seriousness of consequences for the victim), Experiments 1 to 3 reanalysis showed a clear main effect, with harsher moral judgments overall for high severe scenarios compared to low or mid-severe ones. The only significant interaction effect in Exp.1 was found between intent and severity, while social distance was mainly influential under the non-intentional and not caused by the agent scenarios (i.e., consistently with previous confirmatory findings). In Exp.2, social distance effect seemed more pronounced for low-severe situations than for mid or high severe ones, which contradicts previous findings suggesting a stronger influence for high severe situations (e.g., Weidman et al., 2020). Experiment 6, designed with a manipulation of the situation's severity, revealed no significant moderation of social distance to the victim by this factor.

Finally, responsibility—as naively coded by us as part of what caused the harmful outcome and that cannot be attributed to anything else than the agent—followed overall similar patterns to severity, with yet less clear differentiation between low and mid/high responsibility levels on moral judgments, and not interacting with social distance. Subtle and non-systematic influences seem to arise when responsibility and severity are crossed, suggesting complex but not fully consistent dynamics of social distance influence across various moral contexts.

One key limitation regarding these moderators' investigation lies in the central role of these factors, especially intent and causality, which are well-established as primary drivers of moral judgment. Because they explain a substantial portion of the variance, it becomes difficult to detect more nuanced or secondary effects, such as those related to social distance. This may partially account for the inconsistent or weak (non-significant) interaction effects observed across studies presently analyzed. Additionally, the classification of severity and responsibility in the current scenarios (for Exp.1 to 3) was relatively intuitive and may lack therefore of validity. Developing a more robust and empirically grounded classification of severity and responsibility—ideally based on ratings from a large and

representative sample of participants—could clarify whether the absence of interaction effects with social distance reflects a true absence or a limitation of our scenarios' naive classification. Another possibility is that the scenarios could have lacked realism; even if they were drawn from real-life examples, participants may not have been sufficiently engaged to respond as they would in real-life high-stakes situations. This potential limitation of ecological validity will be discussed further in the general discussion in Chapter 9.

Chapter 8

An Innovative and Dynamic Way to Investigate Social Distance and its Moderators in Everyday Moral Decision-Making

The current chapter aims at exploring a more dynamical way to study the influence of social distance in moral decision-making. As part of social cognition, moral cognition may be submitted to the same automatic influences. We may therefore envision social distance as part of these biases that people further want to mask and to correct for social desirability reasons, that is, to appear as impartial moral beings. This may be the reason why we often failed to detect such an influence over final decisions or judgments across the previous experiments, hence arguing in favor of the use of a more dynamic way to analyze this question. Consequently, we will first develop in the current chapter an extended theoretical rationale for the development and use of a more dynamic way to study social distance influence in morality. We then propose the mouse-tracking technique as an innovative tool to answer these needs, while compensating the drawbacks of other dynamic methods. We then empirically tested this novel paradigm (adapted for moral scenarios), first by replicating the effects of classic factors of influence for morality (Experiments 7 and 8), then by directly challenging the effect of social distance within the same innovative paradigm (Experiment 1, part B, on mouse-tracking data). We will finally discuss the relevance of this tool for our question of interest.

Why and How to Investigate Social Distance Influence on Moral Decision-Making Dynamically?

Everyday Morality as a Socially Situated Phenomenon

In the theoretical part of this dissertation (Chapter 1), we defined moral cognition as a sub-area of social cognition, that is, with applications specifically related to morally relevant situations. In the early 2000's, Smith and Semin (2007) argued that social cognition is

situated, that is, dynamically adjusted to the actor's goals, environment, and internal states—contrarily to the abstract and stable vision often assumed before. The authors indeed describe four characteristics proper to this situated vision of social cognition (Smith & Semin, 2004), which could be applied in the same way to moral cognition (as a subpart of social cognition). These criteria are the fact that cognition is action-oriented (i.e., aims at regulating action), embodied (i.e., embedded in a physical and tangible entity), resulting from the dynamic interaction with an environment, and distributed (across the environment and other social agents) (Smith & Semin, 2004).

We could think it is even more the case when dealing with everyday moral situations, as mainly tackled in the current dissertation. Indeed, compared to more unrealistic situations such as trolley-like dilemmas, everyday scenarios involving moral cognition should be even more socially situated in accordance with the previous definition of Smith and Semin (2004, 2007). Although the nature or type of scenarios should not alter the way social or moral cognition works per se, it could however become harsher for people to mentally represent themselves these fictive and very unlikely situations as embedded in a social context. This vision of moral cognition as socially situated may thus encourage researchers to persevere in using moral material that better reflects what people may encounter on a daily basis, even if this possibly also triggers more variability and noise in detecting the effects of interest.

The consequence of this socially situated view of social and moral cognition for studying our main question of interest is that we cannot get rid of taking into account the social context in which moral situations take place. In particular, considering social distance between the various protagonists (in an embedded social context) is already a stakeholder of this situated initiative (see also Christensen & Gomila, 2012 for a review on the various contextual parameters that can be manipulated in experimental moral situations). However, previous studies about social distance influence only rarely tackled at the same time the dynamic interplay of this factor with the environment and/or with other social agents—corresponding to the third point of Smith and Semin's theory (2004). In accordance with this view, we argue in favor of a dynamic analysis of the processes related to social distance as

involved in moral decision-making, that is, looking at the entire decisional process across time and not only at the mere final decision or judgment.

Social Distance as a Potential Automatic Influence

As seen all along the experimental part of this dissertation, the influence of social distance in moral decision-making may be much more nuanced and ambiguous than previously argued in literature. With our experimental paradigms, we found across the various experiments (see Chapters 5, 6, and 7) very few evidence for systematic social distance biases across the three analytical levels (and when present, not as strong as hypothesized in literature). As suggested earlier, this may stem from the fact that people are generally aware of this potential bias on their judgments (all the more in within-participant designs as presently used). People are at the same time also aware that in morally relevant situations, we generally hold as 'moral' beings those who tend to act and judge with impartiality. Due to the discrepancy between the latter ideal (fully reached when the participant takes the explicit role of a judge) and their effective favoritism biases, people may want to compensate their own judgments or decisions to remain as objective as possible in particular when the task is explicitly dealing with a moral issue. We already know from research on attitudes for instance that some effects—prone to intentional correction by individuals due to social desirability (as for racist or sexist biases)—are difficult to identify based on self-reported or explicit responses. Social distance may be also prone to social desirable responding and therefore requires a more indirect and/or dynamic methodology to be detected (see especially other research dealing with the investigation of the ingroup bias at the implicit level, Leach et al., 2017; Maliszewski et al., 2017).

Relevance of Dynamic Tools to Study Social Distance Influence in Morality

As suggested just above, the investigation of social distance in moral decision-making may require more indirect measures to detect its influence at a more automatic level (i.e., before people consciously correct for it). The Implicit Association Test (IAT) created by Greenwald et al. in 1998 is dedicated to fill this objective: it allows to indirectly measure the force of the association between two concepts (for instance applied to our question:

‘immoral’ & ‘close one’ vs. ‘immoral’ & ‘distant other’). However, there has been much criticism about this methodological tool, notably regarding its predictive power to detect people’s truly discriminative behaviors in real life and not only their knowledge of these biases due to their immersion within a stereotyped culture. Another important limitation of these indirect tasks is the fact they almost always demonstrate a favoritism bias toward the dominant group both when tested with its own members and with the non-dominant group members (e.g., Rougier et al., 2020).

These various indirect measures of attitudes yet all rely mostly on final decisions, such as response time or error rates, to determine automatic biases in attitudes, and can be noised by voluntary faking or strategic responding (see Smeding et al., 2016). These tasks therefore are mainly indicative of implicit associations but do not allow to trace people’s online processing during a single item or how they may correct their first thoughts or attractions during more complex decisions (as it is often the case for moral situations). This would require instead a dynamic technique of investigation, which allows to simultaneously trace online processing at the item level, while also gathering people’s self-reported decisions. In other words, in accordance with Kahneman’s theoretical model of the two speeds of thought (2011) and the dual process theory that goes along, dynamic tools could be useful to measure both fast, intuitive, and emotional processes, along with the final responses resulting from later reflective and deliberative reasoning.

Therefore, given that social distance may influence the whole moral decision-making process, using a dynamic technique to trace both intuitive and deliberative processes is particularly relevant. Further refining the dual-process vision of cognition, some authors suggested—in accordance with the situated social cognition view—that parallel processes are dynamically competing, then integrated simulatenously during the decision course (e.g., Wojnowicz et al., 2009). Cognition would within this theoretical framework correspond to continuous changes made of various interacting and potentially overlapping processes (Spivey & Dale, 2006). Additionally, dynamic techniques of investivation also allow to

differentiate the various factors and their respective weight across the decision process (i.e., across time) (see Hehman et al., 2015).

Mouse-Tracking as a Dynamic Tool to Study Social & Moral Cognition

Tracing Hand Movements to Get On-Going Influences of Mind

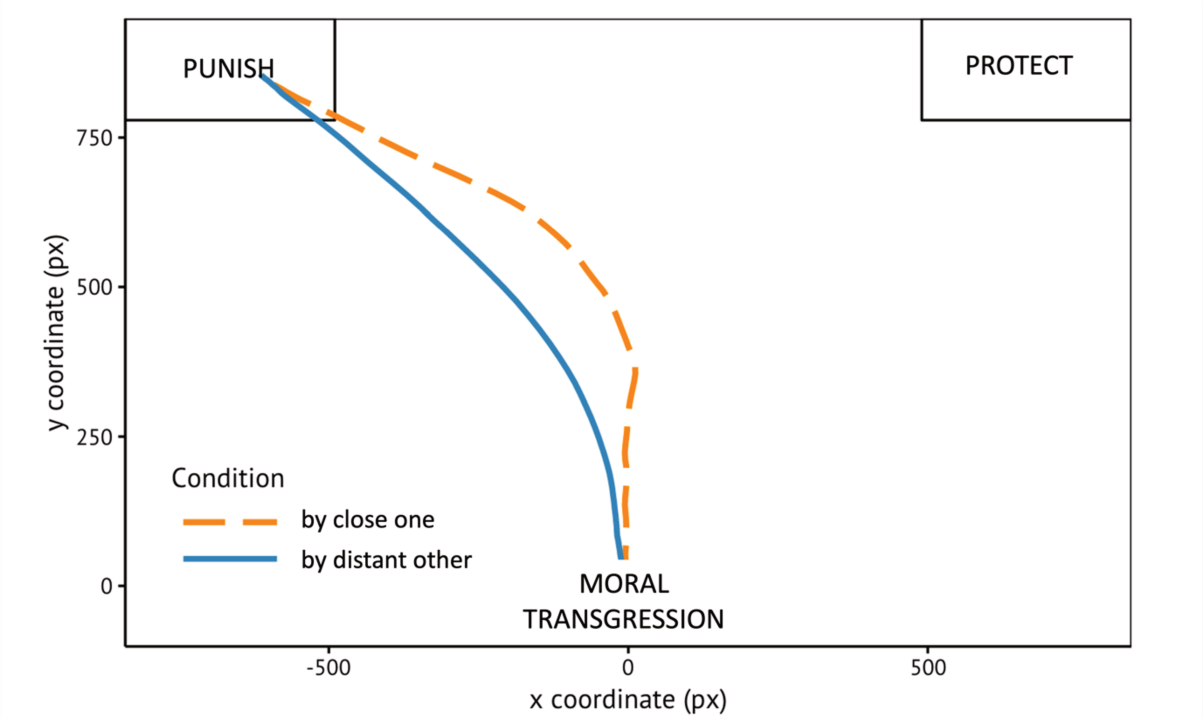
One way to measure decision process in a dynamic way is to rely on measures that allow to study the whole decision process and not the only final decision. As noticed by Spivey and Dale (2006), mouse-tracking technique—the registration of mouse movements across time during a computerized task—is one way to follow the continuity of mind. As well synthesized by Freeman and collaborators (Freeman, Dale, & Farmer, 2011; Freeman & Ambady, 2010) who created the original MouseTracker software: 'Hand in motion reveals mind in motion'. More than a simple eloquent phrasing, it reflects the idea that by mouse movements, we can access a bit of how mind works in real-time. As argued by Freeman, Dale, and Farmer (2011), this is due to the fact that hand dynamics (movements at fine-grained temporal intervals) are inherently connected to what happens at the brain level. Freeman, Ambady, et al. (2011) indeed demonstrated how the categorization of a visual stimulus co-occurs with the immediate activation of motor cortex to already prepare for hand movements. Mouse-tracking is therefore a relevant technique to trace the dynamics of a decision-making process, that is, the whole decisional process in a continuous way across time (see Spivey & Dale, 2006).

Applied to social cognition, plural authors used this tool to investigate social categorization or stereotype processes, starting from Freeman and collaborators themselves (see also Wojnowicz et al., 2009). Freeman and Ambady (2009) found for instance that when people had to categorize atypical targets (faces with mixed features of male and female) within two gender stereotypes, they were continuously influenced by the attractor of the opposite sex-stereotype, as revealed by mouse trajectories across time. Smeding et al. (2016) also provided findings in this sense via adapting IAT-like tasks within a mouse-tracking paradigm to investigate implicit gender-related stereotypes among various social

groups (either stereotypical or counter-stereotypical). They found as expected a higher competition between attractors (with higher deviations of mouse trajectory toward the opposite attractor) for stereotype-incongruent trials within stereotypical social groups (i.e., males in Engineering and females in Humanities) and for stereotype-congruent trials within counter-stereotypical social groups (i.e., females in Engineering and males in Humanities) (see Smeding et al., 2016). We reproduced in Figure 8.1 below what could be an example of a classic mouse-tracking task (as the one developped by Freeman & Ambady, 2009) but applied to our current question of interest (with fictious results).

Figure 8.1

A Fictive Example of a Classic Mouse-Tracking Task Applied to Social Distance



Note. Figure adapted from Kieslich et al. (2018): 'Mouse-tracking : A practical guide to implementation and analysis' [PsyArXiv preprint]

Another example of mouse-tracking application in social cognition but more directly linked to the current dissertation topic is the research from Maliszewski et al. (2017), investigating the association between implicit ingroup bias and following mouse movements in a preference task. In Study 2, the authors found that participants with strong ingroup

favoritism bias (as measured through an IAT) were more inclined in the subsequent task and under the distractor condition to show the ingroup bias, but only within early mouse movements and not on final decisions (Maliszewski et al., 2017). This finding again illustrates the importance of investigating the entire decision process (and not the mere final decision) as well as the potential relevance of the technique of mouse-tracking to do this. It also further highlights the fact that ingroup favoritism biases may be more intuitive and at early stages than usually assumed, at least in a social cognition.

Pros & Cons Compared to Other Dynamic Measures

Other dynamic methods of data collection exist in the psychological literature to reflect the temporal unfolding of the decision process. For instance, Electroencephalography (EEG) and Functional Magnetic Resonance Imaging (fMRI) are both techniques measuring brain activity that have been widely used in the literature on social and moral psychology (e.g., Cheng et al., 2010; Greene et al., 2001; Hirschfeld-Kroen et al., 2021; Peng et al., 2024; Schaich Borg et al., 2006; Zhan et al., 2018). These techniques remain however invasive for participants and very costly to deploy both in terms of financial support and time. Moreover, due to these drawbacks, they are often used over a limited number of participants per study, thus narrowing the generalizability of the effects as well as the statistical power one may expect. These techniques also lack at some point fine-grained resolution, either at the spatial level for EEG or at the temporal level for fMRI. In the end, what remains problematic with these techniques aiming at measuring brain activity is that—contrary to what is sometimes assumed—they do not reflect directly what is happening in the mind, and therefore involve at least some interpretation and inference.

Another technique widely used these last two decades to assess real-time cognitive processes is the eye-tracking technique (e.g., ; Tanenhaus et al., 1995). As for mouse-tracking, it offers good resolution to trace back the participants' eye movements during a computerized task. Although still a bit more invasive than mouse-tracking (as it often requires to put the participant in a non-ecological setting, with some apparatus that cannot be ignored), it is indeed easier to deploy than brain imagery techniques. However, compared

to mouse-tracking that merely requires a computer mouse, eye-tracking still requires a few equipment that cannot be easily exported out of the lab and/or to online experiments under highly standardized and controlled conditions. Moreover, participants simply cannot be unaware through eye-tracking device that we are measuring their own eye-movements, hence less spontaneity in their responses (Rivollier et al., 2020). As pointed by Rivollier et al. (2020), eye-tracking also does not allow for high controllability of eye movements, hence rendering the interpretation of signals at fine-grained level sometimes difficult. Indeed, Magnuson (2005) argues that eye movements are made of many discrete saccades and therefore need to be averaged over many trials (as producing much noise), whereas mouse-(or hand-)tracking methods provide a more direct and continuous path to the ongoing underlying cognitive processes. In the end, mouse-tracking is therefore an accessible, unexpensive, easy-to-implement (e.g., via the MouseTracker, Freeman & Ambady, 2010 or even via the *jsPsych* extension, see version 7.0, de Leeuw, 2015), and user-friendly technique to have access to real-time cognition (Rivollier et al., 2020).

Challenges About Mouse-Tracking Implementation in Morality

However, mouse-tracking implementation also implies several constraints in design. First, spatially speaking, as noticed by Magnuson (2005), the spatial position and nature of the stimuli can have a large impact on mouse movements analyses. Usually mouse-tracking tasks rely mainly on an auditive or visual stimulus, with the latter more often being a one-word or one-picture stimulus (e.g., as in Freeman & Ambady, 2009). In moral decision-making however, it is rare to have materials with such types of stimuli, given the complex nature of this context of application. This is probably the reason why mouse-tracking has only rarely been applied to such materials before (see Gautheron et al., 2023; Kiasoroush, 2023; Koop, 2013, for exceptions).

Gautheron et al. (2023) yet successfully implemented a moral material (i.e., one sentence per stimulus) within a mouse-tracking paradigm, though requiring some adaptations. Indeed, in their first study—with moral sentences displayed within a standard two-dimensional mouse-tracking interface—they found that participants' mouse trajectories

were quite significantly impacted by reading processes, as suggested by Magnuson (2005). Specifically, participants used the mouse cursor to assist reading, resulting in more noisy mouse-tracking data than required because of irrelevant signal (in terms of information about decision-making processes per se). In their second study, the authors therefore created a novel mouse-tracking paradigm, using a slider mode of response, with mouse coordinates being in this way registered only over one spatial dimension (here horizontally, see Gautheron et al., 2023, Figure 6). These adaptations in Study 2 successfully limited the impact of sentence processing on mouse-tracking data as the cursor could not be hovered over the sentences anymore (only few random oscillations were observed on some trials but were negligible in impact on analyses) (Gautheron et al., 2023).

Another constraint on mouse-tracking paradigm (more so the case for moral tasks) is the temporal constraint on response time—usually set at 400ms as initial onset time (Hehman et al., 2015). The experimenter should indeed ensure participants answered the mouse-tracker trial while they were initiating their reflection about the stimulus, and that they did not simply give their final decision in an a posteriori way (and/or remained fixed during their reflection and only moved in the last time of the trial). Therefore, the stimulus should ideally not be displayed before the start of the mouse-tracking trial, and mouse movements should be encouraged as soon as the trial starts (see Gautheron et al., 2023). Koop (2013) partially resolved this issue by proposing almost the entire trolley-like scenario beforehand, then only displaying the proposed action during the mouse-tracking task. However, as already noticed, this may still have allowed participants to reflect about the situations beforehand and give only their response in the mouse-tracking task without having to reflect much further about the moral situations. Added to the redundancy aspect of the task (and predictability for participants of the future moral conflicts), it could explain why he did not reproduce the expected effects of interest. Gautheron et al. (2023) yet provided a more satisfying solution to this problem with their proposition of a slider varying in width across time and depending on participants' mouse movements.

Finally, as noticed by Magnuson (2005), hand movements are usually more conscious and controllable compared to eye movements (as saccades) or brain areas activation. This may therefore limit the power of such indirect mouse-tracking task to grasp more automatic influences in human cognition. But as originally conceptualized by Freeman, Dale, and Farmer (2011) and Spivey and Dale (2006), the idea behind mouse-tracking task is to grasp from an early stage onwards the processes underlying decision making, as well as the deviations and evolution of the decision in real-time even if partially conscious and deliberate. Moreover, as noticed by Hehman et al.'s recommendations (2015), we can avoid mentioning explicitly the recording of the mouse trajectories in the task instructions (with debriefing about this aspect only upon study completion) and encourage people to answer as fast as possible with their coming intuition.

First Empirical Tests with the Replication of Classic Factors' Influence in Morality

Experiment 7[101]—Intent, Outcome, and Causality via Mouse-Tracking Technique

Rationale

In this experiment, we wanted to provide a first empirical test of the mouse-tracking paradigm developed by Gautheron et al. (2023), but applied to even more complex moral material. Although this initiative was already introduced by the work of Koop in 2013, it relied on presenting the whole moral scenario or context beforehand, then presenting only the crucial information under the mouse-tracking decision task (see Koop, 2013). However, this interface design has plural limitations, starting from the fact that people could already analyze and reflect about some crucial contextual information before the proposed action is displayed (and responses allowed). This could have minimized the visualization of real-time cognitive influences, even more so given the repeated-trial design (i.e., participants answered successively 29 to 38 dilemmas, depending on the study, hence a possible

[101] This experiment has been published in a peer-reviewed journal as the following reference: Gaboriaud, A., Gautheron, F., Quinton, J.-C., & Smeding, A. (2022). The Effects of Intent, Outcome, and Causality on Moral Judgments and Decision Processes. Psychologica Belgica, 62(1), 218–229. https://doi.org/10.5334/pb.1157. Note that minor modifications have been made in the following report compared to the published version to avoid redundancies and/or to make links with the other parts of the dissertation.

habituation and/or anticipation of the proposed actions) (Koop, 2013). Gautheron et al.'s (2023) mouse-tracking paradigm allows to palliate these drawbacks through an adapted dynamic and continuous slider's response mode. However, the moral materials they used (readapted from Pärnamets et al., 2015) comprises short sentences, hence this paradigm still being not tested with more complex and longer moral scenarios (as for trolley-like dilemmas that require more contextual information).

In this endeavor, we chose in the current study to replicate key influences for moral decision-making, that is, the influence of intent, outcome, and causality factors. First, the agent's intent as well as the action outcome are factors that take a preponderant place in moral reasoning[102], from childhood for the outcome and later in the moral development for intent features (Kohlberg, 1958; Martin et al., 2021; Piaget, 1932). Intent attribution process refers to the inference of the agent's internal mental states as a combination of beliefs and desires (Cushman, 2008, 2015; Cushman & Mele, 2008; Young & Saxe, 2011). Whereas considering the action outcome refers indirectly to the perception of the severity of the perpetrated harm (i.e., whether it implies some harm or no harmful consequences) (Cushman, 2008; Hirschfeld-Kroen et al., 2021; Leloup et al., 2018). This factor is also sometimes referred in the literature as the moral luck effect, with neutral or absence of harmful consequence representing the lucky condition (see Kneer & Machery, 2019; Kneer & Skoczeń, 2023; Young et al., 2010). People indeed judge more severely intentional (vs. non-intentional) acts and harmful (vs. unharmful or neutral) acts (Cushman, 2008; Leloup et al., 2018). Additionally, the agent's current beliefs regarding the situation seem to matter more when considering his or her intent than the simple desire for causing harm especially for morally wrong actions (Cushman, 2008; Cushman & Mele, 2008).

According to this literature, it would have been expected that manipulating the agent's intent and the mere presence (vs. absence) of a harmful outcome would be sufficient to observe an interaction effect between intent and outcome. That is, the strong effect of

[102] On a related note, see also the episode of the French podcast on psychology vulgarization '100g de savoirs' made in collaboration with Flora Gautheron on the topic of the 'Knobe effect' at the intersection of these two factors in morality (https://milgram.ulb.be/100g/episodes-100g/effet-knobe/).

intent on moral judgments would be even stronger when the action results in a harmful outcome (vs. neutral outcome). However, this interaction effect proved to be significant only when another factor—often confounded within the outcome factor—was explicitly manipulated: the agent's causal role in the outcome (Cushman, 2008, study 3). Often indeed, when the outcome is manipulated (e.g., neutral vs. harmful outcome), there is no explicit information regarding who or what did cause the actual outcome. Knowing that the outcome was caused by the agent or by another source—even sometimes by the victim him or herself—seems to represent another key factor in moral decision-making, and will be manipulated in a novel way in the present research.

The causality factor corresponds here to the perception of the agent's causal role in the outcome of the action (see Cushman, 2008; Cushman & Young, 2011). The outcome is either directly caused by the agent's action (almost mechanically or physically speaking) or caused by another source. As evidenced by Cushman (2008), the agent's causality takes its strongest influence on moral decisions when a judgment of punishment or blame is at stake (compared to judgments of wrongness or permissibility). In these two types of moral judgments (i.e., punishment and blame), the causality factor did impact the strength of the intent effect on the final decision (Cushman, 2008, study 3). This interaction effect was however found as weak, which questions its generalizability, especially as Cushman (2008) did not use mixed models to test it. As morality is a domain where there can be a lot of variability—both at the participant and stimulus levels—it seems highly relevant to model the random effects related to the participants and stimuli, which allows for generalization (Judd et al., 2017).

Based on his results, Cushman (2008) developed a model with two distinct processes of moral judgments. One involved in wrongness and permissibility judgments with inferences mainly about the agent's intent; the other involved in blame and punishment judgments with inferences about both the agent's intent and causality in the outcome (Cushman, 2008). The main effects of intent, outcome, and causality on moral judgments

are therefore rather well-established, while their interactive effects are less clear (see also Leloup et al., 2018).

Furthermore, as argued above, moral decisions can be conceived of as a continuous and dynamic process (Koop, 2013; Spivey & Dale, 2006). We suggest that the influences of intent, outcome, and causality would result in distinguishable dynamics in decision over the course of time. This was also the case at some point for personal vs. impersonal dilemmas, as partially evidenced by Koop in 2013 (study 2) with a mouse-tracking paradigm. As noticed above, mouse-tracking is a relevant technique to study the underlying decision process in its entirety and to give access to continuous real-time cognition through hand movements (Freeman, Ambady, and Farmer, 2011; Freeman & Ambady, 2010; Spivey & Dale, 2006). However, mouse-tracking paradigms have only rarely been used with moral scenarios—not the least because such scenarios are often lengthy and thus ill-adapted to mouse-tracking. Some features of previous studies (e.g., Gautheron et al., 2023; Leloup et al., 2018) yet suggest that a mouse-tracking paradigm can be applied to study moral scenarios, and more generally that a dynamic way to study moral cognition can be both adequate and useful (Malle, 2021).

In the present research, we therefore aimed to conceptually replicate and extend previous findings regarding the influence of intentional and causal factors on moral judgments of punishment (Cushman, 2008; Leloup et al., 2018). By adapting Leloup et al.'s procedure (2018) to suit mouse-tracking, we first tested the replicability of the main effects of intent, outcome, and causality on judgments of punishment with this new paradigm (adapted from Gautheron et al., 2023). As these main effects—and that of intent in particular—have quite systematically been found in previous research and if the present new paradigm does not substantially modify moral decisions, we should at least replicate these main effects on participants' judgments. Second, to clarify the robustness and meaning of the intent-by-causality interaction, we created three conditions for the outcome variable (i.e., neutral outcome, harmful outcome either caused or not caused by the agent) resulting from the adaptation of Leloup et al.'s (2018) French material and from the improvement of Cushman's

(2008) initial design. This feature was particularly important, as it allowed testing whether the agent's causality in the outcome or the outcome itself matters more in moral judgments of punishment, hence possibly extending Cushman's model (2008).

We expected judgments of punishment to be more severe when the agent's action was intentional (vs. non intentional), had a harmful outcome (vs. neutral), and when the outcome was caused by the agent (vs. not caused by him or her). Furthermore, we predicted that the effect of intent on judgments of punishment would depend on knowledge of the actual outcome on the one hand and of its causal origin on the other hand (i.e., interaction effects). Further extending and refining previous research (e.g., Cushman, 2008), mixed models were systematically used to better take into account the variability inherent to morality, both at the participant and stimulus levels. Finally, we relied in an exploratory and innovative vein on the mouse-tracking data to examine when exactly during the decision process the effects of intent, outcome, causality, and their interactions appeared—as mouse trajectories reflect the course of mind (see Freeman & Ambady, 2010; Spivey & Dale, 2006).

Method

For this purpose, we simplified Leloup et al.'s (2018) procedure by removing the factor of order of information presentation and by only focusing on judgments of punishment. These were indeed those—with blame judgments—that triggered to a greater extent the outcome and causality components, in comparison to judgments of permissibility and wrongness (Cushman, 2008). As Leloup et al.'s study (2018) only comprised punishment and not blame judgments, we only kept the former for direct comparison purposes, given the other changes implemented in the paradigm due to classic mouse-tracking requirements (not met in the case of moral scenarios and decisions). This study was preregistered before data collection and approved by the local ethic comittee, with preregistration form as well as other supplementary material available on the OSF page of the project: https://osf.io/3uyps/.

Power analysis. To determine the required sample size for this study, we conducted a power analysis through PANGEA (Westfall, 2016). For intent and outcome variables, we estimated power based on the effect sizes obtained by Leloup et al. (2018) and recomputed

through linear mixed models based on their data (see online supplementary material for more). We found that a power of 90% would be reached with a sample of $n = 40$. For the causality variable, based on Cushman's results (2008), a power of 80% would be reached with $n = 40$ and of 98% with $n = 80$ participants. The intent-by-causality interaction accounted for 5% of the variability in judgments of punishment (Cushman, 2008) corresponding approximatively to $R^2 = 0.05$ equivalent to $d = 0.46$. According to Westfall et al. (2014), a power of 80% for such an effect size and with a counterbalanced design is reached with $n = 32$.

We chose to fix a reference sample size of $n = 80$. As effect sizes were uncertain given the implemented changes, and following Lakens' recommendations (2014), we conducted a sequential analysis. Based on an a priori maximum sample size of 120 participants, we conducted intermediate analyses at 40 and 80 participants. If all main effects of interest were observed at $n = 40$ (with an adjusted alpha threshold at 0.017) or at $n = 80$ (adjusted alpha at 0.022), data collection would have been stopped. As p-values for the main effects were estimated far below adjusted alpha threshold with 80 participants, we stopped data collection and did not apply additional corrections.

Participants. Eighty Psychology undergraduates ($N = 69$ females) participated in exchange for course credit. Data were collected within two sessions after preregistration (i.e., March and October 2020) due to the Covid-19 crisis and subsequent lockdown in France. We therefore conducted the first intermediate analysis at the end of the first session (i.e., with 37 participants) and the second analysis on the total sample of 80 participants (four additional participants were excluded beforehand because they had already participated to the experiment during the first session).

Materials. The original materials from Leloup et al.'s article (2018), composed of 64 moral hypothetical scenarios, were adapted for the purpose of this study. The four original versions of each scenario manipulating the agent's intent (intentional vs. non-intentional) and the presence of a harmful outcome (harmful vs. neutral) were taken unaltered from Leloup et al.'s study (2018). To test the effect of causality, we created another condition

within the ‘harmful’ outcome modality: the agent’s causal role in this harmful outcome (i.e., either caused by the agent or not caused by him or her). This resulted in six (2x3) variants for each scenario (see an example translated in English in Figure 8.2). Note that some scenarios have been slightly rephrased to better suit a French sample–as participants in Leloup et al.’s (2018) research were Belgian (see the entire materials in the OSF page).

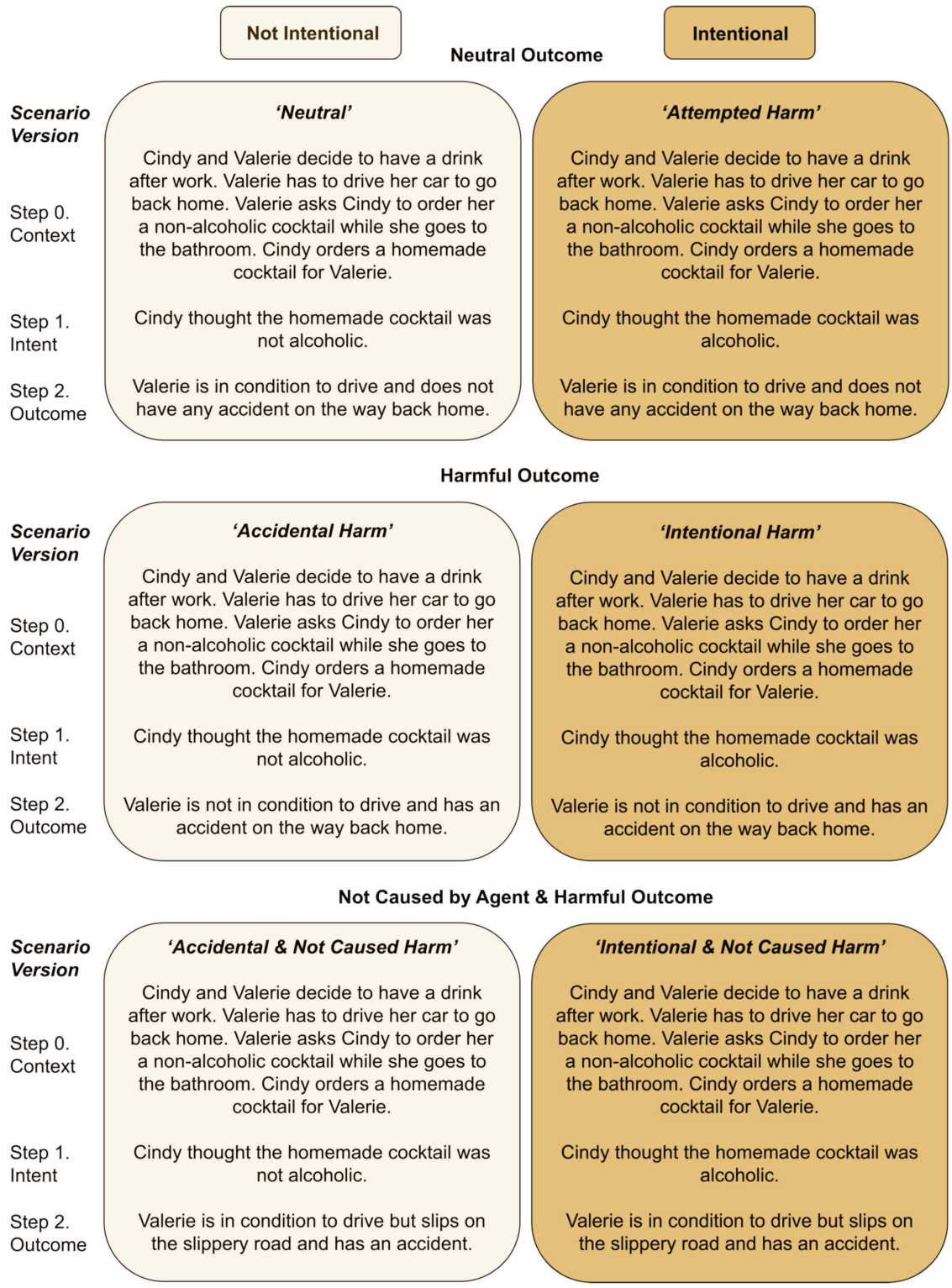
Procedure & Design. Upon arrival, participants signed a consent form and were seated in individual cubicles equipped with a computer. The procedure was mainly based on Leloup et al.’s experiment (2018), but was embedded into an adapted mouse-tracking paradigm (see Gautheron et al., 2023 for another application of the same paradigm). All responses were given with the computer mouse, calibrated so that the speed of the pointer was limited (with pointer acceleration switched off). After a brief training session to get familiar with the interface (i.e., 6 non-moral and 4 moral trials—taken from the original set of scenarios), participants went through the 60 remaining moral scenarios. Each trial (i.e., one scenario) was completed in three successive steps: first, participants only read the context of the scenario without time pressure (step 0); second, they received additional information about the agent’s intent and had to provide a first judgment of punishment targeting the agent (step 1); third, participants received final information about the action outcome and had to give a second judgment of punishment accordingly (step 2) (see Figure 8.3).

For steps 1 and 2, participants allocated a certain number of penalty tokens ranging from 0 (*less severe*) to 6 (*more severe*) to the agent, figuring the harshness of their judgments of punishment. Within a same scenario, the number of tokens was reset at the beginning of each step, so that the participant was not forced to distribute a fixed set of tokens across the two steps or across trials. The pointer position was moreover reset at the beginning of each step (back to the middle of the vertical scale). The two steps could then be considered as independent from a paradigmatic viewpoint. However, from the participant perspective, step 2 was inherently linked to step 1 as the same situation was displayed during both steps, hence a possible influence of step 1 responses on step 2 responses (see Figure 8.3 for a visual of the procedure). Participants were also required to continuously

move the computer mouse (see Hehman et al., 2015). Trials were considered as missed if the participants did not make any move with the mouse at the trial onset. Every 12 trials, a self-paced break was introduced and a short reminder of the instructions was provided.

Figure 8.2

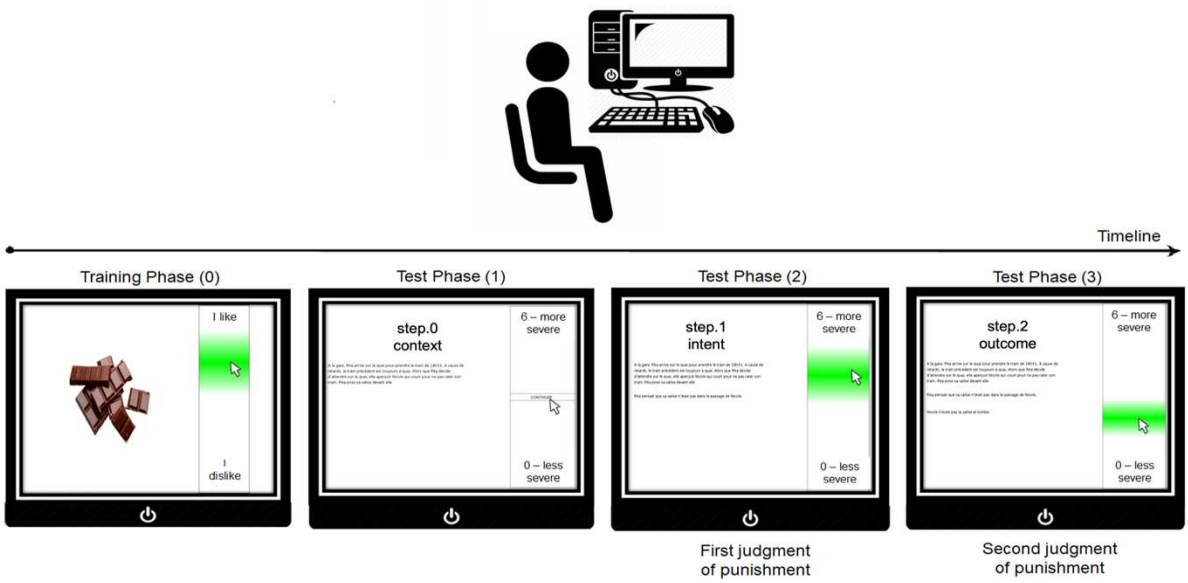
An Example of the Six Versions of One Scenario Adapted from Leloup et al. (2018)



Note. The first four versions of the scenario (i.e., Neutral, Accidental Harm, Attempted Harm, and Intentional Harm) were kept unaltered from the original material (Leloup et al., 2018). The last two were created within the same logic but adapted according to the model offered by Cushman (2008, Experiment 3).

Figure 8.3

Schematic Procedure of Exp.7 with the Adapted Mouse-Tracking Paradigm



Note. The mouse pointer was not displayed during the trials but was included here in the figure for illustrative purposes.

The study followed a within-participant and within-stimulus counterbalanced design. Participants randomly went through each of the 60 scenarios in only one of the six possible versions (i.e., 10 scenarios in each condition, randomly selected to avoid any block effect). Each participant judged a same scenario only once and processed each condition about 10 times across the various scenarios. Each scenario was also randomly presented in each of the six conditions—but each participant only saw each scenario in a single condition (i.e., counter-balanced across participants)—crossing the intent (2: intentional vs. non-intentional) and outcome (3: neutral vs. not caused by agent vs. caused by agent) variables. The main dependent variable was the judgment of punishment figured by the attributed number of

tokens for each scenario, either in step 1 (i.e., when only intent was known) or in step 2 (i.e., when both intent and outcome were known) (see Figure 8.3). Mouse movements—ordinal coordinates in the course of time—were recorded for the exploratory analyses. These Y-coordinates were rescaled from 0 to 6, in order to match the judgment response scale of the final answers.

Analytical Strategy. All analyses (i.e., confirmatory and exploratory) were conducted with *R* programming language (version 1.3, R Core Team, 2021). *lme4* package (Bates et al., 2014) and some other packages (e.g., *lmerTest*, *emmeans*, *car*) were used. Linear mixed models (LMM) were used to better account for large inter-participant and inter-stimulus variabilities, allowing at the same time the generalization of the results to both other participants and stimuli (i.e., other moral scenarios) (Judd et al., 2012). We considered two fixed factors (i.e., intent and outcome/causality) and two random factors (i.e., participant 'P' and stimulus 'S'). We recoded intent by the following contrast: [int] non-intentional (-0.5) vs. intentional (0.5); and the outcome/causality variable by Helmert contrasts: We named the first one the 'outcome' contrast—[out.O] not caused (1/3) and caused (1/3) vs. neutral (-2/3)—to analyze the effect of the mere presence of a harmful outcome. The second one is the 'causality' contrast—[out.C] not caused (-0.5) vs. caused (0.5) vs. neutral (0)—to analyze the effect of the agent's causality on moral judgments.

We then applied a procedure for reducing the random structure of the model to prevent incorrect estimates of statistics—without altering the specification of fixed effects—following the recommendations of Bates et al. (2018). With little variations in the fixed effect estimates relatively to the maximal model, we obtained the following parsimonious model whose random structure was supported by the data (in *R* equation format): Judgment ~ int*(out.O+out.C) + (int*out.C+out.O | P) + (int*out.C | S). Fixed effects were thus estimated for intent, for both contrasts of outcome, as well as their interactions, while only some of these effects were kept as random effects, in addition to all correlations between random estimates for both participants [P] and stimuli [S].

We calculated the *p*-values associated with the different effects of interest using a Satterthwaite approximation for degrees of freedom estimation. Alpha threshold was set at 0.05 to determine significance of the effects. Effect sizes are reported as Cohen's *d*, adapted to linear mixed models (Judd et al., 2017). We excluded from the analysis all missed trials and trials with a response time above 10s. Moreover, two outliers on the participant random effects were detected. They were removed for increased validity of the estimates, while controlling for type I error rate, leaving a final sample of 78 participants and 60 scenarios (see the analysis script for more, available from the OSF page).

Results

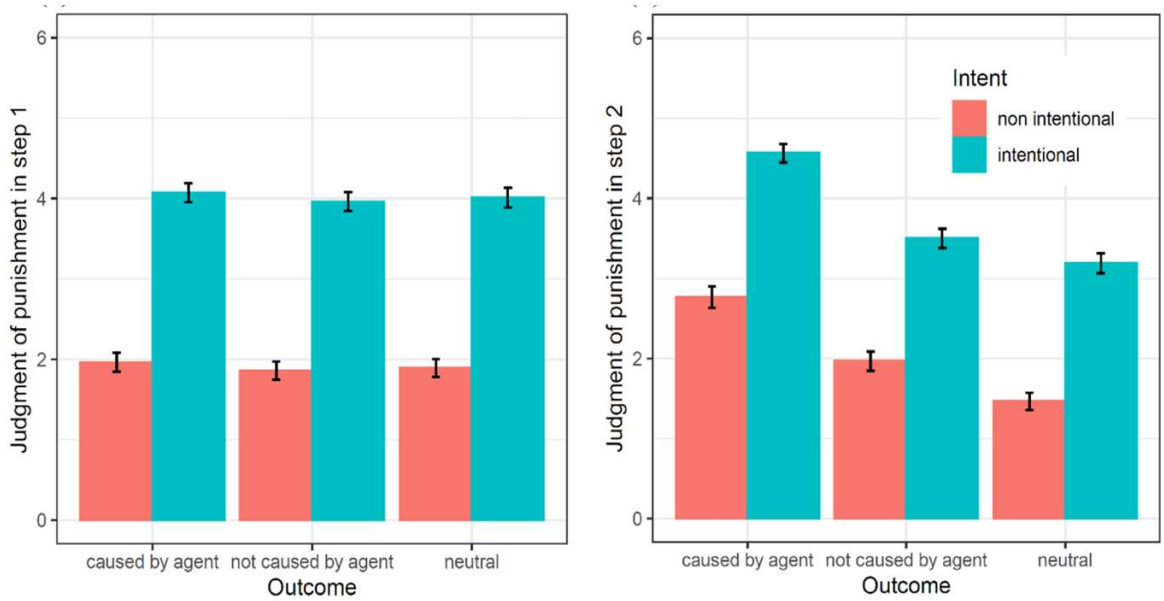
As a reminder, we expected judgments of punishment to be more severe (a) when the action was intentional (vs. non-intentional) in steps 1 and 2; (b) when there was a harmful outcome (vs. neutral outcome) in step 2; and (c) when the outcome was caused by the agent (vs. not caused by him or her) in step 2. We additionally predicted that the effect of intent on judgments of punishment in step 2 would be stronger when there was a harmful outcome (intent-by-outcome interaction) and when the outcome was caused by the agent (intent-by-causality interaction). Although confirmatory, previous research did not systematically find the intent-by-outcome interaction (Cushman, 2008; Leloup et al., 2018).

Confirmatory Results on Final Judgments (Part A). As predicted, results from the mixed model analysis indicated in step 1 (i.e., with only intent information being available) a significant effect of intent, $b = 2.11$, $SE = 0.17$, $t(88.99) = 12.12$, $p < .001$, 95% CI [1.76, 2.46], $d = 1.40$, with people judging more severely the agent when he or she intentionally committed the wrong action ($M = 4.02$, $SE = 0.12$) rather than non-intentionally did so ($M = 1.91$, $SE = 0.11$) (see Figure 8.4, left plot). We also found an unexpected effect (but of lower magnitude) of the agent's causality in the outcome, $b = 0.11$, $SE = 0.05$, $t(55.78) = 2.29$, $p = .03$, [0.01, 0.20], $d = 0.07$, with judgments of punishment being slightly more severe when the outcome was caused by the agent ($M = 3.02$, $SE = 0.08$) rather than caused by another source ($M = 2.91$, $SE = 0.07$). This effect was quite surprising given that the participant was still unaware of the action outcome in step 1. It was however a posteriori explained as the

result of contingencies through the sequence of scenarios. Indeed, despite our randomization efforts across participants and trials, there was a weak non-significant dependency between causality on subsequent trials (*OR* = 0.87, *SE* = 0.09, *z* = -1.34, *p* = .18). This made it possible for the participants to pick up and exploit such contingency between step 2 of one trial and step 1 of the next trial (see the analytic code for more). No other main or interaction effect was found at this step.

Figure 8.4

Results in Exp.7 (Part A) on Final Judgments of Punishment in Step 1 (Left) and in Step 2 (Right) Depending on Intent and Outcome/Causality Conditions



Note. Error bars represent *SE*.

In step 2, when information of both intent and outcome was available, a significant effect for intent was again found, *b* = 1.68, *SE* = 0.16, *t*(83.25) = 10.60, *p* < .001, [1.37, 2.00], *d* = 1.05, with judgments of punishment being more severe when the action was intentional (*M* = 3.75, *SE* = 0.10) rather than non-intentional (*M* = 2.07, *SE* = 0.10) (see Figure 8.4, right plot). Moreover, as expected in this step, a significant effect was observed for the presence of a harmful outcome—be it caused or not caused by the agent—, *b* = 0.87, *SE* = 0.08, *t*(76.23) = 10.76, *p* < .001, [0.71, 1.03], *d* = 0.54, with moral judgments being more severe when there was a harmful outcome (*M* = 3.20, *SE* = 0.07) compared with a

neutral outcome (M = 2.33, SE = 0.08). We additionally found a significant effect for causality, b = 0.93, SE = 0.10, t(79.25) = 9.56, p < .001, 95% CI [0.74, 1.13], d = 0.58. The agent was indeed punished more severely when his or her action directly caused the outcome (M = 3.67, SE = 0.09) compared to when the outcome was caused by another source (M = 2.73, SE = 0.08) (see Figure 8.4, right plot).

Contrary to expectations, there was no significant intent-by-outcome interaction, b = -0.06, SE = 0.08, t(3782.78) = -0.71, p = .48, [-0.22, 0.10], d = -0.02. However, aligned with expectations, a significant interaction between intent and causality was observed, b = 0.26, SE = 0.11, t(65.65) = 2.34, p = .02, [0.04, 0.49], d = 0.08. The effect of intent on judgments of punishment did depend on the agent's causality in the outcome. The intent effect became even stronger (+0.26) as the outcome was caused (vs. not caused) by the agent (see Figure 8.4, right plot). Paired comparisons showed that when the action was caused by the agent, people judged more severely intentional acts (M = 4.56, SE = 0.12) than non-intentional ones (M = 2.77, SE = 0.13), b = 1.80, SE = 0.16, t(89.90) = 10.99, p < .001, [1.47, 2.12]. Similarly, when the outcome was not caused by the agent, participants punished significantly more severely the agent when he or she intentionally acted (M = 3.50, SE = 0.12) compared to scenarios when he or she non-intentionally acted (M = 1.97, SE = 0.12), b = 1.53, SE = 0.18, t(82.71) = 8.61, p < .001, [1.18, 1.89].

Exploratory Results on Mouse Trajectories (Part B). For exploratory analyses on the mouse-tracking trajectories, we rescaled Y-coordinates between 0 and 6, so that they directly reflect the severity of judgments (see Figure 8.5). Although coordinates alone are not sufficient to infer the aimed judgment during the decision process, we expected the following patterns with (a) a strictly positive effect of intent during the time course of step 1 and from an early stage of step 2—as step 1 should serve as a prime for the next step; (b) at the end of step 2, lower values for intent effect compared to the beginning–as the influence of intent should be weighted down by the outcome and causality information; (c) strictly positive effects of outcome and causality in step 2; (d) at the end of step 2, a larger effect of intent

compared to both outcome and causality effects (respecting the differences in raw and standardized effect sizes observed on final judgments).

To explore those patterns, we selected 93% of the mouse trajectories, excluding those which did not meet standard mouse-tracking assumptions or led to violations of statistical assumptions (see analysis script for more, accessible from the OSF page). Mixed models were applied on rescaled Y-coordinates for each step, using the same parsimonious model as for the analysis on final responses. To control for multiple testing through timesteps, analyses were performed on splines coefficients, each focusing on a portion of time-normalized trajectories.[103] Time therefore runs between 0% when clicking on the START button to 100% when clicking on the final response. Apart from small deviations due to the integration of information through time and to the type of functional statistics used for analyzing trajectories, results at 100% of trajectory time should match those reported in confirmatory analyses on final judgments.

These analyses revealed that the effect of intent became significant at 55% of step 1, $b = 0.11$, $SE = 0.05$, $t(55.02) = 2.05$, $p = .04$, 95% CI [0.003, 0.21], $d = 0.07$. It became stronger during the time course, until reaching its maximal effect size at the end of step 1, $b = 2.05$, $SE = 0.17$, $t(88.88) = 11.99$, $p < .001$, [1.71, 2.39], $d = 1.39$ (see Figure 8.5, left plot). In step 2 however, the intent effect was significant from the beginning of the trial (i.e., earlier than 5% of the whole trajectory duration), $b = 0.009$, $SE = 0.003$, $t(63.46) = 2.59$, $p = .012$, [0.002, 0.02], $d = 0.10$; and became stronger until the end, $b = 1.62$, $SE = 0.16$, $t(82.13) = 10.30$, $p < .001$, [1.31, 1.93], $d = 1.03$ (see Figure 8.5, right plot).

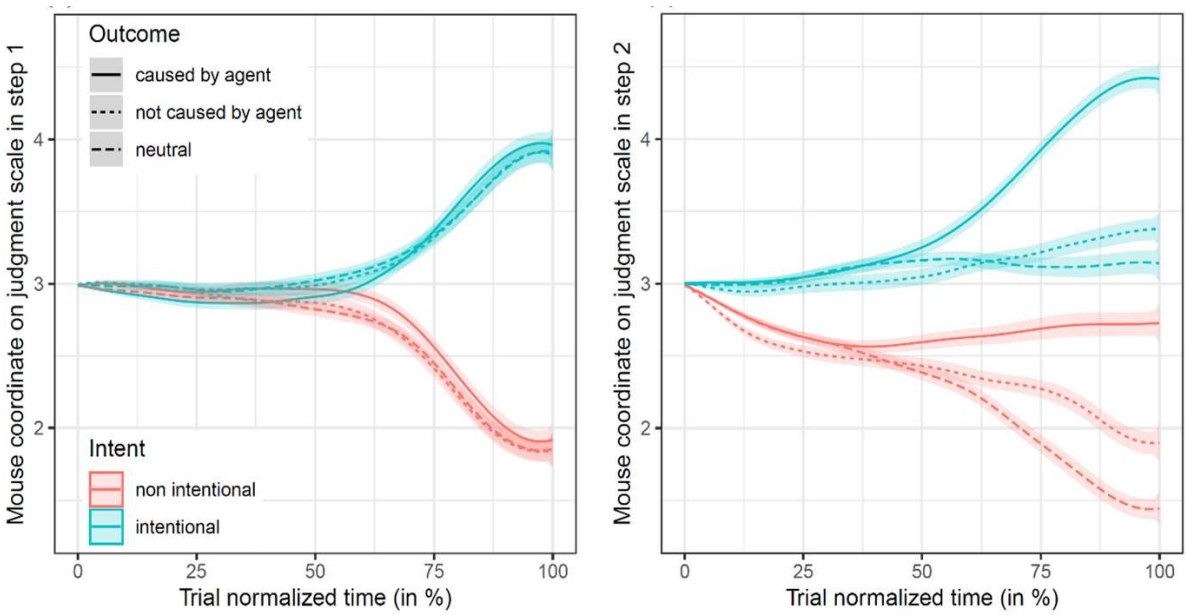
The presence of a harmful outcome had no significant effect in step 1 (as expected), while it became significant at 60% of the time course in step 2, $b = 0.16$, $SE = 0.05$, $t(85.06) = 3.22$, $p = .002$, [0.06, 0.26], $d = 0.12$; and became stronger until the end, $b = 0.81$, $SE = 0.08$, $t(74.97) = 10.44$, $p < .001$, [0.66, 0.97], $d = 0.51$ (see Figure 8.5, right plot). The causality effect was found as significant in step 1 for only a brief part of the trajectory,

[103] Splines are similar to timebins, but respect the functional nature of the mouse-tracking data (i.e., continuity of hand movements) and estimate effects as a function of time (i.e., mean effect trajectories and associated *SE*, as represented on Figure 8.5).

between 75% and 80%, $b < 0.12$, $SE > 0.05$, $t < 2.63$, $p > .010$, $d < 0.08$ (see Figure 8.5, left plot); thus confirming the weakness of this effect found in confirmatory analysis for final judgments in step 1. In step 2 however, the causality effect became significant at 20% of normalized time, earlier than the outcome effect, $b = 0.08$, $SE = 0.03$, $t(67.36) = 2.53$, $p = .01$, [0.02, 0.14], $d = 0.12$, and increased until the end of the trial, $b = 0.93$, $SE = 0.10$, $t(80.00) = 9.28$, $p < .001$, [0.73, 1.13], $d = 0.59$. The intent-by-causality interaction became significant later in the decision process, at 70%, $b = 0.26$, $SE = 0.11$, $t(65.46) = 2.42$, $p = .02$, [0.04, 0.47], $d = 0.09$. Its effect increased until 80% of the whole trajectory duration with $b = 0.36$, $SE = 0.11$, $t(55.18) = 3.17$, $p = .003$, [0.13, 0.60], $d = 0.12$; and was then attenuated until the end of the trial, $b = 0.21$, $SE = 0.11$, $t(55.99) = 1.89$, $p = .06$, [-0.01, 0.43], $d = 0.07$ (see Figure 8.5, right plot).

Figure 8.5

Mouse Trajectory Results in Exp.7 (Part B) in Step 1(Left) and in Step 2 (Right) with Intent, Outcome, Causality, and their Interactions Depending on the Time Course



Note. Y-coordinates were rescaled between (0) *less severe* and (6) *more severe*, so that results at the end of trajectories match the judgment scale of Figure 8.4. Error bars represent *SE*.

Discussion & Conclusion

The present study was designed to conceptually replicate and extend previous findings about intent, outcome, and causality influences on moral judgments of punishment (Cushman, 2008; Leloup et al., 2018) through a novel use of mouse-tracking paradigms, which has so far only rarely been used to explore moral decision-making (Gautheron et al., 2023; Koop, 2013). Findings support the main hypotheses, namely that people judge the agent more severely when the action is intentional (vs. non-intentional), when it induces a harmful outcome (vs. a neutral outcome), and when this outcome is caused by the agent (vs. not caused by him or her). These main effects are consistent with previous findings (Cushman, 2008; Leloup et al., 2018) and allow to reaffirm those variables as key factors for moral decision-making.

Regarding interaction effects, we did not find a significant interaction between intent and outcome factors in step 2, but the expected interaction between intent and causality factors. Together, these findings are consistent with previous results that did not systematically find the intent-by-outcome interaction (Cushman, 2008; Leloup et al., 2018) but showed a significant intent-by-causality effect (Cushman, 2008). The present results support Cushman's hypothesis (2008) that the causal attributional process influences the intentional one, at least for judgments of punishment, whereas the mere presence of a harmful outcome does not. Extending Cushman's model of moral judgment (2008), the causality of the outcome seems to matter more for the final decision of punishment than the outcome itself. This finding should thus be integrated to Cushman's two process model of moral judgment (2008) with a clear distinction between outcome and causality concerns. Future research should test whether the same interaction effect is to be found with judgments of blame, which are hypothesized to trigger the same reasoning process than judgments of punishment in Cushman's model (2008).

Beyond the contribution related to findings on final judgments, the exploratory analyses on mouse trajectories evidenced that intent, outcome, and causality did not influence the decision process at the same time or with the same magnitude. As the intent

information appeared first in the current paradigm, there was first a predominant importance of this factor whereas factors of outcome and causality weighed only later in the trial (from the middle of step 2). In addition, the three main effects took more and more importance in the decision during the time course (stronger effects as the time goes by). The intent effect appeared very early in step 2, as if its high importance in the end of step 1 served as a prime in the next step. Additionally, intent weighed to a lesser extent at the end of step 2 compared to the end of step 1, a finding that is consistent with the competition created by the outcome and causality factors, which reduced its influence. Finally, at the end of step 2, intent weighed to a greater extent compared to causality and outcome factors. It supports the idea, with a different measure than previously investigated (i.e., an adapted mouse-tracking paradigm), that intent explains a larger part of variance than outcome or causality in judgments of punishment (Cushman, 2008).We should however note that the comparison between step 1 and step 2 judgments and dynamic processes is quite tricky as the amount of information—and thus the difficulty of the decision—were not identical throughout the two steps, and what we measured was thus necessarily different between step 1 and step 2. Step 1 judgments with only intent being manipulated could sometimes elicit some uncertainty as the outcome part was not already displayed. But it does not seem to have impeded participants' understanding of the situations, as they had to judge sequentially multiple scenarios with the same structure and could expect as soon as step 1 was displayed that a possible consequence was going to happen in step 2.

Furthermore, having performed two-random-variables mixed model to analyze these effects allows their generalization to both other participants and other moral stimuli, extending this way the scope of previous work. Nonetheless, we should admit that the currently targeted population (i.e., undergraduate students) was not representative of the general population, thus nevertheless somewhat restricting the generalization of the present results. We could however easily reproduce this experiment with a general population, as the current mouse-tracking paradigm is very intuitive, user-friendly, not expensive, and easy to implement on a large scale.

Beyond getting a first grasp of the temporal influence of intent, outcome, and causality factors during moral decision-making, the present research provides evidence for the applicability of a mouse-tracking paradigm to the moral domain. Having found similar results as Cushman (2008, Study 3) and Leloup et al. (2018, Study 1) for intent, outcome, and causality main and interaction effects supports the idea that the current mouse-tracking paradigm did not significantly alter these expected effects. Embedding lengthy moral scenarios into a mouse-tracking paradigm is not trivial though, as reading entire sentences is ill-suited for typical mouse-tracking studies (e.g., Freeman & Ambady, 2010). This may explain why, despite being a promising tool to investigate moral decisions, mouse-tracking has been so far very rarely used in this domain (see Koop, 2013 for an exception). The present mouse-tracking paradigm (adapted from Gautheron et al., 2023) seems however suitable for this endeavor, as hypothesized effects were replicated and further specified in terms of their temporality during the decision process. Even if the current exploratory results on mouse-tracking data would need to be replicated in future research in a confirmatory vein, we argue in favor of harnessing the potential of this tool to have a more in-depth understanding of the complexity of human moral decision-making.

Experiment 8[104]—Intent, Outcome/Causality, and Type of Judgment via Mouse-Tracking Technique

Rationale

In this experiment, we wanted to further test the previously proposed mouse-tracking paradigm, while again manipulating intent and outcome/causality as in the previous design. We however tested these effects in the current experiment on different types of moral judgment, that is, wrongness and blame judgments to further investigate the impact it could

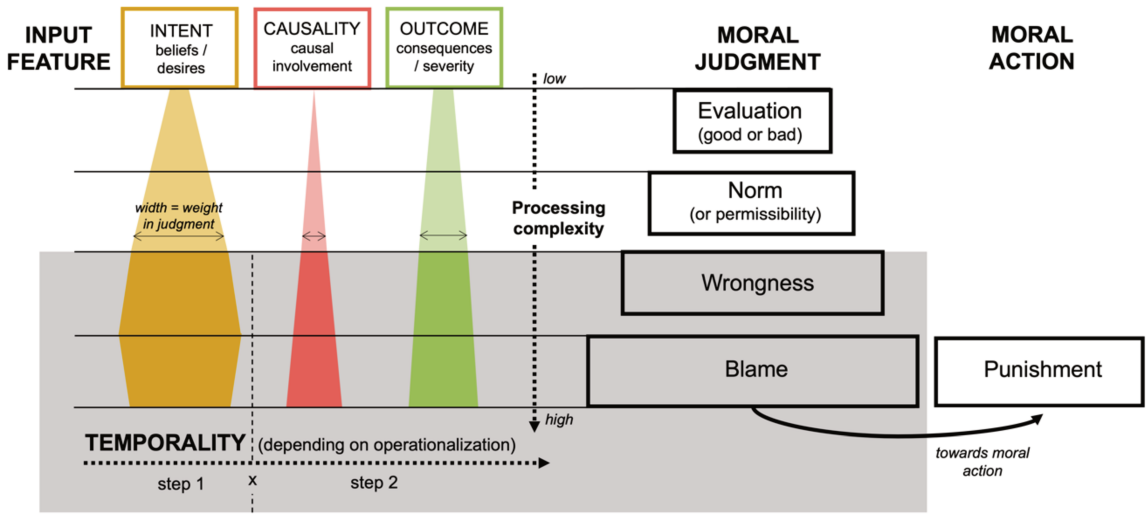
[104] This experiment was published in a peer-reviewed journal as: Gaboriaud, A., Gautheron, F., Quinton, J.-C., & Smeding, A. (2024). Wrongness and blame judgments and their dynamics: Toward a three-input processing model of moral judgment. *International Review of Social Psychology*, *37*(1). https://doi.org/10.5334/irsp.868. Note that some modifications have been made in the following report compared to the published version (mostly cut-off) to avoid redundancies. You can however find the extensive version of this experiment in the published version.

have on moral decision-making as findings are still ambiguous on that point (see in particular Cushman, 2008; Kneer & Machery, 2019; Malle, 2021).

To further refine these findings and better integrate the dynamic aspect of moral decisions, we introduce and illustrate in Figure 8.6 a three-input processing model of moral judgment, inspired by both Malle's (2021) and Cushman's (2008) approaches. On one hand, Cushman's model (2008) does not integrate all types of judgment described by Malle and does not qualify the weight of each feature (i.e., intent or causal information) depending on judgment types and their underlying processing complexity (i.e., how much information is considered). On the other hand, Malle's (2021) classification does not take into account the type of information they trigger. Furthermore, none of these frameworks considers judgment dynamics, that is, the temporal integration of intent and causal information as investigated in Experiment 7.

Figure 8.6

Three-Input Processing Model of Moral Judgment as Developed in Exp.8



Note. This three-input processing model is meant as an integration of Cushman's (2008) and Malle's (2021) models of moral judgment and is represented here with its operationalization in the current study. The right side of the figure contains the various judgment types classified depending on their respective processing complexity. On the left side of the figure, the weight of input features is displayed depending on the type of moral decision process

involved, which may change when navigating through the hierarchy of judgments and actions (varying width). Light colors represent input weights that have not been tested in the current research whereas dark-colored areas stand for weights of input features that are currently tested in the present research (on wrongness and blame judgments only). The grey area with the temporal line corresponds to the current study operationalization with a specified order of presentation for the various features (i.e., intent is presented first, then outcome and causality in a second step).

The present three-input processing model of moral judgment therefore combines Malle's classification along with intent and causal information as inputs for each judgment type as in Cushman (2008). It further differentiates causal information between outcome and causality features, along with input weights as a function of judgment type and their dynamics. We use the *three-input processing* designation for this model to reflect the three inputs considered (i.e., intent, causality, and outcome). Another key precision is that we do neither pretend via the current model to identify the various neural processes at play, nor to demonstrate the existence of three competitive processes (one for each input) in Cushman's meaning of a process (2008), but more to tackle the intertwining (and possible competition) to integrate several information inputs in play during moral judgments.

The present research therefore investigates the influence of the agent's intent (taking only the belief component from Cushman's conceptualization in 2008, as demonstrated to be of main importance compared to the desire component), the outcome (i.e., the mere presence of a harmful consequence), and the causality (i.e., whether the agent is causally responsible for the consequence to occur) inputs on wrongness and blame final judgments and underlying dynamics. Indeed, previous findings from Experiment 7 were consistent with this model for punishment judgments and dynamics (see previous section), hence the new focus on blame. In addition, wrongness judgments are more frequently investigated compared to permissibility judgments, in particular by Cushman (2008) when studying the intervention of causality on this type of moral judgment (study 3), and by Leloup et al. (2018,

study 2) when comparing the effects of type of judgment order. Blame and wrongness judgments were therefore the best candidates for the current study. Given the controversies in the literature about their differentiation (Cushman, 2008; Kneer & Skoczeń, 2023; Malle et al., 2014), it allows here to examine which and when features are processed during both types of judgments.

This experiment also aims to provide a first empirical test of the proposed three-input processing model of moral judgment illustrated in Figure 8.6, integrating both Cushman's (2008) and Malle's (2021) frameworks. First, we will test the influence of intent, outcome, causality, and their interaction effects on wrongness and blame judgments. Leloup et al. (2018) partly tested these effects for punishment and wrongness judgments but did not manipulate causality. The previous Experiment 7 did manipulate causal information—distinguishing cause and outcome (as in Cushman, 2008, study 3)—but only focused on judgments of punishment.

As in the previous experiment, we traced decision processes underlying final judgments using a mouse-tracking tool to measure real-time cognition (see Gautheron et al., 2023, for the original paradigm). Tracing underlying dynamics of wrongness and blame judgments is particularly relevant as highlighted by Malle when suggesting 'tracking [of] the time course and accuracy of various ensuing causal and mental inferences, as well as [of] blame itself' (2021, p. 302). Mouse-tracking can be used in this endeavor, as it allows examining when, in time, the different inputs start and finish exerting their influence during the decision process, before the final judgment is made. Although mouse-tracking has been widely used in social perception (e.g., Freeman, Ambady, et al., 2011; Freeman & Ambady, 2009; Smeding et al., 2016), existing evidence is still scarce for moral scenarios (see Gautheron et al., 2023; and Koop, 2013, for exceptions). The current experiment will therefore either bolster or not (regarding Experiment 7 findings' replication) the usefulness of mouse-tracking as a tool to study moral cognition in real-time.

Regarding confirmatory hypotheses on final judgments, we expected as preregistered main effects of intent, outcome, and causality, as well as the intent-by-

causality interaction, especially for blame judgments. An intent-by-type of judgment interaction effect was hypothesized, with intent having a stronger influence on judgments of wrongness compared to blame (following the proposed model). Interaction effects between type of judgment and outcome/causality were also expected, with a stronger influence of outcome and causality for judgments of blame compared to judgments of wrongness as illustrated in the model (Figure 8.6).

Additional exploratory hypotheses were preregistered for judgment dynamics (mouse-tracking data), to replicate, but also to extend previous findings with this methodology (see Experiment 7). When only intent information was available, we merely expected a growing influence of intent from the middle of the trajectory until its end. When intent, causality, and outcome information were all available, we expected an early influence of intent, and a growing influence of outcome and causality during the time course. The effect of intent should, however, remain strong until the end of the process, compared to outcome and causality (following the larger effect size for intent observed on final judgments in previous research; see Cushman, 2008; Leloup et al., 2018). Although not preregistered, we additionally expected that interaction effects of intent, outcome, and causality factors with type of judgment would be observed concomitantly with the corresponding main effects: from the middle of the trajectory onwards for the intent-by-type of judgment interaction effect when only intent information was available and in a very early stage effect when all information was available; from the middle of the trajectory onwards for outcome and causality-by-type of judgment interaction effects.

Method

We used the same mouse-tracking paradigm and same material as in Experiment 7 to assess the use of mouse-tracking as a suitable tool to study moral decision-making. We manipulated in this study again the intent, outcome, and causality features, as well as the type of judgment at stake (wrongness vs. blame), in extension and partial replication of the previous results. This study was preregistered before data collection. All supplemental

materials and data as well as the preregistration form are available on the OSF project page: https://osf.io/675gm/.

Power Analysis. To determine the required sample size for this study, we ran a power analysis based on the effect sizes and variance partitioning coefficients (VPCs) obtained from a previous experiment with a similar design (see Experiment 7). We ran this power analysis with the PANGEA online application (version 0.2, Westfall, 2016), considering a database of 60 stimuli. Considering three fixed factors (i.e., intent, outcome/causality as one single factor, and type of judgment) and two random factors (i.e., participant and scenario), we needed n = 120 participants for the lowest effect size of interest to be detected (i.e., intent-by-causality interaction, d = 0.08) with a statistical power of 83%.

For the interaction effects with type of judgment, we could not use information from Cushman (2008) (see further developments provided in the supplemental online materials [SOM]). Therefore, aiming at a power of 80% in a counter-balanced design with 60 stimuli and with VPCs by default, we estimated we could reach a smallest effect size of interest (SESOI) of d = 0.28 with 120 participants (calculated with 'Power Analysis with Crossed Random Effects' application, see Westfall et al., 2014). Following Lakens' recommendations (2014), we planned to conduct a sequential analysis. Based on a maximum a priori sample size of 144 participants (i.e., the closest multiple of 48, to guarantee a sufficient sample size for the first interim analysis, and counterbalancing across participants and scenarios), we conducted intermediate analyses at 48 and 96 participants, using a linear spending function (with a general alpha threshold of 0.05). As not all effects of interest were found at these stages, we pursued data collection until reaching n = 144.

Population. We recruited 145 Psychology undergraduates (131 females, aged from 17 to 40, M = 19.97, SD = 2.83) who participated in exchange for course credit (one participant was a posteriori removed, see below). Participants provided informed consent to participation. The research was conducted in accordance with the ethical guidelines of the American Psychological Association and was approved by the local ethics committee.

Materials. Moral scenarios were again those from Leloup et al.'s research (2018). We kept from the previous adaptation in Experiment 7 the manipulation of the agent's causal involvement in the outcome (see Figure 8.7 for an illustrative example, translated in English). To maximize comparison with previous experiment and not adding supplementary variability, all judgments were again made on a vertical continuous seven-point scale. For judgments of wrongness, it was asked 'How wrong was the agent's behavior?' anchored from (0) *Not wrong at all* to (6) *Very wrong*. For judgments of blame, it was asked 'How much blame does the agent deserve?' anchored from (0) *Not blamable at all* to (6) *Very blamable*. These scales were taken from Cushman (2008) but adapted to mouse-tracking. Computer mouse movements over time were recorded as Y-coordinates (on the vertical axis), along with final judgments. These Y-coordinates were rescaled from 0 to 6 to match the judgment response scale. All trajectories were time-normalized from 0% to 100% and trajectories were then decomposed into 20 timebins (Hehman et al., 2015).

Design. This study followed a variant of a counterbalanced design (i.e., within-participant and within-stimulus) with three fixed factors (i.e., Intent, Outcome/Causality, and Type of Judgment) and two random factors (i.e., Participant and Scenario). The intent variable was compounded with two modalities: Non-Intentional vs. Intentional, whereas the outcome and causality variables were unified in one single variable defined by a Helmert contrast: Not Caused Harm vs. Caused Harm vs. Neutral Outcome. Finally, the type of judgment (also manipulated within participants and within stimuli) included two modalities: Blame vs. Wrongness (see supplementary material for more).

Procedure. Data were collected in France, in a lab environment, and in individual cubicles. After having provided their consent and having filled out demographic information, participants performed four training scenarios. They responded to all scenarios with the computer mouse. We followed the same procedure as in Experiment 7, with moral scenarios embedded within a mouse-tracking paradigm (see Figure 8.8 for a reminder). However, in the current experiment, participants made judgments of blame or wrongness, depending on the experimental block. The type of block (blame or wrongness) was counterbalanced within

participants, for a total of six blocks of ten scenarios per participant. A self-paced break was introduced after each block, which ended with an attention check item (five in total). Participants were then thoroughly debriefed, given their course credit, and thanked.

Figure 8.7

An Example of the Six Versions of One Scenario Adapted from Leloup et al. (2018)

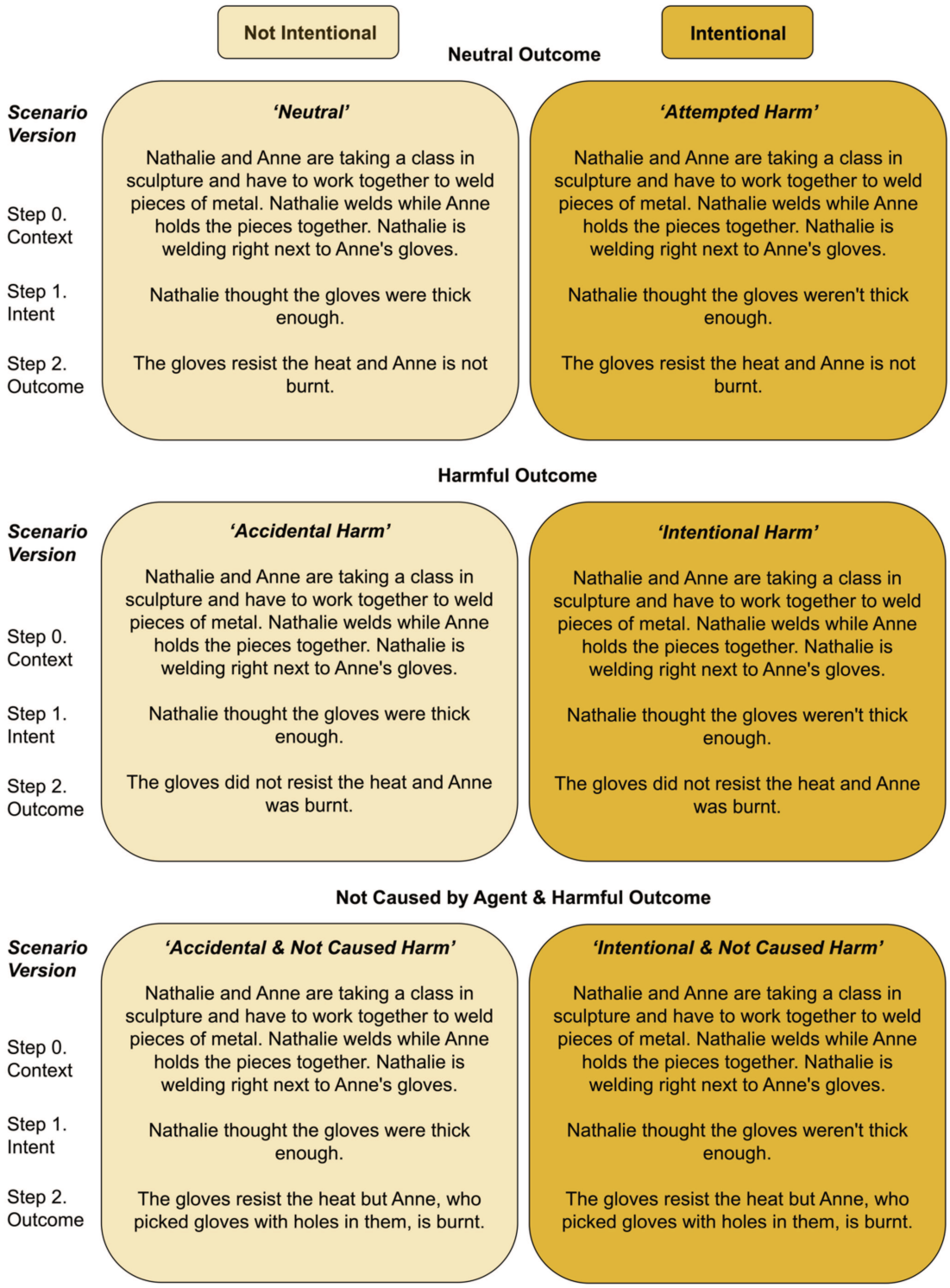
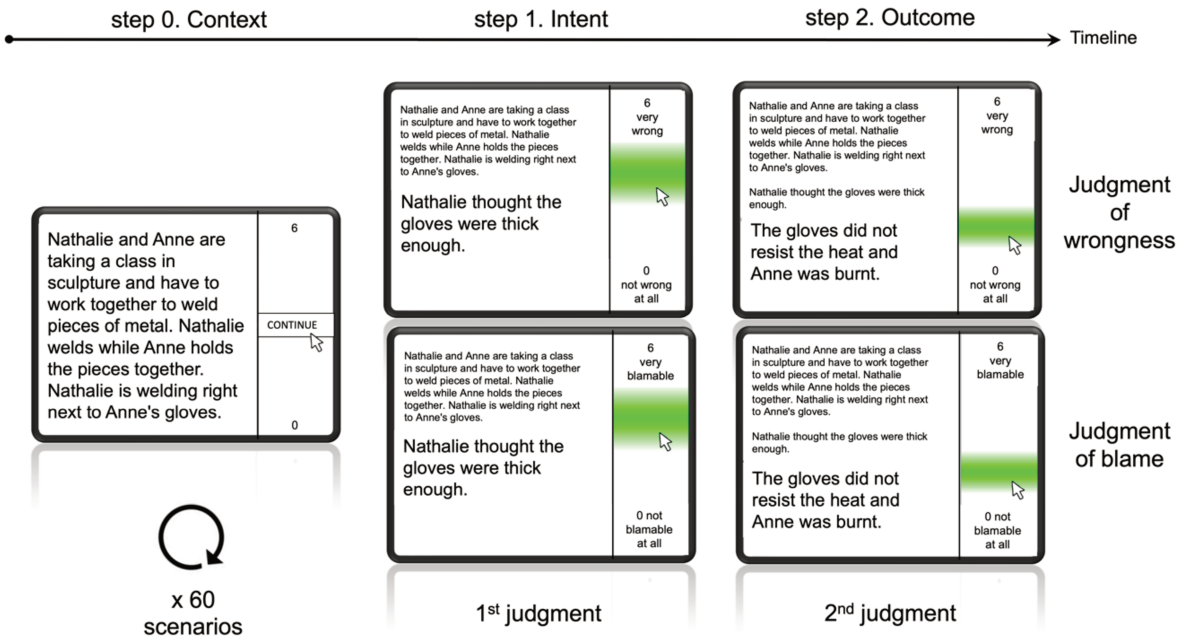


Figure 8.8

Procedure Followed in Exp.8 Including Mouse-Tracking Paradigm and Type of Judgment



Analytical Strategy. Confirmatory and exploratory analyses were conducted with R programming language, lme4 package (Bates et al., 2014), and some other packages (e.g., lmerTest, emmeans, car). Linear mixed models (LMM) were used to generalize results to other participants and moral stimuli (Judd et al., 2012). The intent variable was coded by the following contrast: (-0.5) Non-Intentional vs. (+0.5) Intentional. The outcome and causality variables were unified in one single variable defined by a Helmert contrast. The first one tested the mere presence of a harmful versus neutral outcome; the second contrast tested the agent's causal involvement in the outcome: Not Caused by Agent (+1/3 or -0.5) vs. Caused by Agent (+1/3 or +0.5) vs. Neutral Outcome (-2/3 or 0). Finally, type of judgment (also manipulated within participant and within stimulus) included two modalities: (-0.5) Wrongness vs. (+ 0.5) Blame. Final mixed model (after reduction), trial and participant exclusions are fully reported in the SOM for both confirmatory and exploratory analyses. The final sample comprised 144 participants (one participant was excluded because of deviant values in random effects) and the sample of retained trajectories for mouse-tracking analyses was 95% in step 1 and 96% in step 2.

Results

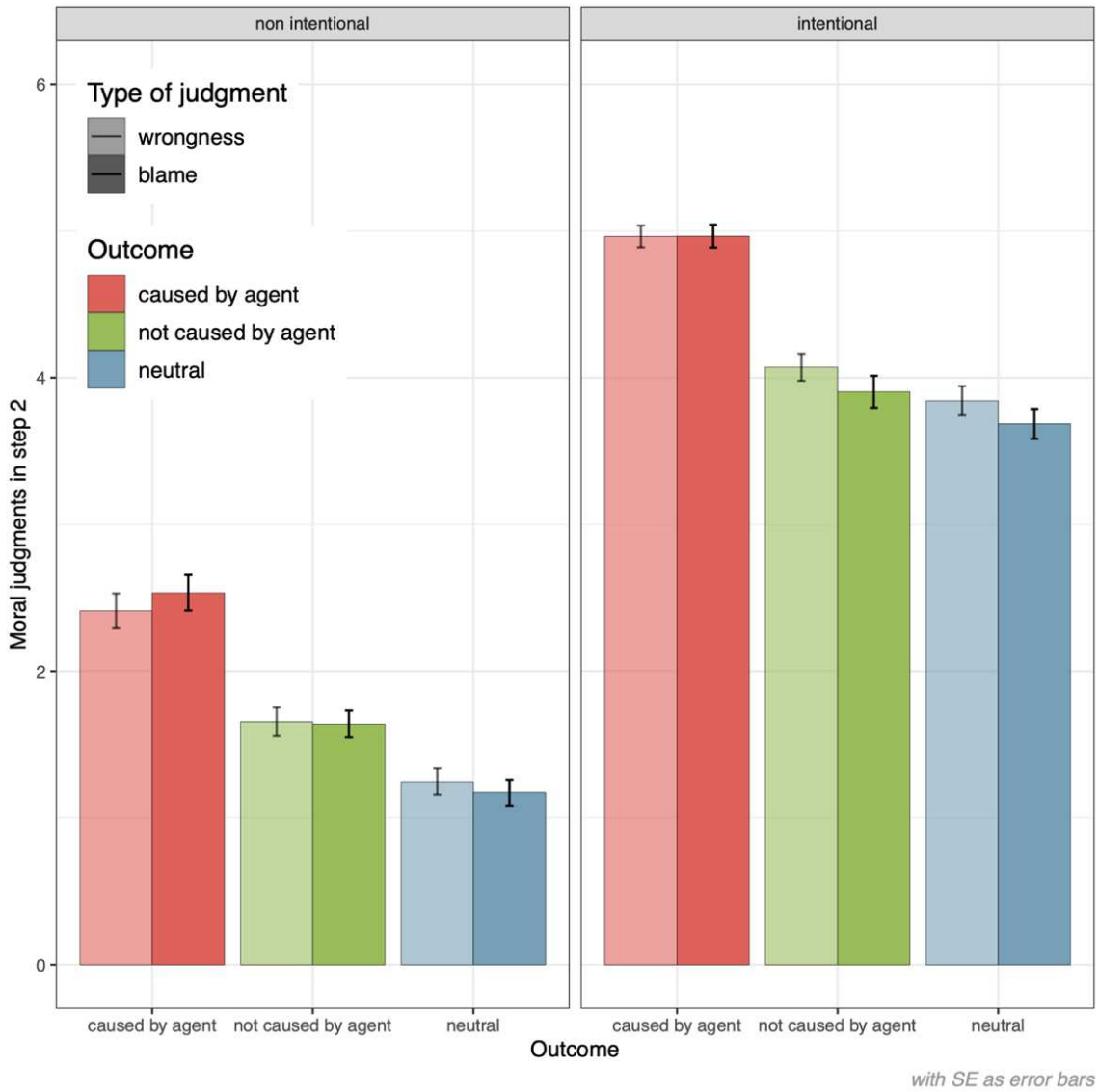
As a reminder, we mainly expected in the current experiment to replicate main and interaction effects for intent, outcome, and causality as they were found in the previous experiment. Moreover, we expected intent and outcome influence to interact with type of judgment, consistently with the model of moral judgment proposed in introduction (see Figure 8.6).

Confirmatory Results on Final Judgments (Part A). Findings in step 1 showed a main effect of intent, b = 2.94, SE = 0.12, t(180.69) = 24.90, p < .001, 95% CI [2.71,3.18], d = 2.06, with more severe judgments when the agent intentionally perpetrated the wrong action (M = 4.44, SE = 0.08) than when he or she did not (M = 1.49, SE = 0.09). Although predicted, we did not find the expected interaction between intent and type of judgment at this step (b = -0.07, SE = 0.08, t(93.34) = -0.89, p = .375, [-0.22,0.09], d = -0.02). Descriptively, however, means were in the expected direction, with a slightly stronger effect of intent for wrongness (b = 2.98) compared to blame judgments (b = 2.91). No other main or interaction effects were observed.

In step 2, findings showed the expected main effect of intent, b = 2.46, SE = 0.10, t(176.68) = 23.65, p < .001, [2.26,2.67], d = 1.68 (see Figure 8.9). We also found a main effect of outcome, b = 0.78, SE = 0.07, t(145.52) = 11.67, p < .001, [0.65,0.91], d = 0.53, with more severe judgments when there was a harmful (M = 3.27, SE = 0.06) rather than a neutral outcome (M = 2.49, SE = 0.07). The main effect of causality was also observed, b = 0.90, SE = 0.07, t(137.37) = 13.26, p < .001, [0.77,1.04], d = 0.61, with more severe judgments when the agent directly caused the outcome (M = 3.72, SE = 0.07) compared to when the outcome was caused by another source (M = 2.82, SE = 0.07) (see Figure 8.9). The main effect of type of judgment did not reach the retained level of significance, b = -0.05, SE = 0.03, t(7597.60) = -1.89, p = .059, [-0.10,0.002], d = -0.03, although descriptively, means went again in the direction of slightly more severe wrongness (M = 3.03, SE = 0.06) than blame (M = 2.98, SE = 0.06) judgments.

Figure 8.9

Results in Exp.8 (Part A) on Final Moral Judgments (in Step 2) Depending on Intent and Outcome/Causality Condition, and Type of Judgment



Note. Moral judgments are displayed from less severe (0) to more severe (6) judgments.

Regarding two-way interactions, an unexpected interaction effect between intent and outcome was observed (b = -0.14, SE = 0.05, t(7439.93) = -2.60, p = .009, [-0.24,-0.03], d = -0.05), but not between intent and causality at set level of significance (b = 0.15, SE = 0.08, t(88.29) = 1.78, p = .079, [-0.02,0.32], d = 0.05). These interaction effects parameters went in the opposite direction, indicating that the effect of outcome was stronger in non-intentional (b = 0.85) compared to intentional scenarios (b = 0.71) whereas the effect of causality was

descriptively stronger in intentional cases (b = 0.98) compared to non-intentional ones (b = 0.85).

The intent-by-type of judgment effect was not significant, b = -0.12, SE = 0.08, t(97.91) = -1.51, p = .135, [-0.27,0.04], d = -0.04. However, as hypothesized, the causality-by-type of judgment interaction effect was observed, b = 0.15, SE = 0.07, t(173.97) = 2.05, p = .042, [0.006,0.30], d = 0.05, such that the causality effect was stronger (+ 0.16) in the case of blame judgments (b = 0.98) compared to wrongness judgments (b = 0.82). The outcome-by-type of judgment interaction effect was not found (b = 0.10, SE = 0.06, t(163.76) = 1.63, p = .105, [-0.02,0.23], d = 0.03). Descriptively, estimated parameters indicated that the effect of outcome was slightly stronger (+ 0.10) for blame (b = 0.83) compared to wrongness (b = 0.73) judgments. Three-way interactions between intent, outcome/causality, and type of judgment were non-significant ($|t_s| \leq 0.50$, $p_s \geq .616$).

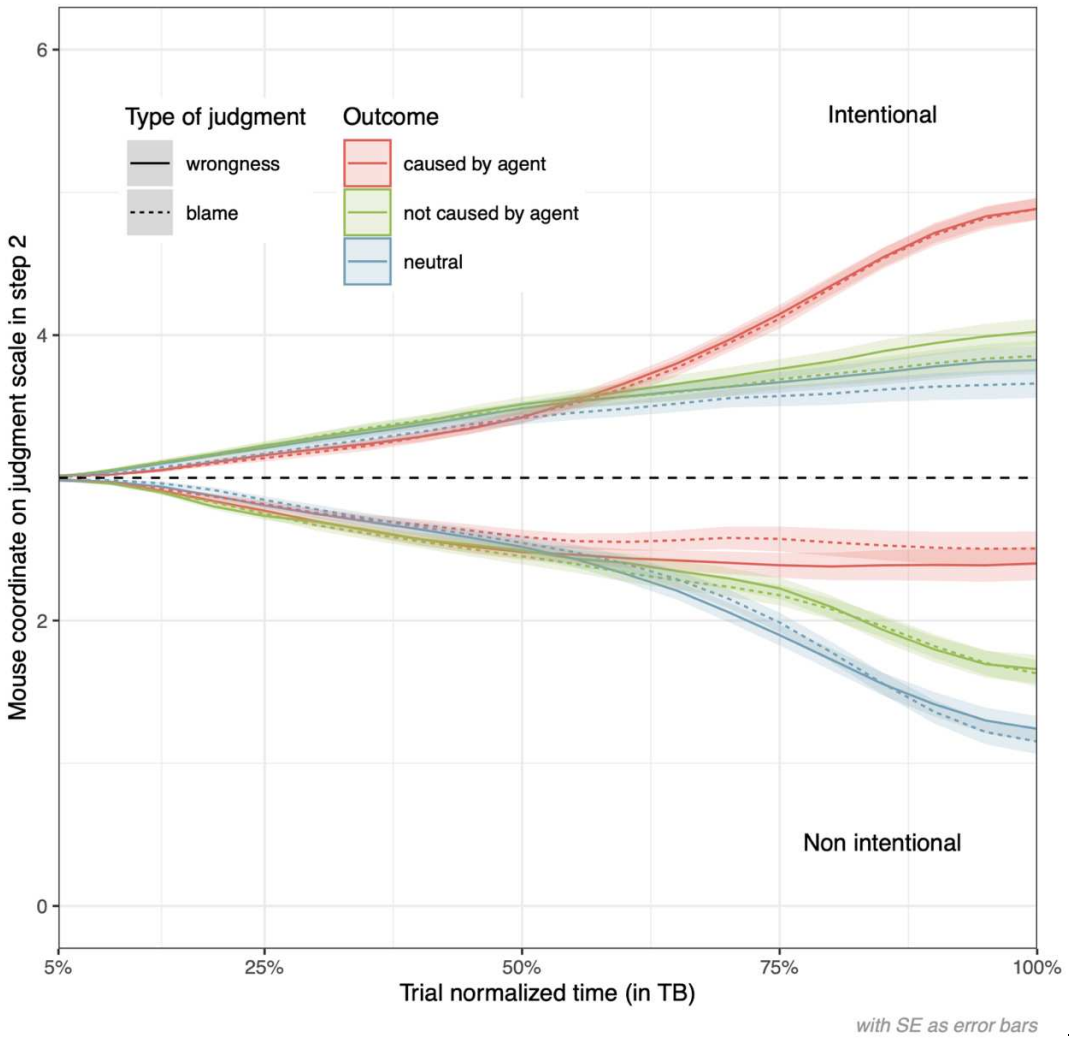
Exploratory Results on Mouse Trajectories (Part B). In the first part of this experiment, when participants only knew about the general context and the agent's intent, we detected that the effect of intent became significant only at the end of the time course (around 90%), b = 0.13, SE = 0.03, t(139.20) = 4.10, p < .001, 95% CI [0.07,0.20], d = 0.18. It became quickly stronger until reaching its maximal value at 100% of the trajectory with b = 2.35, SE = 0.10, t(179.76) = 23.35, p < .001, [2.15,2.55], d = 1.94. No other stable main or interaction effects were detected in this step.

In step 2 however, as participants had already processed intent in step 1, its effect was observed from an early stage onwards (as soon as 0% to 5% of the time course), b = 0.02, SE = 0.004, t(97.28) = 5.86, p < .001, [0.02, 0.03], d = 0.20. It became stronger and reached its maximal value at the end of the trial, b = 2.42, SE = 0.10, t(175.90) = 23.33, p < .001, [2.22, 2.63], d = 1.67 (see Figure 8.10). We observed a main effect of causality, which appeared as stable later on, at 60% of the decision process, where it had a weak effect, b = 0.09, SE = 0.03, t(78.95) = 2.72, p = .008, [0.02, 0.16], d = 0.09). It then became stronger until reaching its maximal value at 100% of trajectory completion, b = 0.88, SE = 0.07, t(136.12) = 12.73, p < .001, [0.74, 1.01], d = 0.60 (see Figure 8.10). The expected significant

effect of outcome was also observed and appeared as strong and stable from 60% onwards (b = 0.08, SE = 0.03, t(61.87) = 3.08, p = .003, [0.03, 0.13], d = 0.08). It was already temporarily significant earlier on, from 10% to 35% of the time course ($-0.03 \leq b_s \leq -0.02$, $p_s \leq .035$). The outcome effect reached its strongest influence at the end of the process (100%), b = 0.76, SE = 0.07, t(144.30) = 11.30, p < .001, [0.63, 0.89], d = 0.52 (see Figure 8.10). Unlike the analysis on final judgments, a main effect of type of judgment was observed, which appeared at the end of the process, around 95%, b = -0.06, SE = 0.03, t(7244.76) = -2.259, p = .024, [-0.11, -0.008], d = -0.04, with wrongness judgments being more severe compared to blame judgments by the end of the trajectory.

Figure 8.10

Results in Exp.8 (Part B) on Mouse Trajectories (in Step 2) Depending on Intent and Outcome/Causality Condition, and Type of Judgment



Note. Y-coordinates were rescaled from 0 to 6 (to facilitate comparison with final judgments). Positive (vs. negative) values signaled an attraction toward the more (vs. less) severe judgments.

Regarding two-way interaction effects, the intent-by-causality and intent-by-outcome interaction effects were found as significant and stable around 75% of the time course, respectively, b = 0.19, SE = 0.08, t(60.58) = 2.34, p = .023, 95% CI [0.03, 0.35], d = 0.08 and b = -0.09, SE = 0.05, t(7229.42) = -2.00, p = .045, [-0.18, -0.002], d = -0.04. They both took their maximal influence at the end of the process (at 80% for the intent-by-causality interaction effect, with b = 0.21, and at 95% for the intent-by-outcome interaction effect, with b = -0.18). Precisely, and consistent with final judgments, the causality effect became stronger when moving from non-intentionally to intentionally perpetrated scenarios, whereas the outcome effect became weaker during the same move. In addition to this later effect, these interactions also had non-negligible influence earlier in the judgment process, as they were significant as soon as 10% and 15% of the time course until around 55% and 35% respectively. For the intent-by-causality interaction effect, it started at 10%, with b = -0.04, SE = 0.01, t(87.99) = -3.01, p = .003, [-0.06, -0.01], d = -0.10 and grew until 55% with b = -0.12, SE = 0.05, t(59.39) = -2.26, p = .027, [-0.22, -0.01], d = -0.06. For the intent-by-outcome interaction effect, it started at 15%, with b = 0.04, SE = 0.01, t(7479.84) = 3.24, p = .001, [0.02, 0.06], d = 0.07 and grew until 35% with b = 0.05, SE = 0.03, t(7531.70) = 1.97, p = .049, [0.0003, 0.11], d = 0.04.

Concerning the intent-by-type of judgment interaction effect, it was unstable throughout the process. Particularly, this interaction effect was significant at different moments: from 15% to 25% and from 65% to 80% ($-0.14 \leq b_s \leq -0.05$, $.021 \leq p_s \leq .048$). It signals a stronger effect of intent in wrongness judgments compared to blame judgments at different moments of the judgment process. Consistent with final judgments, the causality-by-type of judgment interaction effect was found significant starting at 60%, with b = 0.10, SE = 0.05, t(229.38) = 1.97, p = .050, [0.000009, 0.19], d = 0.05, and reached its highest

influence at the end (100%) with b = 0.15, SE = 0.07, t(166.39) = 2.02, p = .045, [0.004, 0.30], d = 0.05. This interaction effect signals that causality took more importance in judgments of blame compared to judgments of wrongness from 60% onwards and became increasingly stronger until the end of the trial. No other two-way interaction effects were observed on dynamics, and the three-way interaction effects were not significant either.

When looking in an exploratory vein at the simple effects of type of judgment within the various conditions of intent and outcome/causality on mouse-tracking data, we found a persistent influence of the type of judgment specifically for the intentional and neutral or not caused scenarios, at the end of the trial (around 90%). These simple effects go in the expected direction with more severe wrongness judgments compared to blame ones (see Figure 8.10). In the non-intentional and caused scenarios, however, there was a persistent significant effect of type of judgment from 30% to 80% of the trajectory as can be seen in Figure 8.10. This simple effect goes in the direction of more severe blame than wrongness judgments, hence inconsistent with what has been found for the other conditions and with the global (albeit non-significant) type of judgment effect direction on final judgments.

Discussion & Limitations

In the current research, we aimed to integrate Malle's (2021) and Cushman's (2008) approaches to moral judgment into a novel three-input processing model and to provide a first empirical test of it. In this endeavor, we tested the effects of the three inputs considered (i.e., intent, outcome, and causality), and their interaction effects on moral judgments of wrongness and blame. To examine the temporal influence of these factors on judgment dynamics, a recent adaptation of a mouse-tracking paradigm for moral decision-making was used in the current experiment (see Gautheron et al., 2023 and previous experiment for implementation with same material).

As previously found in Experiment 7 on judgments of punishment, we first replicated the main effects of intent in steps 1 and 2 (i.e., before and after outcome information was displayed, respectively) and the effects of outcome and causality in step 2 on final moral judgments of wrongness and blame. We did not find clear significant differences between

wrongness and blame final judgments, whether in step 1 or step 2. This contrasts with what would have been expected from the findings of Leloup et al. (2018). They found that judgments of wrongness were significantly harsher than judgments of punishment (Leloup et al., 2018, study 2). This result is consistent with findings of Study 1 from Cushman (2008), who found no significant difference between both in terms of final judgment severity (with a between-participant design), but only evidenced different processes implied for each. As judgments of blame and punishment should be very similar in terms of cognitive processes they trigger, as opposed to permissibility and wrongness judgments (Cushman, 2008) and regarding Leloup et al.'s findings (2018), we expected to find harsher judgments for wrongness compared to blame, which was again not clearly evidenced on final judgments in the current study (although descriptively, means went in the expected direction with slightly harsher ratings for wrongness than for blame judgments).

Interestingly, however, and thanks to the dynamic measure used to trace the decision temporally (i.e., mouse trajectory coordinates across time), findings signal the influence of type of judgment in step 1 during one-third of the whole decision time course (from 30% to 60%), and at the very end of the trial in step 2. These contrasting results for final judgments versus dynamics may indicate that types of judgment exert their influence during the judgment process, but do not have sufficient weight compared to the other factors involved (i.e., intent and outcome/causality) to shift final judgments. Examining the whole decision process in morality (via dynamic methods such as mouse-tracking) therefore provides initial empirical support to Malle's call (2021) to take processing (complexity) into account and not only final judgments.

Mouse-tracking data also made it possible to examine the temporal intervention of intent, outcome, and causality factors to compare it with previous findings of Experiment 7. Current findings indicate that intent intervened in the decision process very late in step 1 and very early in step 2 (which is consistent with the fact that participants were already aware of this information at the beginning of step 2). Outcome and causality factors intervened earlier in step 2, around the middle of the time course, with outcome having some unstable

influence beforehand. In other words, findings suggest a late influence of intent in step 1, an early influence of outcome in step 2, and then a delayed influence of causality. These results therefore provide only partial support for the temporal intervention of these factors found in our last experiment (where we found an early influence of causality compared to the outcome feature in step 2).

However, to provide a more stringent test of this sequence, all input information may be displayed at the same time. Given mouse-tracking constraints—specifically the requirement to favor quick processing (Hehman et al., 2015) and thus to display limited amounts of information—simultaneous display of all informational features is unrealistic. Adapting mouse-tracking paradigms to moral scenarios is already a risky and rather recent advancement, which certainly explains why only very few studies used it for moral scenarios (see Gautheron et al., 2023 and Koop, 2013 for exceptions). One alternative that may be investigated in future research would be to counterbalance informational features order between participants as done by Leloup et al. (2018), but with a measure of underlying dynamics. Such research, however, may not simultaneously focus on differential effects of types of judgment given additional complexity, which was an important aspect of the present study.

When we looked at the interaction effects of these factors with type of judgment, we found evidence for causality, whose effect significantly varied in the expected direction depending on the type of judgment at stake (i.e., more impact in blame than in wrongness judgments). This specific result supports the present new processing model of moral judgment where causality is supposed to get more weight in blame (and punishment) judgments than in the other types of judgment. However, the other two-way interaction effects (i.e., intent-by-type of judgment and outcome-by-type of judgment) did not reach statistical significance. Their respective directions were still consistent with expectations as standing in the proposed model, with slightly more impact of outcome in blame than in wrongness judgments and slightly stronger weight of intent in wrongness compared to blame judgments. These interaction effects directions hence go in line with the present model of

moral judgments in terms of the differentiated weights for informational features depending on type of judgment.

When looking in post-hoc analyses at the effect of type of judgment within each intent-by-outcome condition on dynamics, we detected differences between blame and wrongness judgments. They became permanently significant over a good proportion of the trajectory, especially in some conditions. In intentional and not caused or neutral outcome scenarios (i.e., when nothing too crucial happened in terms of consequence or causality), we detected a partially permanent influence of type of judgment in direction of more severe judgments of wrongness than blame (consistently with what was found in final judgment analysis). For non-intentional and caused scenarios, findings signal a persistent difference between blame and wrongness judgments in the middle of the trial but that disappeared at the end. Such influences during the judgment process would not have been detected without the use of a process-tracing method such as mouse-tracking. Even if the three-way interaction effects were not significant globally, these post-hoc results are consistent with the currently developed processing model which poses wrongness as firstly influenced by intent information, yet also by causal information but to a lesser extent and blame firstly influenced by both intent and causal information.

The present findings may also highlight some limitations of Cushman's methodology (2008) using a between-participant design, comparing results between experiments, and using only a few scenarios to test his model. Cushman (2008) argues that an intentionally perpetrated act would be judged as wrong no matter what outcome follows or who is responsible for the act. Based on the present research using a within-participant design, it seems instead that for both types of judgment (i.e., wrongness and blame), intentional as well as causal features play a role (and a preponderant one) all along the decision process. What differs between the two is more the extent to which they play their role: For wrongness judgments, intent plays a somewhat bigger role than outcome or causality features (even if still non-negligible in weight), whereas, for blame judgments, outcome and causality features play a slightly bigger role than in wrongness (even though intent still has a larger impact).

Cushman (2008) also advances that intent and outcome are not supposed to interact whereas intent and causality do (for blame and punishment only). The current result about intent and outcome interacting no matter the type of judgment at stake challenges this finding too.

Some limitations should be noticed for the current research findings. First, the current observed differences on final judgments as well as on mouse trajectories remain low in size (except for intent, outcome, and causality main effects). Moreover, only partial evidence was found for the differentiation of these effects depending on the type of judgment. It may stem from the fact that type of judgment was currently manipulated in a within-participant design (even if counterbalanced by block), hence a possible lack of differentiation between both questions in the participants' perspective (see Malle, 2021 for more). Concurrently, blame and wrongness judgments also differ in terms of the object they target, with wrongness focusing more on the act itself (independently of who perpetrated it), whereas blame targets more the agent who performed the action (Malle et al., 2014). This may have played in our favor in the current experiment, although the judgment object was not explicitly controlled.

Beyond the conceptual replication of the main effects of key factors in morality on final judgments, we found in this experiment no strong evidence for a clear differentiation of these effects depending on the judgment type (either wrongness or blame), except for causality. Dynamic data (i.e., mouse trajectories over time), however, allowed us—beyond the partial replication of findings from previous research—to detect some unstable evidence of these interaction effects during specific portions of the trials and specifically for some intent-by-outcome conditions. Present findings hereby partially support the proposed three-input processing model of moral judgments, while also suggesting that the distinction between types of judgment (particularly wrongness versus blame) must not be overestimated.

Analyzing Social Distance Effects within a Mouse-Tracking Technique (Experiment 1, Part B)

Rationale

In previous sections, we looked at the effects of some key factors on moral decision-making to validate the possible use of a mouse-tracking adapted paradigm with a much complex moral material (e.g., long sentences). We found quite consistent results, with main effects of intent, outcome, and causality being replicated (as well as partial evidence for their interaction effects). Moreover, the analyses of the mouse trajectories—rendered possible via the mouse-tracking paradigm—allowed us to visualize their continuously growing influence over the decision process (with influences at 100% of the trajectory quite well matching the responses on final judgments). Therefore, and as the final aim of this chapter was to provide a more indirect and dynamic tool to grasp the maybe more implicit or automatic influence of social distance, we presently wanted to investigate social distance effects on moral decision-making throughout this dynamic technique of mouse-tracking.

This is what we actually did in Experiment 1, where social distance was manipulated toward the agent of the moral action (i.e., a close one vs. a stranger as the agent), while intent and causality were both varying within the various situations in play (either an intentional or accidental act and either caused by the agent or not). We decided to keep these previously studied key factors in morality, in order again to replicate and consolidate the relevance of the mouse-tracking task—as adapted by Gautheron et al. (2023)—for a moral complex material.

As we already presented Experiment 1 in terms of method and results in Chapter 5 (pp. 120–130), we will not presently report the whole study details, except for those related to the mouse-tracking paradigm and findings (which have not been presented earlier). Regarding mouse-trajectories specifically, we expected—even if not explicitly preregistered—to replicate the previously found growing influences of intent and causality factors throughout the decision process (with causality intervening only in step 2 due to the order of information presentation). In an exploratory way, we also wanted to check whether social distance may have had a more subtle influence in an early stage of the decision process (either on step 1 or step 2), hence not necessarily detected on final judgments.

Method

Regarding the mouse-tracking task implementation, we reused the paradigm from Gautheron et al. (2023) and adapted it with our moral scenarios coming from Leloup et al.'s database (2018)—as done previously for Experiment 7 and 8 above. The only major difference in material was the manipulation of social distance, as explained in Chapter 5 (see pp. 121–122 for more). However, contrarily to the other experiments dealing with social distance manipulation (i.e., Exp.2 to 6), this experiment is the only one where we recorded at the same time the mouse Y-coordinates across time. In order to recruit participants more easily (and allowed the experiment to be run remotely on participants' own device), we adapted the previously developed mouse-tracking paradigm so that it could be performed using a trackpad. Participants using a computer mouse were thus only 33 out of 84 (39% of the whole participant sample). For all however, in order to fit with the mouse-tracking task constraints, pointer acceleration was asked to be switched off and pointer speed was reduced. As a reminder, the final reduced sample for analyses (including the exploratory analyses below) comprised 73 participants.

For mouse trajectories analyses, we performed a timestep by timestep mixed-effects analysis on rescaled Y-coordinates (i.e., 20 timebins for each step[105]). We used the reduced mixed model from the previous confirmatory analyses and selected 92% of trajectories (for step 1 and step 2) for these analyses[106]. As a reminder, we expected to find the general same tendencies as observed in previous studies (see Experiments 7 and 8 above) at least for intent and causality factors influence, and for their interaction effect. That is, in step 1, a growing influence of intent from the middle of the trial until its end. In step 2, we expected an early influence of intent (in comparison to the other influences), as well as a growing influence of each factor during the time course. During step 2, we expected a less robust or

[105] We chose in the current mouse-tracking analysis to report the mixed analyses on the timebins (and not on splines) as the estimated splines (i.e., usually used to better reflect the natural hand movements) were giving much more noise in the estimation of the actual trajectories.

[106] The criterion to select the viable trajectories was too severe for the current mouse-tracking data. We thus slightly changed the value of this criterion in comparison to classic selection of mouse trajectories, because the current trajectories were much more variable, and this criterion would have otherwise led to 0% of remaining trajectories.

less clear effect of intent than in step 1 due to the involvement of the causality factor. However, we hoped to find again in the end of step 2 a stronger influence of intent than that of causality (hence matching the large effect size for intent found in step 2 final judgments as reported in the confirmatory section of Chapter 5, see p.146).

Results on Mouse Trajectories (Part B)

Results in Step 1

This mixed-effects analyses on step 1 trials revealed that the effect of intent appeared quite very late in the decision process (from 85% of the whole trajectory), $b = 0.12$, $SE = 0.05$, $t(73.54) = 2.60$, $p = .01$, 95% CI [0.03, 0.22], $d = 0.15$. It became stronger until the end of the timecourse, reaching its maximal value in the end of the trial (from 95% to 100%), $b = 1.88$, $SE = 0.15$, $t(84.64) = 12.42$, $p < .001$, [1.58, 2.18], $d = 1.45$ (see Figure 8.11, top chart). No significant effect of causality was detected in this step, in accordance with the fact that the causal information was not already displayed in the screen (hence participants only judging the situation on the basis of the scenario context and the agent's intent). Quite surprisingly however, there was an unexpected and temporary effect of intent-by-causality interaction (from 50% to 65% of the trajectory), $0.02 \leq b_s \leq 0.04$, $2.21 \leq t_s \leq 2.72$, $p_s \leq .03$, $0.08 \leq d_s \leq 0.12$, although it became then non-significant for the rest of the trial course.

Regarding the effect of social distance, there was no significant main effect in any portion of the mouse trajectory. However, when looking exploratorily at the causality-by-social distance interaction across time, we detected partial significant effects, especially from an early stage onward (from 5% to 30% of the trial), and even if very low in size ($b_s = 0.01$, $1.95 \leq t_s \leq 2.78$, $p_s \leq .05$, $0.06 \leq d_s \leq 0$). This temporary interaction effect was in direction of a very slightly stronger effect of causality for a distant compared to a close agent. No other main or interaction effect was detected along the mouse-trajectory in this step.

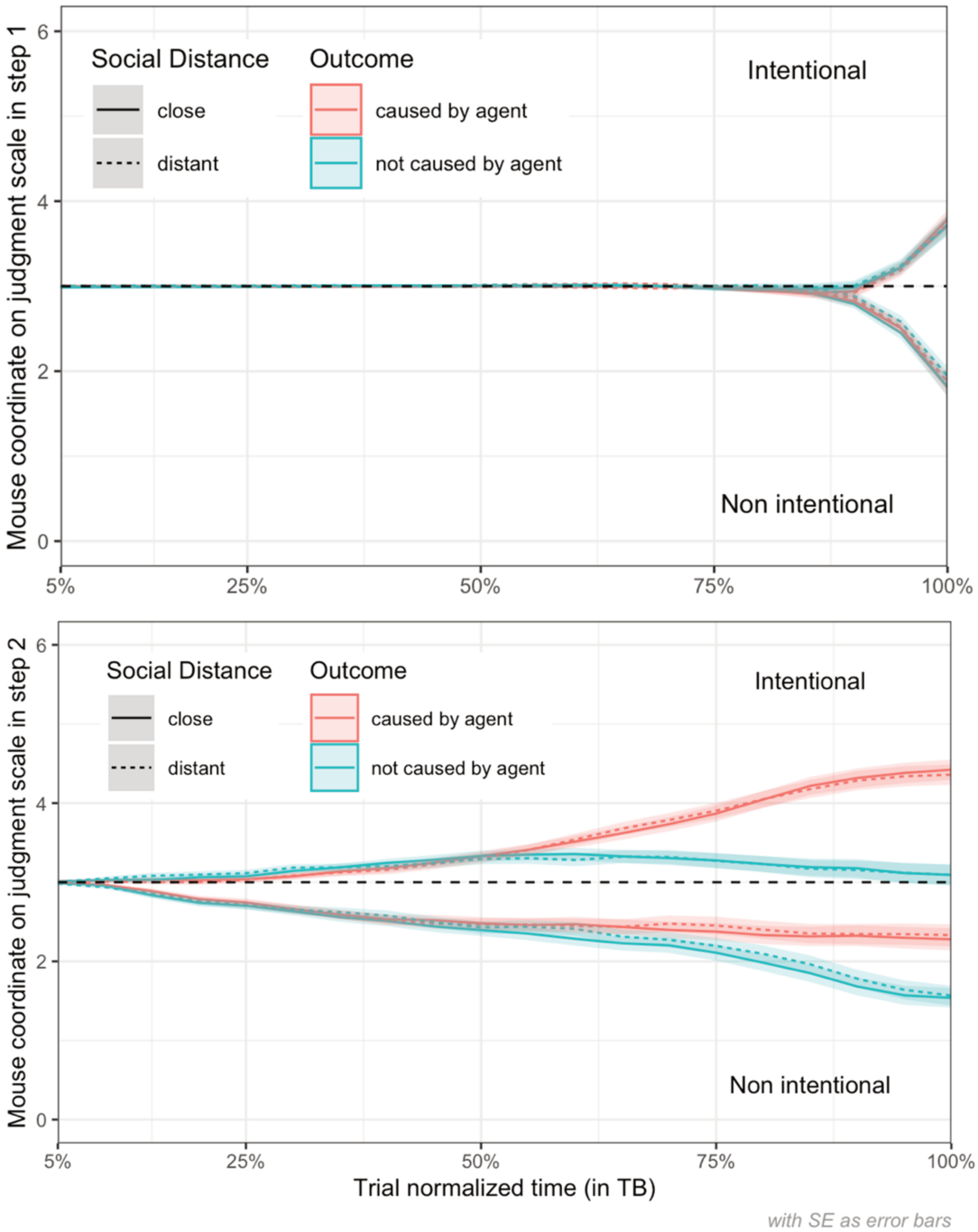
Results in Step 2

The intent effect appeared as predicted from the early beginning of the trial in step 2 (as soon as 5% of the timecourse), b = 0.02, SE = 0.01, t(62.52) = 2.43, p = .02, [0.003, 0.03], d = 0.12. It became then stronger throughout the whole decision process until reaching its maximal value as expected in the end of the trial (at 100% of the trajectory), b = 1.81, SE = 0.14, t(83.89) = 13.13, p < .001, [1.54, 2.09], d = 1.20. (see Figure 8.11, bottom chart). In this step, we also observed an effect for causality, but that appeared as significant only later during the decision process, that is, roughly around the middle of the timecourse (around 55% of the decision process). It was only tendencial there (b = -0.08, SE = 0.04, t(55.77) = -1.91, p = .06, [-0.15, 0.004], d = -0.07), but then became stable very soon afterwards (around 60%, b = -0.16, SE = 0.04, t(58.43) = -3.62, p < .001, [-0.25, -0.07], d = -0.14). This causality effect took its maximum absolute value at the end of the trial (100%), b = -1.03, SE = 0.11, t(75.40) = -9.09, p < .001, [-1.25, -0.80], d = -0.68, going as expected in the same direction than final judgments (harsher judgments when caused compared to non-caused by the agent).

During the decision process in step 2, we also noticed that the interaction effect between intent and causality appeared quite early as significant (as soon as 20% of the whole trajectory), even before the main effect of causality itself (b = 0.10, SE = 0.04, t(91.83) = 2.54, p = .01, [0.02, 0.17], d = 0.08). However, it was there in the opposite direction in regards of what was expected, that is a stronger effect of intent when moving from caused to not caused scenarios. This effect remained significant until about 30% of the timecourse, then diseappaered. It became again significant and in the expected direction from 70% of the time course (b = -0.25, SE = 0.09, t(62.43) = -2.64, p = .01, [-0.44, -0.06], d = -0.10) until the end of the trial where it reached its highest values (at 100% of the trajectory, b = -0.55, SE = 0.12, t(68.91) = -4.42, p < .001, [-0.79, -0.30], d = -0.18) (see Figure 8.11, bottom chart).

Figure 8.11

Results in Exp.1 (Part B) on Mouse Trajectories in Step 1 (Top) and in Step 2 (Bottom) Depending on Intent, Causality, and Social Distance



Note. Y-coordinates were rescaled from 0 to 6 (to facilitate comparison with final judgments). Positive (vs. negative) values signaled an attraction toward the more (vs. less) severe judgments.

Regarding the effect of social distance, this effect was mainly found as non significant in the confirmatory analyses on final judgments of punishment. We can noticed here thanks to the mouse trajectories analysis that this was also the case all along the decision process, as this main effect never reached statistical significance at any time / on any portion of the trajectory. It culminates at b = 0.05 at its highest influence (SE = 0.03, t(3522.27) = 1.70, p = .09, [-0.008,0.12], d = 0.04). We also noticed that this effect was significantly moderated by the causality factor very temporarily at 15% of the trajectory (b = 0.07, SE = 0.03, t(3593.49) = 2.62, p = .009, [0.02,0.12], d = 0.07) but then, became non-significant for the rest of the time course.

To refine these results, we looked exploratorily at the simple effects of social distance throughout the time course, depending on the various intent-by-causality conditions. In doing so, we found a quite systematic and persistent simple effect of distance from about 50% of the trial time course and in the non-intentional and not-caused-by-agent condition only (see Figure 8.11, bottom chart). Even if too low to reach statistical significance (b = 0.03, p = .68, [-0.11, 0.17] at 100% of the trajectory), the direction of this simple persistent effect seems to be in accordance with the idea that participants tend to judge harsher a stranger compared to a close-one. We will further discuss this finding in the discussion.

Discussion & Limitations

In this first experiment, we applied the previous mouse-tracking task—tested on a more complex moral material in Experiments 7 and 8—to the investigation of the social distance factor[107]. Within this experimental design, and although this effect was mainly found as non-significant in the confirmatory analyses on final judgments of punishment (see Chapter 5, pp. 125–126), we were able to detect more subtle influences thanks to the analysis of mouse trajectories. But first, regarding the replication of intent and causality influences, we found quite consistent results with what had previously found. Intent took its influence quite late in step 1 (maybe due to language processing), then very early in step 2

[107] Experiment 8 was conducted before Experiment 1 chronologically. This is why, although a bit counter-intuitive as reported here, we were able to apply this already built mouse-tracking paradigm for the investigation of social distance.

as this information had already been processed. Causality intervened for its part only during step 2, from the middle of the trial onward. Both factors took a growing preponderance in the decision as the time goes by.

Regarding their interaction, it was found as significant and in the expected direction only very late in the step 2 decisional process (from 70%). Other significant temporary influences were detected earlier in the decisional process (both in steps 1 and 2) but that were reversed compared to the expected direction (i.e., stronger influence of intent when passing from caused to non-caused situations). This inversion of the interaction effect along the timecourse was a bit surprising—it can be visualized more clearly on the Figure 8.11—but could reflect some noise in the decision process.

Finally, regarding social distance dynamic influence, we found in step 1—beyond the mere replication of the previous tendency observed for intent—a temporary significant interaction effect between causality and social distance. It was however low in size and only significant at the beginning of the trial. What is also surprising is the fact that at this stage, participants were not aware of the causal information (as displayed only in step 2). It may stem from an influence of the previous trials, but this would be to confirm via further analyses. Moreover, in step 2, a quite systematic and persistent effect of social distance was revealed but for some conditions of intent and causality only. More specifically, it was observed for non-intentional and non-caused scenarios, with participants judging close agents slightly less harshly than distant agents across a significant portion of the decision process. As it was the scenario case where there was almost nothing that happened (i.e., the action was non-intentionnally perpetrated and the harmful outcome was not directly due to the agent's action), we may wonder whether social distance essentially alters moral decision-making when there is no other relevant information to guide judgments. Said differenlty, the absence of evidence for a clear and robust early or later (including final responses) influence of social distance on people's moral judgments may come from the fact this influence is strongly diminished by the weight of other key factors guiding moral judgments (i.e., intent and causality in the current experiment). Current results—even if only

revealing unstable and temporary subtle influences of social distance throughout the decision process—still support the usefulness and suitability of such a dynamic method (i.e., mouse-tracking) to investigate the influence of key factors guiding moral judgments over time.

General Discussion About Mouse-Tracking Use in Moral Decision-Making

In the current chapter, we demonstrated how dynamic tools (i.e., mouse-tracking) can be appropriate to study moral decision-making. Indeed, considering social and moral cognition as socially situated and resulting from continuous changes, dynamic methods are useful to grasp the interactive processes underlying decision making, that is, throughout the whole decision process (and not merely on the final output of it). Moreover, social distance influence in morality could be thought of as a more subtle, early stage, and intuitive influence, thus supporting even more the use of such a dynamic or at least indirect method to be detected. Therefore, we used throughout Experiments 1, 7, and 8 an innovative mouse-tracking paradigm (Gautheron et al., 2023) and successfully adapted it to a quite rich moral material. This allowed us to somewhat palliate some of the drawbacks previously encountered with such a complex material (see Koop, 2013).

In Experiments 7 and 8 first, we tested this novel mouse-tracking paradigm with the material of Leloup et al. (2018) comprising everyday moral situations and varying the agent's intent and causal role in the outcome (either no harmful outcome or a harmful outcome caused vs. not caused by the agent). On final response as well as via mouse-coordinates registering during the decision process, we re-affirmed the growing and preponderant influence of intent, then of outcome/causality in our everyday moral judgments. Only partial (non-perfectly consistent) evidence was found yet for the intent-by-outcome/causality interaction effects.

In Experiment 8, we further distinguished the differentiated weight of these key factors throughout the decision process depending on the type of moral judgment at stake (e.g., blame and wrongness) via the elaboration of a 3-input processing model of moral judgment. The exploratory path on mouse-tracking data indeed provided evidence for the

fact that the various factors did not intervene at the same time and with the same magnitude in the decision process. In the end, beyond getting a first grasp of the temporal influence of intent, outcome, and causality factors during moral decision-making, Experiments 7 and 8 provide evidence for the applicability and usefulness of a mouse-tracking paradigm to a complex moral material (see Gautheron et al., 2023 for more).

In Experiment 1 finally, applying this novel dynamic paradigm to the study of social distance influence allowed us to detect more subtle influences throughout the decision process, which—although often not stable and/or not significant for all conditions—would not have been visible on the mere final moral judgments. More specifically, the fact that the influence of social distance appears as persistent (even if non-significant) in the non-intentionally perpetrated and not caused by the agent scenarios lets us think that social distance may have been hindered in the current experiment by the preponderant influence of the other key factors for moral decision-making (i.e., intent, causality).

As a perspective for future research, we might think about building a novel experiment dealing with moral decision-making and social distance biases, but with other traditional key factors being fixed or controlled for. For instance, we may investigate whether accidental harm situations (with the outcome caused by the agent) would temporarily trigger a significant influence of social distance from an early stage onwards, then potentially compensated by an attraction toward the most impartial option, due to an inherent motivation to appear as a moral being. Applying mouse-tracking to the study of social distance would therefore still require some work for being fully exploitable—as the current research did not detect any significant influence of it both at the final responses or mouse dynamics levels. However, we do encourage other researchers to further exploit this tool to investigate this question, but also more broadly all questions related to social or moral cognitive processes that could gain from a temporal dynamic perspective of analysis. Mouse-tracking is indeed easy to implement, less costly than other dynamic methods (such as eye-tracking), and offers an almost direct window to what happens in mind in real time during everyday decision processes.

Part III.

GENERAL DISCUSSION

Chapter 9

General Discussion & Conclusion

Literature in moral psychology tends to not evenly hold that social distance (the more or less intimate relationship people share) biases our everyday moral decisions. It would occur either in a self-serving manner, via a general favoritism bias toward the self and our close ones (Tendency 1) or in an other-serving way, via harsher judgments toward the self or close ones in case of wrongdoing (Tendency 2). However, research on this topic is vast and low commensurable, due to a large diversity of terminologies and operationalizations used for social distance. How can we then unify and make commensurable the apparently very diverse findings in literature about social distance influence in moral decision-making? This is the question we addressed in the current dissertation, via the use of a threefold psychosocial analytical framework (i.e., intraindividual, interindividual, and intergroup levels) and via a dynamic tool of investigation (i.e., mouse-tracking).

Summary of Experimental Findings

All along the experimental part of this dissertation, we argued that social distance effects in moral decision-making are complex and sometimes ambiguous depending on the various contextual parameters at play. Across a total of eight experiments (N = 1132), we investigated—throughout the diverse levels of analysis and sometimes dynamically—how social distance influences everyday moral decisions, and specifically, which direction of the effect is most strongly supported. We also examined how these effects were influenced by some potentially relevant situational parameters (i.e., moderators). As the findings of each study have already been discussed in the previous experimental chapters, we now provide a comprehensive summary of the results across all experiments, followed by an opening toward the limitations and perspectives of this research.

In **Chapter 5**, we tested the impact of social distance when manipulated toward the agent (i.e., the wrongdoer) of the situation, first at the interindividual level (Experiment 1), then confronting the intraindividual and interindividual levels (Experiment 2). In Exp.1, we found no evidence of a social distance influence when comparing judgments of punishment attributed either to a close-one or to a stranger (see part A on final judgments). Once better controlling the identity of the victim in Exp.2 (i.e., always a stranger), there were significant but weak effects of social distance to the agent, although ambiguous in direction (d = 0.04 and d = -0.07 respectively). People indeed favored their close ones over distant others (in accordance with Tendency 1 findings) but also favored their close ones over themselves (Tendency 2), by judging themselves slightly more harshly. We then tested the same effect at the intergroup level, by comparing judgments of punishment attributed to an ingroup perpetrator compared to an outgroup perpetrator, with group membership induced via a minimal group paradigm (Experiment 3, part A). This study revealed no evidence of a significant influence of social distance on moral judgments of punishment. Participants did not judge the outgroup agent more severely than the ingroup one. Experiment 4 helped refining these results by providing a more ecological test of the same experimental design, while introducing in addition the intraindividual level of analysis within a simulated online multiplayer game. In this experiment (Exp.4, part A), we found contradictory results compared to Exp.2: Participants judged themselves more leniently than others (both close and distant) as agent (d = -0.19), whereas the difference in judgments between a close (ingroup) and a distant (outgroup) agent was not significant (d = 0.05). The significant main effect between self and others was however not stronger for harmful outcome than for neutral outcome cases, hence suggesting a mere self-favoritism bias as generally found in social cognition (not especially proper to moral cognition). At the interindividual level, the present findings seem to suggest that social distance effect is more in accordance with the first tendency (i.e., close ones' favoritism), whereas we found no evidence of any differentiation at the intergroup level when dealing with moral situations. Results at the

intraindividual level are more ambiguous, going either in favor of the first tendency (Exp.4, part A) or in favor of the second tendency (i.e., other-serving bias, Exp.2).

In **Chapter 6**, we tested the same question but with social distance being manipulated toward the victim of the moral situation at stake. At the interindividual level, with oneself always being the perpetrator of the depicted choice, we found contradictory results on moral judgments between both experiments (Experiments 5 and 6), although using the same material (i.e., the EMCS). Exp.5 indicated more extreme judgments when the victim was a distant individual compared to a close one (i.e., higher ratings of wrongness in case of egoistic acts and higher ratings of rightness in case of altruistic acts). In Exp.6 however, focusing on egoistic acts only, people rated the depicted actions as more morally wrong when the involved victim was a close one compared to a distant other, hence more consistent with a defensive reasoning (Tendency 1). Moreover, the latter finding corroborates those of the first experiment but on choices of action (i.e., when participants had to choose how they would act between the altruistic and egoistic option). We indeed found less frequent altruistic choices when the victim was socially distant compared to socially close (and this effect was mainly driven by male participants' responses, as shown by the analysis of simple effects). We then reported the second part of findings from Experiments 3 and 4 (parts B), about the manipulation of social distance to the victim at the intergroup level (i.e., an ingroup vs. outgroup victim). In both experiments, we did not find any evidence of a differentiation in judgments either of punishment (Exp.3) or of wrongness (Exp.4) when a close (ingroup) or distant (outgroup) victim is involved. Although effects of social distance to the victim were hypothesized to be found more evenly in direction of harsher judgments when a close (vs. distant) other is involved, this chapter provided again only partial and still ambiguous evidence of this. At the interindividual level, both directions of results—harsher judgments when close victims are involved (as in Tendency 1) and harsher judgments when distant victims are involved (as in Tendency 2)—were found. At the intergroup level, we found again no evidence of such an effect of social distance on moral judgments.

In **Chapter 7**, we tested various potentially relevant moderators for the effect of social distance in moral decision-making. In particular, in Exp.1, we first tested whether traditionally key factors for morality (i.e., intent and causality) were significant sources of moderation for social distance effects. We were not able, however, to detect any difference, probably due to the preponderant effect of these key intentional and causal factors. As suggested by previous findings in the literature (e.g., Tassy, Deruelle, et al., 2013), we also compared the effect of social distance depending on the type of moral judgment at stake (in Exp.2) and depending on the type of decision (in Exp.5). The former revealed no differentiated effect of social distance between wrongness and punishment judgments. The latter indicated, via a descriptive comparison between the two parts of the study, a slight difference in effect size with social distance effect being stronger on moral judgments than on choices of action. When investigated with regard to the severity of the consequences for the victim, we again found mixed evidence. Be it in an exploratory way—when reanalyzing results from Experiments 1 to 3 with a naïve a priori classification of the situations in terms of outcome severity—or in a confirmatory way in Exp.6 with severity being manipulated, we detected no significant interaction between social distance and severity. Finally, when testing the moderation of social distance by the level of agent's responsibility throughout Experiments 1 to 3 (again within a naïve a priori classification), we found no evidence of such an interactive relationship.

In **Chapter 8**, via an innovative mouse-tracking paradigm adapted to moral materials, we successfully replicated the effects of key factors in moral situations, more specifically intent, outcome, and causality (Experiments 7 and 8). However, when testing social distance effects within the same dynamic paradigm (Exp.1, part B), we did not find any significant influence of social distance at any moment of the trajectory (as for final judgments' results).

Altogether, these findings—especially from Chapters 5 and 6—support the idea that social distance influence on moral decision-making is indeed complex and ambiguous. It further highlights the necessity to take a multifold analytical framework, as we did not find the same tendencies of results depending on the level of analysis considered. Furthermore,

the investigations led in Chapter 7 regarding the various possible moderations of social distance effect did not bring satisfying evidence of any moderator (among those that have been tested or explored). The dynamic analysis of its influence in Chapter 8—although promising—did not provide robust evidence of a more subtle and/or early influence of social distance.

Experimental Findings' Implications for Future Research

What insights, then, have emerged from this experimental section? Despite the generally non-significant and/or quite weak effects of social distance in our experiments' results, we still believe and support the idea that this investigation was useful, even necessary. It will, in our mind, allow for more commensurable research both in designing and thinking future studies and in synthetizing previous findings on this question. For now, let's focus on the future by mentioning some avenues for reflection opened up by this dissertation work.

On Social Distance Effect (Over)Estimation

Traditionally, the effect of social distance in moral decision-making has been assumed as quite robust and medium in terms of effect size (i.e., 0.32 in equivalent Cohen's *d* according to Balliet et al.'s meta-analysis on ingroup favoritism in cooperation, 2014). We can therefore ask why in our own experiments, we did neither find nor replicate such an obvious or robust effect in literature. There may have been some limitations in our experimental operationalizations and/or conceptualization of social distance, as we will develop in the next section, but it seems quite difficult to consider them as the unique cause of that failure. In particular, it would seem hard to believe that participants in a very simple yet refined experimental design (as was the case at the interindividual level in Exp.2) were actually not biased by their social distance to the protagonists.

One could think this could come from the nature of the experimental design we used in our experiments, that is, the within-participant and within-stimulus counterbalanced designs. People may indeed be more aware of the comparison between close and distant protagonists in such factorial designs compared to when social distance is manipulated in a

between-participant design. Participants in a within-participant design (i.e., where they have to answer both scenarios involving socially close others and scenarios involving distant protagonists) could have corrected their answers to palliate this quite well-known self-serving bias. Following the same logic as the desirability bias, they would eventually present themselves as a moral and impartial individual. Given that within-participant designs are generally associated with larger effect sizes in psychology, this also raises the question of what truly makes a difference in moral judgments specifically.

However, as we have seen earlier in Exp.1 (part B) on mouse trajectories analysis, there was no significant influence of social distance, even at an early stage of the decision process. The fact that the agent's intent was also manipulated at the same time in this study prevents us from determining whether social distance alone would have triggered such an effect. Intent has indeed been found to play a predominant role in moral decision-making, as demonstrated by both previous research (see Cushman, 2008) and the current investigation (see Experiments 7 and 8). It could then have presently by-passed the more subtle influence of social distance.

One of the other contributions of the current series of experiments, with a within-participant design, is the fact that participants had always a large set of situations to judge (from 40 to more than 60 different scenarios depending on experiments), hence the possible generalization to other everyday moral situations. The current analyses were indeed conducted with mixed models comprising two random variables, allowing for generalization both at participant and stimulus levels. The consideration of a quite large battery of scenarios in the present experiments may explain why we did not observe findings related to social distance effect as found in previous literature, in particular regarding its effect size and direction. In the other way around, this discrepancy in results may stem from the fact that, in moral psychology literature, research has usually dealt with a limited number of situations, thereby potentially overestimating the real impact of social distance. As suggested by our exploratory analyses on the scenarios' characteristics (see Chapter 7), we observed very differentiated effects of social distance depending on the various situations (being either

positive or negative, with various effect sizes or almost null for some scenarios). We were not able, however, to identify for now which situational parameter is directly responsible for these differences at the scenario-level.

Research with Other Interesting Moderators

Beyond the factors tested earlier as potential moderators of social distance effect on moral decision-making, which happened to be globally non-significatively influential in our experiments, we can also think about other variables of interest. One feature that has not been tested in the current research is the impact of culture.

Culture and Social Distance

The targeted population in our experiments was indeed merely issued from Western, educated, industrialized, rich, and democratic societies (WEIRD, see Henrich et al., 2010). This may have had an impact on how social distance exerted (or not) its influence in our experiments. Cross-cultural research indeed revealed differences in general decision-making depending on people's cultural background (e.g., Yates & de Oliveira, 2016). It seems also true when dealing with moral decision-making specifically, with observed within- and between-societies variations as reviewed by Graham et al. (2016) (see also Bago et al.'s registered report, 2022). Regarding social distance particularly, research seems to indicate that collectivistic (i.e., Eastern) societies seem to be less oriented toward close others' favoritism than individualistic (i.e., Western) societies during moral reasoning (e.g., Baldwin et al., 2024 comparing US and Japan samples). This could stem from the fact that most Eastern cultures base their view of the self as fundamentally interconnected with others (hence valuing first the society as a whole) whereas Western cultures would more emphasize and prone an independent nature of the self (e.g., Markus & Kitayama, 1991 with Asian vs. American comparison). However, more general differences in terms of individualistic vs. collectivistic societies have not been observed on classic moral dilemmas (Bago et al., 2022). We did not focus on such a moderator as our priority was to identify a stable and robust effect of social distance for a population, before searching for its

generalization to other quite different cultures[108]. Once this phenomenon is better understood within a single culture, we may then focus our efforts on cross-cultural comparisons. The scoping review we have undertaken will also help to contribute to this understanding.

Gender and Social Distance

Another factor of interest, that has been evoked very briefly during this dissertation but requires more discussion, is gender. We know already that males and females do not necessarily answer the same way in moral decision-making generally (e.g., Friesdorf et al., 2015; Youssef et al., 2012). As analyzed meta-analytically by Friesdorf et al. (2015), females tend to more likely choose deontological over utilitarian responses to moral dilemmas compared to males, more probably due to differentiated affective (than cognitive) evaluations of harm. Males and females were also found in previous research (see notably Hurwitz & Smithey, 1998) to especially differ in their respective perception of crime and punishment, with women being more afraid of crime in general, consistently with the socialization they experienced.

More specifically related to the influence of social distance, Singer et al. (2019) and Singer, Binapfl et al. (2021) pointed at a possible interaction with gender, without consistently providing evidence for it. Some of the current findings (especially in Experiments 5 and 6) do indicate such a significant interaction but remain contradictory in direction. In Experiment 5 (part A, on choices of action), we replicated the expected interaction effect with males choosing significantly more the altruistic option when a close one is involved than a distant other, whereas this effect was absent for females.

A few explanations could be raised to explain this tendency. This differentiated effect of social distance between males and females could stem from females' tendency to be more egalitarian (i.e., desire to share evenly with others), while males tend to have more polarized behaviors (i.e., acting totally selflessly or totally selfishly, Andreoni & Vesterlund,

[108] Note, however, that participants in our experiments were issued from different countries (i.e., France, UK, USA). As such, even if they do not represent opposite cultures, they may still not be fully interchangeable samples.

2001). Another explanation may be related to gender-stereotypic dimensions that differ for women and men. While stereotypes targeting females involve being gentle, supportive, kind, nurturing, and subordinating the self to others, those targeting males involve being egotistical and unprincipled (Diekman & Eagly, 2000). As stereotypes are widely shared, they may explain more altruism among females for everyone (close and distant others) whereas, for male participants, this would only hold when involved protagonists are close to the self.

These interpretative paths are, however, not perfectly consistent with the next findings observed during our experimentations. On moral judgments (Exp.5, part B), this interaction effect was significant only in egoistic scenarios and reverted as social distance effect was stronger for females than for males. In Exp.6 (dealing with egoistic scenarios only), the interaction effect between gender and social distance was not replicated (i.e., it was found as non-significant). Ambiguous findings have also been found in literature regarding this moderation effect. Indick et al. (2000) found that females reported their relationship with the wrongdoer as more influential on their decision to report the wrongdoing than for males, even when not knowing the wrongdoer. The authors suggest a gender differentiation in the baseline consideration of relationships, with women considering their relationship with others as more significant than men (Indick et al., 2000), again probably due to stereotype internalization.

What is not easy when tackling this question of social distance-by-gender interaction is that we have often to deal with the factor of gender both as individual between-participant characteristics and at the same time, as a manipulation of social distance at the intergroup level via groups of membership based on gender. Quite a few studies have been detected as such during the scoping review (e.g., Chu & Grühn, 2018; McGillicuddy-De Lisi et al., 2003; Olsson et al., 2021; Paul et al., 1996). These operationalizations are not ideal to observe the impact of gender as a moderator of social distance effect as it confounds to a certain extent the mere effect of moderation due to participants' gender on social distance effects with the mere effect of social distance itself.

Moral Development Across Ages and Social Distance

Another approach that has not been developed in the current dissertation framework is the one of moral development and evolution of social distance influence with age. According to Kohlberg (1958), we can determine plural moral stages of development from childhood to adulthood, thus refining Piaget's own view of moral development (Piaget, 1932). The pre-conventional stage (3 to 7 years) is the time where the child only searches to avoid punishment and seeks self-interest (Kohlberg, 1958), hence being probably the stage where social distance bias is the strongest (mostly in direction of the first self-serving tendency). During conventional stage (8 to 13) however, children's morality is more influenced by external ethics, respect of laws and authority than before, hence supposedly less prone to social distance biases (or at least less at intraindividual and interindividual levels of analysis for the self-serving tendency). This stage is not yet incompatible with the other directional tendency of social distance, that is, the other-serving tendency. Individuals of this age could see it morally 'right' to punish themselves and/or a close one (ingroup) harsher for a wrongdoing than more distant (outgroup) others, as an indirect manner to protect their own selves. In the end, the post-conventional stage is supposed to be reached when arriving at adulthood, with moral reasoning supposedly based on a personal ethic of principles and on justice and human rights (Kohlberg, 1958), hence being supposedly the stage where people are the least influenced by social distance (toward impartial judgments).

According to this rationale, and even if the current experiments were not conducted among younger, non-adult participants, we could expect the effect size of social distance to diminish with increasing age. In other words, adolescents would be less prone to show this social distance bias (in direction of the self-serving tendency) than children; and adults even less than adolescents (in both effects' directions). Jennings (2019) found that children aged from 8 to 10 were sensitive to their relationship with the receiver in a multi-recipient dictator game (i.e., they tend to allocate more resources to close recipients compared to distant recipients and they were more ready to share equally with anonymous recipients). Gonzalez-Gadea et al. (2022) provided evidence of a differentiated effect of social distance

depending on participants' age. All participants (aged 6 to 11) showed ingroup protection as they punished more severely selfish behaviors directed against close (ingroup) victims than against distant (outgroup) victims. However, the authors reported a Black Sheep tendency (i.e., punishing more harshly ingroup than outgroup members for acting selfishly) only for younger children, with older children (10 and 11 years old) making no difference between ingroup and outgroup agent in punishment sentences (Gonzalez-Gadea et al., 2022).

Synthetic evidence is however much more ambiguous regarding this moderation of social distance effects across ages. In a meta-analysis on parochial cooperation by children (at intergroup level), Lazić et al. (2021) found an overall weak effect of social distance in direction of more cooperation toward ingroup (vs. outgroup) members (over an all-confounded population from 3 to 19 years old). This effect was however not significantly moderated by participants' age: social distance impact was not stronger for younger children than for older ones.

When studying social distance biases before adulthood, some pieces of research indicate that social distance would be on the contrary more impactful by older children than by younger ones. Marshall et al.'s (2022) findings indicate for example that younger children judged everyone as having a certain moral obligation to help (be it close or distant others), whereas older ones judged parents and friends as more obligated than strangers. As argued by the authors, this could stem from a certain process of *'moral narrowing'* like the perceptual narrowing occurring for babies at an early age. Babies view their perceptual capacity reduced very quickly after their birth and are then quickly influenced and conditioned by the culture they are living in (e.g., Kelly et al., 2007). In a similar way, we could think that, when growing up as a child, then passing to adolescence and to adulthood, some new reasoning processes would develop to specifically entail self-favoritism and then protect one's own self-esteem. The fact this is a competency that varies across world regions, as noticed above (i.e., less self-serving biases in Eastern cultures for instance), could indeed suggest this is an acquired (non-innate) competency.

It is also hard to believe that adults would never be sensitive to this bias. We already noticed various common examples of adults acting in such a biased self-serving way, starting from the illustrative example of the Juror N°2 movie (see Bègue-Shankland's analysis, 2024). As analyzed by Epley et al. (2004), who compared perspective-taking abilities among children and adults, the fact that children tend to act in a more egoistic way than adults does not result from adults being free from egocentric biases. According to them, a better explanation is that adults have learnt how to correct their first mind, hence appearing (and acting in the end) less egocentrically (Epley et al., 2004). Although a bit far from moral situations in comparison to our own current question of interest, the authors used an eye-tracking paradigm to demonstrate this. We can, here again, notice the relevance of dynamic techniques (such as eye-tracking or mouse-tracking) to detect early influences and their correction in real time during the decision-process (e.g., Spivey & Dale, 2006). Unlike adult participants, younger children were unable to correct their initial thoughts due to difficulties at that age in considering multiple perspectives, especially in cases of ambiguous interpretation (Epley et al., 2004).

Toward a Dynamic Way to Study Moral Decision-making

An Innovative Dynamic Technique: Mouse-tracking

In the final experiments of this dissertation (Exp.7, 8 and 1), we implemented and tested an innovative method to study moral decision-making in a more dynamic way, that is, via tracing back the whole decision process through hand movements and not merely through the final answer (see also Gautheron et al., 2023 for the paradigm implementation). This was a quite ambitious undertaking as mouse-tracking had only rarely been applied before to a moral context (see also Kiasoroush, 2023; Koop, 2013 for exceptions). However, this was also a promising endeavor, as it allows to follow the course of mind in real time (Freeman, Dale, et al., 2011; Spivey & Dale, 2006) and with minimal requirements in terms of device, costs, and implementation compared to other similar techniques as eye-tracking (Rivollier et al., 2020).

Constraints on Mouse-tracking Implementation

Mouse-tracking paradigm implementation does not come however without any constraints, in particular when developing such a task with moral materials (see Gautheron et al., 2023). Indeed, investigating morality often implies a significant proportion of textual information to display (as it was the case in almost all our experiments with scenarios). On the contrary, mouse-tracking tasks traditionally require short or visual stimuli (i.e., words or images), as early mouse movements could reflect the influence of the perceptual cues that have already been processed (Hehman et al., 2015). Therefore, and to avoid mouse cursor moving with reading processes, some adaptations were made in the currently used paradigm, in particular the one-dimension continuous slider with dynamic reduction over time (see Gautheron et al., 2023, Figure 6, for an illustration in the original version).

At a practical level, new tools have recently been developed to render the implementation of mouse-tracking paradigms more accessible. For instance, the MouseTracker tool from Freeman and Ambady (2010) directly allows to implement a classic mouse-tracking design and to run the most common mouse-tracking analyses. Another way to implement it, in a different setting, is the dedicated extension plugin of the JsPsych platform (de Leeuw, 2015), which however also remains constrained in terms of experimental design possibilities.

Relevance of Mouse-tracking to Study Moral Psychology

Despite these few drawbacks, we think mouse-tracking remains a promising tool to study moral psychological processes in a finer-gained way and to detect influences all along the time course. Moral psychology has indeed long relied either on self-report responses or on functional data—via event-related potentials (ERP), EEG, and fMRI techniques for instance—to investigate processes at play in moral situations. These last indirect methods allow to get more information than the mere final response (as for mouse-tracking), but often lack of temporal precision about the evolution of specific processes across time.

As an illustration regarding social distance effects in morality, Zhan et al.'s (2018) study—from which Singer et al.'s research (2019) was inspired from—found on behavioral

responses that people acted in a less altruistic way toward strangers than close ones. Through EEG recordings, they moreover found that, at an early stage of the decision-process onward, a specific brain area linked to cognitive conflict and negative emotional response was more largely activated when distant others were involved compared to close ones. Similarly, looking via an ERP technique to people's early expectancies of improbable and important events, Berg et al. (2021) found that expectancy violations were stronger for close others committing a wrongdoing compared to distant others. Hirschfeld-Kroen et al. (2021) also found discrepancies in specified neural areas activation (as measured via fMRI) between conditions where participants were themselves committing an accidental wrongdoing compared to those where they were only witnessing such a third-party accidental transgression.

While neural techniques provide valuable insights into the processes at play in moral situations, they are more invasive and costly to implement in comparison to mouse-tracking. Additionally, these neural methods often offer limited temporal precision, making it difficult to track the evolution of these psychological processes over time. Moreover, neural data alone often cannot reveal the direction of effects, requiring final behavioral responses for a correct interpretation. We could for instance well imagine that close ones' transgressions involve more emotional activation at a brain level but result in harsher punishment at a behavioral level. In contrast, mouse-tracking addresses these limitations, especially by enabling to visualize the evolution of processes dynamically over time, offering both temporal precision and direct interpretability of effects' direction. We therefore highly recommend and encourage the use of such an innovative tool to study moral psychology processes, as a way to get a more comprehensive view of cognitive and behavioral dynamics.

Broader Limitations & Perspectives

This work on social distance effects in moral decision-making suffers from some limitations—as for any other empirical and theoretical contribution. The following section stands to discuss some of the concerns readers could have regarding several aspects of this

dissertation. We will first review the potential theoretical limitations (mostly regarding social distance definition) before tackling the methodological shortcomings of this research. The last part of this section aims to provide perspectives about these various drawbacks to guide future research on this question.

Theoretical Limitations

On Social Distance Definition and Frontiers

First about the mere definition of social distance, as argued all along the theoretical part of this thesis, we considered social distance largely as a continuous dimension starting from the self to strangers and defined as the more or less intimate or proximal relationship one has with them. Some may doubt, in a first instance, about the integration of the self within this continuum of social distance, as we could imagine a continuum starting from the closest person to the self (as a real twin would be). Although not absurd as a reasoning, this would have hindered the fact that in some situations, the self can be directly involved within the situation (as their own judge). This proposition then allows to integrate the first level of analysis with self vs. others comparison (i.e., intraindividual level) within this definitional continuum of social distance, which would have been otherwise non-explicit yet underlying this conceptualization. This is also directly in line with the definitions (i) from psychological distance theory (Trope et al., 2007), which defines social distance as being socially more or less remote from the self; and (ii) from Aron et al.'s concept of inclusion of others in the self (1991).

Moreover, in our opinion, this definition makes explicit that even in situations where the self is not directly involved (as in studies reported at the interindividual or intergroup levels), with protagonists being more or less socially close to one another, we cannot ignore that these protagonists are also situated socially on this continuum with regard to the self-position. In other words, even if not manipulated per se, the social distance between oneself and the involved protagonists should—as much as possible—be controlled for. As an illustration, when a moral situation involves two roommates who are a priori not known from oneself, they would be considered both as socially distant. But as long as I have known one

of these roommates for a while, the other, by definition, is less of a stranger, as they are indirectly connected to the self. This is the difficulty we met in designing our experiments: adapting materials to ensure all distances are controlled for is often hard, sometimes almost impossible.

Second, by defining social distance as such, we included a wide panel of terms in our inventory of social distance acceptations, reflecting the various possible operationalizations we found in literature linked to social distance. This is also open to criticism, as one may argue that some of the inventory terms are not directly synonyms of social distance. For an illustrative case, it is true that the ‘genetic filiation’ terms are not directly reflecting the same idea as social distance. Both dimensions (i.e., genetic filiation and social distance) do not always match. An individual can indeed be very close genetically speaking (to their biological mother, sister, or daughter) but remain very distant socially from these people if they have been adopted for instance. However, in most cases, there is a general congruency between both dimensions, with genetically related individuals being used as an operationalized form of socially close persons. It therefore still made sense to include this dimension within the inventory, the aim of which was first to list the various operationalizations and terms used in literature for social distance. This was also the case for notions such as ‘liking’ (i.e., people we like are generally situated closer to oneself than people we dislike although not directly referring to the same concept as social distance).

Other concerns are related to dimensions included within the category of terms ‘referring to the self’, in particular those involving notions of perspective-taking and empathic concern. Again, those toward whom we have more empathic concern are supposed to be also socially closer. This is why we included these terms in the inventory. However, conceived as continuous scales of increasing vs. decreasing level of empathy and/or perspective-taking ability are very low characterizing and indirect ways to operationalize and/or to measure social distance. We did not therefore include in the scoping review the studies with this type of manipulation or measurement of social distance. Indeed, feeling

temporarily empathetic to another's fate due to a photo inducing pity for instance may not be the most suitable way to implement a close relationship with that person.

Another potential limitation or discussion that could emerge regarding this continuum of social distance is about whether our enemies (or simply outgroup members) should be located at the extreme distant side of it instead of mere strangers. This question again indirectly refers to the question of orthogonality between social distance and the valence of the relationship. Although often correlated (i.e., more probable that a disliked or outgroup person is located at a more distant point of the continuum), we think these are two distinct dimensions. One could indeed consider their relationship with an outgroup member as still closer than relationship with a stranger, in accordance with the 'know your enemy' sermon. This is why we kept on the extreme distant side of the continuum what is supposedly the more distant instances in general (i.e., strangers), independent from the valence of the relationship toward them.

Finally, as noted earlier in the theoretical part of this dissertation, isolating social distance as the unique parameter varying in the moral situation remains challenging, given the partial overlap—and often strong correlation—with the other dimensions of psychological distance. As an example, intergenerational research involves both social distance and temporal distance; situated intergroup conflict (as in the current Israeli-Palestinian conflict) often deals with both social and spatial distance. More directly related to the current investigation, dealing with everyday moral scenarios compared to less plausible situations (as trolley-like dilemmas) involves the hypothetic dimension of psychological distance (also called likelihood). We cannot ensure that even within the everyday moral materials that have been presently used, there was no variation in terms of hypotheticality: For example, ordering a drink at a bar is typically more likely when it is for a friend or a colleague than for a complete stranger. All these possible variations and confounds may help explain in the end why social distance effects are so confusing and hard to pin down.

Methodological Limitations

Difficulty to Control for All Distances

A first limitation in terms of methodological aspects is directly linked with what has been just said above, that is, the difficulty to perfectly control for the various social distances between all protagonists and the overlap with other dimensions of psychological distance. It is specifically true for moral situations, as it often involves more than one protagonist, each having often a different role in the situation. As we have seen along the experimental part of this dissertation, social distance is often hard to manipulate per se in a quite ecological way (via the reference to participants' real close ones) while still controlling for the other distances at play. It was the case in Experiment 1, where we wanted to manipulate social distance between participants and the agent of the situation, whereas we made varying involuntarily at the same time their relationship toward the victim. In the next experiments, we carefully controlled for this relationship, but this required to provide other minor modifications to ensure consistency. For instance, when scenarios involved 'roommates' or 'colleagues', we adapted them with 'a new roommate or colleague' so that the participant did not infer from the situation that he/she would have known the victim (in the same logic as 'the friends of my friends are my friends too'). These minor modifications should not have entailed many changes in the scenarios' understanding, but we cannot ensure this did not play a role at some point. This was however not perfectly controlled in the original material either (Leloup et al., 2018), with protagonists not being always strangers between them.

Another way we found to control this aspect more rigorously was through minimal group paradigm in Experiment 3 notably, that is, creating ad-hoc groups (based on a minimal criterion) and using this group membership for agents' and victims' identities to induce social distance. This was however not without drawbacks, as it quite hampered the realistic aspect of the task. We attempted to create a more ecological minimal group paradigm in Experiment 4, which did not demonstrate the expected effects, probably due to other problematic aspects of the task (see discussion in Chapter 5, pp. 171–172).

In future research, we might therefore adapt the current battery of moral situations to make them having either no directly identifiable victim or no directly identifiable agent (which both may be difficult for some scenarios). Another possibility would be to make the agent

and the victim as the same person (e.g., Anna put the gloves that she thought would be thick enough to weld pieces together, but then she burnt herself despite the gloves). But this would even more confound both statuses of agent and victim (as in Kouabenan et al., 2001), hence making it even harder to infer what social distance effect we are testing precisely. Making the agent or the victim not explicitly mentioned (or even non existing when possible, depending on scenarios) may also induce other theoretical issues, as for whether we should consider animals and/or the environment as 'victims' when they are harmed, and therefore whether social distance should also be considered when it varies toward non-human entities.

Low Ecological Validity of Moral Situations

We also have to acknowledge that the moral materials we used in the current experiments are mostly composed of hypothetical situations. Even if we targeted everyday moral situations (i.e., that could be encountered in real life, hence more plausible than trolley-like dilemmas for instance), they remain, on average, low-ecological situations. Participants indeed knew in all experiments presented above that they were participating to a psychology study, hence their potential lack of natural reactions and responses—as if they were experiencing these situations in real life. Experiment 4 with the online multiplayer game (i.e., without scenarios) was designed notably to palliate this low-ecological aspect of our previous experiments. However, as noticed above, it was not successful as the rules and purpose of the game as presented in Exp.4 were not fully understood by participants.

All in all, the lack of ecological paradigms represents an important limitation of the current investigation, as well as of moral psychology more broadly. This may imply that effects of social distance in real-life contexts would have been more pronounced, as indicated by previous research conducted in organizations (see Kouabenan et al., 2001). However, we can imagine that this difference in social distance effects would not have been much greater than the one between choices of action versus judgments, provided we had included a similar diversity of situations as in our hypothetical scenarios. In our view, these two gaps reflect a comparable issue: choices of action are more likely perceived by

participants as more vivid and realistic (that is, more ecological) than mere abstract judgments, probably because they involve a stronger sense of projecting oneself into future consequences. Ultimately, while the lack of ecological validity is an important concern, it should not be overstated, given that hypothetical scenarios also offer greater experimental control and consistency across participants, an advantage that remains essential for isolating specific psychological phenomenon.

There was also another advantage to keep hypothetical situations: We could easily multiplicate the number of situations one participant had to judge, in order to increase reliability and generalization over various everyday moral situations. This would have been much more complicated in an ecological real-life experimentation. This is one long standing difficulty in moral psychology to study moral decision-making at the same time in an ecological way and with the possibility to generalize to other similar situations. New techniques are emerging as virtual (or augmented) reality, which allows to reproduce a virtual but realistic environment for participants. Some research in moral psychology applied it already to trolley-like dilemmas (e.g., Francis et al., 2016; Liu et al., 2025; Niforatos et al., 2020; Skulmowski et al., 2014; Wang et al., 2023). Others are directly applied to social distance biases reduction via the manipulation of perspective taking in virtual reality settings (e.g., Chen et al., 2021; Herrera et al., 2018). However, it remains an expensive technique, not easy to implement, which moreover does hardly resolve the issue of generalization (as new environments for each of the 60 scenarios of our database would be very costly and long to develop).

In the end, this problem of the low ecological aspect of our experimental tasks also questions whether we believe participants answer to these fictional situations in an authentic way—beyond the fact that social distance effect could be more subtle and consciously corrected than previously assumed. Some argue that self-report measurements are, in the end, often more accurate to reflect people's mind than what has been summed up as 'implicit measures' (Corneille & Gawronski, 2024). Although still submitted to social desirability biases, people may be more accurate by reporting anonymously in self-report

what they think they would do confronted to such situations or how they would judge these situations as a reflection of their own moral processes than in real life. Indeed, people do not always act in real life as they would like, due to unforeseen circumstances or to the vagaries of life. Behind this also lies the question of whether what we're trying to find out is how people actually act in moral situations, or how they think they should act according to their personal moral convictions.

Non-Naivety of Participants

Despite our best efforts to make the task dealing with moral decisions in general and not specifically orientate participants on social distance as the factor of interest, we may think they were not fully naïve about our hypotheses. Indeed, most of our experiments were conducted in a within-participant design, hence participants judging both close and distant conditions of social distance (on different scenarios, randomized in order). We could think this may have an impact on how they respond to these situations, notably once they have already answered to both conditions. When people are aware to some extent that we want to compare their judgments between both types of situations (involving close vs. distant others), they could (in)consciously correct their first idea of response to appear as impartial and moral beings. We did not observe any such patterns of response correction during the decision process in the mouse-tracking data analysis, nor did participants report such tendencies in debriefing sessions.

One way to ensure this within-participant design did not impede the visualization of social distance effects would be to either (i) test the same paradigm in a between-participant design (although much more costly to settle as only weak social distance effects were found for the moment with these materials) or (ii) implement this moral task in a within-participant design, but counterbalanced per block of scenarios (and not per scenario directly). That is, participants answering first to half of the scenarios in the close condition, then the other half in the distant condition (and same in the reversed order for other participants). This latter suggestion is currently being tested in a quite different experiment but with the same material (on severity and part of external circumstances measurements). Once data is

collected, we will be able to compare the effect of social distance for all participants on the first half of the task only (between-participant-like design) and the same effect for all participants on the second half of the task (i.e., where they should be aware of what happened before).

Perspectives

In front of these limitations, that apply to the current research as well as to many other investigations in the field, we propose several ways to move forward. First, we re-highlight the necessity to take as much as possible a pluri-level integrative framework to analyze social distance, as its effects are multifaceted (at least in the theoretical rationale, with then the possibility to focus experimentally on a sub-area of application only). We also encourage researchers studying this question to control as much as possible social distances between all involved protagonists. Even if not always easy to pursue as an ideal while still making the situations realistic and plausible, this is of crucial importance to get a clear view on what guides such effects. In the same idea, distinguishing manipulations of social distance to the agent and manipulations of social distance to the victim of the situation (as we did in Chapters 5 and 6 respectively) is essential as they could reflect two distinct directions of social distance effects.

In the current research, we voluntarily avoided to tackle the effects of social distance at the neural or the inter-cultural levels (mostly because this was not the focus of our experimental project). We do however encourage researchers working at these levels of analysis to integrate their findings and confront them to the current ones. More generally, we selected and analyzed only a portion of studies found during the process of scoping review, focusing especially on dark morality, on adult participants, and on experimental research. The sample of studies to include was indeed very large without these criteria. However, we kept the pre-selected database of relevant articles for the current topic of interest, with the idea that it could serve future research interests. This is indeed an open path and opportunity for future research to seize this pre-selection of articles in order to deal with this

question of social distance influence in moral decision-making at other levels and/or applied to other contexts of analysis (i.e., with children only, with pro-social morality only etc.).

Regarding the methodological aspects to investigate social distance influence, we suggest using as far as possible plausible (i.e., everyday-like) and realistic hypothetical situations. We acknowledge this is not an easy task as most realistic situations are the ones really experienced by participants, and this constrains at the same time the number of situations being judged. We still maintain yet that in order to be able to generalize to other similar situations, researchers should not only select a few situations but test the effects of social distance on a wide range of various scenarios. As we have seen earlier, effects of social distance seem to vary a lot (in direction and size) between situations (although similar in construction). To combine both requirements, one way forward could be to foster the development of a variety of *everyday* moral scenarios (and less of low plausible trolley-like dilemmas) in virtual reality, although the costs and limitations of it are not negligible (e.g., Laurell et al., 2019; Li, 2015).

Finally, we think it would be relevant to continue developing dynamic techniques to study social distance effects in moral decision-making, as our mouse-tracking investigation was a promising and accessible, yet not fully exploited initiative (as we merely tested it on our first non-ideal operationalization of social distance). We therefore vividly recommend the replication of mouse-tracking paradigms applied to the study of social distance in morality, but with more adequate forms of social distance (e.g., ingroup vs. outgroup members or friend vs. stranger while controlling for the other protagonists' distance) and without mixing other key factors (as intent and causality in our Exp.1).

Concluding Remarks

This dissertation thesis started with the—seemingly—quite simple question of whether people would be more or less lenient when judging a close one (vs. a stranger) in case of moral wrongdoing. Remember the first example we gave in the preamble, with your own son being accused of rape or murder, how would you react? Would you still stand by him body and soul? This questioning eventually evolved into a much more complex and

broader issue during this dissertation. We first noticed that it did not only involve one literature of close relationships' impact on moral decisions, but also multiple other twin literatures such as ingroup/outgroup prejudice; social discounting; actor-observer bias (or first- vs. third-person perspective) and so on. Across the study of these various literatures, we further found there were two opposite tendencies of findings, summed up earlier as self-serving vs. other-serving tendencies (Tendencies 1 & 2, respectively). Although results more frequently indicated a general favoritism for self and close ones (as shown descriptively in the scoping review preliminary results), we did not find a precise criterion of why and how the effects of social distance could be reversed in some specific contexts (see the Black Sheep effect in particular). Moreover, an integrative framework investigating this question in all identified twin literatures (i.e., at intraindividual, interindividual, and intergroup levels of analysis) was lacking.

To tackle this research question, we experimentally tested at all three levels of analysis the effects of social distance on moral decision-making, both when manipulated toward the agent of the immoral act and when manipulated toward the victim of such wrongdoing. Across six different experiments (Experiments 1 to 6), we found ambiguous and contradictory results regarding the effects of social distance, either pointing toward a self-serving tendency or an other-serving tendency, even suggesting no significant effect for some (in particular at intergroup level). Testing some of the potentially crucial parameters for social distance (such as choice vs. judgment response, severity, responsibility etc.) did not further explain its differentiated effects on moral decision-making. Despite these still ambiguous findings, we argue that the use of a threefold analytical framework which allows to combine plural levels of investigation for social distance remains fruitful for future research on this question. It also ensures more commensurable findings (both for past results and future research), hence promoting a more cumulative psychological science (Almaatouq et al., 2022). We also further encourage researchers to use more dynamic methods of investigation—such as currently done via mouse-tracking (Experiments 7 and 8)—to detect early and/or more subtle influences of social distance than previously assumed.

Ultimately, the effect of social distance should not be overestimated in magnitude, particularly when assessed through a limited set of moral scenarios. This may constrain the generalizability of the findings to broader moral contexts, while they often trigger considerable variability. In the end, what appears as a strong and robust effect in one situation might fade or shift in another, reminding us that moral decisions are rarely one-size-fits-all.

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