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Three essays in experimental economics : plant-based diets and Registered Reports

Thibaut Arpinon

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Thibaut ARPINON

Three essays in experimental economics: plant-based diets and Registered Reports

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Unité de recherche : CNRS UMR-6211 CREM

Rapporteurs avant soutenance :

Paolo CROSETTO Directeur de recherche INRAE, GAEL

Jan STOOP Professeur associé, Erasmus University

Composition du Jury :

Président : Laurent DENANT-BOEMONT Professeur, Université de Rennes

Examineurs : Paolo CROSETTO Directeur de recherche INRAE, GAEL

Jan STOOP Professeur associé, Erasmus University

Nicolas TREICH Directeur de recherche INRAE, TSE

Marianne LEFEBVRE Maitresse de conférences, Université d'Angers

Lisa KRAMER Professeure, University of Toronto

Directeur de thèse : Fabien MOIZEAU Professeur, Université de Rennes

Co-directeur de thèse : Romain ESPINOSA Chargé de recherche CNRS, CIRED

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Résumé

Cette thèse vise à contribuer au champ de recherche de l'économie expérimentale par l'étude des *i) comportements associés à l'alimentation végétale* et *ii) nouvelles méthodes de science ouverte*. La première partie, *i) comportements associés à l'alimentation végétale*, utilise les méthodes de l'économie expérimentale pour identifier et lever les freins à la végétalisation de l'alimentation. Malgré l'accumulation de preuves scientifiques sur le lien entre la végétalisation de l'alimentation et la réduction des externalités négatives (environnementales, sanitaires et de bien-être animal), la transition vers les alimentations végétariennes et végétaliennes tend à être limitée par des biais sociaux et cognitifs. Dans le chapitre 1, le travail de recherche vise à identifier un cas spécifique de frein à la transition vers l'alimentation végétale, connu sous le nom de végéphobie, en utilisant les outils développés en économie expérimentale. Le chapitre 2 s'intéresse au traitement de l'information des médecins généralistes en France par le biais d'une campagne d'information sur l'alimentation végétale. Une importance particulière est accordée au processus de traitement de l'information en rapport avec l'alimentation végétalienne. La deuxième partie, *ii) nouvelles méthodes de science ouverte*, propose une initiative visant à répondre au questionnement des pratiques statistiques en économie expérimentale et aux biais engendrés par le système académique.

Le chapitre 3 discute les aspects méthodologiques liés aux *Registered Reports*. Ce nouveau format d'article scientifique vise à développer des pratiques de recherche plus vertueuses et cohérentes avec la théorie statistique, et à favoriser la répliquabilité des résultats au travers d'une méthodologie rigoureuse.

Chapitre 1 - Identification des freins à l'alimentation végétale : le cas de la végéphobie

Le chapitre 1 de la thèse porte sur une expérimentation en ligne visant à identifier l'existence de "végéphobie" (discrimination envers les végétariens et végétaliens). L'adoption d'une alimentation végétale stricte est associée à l'endossement d'une identité sociale supplémentaire chez les individus et à une disruption de la norme sociale. Paradoxalement, alors que la végétalisation de l'alimentation est associée à une diminution des externalités négatives (envi-

ronnementales, sanitaires et de bien-être animal), les végétariens et végétaliens déclarent subir un coût social sous la forme de discrimination de groupe. Ce phénomène a été mis en avant par des travaux en psychologie et en sociologie comme frein à la végétalisation de l'alimentation. Ce chapitre propose la première approche de mesure des biais de groupe engendrés par l'arrêt de la consommation de viande, en utilisant les méthodes d'élicitation des préférences sociales dans une expérience économique.

Dans cette optique, 1483 participants américains sont recrutés en ligne via la plateforme de sondage *Prolific*. La tâche principale de l'expérience est une version modifiée du jeu du dictateur, dans laquelle les dictateurs doivent décider de quinze allocations de budget entre eux-mêmes et un receveur. L'expérience suit un design inter-sujet. 1158 participants omnivores¹ jouent le rôle de dictateur et sont aléatoirement assignés dans un traitement selon l'identité du receveur. Dans le premier traitement, les dictateurs sont informés que leur partenaire a une alimentation omnivore, dans le deuxième une alimentation végétarienne et dans le troisième une alimentation végétalienne.² Les 278 participants restants jouent le rôle de receveur et sont assignés dans l'un des trois traitements, selon leur identité alimentaire. Les receveurs sont informés que leur partenaire a une alimentation omnivore.³ En leur qualité de receveur, ils n'ont pas de rôle actif dans le jeu du dictateur et ne prennent donc pas de décisions. En revanche, *a posteriori*, les receveurs visualisent les quinze allocations de budget auxquelles ont fait face les dictateurs et doivent prédire, pour chaque allocation, le choix du dictateur. Les participants à l'expérience reçoivent une compensation financière pour leur participation, couplée avec une opportunité de gain supplémentaire dans le jeu dictateur. Le protocole expérimental ainsi que les analyses ont été pré-enregistrés sur *Open Science Framework*.

La végéphobie est quantifiée en estimant les paramètres d'aversion à l'inégalité (Fehr and Schimdt, 1999) des dictateurs. La sensibilité des paramètres d'aversion

¹Les participants recrutés ne savent pas qu'ils ont été recrutés selon leur alimentation afin de ne pas dévoiler l'objectif de l'expérience et de limiter les biais de comportements.

²Cette information est donnée en sus à d'autres informations sur l'identité du receveur.

³Cette information est donnée en sus à d'autres informations sur l'identité du dictateur. Les receveurs sont aussi informés que le dictateur a reçu des informations sur leur identité avant de réaliser les allocations de budget. Dans l'expérience, les dictateurs et les receveurs ont les mêmes caractéristiques, la seule différence étant l'identité alimentaire, selon le traitement.

à l'inégalité désavantageuse (envie) et d'aversion à l'inégalité avantageuse (culpabilité) est comparée entre les traitements. Parallèlement, la végéphobie perçue des receveurs est estimée par le biais de l'estimation de paramètres d'aversion à l'inégalité perçue. En accord avec la littérature, deux hypothèses directionnelles sur l'envie des dictateurs sont pré-enregistrées pour l'analyse confirmatoire et le reste des résultats sont laissés en analyses exploratoires. L'analyse confirmatoire n'est pas cohérente avec les hypothèses pré-enregistrées et ne détecte pas de végéphobie. Ces résultats sont robustes à des manipulations supplémentaires pré-enregistrées. Les résultats exploratoires révèlent la présence de végéphobie sous la forme de culpabilité moins importante des dictateurs envers les végétariens. Au niveau individuel, la végéphobie dépend des caractéristiques individuelles et de l'environnement social des dictateurs. Paradoxalement, les végétaliens ne s'attendent pas à être discriminés dans le jeu du dictateur mais déclarent subir de la discrimination quotidiennement en raison de leurs choix alimentaires. Les résultats du chapitre suggèrent que la méthode expérimentale mobilisée ne permet pas de détecter la végéphobie, qui est pourtant rapportée par les végétaliens et observée dans d'autres disciplines. Ce résultat laisse penser que les travaux qui mesurent la discrimination par des jeux économiques sous-estiment l'importance du phénomène.

Chapitre 2 - Éliminer les barrières à l'alimentation végétales : informer les médecins généralistes français

Le chapitre 2 de la thèse s'intéresse aux connaissances des médecins généralistes français et à leur accompagnement de patients souhaitant adopter une alimentation végétalienne. Les médecins généralistes sont une source d'information immédiate en termes de nutrition et peuvent influencer la décision d'un patient de poursuivre, ou non, la végétalisation de son alimentation. Malgré les enjeux sanitaires, environnementaux, et de bien-être animal, les médecins freinent potentiellement leurs patients par le biais de conseils inadaptés. Ce phénomène est accentué par le manque de formations nutritionnelles spécialisées dans le cursus de médecine français. L'acquisition d'information par les médecins généralistes est aussi limitée par leur contrainte de temps et par des biais cognitifs généraux liés à la consommation de viande, tels que le biais de confirmation, la résistance à l'information ou encore la dissonance cognitive. L'objectif du travail de recherche présenté dans ce

chapitre, rédigé sous la forme d'un *Registered Report*, est de tester l'impact d'une campagne d'information sur l'alimentation végétale auprès des médecins. La campagne d'information, élaborée par l'équipe de recherche composée de médecins spécialistes, est distribuée sous la forme d'un livret et son impact est mesuré au travers de l'étude cas cliniques.

Un échantillon représentatif de 400 médecins généralistes est recruté pour participer à l'expérience. Les médecins sont aléatoirement divisés en deux groupes. Dans le groupe de contrôle, 200 médecins répondent à un questionnaire en ligne avec des cas cliniques portant sur un patient souhaitant adopter une alimentation végétalienne. Ce questionnaire vise à mesurer l'avis général des médecins sur l'alimentation végétalienne, les pratiques avec un patient végétalien, et les actions pro-actives de diffusion d'information sur l'alimentation végétalienne. Dans le groupe de traitement, les 200 autres médecins sont exposés à la brochure d'information puis répondent au questionnaire.

Les résultats confirmatoires démontrent un effet positif de la brochure d'information sur l'avis général des médecins. Concernant les pratiques des médecins avec un patient végétalien, l'impact de la brochure est positif mais l'effet observé est inférieur à la taille d'effet minimale d'intérêt, ne permettant pas de conclure de manière certaine au succès de la brochure. L'impact de la brochure sur les actions pro-actives de diffusion d'information n'est pas analysé par manque de pouvoir statistique. L'analyse exploratoire ne révèle aucune hétérogénéité de l'effet de traitement à l'exception du temps passé sur la brochure. L'impact positif de la brochure est plus important pour les médecins ayant dédié plus de temps à la lecture. Une analyse descriptive des données révèle que les médecins surestiment leurs connaissances en matière d'alimentation et qu'ils sont conscients de leurs effets négatifs potentiels sur les patients souhaitant adopter une alimentation végétale. Les résultats du travail de recherche démontrent qu'une intervention informative courte a des effets positifs importants. La généralisation et la répétition de ce type d'intervention, en parallèle avec une amélioration du cursus de médecine sur l'alimentation, ont le potentiel d'améliorer la prise en charge des patients végétaliens et ainsi faciliter la transition vers l'alimentation végétale.

Chapitre 3 - Un guide pratique à la mise en place des *Registered Reports* pour les économistes

Le chapitre 3 de la thèse est dédié à l'amélioration de la méthodologie en économie expérimentale au travers des *Registered Reports*. Ce chapitre est une version longue d'un article publié dans la revue *Journal of the Economic Science Association*.

Le système de publication en économie est touché par des biais de publication et de citation, qui se traduisent par une sur-représentation des résultats statistiquement significatifs. La présence de ces biais incite les chercheurs à employer des pratiques de recherche controversées telles que le p-hacking, le HARKing, le forking ou l'abandon de travaux de recherche aux résultats négatifs. La conséquence de ces pratiques est une perte de crédibilité des résultats scientifiques avec une mauvaise accumulation de connaissances ainsi qu'une incapacité à répliquer les résultats. Une solution proposée pour contrer ces pratiques est le pré-enregistrement. Le pré-enregistrement consiste à pré-spécifier les analyses qui vont être réalisées avant que la collecte de données ne soit effectuée, en déposant un document sur une plateforme en ligne dédiée. Cependant, le pré-enregistrement ne dispose pas d'un ensemble de règles pré-définies et les chercheurs jouissent d'un degré de liberté important, ce qui peut limiter les bénéfices espérés. Alors que le pré-enregistrement gagne en popularité en économie, il s'avère être une solution limitée à cause de son inefficacité à prévenir les biais de publication et l'abandon de travaux de recherche donnant des résultats négatifs.

Le *Registered Report* est un nouveau format d'article dans lequel la priorité est donnée au processus scientifique plutôt qu'aux résultats. Les chercheurs rédigent leur travail de recherche jusqu'à la partie résultats, sans collecter de données, et soumettent l'article pour obtenir une première évaluation par les pairs. La première version du travail de recherche consiste à rédiger l'introduction, la question de recherche, la partie théorique, les hypothèses, le processus de collecte des données et l'analyse des données, sans avoir collecté de données (hors données pilotes). Une fois la première étape complétée, les chercheurs obtiennent un accord de principe de la revue scientifique qui garantit la publication de l'article, indépendamment

des résultats.⁴ Les chercheurs procèdent ensuite à la récolte de données, rédigent la section résultats et re-soumettent pour publication dans le journal. La popularité des *Registered Reports* dans la recherche scientifique est grandissante, et le domaine de l'économie se met à niveau avec, à ce jour, dix journaux économiques acceptant ce type de soumission.

Le chapitre 3 vise à fournir les éléments indispensables à la rédaction d'un *Registered Report* pour les économistes, avec un focus sur l'économie expérimentale. Les éléments principaux d'un *Registered Report* tels que le plan d'analyse, l'analyse de pouvoir statistique, la correction du seuil de significativité, la taille d'effet minimal d'intérêt, les explorations et déviation du plan d'analyse, le niveau du *Registered Report*, la table de design de l'étude, l'approbation éthique, et le choix du journal optimal sont explicités et détaillés. Ces éléments sont accompagnés d'exemples spécifiques en économie expérimentale, ainsi que de codes R et Stata pour faciliter la mise en place.

⁴Une condition nécessaire est le respect du protocole décrit lors de la première soumission.

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General Introduction

This dissertation contributes to the experimental economics literature by studying the social and behavioral factors associated with the adoption of a plant-based diet, as well as discussing new Open Science methodologies. The introduction first provides context on the relevance of experimental economic tools to study the implications of adopting a plant-based diet. Second, it presents some of the limitations that experimental economics is facing, partly due to the recent lack of replicability, and discusses alternatives to enhance the quality of evidence. Lastly, it describes the three chapters individually along with their contributions to the literature.

Experimental economics and plant-based diets

Are individuals rational? Standard microeconomic theory suggests so. Decisions are modeled following the Homo economicus, a perfectly rational and selfish individual, with utilitarian principles, who is capable of playing around with probabilities to maximize outcomes (Levitt and List, 2008). This theoretical assumption is convenient to model individual behaviors and is applied to many subfields in economics. Subsequently, economists started to question the Homo economicus and developed tools to test the veracity of the assumption, leading to the development of experimental economics. In particular, the release of *Theory of games and economic behavior* by Von Neumann and Morgenstern (1947) and the concept of expected utility theory provided a framework for economists to test rationality with simple experiments. The first major contributions to experimental economics, such as the Allais paradox (Allais, 1953), questioned the foundations of decision theory as presented by expected utility. Interestingly, the beginnings of experimental economics stemmed from interdisciplinary collaborations combining mathematicians, psychologists, and economists, rather than a strictly economical approach (Guala, 2010).

The interdisciplinary approach broadened the horizon of experimental economics but led to an untraceable and chaotic development for a couple of years. Smith (1976) then unified the field and defined the relevance of laboratory settings to studying economic theories. Simultaneously, Tversky and Kahneman (1974) further challenged the theoretical economic conception that humans are rational individuals and initiated the field of behavioral economics with prospect theory (Kahneman and Tversky, 1979). From this point on, experimental economics and behavioral economics, although different, have become intimately related in testing individuals' deviations from rationality (Serra, 2017). Major contributions to behavioral economics, to mention a few, include regret theory (Bell, 1982, Loomes and Sugden, 1982), endowment effect (Kahneman et al., 1990), bounded rationality (Simon, 1997), and hyperbolic discounting (Thaler, 1981, Elster, 1979). The accumulation of evidence led to extending the range of behavioral economics through experimentation.

The conjoint evolution of experimental and behavioral economics allowed experimentalists to develop theories based on observations and experimentally test their accuracy, while also proposing updated utility models built upon stylized facts. Some examples include identity economics (Akerlof and Kranton, 2000), prosocial behaviors (Bénabou and Tirole, 2006), and social preferences (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000, Charness and Rabin, 2002). The development and findings of experimental economics, with applications to behavioral economics, demonstrated that it is a powerful tool to study individual economic behaviors when monetary incentives are involved (Jacquemet et al., 2019). From this point on, experimental economic designs and theories have evolved and have been used in more and more applied areas to study the tenants of individual behaviors in situations involving interactions, such as financial contributions to public goods (Marwell and Ames, 1979, Andreoni, 1990), donations (Forsythe et al., 1994), trust (Berg et al., 1995), group bias (Fershtman and Gneezy, 2001), beliefs (Charness et al., 2021), and behavior correction through nudges (Thaler and Sunstein, 2009). In the last decades, experimental economists have broadened the range of possibilities by exporting the lab settings to field experiments via randomized controlled trials (Dufflo, 2020), online experiments (Arechar et al., 2018), and introduced practices from other disciplines such as neuroeconomics (Montague and Berns, 2002, Ross, 2008). The scope of experimental economics applications seems unbounded.

Over the last few years, experimental economists have started applying the ex-

perimental methodology to study individual economic decisions in relation to food choices. Indeed, individuals make, on a daily basis, food choices that are determined by economic factors (Drewnowski and Darmon, 2005). The influence of economic factors on food choices is often captured using aggregated data (Park et al., 1996, Andreyeva et al., 2010), which only provides an overview of food trends. However, food choices are also co-determined by individuals' physiological,⁵ cultural (Leng et al., 2017), and emotional factors (Gibson, 2006), as well as individual preferences and behavioral components (e.g., cognitive biases) that are difficult to capture with aggregate estimates. Food choices entail a complex scheme that extends beyond the scope of a simple choice of consumption. Therefore, the combination of economic and behavioral factors provides an ideal setup for experimental economists. Specifically, the experimental approach is pertinent as it allows the study of individual decisions, rather than aggregate, in controlled environments. This method is particularly relevant to measure the weight of behavioral components on food choices as researchers can induce exogenous variations while controlling for economic characteristics.

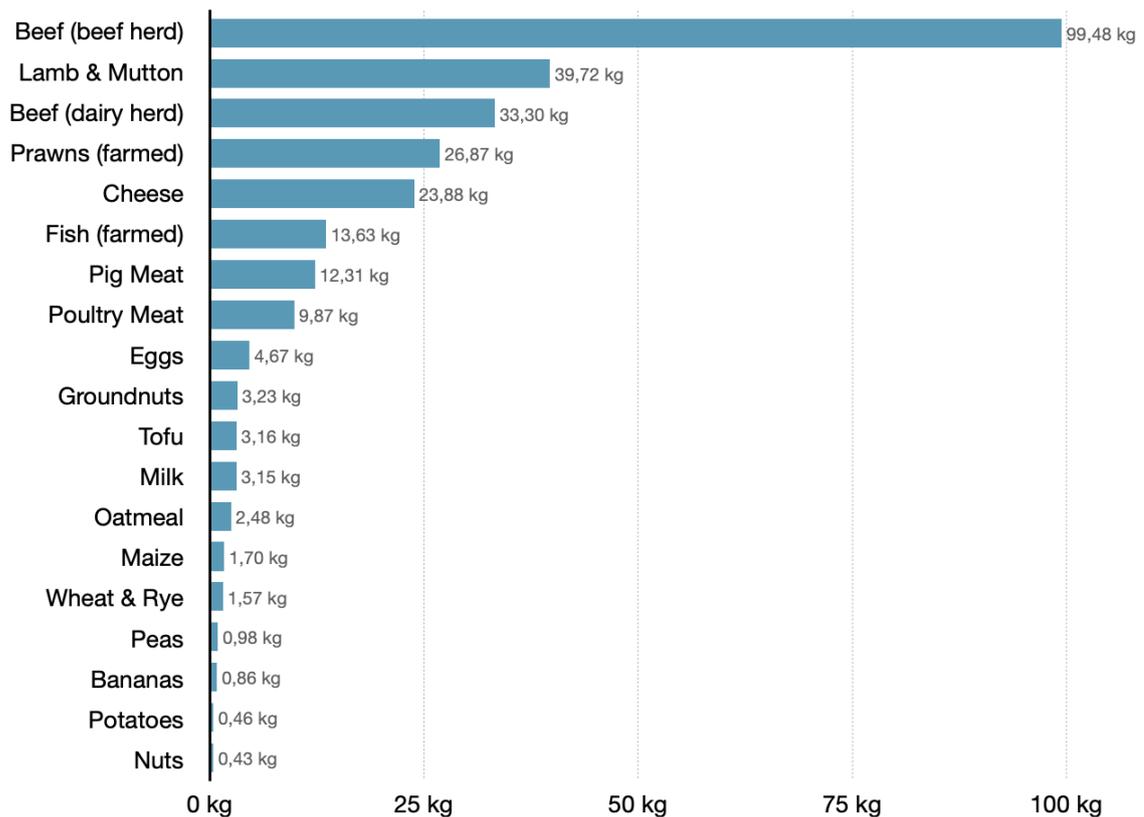
The experimental economics approach to food choices is popular and has been applied to test the impact of food labels (Crosetto et al., 2016, 2020) and nudges (Kroese et al., 2016), as well as estimate the willingness to pay for novel foods (Roosen et al., 2015). Additional examples include testing social preferences predictions (Chang and Lusk, 2009) and time preferences (De Marchi et al., 2016) on food-related choices, along with assessing the role of social norms (Bunten et al., 2021) and confirmation bias (Dickinson and Kakoschke, 2021). Similarly to the beginnings of experimental economics, the approach to food consumption is often interdisciplinary, mixing methodologies from economics, psychology, nutrition, and medicine, and drawing conclusions on economic behaviors. For the experimentalist, these results contribute to a better understanding of the underlying mechanisms surrounding food choices, which is essential to provide accurate public policy recommendations and optimally guide individuals toward healthier and more sustainable food consumption. Specifically, experimental economics might contribute to solving one of the greatest challenges of the 21st century: animal-based consumption.

The current food system raises concerns regarding the sustainability of human diets. From an environmental perspective, it is one of the main drivers of climate

⁵The influences of ghrelin, leptin, and insulin are particularly important for food choices (Leng et al., 2017).

change (Vermeulen et al., 2012), freshwater use (Wada et al., 2010), and land use (Foley et al., 2005). Among food products, animal-based products have the highest greenhouse gas emission per kilogram (see Figure I.1). As the earth’s population increases, the current trajectory will lead to an estimated 80% increase in agricultural greenhouse gas emissions by 2050 (Tilman and Clark, 2014) and will amplify the negative environmental consequences (Willett et al., 2019). Current diets are also associated with negative impacts on human health (Springmann et al., 2018). Improvements in diet quality have the potential to reduce premature deaths by 24% (Wang et al., 2019) and global mortality by 6 to 10%, while also reducing health costs by 0.4 to 13% of the global gross domestic product (Springmann et al., 2016). Overall, current food consumption patterns are referred to as lose-lose diets (Garrett, 2016) as a result of their detrimental consequences to the environment and human health.

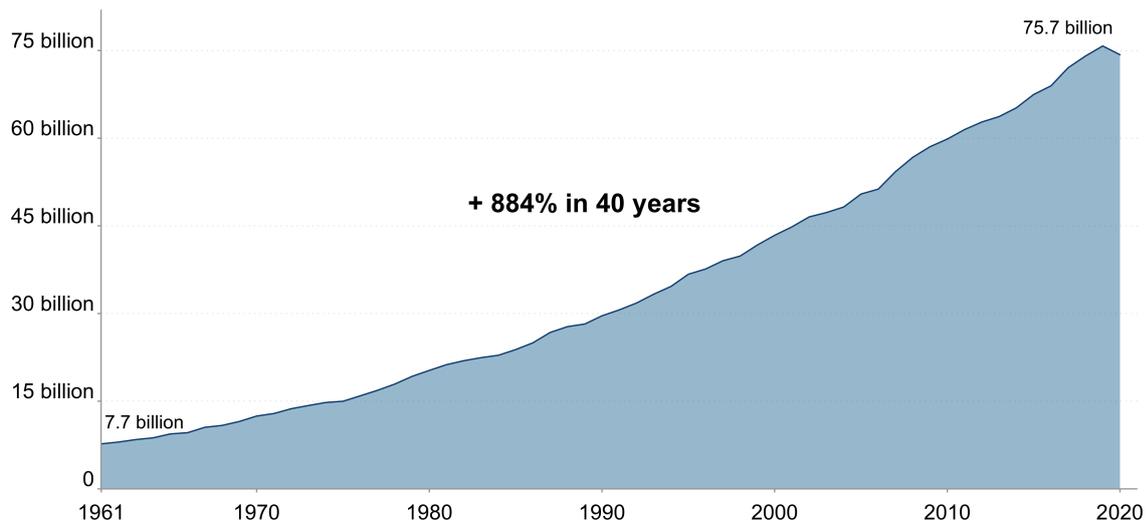
Figure I.1: Greenhouse gas emissions per kilogram of food product.



Source: Our World in Data.

Moreover, current food consumption also impact animal welfare. Figure I.2 represents the evolution of the number of animals slaughtered in the world from 1961 to 2020. In total, over 75 billion animals were slaughtered for their meat in 2019, representing an 884% growth from 1961. This number is expected to rise in the upcoming years (Godfray et al., 2018), which will automatically increase the number of factory-farmed animals, and further degrade total animal welfare. The animal slaughtering upward trajectory is in contradiction with the expanding literature on animal sentience⁶ showing that animals are complex cognitive creatures able to feel physical and emotional pain (Sneddon et al., 2014, Broom, 2016). Even though there is no clear-cut evidence on whether an animal’s death causes pain, one may reasonably assume that the living conditions of animals in factory farms are detrimental to animal welfare.⁷ A switch to greener dietary patterns could help mitigate some of the downsides of animal-based consumption.

Figure I.2: Number of animals slaughtered for food per year worldwide, from 1961 to 2020.



Note: The numbers are based on livestock production for meat and do not include dairy or egg production, nor seafood. The decrease in 2020 is most likely due to the impact of the covid crisis on food production. Source: Our World in Data and author’s own calculations.

⁶The Cambridge dictionary defines sentience as “the quality of being able to experience feelings”. Animal sentience refers to the animals’ ability to experience feelings.

⁷The sentience institute estimates that 99% of the United States farmed animals are raised in factory farms (www.sentienceinstitute.org). In France, the pro-animal non-profit organization L214 (www.l214.com) estimates that 95% of pigs are raised in intensive farms, despite being among the animals with the highest cognitive abilities (Marino and Colvin, 2015).

Growing scientific evidence points towards plant-based diets as an answer to the negative externalities caused by meat consumption. Diets containing a higher proportion of plant-based food and a lower proportion of animal-based consumption are associated with less negative impacts on the environment (Peters et al., 2016, Nelson et al., 2016, Willett et al., 2019) and positive outcomes on human health (Orlich et al., 2013, Song et al., 2016, Satija et al., 2016). Switching to plant-based diets would also mechanically lower the demand for animal-based consumption and decrease the number of intensively farmed animals. Although plant-based diets have the potential for a win-win-win outcome (Scherer et al., 2019),⁸ orienting individuals towards a reduction in animal-based consumption is not straightforward.

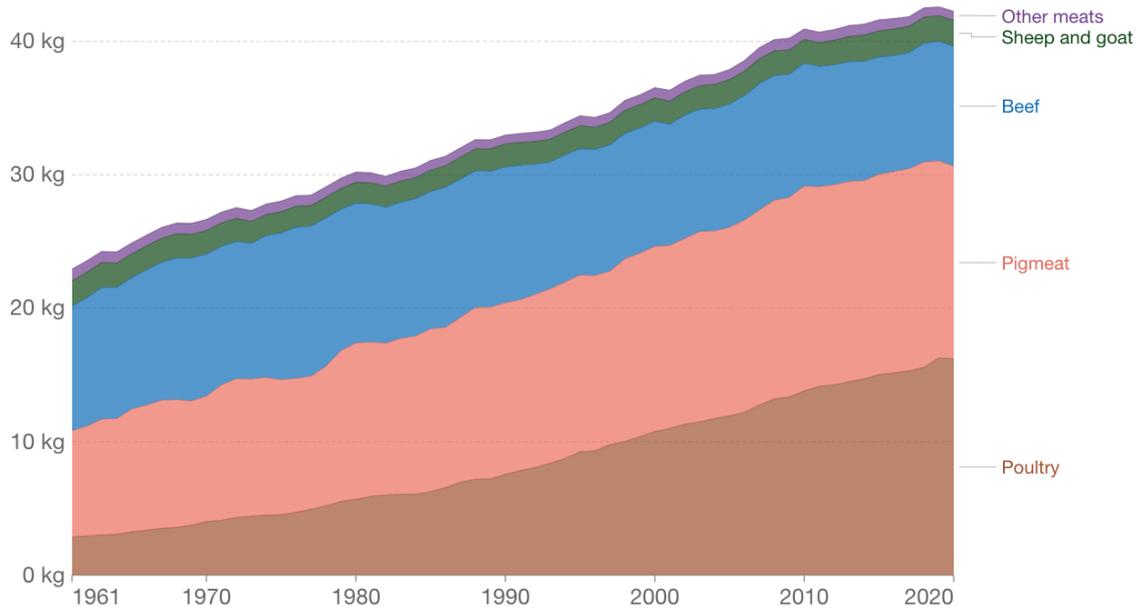
Meat consumption is paradoxical. The per capita meat consumption has been increasing despite the identified negative effects (see Figure I.3). To counter this trend, economists have outlined the need for meat taxation (Funke et al., 2022) and developed optimal meat taxation models (Katare et al., 2020). Nevertheless, these models are not an immediate solution due to the prolonged implementation processes. At the government level, other limitations such as adjustment delays of national dietary guidelines, the slow transition of agricultural models, and the disregard of scientific evidence slow down the transition. Faster changes might be induced by targeting individuals, but a better understanding of the biases surrounding meat consumption is necessary. First of all, sincere ignorance favors meat consumption (Espinosa, 2021). Individuals might not be aware of the consequences of eating meat because they do not have immediate access to the information. This arises from the lack of access to scientific evidence and vulgarized results, the dysfunction of information channels, and the hidden living conditions of factory-farmed animals. However, individuals are also subject to cognitive biases.

Meat consumption is affected by the meat paradox (Loughnan and Davies, 2019), which refers to the contradiction between peoples' empathy for animals and slaughtering animals for food. The meat paradox is itself a result of other cognitive biases. Specifically, the cognitive dissonance⁹ framework applies well to meat consumption (Rothgerber, 2014, 2020), and favors other associated cognitive biases such as strategic ignorance (Onwezen and van der Weele, 2016, Leach et al., 2022), reactance

⁸Win-win-win refers to benefits for the environment, health, and animal welfare (Scherer et al., 2019).

⁹Cognitive dissonance was first defined by Festinger (1962) as a state of discomfort experienced by inconsistent beliefs and actions.

Figure I.3: Per capita meat consumption by type, World, 1961 to 2020.



Source: Our World in Data.

(Spelt et al., 2019), denying animal suffering (Bastian et al., 2012, Rothgerber, 2013), rationalizing meat consumption (Piazza et al., 2015), and motivated reasoning (Hestermann et al., 2020). The meat paradox is also accentuated by social constructs through social norms (Sparkman and Walton, 2017, Rosenfeld and Tomiyama, 2021, Nguyen and Platow, 2021, Hansen et al., 2021) and social identity (Graça et al., 2015, Rosenfeld et al., 2020). A recent example highlighting the denial of animal suffering and the role of social norms is the Twitter account Elwood’s Organic Dog Meat.¹⁰ This satiric Twitter account promotes its “organic dog meat” by utilizing popular communication techniques from the meat industry, which involve denying animal suffering, normalizing meat consumption, and romanticizing meat farming (see Figure I.4).¹¹ The account’s objective is to highlight the meat paradox by offering dog meat rather than a normally accepted type of meat (e.g., beef or pork), thus disrupting the norm that dogs are pets and not food. Some literal-minded Twitter users emphasize the meat paradox by posting vivid comments and criticizing the “activity” of the farm. The reactions confirm individuals’ inconsistencies and

¹⁰Elwood’s Organic Dog Meat also has a website www.elwooddogmeat.com.

¹¹Examples include the use of terms such as “humanely slaughtered”, “local and organic”, “family farm”, and “live happily”.

internal tensions in relation to meat consumption. The meat paradox is a barrier to reductions in meat consumption and it is essential to better understand its' underlying mechanisms to orient individuals towards a reduction. The experimental economics approach helps bringing new insights to the issue.

Figure I.4: Tweet example from Elwoods Organic Dog Farm.



Economic experiments provide a new approach to the meat paradox and social constructs that surround meat consumption. First, cognitive dissonance induced by the meat paradox is a deviation from rational decision making (Akerlof and Dickens, 1982) and prompts people to adopt inconsistent behavioral patterns. This phenomenon can be assimilated to inconsistent patterns (e.g., Heuristics) highlighted by Tversky and Kahneman (1974). Specifically here, the tensions created by the individuals' love for animals and consumption of meat distort the predictability of actions under Bayesian decision theory. A greater understanding of the mechanisms at play with meat consumption helps in defining prior and likelihood probabilities

to predict individual behaviors. This is essential for economists to develop powerful theoretical models, but also to design and provide optimal public policy recommendations. Second, due to field-specific methodologies, research on meat consumption behaviors has relied heavily on evidence involving stated preferences and hypothetical situations (Rosenfeld, 2018). Economic experiments differ in that they allow to study individual behaviors under revealed preferences (Jacquemet et al., 2019). Economic experiments involve financial incentives within environments free from deceit or trickery. Participants are fully informed of their actions' consequences, which ensures the measurement of real actions rather than attitudes or intentions. This concept is key with meat consumption because of the gap between individuals' actions and beliefs (i.e., cognitive dissonance). This discrepancy is emphasized by the warm-glow theory (Andreoni, 1990), where individuals might state that they are willing to cease eating meat (e.g., to reduce the number of animals killed) to feel good, but keep consuming meat (Espinosa, 2021). Experimental economics contributes to filling this gap and provides a new wave of evidence to understand and identify the related biases.

Recent studies have started applying the experimental economics methodology to study the meat-related behaviors. For example, some studies have sought to explore consumers' non-hypothetical willingness to pay for meat alternatives, such as plant-based meat (Katare et al., 2023) and cultured meat (Treich and Espinosa, 2023).¹² This framework has also been applied to elicit individuals' non-hypothetical willingness to pay for animal welfare measures (Norwood and Lusk, 2011) and votes for different levels of meat taxation (Perino and Schwickert, 2023). Other studies have focused on comparing donations to non-governmental organizations according to a welfarist *vs.* abolitionist discourse (Espinosa and Treich, 2021) and donations following a reduction in meat consumption (Carrico et al., 2018). Framing effects also have an impact on the choice of meatless alternatives at restaurants (Rosenfeld et al., 2022). Lastly, the information channel has been tested, with results showing information avoidance on the living conditions of animals in intensive farms (Epperson and Gerster, 2021) and information resistance for animal-based diets (Espinosa and Stoop, 2021), but also confirming the sincere ignorance through information asymmetries (Paul et al., 2019). Overall, the literature confirms that meat consumption

¹²Cultured meat refers to meat that is created in-vitro using animal cells. This meat production technique is pain-free for animals and does not require death.

entails cognitive biases and social constructs that distort rational behaviors and that using real decisions and financial incentives provides new insights. The evidence on the question is still relatively scarce but the field is rapidly growing due to the stakes at play.

Chapter 1 and 2 contribute to this literature by providing new insights. Chapter 1 explores the role of social norms and social identities derived from foregoing meat by investigating the role of vegetarian and vegan social identities on social preferences. Chapter 2 focuses on the information channel by analyzing how French doctors process information on plant-based diets.

The importance of Registered Reports

Science is defined by the Merriam-Webster dictionary as “a system of knowledge covering general truths or the operation of general laws especially as obtained and tested through scientific method”. Accordingly, the product of science (i.e., scientific results) is an ensemble of evidence that is acquired by testing concepts following the scientific method. This process is at the heart of all sciences and dictates our understanding of the world, but a question arises: how much can we trust scientific evidence? This question is fundamental and can be answered through the replication of scientific results. Scientific evidence, if true, should be consistently replicable by multiple research teams using the same scientific method. The more a scientific result is replicated, the greater the strength of the evidence. However, failure to replicate suggests that a scientific finding is not a general truth and might stem from an isolated observation. While failure to replicate is acceptable in the case of an isolated occurrence, the multiplication of irreplicable results in science is detrimental as it blurs the spectrum of evidence. Recently, failure to replicate has been accentuated by the publish or perish academic culture (Edwards and Roy, 2017). Researchers are pressured to produce original and meaningful results to have successful careers in academics, which creates a misalignment between researchers’ self-interest and science’s interest (Ebersole et al., 2016). The abundance of positive results, especially in the social sciences (Fanelli, 2010), is contaminating scientific evidence and leading toward a replication crisis.

One of the first disciplines in the social sciences to declare itself in a replication

crisis was psychology. The crisis¹³ started after the publication of revolutionary, yet suspicious results by Bem (2011). Wagenmakers et al. (2011) showed that the data analysis process used by Bem (2011) was flawed and led to erroneous conclusions. Wagenmakers et al. (2011) also argued that research practices in experimental psychology were questionable and called for improved research methodologies. Following this event, psychologists began to question and replicate major studies (Wiggins and Christopherson, 2019). One famous example is the uncovering of fraudulent data from Diederik Stapel (Callaway et al., 2011). Researchers identified and reported fake data in Stapel's papers, forcing him to withdraw many published articles. However, rather than a simple callout of researchers' fraudulent actions, the replication crisis in psychology shed light on the importance of replication. From there, replication studies became popular in psychology (Koole and Lakens, 2012), and extended to other disciplines (Camerer et al., 2018). Moreover, the replication crisis highlighted the biases fostered by the publication system and raised questions about the blurriness of some research practices.

Publication bias and citation bias are two consequences of the publish or perish academic culture. Publication bias, first defined by Sterling (1959), refers to the greater number of statistically significant results¹⁴ published in scientific journals. This bias is caused by the academic journals' higher likelihood to publish statistically significant results and leads researchers to submit only positive results. Publication bias diminishes the value of null results and is intimately related to citation bias, where positive results are more likely to be cited (De Vries et al., 2018). The appraisal of positive results also pushes researchers to engage in questionable research practices and to abandon research projects yielding null results, which is known as the file drawer issue (Rosenthal, 1979). Questionable research practices correspond to a collection of methods that researchers can use to manipulate and arrange their results to respond to the demand for positive results (Pennington, 2023). Parsons et al. (2022) provide a detailed glossary of the questionable research practices and main vocabulary for Open Science. The malpractices can be divided into subset categories. First, researchers can embellish a result by manipulating the level of significance in a frequentist approach. P-hacking, forking, and fishing (Gelman and Loken, 2013) refer to the practice of running statistical analyses and arranging data

¹³Some researchers refer to this episode as a "Crisis of Confidence" or "Credibility Revolution".

¹⁴I use the terms "statistically significant result" and "positive result" interchangeably.

until a statistically significant result is achieved.¹⁵ Second, researchers can arrange results ex-post. Hypothesizing After the Results are Known - HARKing (Kerr, 1998), Preregistering After the Results are Known - PARKing, and Critiquing After the Results are Known - CARKing (Parsons et al., 2022) are terms used to describe researchers' ex-post tactics to rationalize results and present unanticipated findings as predicted and positive results. Both types of questionable research practices are detrimental to scientific evidence as they increase the number of false positives, which undermines replicability and worsens scientific evidence. Questionable research practices are widespread in science, making it difficult to distinguish whether they originate from a lack of scientific integrity or unintentional practices.

Questionable research practices can be attributed, in some cases, to intentional methods that researchers use to secure positive results. The case of Diederik Stapel (Callaway et al., 2011) mentioned above corroborates this fact. Nevertheless, questionable research practices are rooted in the scientific method and can be unintentionally used by researchers (Pennington, 2023). Similarly to the meat paradox, researchers might suffer from sincere ignorance and apply questionable research practices without realizing it. After all, researchers were first students and might have been taught malpractices as good practices. Researchers are also subject to cognitive biases that lead to questionable research practices. In analyzing and interpreting their data, researchers might have prior beliefs that they seek to confirm. This confirmation bias pushes researchers to look for spurious relationships and induces the file drawer issue by increasing the abandonment of results that do not confirm prior beliefs (Pennington, 2023). Researchers might also display scientific apoplexia, which refers to finding meaningful yet inexistent patterns in data (Goldfarb and King, 2016), and be prone to hindsight bias, which is designating a result as predictable after its occurrence (Pennington, 2023). Lastly, publication bias creates a social norm of positive results among researchers, leading to malpractices in a desire for conformity.

Questionable research practices are widespread across multiple scientific disciplines, and economics is not exempt. P-hacking is present in economics with estimated lower bounds of inflated significance ranging from 10 to 20% in top journals (Brodeur et al., 2016), and 56.2 to 71.3% for a sample of publications in economics

¹⁵Examples of practices within p-hacking include optional stopping rules or unnecessary controls (Simmons et al., 2016).

journals (Bruns et al., 2022). Ioannidis et al. (2017) also reported high rates of underpowered studies in economics. In a recent survey of 108 economists from the German Association of Agricultural Economists, over 60% of the respondents reported having felt pressured to publish statistically significant findings and having witnessed colleagues engaging in p-hacking (Heckelei et al., 2023). Additionally, researchers are beginning to question findings in experimental economics. Camerer et al. (2016) replicated 18 laboratory experiments from top economic journals and found a replication rate of two-thirds, which is relatively higher than a similar study in psychology. While experimental economics results in top journals appear relatively robust, they do not provide a full assessment of the evidence. Experimental economists are questioning the validity of certain results and are calling for the implementation of better practices to bullet-proof future evidence and avoid a replication crisis (Page et al., 2021).

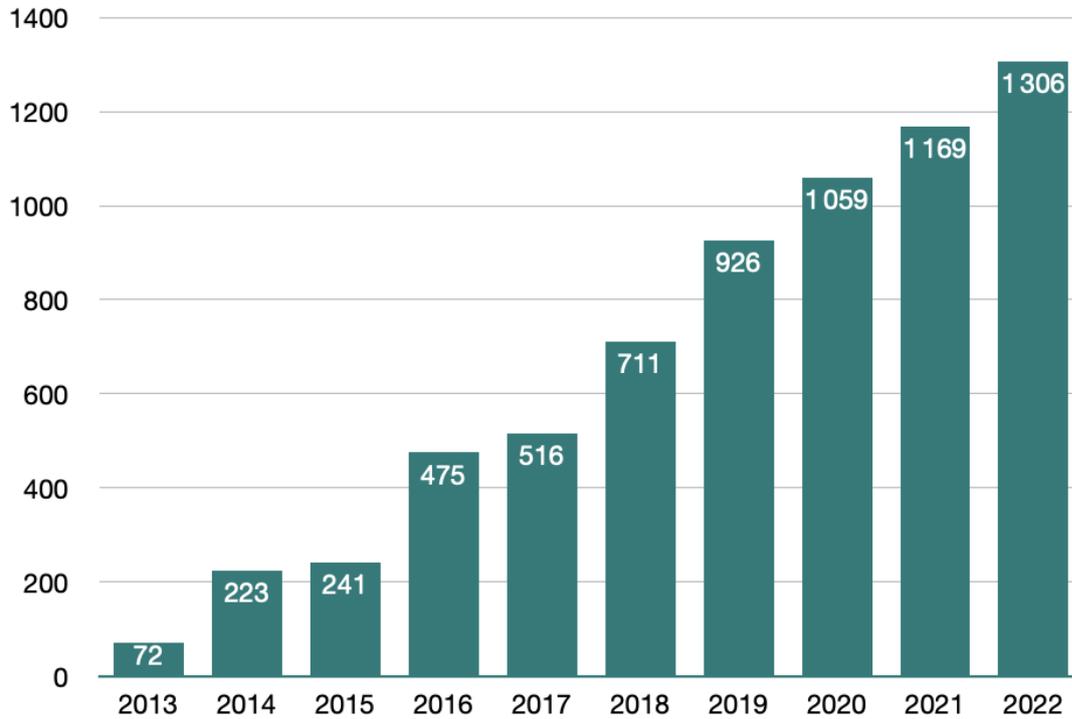
Different initiatives have been introduced to improve the reproducibility, replicability, and transparency of empirical and experimental economics research. First, economic journals are taking steps to improve the quality of publications. Many journals have modified their submission guidelines to ensure reproducibility, here defined as the ability to reproduce scientific results with the original data and code. Researchers are now required to submit their data and codes along with their paper submissions. Recently, the journal *Management Science* took this initiative one step further by launching the *Management Science Reproducibility Project*, which aims at reproducing papers published in the journal before and after the implementation of new submission guidelines in 2019 (Simchi-Levi, 2023). Some economic journals now also encourage replicability, which is the ability to find the same results as previous studies with a new set of data. Camerer et al. (2019) underline the importance of replications in economics to enhance the strength of evidence and define the trajectory of future research. Even though replicability is essential, the incentives for replications are low. Replication studies are often difficult to publish and only a few journals, such as the *Journal of the Economic Science Association*, encourage replication submissions (Nikiforakis and Slonim, 2015). Second, a growing number of researchers engage in meta-analyses. Meta-analyses consist in combining data from independent research projects and analyzing it as a whole (Havránek et al., 2020). This type of study is useful, especially if null results are included, as it allows to test the strength of evidence of a body of research. The issue with meta-analyses relies upon hidden evidence. The overrepresentation of positive results and impos-

sibility to include abandoned null results likely biases and undermines the power of meta-analyses, thus limiting the outcome. The mentioned initiatives attempt to limit questionable research practices ex-post, but a more efficient solution might be to tackle them ex-ante, before the data analysis is conducted.

A suggested step to elude a replication crisis in experimental economics is pre-registration. Preregistering a study implies depositing, on a dedicated platform online, a document stating how the data will be collected and analyzed, before data collection occurs or before access to a dataset is granted. Pre-registration is gaining in popularity, as illustrated by the exponential growth of trial registrations at the American Economic Association (see Figure I.5). Contrary to the reproducibility and replicability initiatives listed above, preregistration targets research practices ex-ante by reducing the researchers' degree of freedom and enabling to distinguish predictions from postdictions (Nosek et al., 2018). Although pre-registration is better than no registration (see Section 2 from Chapter 3), it does not completely preclude questionable research practices. Indeed, while some general guidelines on pre-registration are available (Olken, 2015), the lack of general agreement on specific rules leaves researchers with flexibility. As long as a document has been deposited online, any research article can claim that it is a preregistered study, without any ex-ante quality check. For example, on the platform *AsPredicted.org*, a research team could decide to preregister the data collection process, exclusion rules, and dependent variable, but could omit to specify the statistical model and covariates for the analysis, therefore increasing the degree of freedom. Additionally, pre-registration does not prevent publication bias nor the file drawer issue. Whether a study is preregistered or not does not influence the journals' propensity to publish positive results, which in turn causes researchers to abandon preregistered studies that yield negative results. The limited benefits of pre-registration in preventing a replication crisis and economic researchers' growing interest in open science (Swanson et al., 2020) call for new alternatives.

Registered Reports are a new article format launched in 2012 and that were first adopted by the scientific journal *Cortex* in 2013 (Chambers and Tzavella, 2022). Registered Reports emerged in psychology to further improve pre-registration protocols, reduce researchers' degree of freedom, and mitigate publication bias. The originality of Registered Reports is based on a two-stage reviewing process that ensures optimal ex-ante and ex-post research practices (Stewart et al., 2020). In Stage-1, researchers submit their introduction and methods (i.e., analysis plan) for

Figure I.5: Number of *Pre-registrations* at the American Economic Association per year, from 2013 to 2022.



Source: data retrieved from www.socialscienceregistry.org and author's own calculations.

revision before the data collection occurs. Once the revision is completed, the researchers receive an In-Principle Acceptance, guaranteeing that the paper will be published regardless of the results. Researchers can then collect and analyze the data according to the analysis plan, and write the conclusion. The paper is re-submitted for Stage-2 approval, where the conformity with the analysis plan from Stage-1 is verified. Upon conformity, the paper is approved and published.

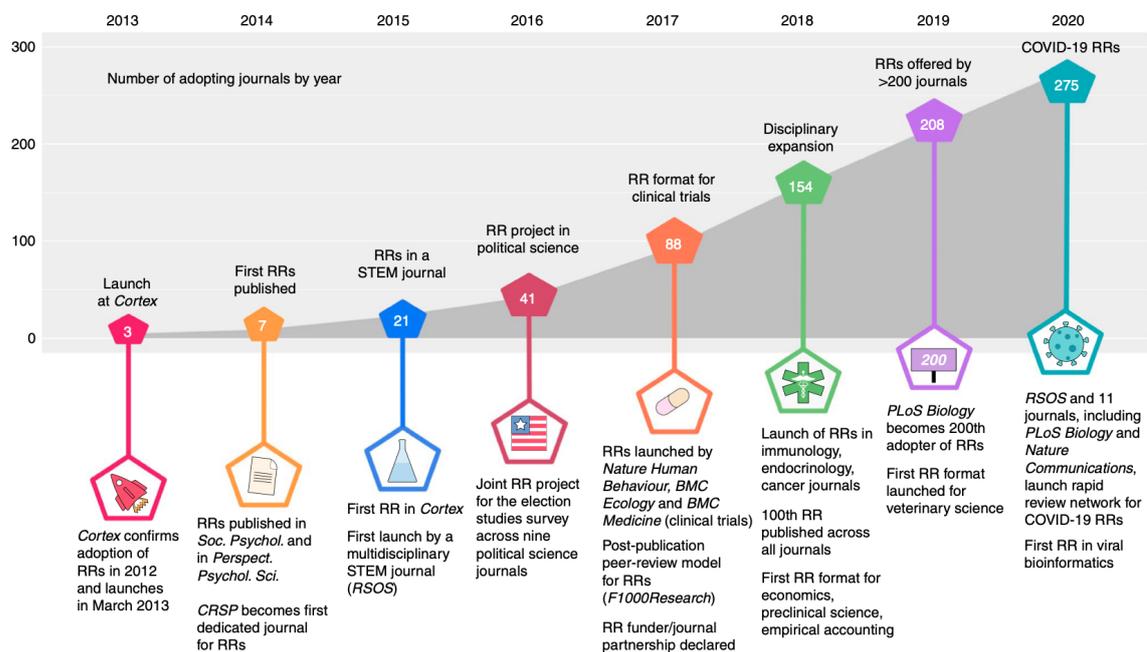
Registered Reports outperform pre-registration in multiple ways. First, Registered Reports improve the lack of general agreement on pre-registration by establishing a structured set of procedures and methods that researchers must respect.¹⁶ This further reduces the researchers' degree of freedom and alleviates the risk of questionable research practices. Second, the Stage-1 revision offers the possibility to improve the analysis plan ex-ante, before undertaking the data collection.

¹⁶The steps are detailed in Chapter 3.

GENERAL INTRODUCTION

Pre-registration is often realized independently by the researchers, leaving room for errors and misspecification that can only be acknowledged ex-post. Third, Registered Reports diminish the risk of the file drawer issue and publication bias through the In-Principle Acceptance. After passing Stage-1, researchers know that their study will be published regardless of a null outcome. Fourth, researchers might gain bargaining power through Registered Reports. Indeed, researchers can use the In-Principle Acceptance as a quality check and proof of interest when asking for project funding. Recent evidence confirms the benefits of Registered Reports. Scheel et al. (2021) find a rate of 96% of positive results with standard paper in social psychology, against only 44% for Registered Reports. Soderberg et al. (2021) compare the quality of standard articles with Registered Reports in psychology and neuroscience using peer-review and a performance scale and find that Registered Reports outperform in all aspects. As a result, the article format has gained in popularity. Today, over 300 journals accept Registered Reports as a valid submission format,¹⁷ including the journal Nature since February 2023. Figure I.6 summarizes the evolution of Registered Reports.

Figure I.6: Evolution of Registered Reports from Chambers and Tzavella (2022)



¹⁷According to the Center for Open Science www.cos.io/initiatives/registered-reports.

The evolution of Registered Reports have brought about limitations and challenges to overcome. Registered Reports eliminate publication bias but have limited effects on citation bias, as researchers might still prefer to cite positive results over null results. However, one may hope that an increase in the number of published null findings will induce a reduction of the citation bias. Time constraints limit the adoption of Registered Reports. The preparation of a Stage-1 submission and reviewing process before undertaking the data collection require time and efforts that researchers might not be able to afford. Some projects necessitate quick actions and implementations, and might not be well suited for Registered Reports. On the journal side, Chambers and Tzavella (2022) mention that the next important steps will be standardization, transparency, and data availability. The format is at its' premises and different journals apply different practices. For example, Obels et al. (2020) outline that only 58% of a selected sample of psychology Registered Reports had data and codes shared to the public, and that only 58% of these articles were computationally reproducible.

An initiative to lay a common ground is the Peer Community In Registered Reports - PCI-RR. This regroupment of researchers provides free peer-reviews and recommendations of Registered Reports. Once an article has undergone Stage-1 and Stage-2 reviewing process at PCI-RR, it is published as a pre-print on the platform and the article is recommended to affiliated journals. PCI-RR offers the advantage of standardizing the reviewing process by setting a common procedure for all submitted Registered Reports. It also frees journals from the burden of peer-reviewing. Another challenge that Registered Reports face is the journals' reluctance and perceived barriers. For journals unaffiliated with PCI-RR, implementing Registered Reports requires finding qualified reviewers and adapting the submission process. The two-stage reviewing process might also be time costly for journals, in comparison with standard submissions. This last point remains unclear as there are no empirical evidence on the reviewing process timeframe yet. Registered Reports still have room for improvements, but the multiple benefits expand the range of the format to new disciplines.

Registered Reports are expanding to economics. The format is particularly well-suited for experimental economics, as researchers often plan data collection and statistical analyses ex-ante. As of today, ten economic journals accept Registered Reports as a valid submission format. Data from a recent survey of 26 economic

journals that accept experimental submissions¹⁸ reveals a growing interest in the format. Out of 20 journals not accepting Registered Reports, 11 are discussing it. The survey suggests that journals are aware of the questionable research practices and biases surrounding data analysis for experimental economics, particularly journals accepting and discussing the adoption of Registered Reports. The data also confirms that the reviewing process timeframe, logistic efforts required, and rigidity of the format are perceived barriers. Some other challenges arise, such as the suitability of Registered Reports for empirical research projects with existing data. Nevertheless, Registered Reports remain a convincing option to improve the credibility and replicability of economic findings, in particular experimental findings, and avoid a replication crisis in economics. To this end, Chapter 3 is dedicated to providing a guide for experimental economists who desire to take on a Registered Report. The guide describes the main components of Registered Reports, with experimental examples, to simplify and encourage the adoption of the format.

Outline

The dissertation is divided into three independent chapters. Chapters 1 and 2 analyze two specific issues in relation to plant-based diets by providing empirical contributions with experimental data. Chapter 1 evaluates the role of social identity induced by foregoing meat and adopting a plant-based diet. Chapter 2 explores the impact of informing health professionals on plant-based diets. Chapter 3 offers a contribution to improve the reliability of experimental results by providing a guide to Registered Reports specifically tailored for economists.

Chapter 1 quantifies whether the decision to adopt a plant-based diet is enough to induce an out-group bias. Vegetarian and vegan diets disrupt the norm of eating meat and are associated with the endorsement of a social identity. Despite the positive externalities of plant-based diets, vegetarians and vegans pay a social cost in the form of an out-group bias known as *vegephobia*. Literature in sociology and psychology has accumulated evidence of *vegephobia* and demonstrated its role as a

¹⁸This is part of an ongoing project with Marianne Lefebvre, Sylvain Chabé-Ferret, and Romain Espinosa.

barrier to plant-based diets. Chapter 1 proposes the first measurement of vegophobia using social preferences within an incentive-compatible environment.

In an online experiment using a between-subject design, omnivore participants play a modified version of the dictator game and are matched with either an in-group (i.e., omnivore) or an out-group member (vegetarian or vegan). Omnivore dictators receive information on the dietary identity of their matched recipient, alongside other identity information, and face 15 monetary allocations. The dictators must choose between the *Top* or *Bottom* option for each allocation, dividing money between themselves and the recipient. One of the allocations is randomly selected and dictators receive a bonus payment accordingly. Recipients have a passive role in the first part of the experiment, but then face the same 15 allocations and must guess for each allocation, the dictator's choice. Recipients are provided with the dictators' dietary identity (i.e., omnivore), alongside other identity information, and are informed that the dictators were provided with their dietary identity, depending on the treatment. Recipients receive a bonus payment either from the dictator's choice or from the accuracy of their guesses. Lastly, all participants report their beliefs and answer a socio-demographic survey. The experimental protocol and analyses were pre-registered on the *Open Science Framework*.

Vegophobia is quantified by estimating the dictators' inequity aversion parameters (Fehr and Schmidt, 1999) and comparing the sensitivity of the parameters between treatments (i.e., according to the recipients' dietary identity). Perceived vegophobia is also quantified by estimating the recipients' expected inequity aversion parameters. Greater *envy* and less *guilt* from omnivore dictators with vegetarian and vegan recipients, in comparison with omnivore recipients, are interpreted as out-group bias and evidence in favor of vegophobia. For recipients, greater expected *envy* and lower expected *guilt* from vegetarians and vegans, in comparison with omnivores, are interpreted as expectations of out-group bias and evidence in favor of perceived vegophobia. Following the literature, two hypotheses on dictators' envy are pre-registered and the rest of the analyses are relegated to exploratory analyses.

The results reject the preregistered hypotheses and conclude to the absence of vegophobia. Additional exploratory results suggest that vegophobia is driven by individual characteristics. Females display less vegophobia with vegans and do not perceive the rise of plant-based diets as a threat. Dictators who are white, have higher levels of education and feel closer to the left-wing or independents display greater levels of out-group bias. Against evidence from the contact theory literature,

the presence of a vegetarian or vegan friend in the dictators' social environment triggers more vegephobia. For recipients, vegans report higher levels of discrimination as a result of their dietary choices but expect less out-group bias from dictators in the choice task. Overall, the economic tools fail at capturing the reported discrimination, which suggests that previous studies with similar approaches might have underestimated the actual effect sizes out-group biases.

Chapter 2 is a Registered Report that explores the impact of an information campaign about plant-based diets on doctors' opinions and practices. Doctors are an available source of information for patients who seek to transition toward plant-based diets. However, evidence shows that doctors lack specific knowledge of plant-based diets and do not have access to the latest scientific information. Doctors are also subject to cognitive biases such as confirmation bias or information resistance when acquiring new information. The combination of both factors undermines the therapeutic relationship with vegetarian and vegan patients and might hinder patients' transitions toward plant-based diets.

In order to evaluate doctors' information acquisition on plant-based diet, a sample of 400 representative French doctors is recruited. Using a randomized controlled trial, half of the doctors are randomly assigned to the control condition and half to the treatment condition. Doctors in the control condition answer a questionnaire with two case studies involving a patient who desires to adopt a vegan diet, and participate in a variation of the charity giving game designed to support the diffusion of health information on vegan diets. Doctors in the treatment condition are exposed to an information campaign in the form of a booklet before answering the questionnaire. The booklet is designed by the research team composed of medical experts and contains information on nutrients and risks associated with a vegan diet, as well as some of the latest available scientific information. The impact of the information campaign is evaluated through three indicators. The *Veganism Disapproval Index* captures the doctors' general opinion on vegan diets, the *Proper Medical Practice Index* assesses the doctors' practices with vegan patients, and the *Veganism Promotion Index* measures the doctors' active actions to spread information on vegan diets.

The results show that the information campaign has a positive impact on the doctors' opinions, but the impact on the doctors' practices is limited. Though it is positive on average, the effect is smaller than the smallest effect size of interest

and is not statistically different from zero, which does not provide enough evidence to support a positive impact. The effect on the active promotion of vegan diets is not analyzed due to the lack of power. Robustness checks indicate no heterogeneous treatment effects except for the time spent reading the booklet, which increases the treatment effect. Additional results highlight the fact that doctors are aware of their colleagues' lack of nutrition knowledge but overestimate their own knowledge. Doctors are also conscious that their opinions on plant-based diets might alter the therapeutic relationships with vegetarian and vegan patients. Overall, the results suggest that a short and inexpensive information campaign has the potential to improve doctors' opinions and practices with patients who desire to adopt a plant-based diet.

Chapter 3 is dedicated to providing a guide to Registered Reports for economists.¹⁹ The publication system in economics is biased by an overrepresentation of positive empirical results. Specifically, publication bias and citation bias encourage researchers to submit positive results and to engage in questionable research practices such as p-hacking, HARKing, forking, and abandoning studies with null results. Questionable research practices harm the credibility and reliability of scientific results, prevent replication, and might guide economics toward a replication crisis. A suggested solution is pre-registration, in which researchers specify the data collection process and analyses to be conducted before the data is collected by submitting a document on a dedicated online platform. While pre-registration may solve some of the scientific malpractices, it remains unstructured and arbitrary, provides researchers with a high degree of freedom, and is ineffective to counter publication bias and the file drawer issue.

Registered Reports are a new article format that prioritizes the scientific process rather than the results. Registered Reports function through a two-stage process. First, before any data is collected, researchers write the introduction, research question, hypotheses, data collection process, and analysis plan, and submit it to a journal. After revision, the paper receives an In-Principle Acceptance, guaranteeing the publication of the paper provided that the analyses adhere to the previously communicated procedures, independently from the results. Researchers then collect the data, analyze it, and write up the result section. The paper is then re-submitted

¹⁹This chapter is an extended version of an article published in the *Journal of the Economic Science Association* in 2023.

and published. The format is well-suited for experimental economics research, but can also be broadened to empirical work. Registered Reports are gaining in popularity in the scientific community due to their efficiency in successfully countering questionable research practices and preventing publication bias. Economic journals have started to adopt Registered Reports as a valid submission format with ten journals accepting the format so far.

Chapter 3 provides a guide to Registered Reports specifically adapted for experimental economists. The guide first demonstrates the advantages of Registered Reports over no pre-registration, poorly executed pre-registration, and well-executed pre-registration. The main components of Registered Reports are then discussed and detailed, such as the analysis plan, statistical power, multiple hypothesis testing, smallest effect size of interest, exploratory analyses and deviations for the analysis plan, levels of Registered Reports, design table, ethics approvals, and optimal journal choice. These elements are completed with specific examples for experimental economics, as well as R and Stata codes to facilitate the implementation.

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Chapter 1

Identifying the barriers to plant-based diets: the case of vegephobia¹

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¹The draft corresponding to this chapter is single authored.

1 Introduction

Social identity shapes social interactions and economic behaviors. Individuals have a set of identities that affiliates them to homophilic groups, affecting their attitudes (Tajfel et al., 1979) and utilities (Akerlof and Kranton, 2000). They tend to favor in-group and discriminate out-group members (Shayo, 2020), through different behaviors (e.g., altruism or pro-sociality) and across various settings (e.g., teamwork or social interactions). This holds whether the identity is inherent, chosen, or simply derived from a random group assignment.² While economists have explored many types of identities, discrimination based on dietary choices remains largely unexplored. Specifically, individuals who adopt a plant-based diet (i.e., vegetarianism and veganism) share common values, beliefs, and attitudes (Nezlek and Forestell, 2020), as well as group-recognition (Plante et al., 2019), defining plant-based diets as a social identity. In this paper, I explore whether the decision to forego meat is enough to induce out-group discrimination.

The negative stigma associated with vegetarian and vegan (veg) identities might hinder the adoption of plant-based diets. Despite multiple benefits, pushing consumers towards plant-based diets in Western societies remains challenging.³ Omnivores (i.e. meat eaters) are subject to cognitive biases that prevent them from switching to plant-based diets. They tend to overestimate the healthiness of meat (De Bakker and Dagevos, 2012), underestimate its' environmental impact (Hartmann and Siegrist, 2017), and continue to perceive meat consumption through the 4Ns: Natural, Necessary, Nice and Normal (Piazza et al., 2015). Adopting a plant-based diet disrupts the normality of meat consumption (Rosenfeld and Burrow, 2017a) and lays the ground for discriminatory behaviors (MacInnis and Hodson, 2017). This bias oriented towards vegs, commonly known as vegophobia (Carrié, 2018), may be one of the factors limiting the transition towards plant-based diets (Markowski and Roxburgh, 2019).

²Examples of in-group bias favoritism and out-group discrimination of inherent identities include ethnicity (Fershtman and Gneezy, 2001, Chuah et al., 2014, Cetre et al., 2020) and gender (Slonim and Guillen, 2010, Delavande and Zafar, 2019, Cigarini et al., 2020), chosen identities include religion (Tan and Vogel, 2008, Chuah et al., 2016, Benjamin et al., 2016) and political affiliation (Kranton and Sanders, 2017, Chang et al., 2019), and random group assignment include preferences for paintings or a random color attribution (Yamagishi and Kiyonari, 2000, Goette et al., 2006, Andreoni and Croson, 2008, Güth et al., 2008, Chen and Li, 2009, Müller, 2019)

³Plant-based diets represent 5% to 8% of the population in the US (see Table A1 and (Reinhart, 2018)), 2.2% in France (FranceAgriMer, 2021) and 5% to 8% in the UK (FranceAgriMer, 2018).

But, why plant-based diets? Today, agriculture is estimated to represent around 30% of global greenhouse gas emissions (Clark et al., 2019), among which animal-based foods have the highest environmental impact (Clark and Tilman, 2017). In 2020, the estimated health cost of red and processed meat consumption was USD 285 billion (Springmann et al., 2018a), and current consumptions are projected to increase the incidence of type II diabetes, cancer, and coronary heart disease (Tilman and Clark, 2014). The global food system is the largest factor causing biodiversity loss (Rockström et al., 2020) and animal-based consumptions raise animal welfare concerns. Pushing consumers towards plant-based diets is necessary to help mitigate some of the negative externalities associated with meat consumption.

In this article, I study whether discriminatory behaviors directed towards vegs (i.e., vegophobia) are real and if they are measurable in an experimental economic environment. While adopting a plant-based diet is a pro-social action with positive impacts, vegs incur a social punishment in the form of discriminatory behaviors from omnivores (MacInnis and Hodson, 2017). This anti-social punishment (Herrmann et al., 2008) is detrimental, as it may act as a barrier for individuals who are wishing to switch to plant-based diets (Markowski and Roxburgh, 2019). Extending the knowledge on the mechanisms involved with such behaviors helps reducing the gap between meat-eaters and vegs, and design optimal policies to guide individuals towards more sustainable diets.

To examine the role of veg dietary identity on omnivores' social preferences, I design a between-subject online experiment with a modified version of the dictator game where all dictators are omnivores. The experiment is divided into three treatments, according to the recipient's identity. Recipients are either omnivores, vegetarians, or vegans. Dictators receive identity information about their matched partner and face fifteen allocations of income. I quantify vegophobia by estimating the dictators' inequity aversion parameters (Fehr and Schmidt, 1999), envy (i.e., aversion to disadvantageous inequality) and guilt (i.e., aversion to advantageous inequality), and compare the sensitivity of the parameters between treatments. In line with previous findings (Kranton et al., 2020, Chen and Li, 2009), I interpret more envy and less guilt with vegs, in comparison with omnivores, as out-group discrimination and evidence in favor of vegophobia. For recipients, I estimate perceived inequity aversion parameters by recovering incentivized guesses on their matched dictators' allocations in the dictator game. I compare recipients' responses between treatments and interpret greater expectations of envy and lower expectations of guilt

as expected discriminatory behaviors. Following previous findings, I pre-register two directional hypotheses on the behaviors of omnivores (i.e., omnivores will display greater envy with vegs than with omnivores) and leave the rest of the analyses for exploratory results.

The confirmatory analysis does not support the directional pre-registered hypotheses and rejects the presence of vegephobia at the group level. Omnivore dictators do not display more envy with vegs than omnivores in the choice task. The results are robust to pre-registered checks. Further exploratory results confirm the absence of vegephobia towards vegans but reveal vegephobia in the form of less guilt with vegetarians. At the individual level, common traits among dictators who display vegephobia emerge. In line with previous findings on vegephobia, females display less vegephobia with vegans and perceive vegetarianism and veganism as lower threats than men. Oddly, dictators who feel closer to the left-wing or independent, are white, and have higher levels of education display greater vegephobia with vegs. Dictators' social environment also matters. Contrary to the intuitions from contact theory, the presence of a veg friend in the dictators' social circles leads to more vegephobia. In contrast, the presence of a veg family member has no effect. Overall, dictators in the sample are plant-based diet friendly, which potentially mitigates the out-group bias.

On the recipient side, vegans report experiencing discrimination due to their dietary choices more frequently than vegetarians and omnivores. Paradoxically, vegans do not expect greater discrimination in the choice task, nor do vegetarians. This result might be explained by the pro-social nature of vegs, who possibly project their own preferences when asked to guess the dictators' choices. I argue that this is unlikely due to the financial incentives involved. An alternative explanation, which also relates to the absence of out-group bias from dictators, is that the economic tools employed fail at capturing discriminatory behaviors. This suggests that previous results in experimental economics using similar tools might have underestimated the actual effect sizes of discriminatory behaviors. These results are part of the exploratory analysis and more specific research is needed to confirm.

This paper makes a contribution to two research areas. First, it contributes to the literature on social identity in economics. Economists have been studying social identities since seminal work by Akerlof and Kranton (2000), particularly in the field

of experimental economics.⁴ I contribute to this literature by assessing the impact of confronting omnivores (i.e., meat eaters) to vegs, and quantifying the extent of out-group bias using inequity aversion parameters. This paper is one of the first attempts at studying social identities derived from dietary choices in economics, and more specifically social identities derived from plant-based diets. Second, it contributes to the literature that explores vegephobia in sociology and psychology.⁵ Most studies, due to field-specific techniques and requirements, do not explore the interactions between meat-eaters and vegs using revealed preferences. In this paper, I study vegephobia in an experimental economic environment involving an incentive-compatible scheme (i.e., modified version of the dictator game). The results provide novel evidence and contribute to the expansion of this literature. To the best of my knowledge, this paper is the first to contribute to this literature in such a way. Ultimately, the goal of this paper is to bridge the gap between the literature on vegephobia in sociology and psychology, and the literature on social identities in economics, as well as provide further evidence on the implications of adopting a plant-based diet.

The remainder of the paper is organized as follows. Section 2 reviews the relevant literature. Section 3 details the experimental design, and section 4 the theoretical framework. Section 5 presents the confirmatory and exploratory results. Last, Section 6 concludes the paper and discusses potential follow-ups.

⁴For examples of work on social identity in economics see Section 2.1.

⁵For examples of work on vegephobia see Section 2.2.

2 Background

2.1 Social identity in economics

The role of social identity on economic decisions has been explored both theoretically (Akerlof and Kranton, 2000, Charness and Chen, 2020) and experimentally (Anderson et al., 2006). Specifically, experimentalists have studied the impact of identity divergences such as religion (Tan and Vogel, 2008, Chuah et al., 2016, Benjamin et al., 2016), ethnicity (Fershtman and Gneezy, 2001, Chuah et al., 2014, Cetre et al., 2020), politics (Kranton and Sanders, 2017, Chang et al., 2019), gender (Slonim and Guillen, 2010, Delavande and Zafar, 2019, Cigarini et al., 2020) and endogenous or random group assignment (Yamagishi and Kiyonari, 2000, Goette et al., 2006, Andreoni and Croson, 2008, Güth et al., 2008, Chen and Li, 2009, Müller, 2019). Most findings show that participants exhibit out-group bias and in-group favoritism, regardless of the type of identity involved.

The economic literature on social identity has two weaknesses. First, many different approaches exist, both in terms of experimental design and measure of social preferences, making it difficult to compare the effect sizes across different types of identities. The approach I choose in this paper is closest to Chen and Li (2009) and Kranton et al. (2020). Chen and Li (2009) explore the role of social identity on distributive preferences using minimal group identity with preferences for paintings in a lab setting and inequity aversion parameters. Their results show that individuals exhibit greater envy (i.e., aversion to disadvantageous inequality) and less guilt (i.e., aversion to advantageous inequality) with out-group members. Kranton et al. (2020) study the impact of political identities on social preferences using a lab experiment. The authors match participants based on political preferences and estimate inequity aversion parameters, enabling them to distinguish participants' behaviors with in-group and out-group members. Participants display greater envy and less guilt with out-group members than with in-group members,⁶ with effect sizes larger for envy. I use these results to pre-register the two main hypotheses (see Section 4.2). Second, to the best of my knowledge, no paper in this literature has studied the effect of social identity induced by dietary choices on social preferences. I fill this gap by studying the impact of confronting meat-eaters (i.e., omnivores) to in-group

⁶This result also holds with a minimal group-identity treatment.

members (i.e., other omnivores) and out-group members (i.e., vegs), and making dietary identity salient.

2.2 Vegophobia

In recent years, the benefits of plant-based diets have become increasingly accepted (Tilman and Clark, 2014, Springmann et al., 2018a, Willett et al., 2019), and plant-based diets have become increasingly popular.⁷ Following this, researchers have sought to identify the barriers to the transition towards plant-based diets.⁸ Among others, the discrimination towards vegs, referred to as vegophobia (Carrié, 2018), has been designated as a potential barrier (Markowski and Roxburgh, 2019). Examples of vegophobia in psychology and sociology include the disclosure of dissonance among meat eaters (Rothgerber, 2014), the association of negative terms and moral judgments when individuals are faced with vegetarians (Minson and Monin, 2012) and the absence of negative thoughts when confronted with anti-vegan statements (Bresnahan et al., 2016). Individuals also tend to distance themselves physically and behaviorally from vegs, as well as anticipate veg stigma (Markowski and Roxburgh, 2019). Social predictors of vegophobia emerge from the literature such as social norms, social environment, social pressure (Delormier et al., 2009, Graça et al., 2015), gender conformation (i.e. masculinity), older age, lower education and traditionalism (Vandermoere et al., 2019, Pohjolainen et al., 2015, Rothgerber, 2013, De Backer et al., 2020). Plant-based motivation sources are also judged differently. Animal welfare and environmental motivations, in comparison with health motivations, provoke stronger vegophobia (MacInnis and Hodson, 2017). Finally, among plant-based diets, veganism suffers from significantly less positive attitudes than vegetarianism (Judge and Wilson, 2019), which is identified in the media through negative discourses in UK newspapers (Cole and Morgan, 2011). The current paper contributes to this literature by assessing the impact of a salient veg dietary identity on social preferences, in an incentive-compatible environment.

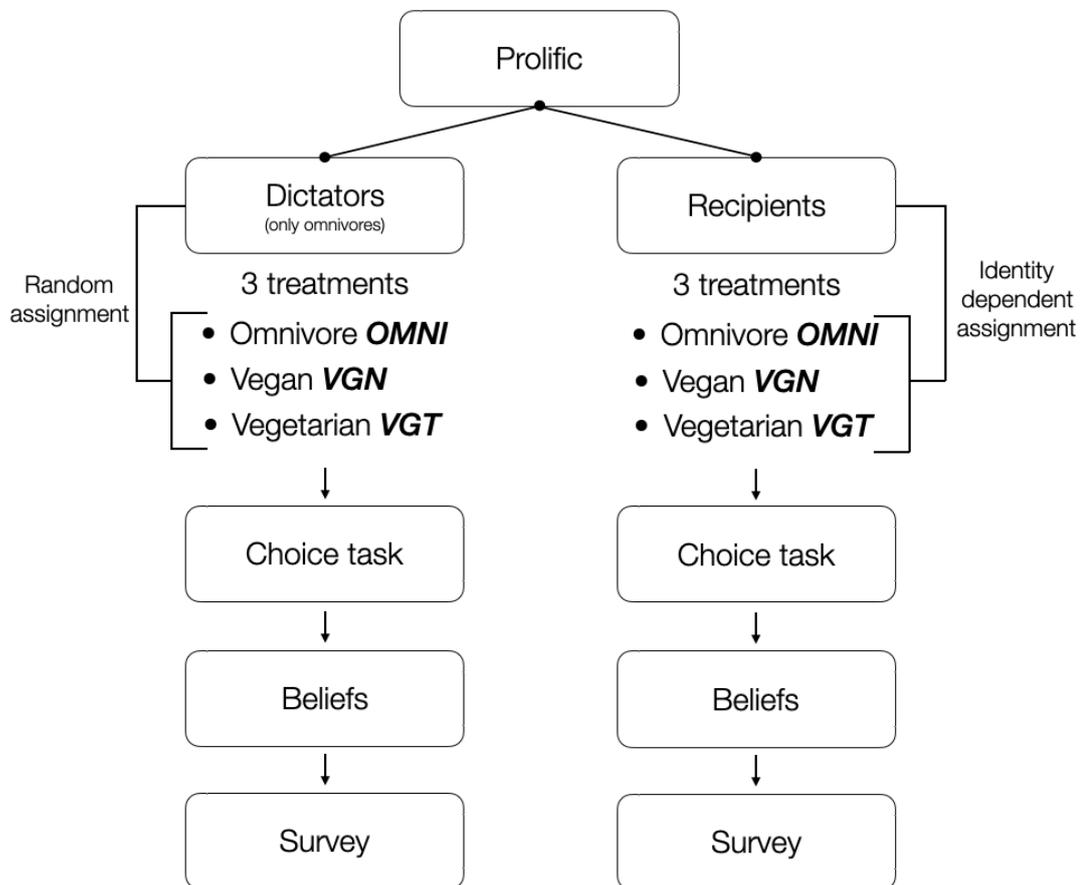
⁷In the United States, the percentage of vegs went from 3% in 1994 to 6% in 2022 (see Figure A1 in Appendix).

⁸See for examples papers by Pohjolainen et al. (2015) and Fehér et al. (2020).

3 Experimental Design

The experiment is a between-subject design divided into three main parts: choice task⁹, beliefs, and a socio-demographic survey. Participants are recruited online via Prolific and are assigned either the role of dictator or recipient. Participants are then assigned to a treatment and a partner, depending on their individual characteristics. The experimental process is displayed in Figure 1.1 and the screenshots from the experiment are available in the Supplementary Materials. The experimental protocol and primary analyses were pre-registered on the Open Science Framework.¹⁰ I detail each part of the experiment in the subsections below.

Figure 1.1: Experimental process.



⁹The choice task corresponds to the modified dictator game.

¹⁰The pre-registration document is available in the Supplementary Materials and on the Open Science Framework platform: <https://osf.io/h92wn>.

3.1 Treatments and identity information

Dictators are all omnivores (i.e., meat eaters) and are randomly assigned with equal probability to the *OMNI*, *VGT*, or *VGN* treatment. Dictators are given the dietary identity of their matched recipient. The only variation between treatments is the dietary identity of the recipient. In *OMNI*, dictators are matched with an omnivore recipient, in *VGT* with a vegetarian recipient, and *VGN* with a vegan recipient. Additionally, and to minimize dietary identity saliency, dictators also receive the following additional information about the recipient: is over 18 years old, lives in the United States, is a Prolific member, first language is English, and enjoys traveling.¹¹ This information is displayed in random order to avoid any order bias. The identity information is given before the first decision and is available underneath each decision in the choice task. The information provided is extracted from the participants' Prolific profiles and is therefore accurate.

Recipients are assigned to the *OMNI*, *VGT*, or *VGN* treatment. The assignment is not random and depends on the participant's dietary identity. Recipients assigned to *OMNI* are all omnivores, *VGT* all vegetarians, and *VGN* all vegans. Recipients are given two additional pieces of information. First, they receive the information that their matched dictator is over 18 years old, lives in the United States, is an omnivore, is a Prolific member, first language is English, and enjoys traveling. Second, they are informed that the dictator receives the following information about them: over 18 years old, lives in the United States, is a Prolific member, *dietary identity*, first language is English, and enjoys traveling. As for the dictator branch, this information is displayed in random order. The information is given before the first decision and is available underneath each decision in the choice task. This information is extracted from the participants' Prolific profiles and is therefore accurate. The screenshots of the experiment are available in the Supplementary Materials.

3.2 Choice task

In the first part of the experiment, dictators are matched with a recipient and play a modified version of the dictator game introduced by Kranton et al. (2020). The

¹¹Results from a pilot session suggested that with fewer identity information, the dietary identity becomes too salient and induces potential experimenter demand effect (see the Supplementary Materials)

game is unilateral (i.e., dictators are only assigned the role of a dictator). The choice task comprises 15 decision matrices with varying amounts,¹² displayed in random order, in which dictators must choose between the *Top* or *Bottom* option. Dictators are matched with a unique recipient for all the decisions. The amounts are displayed in Experimental Currency Units (ECU) and the conversion rate is 2ECUs = £0.01. Each option presents a different payoff combination, allowing dictators to allocate money to themselves and a recipient. Figure 1.2 provides an example of a decision matrix. The complete list of decision matrices is available in Table A1.

For all the decision matrices, choosing the *Bottom* (resp. *Top*) option decreases (resp. increases) inequality. One decision matrix is displayed per screen and once a decision is submitted, the dictator cannot go back to change it. Dictators are aware that one of the decisions is randomly selected and applied for bonus payment (i.e., the payoff combination is applied to the dictator and the recipient). In Figure 1.2, if the dictator chooses the *Top* option, she allocates herself 140 ECUs and her partner 40 ECUs. Conversely, if the dictator chooses the *Bottom* option, she allocates herself 80 ECUs and her partner 80 ECUs, thus reducing the payoff inequality.

Figure 1.2: Example of decision matrix from the modified dictator game.

	Own payoff	Partner's payoff
<input type="radio"/> Top	140	40
<input type="radio"/> Bottom	80	80

Participants in the recipient branch face the same 15 decision matrices and must guess, for each decision matrix, the dictator's choice. Half of the recipients in each treatment receive a payment according to the dictator's decision. For the other half, one of the guesses is randomly selected and compared to the dictator's decision. If the guess is correct, the recipient receives a bonus payment of £0.5. Recipients are informed that their bonus payment is determined either by the dictator's choice in the choice task or from the accuracy of the guess, not both.¹³ The bonus payment is determined by a random draw between the two tasks.

¹²The original game from Kranton et al. (2020) comprises 26 decisions. Simulations using estimates from Kranton et al. (2020) show that the estimations converge towards the original parameter value using only 15 decisions.

¹³The bonus payment for recipients is set up this way to prevent hedging effects (Armantier and Treich, 2013).

3.3 Belief elicitation task

In the second part of the experiment, I recover the participants' beliefs. The belief section is divided into two subsections, displayed in random order, and is the same for dictators and recipients. The outcomes recovered in this section are not part of the pre-registered analyses and are non-incentivized. First, based on the identity information given earlier in the experiment (see section 3.1), participants are asked to report their beliefs about their partners' annual income category (broken into 11 categories, from *Less than \$10,000* to *\$100,000 or more*, in increments of \$10,000), political orientation (i.e., *independent*, *left-wing* or *right-wing*), gender (i.e., *male* or *female*) and sexual orientation (i.e., *heterosexual* or *Lesbian Gay Bisexual Transgender (LGBT)*). The questions appear in random order on a unique screen. Second, I recover participants' beliefs on the number of vegetarians and the number of vegans in the United States. Participants are asked to report their beliefs on the percentage of vegetarians and vegans in the United States. The two questions appear on the same screen.

3.4 Socio-demographic survey

In the final part of the experiment, participants answer survey questions. Dictators answer two psychological scales, the Vegetarian (resp. Vegan) Threat Scale and the Social Dominance Orientation Scale, in random order of appearance. The Vegetarian (resp. Vegan) Threat Scale (Dhont and Hodson, 2014) is a psychological scale designed to capture one's perceived degree of threat from the rise of vegetarianism (resp. veganism). The scale comprises 8 items, displayed in random order, for which dictators report their degree of agreement with the items using a 7-point Likert scale (i.e., answers range from 1 *strongly disagree* to 7 *strongly agree*). Dictators in the *VGT* (resp. *VGN*) treatment answer the Vegetarian (resp. Vegan) Threat Scale. Half of the dictators in the *OMNI* treatment are randomly assigned to the Vegetarian Threat Scale and the other half to the Vegan Threat scale. Higher (resp. lower) scores on the scale denote greater (resp. lower) perceived threats from the rise of vegetarianism (resp. veganism). The questions are summarized in Table A2. Dictators also answer the Social Dominance Orientation Scale (Pratto et al., 1994), which aims at capturing one's degree of preference for social dominance among groups in society. I reduced the original scale from 16 to 8 items. Dictators report how they feel (i.e., answers range from 1 *very negative* to 7 *very positive*)

regarding the 8 items, displayed in random order. Higher (resp. lower) scores on the scale denote an increased (resp. decreased) preference for situations in which one or several groups dominate the others. The questions are summarized in Table A3.

Recipients answer the Everyday Discrimination scale and the Social Dominance Orientation Scale, in random order of appearance. Recipients do not answer the Vegetarian (resp. Vegan) Threat Scale. The Everyday Discrimination (Williams et al., 1997) recovers one’s perceived everyday discrimination in different situations. I modified the questions to identify the frequency of discriminating experiences felt by participants due to their dietary choices. Recipients report how often they experience 8 discriminating experiences (i.e., answers range from 1 *never* to 6 *almost everyday*), displayed in random order. Higher (resp. lower) scores on the scale denote an increased (resp. decreased) frequency of situations in which a participant’s dietary choices made her feel discriminated against. The questions are summarized in Table A4. The Social Dominance Orientation Scale is the same as for the dictator branch.

Lastly, all participants answer standard demographic questions on age, income, political orientation, religious family, religious attendance, presence of a vegetarian or vegan family member, presence of a vegetarian or vegan friend, ethnicity, and highest education level.

4 Theoretical Framework

4.1 Theoretical background

I use an outcome-based model of social preferences to quantify vegophobia. Outcome-based models define the utility function of individual i as dependent on player i ’s own payoff and another player j ’s payoff (Jacquemet and L’Haridon, 2018). Standard economic theory suggests that under an incentivized scheme, the individual i should maximize her payoff. However, extensive literature demonstrates that individuals may have other-regarding preferences.¹⁴ Following Akerlof and Kranton (2000), individuals also derive utility payoffs from their choice set of identities.

¹⁴See for example (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000, Charness and Rabin, 2002, Falk and Fischbacher, 2006, Chen and Li, 2009)

Consequently, incorporating identity interactions in the outcome-based model is one way to study the impact of identity on payoff differences. Specifically here, I quantify vegophobia under the assumption that vegetarianism and veganism are social identities (Rosenfeld and Burrow, 2017a, Plante et al., 2019, Nezlek and Forestell, 2020) and that the saliency of such identities modifies one’s social preferences.

Among the various outcome-based models of social preferences, inequity aversion by Fehr and Schmidt (1999) is a popular and extensively studied model. One feature of this model is that it distinguishes between guilt and envy in situations involving an allocation. The utility function of player i is modeled as dependent on her own payoff and some concern for differences in payoffs between i and j , described as inequity aversion. The player’s i utility function is defined as:

$$U_i(x) = x_i - \alpha_i \max(x_j - x_i, 0) - \beta_i \max(x_i - x_j, 0), \quad i \neq j$$

where x_i represents the player’s i payoff, α_i the utility loss player i suffers from disadvantageous inequality (i.e., envy), and β_i the utility loss from advantageous inequality (i.e., guilt).

In order to quantify vegophobia with the inequity aversion model, I compare the sensitivity of the α_i and β_i parameters between treatments (i.e., contingent on the matched partners’ given identity). For dictators, I interpret an increase in envy between treatments, $\alpha_i^{VGT} < \alpha_i^{OMNI}$ or $\alpha_i^{VGN} < \alpha_i^{OMNI}$, and a decrease in guilt, $\beta_i^{VGT} > \beta_i^{OMNI}$ or $\beta_i^{VGN} > \beta_i^{OMNI}$, as vegophobia. For recipients, I interpret an increase in dictators’ expected envy between treatments, $\alpha_i^{VGT} < \alpha_i^{OMNI}$ or $\alpha_i^{VGN} < \alpha_i^{OMNI}$, and a decrease in expected guilt, $\beta_i^{VGT} > \beta_i^{OMNI}$ or $\beta_i^{VGN} > \beta_i^{OMNI}$, as perceived vegophobia.

One important note is that social identity derived from vegetarianism and veganism results from a choice rather than an inherited characteristic, and may therefore respond to different mechanisms. Several papers study discrimination through social preferences using inherited or naturally occurring characteristics such as gender (Slonim and Guillen, 2010), ethnicity (Cetre et al., 2020) or sexual orientation (Aksoy et al., 2023). Other papers focus on choice-induced identities such as religion (Benjamin et al., 2016) or political preferences (Kranton et al., 2020, Dimant, 2023). The magnitude differences in the effect sizes between inherited or naturally occurring identities and choice-based identities are difficult to compare as papers use

different theoretical approaches. Specifically, by foregoing meat, vegs adopt a social identity that deviates from the norm of consuming meat and intentionally set themselves apart, potentially leading to vegephobia. Akerlof and Kranton (2000) assert that, when individual j intentionally deviates from the norm, it affects individual i and may lead to distancing to reaffirm i 's self-image. While switching to plant-based diets is associated with positive externalities (e.g., lower GHG emissions) and contributes to social welfare, individuals may incur a social cost in the form of vegephobia that could prevent more individuals to adopt plant-based diets. In this paper, I focus on quantifying the presence of vegephobia in an economic environment and leave the identification of mechanisms open for future research.

4.2 Pre-registered hypotheses

I pre-register two hypotheses. First, findings from the literature described in Section 2.1 point towards out-group bias and in-group favoritism when identity is salient. Second, the literature on vegephobia (see Section 2.2) indicates that a vegetarian or vegan diet is enough to induce an in-group out-group effect from meat-eaters (Ruby, 2012, Graça et al., 2015), that leads to discriminating behaviors (Minson and Monin, 2012, Rothgerber, 2014, Markowski and Roxburgh, 2019, Judge and Wilson, 2019). In line with these findings, I expect that exposing omnivores to vegs will be enough to induce out-group bias and discrimination. I pre-register two main hypotheses for the confirmatory analysis and leave the rest for the exploratory analyses. Following the magnitude of the effect sizes in Kranton et al. (2020), I focus the hypothesis testing solely on the envy parameter and leave the guilt parameter for exploratory results. I also leave perceived inequity aversion parameters for exploratory results as no previous work has focused on this issue. Therefore, the pre-registered hypotheses are set up to assess whether α_i is statistically smaller in *VGN* and *VGT*, in comparison with *OMNI*. The hypotheses are presented below.¹⁵

H1 - Dictators will display greater envy (i.e., aversion to disadvantageous inequality) with vegans than with omnivores.

H2 - Dictators will display greater envy (i.e., aversion to disadvantageous inequality) with vegetarians than with omnivores.

¹⁵The hypotheses wording slightly differs from the pre-registration as the word envy was added for clarity.

4.3 Empirical approach

The pre-registered hypotheses test whether a variation in recipients' dietary identity, in comparison with *OMNI* (i.e., omnivore recipient), leads to an increase in the dictators' envy (i.e., α_i parameter statistically smaller in *VGT* and *VGN*). In accordance with the pre-registered analysis plan, I test hypotheses H1 and H2 using one-sided tests. I retain a statistical significance level of $\alpha = 0.05$ and correct p-values for multiple hypothesis testing with a Holm-Bonferroni correction for two hypotheses. The pre-registered analyses are detailed in the Study Design Table A5 and were pre-registered on the Open Science Framework.¹⁶ The hypothesis set up is displayed in Table A6.

In order to test the hypotheses, I first estimate the inequity aversion parameters for the total sample and within each treatment using a mixed logit model assuming a correlation between α_i and β_i . I also estimate a treatment-dependent interaction term on the α_i for *VGN* and *VGT*. The interaction term captures the α_i parameter variation from *OMNI*. I then divide the value of the parameters and treatment-dependent interaction term by the weight player i puts on her payoff to recover their true value.

Additionally, I ran a pilot session in January 2022. The pilot session replicated the final experiment with the same compensation scheme but only explored the *OMNI* and *VGN* treatments. The inequity aversion parameters and treatment effect estimations from the pilot are available in the Supplementary Materials. I used the estimates from the pilot data to calibrate the power analysis and find the number of observations needed to reject the null hypotheses for *H1* and *H2*. The simulations ($S = 1,000$) yield satisfactory power for α_i with 400 participants per treatment. The estimated probabilities to detect a positive difference are 100% for α_{VGN} and 82.3% for α_{VGT} (see the Supplementary Materials). I retain a final sample size of 400 participants per treatment.

¹⁶The complete pre-registration is available in the Supplementary Materials and online here: <https://osf.io/h92wn>.

5 Results

5.1 Procedure

Participants were recruited on the platform Prolific and completed the experiment online via LimeSurvey. The experiment lasted average 13 minutes and participants received a £1.83 fixed fee. Additionally, participants were informed that they had the opportunity to earn up to £1 in the form of a bonus payment.¹⁷ The amounts were given in pounds as it is the currency used by Prolific, regardless of the participants' location. For transparency and comprehension reasons, I provided participants with the pound-to-dollar conversion rate as they were located in the United States. The partner matching occurred ex-post and participants received their bonus payments in the days following the experiment.

5.2 Sample

In total, 1483 participants from the United States were recruited on Prolific. The sample size was determined using a power analysis calibrated on the pilot data effect sizes (see Section 4.3). There are two main advantages to running this experiment online. First, vegs represent a small portion of the population¹⁸ and are therefore difficult to recruit for a lab experiment without inducing any selection bias. Prolific solves this issue by allowing to recruit participants based on personal characteristics available in their Prolific profile. Participants are not aware that they are recruited for having these characteristics, thus not compromising the purpose of the experiment. Second, lab experiments mostly involve student participants. While student samples are convenient, they are a sub-group of the general population and tend to be younger, more educated, and more homogeneous than non-student samples (Belot et al., 2015). In the case of vegophobia, the use of such a sample could lead

¹⁷A fixed fee of £1.83 for 13 minutes was considered "Good" by Prolific at the time of the experiment. Participants had the opportunity to earn up to £2.83 based on their decisions, which is much higher than the usual Prolific rates.

¹⁸Plant-based diets represent 5% to 8% of the population in the US (see Table A1 and (Reinhart, 2018))

to more plant-based diet-friendly attitudes¹⁹ and an underestimation of the actual phenomenon.

The data collection process took place between June and August 2022. After cleaning the data and applying the pre-registered exclusion rules, the final sample is composed of 1436 observations, with 1158 dictators and 278 recipients.²⁰

5.3 Summary statistics

Summary statistics of dictators and recipients are displayed respectively in Tables A7 and A8. Dictator samples are balanced across treatments except for the age variable. Dictators in *OMNI* are older than dictators in *VGN* and *VGT* (see Table A9). The mean age is 38.63, which is close to the median US age of 38.2.²¹ The dictator's type profile is a white left-winger, who earns between \$10,000 and \$99,999, is highly educated (i.e., bachelor's degree or higher), and is Christian but never attends religious service.

Recipient samples are also balanced with small exceptions. Females are over-represented in *VGT* and *VGN*. More recipients are Christian and closer to the right-winger in *OMNI*, while there are more left-wingers in *VGT* and *VGN*. Participants who earn less than \$10,000 are underrepresented in *VGT*. Finally, participants with a master's degree are underrepresented in *VGN*. The mean age of recipients is 34.58, with younger recipients in *VGN* (see Table A10). The typical recipient profile is a white left-winger female who earns between \$10,000 and \$99,999, is highly educated, and never attends religious service. Overall the reported differences are economically insignificant and will be accounted for in the estimations.

¹⁹Previous studies have shown that older and less educated individuals are more likely to have negative attitudes towards plant-based diets (Vandermoere et al., 2019, Rosenfeld and Tomiyama, 2020)

²⁰For details on the pre-registration and exclusion rules please see the Supplementary Materials.

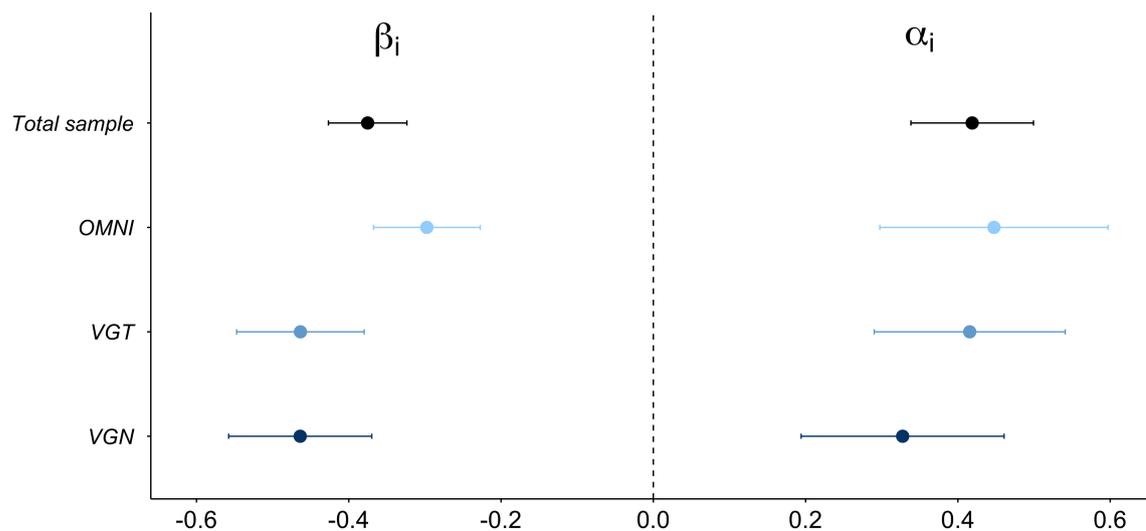
²¹This information was taken from `data.census.gov/`.

5.4 Confirmatory results

This section presents the results from the pre-registered analysis and hypotheses detailed in Section 4 and Study Design Table A5.

Figure 1.3 reports the estimated α_i and β_i parameters.²² First, the estimation yields a positive coefficient for α_i and a negative for β_i , verifying the assumption from Fehr and Schmidt (1999) that $\alpha_i \geq \beta_i$. Second, I focus on the envy results (i.e., α_i parameter) as they are part of the pre-registered analysis, and leave the guilt results (i.e., β_i parameter) for exploratory results. The interaction terms on α_i yield no significant differences between *OMNI* and *VGN*, and *OMNI* and *VGT* (two-sided coefficient testing α_i , p-value > 0.1), indicating no treatment differences with two-sided testing (see Table A11).

Figure 1.3: Inequity aversion parameters estimation for the total sample and by treatment.



Note: Inequity aversion parameters estimated using a mixed logit model assuming a correlation between the α_i and β_i parameters. Estimations include 15 decisions per participant. Each point corresponds to an independent estimation. The estimation for *Total sample* regroups all the observations regardless of the treatment ($N = 17370$). The other estimations include respectively dictators from *OMNI* ($n = 5760$), *VGT* ($n = 5805$), and *VGN* ($n = 5805$). Errors bars represent 95% confidence intervals. No control variables are included. The estimated parameters and treatment differences using two-sided tests are reported in Table A11.

²²The estimated parameters and treatment-dependent interaction terms are available in Table A11.

I proceed to test hypotheses H1 and H2, according to the pre-registered analyses. The results are reported in Table 1.1. I fail to reject the null hypotheses for H1 and H2, confirming that there are no statistical differences between *OMNI* and *VGN*, and *OMNI* and *VGT* (one-sided α_i coefficient testing, p-value > 0.05). Contrary to previous literature findings, dictators do not display greater envy with vegans or vegetarians than with omnivores. This result suggests that adopting a plant-based diet is not enough to induce out-group discrimination, and provides evidence against the presence of vegephobia in an experimental economic environment. As pre-registered robustness checks, I verify the sensitivity of the confirmatory results to changes in statistical models and the inclusion of control variables. The confirmatory results are robust to additional pre-registered robustness checks (see Table A12 in Appendix).

Confirmatory result 1

Dictators do not display greater envy with vegans in comparison with omnivores (i.e., H_1 p-value > 0.05). Hypothesis H1 is not verified.

Confirmatory result 2

Dictators do not display greater envy with vegetarians in comparison with omnivores (i.e., H_2 p-value > 0.05). Hypothesis H2 is not verified.

Table 1.1: Pre-registered hypothesis testing.

Hypotheses	Effect size	Unadjusted p-values	Holm-Bonferroni adjusted p-values	Num. obs.
$H_0^1: \alpha_{VGN} \geq \alpha_{OMNI}$	-0.029	0.618	0.689	11565
$H_1^1: \alpha_{VGN} < \alpha_{OMNI}$	(0.097)			
$H_0^2: \alpha_{VGT} \geq \alpha_{OMNI}$	0.033	0.345	0.689	11565
$H_2^2: \alpha_{VGT} < \alpha_{OMNI}$	(0.083)			

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Hypothesis setup presented with null hypothesis H_0 and alternative H_1 . Estimations using a mixed logit model assuming a correlation between the α and β parameters. Coefficient testing is assessed using one-sided tests. 15 decisions per participant. Standard errors are shown in parentheses. No control variables are included.

5.5 Exploratory results

In the following section, I explore additional unregistered results from the experiment.

The confirmatory results show that dictators do not display greater envy with vegans and vegetarians than with omnivores, rejecting the two pre-registered hypotheses. This result also holds when comparing dictators matched with vegetarians and those matched with vegans (two-sided α_i coefficient testing, p-value > 0.1 ; see Table 1.2), showing no overall effect of plant-based diet identity on envy.²³

I then explore dictators' sensitivity to guilt. A dictator's behavior is considered discriminatory if she displays less guilt with vegs than with omnivores (i.e., β_i smaller in *VGT* and *VGN* than in *OMNI*). Table 1.2 reports the β_i parameter estimations. Dictators in *VGT* display less guilt than dictators in *OMNI* (two-sided β_i coefficient testing, p-value < 0.01). In other words, dictators benefit positively from having more than vegetarian recipients, in comparison with dictators matched with omnivore recipients. There are no statistical differences on the guilt parameter between *VGN* and *OMNI*, nor between *VGT* and *VGN* (two-sided β_i coefficient testing, p-value > 0.1).²⁴ Overall, dictators' inequity aversion parameters at the group-level are weakly sensitive to vegetarian and vegan identity.

Exploratory result 1

Dictators display less guilt with vegetarians than with omnivores (two-sided β_i coefficient testing, p-value < 0.01).

²³For this section, I use two-sided testing as the analyses were not pre-registered and increase the statistical significance threshold to $\alpha = 0.1$, as per use in economics.

²⁴The difference between *VGT* and *VGN* becomes significant with the inclusion of control variables, suggesting heterogeneous effects. I further explore this issue in the following exploratory results.

Table 1.2: Dictators' inequity aversion parameters and treatment differences.

Inequity aversion parameters treatment differences	α_i	β_i	Controls	Num. Obs.
<i>OMNI</i> × <i>VGT</i>	0.033 (0.083)	-0.152*** (0.057)	No	11565
	0.016 (0.083)	-0.193*** (0.058)	Yes	11565
<i>OMNI</i> × <i>VGN</i>	-0.029 (0.097)	-0.030 (0.079)	No	11565
	-0.012 (0.092)	-0.082 (0.056)	Yes	11565
<i>VGN</i> × <i>VGT</i>	0.048 (0.081)	-0.108 (0.066)	No	11610
	-0.006 (0.078)	-0.181*** (0.054)	Yes	11595

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. Treatment differences are assessed using two-sided coefficient testing. P-values are uncorrected for multiple-hypothesis testing. Control variables include age, sex, annual income, ethnicity, education, and political preferences.

The analysis of treatment differences at the group level mostly rejects the presence of vegophobia. I then focus the analysis at the individual level and investigate whether dictators' heterogeneity influences guilt and envy. Tables A13, A14 and A15 report the heterogeneity analysis within each treatment. The analysis provides mixed results. Female dictators in *OMNI* display less guilt than male dictators (two-sided β_i coefficient testing, p-value < 0.05), while female dictators in *VGN* display less envy²⁵ (two-sided α_i coefficient testing, p-value < 0.05). This result confirms previous findings that females have more positive attitudes towards plant-based diets (Pohjolainen et al., 2015, Judge and Wilson, 2019, Vandermoere et al., 2019). Surprisingly, and contrary to the literature (Pohjolainen et al., 2015, Vandermoere et al., 2019), dictators with higher levels of education (i.e., bachelor degree or higher) discriminate more by displaying greater envy in *VGT* and *VGN* (two-sided α_i co-

²⁵There is no effect of sex in *VGT*.

efficient testing, p-value < 0.05 and < 0.01 respectively). The effect is strong for dictators in *VGN* but is contrasted by the display of more guilt in *VGT* (two-sided α_i coefficient testing, p-value < 0.05). Dictators closer to the left wing or independent, in comparison with those closer to the right-wing, display less guilt in *VGT* and *VGN* (two-sided β_i coefficient testing, p-value < 0.01 and < 0.1 , respectively). Once again, this result is unexpected as previous findings pointed out that meat consumption and meat-positive attitudes are often associated with more traditional views (Pohjolainen et al., 2015) and right-wing ideology (Dhont and Hodson, 2014, Dhont et al., 2016).

The heterogeneity analysis also reveals novel results. In *OMNI*, white dictators display less guilt (two-sided β_i coefficient testing, p-value < 0.05), but display more envy in both *VGN* and *VGT* (two-sided α_i coefficient testing, p-value < 0.05). The effect on envy seems mainly driven by discriminatory behaviors from whites towards vegs. A deeper analysis reveals that African Americans have more positive attitudes toward vegs, while some ethnic groups have different preferences depending on the type of veg diet (see Appendix A.4.2). These results suggest that ethnicity could be an important driver of vegephobia, but more research is needed to confirm.

Exploratory result 2

Demographic characteristics matter with vegephobia. Females discriminate less, whereas dictators who feel closer to the left-wing or independent, have higher levels of education, and are white discriminate more.

Vegephobia might also be affected by the dictators' frequency of exposure to vegs. Contact theory suggests that increased contact between groups decreases intergroup conflict and prejudice (Allport et al., 1954).²⁶ Applying this framework to vegephobia, I assess whether the presence of a veg family member, friend, or both has an impact on the dictators' inequity aversion parameters. First, given the relatively low percentage of vegs in the US,²⁷ dictators in this study appear to have proximity and contact with vegs. In the sample, 22% of dictators report having a

²⁶For examples of contact theory see (Van Laar et al., 2005, Boisjoly et al., 2006, Steinmayr, 2021).

²⁷Plant-based diets represent 5% to 8% of the population in the US (see Table A1 and (Reinhart, 2018)).

veg family member, 55% a veg friend, and 16% both (see Table A19).²⁸ I estimate the inequity aversion parameters controlling for the presence of vegs in the dictators' social environment. I focus here only on the *VGT* and *VGN* treatments and estimate within treatment variations. Table 1.3 reports the estimated coefficients. For the family circle, the presence of a veg family member has no effect on inequity aversion in *VGT* and *VGN* (two-sided α_i and β_i coefficient testing, p-value > 0.1). For the friend circle, in both *VGT* and *VGN*, dictators who report having a veg friend display less guilt, (two-sided β_i coefficient testing, p-value < 0.05 and < 0.05 respectively) and more envy (two-sided α_i coefficient testing, p-value < 0.01 and < 0.1 respectively).²⁹ Overall, having a veg family member has no influence on social preferences, but the presence of a veg friend appears to increase vegophobia.

This result is surprising for two reasons. First, it goes against contact theory (Allport et al., 1954) and previous results showing lower levels of discrimination with the presence of out-group friendships (Herek and Capitanio, 1996, Pettigrew and Tropp, 2013). Second, Vandermoere et al. (2019) showed that a veg presence in the social circle reduced vegophobia.³⁰ The main difference here is that the choice task involved revealed preferences with monetary incentives (versus survey data for Vandermoere et al. (2019)). Individuals may therefore respond differently to a vegetarian and vegan identity under contact theory when monetary incentives are involved, but more specific research on this effect is needed to confirm.

Exploratory result 3

The presence of a vegetarian or vegan friend in the social circle increases vegophobia in the form of less guilt and greater envy.

As shown above, patterns of discrimination emerge at the individual level, but the presence of vegophobia is mostly rejected at the group level. I now turn to the recipient side and verify how discrimination is perceived by vegs. First, I focus the analysis on the recipients' perception of discriminatory behaviors using the Everyday Discrimination scale (see Section 3.4). The scores are reported in figure 1.4.

²⁸There are no differences in the proportion of dictators who report having a veg friend and a veg family member between treatments (see Figure A2 and A3).

²⁹The effect on envy in *VGN* fades away with the inclusion of control variables the effect sizes stay relatively stable (i.e., no great changes in direction or magnitude).

³⁰Vandermoere et al. (2019) report that a veg presence in the household and a veg friend in one's social circle are negatively correlated with vegophobia.

Table 1.3: Influence of a vegetarian/vegan presence in the social circle on the inequity aversion parameters, by treatment.

	(1)		(2)		(3)		(4)		(5)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i
VGT	0.358*** (0.065)	-0.510*** (0.041)	0.270*** (0.082)	-0.336*** (0.053)	0.358*** (0.064)	-0.437*** (0.046)	0.256*** (0.084)	-0.332*** (0.055)	0.207 (0.168)	-0.076 (0.109)
Presence of vegetarian/vegan family members	0.156 (0.125)	0.001 (0.086)					0.091 (0.130)	-0.020 (0.086)	0.109 (0.129)	-0.043 (0.099)
Presence of vegetarian/vegan friends			0.240** (0.108)	-0.238*** (0.072)			0.230** (0.108)	-0.236*** (0.073)	0.158 (0.109)	-0.261** (0.108)
Presence of vegetarian/vegan family and friends					0.283* (0.158)	-0.121 (0.106)				
Num. obs.	5805		5805		5805		5805		5805	
Controls	No		No		No		No		Yes	

	(1)		(2)		(3)		(4)		(5)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i
VGN	0.399*** (0.079)	-0.298*** (0.043)	0.278*** (0.095)	-0.237*** (0.051)	0.398*** (0.078)	-0.301*** (0.043)	0.274*** (0.097)	-0.237*** (0.052)	0.303 (0.215)	0.031 (0.123)
Presence of vegetarian/vegan family members	0.110 (0.167)	-0.067 (0.096)					0.044 (0.173)	0.102 (0.111)	0.002 (0.150)	-0.016 (0.105)
Presence of vegetarian/vegan friends			0.247* (0.133)	-0.197* (0.104)			0.247* (0.136)	-0.230** (0.091)	0.140 (0.127)	-0.281*** (0.076)
Presence of vegetarian/vegan family and friends					0.156 (0.186)	-0.055 (0.098)				
Num. obs.	5805		5805		5805		5805		5790	
Controls	No		No		No		No		Yes	

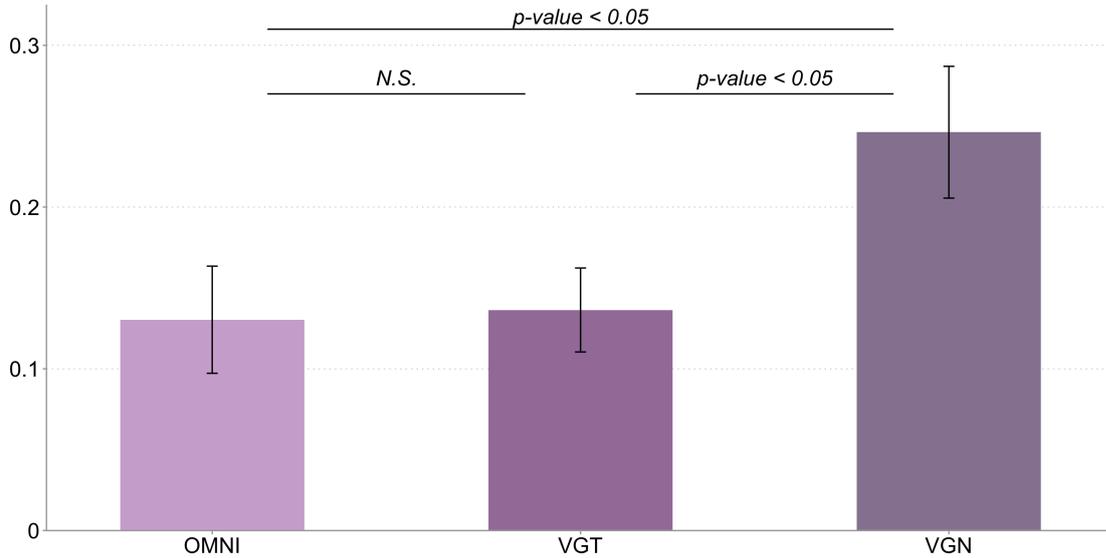
Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. Treatment differences are assessed using two-sided coefficient testing and dummy variables. The dummy variable is equal to 1 if the participant has a vegetarian/vegan family member, and 0 if not. The dummy variable is equal to 1 if the participant has a vegetarian/vegan friend, and 0 if not. The dummy variable is equal to 1 if the participant has a vegetarian/vegan family member and friend, and 0 if not. Control variables include age, sex, annual income, ethnicity, education, and political preferences. For regressions with VGN, one observation was dropped when including the controls as the sex variable had "consent revoked".

Vegans report significantly higher scores on the scale in comparison with omnivores and vegetarians (two-sided Wilcoxon rank-sum test, p -value < 0.01). There are no significant differences between omnivores and vegetarians (two-sided Wilcoxon rank-sum test, p -value > 0.1). This result suggests that vegophobia is perceived and reported, but that differences in identity profiles between vegetarians and vegans surface. While refraining from consuming meat has no impact on perceived discrimination, refraining from consuming animal products increases it. This is in line with previous findings showing that vegans are judged more negatively than vegetarians, and experience more discrimination (Judge and Wilson, 2019).

Exploratory result 4

Vegans report greater discrimination due to their dietary choices than vegetarians and omnivores. There are no differences between omnivores and vegetarians.

Figure 1.4: Recipients' everyday discrimination mean score, by treatment.



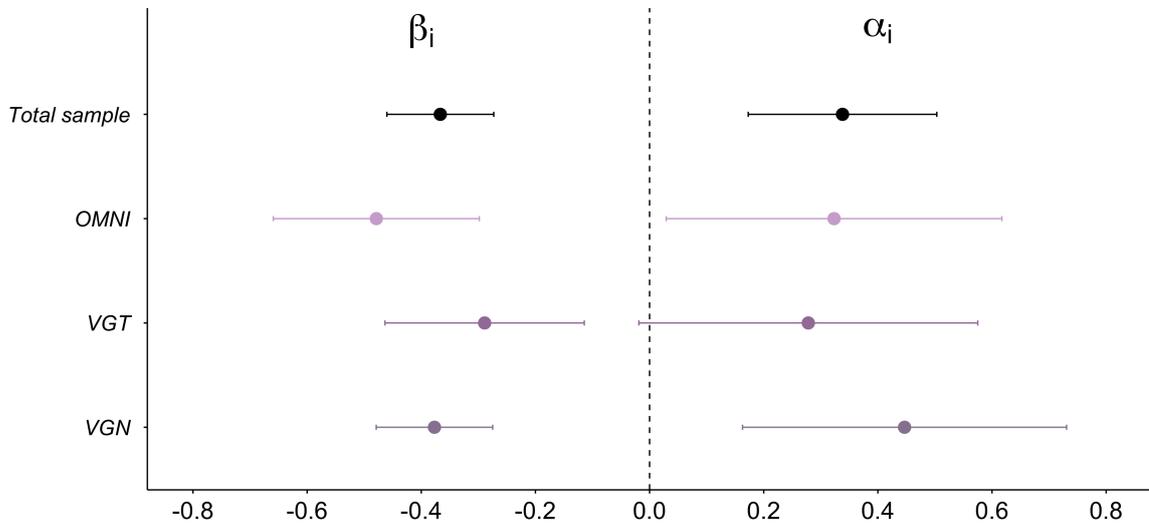
Note: Treatment comparisons using Wilcoxon rank-sum test. Errors bars represent 95% confidence intervals. Scores are normalized between 0 and 1. N = 278 (observations by treatment: OMNI = 98, VGT = 99, VGN = 81).

Second, I explore expectations of discrimination in the choice task. Recipients were asked to guess, for each decision matrix, the dictator's choice (see Section 3.2). Using the same empirical approach as for dictators, I estimate perceived inequity aversion parameters and treatment-dependent interaction terms. Figure 1.5 reports the estimated perceived α_i and β_i parameters.³¹ The estimations show that recipients have accurate beliefs. The perceived parameters yield a positive expected value α_i and a negative expected value β_i , for the total sample and within each treatment, which corresponds to the directions found in Section 5.4. The perceived coefficients' directions also confirm the assumption from Fehr and Schmidt (1999) that $\alpha_i \geq \beta_i$.

Table 1.4 reports the treatment differences. Vegans expect their matched dictator to display greater guilt than omnivore and vegetarian recipients (two-sided β_i

³¹The estimated perceived parameter are available in Table A20.

Figure 1.5: Perceived inequity aversion parameters estimation for the total sample and by treatment.



Note: Perceived inequity aversion parameters estimated using a mixed logit model assuming a correlation between the α_i and β_i parameters. Estimations include 15 decisions per participant. Each point corresponds to an independent estimation. The estimation for *Total sample* regroups all the observations regardless of the treatment ($N = 4170$). The other estimations include respectively dictators from *OMNI* ($n = 1470$), *VGT* ($n = 1485$), and *VGN* ($n = 1215$). Errors bars represent 95% confidence intervals. No control variables are included. The estimated parameters are reported in Table A20.

coefficient testing, p -value < 0.05 and < 0.10 respectively).³² This absence of perceived discrimination by vegs, and evidence of expectations of pro-social behaviors by vegan recipients in the choice task are unexpected and can be interpreted in two ways. First, vegs have been shown to be more pro-social individuals (Rosenfeld and Burrow, 2017b, Rosenfeld et al., 2020), with higher levels of prosociality for vegans (Rosenfeld, 2019a, Kirsten et al., 2020). This tendency is confirmed, with vegs displaying greater pro-social behaviors than omnivores (see Appendix A.7). Therefore, vegans' expectations of pro-social behaviors in the choice task might stem from a bias toward their own morality. Vegans might project their own preferences when asked to guess the dictators' choices and bias the analysis with more pro-social guesses. Nevertheless, this phenomenon should be countered by the financial incentives involved (see section 3.2). A second explanation could be that the economic tools used in this experiment fail at capturing discrimination. On one hand, vege-

³²The analysis is underpowered due to the low number of recipients in each treatment.

phobia is well documented and reported by vegans in the Everyday Discrimination Scale (see Figure 1.4). On the other hand, the estimations of inequity aversion and perceived inequity aversion parameters fail at capturing the reported discrimination, even though previous work using similar tools identified group-level discrimination (Chen and Li, 2009, Kranton et al., 2020). The results presented in this paper could indicate that the previous results in experimental economics underestimate the actual effect sizes of the discrimination and that there is room for the development of better tools. This conjecture is not assessable with the available data. Once again, this result is inferred from the exploratory analysis and more specific research is needed to confirm.

Exploratory result 5

Vegetarians and vegans, in comparison with omnivores, do not expect greater discrimination in the choice task.

Table 1.4: Recipients' perceived inequity aversion parameters and treatment differences.

Perceived inequity aversion parameters treatment differences	α_i	β_i	Controls	Num. Obs.
<i>OMNI</i> vs <i>VGN</i>	-0.078 (0.204)	0.220* (0.125)	No	2685
	-0.032 (0.219)	0.320** (0.140)	Yes	2685
<i>OMNI</i> vs <i>VGT</i>	0.069 (0.095)	0.040 (0.046)	No	2955
	-0.006 (0.101)	0.081 (0.060)	Yes	2955
<i>VGN</i> vs <i>VGT</i>	0.183 (0.192)	-0.145 (0.102)	No	2700
	0.013 (0.190)	-0.202* (0.116)	Yes	2700

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. Treatment differences are assessed using two-sided coefficient testing. P-values are uncorrected for multiple-hypothesis testing. Control variables include age, sex, annual income, ethnicity, education, and political preferences.

6 Discussion and Conclusion

The current food system is unsustainable and reductions in meat consumption are associated with positive health, environmental, and animal-welfare benefits (Clark and Tilman, 2017, Springmann et al., 2018a). While switching to plant-based diets becomes necessary to mitigate meat-related negative externalities, it involves adopting a new social identity (Rosenfeld and Burrow, 2017a, Nezlek and Forestell, 2020) that divides individuals into groups. This may lead to discriminatory behaviors known as vegephobia and prevent individuals from becoming vegetarian or vegan (Markowski and Roxburgh, 2019).

In this paper, I propose a first attempt to measure the presence of vegephobia in an experimental economic environment. I focus on observing the presence or absence of vegephobia rather than identifying the underlying mechanisms. Using a between-subject design in an online experiment and a modified version of the dictator game (Kranton et al., 2020), I expose omnivore dictators to omnivore, vegetarian and vegan recipients, and make dietary identity salient. Additionally, I recover recipients' expectations of discrimination. The method I use provides rich statistical information that enables the estimation of inequity aversion parameters (Fehr and Schmidt, 1999). I pre-register two directional hypotheses on the sensitivity of dictators' envy conditional on the recipient's dietary identity and leave the rest of the results for exploratory analyses.

The confirmatory analysis does not allow to reject the null hypothesis and concludes to the absence of vegephobia in this specific experimental economic environment. The results are robust to changes in statistical models and the inclusion of control variables. Further explorations confirm the absence of discrimination towards vegans but suggest vegephobia guided towards vegetarians in the form of less aversion to guilt. At the individual level, discriminatory behaviors are driven by ethnicity (i.e., whites), education (i.e., higher education levels), sex (i.e., male), and political preferences (i.e., left-wingers or independents). Social environment also matters, as having a veg friend increases vegephobia, going against intuitions from contact theory.

For recipients, vegans, in comparison with omnivores and vegetarians, report experiencing greater discrimination due to their dietary choices in their everyday life. Surprisingly, this result does not carry on to the choice task as veg recipients do not expect more discrimination than omnivore recipients. Financial incentives are

involved, which rules out the conjecture that discrimination is not detected because vegs project their own preferences when asked to guess the dictators' choices.³³ Rather, the economic tools used in the experiment might not be powerful enough to capture vegophobia. While similar tools have been proficient with other types of social identities (Chen and Li, 2009, Kranton et al., 2020), they seem unable to capture vegophobia, even though it is well-identified in the literature (see Section 2) and reported by vegans (see Figure 1.4). This suggests that previous findings using these tools might have underestimated the real effect sizes of the discrimination.

The results of the paper are possibly mitigated by some unanticipated factors. First, the salience of the recipient's dietary identity might have induced some experimenter demand (Zizzo, 2010). To anticipate this effect, I ran two pilot sessions (see Supplementary Materials). The first pilot suggested the presence of experimenter demand when the dietary identity was highly salient in comparison with a low-salience option. The second pilot session, using the low-salience option from the first pilot session, indicated the presence of vegophobia and minimal levels of experimenter demand. Additional steps were taken to minimize the presence of experimenter demand such as total anonymization of answers, randomization in the order of identity characteristics, and monetary incentives. Nevertheless, I cannot completely rule out the presence experimenter demand effect.

Second, the experimental setup might also not be ideal to unveil the presence of vegophobia. Prolific offers the advantage of selecting participants based on characteristics without revealing the purpose of the experiment. The counterpart is that Prolific samples tend to be overly cooperative in comparison with other online survey platforms (e.g., ClourdResearch) or laboratory participants (Gupta et al., 2021). Evidence also suggests that the use of an adult sample over a student sample can lead to more pro-social behaviors (Anderson et al., 2013). The experimental setting also likely leads to an underestimation of vegophobia due to the identity information. While the dietary identity is made salient in the experiment, it is not as disruptive as it would be in a real-life social interaction. Dietary identities induced by vegetarianism and veganism are mostly revealed in social events (i.e., gatherings with food) or during demonstrations (e.g., PETA protest). These situations create more

³³Vegs are more pro-social individuals (Rosenfeld and Burrow, 2017b, Rosenfeld et al., 2020). This is also confirmed by the Social Dominance Orientation scores reported in Appendix A.7.

confrontation and can lead to greater vegephobia. Finally, the sample is plant-based diet friendly,³⁴ which also possibly hinders the presence of vegephobia.

Overall, this paper shows no evidence of vegephobia in an experimental economic environment from omnivores towards vegetarians and vegans. Exploratory analyses reveal heterogeneity among participants and some forms of vegephobia, but no clear pattern of discrimination emerges. Vegephobia is reported by vegans but the economic tools fail at capturing the phenomenon. The results suggest that the economic tools employed fail at capturing vegephobia and that previous papers might have underestimated the effect sizes of the discrimination for other social identities. The absence of vegephobia is also potentially alleviated by the experimental setting and the plant-based friendliness of the sample, demonstrating that a shift towards more positive attitudes and greater acceptability of plant-based diets are on the way.

³⁴Individuals do not perceive vegetarianism and veganism as threats (see Appendix A.8).

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Appendix A

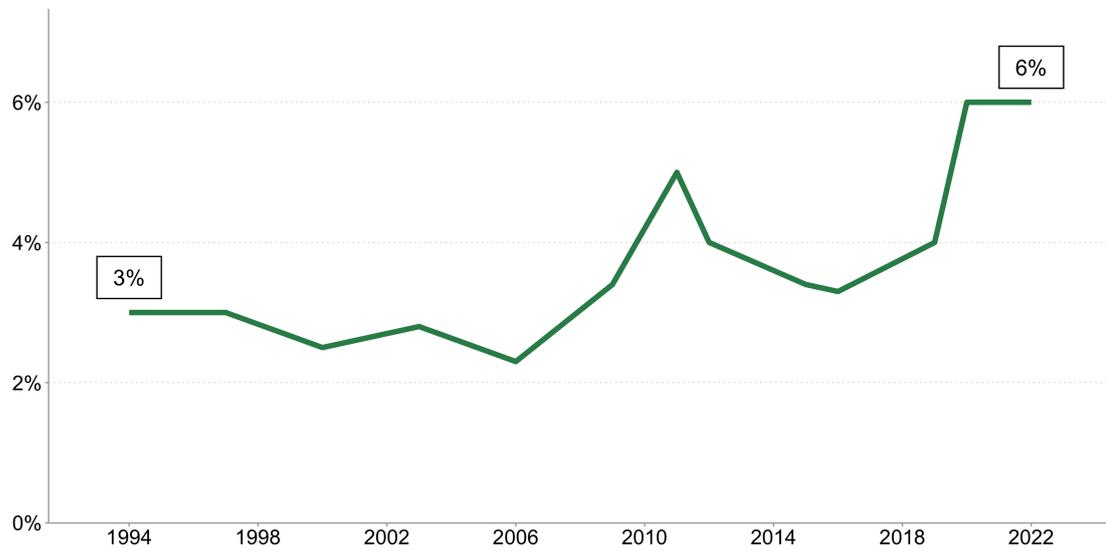
Identifying the barriers to plant-based diets: the case of vegephobia

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A.1 Additional tables and figures

Figure A1: Evolution of the percentage of the United States population who declare following a plant-based diet, from 1994 to 2022.



Note: Data recovered from www.vrg.org. Plant-based diet accounts here for both vegetarians and vegans.

Table A1: All decision matrices for the choice task.

Decision number	Choice	Own payoff	Partner's payoff
1	Top	100	200
1	Bottom	100	100
2	Top	100	200
2	Bottom	100	140
3	Top	120	80
3	Bottom	100	100
4	Top	140	0
4	Bottom	120	140
5	Top	140	20
5	Bottom	120	100
6	Top	140	40
6	Bottom	80	80
7	Top	140	40
7	Bottom	120	120
8	Top	140	60
8	Bottom	120	100
9	Top	140	80
9	Bottom	120	120
10	Top	140	120
10	Bottom	80	80
11	Top	160	0
11	Bottom	100	100
12	Top	160	40
12	Bottom	120	80
13	Top	160	80
13	Bottom	140	160
14	Top	200	0
14	Bottom	100	100
15	Top	200	0
15	Bottom	180	20

Table A2: Vegetarian (resp. Vegan) Threat Scale.

How much do you agree with the following statements?
 Answers range from 1 (*strongly disagree*) to 7 (*strongly agree*).

1. Vegetarianism (resp. Veganism) has a negative influence on the United States economy.
2. The vegetarian (resp. vegan) movement is too involved in local and national politics.
3. Nowadays, when it comes to nutrition and meals, people listen too much to what a minority of vegetarians (resp. vegans) wants.
4. The rise of vegetarianism (resp. veganism) poses a threat to our country's cultural customs.
5. Eating meat is part of our cultural habits and identity, and some people should be more respectful of that.
6. Important culinary traditions which are typical to our country are starting to die out due to the rise of vegetarianism (resp. veganism).
7. Vegetarianism (resp. Veganism) has a negative influence on the United States economy.
8. Important family traditions and celebrations are increasingly being ruined and disappearing because of the presence of vegetarians (resp. vegans) in certain families.

Table A3: Social Dominance Orientation Scale.

How do you feel about the following statements?
Answers range from 1 (*very negative*) to 7 (*very positive*).

1. In getting what you want, it is sometimes necessary to use force against other groups.
2. No one group should dominate in society.
3. We should strive to make incomes as equal as possible.
4. All groups should be given an equal chance in life.
5. If certain groups stayed in their place, we would have fewer problems.
6. We would have fewer problems if we treated people more equally.
7. Some groups of people are simply inferior to other groups.
8. It's OK if some groups have more of a chance in life than others.

Table A4: Everyday Discrimination.

In your day-to-day life, how often do any of the following happen to you due to your dietary choices?
Answers range from *never* to *almost everyday*.

1. Being treated with less courtesy than others.
 2. Being treated with less respect than others.
 3. Receiving poorer service than others in restaurants or stores.
 4. People acting as if you are not smart.
 5. People acting as if they are better than you.
 6. People think you are dishonest.
 7. Being called names or insulted.
 8. Being threatened or harassed.
-

Table A5: Study design table.

Question	Hypothesis	Sampling plan	Analysis Plan	Interpretation
Do omnivore dictators discriminate more vegan recipients than omnivore recipients in an economic environment?	Dictators will display greater aversion to disadvantageous inequality with vegans than with omnivores.	1200 observations recruited via Prolific (400 participants per treatment). Data collection between June and July 2022. Exclusion of participants who: spent less than 4 mins 20 sec on the experiment, did not pass the comprehension and did not pass attention checks.	Mixed logit estimation of individual α inequity aversion parameter, assuming correlation between the random terms, with treatment dependent interaction terms. One-sided testing of H_0^1 : $\alpha_{VGN} \leq \alpha_{OMNI}$ with $\alpha = 0.05$ and Holm-adjusted p-values for two hypotheses.	If H_0^1 is rejected, I will conclude that dictators discriminate more vegans than omnivores, providing evidence in favor of vegophobia in the economic environment. If I fail to reject H_0^1 , I will conclude that dictators do not discriminate more vegans than omnivores, rejecting the presence of vegophobia in the economic environment.
Do omnivore dictators discriminate more vegetarian recipients than omnivore recipients in an economic environment?	Dictators will display greater aversion to disadvantageous inequality with vegetarians than with omnivores.	1200 observations recruited via Prolific (400 participants per treatment). Data collection between June and July 2022. Exclusion of participants who: spent less than 4 mins 20 sec on the experiment, did not pass the comprehension and did not pass attention checks.	Mixed logit estimation of individual α inequity aversion parameter, assuming correlation between the random terms, with treatment dependent interaction terms. One-sided testing of H_0^2 : $\alpha_{VGT} \leq \alpha_{OMNI}$ with $\alpha = 0.05$ and Holm-adjusted p-values for two hypotheses.	If H_0^2 is rejected, I will conclude that dictators discriminate more vegetarians than omnivores, providing evidence in favor of vegophobia in the economic environment. If I fail to reject H_0^2 , I will conclude that dictators do not discriminate more vegetarians than omnivores, rejecting the presence of vegophobia in the economic environment.

Table A6: Pre-registered hypotheses set up.

Hypothesis	Null	Alternative
H1	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$
H2	$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$

A.2 Summary statistics

Table A7: Summary statistics of demographic variables for dictators, by treatment.

	OMNI	VGN	VGT	p-value
Observations	384	387	387	
Age				
Mean	40.09	37.65	38.17	See table A9
Sd	±12.91	±13.62	±13.62	
Sex				
Male	49%	50%	50%	0.998
Female	51%	50%	50%	0.979
Political preferences				
Left-wing	47%	45%	50%	0.462
Right-wing	22%	19%	19%	0.579
Independent	31%	35%	31%	0.587
Ethnicity				
White	80%	72%	78%	0.480
Asian	8%	11%	6%	0.059*
African American	6%	7%	7%	0.507
Hispanic	4%	7%	5%	0.142
Other	2%	2%	3%	0.507
Income in dollars				
Less than \$10,000	9%	13%	10%	0.204
Between \$10,000 and \$49,999	34%	29%	37%	0.175
Between \$50,000 and \$99,999	38%	38%	37%	0.980
More than \$100,000	19%	19%	16%	0.413
Education				
Less than Bachelors	33%	38%	38%	0.384
Bachelors	46%	43%	42%	0.801
Masters	16%	15%	16%	0.899
More than Masters	6%	4%	4%	0.464
Nationality				
United States	96%	95%	96%	0.983
Other	4%	5%	4%	0.551
Religious identification				
Christian	27%	29%	29%	0.815
Agnostic	23%	21%	22%	0.950
Atheist	14%	16%	18%	0.386
Catholic	13%	12%	9%	0.218
Protestant	7%	6%	10%	0.221
Other	16%	15%	13%	0.589
Religious attendance				
Never	58%	56%	57%	0.972
At least once a year	24%	21%	20%	0.502
At least once a month	7%	9%	8%	0.658
At least once a week	10%	13%	15%	0.162

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Proportion tests using chi-square test of independence. H_0 setup as all proportions equal; H_1 as unequal proportions. One participant had "consent revoked" for the sex variable and was deleted for the analysis of the sex proportion.

Table A8: Summary statistics of demographic variables for recipients, by treatment.

	OMNI	VGN	VGT	p-value
Observations	98	81	99	
Age				
Mean	36.46	31.48	35.25	See table A10
Std. dev.	± 13.12	± 11.03	± 11.26	
Sex				
Male	50%	14%	14%	0.000***
Female	50%	86%	86%	0.001***
Political preferences				
Left-wing	43%	64%	67%	0.065*
Right-wing	17%	6%	6%	0.009***
Independent	40%	30%	27%	0.122
Ethnicity				
White	77%	81%	82%	0.463
Asian	9%	2%	8%	0.104
African American	6%	4%	4%	0.584
Hispanic	7%	7%	3%	0.444
Other	1%	5%	3%	0.417
Income in dollars				
Less than \$10,000	17%	17%	3%	0.008***
Between \$10,000 and \$49,999	34%	41%	42%	0.472
Between \$50,000 and \$99,999	36%	33%	40%	0.282
More than \$100,000	13%	9%	14%	0.282
Education				
Less than Bachelors	37%	35%	26%	0.393
Bachelors	36%	49%	48%	0.350
Masters	20%	9%	18%	0.038**
More than Masters	7%	7%	7%	0.951
Nationality				
United States	97%	99%	97%	0.411
Other	3%	1%	3%	0.565
Religious identification				
Christian	32%	20%	12%	0.006***
Agnostic	19%	23%	31%	0.124
Atheist	12%	21%	26%	0.064*
Catholic	8%	9%	5%	0.705
Protestant	8%	4%	2%	0.092*
Other	20%	23%	23%	0.811
Religious attendance				
Never	58%	69%	61%	0.928
At least once a year	22%	17%	28%	0.099*
At least once a month	6%	9%	4%	0.663
At least once a week	13%	5%	7%	0.072*

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Proportion tests using Chi-square test of independence. H_0 setup as all proportions equal; H_1 as unequal proportions. One participant had "consent revoked" for the sex variable and was deleted for the analysis of the sex proportion.

Table A9: Dictators' age differences, by treatment.

	OMNI	VGN	VGT
Age			
Mean	40.09	37.65	38.17
Sd	±12.91	±13.62	±13.62
	OMNI	VGN	VGT
OMNI	-	-	-
VGN	0.011**	-	-
VGT	0.046**	0.588	-

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Pairwise t-test with pooled standard deviation. P-values are unadjusted for multiple hypothesis testing.

Table A10: Recipients' age differences, by treatment.

	OMNI	VGN	VGT
Age			
Mean	36.46	31.48	35.25
Std. dev.	± 13.12	± 11.03	± 11.26
	OMNI	VGN	VGT
OMNI	-	-	-
VGN	0.003***	-	-
VGT	0.409	0.031**	-

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Pairwise t-test with pooled standard deviation. P-values are unadjusted for multiple hypothesis testing.

A.3 Confirmatory analysis additional tables

Table A11: Estimation of inequity aversion parameters with treatment dependent interactions.

Inequity aversion parameters	α_i	β_i	Num. Obs.
Total sample	0.419*** (0.041)	-0.375*** (0.026)	17370
<i>OMNI</i>	0.447*** (0.076)	-0.297*** (0.036)	5760
<i>VGT</i>	0.416*** (0.064)	-0.463*** (0.043)	5805
<i>VGN</i>	0.327*** (0.068)	-0.464*** (0.048)	5805
Treatment differences			
<i>OMNI</i> × <i>VGT</i>	0.033 (0.083)	-0.152*** (0.057)	11565
<i>OMNI</i> × <i>VGN</i>	-0.029 (0.097)	-0.030 (0.079)	11565

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. No control variables are included.

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Table A12: Sensitivity of adjusted and unadjusted p-values of pre-registered hypotheses to statistical models and control variables.

Model	Control variables	Hypotheses	Unadjusted p-values	Holm-Bonferroni adjusted p-values
<i>Mixed Logit with correlation</i>	No	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	0.618	0.689
		$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$		
		$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	0.345	0.689
		$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$		
<i>Mixed Logit with correlation</i>	Yes	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	0.552	0.842
		$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$		
		$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	0.421	0.842
		$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$		
<i>Mixed Logit no correlation</i>	No	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	0.590	0.617
		$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$		
		$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	0.308	0.617
		$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$		
<i>Mixed Logit no correlation</i>	Yes	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	0.353	0.558
		$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$		
		$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	0.279	0.558
		$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$		
<i>Conditional logit</i>	No	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	0.701	0.747
		$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$		
		$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	0.373	0.747
		$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$		
<i>Conditional logit</i>	Yes	$H_0^1: \alpha_{VGN} \leq \alpha_{OMNI}$	0.443	0.667
		$H_1^1: \alpha_{VGN} > \alpha_{OMNI}$		
		$H_0^2: \alpha_{VGT} \leq \alpha_{OMNI}$	0.333	0.667
		$H_1^2: \alpha_{VGT} > \alpha_{OMNI}$		

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Hypothesis setup presented with null hypothesis H_0 and alternative H_1 . Coefficient testing assessed using one-sided tests. 15 decisions per participant. P-values are corrected for two hypotheses. The control variables include a set of dummy variables. The variable Age is equal to 1 if the participant's age is equal or above the sample median age (i.e., 36 for this estimation), 0 if below; Sex equals 1 if the participant is a female, 0 if male; Income equals 1 if the participant reports that her yearly income is above \$50,000, 0 if below \$50,000; Ethnicity equals 1 if the participant is not white, 0 if any other ethnicity; Education equals 1 if the participant has a bachelor degree or higher, 0 if less than a bachelor degree; Political preferences equals 1 if the participants feels closer to the left-wing or independent, 0 if closer to the right-wing. For regressions with *VGN*, one observation was dropped when including the controls as the sex variable had "consent revoked".

A.4 Heterogeneity analysis

A.4.1 Heterogeneity analysis with all dictators' characteristics

Table A13: Dictators' structural estimation of inequity aversion parameters with treatment dependent heterogeneous effect, *OMNI*.

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i								
Total sample	0.447*** (0.076)	-0.297*** (0.036)	0.332*** (0.098)	-0.358*** (0.054)	0.548*** (0.107)	-0.225*** (0.049)	0.404*** (0.108)	-0.331*** (0.062)	0.486*** (0.085)	-0.336*** (0.046)	0.289** (0.112)	0.354*** (0.076)	0.451*** (0.145)	-0.240*** (0.075)	0.496** (0.230)	-0.171 (0.115)
Age			0.214 (0.135)	0.113 (0.086)											0.110 (0.144)	0.044 (0.063)
Sex					-0.216 (0.141)	-0.192** (0.081)									-0.271 (0.148)	-0.258*** (0.078)
Income							0.074 (0.135)	0.055 (0.078)							-0.062 (0.160)	0.028 (0.086)
Ethnicity									-0.171 (0.171)	0.219** (0.092)					-0.165 (0.172)	0.225*** (0.079)
Education											0.257* (0.140)	0.084 (0.090)			0.172 (0.161)	-0.079 (0.082)
Political preferences													-0.006 (0.161)	-0.084 (0.086)	-0.044 (0.171)	-0.091 (0.082)
Num. obs.	5760		5760		5760		5760		5760		5760		5760		5760	

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. The controls are dummy variables defined as so: Age equals 1 if the participant's age is equal to or above the sample median age (i.e., 38 for this estimation), 0 if below; Sex equals 1 if the participant is a female, 0 if male; Income equals 1 if the participant reports that her yearly income is above \$50,000, 0 if below \$50,000; Ethnicity equals 1 if the participant is non-white, 0 if white; Education equals 1 if the participant has a bachelor degree or higher, 0 if less than a bachelor degree; Political preferences equals 1 if the participant feels closer to the left-wing or independent, 0 if closer to the right-wing.

Table A14: Dictators' structural estimation of inequity aversion parameters with treatment dependent heterogeneous effect, *VGT*.

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i											
Total sample	0.416*** (0.064)	-0.463*** (0.043)	0.441*** (0.087)	-0.505*** (0.071)	0.442*** (0.085)	-0.398*** (0.052)	0.353*** (0.082)	-0.561*** (0.064)	0.470*** (0.072)	-0.467*** (0.052)	-0.234** (0.084)	-0.597*** (0.066)	0.305** (0.121)	0.033 (0.103)	0.237 (0.169)	-0.258*** (0.098)	
Age			-0.069 (0.106)	0.076 (0.086)												-0.104 (0.108)	-0.003 (0.072)
Sex					-0.079 (0.105)	-0.120 (0.076)										-0.101 (0.103)	-0.088 (0.069)
Income							0.091 (0.105)	0.185** (0.082)								-0.006 (0.112)	0.119 (0.080)
Ethnicity									-0.304** (0.122)	0.040 (0.087)						-0.317** (0.125)	0.142* (0.083)
Education											0.261** (0.108)	0.180** (0.086)				0.280** (0.114)	0.161** (0.072)
Political preferences													0.133 (0.131)	-0.621*** (0.107)	0.208 (0.133)	-0.461*** (0.091)	
Num. obs.	5805		5805		5805		5805		5805		5805		5805		5805		

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. The controls are dummy variables defined as so: Age equals 1 if the participant's age is equal to or above the sample median age (i.e., 35 for this estimation), 0 if below; Sex equals 1 if the participant is a female, 0 if male; Income equals 1 if the participant reports that her yearly income is above \$50,000, 0 if below \$50,000; Ethnicity equals 1 if the participant is non-white, 0 if white; Education equals 1 if the participant has a bachelor degree or higher, 0 if less than a bachelor degree; Political preferences equals 1 if the participants feel closer to the left-wing or independent, 0 if closer to the right-wing.

Table A15: Dictators' structural estimation of inequity aversion parameters with treatment dependent heterogeneous effect, *VGN*.

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i
Total sample	0.327*** (0.068)	-0.464*** (0.048)	0.324*** (0.096)	-0.293*** (0.058)	0.545*** (0.101)	-0.356*** (0.073)	0.286*** (0.100)	-0.352*** (0.065)	0.562*** (0.090)	-0.288*** (0.043)	0.097 (0.100)	-0.348*** (0.056)	0.405*** (0.146)	-0.203*** (0.068)	0.268 (0.204)	-0.260** (0.130)
Age			0.188 (0.128)	-0.036 (0.074)											0.120 (0.126)	0.047 (0.078)
Sex					-0.268* (0.127)	-0.018 (0.087)									-0.276** (0.125)	-0.018 (0.072)
Income							0.242* (0.130)	0.078 (0.077)							-0.091 (0.138)	-0.022 (0.090)
Ethnicity									-0.549*** (0.149)	-0.231** (0.231)					-0.492*** (0.147)	-0.131 (0.095)
Education											0.528*** (0.138)	0.088 (0.073)			0.534*** (0.139)	0.078 (0.082)
Political preferences													0.018 (0.159)	-0.145* (0.080)	0.109 (0.154)	-0.163 (0.116)
Num. obs.	5805		5805		5790		5805		5805		5805		5805		5790	

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. The controls are dummy variables defined as so: Age equals 1 if the participant's age is equal to or above the sample median age (i.e., 35 for this estimation), 0 if below; Sex equals 1 if the participant is a female, 0 if male; Income equals 1 if the participant reports that her yearly income is above \$50,000, 0 if below \$50,000; Ethnicity equals 1 if the participant is non-white, 0 if white; Education equals 1 if the participant has a bachelor degree or higher, 0 if less than a bachelor degree; Political preferences equals 1 if the participants feel closer to the left-wing or independent, 0 if closer to the right-wing.

A.4.2 Ethnicity heterogeneity analysis

In this section, I deepen the results on ethnicity unveiled in Appendix A.4.1 and explore whether the discrimination effect is entirely driven by white dictators, as well as explore the attitudes of each ethnicity towards vegs. I estimate the inequity aversion parameters within each treatment and compare the parameters from each ethnicity to the rest.¹ The results are reported in Tables A16, A17 and A18. Asian individuals display more guilt with omnivore recipients (two-sided β_i coefficient testing, p-value < 0.05), and less envy but paradoxically also less guilt with vegan recipients (two-sided α_i and β_i coefficient testing, p-value < 0.01 and < 0.05, respectively). In comparison with the other ethnic groups, African Americans display less envy with vegetarians (two-sided α_i coefficient testing, p-value < 0.1) and vegans (two-sided α_i coefficient testing, p-value < 0.05). Hispanic individuals have more positive attitudes toward vegetarians and display less envy (two-sided α_i coefficient testing, p-value < 0.01). Overall, the results confirm on White individuals and reveal that the different ethnic groups have different preferences. African American individuals have more positive attitudes toward vegetarians and vegans, while Hispanic individuals have more positive attitudes toward vegetarians but not vegans. Asian individuals do not display clear behavioral patterns.

¹I do not comment on the inequity aversion parameters for Whites vs rest as they are identical to the results already commented in Section 5.5.

Table A16: Dictators' structural estimation of inequity aversion parameters with ethnicity dependent heterogeneous effect, *OMNI*.

	(1)		(2)		(3)		(4)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i
Total sample	0.315** (0.159)	-0.116 (0.087)	0.437*** (0.079)	-0.323*** (0.039)	0.475*** (0.080)	-0.295*** (0.037)	0.459*** (0.078)	-0.297*** (0.037)
White vs rest	0.171 (0.171)	-0.219** (0.092)						
Asian vs rest			0.043 (0.222)	0.251** (0.120)				
African American vs rest					-0.383 (0.262)	-0.041 (0.136)		
Hispanic vs rest							-0.288 (0.327)	0.011 (0.122)
Num. obs.	5760		5760		5760		5760	

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. Ethnicity differences are assessed using dummy variables. For White vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is White, and 0 if not. For Asian vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is Asian, and 0 if not. For African American vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is African American, and 0 if not. For Hispanic vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is Hispanic, and 0 if not. Other ethnic groups are not explored as they represent less than 2% of the total sample.

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Table A17: Dictators' structural estimation of inequity aversion parameters with ethnicity dependent heterogeneous effect, *VGT*.

	(1)		(2)		(3)		(4)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i
Total sample	0.166 (0.105)	-0.427*** (0.071)	0.387*** (0.062)	-0.504*** (0.040)	0.427*** (0.065)	-0.459*** (0.045)	0.441*** (0.066)	-0.460*** (0.045)
White vs rest	0.304** (0.122)	-0.040 (0.087)						
Asian vs rest			0.095 (0.208)	-0.076 (0.126)				
African American vs rest					-0.370* (0.198)	0.043 (0.140)		
Hispanic vs rest							-0.639*** (0.213)	0.002 (0.155)
Num. obs.	5805		5805		5805		5805	

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. For White vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is White, and 0 if not. For Asian vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is Asian, and 0 if not. For African American vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is African American, and 0 if not. For Hispanic vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is Hispanic, and 0 if not. Other ethnic groups are not explored as they represent less than 2% of the total sample.

Table A18: Dictators' structural estimation of inequity aversion parameters with ethnicity dependent heterogeneous effect, *VGN*.

	(1)		(2)		(3)		(4)	
Inequity aversion parameters	α_i	β_i	α_i	β_i	α_i	β_i	α_i	β_i
Total sample	0.012 (0.115)	-0.518*** (0.077)	0.476*** (0.080)	-0.305*** (0.041)	0.447*** (0.076)	-0.312*** (0.041)	0.449*** (0.078)	-0.299*** (0.043)
White vs rest	0.549*** (0.149)	0.231** (0.091)						
Asian vs rest			-0.512*** (0.193)	-0.249** (0.112)				
African American vs rest					-0.572** (0.251)	-0.217 (0.133)		
Hispanic vs rest							-0.386 (0.257)	0.200 (0.168)
Num. obs.	5805		5805		5805		5805	

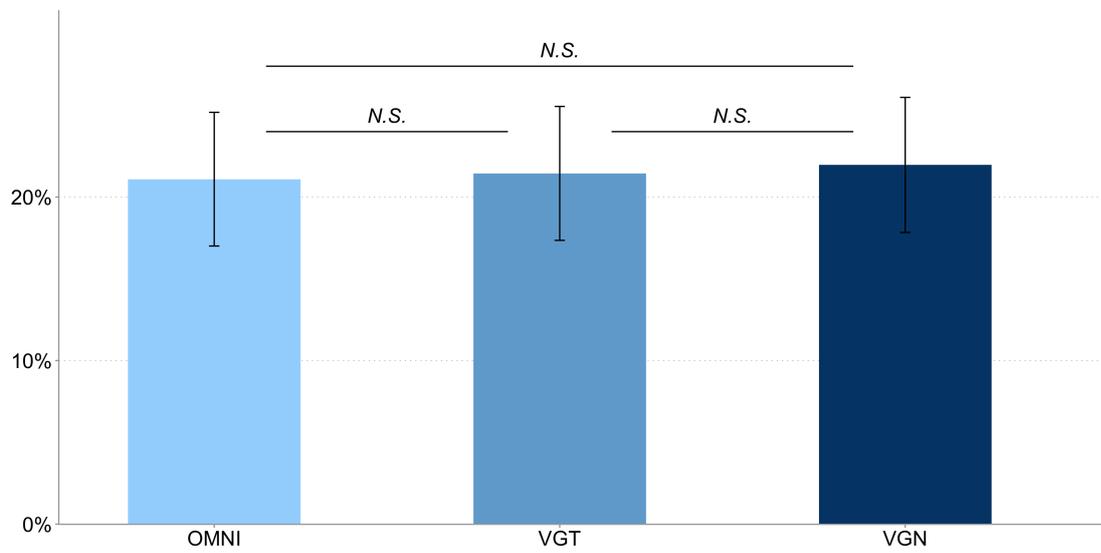
Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. For White vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is White, and 0 if not. For Asian vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is Asian, and 0 if not. For African American vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is African American, and 0 if not. For Hispanic vs. rest, the dummy variable equals 1 if the dictator's declared ethnicity is Hispanic, and 0 if not. Other ethnic groups are not explored as they represent less than 2% of the total sample.

A.5 Social environment

Table A19: Proportion of dictators with vegetarians or vegans in their social environment.

	OMNI	VGN	VGT
Vegetarian or vegan family			
Yes	21%	22%	21%
No	79%	78%	79%
Vegetarian or vegan friends			
Yes	51%	55%	58%
No	49%	45%	42%
Vegetarian or vegan family and friends			
Yes	14%	18%	16%

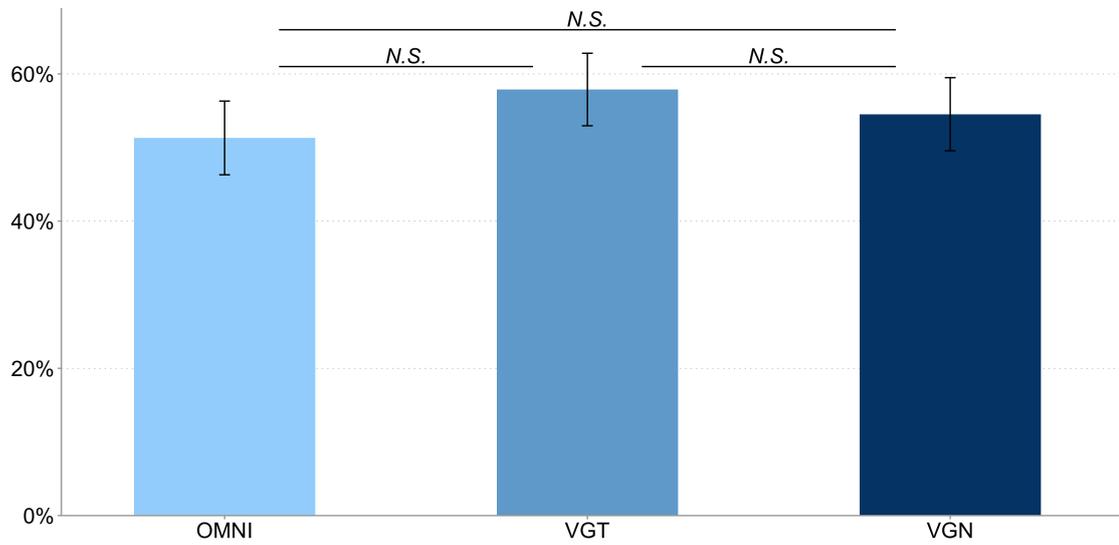
Figure A2: Proportion of dictators who report having a vegetarian or vegan family member, by treatment.



Note: Comparisons using two-sided Wilcoxon rank-sum test. Errors bars represent 95% confidence intervals. Scores normalized between 0 and 1. N = 1158 (OMNI = 384, VGN = 387 and VGT = 387).

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Figure A3: Proportion of dictators who report having a vegetarian or vegan friend, by treatment.



Note: Comparisons using two-sided Wilcoxon rank-sum test. Errors bars represent 95% confidence intervals. Scores normalized between 0 and 1. N = 1158 (OMNI = 384, VGN = 387 and VGT = 387).

A.6 Perceived inequity aversion parameters

Table A20: Recipients' perceived inequity aversion parameters and treatment differences.

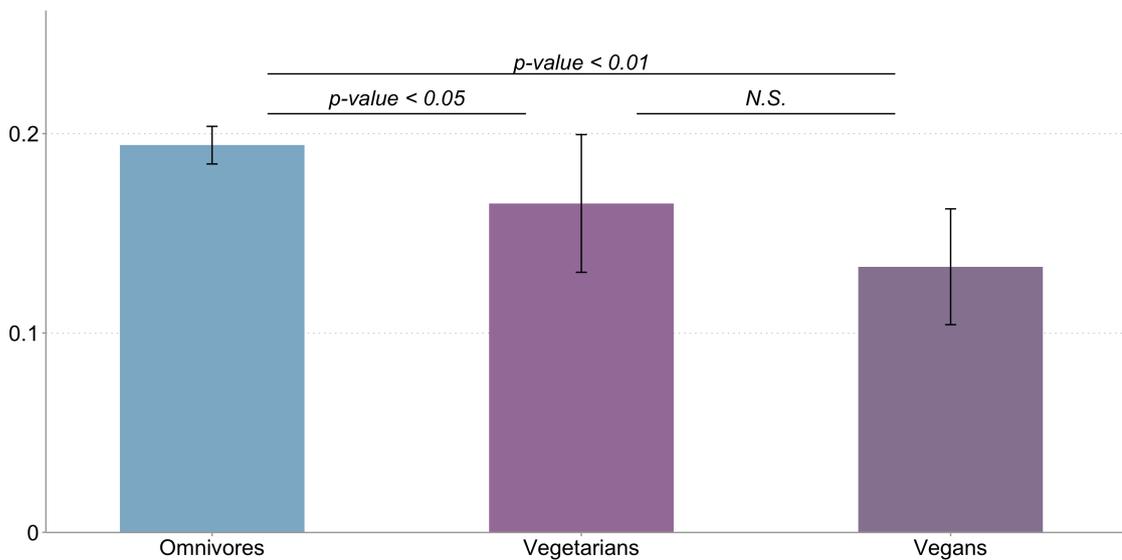
Perceived inequity aversion parameters	α_i	β_i	Controls	Num. Obs.
Total sample	0.338*** (0.084)	-0.367*** (0.048)	No	4170
	0.148 (0.309)	-0.451*** (0.170)	Yes	4170
OMNI	0.323** (0.150)	-0.488*** (0.092)	No	1470
	-0.168 (0.403)	-0.451** (0.222)	Yes	1470
VGT	0.447*** (0.145)	-0.368*** (0.052)	No	1485
	0.424 (0.739)	-0.402 (0.356)	Yes	1485
VGN	0.278* (0.151)	-0.289*** (0.089)	No	1215
	1.131 (0.737)	0.152 (0.290)	Yes	1215

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Estimations using a mixed logit model assuming a correlation between the α and β parameters. 15 decisions per participant. Standard errors are shown in parentheses. Control variables include age, sex, annual income, ethnicity, education, and political preferences.

A.7 Pro-social behaviors

In the following section, I compare the participants' answers to the Social Dominance Orientation scale (SDO) (see Section 3.4 for more details) to assess the degree of pro-social behaviors according to participants' dietary preferences. I pool together all the omnivores' scores, regardless of the treatment (i.e., *OMNI*, *VGT* or *VGN*) and branch (i.e., dictators and recipients), and compare them to the SDO scores of vegetarians and vegans. The scores are reported in figure A4. Omnivores' scores are significantly higher than those of vegetarians (two-sided Wilcoxon rank-sum test, $p\text{-value} < 0.05$) and vegans (two-sided Wilcoxon rank-sum test, $p\text{-value} < 0.01$). This result holds when limiting the analysis to recipients only (i.e., omnivores and plant-based diet recipients).

Figure A4: Social Dominance Orientation scores of omnivores *vs* vegetarians and vegans.



Note: Comparisons using two-sided Wilcoxon rank-sum test. Errors bars represent 95% confidence intervals. Scores normalized between 0 and 1. $N = 1436$ (omnivores = 1256, vegetarians = 99, vegans = 81).

A.8 Perceptions of vegetarianism and veganism

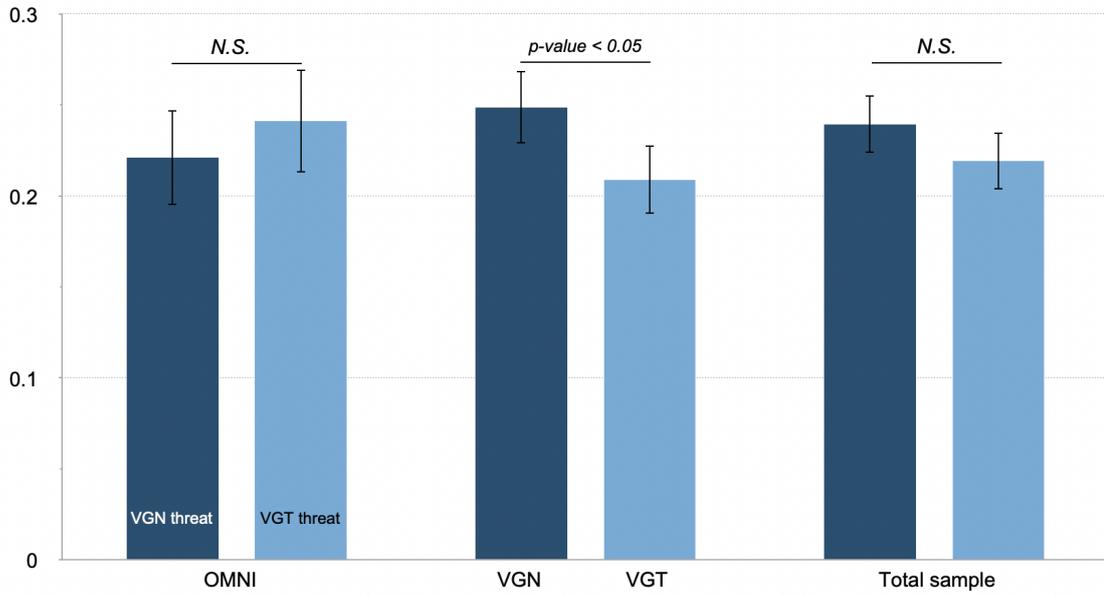
In this section, I explore the results from the Vegetarian (VGT) and Vegan (VGN) threat scales. Table A21 reports the scales' summary statistics. The scales yield satisfactory internal validity (Cronbach's alpha = 0.91). Dictators do not perceive vegetarianism and veganism as threats with means low mean scores (VGT threat mean = 0.22 and sd = 0.19; VGN threat mean = 0.24 and sd = 0.20). Figure A5 reports the differences between the VGT and VGN threat scales. In *OMNI*, half the dictators answered the VGT threat and the other half the VGN threat. There are no differences between the VGN and VGT threat (two-sided Wilcoxon rank-sum test, p-value > 0.05). Pooling scores from VGN threat and VGT threat, regardless of the treatment, yields no significant differences (two-sided Wilcoxon rank-sum test, p-value > 0.05). Exploring treatment differences, VGN threat scores in *VGN* are higher than VGT threat scores in *VGT* (two-sided Wilcoxon rank-sum test, p-value < 0.05). Dictators perceive veganism as a higher threat after an exposure to a vegan than dictators perceive vegetarianism after exposure to a vegetarian.

Table A21: Vegetarian and Vegan threat scales summary statistics.

	N	Mean	Median	Sd	Min.	Max.	Cronbach's α
VGT threat	572	0.22	0.19	0.19	0	1	0.91
VGN threat	586	0.24	0.2	0.2	0	1	0.91

Scores are normalized between 0 and 1.

Figure A5: Dictators comparisons of Vegetarian *vs* Vegan threat scale scores.



Note: Comparisons using two-sided Wilcoxon rank-sum test. Errors bars represent 95% confidence intervals. $N = 1158$. Scores normalized between 0 and 1. Observations by comparison: OMNI VGN threat = 199 and OMNI VGT threat = 185, VGN VGN threat = 387 and VGT VGT threat = 387, Total sample VGN Threat = 586 and Total sample VGT Threat = 572.

A.9 Supplementary Materials

A.9.1 Power analysis

A.9.1.1 Power analysis for sample size

The sample size was determined using a power analysis calibrated on estimates and effect sizes from pilot data (see Table A23). The pilot data replicated the final experiment with the omnivore and vegan condition for dictators and recipients, and a small sample size (i.e., 100 participants for the dictator branch and 20 participants for the recipient branch). The power analysis simulated samples of 200, 400, 600 and 800 participants for the treatments (i.e., 400 in *VGN* and 400 in *VGT* for the 800 participants simulated sample), 1000 times, and estimated the probability to detect a statistically significant effect. The calculations simply ran for dictators (i.e., dictator branch, treatments *VGT* and *VGN*) as the sample size from the pilot data for recipients was too small to run a precise analysis. The chosen statistical threshold alpha was 5%. I used one-sided testing and applied a Bonferroni correction for multiple hypothesis testing as I will be testing two pre-registered hypotheses. In order to be conservative, I used a Bonferroni correction in the power analysis and use a Holm-Bonferroni correction in the final analysis. The power analysis estimated the treatment effect size for the aversion to advantageous inequality and disadvantageous inequality. Since no pilot data was gathered on the vegetarian condition, I divided the effect size by 2 as I expected less discrimination towards vegetarians than towards vegans.

The results show that with a sample size of 400 participants per treatment, the probability to detect an effect in the *VGN* treatment on disadvantageous inequality (i.e., α_i) is 100%, and 10.7% on advantageous inequality (i.e., β_i). For the *VGT* treatment, the probability to detect an effect on advantageous inequality is 82.3% and 6.5% on disadvantageous inequality.

The results of the power analysis are summarized in Table A22 and the code is available in Listing A.1.

Table A22: Power analysis results for the dictator branch, VGN and VGT treatments.

Sample size	Probability to detect			
	α_{VGN}	β_{VGN}	α_{VGT}	β_{VGT}
200	79.5%	6.1%	27.8%	4.3%
400	98.1%	9.2%	49.9%	4.9%
600	99.8%	8.7%	66.9%	4.7%
800	100%	10.7%	82.3%	6.5%

A.9.1.2 Power analysis code

```
1 /* Power analysis */
2 clear all
3 set more off
4
5 // Packages to install
6 //net install parallel, from(https://raw.githubusercontent.com/gvegayon/
7 //ssc install mat2txt
8
9 /* -----
10 */
11 /* Power analysis for dictators branch, VGN treatment
12 */
13 /* -----
14 */
15 parallel setclusters 8 // Set number of cores on computer
16
17 set seed 1579435 // Set seed for reproduction
18 local j=1
19 local S=1000 // Number of simulations
20 local alpha=0.05/2 // Significance level 5% with Bonferroni
21 // correction for 2 hypotheses
22 mat statPower=J(4,2,.) //Vector to store the results
23
24 capture program drop my_sim
25 program my_sim, rclass
26 version 14.0
27     args N_sim alpha_sim // Setting arguments
28     // Setting temporary variables names
29     tempname b_sim V_sim zscore_sim_alpha pvalue_sim_alpha
30     rejection_sim_alpha zscore_sim_beta pvalue_sim_beta
31     rejection_sim_beta
32     tempname id treat lambda_base alpha_base beta_base decision
33     decision_number alternative
34     tempname own_payoff other_payoff more less lessneg moreneg
35     interaction_lessneg interaction_moreneg
36     tempname utility_option utility_second utility_second_group
37     proba_first proba_first_group proba_second proba y t
38     tempname interaction_alpha interaction_beta
39     drop _all
```

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```

31
32 set obs `N_sim' // Set number observations
33 gen `id'=_n // Generate id
34 gen `treat' = cond(`id'-1<`N_sim'/2,0,1) // Treatment assignment
35
36 // Generate structural estimates based on pilot data estimations
37 gen `lambda_base' = 25.204029
38 gen `alpha_base' = rnormal(.02119151, 1.3344682) - .66096018 * `
    treat' // Mean, standard deviation and treatment effect from
    pilot
39 gen `beta_base' = rnormal(.5407629, .70792477) - .0351949 * `treat'
    // Mean, standard deviation and treatment effect from pilot
40
41 expand 15 // Expand 15 decisions by individual
42 sort `id' // Sorting by id
43 bys `id': gen `decision'=_n // Generate decision number
44 expand 2 // 2 alternatives per decision
45 sort `id' `decision' // Sorting by id and decision
46 bys `id' `decision': gen `alternative'=_n // Generate alternative
    number
47 egen `decision_number' = fill(1 1 2 2) // Decision number
48
49 // Adding structure of the payoffs for each decision
50 egen `own_payoff' = fill(100 100 100 100 120 100 140 120 140 120
    140 80 140 120 140 120 140 120 140 80 160 100 160 120 160 140
    200 100 200 180/*
51 */
    100 100 100 100 120 100 140 120 140 120
    140 80 140 120 140 120 140 120 140 80 160 100 160 120 160 140
    200 100 200 180)
52 egen `other_payoff' = fill(200 100 200 140 80 100 0 140 20 100 40
    80 40 120 60 100 80 120 120 80 0 100 40 80 80 160 0 100 0 20/*
53 */
    200 100 200 140 80 100 0 140 20 100 40
    80 40 120 60 100 80 120 120 80 0 100 40 80 80 160 0 100 0 20)
54
55 gen `more' = max(0, `own_payoff' - `other_payoff') // Generate
    advantageous inequality or beta
56 gen `less' = max(0, `other_payoff' - `own_payoff') // Generate
    disadvantageous inequality or alpha
57 gen `lessneg' = -`less' // Set less as negative value, easier for
    interpretation in utility function
58 gen `moreneg' = -`more' // Set more as negative value, easier for
    interpretation in utility function

```

```
59 gen `interaction_lessneg' = `treat'*`lessneg' // Generate
    treatment interaction term
60 gen `interaction_moreneg' = `treat'*`moreneg' // Generate
    treatment interaction term
61
62 gen `utility_option'=(`own_payoff' - `alpha_base'*`less' - `beta_base
    '*`more')/`lambda_base' // Generate latent utility based on
    baseline parameters
63 gen `utility_second' = cond(`alternative'==2, `utility_option',.) //
    Generate utility of choosing thensecond option
64 egen `utility_second_group' = max(`utility_second'), by(`id' `
    decision') // Generate max utility
65
66 gen `proba_first' = exp(`utility_option')/(exp(`utility_option')+
    exp(`utility_second_group')) if `alternative'==1 // Generate
    probability of choosing first option
67 egen `proba_first_group' = max(`proba_first'), by(`id' `decision')
    // Generate max utility
68
69 gen `proba_second' = 1 - `proba_first_group' if `alternative'==2 //
    // Generate max utility probability of choosing second option
70 gen `proba' = cond(`alternative'==1, `proba_first', `proba_second')
    // Generate max utility
71
72 gen `y' = rbinomial(1, `proba') if `alternative'==1 // Generate
    choice between alternatives
73 egen `t' = max(`y'), by(`id' `decision') // Generate temp variable
74 replace `y' = 1 - `t' if `alternative'==2 // Replace choice for
    alternative
75
76 /* When the values of probabilities are very small or very large
    (i.e., 0 or 1) function does not attribute value */
77 replace `y' = 1 if `proba' == 1
78 replace `y' = 0 if `proba' == 0
79 replace `y' = 0 if missing(`y') // Additional line to deal with
    values very close to 0
80
81 /*-----*/
82 // Estimation using mixed logit assuming correlation between the
    random terms
83 mixlogit `y' `own_payoff' `interaction_lessneg' `
```

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```

interaction_moreneg', group(`decision_number') id(`id') rand(`
lessneg' `moreneg') corr nrep(100) burn(50)
84
85 // Storing interaction terms (i.e., treatment effect)
86 nlcom (`interaction_alpha' : _b[`interaction_lessneg']/_b[`
own_payoff']) (`interaction_beta' : _b[`interaction_moreneg']/_b
[`own_payoff']), post
87
88 // Storing estimation elements
89 mat `b_sim'=e(b) // Vector of coefficients
90 mat `V_sim'=e(V) // Var-Covar matrix
91
92 // Rejection for alpha
93 scalar `zscore_sim_alpha'=`b_sim'[1,1]/sqrt(`V_sim'[1,1])
94 scalar `pvalue_sim_alpha'=normprob(`zscore_sim_alpha') // one-
sided test, H0: coef is greater than or equal to 0
95 scalar `rejection_sim_alpha'=cond(`pvalue_sim_alpha'<`alpha_sim
',1,0) // H0 is rejected if z-score is negative and p-value is
lower than alpha threshold
96
97 // Rejection for beta
98 scalar `zscore_sim_beta'=`b_sim'[1,2]/sqrt(`V_sim'[2,2])
99 scalar `pvalue_sim_beta'=normprob(`zscore_sim_beta') // one-sided
test, H0: coef is greater than or equal to 0
100 scalar `rejection_sim_beta'=cond(`pvalue_sim_beta'<`alpha_sim
',1,0) // H0 is rejected if z-score is negative and p-value is
lower than alpha threshold
101
102 return scalar reject_alpha=`rejection_sim_alpha' //Return
rejection decision for alpha
103 return scalar reject_beta=`rejection_sim_beta' //Return rejection
decision for beta
104 end
105
106 forvalues N=200(200)800{ //Simulating for sample size of 200, 400,
600, 800
107 //simulate rejectResultsAlpha=r(reject_alpha) rejectResultsBeta=r(
reject_beta), reps(`S') nodots: my_sim `N' `alpha'
108 parallel sim, reps(`S') expr(rejectResultsAlpha=r(reject_alpha)
rejectResultsBeta=r(reject_beta)) nodots: my_sim `N' `alpha'
109 qui su rejectResultsAlpha
110 mat statPower[`j',1]=round(`r(mean)',0.001)

```

```
111 qui su rejectResultsBeta
112 mat statPower[`j',2]=round(`r(mean)',0.001)
113 local j=`j'+1
114 }
115
116 mat list statPower // Display results of the power analysis
117
118 // Export table
119 mat2txt, matrix(statPower) saving(/Users/ThibautArpinon_1/Desktop/
    Power_analysis/Power_Analysis_Dict_VGN) title(Table 1. Power
    analysis Dictators in Vegan condition)
120
121
122 /* -----
    */
123 /* Power analysis for dictators branch, VGT treatment
    */
124 /* -----
    */
125 parallel setclusters 8 // Set number of cores on computer
126
127 set seed 1579435 // Set seed for reproduction
128 local j=1
129 local S=1000 // Number of simulations
130 local alpha=0.05/2 // Significance level 5% with Bonferroni
    correction for 2 hypotheses
131 mat statPower=J(4,2,.) //Vector to store the results
132
133 capture program drop my_sim
134 program my_sim, rclass
135 version 14.0
136     args N_sim alpha_sim // Setting arguments
137     // Setting temporary variables names
138     tempname b_sim V_sim zscore_sim_alpha pvalue_sim_alpha
        rejection_sim_alpha zscore_sim_beta pvalue_sim_beta
        rejection_sim_beta
139     tempname id treat lambda_base alpha_base beta_base decision
        decision_number alternative
140     tempname own_payoff other_payoff more less lessneg moreneg
        interaction_lessneg interaction_moreneg
141     tempname utility_option utility_second utility_second_group
        proba_first proba_first_group proba_second proba y t
```

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```

142 tempname interaction_alpha interaction_beta
143 drop _all
144
145 set obs `N_sim' // Set number observations
146 gen `id'=_n // Generate id
147 gen `treat' = cond(`id'-1<`N_sim'/2,0,1) // Treatment assignment
148
149 // Gen structural estimates based on pilot data estimations
150 gen `lambda_base' = 25.204029
151 gen `alpha_base' = rnormal(.02119151, 1.3344682) - (.66096018/2) *
    `treat' // Mean, standard deviation and treatment effect from
    pilot
152 gen `beta_base' = rnormal(.5407629, .70792477) - (.0351949/2) * `
    treat' // Mean, standard deviation and treatment effect from
    pilot
153
154 expand 15 // Expand 15 decisions by individual
155 sort `id' // Sorting by id
156 bys `id': gen `decision'=_n // Generate decision number
157 expand 2 // 2 alternatives per decision
158 sort `id' `decision' // Sorting by id and decision
159 bys `id' `decision': gen `alternative'=_n // Generate alternative
    number
160 egen `decision_number' = fill(1 1 2 2) // Decision number
161
162 // Adding structure of the payoffs for each decision
163 egen `own_payoff' = fill(100 100 100 100 120 100 140 120 140 120
    140 80 140 120 140 120 140 120 140 80 160 100 160 120 160 140
    200 100 200 180/*
164 */
    100 100 100 100 120 100 140 120 140 120
    140 80 140 120 140 120 140 120 140 80 160 100 160 120 160 140
    200 100 200 180)
165 egen `other_payoff' = fill(200 100 200 140 80 100 0 140 20 100 40
    80 40 120 60 100 80 120 120 80 0 100 40 80 80 160 0 100 0 20/*
166 */
    200 100 200 140 80 100 0 140 20 100 40
    80 40 120 60 100 80 120 120 80 0 100 40 80 80 160 0 100 0 20)
167
168 gen `more' = max(0, `own_payoff' - `other_payoff') // Generate
    advantageous inequality or beta
169 gen `less' = max(0, `other_payoff' - `own_payoff') // Generate
    disadvantageous inequality or alpha

```

```
170 gen `lessneg' = -`less' // Set less as negative value, easier for
    interpretation in utility function
171 gen `moreneg' = -`more' // Set more as negative value, easier for
    interpretation in utility function
172 gen `interaction_lessneg' = `treat'*`lessneg' // Generate
    treatment interaction term
173 gen `interaction_moreneg' = `treat'*`moreneg' // Generate
    treatment interaction term
174
175 gen `utility_option'=(`own_payoff'-`alpha_base'*`less'-`beta_base
    '*`more')/`lambda_base' // Generate latent utility based on
    baseline parameters
176 gen `utility_second'=`cond(`alternative'==2,`utility_option',.) //
    Generate utility of choosing thensecond option
177 egen `utility_second_group'=`max(`utility_second'), by(`id' `
    decision') // Generate max utility
178
179 gen `proba_first'=`exp(`utility_option')/(exp(`utility_option')+
    exp(`utility_second_group')) if `alternative'==1 // Generate
    probability of choosing first option
180 egen `proba_first_group'=`max(`proba_first'), by(`id' `decision')
    // Generate max utility
181
182 gen `proba_second'=`1-`proba_first_group' if `alternative'==2 //
    // Generate max utility probability of choosing second option
183 gen `proba'=`cond(`alternative'==1, `proba_first', `proba_second')
    // Generate max utility
184
185 gen `y' = rbinomial(1,`proba') if `alternative'==1 // Generate
    choice between alternatives
186 egen `t'=`max(`y'), by(`id' `decision') // Generate temp variable
187 replace `y'=`1-`t' if `alternative'==2 // Replace choice for
    alternative
188
189 /* When the values of probabilities are very small or very large
    (i.e., 0 or 1) function does not attribute value */
190 replace `y' = 1 if `proba' == 1
191 replace `y' = 0 if `proba' == 0
192 replace `y' = 0 if missing(`y') // Additional line to deal with
    values very close to 0
193
194 /*-----*/
```

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```

195 // Estimation using mixed logit assuming correlation between the
      random terms
196 mixlogit `y' `own_payoff' `interaction_lessneg' `
      interaction_moreneg', group(`decision_number') id(`id') rand(`
      lessneg' `moreneg') corr nrep(100) burn(50)
197
198 // Storing interaction terms (i.e., treatment effect)
199 nlcom (`interaction_alpha' : _b[`interaction_lessneg']/_b[`
      own_payoff']) (`interaction_beta' : _b[`interaction_moreneg']/_b
      [`own_payoff']), post
200
201 // Storing estimation elements
202 mat `b_sim'=e(b) // Vector of coefficients
203 mat `V_sim'=e(V) // Var-Covar matrix
204
205 // Rejection for alpha
206 scalar `zscore_sim_alpha'=`b_sim'[1,1]/sqrt(`V_sim'[1,1])
207 scalar `pvalue_sim_alpha' =normprob(`zscore_sim_alpha') // one-
      sided test, H0: coef is greater than or equal to 0
208 scalar `rejection_sim_alpha' =cond(`pvalue_sim_alpha' < `alpha_sim
      ',1,0) // H0 is rejected if z-score is negative and p-value is
      lower than alpha threshold
209
210 // Rejection for beta
211 scalar `zscore_sim_beta'=`b_sim'[1,2]/sqrt(`V_sim'[2,2])
212 scalar `pvalue_sim_beta' =normprob(`zscore_sim_beta') // one-sided
      test, H0: coef is greater than or equal to 0
213 scalar `rejection_sim_beta' =cond(`pvalue_sim_beta' < `alpha_sim
      ',1,0) // H0 is rejected if z-score is negative and p-value is
      lower than alpha threshold
214
215 return scalar reject_alpha=`rejection_sim_alpha' //Return
      rejection decision for alpha
216 return scalar reject_beta=`rejection_sim_beta' //Return rejection
      decision for beta
217 end
218
219 forvalues N=200(200)800{ //Simulating for sample size of 200, 400,
      600, 800
220 //simulate rejectResultsAlpha=r(reject_alpha) rejectResultsBeta=r(
      reject_beta), reps(`S') nodots: my_sim `N' `alpha'

```

```
221 parallel sim, reps(`S') expr(rejectResultsAlpha=r(reject_alpha)
    rejectResultsBeta=r(reject_beta)) nodots: my_sim `N' `alpha'
222 qui su rejectResultsAlpha
223 mat statPower[`j',1]=round(`r(mean)',0.001)
224 qui su rejectResultsBeta
225 mat statPower[`j',2]=round(`r(mean)',0.001)
226 local j=`j'+1
227 }
228
229 mat list statPower // Display results of the power analysis
230
231 // Export table
232 mat2txt, matrix(statPower) saving(/Users/ThibautArpinon_1/Desktop/
    Power_analysis/Power_Analysis_Dict_VGT) title(Table 1. Power
    analysis Dictators in Vegetarian condition)
233
234
235 /* END */
```

Listing A.1: Power analysis for sample size.

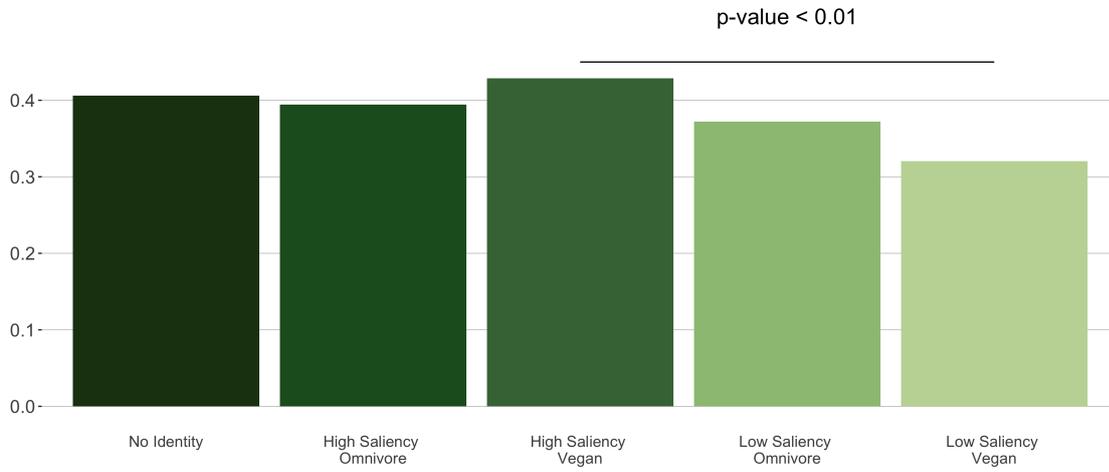
A.9.2 Pilot data

A.9.2.1 Pilot data session 1

I ran the first pilot session in November 2021. The pilot session was a simple dictator game with an initial endowment of £1 that dictators divided between themselves and a recipient. The session included United Kingdom participants, 400 dictators, and 50 recipients. The dictators' decisions were effectively applied and the bonus payment was defined according to the dictators' decision. The dictators were all omnivores and were randomly matched in one of the five conditions: *No Identity*, *High Saliency Omnivore*, *High Saliency Vegan*, *Low Saliency Omnivore*, *Low Saliency Vegan*. The difference between treatments was the information given to the dictator. In *No Identity*, dictators received the following information on their matched recipient: is over 18 years old, lives in the United Kingdom, and Prolific member. In *High Saliency Omnivore* and *High Saliency Vegan*, dictators received the following information on their matched recipient: is over 18 years old, lives in the United Kingdom, Prolific member, and dietary identity. The dietary identity was an omnivore for dictators in *High Saliency Omnivore* and vegan for dictators in *High Saliency Vegan*. Finally, dictators in *Low Saliency Omnivore* and *Low Saliency Vegan* received the following information about their matched recipient: is over 18 years old, lives in the United Kingdom, Prolific member, dietary identity, first language is English and enjoys traveling. The dietary identity was omnivore for dictators in *Low Saliency Omnivore* and vegan for dictators in *Low Saliency Vegan*.

Results from the pilot session are reported in Figure A6. Dictators in *High Saliency Vegan* discriminated less than dictators in *Low Saliency Vegan* (two-sided Wilcoxon rank-sum test, p-value < 0.01). In other words, when the recipient's dietary is highly salient (i.e., less information is given) dictators gave more money than when the dietary identity was less salient. This result suggests that the experimenter demand was possibly too high and that participants anticipated the goal of the experiment when the dietary identity is too salient (Zizzo, 2010). Following this first pilot session, I decided to keep only the low-saliency option.

Figure A6: Dictators allocations in pilot session 1, by treatment.



Note: Comparisons using Wilcoxon rank-sum test. $N = 400$ ($n = 80$ in each treatment).

A.9.2.2 Pilot data session 2.

I ran the second pilot session in January 2022. This pilot session replicated the final experiment with only the *OMNI* and *VGN* treatment. The session included United States participants from Prolific, 100 dictators (i.e., 50 in *OMNI* and 50 in *VGN* treatment) and 20 recipients (i.e., 10 in *OMNI* and 10 *VGN* treatment). Table A23 reports the estimation of the inequity aversion parameters for the dictators in the pilot session. The main results from the pilot data indicated a statistically significant negative treatment effect on the α parameter (i.e., aversion to disadvantageous inequality). Dictators exhibited greater aversion to disadvantageous inequality with vegans than with omnivores. The results on the β parameter (i.e., aversion to advantageous inequality) did not allow me to interpret a clear treatment effect.

Table A23: Pilot session structural estimation of inequity parameters for dictators.

Model	Parameters full sample			Treatment effect	
	λ	α	β	α_{vgn}	β_{vgn}
<i>MixLogit</i> _{correlation}	23.737*** (2.512)	0.068 (0.237)	0.524*** (0.112)	-0.661* (0.362)	-0.035 (0.147)
<i>MixLogit</i> _{nocorrelation}	23.602*** (2.494)	0.008 (0.232)	0.543*** (0.110)	-0.660* (0.059)	-0.144 (0.363)
<i>CLogit</i>	39.193*** (4.534)	-0.026 (0.117)	0.554*** (0.070)	-0.364** (0.183)	-0.156 (0.105)

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors shown in parentheses. (N = 100).

A.9.3 Pre-registration on OSF

The following information is taken from the pre-registration entry on OSF. The pre-registration is available by clicking on this link: <https://osf.io/h92wn>.

Study Information

Hypotheses

Research questions:

Is vegophobia real? Is it measurable in an economic environment?
How is vegophobia perceived by plant-based diets adepts?

Hypotheses:

O Dictators will display greater aversion to disadvantageous inequality with vegans than with omnivores.

O Dictators will display greater aversion to disadvantageous inequality with vegetarians than with omnivores.

Important notes:

- No registered hypothesis on advantageous inequality for dictators.
- No registered hypothesis for recipients.

Design Plan

Study type

Experiment - A researcher randomly assigns treatments to study subjects, this includes field or lab experiments. This is also known as an intervention experiment and includes randomized controlled trials.

Blinding

For studies that involve human subjects, they will not know the treatment group to which they have been assigned.

Is there any additional blinding in this study?

No response

Study design

The study is an online experiment with a between subject design involving US participants recruited via Prolific. Participants will receive a fix fee of £1.83 and will have the opportunity to earn up to £1 in bonus payment. Participants are used to deal with £ as it is the platform's used currency. I will give participants the maximum they can earn in dollar. The experiment is estimated to last 13 minutes.

The experiment will contain two branches, dictators and recipients. Each branch will contain three conditions. Dictators will be randomly matched with recipients

according to the conditions.

For dictators, the three conditions will be omnivore (baseline), vegetarian and vegan. Dictators will all be omnivores and will be randomly assigned to a condition.

For recipients, the three conditions will be omnivore (baseline), vegetarian and vegan. The recipients will be assigned to a condition according to their dietary identity. Recipients will not know that they are assigned to a condition based on this characteristic.

The experiment will be divided in three parts: choice task, beliefs and survey.

The main variable will be a repeated binary outcome, clustered at the individual level.

Experiment_tree.key

Randomization

Participants will be assigned to a condition based on their individual characteristic (see above).

Dictators will randomly be assigned to a condition. The randomization will occur on the platform Prolific.

Dictators and recipients will randomly be matched, ex-post, for bonus payment. The random matching will be done using R.

Sampling Plan

Existing Data

Registration prior to creation of data

Explanation of existing data

N/A

Data collection procedures

Participants will be recruited via the online platform Prolific and will participate to the experiment on LimeSurvey. Participants will receive a fix fee of £1.83 and will have the opportunity to earn up to £1 in bonus payment. The experiment is estimated to last 13 minutes.

Participants in the dictator branch will all be omnivores and will have the following characteristics: over 18 years old, live in the United States, prolific member, first language is English and enjoy travelling. These characteristics will be recovered from their Prolific account and will therefore true. Participants do not know that they

are selected for having these characteristics. Additionally, the dictators will be given the following information about their partner (i.e., recipient), depending on the condition: over 18 years old, lives in the United States, prolific member, first language is English, enjoy travelling and dietary identity. This information will be given in random order.

Participants in the recipient branch will either be omnivores, vegetarian or vegans and will be matched to a condition according to their dietary identity. They will also have the following characteristics: over 18 years old, live in the United States, prolific member, first language is English and enjoy travelling. These characteristics will be recovered from their Prolific account and will therefore be true. Participants do not know that they are selected for having these characteristics. Additionally, the recipients will be given the following information about their partner (i.e., dictator): over 18 years old, lives in the United States, prolific member, first language is English, enjoy travelling and is omnivore. This information will be given in random order. Recipients will also be told that the dictator was given the following information about them, depending on the condition: over 18 years old, lives in the United States, prolific member, first language is English, enjoy travelling and dietary identity. This information will be given in random order.

Power_analysis.do

Sample size

The total sample size will be 1500 observations.

For dictators, the three conditions will be omnivore (baseline), vegetarian and vegan. Each condition will have 400 dictators. This number was determined using a power analysis.

For recipients, the three conditions will be omnivore (baseline), vegetarian and vegan. Each condition will have 100 recipients. This number was determined using a power analysis.

Sample size rationale

The number of participants was determined by running a power analysis (see Power_analysis.do). I used the estimates and effect sizes from pilot data to calculate the probability to detect a statistically significant effect. The pilot data replicated the final experiment with the baseline and vegan condition for dictators and recipients, and a small sample size (i.e., 100 participants for the dictator branch and 20 participants for the recipient branch).

The power analysis simulated samples of 200, 400, 600 and 800 participants, 1000 times each, calibrated with estimates and effect sizes from the pilot data. This simulation ran for the dictator branch with the vegetarian and vegan conditions. The chosen alpha (i.e., statistical threshold) was 5%. I used uni-lateral tests and applied a Bonferroni correction for multiple hypothesis testing as I will be testing two hypotheses. I used a Bonferroni correction in the power analysis to be conservative but I will use a Holm-Bonferroni correction in the analysis.

The power analysis estimated the treatment effect size for aversion to advantageous inequality and disadvantageous inequality. Since no pilot data was gathered on the vegetarian condition, I divided the effect size by 2 as I expect less discrimination towards vegetarians than towards vegans. The power analysis code will be included in the final paper. The probabilities to detect a statistically significant effect are reported below, per branch and condition:

Dictator branch and vegan condition

Total sample size	disadvantageous inequality	advantageous inequality
200	79.5%	6.1%
400	98.1%	9.2%
600	99.8%	8.7%
800	100%	10.7%

Dictator branch and vegetarian condition

Total sample size	disadvantageous inequality	advantageous inequality
200	27.8%	4.3%
400	49.9%	4.9%
600	66.9%	4.7%
800	82.3%	6.5%

Stopping rule

N/A

Variables

Manipulated variables

N/A

No files selected

Measured variables

The dependent variable will be a binary outcome defined by choosing either the Top or Bottom option in the decision matrices.

Participants will enter the experiment and will be matched in one of the two branches (i.e., Dictator or Recipient) and to one of the conditions (i.e., Omnivore, Vegetarian or Vegan). Participants will then face 15 decision matrices.

Decision number	Choice	Own	Other
1	Top	100	200
1	Bottom	100	100
2	Top	100	200

2	Bottom	100	140
3	Top	120	80
3	Bottom	100	100
4	Top	140	0
4	Bottom	120	140
5	Top	140	20
5	Bottom	120	100
6	Top	140	40
6	Bottom	80	80
7	Top	140	40
7	Bottom	120	120
8	Top	140	60
8	Bottom	120	100
9	Top	140	80
9	Bottom	120	120
10	Top	140	120
10	Bottom	80	80
11	Top	160	0
11	Bottom	100	100
12	Top	160	40
12	Bottom	120	80
13	Top	160	80
13	Bottom	140	160
14	Top	200	0
14	Bottom	100	100
15	Top	200	0
15	Bottom	180	20

Each decision will contain a different payoff combination. The dependent variable will therefore be 15 binary decisions per participant.

For the dictator branch, the dependent variable will be the 15 binary choices.
For the recipient branch, the dependent variable will be the 15 binary choices that they believe their match dictator chose.

No files selected

Indices

N/A

No files selected

Analysis Plan

Statistical models

I will estimate parameters of inequity aversion introduced by Fehr and Schmidt (1999). The utility function for player i is defined as so:

$U_i(x) = x_i - \alpha_i \max(x_j - x_i, 0) - \beta_i \max(x_i - x_j, 0)$ where i is different from j

x_i represents the weight player i places on her own income, α_i the utility loss when player j has a higher payoff than player i (i.e., aversion to disadvantageous inequality) and β_i the utility loss when player i has a higher payoff than player j (i.e., aversion to advantageous inequality).

To estimate the parameters, I will use a mixed logit model assuming correlation between the random terms. This method allows to recover the mean and standard deviation of the parameters. I will regress the decision of choosing top or bottom on the weight player i places on her own payoff, the two random parameters α and β , and condition interaction terms on the α and β to determine variations between conditions. The interaction terms are treatment dependent variables, meaning that they measure the variation from the baseline condition. I will then recover the estimates and make non-linear combinations to recover the true value of the utility function parameters.

The parameters, which are the mean of the estimated parameters, are calculated as follows:

$\Lambda = 1 / \text{coefficient own payoff}$

$\alpha = \text{coefficient } \alpha / \text{coefficient own payoff}$

$\beta = \text{coefficient } \beta / \text{coefficient own payoff}$

$\text{interaction_}\alpha\text{_VGT} = \text{coefficient interaction_}\alpha\text{_VGT} / \text{coefficient own payoff}$

$\text{interaction_}\beta\text{_VGT} = \text{coefficient interaction_}\beta\text{_VGT} / \text{coefficient own payoff}$

$\text{interaction_}\alpha\text{_VGN} = \text{coefficient interaction_}\alpha\text{_VGN} / \text{coefficient own payoff}$

$\text{interaction_}\beta\text{_VGN} = \text{coefficient interaction_}\beta\text{_VGN} / \text{coefficient own payoff}$

As robustness checks, I will also estimate a conditional logit model and a mixed logit model assuming no correlation between the two random parameters. I will explore the sensitivity of the treatment effect to control variables.

No files selected

Transformations

N/A

Inference criteria

The statistical analysis that will be used is a frequentist approach with p-values and a statistical threshold of 5% (i.e., $\alpha = 0.05$). The two hypotheses will be tested using one-tailed tests. I will use a Holm-Bonferroni correction method for multiple hypothesis testing.

Data exclusion

For this experiment, I will use the following exclusion rules :

O If a participant completes the experiment in less than 4 minutes and 20 seconds (i.e., one third of the estimated total time), then he/she will be excluded from the

analysis.

O The experiment contains two comprehension checks and an additional example available on request for participants. The two comprehension checks are simple questions to make sure that the participant understands the payoff structure of the decisions matrices. Several scenarios may occur :

- If a participant fails to correctly answer both comprehension questions and does not request the additional example, then he/she will be excluded from the analysis.
- If a participant fails to correctly answer both the comprehension questions, requests an additional example and answers no to understanding the payoffs, then he/she will be excluded from the analysis.
- If a participant fails to correctly answer both the comprehension questions, requests an additional example and answers yes to understanding the payoffs, then he/she will be included in the analysis.
- If a participant correctly answers one of the comprehension questions and does not request the additional example, then he/she will be included in the analysis.
- If a participant correctly answers one of the comprehension questions and requests an additional example, then he/she will be included in the analysis.
- If a participant correctly answers both comprehension questions and does not request the additional example, then he/she will be included in the analysis.

O The experiment contains two attention checks. One attention check is located before the main decision task and the other one is located on the survey page. The two attention checks are as follow:

Question: This is an attention check. Please select the word «Green».

- Blue
- Red
- Green
- Yellow

Question: This is an attention check. Please select the word «Car».

- Bus
- Car
- Train
- Plane

Several scenarios may occur :

- If a participant fails to correctly answer both attention checks, then he/she will be excluded from the analysis.
- If a participant correctly answers at least one attention check, then he/she will be included in the analysis.
- If a participant correctly answers both attention checks, then he/she will be included in the analysis.

Missing data

Participants must complete the full experiment in order to receive a payoff.

Observations with incomplete or missing data could occur if a participant does not complete the full experiment. In this case, the observation will be deleted from the final sample. This will not affect the final sample size as Prolific will provide participants up to the desired sample size.

Exploratory analysis

This study will have exploratory analyses without pre-registered hypotheses, as listed below.

O Vegetarian / Vegan Threat Scale. All omnivore participants (i.e., dictators in all conditions) will answer either a vegetarian threat scale or a vegan threat scale. This scale measures the perceived threat from the rise of vegetarianism or veganism. Participants report their degree of agreement with statements using a Likert scale ranging from 1 (strongly disagree) to 7 (strongly agree). The Vegetarian / Vegan Threat Scale comprises 8 statements, displayed in random order:

Question: How much do you agree with the following statements?

- Vegetarianism [Veganism] has a negative influence on the United States economy.
- The vegetarian [vegan] movement is too involved in local and national politics.
- Nowadays, when it comes to nutrition and meals, people listen too much to what a minority of vegetarians [vegans] wants.
- The rise of vegetarianism [veganism] poses a threat to our country's cultural customs.
- Eating meat is part of our cultural habits and identity, and some people should be more respectful of that.
- Important culinary traditions which are typical to our country are starting to die out due to the rise of vegetarianism [veganism].
- Vegetarianism [Veganism] has a negative influence on the United States economy.
- Important family traditions and celebrations are increasingly being ruined and disappearing because of the presence of vegetarians [vegans] in certain families.

Participants will answer according to the following rules:

- Participants in the dictator branch and vegetarian condition, and half of the participants in the dictator branch and omnivore condition will answer the vegetarian threat scale.
- Participants in the dictator branch and vegan condition, and half of the participants in the dictator branch and omnivore condition will answer the vegan threat scale.

I will compare the vegetarian threat scale scores between conditions. I will compare the vegan threat scale scores between conditions. I will compare the vegetarian threat scale and vegan threat scale scores.

O Social Dominance Orientation Scale. All participants, regardless of the branch or condition, will answer the social dominance orientation scale. This scale is designed to capture the participants' degree of preference for dominance in the society. Participants report how they feel regarding statements using a Likert scale ranging from 1 (very negative) to 7 (very positive). The Social Dominance Orientation Scale comprises 8 statements, displayed in random order:

Question: how do you feel about the following statements?

- In getting what you want, it is sometimes necessary to use force against other groups.
- No one group should dominate in society.
- We should strive to make incomes as equal as possible.
- All groups should be given an equal chance in life.
- If certain groups stayed in their place, we would have fewer problems.
- We would have fewer problems if we treated people more equally.
- Some groups of people are simply inferior to other groups.
- It's OK if some groups have more of a chance in life than others.

I will compare the social dominance orientation scores between conditions.

O Everyday Discrimination. All recipients will answer the Everyday Discrimination Scale, dictators do not answer the scale. The scale measures one's perceived everyday discrimination in different situations. Participants report how often they experience different discriminating situations due to their dietary choices using a Likert scale ranging from never to almost everyday. The Everyday Discrimination Scale comprises 9 situations, displayed in random order:

Question: In your day-to-day life, how often do any of the followings happen to you due to your dietary choices?

- Being treated with less courtesy than others.
- Being treated with less respect than others.
- Receiving poorer service than others in restaurants or stores.
- People acting as if you are not smart.
- People acting as if they are better than you.
- People think you are dishonest.
- Being called names or insulted.
- Being threatened or harassed.

I will compare the Everyday Discrimination scores between conditions.

O Vegetarian / Vegan number belief. All participants, regardless of the branch or condition, will be asked to report their beliefs on the number of vegetarians and vegans in the US. The two questions will appear on the same screen. Participants will enter a number between 0 and 100.

Question: What percentage of the United States population do you believe is vegetarian?

Please enter a number between 0 and 100:

Question: What percentage of the United States population do you believe is vegan?

Please enter a number between 0 and 100:

I will compare the reported number of vegetarians and vegans within each condition.
I will also compare the reported number of vegetarians and vegans between

branches and conditions.

O Partner's beliefs. All participants, regardless of the branch or condition, will be asked to report their beliefs on their partner's income, gender, political preferences and sexual orientation according to the information that they were given (see above). All answers will be displayed in random order except for the partner's income. The questions will be displayed in random order:

In the first part of the experiment, you were matched with a partner who has the following characteristics (displayed in random order):
over 18 years old, lives in the United States, prolific member, first language is English, dietary identity and enjoys travelling.

Question: How much do you believe this person earns per year? (select one answer)

- Less than \$10,000
- \$10,000 to \$19,999
- \$20,000 to \$29,999
- \$30,000 to \$39,999
- \$40,000 to \$49,999
- \$60,000 to \$69,999
- \$70,000 to \$79,999
- \$80,000 to \$89,999
- \$90,000 to \$99,999
- \$100,000 or more

Question: Which political side do you believe this person feels closer to? (select one answer)

- Independent
- Left-wing
- Right-wing

Question: What do you believe this person's gender to be? (select one answer)

- Male
- Female

Question: What do you believe this person's sexual orientation to be? (select one answer)

- LGBT (Lesbian, Gay, Bisexual, Transgender)
- Heterosexual

I will compare the beliefs between branches and conditions.

O Control variables. I will explore the sensitivity of the treatment effect to control variables.

Other

Other

N/A

A.9.4 Experiment screenshots

A.9.4.1 Experiment screenshots for the dictators in the *OMNI* treatment.

Screen #1

Language: English - English

Choice Experiment - FSDO

Welcome!

Thank you for participating in this experiment.

The experiment is expected to last **13 minutes** and you will receive a fix fee of **£1.83**. Additionally, you will have the opportunity to earn up to **£1 in bonus payment** based on your decisions.

In total, you will have the opportunity to earn up to **£2.83**, which corresponds approximately to **\$3.60**.

The experiment is divided in two parts. Every step of the experiment will be explained in detail.

Next

Screen #2

• This is an attention check. Please select the word "Green".

Choose one of the following answers

- Yellow
- Red
- Blue
- Green

Next

Screen #3

Choice Task - Explanations

For this first part, you are a decision-maker and are randomly matched with another participant. You will be asked to make choices, which will determine your bonus payment and your matched partner's bonus payment. Your matched partner does not make any decision. All your decisions are anonymous.

The choices are independent from one another, meaning that your decision in one choice does not affect any subsequent choices. For each choice, you will be asked to allocate ECUs (Experimental Currency Units) between yourself and a matched partner, and he or she will remain the same for all the decisions. One of your decisions will be randomly selected, and you will be paid accordingly as bonus payment. Your decision will determine your matched partner's payoff if it is randomly selected.

(Note: Your decision will always determine your payoff. Your partner might be matched with several decision-makers. In this case, only one decision will be randomly selected to be applied to him / her.)

The conversion rate is : 2 ECUs = £0.01 or 1 cent.

Examples: if you allocate yourself 50 ECUs, then your bonus payment is £0.25 or 25 cents. If you allocate yourself 200 ECUs, then your bonus payment is £1.

The decision tables that you will face will look like this:

	Own payoff	Partner's payoff
Top	200	160
Bottom	180	170

The amounts will change across tables. For each decision table, you will have to choose between two options, "Top" or "Bottom". Each choice corresponds to a combination of payoffs for you and your partner. The "Own payoff" column corresponds to your payoffs and the column "Partner's payoff" corresponds to the payoffs of your partner. It is important to understand that you have full control over your own payoffs but also the payoffs of your partner. Your partner cannot influence your or his / her payoffs.

In the example decision table below, if you choose the "Top" option then your payoff will be 200 and your partner's payoff will be 160. This is shown in green in the table below.

	Own payoff	Partner's payoff
Top	200	160
Bottom	180	170

If you choose the "Bottom" option, then your payoff will be 180 and your partner's payoff will be 170. This is shown in green in the table below.

	Own payoff	Partner's payoff
Top	200	160
Bottom	180	170

You will get examples to practice and make sure that you understand the decisions and structure of the payoffs on the following page.

Next

Screen #4

The followings are example questions. No monetary payoffs are derived from these questions. If you wish to see the instructions again, scroll down and click on "Show instructions".

*Pick the option that would give you 120 ECU and give your partner 140 ECU.

	Own payoff	Partner's payoff
Top	100	120
Bottom	120	140

Choose one of the following answers

- Top
- Bottom

*Pick the option that would allow you to obtain a higher payoff than your partner.

	Own payoff	Partner's payoff
Top	200	240
Bottom	200	160

Choose one of the following answers

- Top
- Bottom

If you wish to see the instructions again please click on the button below.

- Show instructions

*Overall, do you understand the game and the payoffs ?

Choose one of the following answers

- Yes
- Unsure, I would like one more example

Next

Screen #4 bis

Here is an additional example to help you better understand.

Assume that the following decision table is displayed:

	Own payoff	Partner's payoff
Top	100	80
Bottom	200	120

Your payoffs always correspond to the "Own payoff" column and the payoff of your partner always correspond the column "Partner's payoff".

In this example, if you choose the top option then your payoff will be **100** and your partner's payoff will be **80**.

On the other hand, if you choose the bottom option, then your payoff will be **200** and your partner's payoff will be **120**.

*Now, do you understand the game and the payoffs ?

Choose one of the following answers

- Yes
- No

Next

Screen #5

Part 1 - Choice Task

You will now face choices. One of your decision will be randomly selected and paid as a bonus payment.

Your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

Next

Screen #6

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	40
Bottom	80	80

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #7

*Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	160	0
Bottom	100	100

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #8

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	120
Bottom	80	80

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #9

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	40
Bottom	120	120

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #10

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	0
Bottom	120	140

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #11

• Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	60
Bottom	120	100

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #12

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	200	0
Bottom	100	100

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #13

*Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	80
Bottom	120	120

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #14

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	140	20
Bottom	120	100

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #15

• Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	100	200
Bottom	100	140

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #16

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	160	40
Bottom	120	80

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #17

*Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	160	80
Bottom	140	160

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #18

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	120	80
Bottom	100	100

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #19

*Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	200	0
Bottom	180	20

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #20

Please choose between the Top or Bottom option.

	Own payoff	Partner's payoff
Top	100	200
Bottom	100	100

Choose one of the following answers

Top

Bottom

Remember that your matched partner has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

If you wish to see the instructions again please click on the button below.

Show instructions

Next

APPENDIX A

Screen #21

The choice task is done. All your decisions have been recorded and one will be randomly selected as bonus payment for you and your matched partner.

Let's move on to the second part.

Next

Screen #22

PART 2 - Survey

In the next part you will be asked to answer a series of questions. Please answer them as truthfully as possible. All your answers are anonymous.

Next

APPENDIX A

Screen #23

In the first part of the experiment, you were matched with a partner who has the following characteristics:

- Over 18 years old
- Lives in the United States
- First language is English
- Enjoys travelling
- Prolific Member
- Omnivore

*How much do you believe this person earns per year?

Choose one of the following answers

- Less than \$10,000
- \$10,000 to \$19,999
- \$20,000 to \$29,999
- \$30,000 to \$39,999
- \$40,000 to \$49,999
- \$50,000 to \$59,999
- \$60,000 to \$69,999
- \$70,000 to \$79,999
- \$80,000 to \$89,999
- \$90,000 to \$99,999
- \$100,000 or more

*Which political side do you believe this person feels closer to?

Choose one of the following answers

- Right-wing
- Independent
- Left-wing

*What do you believe this person's gender to be?

Choose one of the following answers

- Male
- Female

*What do you believe this person's sexual orientation to be?

Choose one of the following answers

- LGBT (Lesbian, Gay, Bisexual, Transgender)
- Heterosexual

Next

Screen #24

*What percentage of the United States population do you believe is vegetarian?
Please enter a number between 0 and 100.

ⓘ Your answer must be between 0 and 100
ⓘ Only an integer value may be entered in this field.

*What percentage of the United States population do you believe is vegan?
Please enter a number between 0 and 100.

ⓘ Your answer must be between 0 and 100
ⓘ Only an integer value may be entered in this field.

Next

APPENDIX A

Screen #25

On a scale from 1 (very negative) to 7 (very positive), how do you feel about the following statements?

	1 (Very negative)	2 (Negative)	3 (Slightly negative)	4 (Neither positive nor negative)	5 (Slightly positive)	6 (Positive)	7 (Very positive)
In getting what you want, it is sometimes necessary to use force against other groups.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
If certain groups stayed in their place, we would have fewer problems.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
All groups should be given an equal chance in life.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
We should strive to make incomes as equal as possible.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
We would have fewer problems if we treated people more equally.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Some groups of people are simply inferior to other groups.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
It's OK if some groups have more of a chance in life than others.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
No one group should dominate in society.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Next

Screen #26

On a scale from 1 (strongly disagree) to 7 (strongly agree), how much do you agree with the following statements?

	1 (Strongly disagree)	2 (Disagree)	3 (Slightly disagree)	4 (Neither agree nor disagree)	5 (Slightly agree)	6 (Agree)	7 (Strongly agree)
The rise of veganism poses a threat to our country's cultural customs.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Important family traditions and celebrations are increasingly being ruined and disappearing because of the presence of vegans in certain families.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Veganism has a negative influence on the United States economy.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Eating meat is part of our cultural habits and identity, and some people should be more respectful of that.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The vegan movement is too involved in local and national politics.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Important culinary traditions which are typical to our country are starting to die out due to the rise of veganism.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Nowadays, when it comes to nutrition and meals, people listen too much to what a minority of vegans wants.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Vegans should have more respect for our traditional eating customs, which meat is simply a part of.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Next

APPENDIX A

Screen #27

***Please select your age:**

...

***What is your yearly income?** Choose one of the following answers

- Less than \$10,000
- \$10,000 to \$19,999
- \$20,000 to \$29,999
- \$30,000 to \$39,999
- \$40,000 to \$49,999
- \$50,000 to \$59,999
- \$60,000 to \$69,999
- \$70,000 to \$79,999
- \$80,000 to \$89,999
- \$90,000 to \$99,999
- \$100,000 or more

***Regarding politics, do you consider yourself closer to:** Choose one of the following answers

- Left-wing
- Right-wing
- Independent

***This is an attention check. Please select the word "Car".** Choose one of the following answers

- Bus
- Plane
- Train
- Car

***What religious family do you belong to or identify yourself most close to?** Choose one of the following answers

Please choose... ▾

Screen #27 bis

*How often do you attend religious service?
Choose one of the following answers

Please choose... ▾

*Are any of your family members vegetarians or vegans?
Choose one of the following answers

Yes
 No

*Are any of your friends vegetarians or vegans?
Choose one of the following answers

Yes
 No

*How would you best describe yourself?
Choose one of the following answers

Please choose... ▾

*What is the highest education level that you have completed?
Choose one of the following answers

Please choose... ▾

Submit

APPENDIX A

Screen #28

Thank you for participating!

Please click on the following link to go back to prolific: [Back to Prolific](#)

A.9.4.2 Experiment screenshots for the recipients in the *OMNI* treatment.

Screen #1

Language: English - English Change the language

Choice Experiment - FSRO

Welcome!

Thank you for participating in this experiment.

The experiment is expected to last **13 minutes** and you will receive a fix fee of **£1.83**. Additionally, you will have the opportunity to earn up to **£1 in bonus payment** based on your decisions.

In total, you will have the opportunity to earn up to **£2.83**, which corresponds approximately to **\$3.60**.

The experiment is divided in two parts. Every step of the experiment will be explained in detail.

Next

Screen #2

• This is an attention check. Please select the word "Green".

Choose one of the following answers

- Yellow
- Red
- Blue
- Green

Next

Screen #3

Choice Task - Explanations

In this first part, you will not make any decisions but it is important that you understand the structure of the decisions that your matched partner will make. You will have similar questions in the second part of the experiment and will have the opportunity to earn bonus payment depending on the accuracy of your answers.

You will be matched with a partner who will be presented with a series of choices and will be the decision maker. Each choice is anonymous and the choices are independent from one another, meaning that a decision in one choice does not affect any subsequent choices. For each choice, the decision maker will be asked to allocate ECUs (Experimental Currency Units) between himself / herself and you. You will be matched with one decision maker for all the decisions and he / she will be given some information about you. One of the decisions will be randomly selected and the decision maker will receive a bonus payment accordingly.

Your bonus payment will either be determined by the decision maker's decision or by the second part of the experiment. A random draw will determine which bonus payment you will get.

The conversion rate is : 2 ECUs = £0.01 or 1cent.

Examples: if the decision maker allocates you 50 ECUs, then your bonus payment is £0.25 or 25 cents. If he / she allocates you 200 ECUs, then your bonus payment is £1.

The decision tables that the decision maker will face will look like this:

	Decision maker's payoff	Own payoff
Top	200	160
Bottom	180	170

The amounts will change across tables. For each decision table, the decision maker will have to choose between two options, "Top" or "Bottom". Each choice corresponds to a combination of payoffs for the decision maker and you. The "Decision maker's payoff" column corresponds to the payoffs of the decision maker and the column "Own payoff" corresponds to your payoffs. Again, it is important that you understand that you have no control over your own payoffs or the decision maker's payoffs, and that all the payoffs are decided by the decision maker.

In the decision table below, if the decision maker chooses the "Top" option then his / her payoff will be 200 and your payoff will be 160. This is shown in green in the table below.

	Decision maker's payoff	Own payoff
Top	200	160
Bottom	180	170

If the decision maker chooses the "Bottom" option, then his / her payoff will be 180 and your payoff will be 170. This is shown in green in the table below.

	Decision maker's payoff	Own payoff
Top	200	160
Bottom	180	170

You will get examples to practice and make sure that you understand the decisions and structure of the payoffs on the following page.

Next

Screen #4

The followings are example questions. No monetary payoffs are derived from these questions. If you wish to see the instructions again, scroll down and click on "Show instructions".

* Which option gives you the highest payoff?

	Decision maker's payoff	Own payoff
Top	200	240
Bottom	200	160

Choose one of the following answers

- Top
- Bottom

* If the decision maker wants to earn 120 ECU and give you 140 ECU, which option should he / she pick?

	Decision maker's payoff	Own payoff
Top	100	120
Bottom	120	140

Choose one of the following answers

- Top
- Bottom

If you wish to see the instructions again please click on the button below.

- Show instructions

* Overall, do you understand the game and the payoffs ?

Choose one of the following answers

- Yes
- Unsure, I would like one more example

Next

Screen #4 bis

Here is an additional example to help you better understand.

Assume that the following decision table is displayed:

	Decision maker's payoff	Own payoff
Top	100	80
Bottom	200	120

Your payoffs always correspond to the "Own payoff" column and the payoff of the decision maker always correspond the column "Decision maker's payoff".

In this example, if the decision maker chooses the top option then your payoff will be 80 and the decision maker's payoff will be 100.

On the other hand, if the decision maker chooses the bottom option, then your payoff will be 120 and his / her payoff will be 200.

*Now, do you understand the game and the payoffs ?

Choose one of the following answers

- Yes
- No

Next

Screen #5

Part 1 - Choice Task

The decision maker will face a series of choices and will make decisions. The decision maker will allocate ECUs between himself / herself and you.

One of the decision maker's choices will be randomly selected for bonus payment.

The decision maker will be given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

This information was extracted from your prolific profile.

You are matched with a decision maker who has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

Next

Screen #6

The choice task is done. Your matched decision maker has now completed all the decisions.

Let's move on to the second part.

Next

Screen #7

PART 2 - Choice task beliefs

You will now face the decision tables that the decision maker faced in the first part of the experiment.

For each decision table, select the option (Top or Bottom) that you believe your matched decision maker selected. You may have the opportunity to earn a bonus payment depending on the accuracy of your answers.

One of your guesses will be randomly selected and compared to your matched decision maker's decision. If your guess is correct, you may then receive an additional bonus payment of £1.

Your bonus payment will either be determined by the decision maker's decision or by your guess in this section. A random draw will determine which bonus payment you will get.

Your matched decision maker has the following characteristics :

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

This information was extracted from your prolific profile.

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #8

Please remember to select the option that you believe your matched decision maker selected.

Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	80
Bottom	120	120

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

Screen #9

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	120	80
Bottom	100	100

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #10

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	0
Bottom	120	140

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

Screen #11

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	120
Bottom	80	80

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #12

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	200	0
Bottom	180	20

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

Screen #13

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	100	200
Bottom	100	140

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #14

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	160	40
Bottom	120	80

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #15

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	160	80
Bottom	140	160

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #16

Please remember to select the option that you believe your matched decision maker selected.

Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	160	0
Bottom	100	100

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

Screen #17

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	40
Bottom	120	120

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #18

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	100	200
Bottom	100	100

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

Screen #19

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	40
Bottom	80	80

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #20

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	60
Bottom	120	100

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

Screen #21

Please remember to select the option that you believe your matched decision maker selected.

*Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	140	20
Bottom	120	100

Choose one of the following answers

Top

Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

Show instructions

Next

Screen #22

Please remember to select the option that you believe your matched decision maker selected.

Please choose between the Top or Bottom option.

	Decision maker's payoff	Own payoff
Top	200	0
Bottom	100	100

Choose one of the following answers

- Top
- Bottom

Remember that your matched decision maker has the following characteristics:

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

The decision maker was given the following information (not necessarily in this order) about yourself:

- Enjoys travelling
- Over 18 years old
- Prolific Member
- First language is English
- Omnivore
- Lives in the United States

If you wish to see the instructions again please click on the button below.

- Show instructions

Next

APPENDIX A

Screen #23

All your guesses have been recorded. Let's move on to the second part.

Next

Screen #24

PART 2 - Survey

In the next part you will be asked to answer a series of questions. Please answer them as truthfully as possible. All your answers are anonymous.

Next

APPENDIX A

Screen #25

*What percentage of the United States population do you believe is vegetarian?
Please enter a number between 0 and 100.

ⓘ Your answer must be between 0 and 100
ⓘ Only an integer value may be entered in this field.

*What percentage of the United States population do you believe is vegan?
Please enter a number between 0 and 100.

ⓘ Your answer must be between 0 and 100
ⓘ Only an integer value may be entered in this field.

Next

Screen #26

In the first part of the experiment, you were matched with a decision maker who had the following characteristics:

Your matched decision maker has the following characteristics :

- Prolific Member
- Lives in the United States
- Omnivore
- Enjoys travelling
- First language is English
- Over 18 years old

What do you believe this person's sexual orientation to be?

Choose one of the following answers

- LGBT (Lesbian, Gay, Bisexual, Transgender)
- Heterosexual

What do you believe this person's gender to be?

Choose one of the following answers

- Male
- Female

How much do you believe this person earns per year?

Choose one of the following answers

- Less than \$10,000
- \$10,000 to \$19,999
- \$20,000 to \$29,999
- \$30,000 to \$39,999
- \$40,000 to \$49,999
- \$50,000 to \$59,999
- \$60,000 to \$69,999
- \$70,000 to \$79,999
- \$80,000 to \$89,999
- \$90,000 to \$99,999
- \$100,000 or more

Which political side do you believe this person feels closer to?

Choose one of the following answers

- Left-wing
- Independent
- Right-wing

Next

Screen #27

*In your day-to-day life, how often do any of the followings happen to you due to your dietary choices?

	Never	Less than once a year	A few times a year	A few times a month	At least once a week	Almost everyday
Receiving poorer service than others in restaurants or stores	<input type="radio"/>					
Being called names or insulted	<input type="radio"/>					
Being treated with less respect than others	<input type="radio"/>					
People acting as if they are better than you	<input type="radio"/>					
People acting as if you are not smart	<input type="radio"/>					
Being threatened or harassed	<input type="radio"/>					
Being treated with less courtesy than others	<input type="radio"/>					
People think you are dishonest	<input type="radio"/>					

Next

Screen #28

On a scale from 1 (very negative) to 7 (very positive), how do you feel about the following statements?

	1 (Very negative)	2 (Negative)	3 (Slightly negative)	4 (Neither positive nor negative)	5 (Slightly positive)	6 (Positive)	7 (Very positive)
In getting what you want, it is sometimes necessary to use force against other groups.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Some groups of people are simply inferior to other groups.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
It's OK if some groups have more of a chance in life than others.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
We should strive to make incomes as equal as possible.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
No one group should dominate in society.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
All groups should be given an equal chance in life.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
We would have fewer problems if we treated people more equally.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
If certain groups stayed in their place, we would have fewer problems.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Next

APPENDIX A

Screen #29

*Please select your age:

...

*What is your yearly income?

Choose one of the following answers

- Less than \$10,000
- \$10,000 to \$19,999
- \$20,000 to \$29,999
- \$30,000 to \$39,999
- \$40,000 to \$49,999
- \$50,000 to \$59,999
- \$60,000 to \$69,999
- \$70,000 to \$79,999
- \$80,000 to \$89,999
- \$90,000 to \$99,999
- \$100,000 or more

*What religious family do you belong to or identify yourself most close to?

Choose one of the following answers

Please choose...

*Regarding politics, do you consider yourself closer to:

Choose one of the following answers

- Left-wing
- Right-wing
- Independent

*This is an attention check. Please select the word "Car".

Choose one of the following answers

- Train
- Plane
- Car
- Bus

*How often do you attend religious service?

Choose one of the following answers

Please choose...

Screen #29 bis

*Are any of your family members vegetarians or vegans?
Choose one of the following answers

Yes
 No

*Are any of your friends vegetarians or vegans?
Choose one of the following answers

Yes
 No

*How would you best describe yourself?
Choose one of the following answers

Please choose... ▾

*What is the highest education level that you have completed?
Choose one of the following answers

Please choose... ▾

Submit

APPENDIX A

Screen #30

Thank you for participating!

Please click on the following link to go back to prolific: [Back to Prolific](#)

Chapter 2

Removing barriers to plant-based diets: assisting doctors with vegan patients¹

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¹The working paper corresponding to this chapter is coauthored with Romain Espinosa (CIRED, CNRS, Nogent-sur-Marne, France), Paco Maginot (Observatoire National de l'Alimentation Végétale, Paris, France), Sebastien Demange (Observatoire National de l'Alimentation Végétale, Paris, France) and Florimond Peureux (Observatoire National de l'Alimentation Végétale, Paris, France). This chapter is a Registered Report that received Stage-1 and Stage-2 reviews, and was accepted at PCI-RR (<https://rr.peercommunityin.org/articles/rec?id=307>).

1 Introduction

Dietary changes are expected to play a growing role in climate-change mitigation over the coming years. The global food system generates a variety of greenhouse gases (CO₂, CH₄, N₂O) from multiple sources, including deforestation, fertilizer use, and enteric fermentation and manure from livestock. Agriculture currently accounts for about 40% of global land use (Foley et al., 2005), with food production generating up to 30% of global greenhouse gas emissions (Vermeulen et al., 2012) and representing 70% of freshwater consumption (Molden, 2013, Steffen et al., 2015). Limiting the increase in global temperature to 1.5° or 2° above pre-industrial levels requires substantial shifts in dietary habits in addition to efforts to reduce fuel emissions (Clark et al., 2020).

More globally, accumulating evidence has shown considerable heterogeneity of types of diet with respect to their contribution to climate change (Theurl et al., 2020). Animal-based foods are shown to emit more GHG and to use more land (Poore and Nemecek, 2018), while vegetarian and vegan diets have the lowest environmental footprint together with the lowest mortality rates (Springmann et al., 2016, Clark and Tilman, 2017, Clark et al., 2019, Springmann et al., 2020). Recently, the Eat-Lancet Commission issued new guidelines in an attempt to define the contours of healthy and sustainable diets and urged for a reduction in animal-based foods (Willett et al., 2019). Similarly, the 2019 IPCC special report on GHG and land use emphasized the importance of dietary changes to mitigate climate change.

Pushing consumers towards plant-rich diets is therefore a key element in limiting global warming (Rust et al., 2020). The limited acceptability of coercive policies (Espinosa and Nassar, 2021) such as meat taxation (Douenne and Fabre, 2020) has encouraged governments to support individual spontaneous changes in diet. NGOs (Espinosa and Treich, 2021) and public authorities (Espinosa and Stoop, 2021) play an important role in informing consumers about the benefits of adopting a plant-based diet. However, some consumers who are willing to adopt such diets may be concerned about their health impact, and are likely to consult their doctor for advice. While the general opinion of doctors is thus a key element for the success of spontaneous shifts towards plant-based diets, some may lack information about recent scientific work showing the health benefits of these diets (Springmann et al., 2016, Clark and Tilman, 2017, Clark et al., 2019, Springmann et al., 2020).

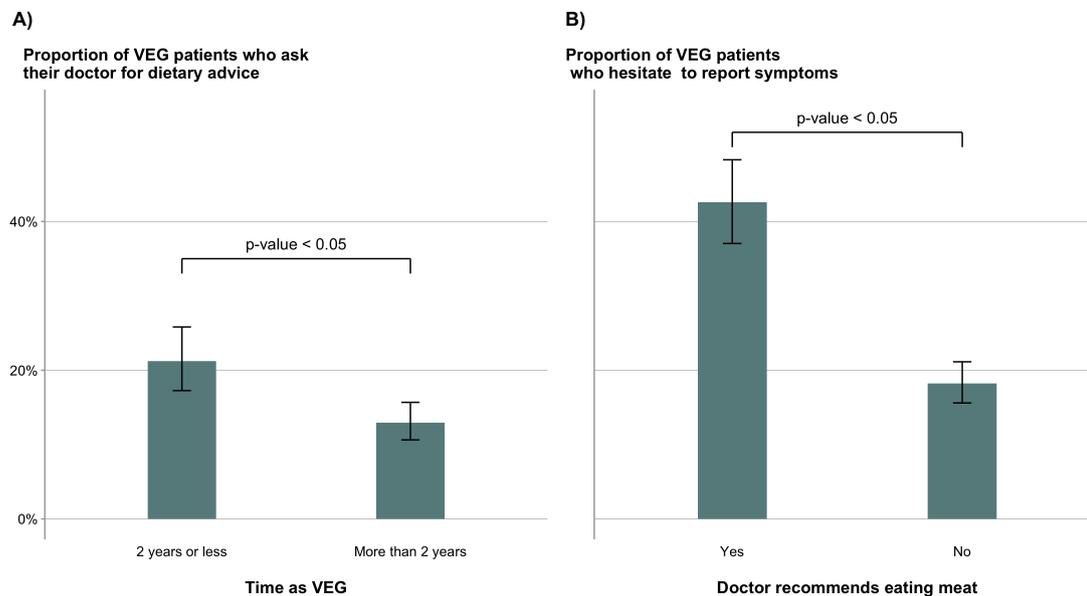
The outdated or incomplete knowledge of doctors may therefore hinder the global environmental objective of a shift toward plant-based diets.

Doctors are an available source of information and frequently give nutritional advice to their patients to improve their health (Anis et al., 2004). Some studies have demonstrated that doctors might positively influence dietary changes through counseling (Rousset et al., 2003), specifically the reduction of red meat consumption (McIntosh et al., 1995), and switches to vegetarianism or veganism (Cramer et al., 2017). Doctors might also negatively influence their patients' dietary choices. Qualitative research in France has shown that some doctors try to prevent their patients from adopting plant-based diets (Villette, 2019, Boyadijan, 2018) and promote animal-based food (Borel, 2017). Doctors show increasing concern the greater the share of plant-based food in the diet (Borel, 2017), and are more likely to look for dietary deficiencies in vegetarian or vegan patients (Villette, 2019, Boyadijan, 2018, Passelergue, 2018). This aversion towards plant-based diets has been shown to be detrimental to the doctor-patient relationship (Vittoriani, 2021, Hardouin, 2018). Previous work estimates that one in four French vegetarians/vegans does not inform their doctor about their diet, and one in three has thought about changing their doctor because of his/her views about vegetarianism (Demange, 2017). Doctors' disapproval of their patients' diets can then harm the therapeutic relationship, lead patients to not reveal information about their behaviour or symptoms (Vittoriani, 2021, Demange, 2017, Rughoo, 2019), and produce mistrust of medical authority (Hardouin, 2018, Farella et al., 2020, Lahmer, 2018), a shift to alternative forms of medicine (Hardouin, 2018, Rughoo, 2019), and the use of other sources of medical information such as the internet (Rughoo, 2019, Lahmer, 2018). In addition, vegetarian and vegan patients frequently report that their doctor lacks information and provides inadequate advice about plant-based diets (Vittoriani, 2021, Hardouin, 2018, Demange, 2017, Rughoo, 2019, Lahmer, 2018, Baldassarre et al., 2020, Jarson and Maginot, 2020).

Doctors' inability to provide effective guidance on vegetarianism may weaken the commitment of vegetarian and vegan patients to their diet, by leading them to question the healthiness and suitability of plant-based diets. Figure 2.1 shows that French patients who have adopted a plant-based diet for two years or less are more likely to ask their doctors for advice, as compared to those who have been vegetarian or vegan for longer. Figure 2.1 further shows that the proportion of vegetarian or

vegan patients who hesitated to report symptoms to their doctor is significantly higher when the latter had advised them to eat meat.

Figure 2.1: Vegetarian/vegan (VEG) patients and doctor interactions, the importance of efficient dietary advice.



A) The proportion of vegetarian and vegan patients ($N = 1090$) who asked their doctor for dietary advice by vegetarian or vegan duration, using data from Demange (2017). The two-proportions Z-test indicates a statistically significant difference between the groups at the 5% level ($p\text{-value} < 0.05$). The error bars show the 95% confidence intervals. B) The proportion of vegetarian and vegan patients ($N = 1090$) who hesitate to report symptoms to their doctor according to whether their doctor recommended meat consumption, using data from Demange (2017). The two-proportions Z-test indicates a statistically significant difference between the groups at the 5% level ($p\text{-value} < 0.05$). The error bars show the 95% confidence intervals.

Doctors' imperfect knowledge of plant-based diets has been shown to result from a lack of training in nutrition (and especially plant-based nutrition) in their curriculum (Chung et al., 2014, Crowley et al., 2019). A number of countries and groups of researchers have sought to address this issue by creating educational platforms² and tools for medical professionals and patients (Baroni et al., 2018). In France, ini-

²Italy has a dedicated platform on which patients and medical professionals can find information on plant-based diets (*Famigliaveg*). The NHS in the UK has dedicated health pages for vegetarian and vegan diets (*NHS vegan* and *NHS vegetarian*). The USDA also provides dedicated resources for vegetarian diets (*USDA vegetarian*). In France, the main source of information for patients is the government's nutritional recommendations (*PNNS*). At the time of this study, it recommends the consumption of 500 grams of meat per week and encourages vegetarians and vegans to seek information on possible deficiencies directly with their doctor.

tiatives to convey information on plant-based diets to doctors, such as the Vegeclie platform and an information booklet designed by Blanchet-Mazuel and Wissocq (2018), were the first attempts to improve the therapeutic relationship. However, the diffusion and efficiency of these kinds of tools remain to be established. In particular, we do not know the extent to which doctors are willing, even passively, to become informed.

The objective of our research is to investigate the effectiveness of informing doctors (general practitioners) of the benefits and risks of plant-based diets on their recommendations to their patients who wish to adopt such diets and their associated health monitoring. More specifically, we test the impact of an information campaign that consists of a booklet and an online platform (Vegeclie) developed by French doctors to help their colleagues inform vegetarian and vegan patients. This platform was developed in a similar way to platforms on other medical issues such as the use of antibiotics (Antibioclie) or the monitoring of pregnancy (Gestaclic). It offers general recommendations for patients who have adopted a plant-based diet for a large set of nutrients (proteins, vitamins B12 and D, iodine, omega-3, calcium, iron, and zinc) and allows doctors to obtain detailed formation for specific patient profiles (infants, young children, pregnant or breastfeeding women, and the elderly). It provides guidelines to achieve a balanced plant-based diet including supplements, based on the most recent scientific findings. The booklet, developed for the experiment, comprises the most relevant information from Vegeclie about the risks and benefits of plant-based diets.

Information campaigns can be effective tools to educate, update beliefs and induce behavioural changes, may it be in politics (Kendall et al., 2015), environmental behaviors (Carlsson et al., 2021) or health (Noar, 2006, Perloff, 1993). Specifically, health information campaigns have been proven successful to educate the general population on topics such as nutrition (Snyder, 2007), HIV (Dupas, 2011), strokes (Haesebaert et al., 2020), vaccines (Nyhan and Reifler, 2015), and more recently guidelines during the COVID-19 pandemic (Barari et al., 2020). Additionally, informing professionals about the latest scientific advances and methods, in particular doctors, can lead to better practices and updated knowledge (Friis et al., 1989). However, informing doctors about the risks and benefits of plant-based diets may have only a limited impact on their practices, for behavioural and environmental reasons. First, previous work has shown that individuals generally have a tendency to reject contradictory information due to cognitive dissonance (Rothgerber, 2014,

2020), social norms (Higgs, 2015, Cheah et al., 2020), confirmation bias (Lord et al., 1979, Nickerson, 1998, Piazza et al., 2015, Dickinson and Kakoschke, 2021), and information avoidance (Loewenstein, 2006, Golman et al., 2017, Sharot and Sunstein, 2020, Ho et al., 2021), including information related to plant-based or animal-based diets (Espinosa and Stoop, 2021). Doctors are also subject to imperfect information acquisition (Grol and Grimshaw, 2003, Francke et al., 2008), and may therefore interpret the new evidence via their own beliefs and diet, preventing the success of any information campaign. Second, most doctors are practitioners who may lack the time to keep up with the scientific literature, and may prefer to rely on dietary guidelines issued by national institutions (the ANSES in France). In their eyes, these guidelines are more informative than single scientific articles used in information campaigns, as they are based on reviews of the existing literature and should be, in this respect, more robust. However, these recommendations vary greatly across countries (Springmann et al., 2020) and often result from a trade-off between political (including economic) and scientific considerations. In addition, these recommendations are issued for a number of years, which can create delays between the production of scientific knowledge and its incorporation into dietary guidelines. Overall, doctors who prefer to stick to the national dietary guidelines might overlook recent scientific evidence and may thus be relatively insensitive to information campaigns.

To assess the impact of a plant-based nutrition information campaign on doctors' views, we run a randomized control trial, exposing a representative sample of 200 doctors to the booklet and its associated platform and comparing their attitudes to a representative control group of 200 additional doctors. We test three hypotheses that are summarized in the design table (Appendix B.1).

First, assuming that doctors are willing to become informed about plant-based nutrition, we postulate that the information campaign will improve the general opinion doctors give to their patients who are willing to shift towards plant-based diets. To test this, we define a series of questions in our survey to measure the general opinion towards plant-based diets given to the patient, which we aggregate into a *Veganism Disapproval Index* (VDI). We will test whether our information campaign reduces the VDI scores. Second, we also explore the impact of the information campaign on the doctors' medical practice. We expose the treatment group to a case study involving an asymptomatic vegan patient and examine the biological tests they prescribe to check for nutritional deficiencies. We consider eight relevant

biological tests, two of which should be prescribed and six of which are not relevant. We construct a *Proper Medical Practice Index* (PMPI) that reflects the quality of the medical practice (i.e., prescribing the relevant tests and avoiding the non-relevant tests). We will test whether our information campaign increases the PMPI scores.

Third, we also explore whether the information campaign influences doctors by making them more actively recommend a plant-based diet with an incentivized decision. We introduce the doctors to a variation of the charity giving game, in which they can give part of their earnings to an information campaign project developed by a research institute aimed at informing the general population about the importance of a well-balanced vegan diet and its expected health benefits. We analyze the share of the funds given to the research agency, which we label *Veganism Promotion Index* (VPI). We test whether our information campaign increases the share of funds given to the information campaign project.

Our results show the positive effects of our information intervention. Our confirmatory analysis shows indeed that the exposition to the booklet is very effective in improving doctors' views about plant-based diets. However, the impact on expected medical practice (i.e., biological tests prescribed to the patients) is much more limited: while we cannot rule out the possibility of a positive treatment effect, the information intervention is below our smallest effect size of interest and is not statistically larger than zero. We do not analyze the impact of the information campaign on the VPI because the associated statistical test does not have sufficient statistical power (outcome-neutral test). Our exploratory analysis does not suggest any heterogeneous effect of the intervention. One exception is the fact that the treatment effects seem to be larger for doctors who spent more time reading the booklet. However, our setup is not capable to distinguish whether this results from a selection effect (i.e., doctors that are the most adverse to plant-based diets spent less time reading the booklet) or from an attention effect (i.e., doctors that spent less time reading the booklet learned fewer things). Descriptive statistics of the questionnaire indicate that doctors are aware of the adverse effects of their negative opinions about plant-based diets on patients and tend to overestimate their knowledge of nutrition. Overall, our study shows that informing or/and training doctors through simple booklets is an effective strategy that could lead to better relationships with patients who follow a plant-based diet and could, in the long run, encourage the development of more sustainable diets. However, more intensive ac-

tions, like better training at the university, might be required to effectively improve the supervision of patients adopting a plant-based diet by doctors.

The remainder of the paper is organized as follows. First, we present our methods. We detail our experimental design, our pilot data, and our sampling and analysis plans. Second, we show the results of the randomized controlled trial. Last, we discuss the implications of our findings.

2 Methods

2.1 Ethics information

This work received the approval of the ethics committee of the *Centre de Recherche en Économie et Management* in June 2021 (IRB approval number: 010721-01). It complies with all relevant ethical regulations. Participants in the study were recruited and compensated by the polling institute. The participants were selected from the institute’s database of doctor participants and were not recruited for this specific experiment. They were randomly allocated to one of the two conditions (control vs. treatment) and received a fixed amount of money (18€) determined by the standard practice of the polling institute.

2.2 Design

Survey. The experiment is a randomized control trial with one control and one treatment condition. In the control condition, a representative sample of French doctors is contacted via the polling institute to answer a short questionnaire about their perception and views of plant-based diets (see Supplementary Materials). The two first screens contain case studies and their answers constitute our primary outcomes. In the first case study, we seek to elicit what doctors usually tell their patients when the latter desire to adopt a plant-based diet. The first case study reads as follows:

A 27-year-old man, presenting no symptoms or previous illness history, comes to you for a consultation. He currently consumes animal-based products regularly but wishes to become vegan (no red meat, no white meat, no fish, no dairy products, no eggs). He asks your opinion on the potential health risks associated with this diet.

Doctors are asked to report, on 0%-to-100% scales, how likely they would tell this patient that (i) they strongly advise against such a diet (Q1), (ii) it creates important deficiency risks (Q2), (iii) there is no problem if the diet is diversified and the patient takes food supplements (Q3), (iv) it is a bad decision that can have negative long-term consequences (Q4), and (v) it can be beneficial for his health (Q5). The items are displayed in a random order, and we ask doctors to answer as truthfully as possible to reflect what they would do in their office. We decided to consider a young male as this type of patient has the lowest risk of deficiencies. Female patients are more likely to suffer from iron deficiencies and might need specific treatment if they plan to become pregnant.

On the next screen, doctors are presented with a second case study, which is a follow-up of the first scenario:

The same patient returns after four years as a vegan (no consumption of animal-based products – meat, fish, eggs, dairy products, etc.) with no supplements and asks if he should have a blood test to look for any deficiencies. He reports eating fruit, vegetables, pulses, seaweed, and grains regularly. He does not have any symptoms.

Doctors are asked whether they would prescribe biological tests. If they answer “Yes”, they are presented with the list of ten tests displayed in Table 2.1. The tests are displayed in a random order, and doctors select the tests that they would prescribe. Of this list, only the first test is relevant for an asymptomatic vegan patient who does not take food supplements, i.e., the methylmalonic acid urine test. Vitamin B12 is indeed the only nutrient that cannot be found in a well-diversified plant-based diet so that a strictly vegan patient should suffer from deficiencies in this vitamin if they do not take supplements. The blood test for vitamin B12 has limited relevance given that the patient eats seaweed regularly, which contains B12-analogs that distort the results of the blood test. Regarding the other items, a vegan patient

with a diversified diet should not have any greater risk of deficiency than the general population. While prescribing T1 is a sign of good medical practice, the prescription of T2 to T8 is unnecessary and indicates a limited knowledge of plant-based diets.³ Tests T9 and T10 cannot be seen as good or bad medical practice and are included here only to provide the usual list of tests that doctors may be willing to prescribe.

Table 2.1: List of biological tests prescribed to an asymptomatic patient after four years of veganism

	Test name	Objective of the test
T1	Methylmalonic acid urine test	To measure the level of methylmalonic acid, which is the reflection of vitamin B12 deficiency.
T2	Phosphate, calcium, and PTH (parathormone) test	To measure levels of phosphate and calcium, important for bones, muscles, and tiredness.
T3	Vitamin B9 (folic acid test)	To measure the presence of vitamin B9, important for neuronal development, the immune system, the production of DNA, and red blood cells.
T4	Zinc test	To measure the level of zinc, which is important for cell growth, cell division, the immune system and the breakdown of carbohydrates.
T5	Albuminemia	To measure the level of albumin. Used to detect under-nutrition.
T6	Ferritinemia	To measure the level of Ferritin, which is a protein that stores iron, to detect iron deficiencies.
T7	25-hydroxy vitamin D test	To measure the level of vitamin D, which is transformed to 25-OH-D by the liver. The vitamin is useful for bone mineralization and neuromuscular activity.
T8	Thyroid-stimulating-hormone	To measure the presence of TSH, which reflects the activity of the thyroid.
T9	Vitamin B12 blood test	To measure the level of vitamin B12 and analogs in the blood.
T10	Complete Blood Count, Platelets	To measure hemoglobin level, which serves for anemia diagnosis.

³Allès et al. (2017) report the average daily nutrient intakes of the largest French cohort (Nutrinet) and compare them with the French national nutrition recommendations (ANSES). Without considering supplements, French vegans have on average a daily intake of 760 mg of calcium (recommendation: 750 mg/day), 481 µg of Vitamin B9 per (250µg/day), 10mg of Zinc per day (9.3 mg/day), 18.6 µg of iron per day (6 µg/day), and 248.8 µg of Iodine per day (150µg/day). As far as vitamin D is concerned, the deficiency risks are similar for vegans as for the general population, as sun exposure is the main source, and intakes through nutrition are very limited. However, vegans have a daily intake of 2.7 µg of Vitamin B12 per day, below the recommended intake of 4 µg/day.

On the third screen, doctors are introduced with a variation of the charity giving game. Participants receive 2€ and can decide to give some of this earning to fund an information campaign developed by a research agency (CNRS) aimed at informing the general population about the importance of a well-balanced vegan diet and its expected health benefits. Doctors can give money in increments of 0.20€. Any unallocated money is theirs to keep.

Treatment. In the treatment condition, participants are exposed to an information campaign before answering the survey. The information campaign consists of a booklet (see Supplementary Materials) created by the research team (including two doctors) and is inspired by the information available on the *Vegecliv* website that summarizes the latest scientific recommendations for doctors regarding plant-based diets. The booklet contains five pages of information: one page about vegetarianism and veganism in France, two pages about essential nutrients and the risks of deficiencies for vegetarian/vegan patients, one page about the recommendations for specific populations, and one page about the health benefits of plant-based diets. At the end of the booklet, the doctors are invited to visit the *Vegecliv* website for more information and detailed case studies. Last, we show the list of scientific references that were used to generate the booklet. Participants are told that they have 10 minutes to read the booklet and consult the *Vegecliv* website. We record the time spent on the information campaign and whether the participants consult the website.

Methodological details. The study is a between-subject RCT that randomizes treatment allocation at the individual level. As soon as a condition received 200 complete answers, the remaining participants were assigned to the other treatment to provide up to 200 complete answers. The two groups of participants are representative samples of the population of French doctors regarding gender, age, and area of practice. Participants were anonymous and received monetary compensation for their participation in the survey. The collection of data was carried out by the polling institute.

Data analysis was conducted by the research team and was therefore not blinded. We excluded from the data analysis participants from the two conditions who spent less than one and a half minutes on the survey.

Outcome variables. We consider three outcome variables to assess the effect of the campaign on doctors. We first define a score that reflects the respondents' general opinion about plant-based diets. We consider answers to the first case study (i.e., questions Q1 to Q5) and define a *Veganism Disapproval Index* (VDI) as follows:

$$\text{Veganism Disapproval Index} = \frac{Q1 + Q2 + Q3 + Q4 - Q5 + 20}{50} \in [0; 1]$$

The answers to questions Q1 to Q5 take on values between 0% and 100% in increments of 10 percentage points. The VDI, once normalized, ranges between 0 and 1. The lower the VDI score, the more positive a doctor's opinion about the healthiness and appropriateness of plant-based diets.

Second, we plan to test whether our intervention has the potential to successfully improve medical practice with patients who have adopted a plant-based diet. To do so, we construct a score of good medical practice for our case study. In the case of a patient who has adopted a diversified plant-based diet, does not take supplements, and exhibits no clinical symptoms, the only relevant test is the methylmalonic acid urine test. Tests T2 to T8 are unnecessary as the patient has no greater deficiency risk than someone who consumes animal-based foods. Tests T9⁴ and T10 are not relevant. The Proper Medical Practice Index (PMPI) therefore assigns a positive point for the urine test (T1) and negative points for the remaining tests T2 to T8. It is defined as:

$$\text{Proper Medical Practice Index} = \frac{T1 - \sum_{i=2}^8 T_i + 7}{8}$$

The PMPI takes on values between 0 and 1. Higher scores on the PMPI indicate better medical practices.

Third, we analyse the doctors' decision to give funds to the information campaign. We refer to the share of funds that doctors give to the project as the Veganism Promotion Index (VPI). Unlike the VDI, the VPI is incentivized and is thus more likely to reflect a more active involvement in the promotion of information about plant-based diets. The VPI, once normalized, ranges between 0 and 1.

2.3 Pilot data

We ran a pilot session in October 2021. The pilot session included medical students in years 7, 8, or 9 of the medical curricula from the University of Nantes. Stu-

⁴Testing for the presence of Vitamin B12 with a blood test is uninformative as the patient eats seaweed.

dents received a link via the board of students and were invited to participate in the online experiment. The pilot session was a shorter version of our experimental protocol designed to test the reliability of the three outcome variables (i.e., VDI, VPI, and PMPI) and did not include the information campaign. The pilot session was estimated to last around 5 minutes. For practical reasons, we did not incentivize the VPI. Instead, students faced a hypothetical version of the VPI (see Supplementary Materials). We did not ask for any individual data such as gender or age for confidentiality issues.

In total, we recovered 28 exploitable answers. The outcome variables are summarized in Table 2.2. The VDI scale has a satisfactory internal validity (Cronbach's $\alpha = 0.80$) and a mean below the middle of the scale, denoting a positive opinion about plant-based diets (i.e., VDI mean < 0.5). The average PMPI and donations to the information campaign are above the middle of the scale (i.e., PMPI mean > 0.5 and VPI mean > 0.5), denoting good medical practices with patients who have adopted a plant-based diet and an active promotion of well-balanced vegan diets.

The pilot data show limited risks of floor or ceiling effects for the VDI and the PMPI. No observation hits the lower or upper bound of the scales for VDI and PMPI. However, the VPI may suffer from a ceiling effect as 61% of the participants give all the money to the information campaign project while 7% give nothing. These numbers are nevertheless likely to overestimate the willingness-to-contribute of our main experiment in two ways. First, the hypothetical version of the VPI might lead participants to overstate their donations compared to an incentivized setting (stated vs. revealed preferences). Second, the pilot sample is composed of students who are generally younger and more female than the general population of doctors. Sample selection might thus lead to more vegan-friendly attitudes compared to the general population of doctors.

Table 2.2: Summary pilot session October 2021

	N	Mean	Median	St. Dev.	Min	Max	Cronbach's α
VDI	28	0.381	0.410	0.224	0.020	0.780	0.803
PMPI	28	0.625	0.625	0.220	0.125	0.875	-
VPI	28	0.764	1	0.339	0	1	-

2.4 Sampling plan

Power analysis. The sample consists of 400 observations (200 in the control group, and 200 in the treatment group). The key element for our data analysis is to define the smallest effect size of interest (SESOI) (Lakens et al., 2018, Dienes, 2020, 2021), i.e., the smallest effect below which the campaign is not seen as sufficiently effective to be worthy of interest for policy-making. We rely on previous literature to define the SESOI. For the VDI, we use previous research by Espinosa and Stoop (2021), who found a 20 percentage-point increase in the share of correct responses following a short information campaign on nutrition with short responses. The intervention in our study is longer and more complex, and we thus consider that a policy with half of this effect size (i.e., 10 percentage points) would be worth considering for policymakers. For the VPI, we rely on results comparing the impact of NGOs' discourses on donations in a charity-giving game by Espinosa and Treich (2021). The authors found a 6 percentage-point increase in donations when individuals are exposed to a two-paragraph welfarist discourse. Given that the information campaign we develop is more complex and would generate larger costs than a simple exposure to a two-paragraph discourse, we consider that a SESOI at least twice higher (i.e., 12 percentage points) would be worth considering for policymakers. For the PMPI, there is no existing literature that explores the effect of a plant-based diet information campaign on doctors' practices to our knowledge. We consider that an improvement by at least one additional test in doctors' prescriptions (either a useless test is abandoned, or an additional useful test is prescribed) would have the potential to successfully improve medical practices and would be worth considering for policymakers. We define the SESOI for the PMPI as $1/n$, where n represents the total number of tests available. The SESOI is $1/8 = 0.125$ (i.e., 12.5 percentage points). In our power analysis, we estimate the probability to reject the null hypothesis (i.e., no effect or negative effect of the information campaign) for the SESOI.

We simulate our data using normal distributions for the VDI and VPI scores, with censorship at 0 and 1. We consider a normalized binomial distribution for the PMPI. All distributions are calibrated using the means and standard deviations of the pilot data. Note that for the VPI we estimate the mean and the standard deviation of the pilot data using a Tobit model. We estimate the treatment effect

in the same way as we commit for the final data (see Section 2.5). We display the R code for the power analysis in the Supplementary Materials.

Our simulations (S=2,000) calibrated on the pilot data yield satisfactory statistical powers for the VDI and PMPI with probabilities to estimate the SESOI equal to 99.6% for the VDI and 100% for the PMPI. The statistical power is low for the VPI with a probability to estimate equal to 15.7%.

2.5 Analysis plan

The three outcomes of interest (VDI, PMPI, VPI) were generated using the two formulae set out previously. Under both conditions (control and treatment), participants who cannot see images (i.e., those who wrongfully answer the image question on screen 2) were not able to participate in the rest of the experiment and were not included in the final 400 observations.⁵ Participants who spent less than one and a half minutes on the survey were excluded from the empirical analysis. We committed to estimating the effect of the information campaign on the three outcome variables using a Tobit model to take into account the possible inflation of observations at the lower and upper limits of the scales. In case there were no such observations, we committed to estimating the treatment effect using OLS. We use a significance threshold of 0.05 but we apply a Bonferroni correction for multiple hypothesis testing and retain $p=0.0167$ as a significance threshold (three outcome variables, $\alpha=0.05$).

We set up outcome-neutral tests to anticipate potential ceiling or floor effects. Prior to hypothesis testing, we committed to running the above power analysis again using data from the control group. We committed to analysing only the outcome variables for which we are able to estimate the SESOI with a probability of 80 percentage points or higher (see Section 2.4).

We committed to perform hypothesis testing for the PMPI and VPI as presented in Figure 2.2 (for which we expect an increase in the scores). Hypothesis testing for the VDI is reverted (for which we expect the campaign to decrease the score). Using the estimates of the Tobit (or OLS) estimation, we perform unilateral hypothesis testing to determine the impact of the information campaign. First, we test whether

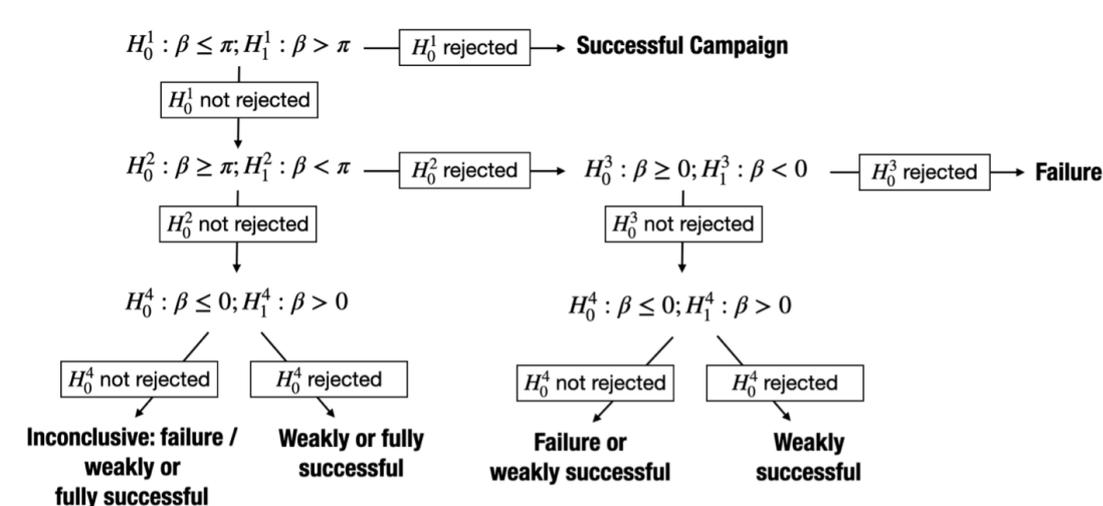
⁵This exclusion condition was added after the Stage-1 manuscript was accepted but was approved by the recommender before data collection.

the treatment effect is smaller than the SESOI (i.e., $H_0^1 : \beta \leq \pi$). If we reject H_0^1 , we conclude that the information campaign is successful ($\beta > \pi$). If we are unable to reject H_0^1 , we test the reverse hypothesis, i.e., whether the treatment effect is larger than the SESOI (i.e., $H_0^2 : \beta \geq \pi$).

On the one hand, if we reject H_0^2 , we have confirmation that the treatment effect is smaller than the SESOI ($\beta < \pi$). We then test whether the treatment effect is greater than zero (i.e., $H_0^3 : \beta \geq 0$). If we reject H_0^3 , we conclude that the information campaign is a failure as the treatment effect is negative ($\beta < 0$). If we are not able to reject H_0^3 , we test the reverse hypothesis, i.e., whether the treatment effect is lower than zero (i.e., $H_0^4 : \beta \leq 0$). If we reject H_0^4 , we conclude that the information campaign is weakly successful as the treatment effect is located between zero and the SESOI ($\pi > \beta > 0$). If we are unable to reject H_0^4 , we are not able to distinguish between a failure and a weakly successful information campaign as the treatment effect is lower than the SESOI but could be positive or negative.

On the other hand, if we are unable to reject H_0^2 , we are not able to conclude whether the treatment effect is greater or lower than the SESOI ($\beta \leq \pi$ or $\beta \geq \pi$). We then test whether the treatment effect is negative (i.e., $H_0^4 : \beta \leq 0$). If we reject H_0^4 , we conclude that the information campaign is either weakly or fully successful as the treatment effect is positive but could be greater or lower than the SESOI ($\beta > 0$ but $\beta \leq \pi$ or $\beta \geq \pi$). If we are unable to reject H_0^4 , our results are inconclusive as the treatment effect could be positive or negative, greater or smaller than the SESOI ($\beta \leq 0$ or $\beta \geq 0$ and $\beta \leq \pi$ or $\beta \geq \pi$).

Figure 2.2: Hypothesis testing for the VPI and PMPI and interpretation of possible results.



Note: unilateral hypothesis testing using the marginal effects estimates β from the regression and π the smallest effect size of interest. The hypothesis testing is set up for the PMPI and VPI for which we expect the information campaign to increase the scores. The hypothesis testing for the VDI, for which we expect the information campaign to decrease the score, is reverted. In this case, π is negative.

3 Results

The experiment took place in June and July 2022. Data collection happened as originally planned with no deviation from the pre-registration. In total, 400 doctors took part in the experiment (200 in the control group, 200 in the treatment condition). All participants spent more than 90 seconds on the survey. Among the 400 doctors included in the data analysis, 87% are private practitioners (87.5% in the control group vs. 86.5% in the treated group), 44.3% are female (44% vs. 45.5%), and 50% live in an urban area with 100,000 inhabitants or more (55% vs. 44.5%). About 27% of the participants are below 40 years old (27% vs. 27%), and 32.8% are 60 years old or older (34% vs. 31.5%).

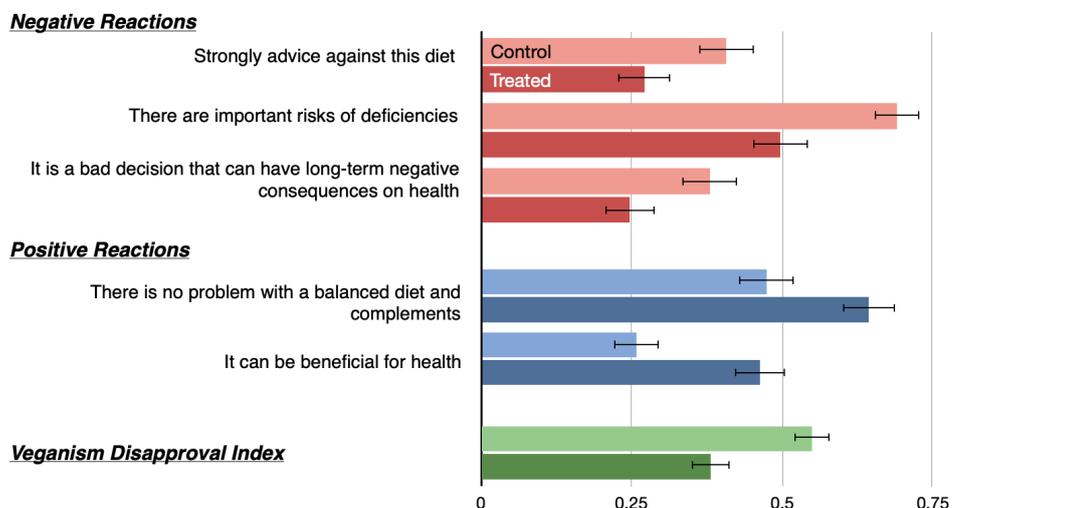
As planned, we reproduced the power analysis using the control group data rather than the pilot data. We obtain a 99.7% chance of detecting an effect size equal to the SESOI for the VDI, 100% for the PMPI, and only 8.1% for the VPI. Following our pre-registration, we do not analyze the VPI as we lack statistical power due to the large share of zeros (49% of the decisions in the control group). Although we

test only two hypotheses, we did not explicitly mention that we would adapt our Bonferroni adjustment accordingly, so we retain $p=0.0167$ as a significance threshold ($\alpha=0.05$). Following our pre-registered procedure, we use Tobit estimations for all analyses as we have values at the upper or/and lower bounds of the scales.

3.1 Confirmatory analyses

First, we observe that the treatment significantly affects the advice doctors would give to the patient willing to adopt a plant-based diet. Figure 2.3 shows indeed that the treatment significantly decreases veganism disapproval by doctors. The average VDI score reduces from 0.550 (SD=0.214) in the control condition to 0.382 in the treatment group (SD=0.226). The estimated marginal effect of the treatment using a Tobit regression model is equal to -0.166 (SE=0.022), which is significantly smaller than the pre-registered SESOI of -0.10 (one-sided test: $p=0.001$). According to the pre-registered decision rule, the information intervention can be considered as successful.

Figure 2.3: Differences in the advice given to a 27 years-old male patient willing to adopt a plant-based diet.

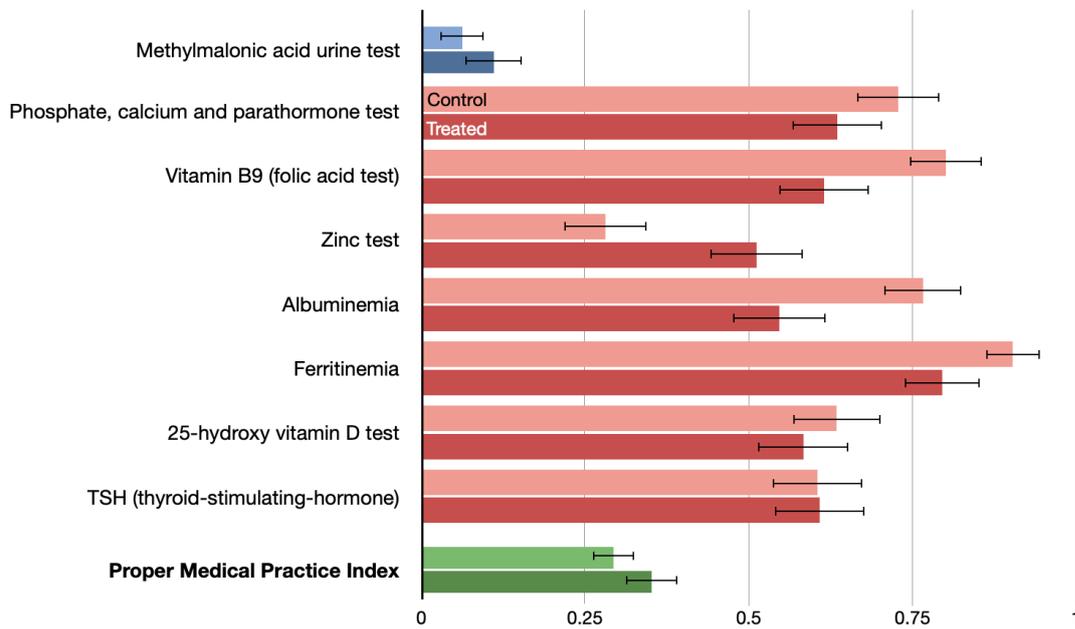


Note: bars represent averages, and spikes represent 95% confidence intervals. N=200 in the control condition, N=200 in the treatment condition.

Second, we see that the treatment also affects the medical practice, i.e., the biological tests that the doctors would prescribe to the vegan patient. The average PMPI score increases from 0.293 (SD=0.228) in the control group to 0.348

(SD=0.273) in the treatment condition (see Figure 2.4). The estimated marginal effect of the treatment is equal to 0.044 (SE=0.024), which is lower than the pre-registered SESOI of 0.125. The treatment effect is not statistically different from zero given our Bonferroni adjustment (one-sided test: $p=0.0343$). Following our pre-specified decision rule, we can conclude that the information intervention is either weakly successful or fails at improving medical practice.

Figure 2.4: Differences in biological tests doctors would prescribe to a 31 years-old male patient who has been vegan for four years without taking supplements and who has no symptoms.



Note: bars represent averages, and spikes represent 95% confidence intervals. N=200 in the control condition, N=200 in the treatment condition.

3.2 Exploratory analyses

First, our results show that the information intervention successfully impacts doctors' (hypothetical) advice about plant-based diets. We see that the information intervention increases all positive reactions (the diet can be beneficial for health, there is no problem if the diet is well balanced and the patient is supplemented) and mitigate negative reactions (strongly advice against this diet, there are important risks of deficiencies, it is a bad decision with negative long-term consequences on

health). Means, standard deviations, marginal effects, standard errors, and p-values for two-sided tests are reported in Table B.1.

Second, the information intervention has however a limited or even null impact on (hypothetical) medical prescriptions. Exploratory analyses suggest that the intervention increases the prescription of T1 and decreases the prescription of several other tests (T2, T3, T5, T6). The intervention seems to have limited or no impact on T7 or on T8. More surprisingly, the intervention seems to increase the prescription rate for the Zinc test (T4). In the control group, 28% of the doctors ask for a Zinc test while 51% of them do so in the treatment group. It suggests that our booklet can be improved regarding the information provided for Zinc. In the experiment, we mentioned that Zinc concentration levels tend to be smaller for vegan patients than for the general population but that there is no significant clinical impact. Doctors might have retained that Zinc levels can be smaller and might have preferred to test for it. When we drop the Zinc variable, the estimated marginal treatment effect on PMPI (scaled between 0 and 1 in both cases) goes from 0.044 to 0.096. In this case, the one-sided p-value decreases to less than 0.001. This suggests that the lack of results is likely to be due to the Zinc effect.

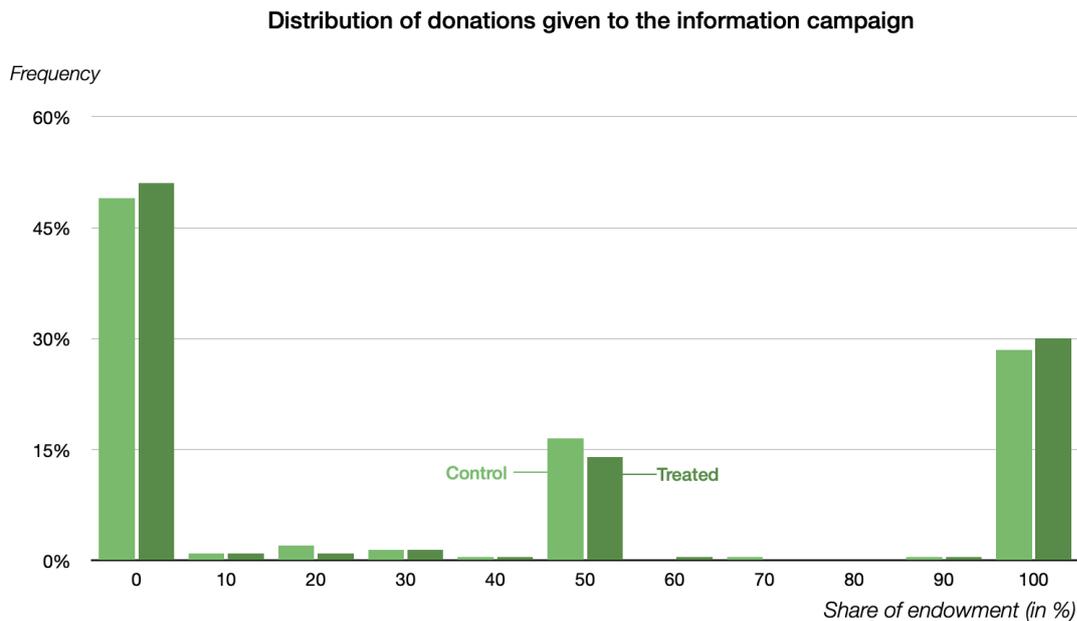
Third, we then explore whether the treatment has a heterogeneous effect on the doctor population. We run a series of Tobit regressions in which we interact the treatment effect with (i) a dummy variable for female doctors, (ii) a dummy variable for doctors living in urban areas with 100,000 inhabitants or more, or (iii) a dummy variable for doctors who are strictly younger than 50 years old. We also include the dummy variable in the regression. Results are reported in Tables B.2 and B.3. We do not detect any significant association for the VDI or for the PMPI scores.

Fourth, the treatment effect might also depend on the attention devoted to the booklet. The median doctor in the treatment group spent 3.6 minutes looking at the information booklet, while the most rapid doctor devoted only 22 seconds to our information campaign. To explore the sensitivity of our results to this issue, we run the above statistical analysis by considering only participants who spent at least 90 seconds on the information booklet in the treatment group (21 observations excluded). In this case, the estimated marginal treatment effects become larger both for the VDI (from -0.166 to -0.173) and for the PMPI (from 0.044 to 0.054). The conclusions remain the same for the VDI, and the intervention becomes weakly successful for the PMPI if we follow our decision rule. The larger effects obtained by excluding the most rapid participants might either result from larger attention

devoted to the information campaign or/and from a selection effect (i.e., doctors who are the least friendly to plant-based diets spend the least time on the booklet).

Fifth, our experiment contained the Veganism Promotion Index (VPI), which aimed at capturing more active behaviors in favor of the promotion of a plant-based diet. We did not analyze the treatment effect on the VPI in the confirmatory analysis because it did not pass our outcome-neutral test (i.e., low statistical power). The lack of statistical power is mostly driven by the very large share of observations at the boundaries of the scale, e.g., doctors who do not give anything to the information campaign (49% in the control group, 51% in the treatment group). The average donation score is the same in both conditions (mean: 38.7% of endowment). Figure 2.5 shows that the distribution is three-modal with peaks at 0%, 100%, and 50% (in decreasing order of prevalence). While the VPI does not seem to be affected by the treatment, we observe however a strong correlation with the VDI. A Tobit regression indicates that going from 0 to 100% in the VDI score decreases donations by 46.5 percentage points ($p < 0.001$).

Figure 2.5: Differences in VPI scores.

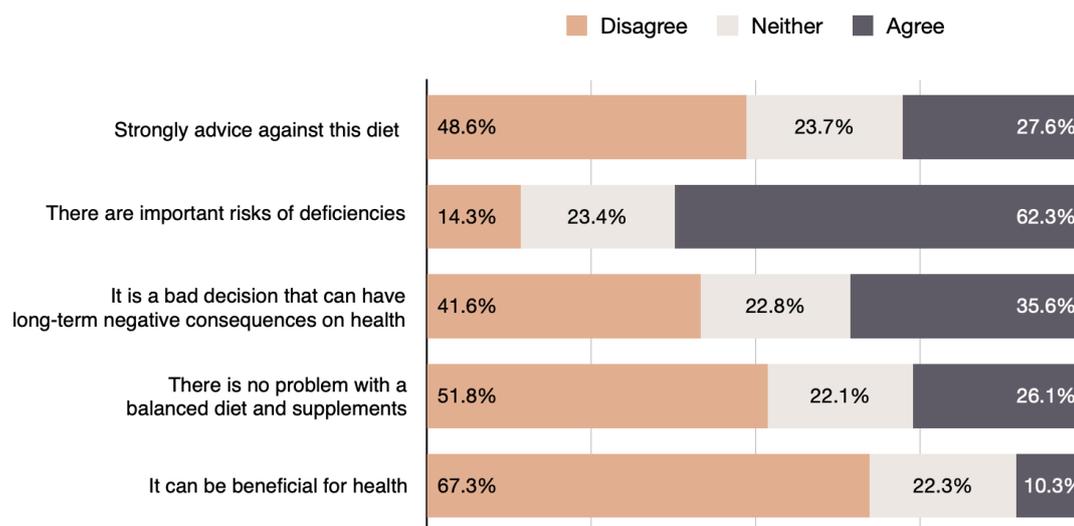


Note: bars represent frequencies in the data. N=200 in the control condition, N=200 in the treatment condition.

Sixth, we document the average view among the general population of doctors about plant-based diets by using data from the control group. We recode the answers

to the items of the VDI. We consider that doctors who give scores between 0 and 3 disagree with the statement, and those who give scores between 7 and 10 agree with the statement. We consider that doctors who report answers between 4 and 6 neither agree nor disagree with the statement. We weigh answers to get a representative sample of the doctor population. Figure 2.6 reports the VDI-recoded answers for the control group. We observe that most doctors in the population highlight large risks of deficiencies (62.3%) when the patient tells them about his plans to go vegan. Doctors are also very unlikely to tell the patient that a vegan diet could be beneficial for health (10.3%). Only one out of four doctors reports that there is no problem with a balanced vegan diet with supplements (26.1%). However, and surprisingly, only one out of four doctors strongly advise against this diet (27.6%), which suggests that most doctors would not express all their concerns to the patient.

Figure 2.6: Advice given to a 27 years-old male patient willing to adopt a plant-based diet.

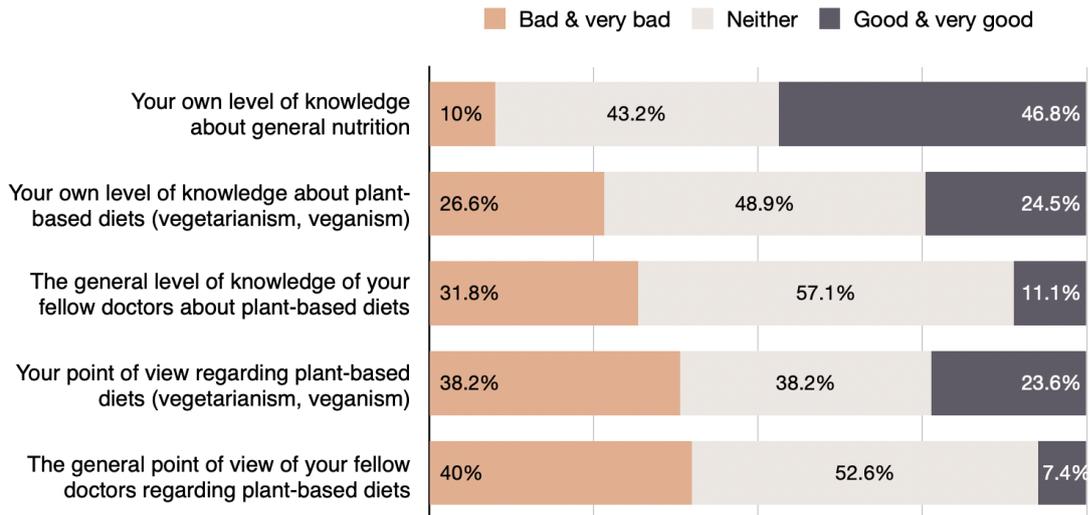


Note: weighted averages for representativeness. Control group only (N=200).

Last, exploratory analysis of the control group only suggests that doctors are aware of the issues associated with plant-based diets and medical practice. First, Figure 2.7 below shows that only 24.5% of French doctors recognize to have good knowledge of plant-based diets. When we ask about other doctors, only 11.1% of the French doctors declare that their colleagues are sufficiently knowledgeable on the topic. This suggests that doctors might have on average an overconfidence bias, i.e., considering that they know, on average, more than their colleagues about plant-

based diets. In addition, half of the doctors say that they have good knowledge about diets but medical schools in France devote very little time to the topic. So, doctors might either actively get informed on their own initiative on the topic (but they get informed less on plant-based diets), or they might overestimate their knowledge on the issue.

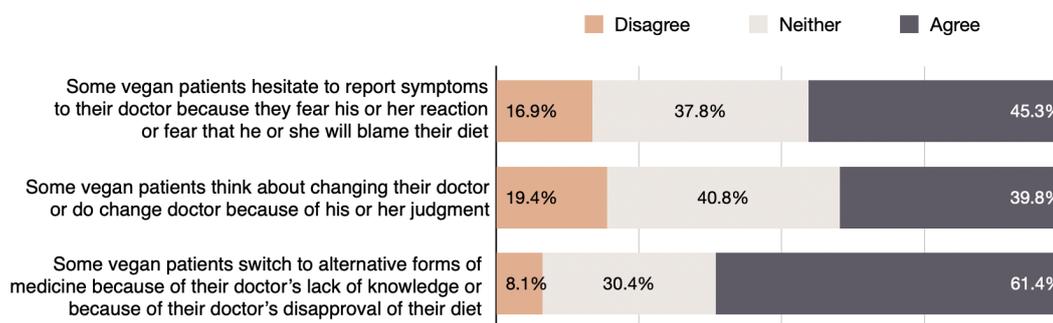
Figure 2.7: Self-reported knowledge about diets and plant-based diets by French doctors.



Note: weighted averages for representativeness. Control group only (N=200).

Next, doctors seem aware of the fact that doctors' opinions on plant-based diets might have negative consequences for patients. Figure 2.8 shows that most doctors (61.4%) consider that the doctors' lack of knowledge or disapproval of one's vegan diet might lead patients to switch to alternative forms of medicine. Only a small minority of doctors do not think that a vegan patient could hide their symptoms (16.9%) or change doctors (19.4%) because of their reaction to his/her plant-based diet.

Figure 2.8: Doctors' perception of the negative consequences of the doctors' negative views on plant-based diets on the patient-doctor relationship.



Note: weighted averages for representativeness. Control group only (N=200).

4 Discussion

Our study shows that doctors hold on average negative views about plant-based diets. While recent scientific evidence suggests that vegan diets could substantially decrease mortality rates, only a small share of doctors tend to think that a vegan diet can be beneficial for health. On the contrary, strong opinions against vegan diets persist, especially regarding the risks of deficiencies. More particularly, our results show that doctors are still largely concerned with the potential lack of iron in plant-based diets. These results show that there is room for better informing and training doctors with the latest scientific recommendations on plant-based diets.

Our information intervention aims to fill this gap by giving precise information about the risks and benefits of plant-based diets. We find strong evidence that our information booklet successfully impacted doctors' views about vegan diets, and the estimated treatment effect resulting from our confirmatory analysis is 66% larger than the pre-registered SESOI. Doctors feel reassured about the risks of deficiencies, and the long-term consequences on health, and are more likely to see benefits for health. The success of the information intervention suggests that the current opposition to plant-based diets is likely to result from a lack of information rather than a strong bias against this diet. Nevertheless, we find limited empirical support for the impact of our booklet on doctors' (hypothetical) medical practice with vegan patients (i.e., the estimated effect is positive, but it is below the pre-specified SESOI and is not statistically different larger than zero). The exploratory analyses suggest that not all tests might have been affected in the same way by our information

treatment, as the prescription of Zinc tests increases after exposure to the treatment. However, the exploratory analyses also suggest that the most frequently prescribed tests (Ferritinemia, Vitamin B9, and Albuminemia) are significantly affected by the treatment. Altogether, we believe that these results show that there is some room to improve the doctors' medical practice with patients adopting a plant-based diet, but that it might require more elaborate training than a simple booklet. In our view, it highlights the need for training on plant-based nutrition at the university.

The results of the study are positive and show that the booklet has the potential to improve doctors' opinions and, to a smaller extent, medical practices with vegan patients. Our sample is representative so that the results are generalizable to the entire population of French doctors. Further studies are required to assess the effectiveness of the conveyed information. The study is based on case studies, thus reflecting stated rather than revealed preferences, and may therefore not reflect doctors' actions in real practice. Responding to case studies involves less responsibility for doctors, allowing them to express more sympathetic views and adopt riskier behaviors without consequences. Future studies should focus on the impact of the booklet on real life cases as well as the long-term effect of the information on medical practices.

Our intervention seems to have an effect on the average of those observed by other studies on medical information campaigns. Giguère et al. (2020) provide a systematic review of the impact of printed educational materials on medical practices and healthcare outcomes. The authors report a mean Cohen's *d* of 0.41 across a range of studies (Cohen's *d* range from 0.04 to 0.79).⁶ We find that our booklet outperforms previous studies on the VDI (Cohen's *d* = 0.71) and performs below average for the PMPI (Cohen's *d* = 0.22; Cohen's *d* = 0.35 when Zinc is removed). Due to the heterogeneity of practitioners, practices, and information, it is difficult to compare effect sizes across studies. Nevertheless, our booklet synthesizes information, requires a minimal cost of time and money for doctors, and is tailored to the specific needs of doctors, which seem to be the most important factors to convey information in the medical field (Dawes and Sampson, 2003, Marriott et al., 2000).

Our study suggests that information campaigns can be an efficient but not sufficient way to favor the transition toward a more sustainable food system. On the

⁶Their findings only apply to comparisons between printed educational materials versus no intervention. Our intervention slightly differs as we provide a computerized booklet.

one side, the booklet significantly improves the views on plant-based diets with relatively small costs as some doctors and experts already gather scientific evidence about the risks and benefits of plant-based diets on a regular basis. Conveying this information can be an effective way to improve the doctors' reactions to patients willing to adopt a plant-based diet. This, in turn, could reduce the risks of patients hiding symptoms or switching to alternative medicine. On the other side, information campaigns might not be enough to help doctors deal with plant-based diets. While it is true that they yield short-term large benefits (i.e., doctors rapidly change their minds after a simple exposure to the booklet), we observe more modest effects (if any) on medical practice. The long-term success of the transition towards more plant-based diets highly depends on the capacity to effectively monitor the health of patients willing to switch. In addition, the long-term benefits of the intervention on the doctors' opinions about plant-based diets might be smaller than what we report here due to information decay or to an experimental demand effect, although we can also anticipate some form of information spillovers across colleagues which could augment the benefits of such interventions. More formal and detailed training at the university could help doctors better monitor these patients. These remain open questions.

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Appendix B

Removing barriers to plant-based diets: assisting doctors with vegan patients

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B.1 Design table

Question	Hypothesis	Sampling plan (e.g. power analysis)	Analysis Plan	Rationale for deciding the sensitivity of the test confirming or disconfirming the hypothesis	Interpretation given to different outcomes	Theory that could be shown wrong by the outcomes
Does providing information to doctors decrease their aversion towards plant-based diets?	Tests on VDI: $H_0^1: \beta \geq \pi; H_1^1: \beta < \pi$ $H_0^2: \beta \leq \pi; H_1^2: \beta > \pi$ $H_0^3: \beta \geq 0; H_1^3: \beta < 0$ $H_0^4: \beta \leq 0; H_1^4: \beta > 0$	200 observations in each condition. Alpha=1.67% (Benchmark of 5% with Bonferroni correction for three hypotheses.) Smallest effect size of interest of 0.1 or above.	Outcome neutral tests: based on data of the control group, we can estimate an effect size of 0.1 with probability 80% or larger. Tobit model estimation if observations at the upper or lower bound of the scale (i.e., at 0 or 1); OLS estimation otherwise. Unilateral tests. Exclusion rule: participants who spend less than 1min30sec on the survey in both conditions.	Simulations based on pilot data (S = 2,000) demonstrate that the smallest effect size of interest of 0.1 can be detected with a probability of 99.6%.	If H_0^1 rejected: successful campaign. If H_0^1 not rejected, H_0^2 and H_0^3 rejected: failure. If H_0^1 and H_0^3 not rejected, H_0^2 and H_0^4 rejected: weakly successful. If H_0^1, H_0^3 and H_0^4 not rejected, H_0^2 rejected: failure or weakly successful. If H_0^1, H_0^2 and H_0^4 not rejected: inconclusive. If H_0^1 and H_0^2 not rejected, H_0^4 rejected: weakly or fully successful.	Non-applicable

<p>Does providing information to doctors modify their medical practices with vegan patients?</p>	<p>Test on PMPI: $H_0^1: \beta \leq \pi; H_1^1: \beta > \pi$ $H_0^2: \beta \geq \pi; H_1^2: \beta < \pi$ $H_0^3: \beta \leq 0; H_1^3: \beta > 0$ $H_0^4: \beta \geq 0; H_1^4: \beta < 0$</p>	<p>200 observations in each condition. Alpha=1.67% (Benchmark of 5% with Bonferroni correction for three hypotheses.) Smallest effect size of interest of 0.125 or above.</p>	<p>Outcome neutral tests: based on data of the control group, we can estimate an effect size of 0.125 with probability 80% or larger. Tobit model estimation if observations at the upper or lower bound of the scale (i.e., at 0 or 1); OLS estimation otherwise. Unilateral test. Exclusion rule: participants who spend less than 1min30sec on the survey in both conditions.</p>	<p>Simulations based on pilot data (S = 2,000) demonstrate that the smallest effect size of interest of 0.125 can be detected with a probability of 100%.</p>	<p>If H_0^1 rejected: successful campaign. If H_0^1 not rejected, H_0^2 and H_0^3 rejected: failure. If H_0^1 and H_0^3 not rejected, H_0^2 and H_0^4 rejected: weakly successful. If H_0^1, H_0^3 and H_0^4 not rejected, H_0^2 rejected: failure or weakly successful. If H_0^1, H_0^2 and H_0^4 not rejected: inconclusive. If H_0^1 and H_0^2 not rejected, H_0^4 rejected: weakly or fully successful.</p>	
<p>Does providing information to doctors increase their willingness to promote plant-based diets?</p>	<p>Test on VPI: $H_0^1: \beta \leq \pi; H_1^1: \beta > \pi$ $H_0^2: \beta \geq \pi; H_1^2: \beta < \pi$ $H_0^3: \beta \leq 0; H_1^3: \beta > 0$ $H_0^4: \beta \geq 0; H_1^4: \beta < 0$</p>	<p>200 observations in each condition. Alpha=1.67% (Benchmark of 5% with Bonferroni correction for three hypotheses.)</p>	<p>Outcome neutral tests: based on data of the control group, we can estimate an effect size of 0.12 with probability 80% or larger.</p>	<p>Simulations based on pilot data (S = 2,000) demonstrate that the smallest effect size of interest of 0.12 can be detected with a probability of 15.7%.</p>	<p>If H_0^1 rejected: successful campaign. If H_0^1 not rejected, H_0^2 and H_0^3 rejected: failure. If H_0^1 and H_0^3 not rejected,</p>	

		Smallest effect size of interest of 0.12 or above.	<p>Tobit model estimation if observations at the upper or lower bound of the scale (i.e., at 0 or 1); OLS estimation otherwise.</p> <p>Unilateral test.</p> <p>Exclusion rule: participants who spend less than 1min30sec on the survey in both conditions.</p>		<p>H_0^2 and H_0^4 rejected: weakly successful.</p> <p>If H_0^1, H_0^3 and H_0^4 not rejected, H_0^2 rejected: failure or weakly successful.</p> <p>If H_0^1, H_0^2 and H_0^4 not rejected: inconclusive.</p> <p>If H_0^1 and H_0^2 not rejected, H_0^4 rejected: weakly or fully successful.</p>	
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B.2 Additional tables

Table B.1: Mean, standard deviation, and marginal effects for the VDI and PMPI.

	Control group		Treated group		Marginal effects		
	Mean	SD	Mean	SD	Estimate	SE	p-values
VDI total	0.550	0.214	0.382	0.226	-0.166	0.021	<0.001
VDI 1	0.409	0.328	0.269	0.308	-0.142	0.032	<0.001
VDI 2	0.691	0.273	0.501	0.325	-0.185	0.030	<0.001
VDI 3	0.382	0.326	0.247	0.297	-0.125	0.031	<0.001
VDI 4	0.473	0.330	0.644	0.306	0.174	0.032	<0.001
VDI 5	0.259	0.271	0.464	0.301	0.213	0.029	<0.001
PMPI total	0.293	0.228	0.348	0.273	0.044	0.024	0.069
T1	0.060	0.238	0.110	0.315	0.050	0.028	0.073
T2	0.730	0.445	0.645	0.480	-0.085	0.046	0.067
T3	0.800	0.401	0.625	0.485	-0.175	0.044	<0.001
T4	0.280	0.450	0.500	0.501	0.220	0.048	<0.001
T5	0.765	0.425	0.560	0.498	-0.205	0.046	<0.001
T6	0.900	0.301	0.800	0.401	-0.100	0.035	0.005
T7	0.635	0.483	0.590	0.493	-0.045	0.049	0.357
T8	0.605	0.490	0.610	0.489	0.005	0.049	0.919
PMPI no zinc	0.232	0.242	0.326	0.282	0.096	0.026	<0.001

Note: the marginal effects for the VDI total, VDI items 1 through 5, PMPI total, and PMPI no zinc are estimated using a Tobit regression. The marginal effects for T1 through T8 are estimated using an OLS regression. The p-values correspond to a two-sided test where the null hypothesis is that the marginal effect is equal to zero.

Table B.2: Heterogenous treatment effects on VDI using a Tobit regression

	(1)	(2)	(3)	(4)	(5)
Treatment	-0.171*** (0.023)	-0.156*** (0.030)	-0.156*** (0.032)	-0.190*** (0.030)	-0.161*** (0.043)
Female		0.005 (0.032)			0.005 (0.032)
Treatment \times Female		-0.035 (0.045)			-0.033 (0.045)
Urban area			0.007 (0.032)		0.002 (0.032)
Treatment \times Urban area			-0.032 (0.045)		-0.030 (0.045)
Age below 49				-0.070* (0.032)	-0.070* (0.032)
Treatment \times Age below 49				0.042 (0.045)	0.041 (0.045)
Log Likelihood	13.204	13.643	13.546	16.021	16.806
Num. obs.	400	400	400	400	400
Left-censored	5	5	5	5	5
Uncensored	389	389	389	389	389
Right-censored	6	6	6	6	6

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The figures here are the estimated coefficients, with standard errors in parentheses.

Table B.3: Heterogenous treatment effects on PMPI using a Tobit regression

	(1)	(2)	(3)	(4)	(5)
Treatment	0.052 (0.029)	0.067 (0.038)	0.080* (0.041)	0.054 (0.038)	0.098 (0.055)
Female		0.001 (0.041)			0.003 (0.040)
Treatment × Female		−0.033 (0.057)			−0.036 (0.057)
Urban area			0.072 (0.041)		0.074 (0.040)
Treatment × Urban area			−0.044 (0.057)		−0.043 (0.057)
Age below 49				0.022 (0.041)	0.027 (0.041)
Treatment × Age below 49				−0.003 (0.058)	−0.004 (0.057)
Log Likelihood	−111.802	−111.493	−110.004	−111.557	−109.306
Num. obs.	400	400	400	400	400
Left-censored	53	53	53	53	53
Uncensored	347	347	347	347	347
Right-censored	0	0	0	0	0

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The figures here are the estimated coefficients, with standard errors in parentheses.

B.3 Supplementary Materials

B.3.1 Survey

Questionnaire – Translated version (original version in French)

Introduction screen #1

Dear Doctor,

We are reaching out to you to participate in a CNRS research project conducted by the BVA institute.

The objective of the research project is to better understand the practices of doctors with their patients with respect to nutritional issues. In this survey, you will face case studies. It is important that you respond as sincerely as possible so as to reflect what you would practice in your office with your patients.

If you fully answer the following survey, you will receive a fix fee of 18€ and possibly a variable fee based on your decisions.

FOR YOUR INFORMATION:

- According to the data protection laws, all the answers from this study will be confidential and anonymized. The identity of the respondents is strictly confidential: none of your personal information will be given to the leading research team.
- Individual responses will be aggregated in order to obtain a global vision of the topics in this study. This study has no commercial target: your information will neither be sold nor used for direct marketing purposes.
- According to the General Regulation on Data Protection (RGPD) UE 2016/679 from April 27th 2016, we inform you that we only collect and treat personal data that are indispensable to the study and to the accounting treatment of you compensation. Collected personal data from this study will be kept for duration of the study and then deleted. As so, we will not be able to identify your personal information if you require it once the study is over. Please be assured that we do all that is possible to maintain the security and confidentiality of your personal data.
- According to regulatory provisions, a statement will be given to the National Medical Order.
- As part of this study, we may present you documents that you are asked not to communicate nor diffuse (no screenshots).

On this basis, do you accept to participate to this study?

Yes

No

Introduction screen #2¹

In this survey, images may be displayed. To ensure that you can see the images, please answer the following question:

What vehicle is displayed on the image below?



- A plane
- A car
- A bike
- A train
- I do not see the image

¹ This screen was added after the Stage 1 manuscript was accepted but was approved by the recommender before data collection.

APPENDIX B

Introduction screen #3

To start, as a doctor, could you please confirm your professional status? You practice as ...

1. Exclusively liberal
2. Exclusively salaried
3. Liberal and salaried (mixed activity)

You are...

1. A man
2. A woman

How old are you?

1. Less than 40 years old
2. 40 to 49 years old
3. 50 to 59 years old
4. 60 years old and older

Please indicate your postal code and the name of the city in which you practice.

This data will allow to determine the region and size of the urban area in which you practice. This data will solely be used for statistical purposes. We remind you that all collected data is confidential and anonymous.

Please enter your postal code

/ _____ Dropping list of cities according to postal code _____ /

Currently, you practice ...

1. Group practice
2. Pluri-professional group practice (doctors, nurses, physical therapists,...)
3. Individual practice

Out of a 100 of your patients, approximately how many belong to the following categories?

Number between 0 and 100

a) Infants from 0 to 6 months	/___/
b) Children from 7 months to 15 years old	/___/
c) Adults from 16 to 49 years old	/___/
d) Adults from 50 to 69 years old	/___/
Sum	/___/

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Treatment screen #1 – *Visible only for participants in the treated condition*

Before moving on to the questions, please take some time to read carefully the brochure on the following pages. You have 10 mins to consult the brochure.

1. Treatment - page 1
2. Treatment - page 2
3. Treatment - page 3
4. Treatment - page 4
5. Treatment - page 5
6. Treatment - page 6
7. Treatment - page 7
8. Treatment - page 8

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Screen #1:

We present here a case study. We invite you to respond as sincerely as possible so as to reflect what you would practice in your office.

A 27-year-old man, presenting no symptoms or previous illness history, comes to you for a consultation. He currently consumes animal-based products regularly but wishes to become vegan (no red meat, white meat, fish, dairy products or eggs). He asks your opinion about the potential health-risks associated with this diet.

How likely are you to give them the following advice?

Knowing that 0% = No not at all and 100% = Yes absolutely, with intermediary choices allowing you to express balanced views. ²

	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
Advise against this diet.	<input type="radio"/>										
Explain to him that there are significant risks of nutritional deficiency.	<input type="radio"/>										
Explain to him that there are no problems as long as he eats a well-balanced	<input type="radio"/>										
Tell him that there are health benefits.	<input type="radio"/>										
Tell him that it is a bad decision that could have serious consequences for his health in the long term.	<input type="radio"/>										

² The terms "Strongly disagree" and "Strongly agree" contained in the Stage 1 manuscript were replaced by "No, not at all" and "Yes absolutely" respectively. These changes were approved by the recommender before data collection.

Screen #2:

The same patient returns after four years as a vegan (no consumption of animal-based products – meat, fish, eggs, dairy products etc.) with no supplements and ask if he should have a blood test to look for any deficiencies. He reports eating fruit, vegetables, pulses, seaweed and grains regularly. He does not have any particular symptoms.

Would you prescribe biological tests?

- Yes
- No

If yes, what tests would you prescribe?

- Methylmalonic acid urine test
- Phosphate, calcium and parathormone test
- Vitamin B9 (folic acid test)
- Zinc test
- Albuminemia
- Ferritinemia
- 25-hydroxy vitamin D test
- TSH (thyroid-stimulating-hormone)
- Vitamin B12 blood test
- Complete blood count, platelets
- Other

APPENDIX B

Screen #3:

If you fully complete the study, you will receive a financial compensation of 18€.

We can give you an additional 2€ that will be added to the fix fee.

Nevertheless, you can decide to give a portion or the full 2€ to develop an **information campaign aiming to promote vegan diets to the general population**. Your contribution would help to increase the impact of the information campaign that will be put in place in a near future.

This campaign, developed by the CNRS and doctors, would emphasize the importance of a well-balanced vegan diet and the expected health benefits from such diet. Any unallocated money is yours to keep. For example, this campaign could be distributed as a brochure or poster.

If you decide to give a portion or the full 2€, the amount that you indicated will be automatically given to the CNRS to implement this campaign.

Any unallocated amount will be yours to keep and will be added to the 18€ fix fee that you will receive if you fully answer the survey.

Once you have answered this question, you will not be able to modify your answer and your final fee will take into account your decision if you fully answer the survey.

Out of the €2 received, how much would you like to donate to the information campaign project?

You must choose an amount between €0 and €2. The amounts are in increments of €0.20.

€0	€0.20	€0.40	€0.60	€0.80	€1	€1.20	€1.40	€1.60	€1.80	€2
<input type="radio"/>										

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Screen #4:

How would you rate your own **level of knowledge** about general nutrition?

Knowing that 0 = very poor and 10 = very good, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

How would you rate your own **level of knowledge** about plant-based diets (vegetarianism, veganism)?

Knowing that 0 = very poor and 10 = very good, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

What is, in your opinion, the **general level of knowledge** of your fellow doctors about plant-based diets?

Knowing that 0 = very poor and 10 = very good, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

What is your **point of view** regarding plant-based diets (vegetarianism, veganism)?

Knowing that 0 = very negative and 10 = very positive, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

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What is, in your opinion, the **general point of view** of your fellow doctors regarding plant-based diets?

Knowing that 0 = very negative and 10 = very positive, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
<input type="radio"/>										

Do you believe that the medical curriculum should be reinforced in terms of nutrition?

Knowing that 0 = No not at all and 10 = Yes absolutely, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
<input type="radio"/>										

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Screen #5:

Do you believe that some vegan patients hesitate to report symptoms to their doctor because they fear his or her reaction or fear that he or she will blame their diet?

Knowing that 0 = No not at all and 10 = Yes absolutely, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

Do you believe that some vegan patients think about changing their doctor or do change doctor because of his or her judgement?

Knowing that 0 = No not at all and 10 = Yes absolutely, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

Do you believe that some vegan patients switch to alternative forms of medicine because of their doctor's lack of knowledge or because of their doctor's disapproval of their diet?

Knowing that 0 = No not at all and 10 = Yes absolutely, with intermediary choices allowing you to express balanced views.

0	1	2	3	4	5	6	7	8	9	10
0	0	0	0	0	0	0	0	0	0	0

APPENDIX B

Screen #6:

What is your opinion regarding the level of consumption by the French population of the following foods? From Strongly insufficient (*i.e., they eat too little of this food*) to Strongly excessive (*i.e., they eat too much of this food*).

	Strongly insufficient	Slightly insufficient	Adequate	Slightly excessive	Strongly excessive	I don't know
Fruit and vegetables	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Whole grains	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Pulses	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Nuts	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Meat	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Dairy	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Eggs	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Fish	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Removing barriers to plant-based diets: assisting doctors with vegan patients

Screen #7:

Would you be willing to advise a reduction in meat consumption to patients with the following pathologies:

Knowing that 0 = No not at all and 10 = Yes absolutely, with intermediary choices allowing you to express balanced views.

	0	1	2	3	4	5	6	7	8	9	10
Dyslipidemia	<input type="radio"/>										
Ischemic cardiopathy	<input type="radio"/>										
Overweight / obesity	<input type="radio"/>										
Type 2 diabetes	<input type="radio"/>										
Arterial hypertension	<input type="radio"/>										

APPENDIX B

Screen #8:

Before today did you know about the following information sources / initiatives, designed for either health professionals or patients, about plant-based diets?

Website / Organization / Initiative	Yes	No
Observatoire national de l'alimentation végétale	<input type="radio"/>	<input type="radio"/>
Lundi vert	<input type="radio"/>	<input type="radio"/>
Végécantine	<input type="radio"/>	<input type="radio"/>
Vegetarisme.fr	<input type="radio"/>	<input type="radio"/>
Végéclic	<input type="radio"/>	<input type="radio"/>
Vegan pratique	<input type="radio"/>	<input type="radio"/>

B.3.2 Booklet

Page 1



PLANT-BASED DIETS IN A NUTSHELL

	Fruits Vegetables	Grains	Pulses	Algae	Oleaginous	Eggs	Dairy products	Meat	Fish
Vegetarian	✓	✓	✓	✓	✓	✓	✓	✗	✗
Vegan	✓	✓	✓	✓	✓	✗	✗	✗	✗

- A growing interest in vegetarianism:**

 - 5.2% of the French population reported being vegetarian or vegan in 2018.
 - 24% of the French population report that they could become vegetarian.
- A phenomenon particularly present amongst young adults:**

 - 12% of 18-24 year olds are vegetarian.
 - 44% could become vegetarian.
- A shift towards a reduction of animal-based products.**

 - 50% of the French population report having reduced their meat consumption in recent years.
- A demand from patients to doctors for a more open mind.**

 - 43% of vegetarians report having **hesitated to talk about certain symptoms to their doctors** when he or she had previously advised them to eat meat.



WHAT ABOUT NUTRIENTS?

✓ Calcium

The recommended daily intake for adults is 1000mg. In general, **vegetarian and vegan patients have a sufficient calcium intake** if their diet is well-balanced.

Cabbage, soy, beans, oranges, nuts, seeds, chocolate.

✓ Iron

The non-heme iron contained in plants is less easily absorbed than the iron from animal-based products, and its absorption is facilitated by vitamin C and citric acid. In comparison to the general population, **vegetarians and vegans** have lower ferritin levels, although their levels are within normal limits, and do not present a greater risk of anaemia.

Lentils, chickpeas, beans, soy, whole grains.

✓ Proteins

All the amino acids can be found in a well-balanced plant-based diet (pulses, grains, fruit, vegetables, oleaginous). **Vegetarian and vegan diets meet daily protein recommendations** if the diet is diversified and the caloric intake is sufficient.

Pulses, soy (e.g.: tofu), grains, seitan (wheat protein), oleaginous.

✓	Zinc	<p>Zinc intake can be lower among vegetarians and vegans compared to the general population, even though their serum levels stay within the normal range. There are no studies showing a significant clinical impact.</p>	<p><i>Whole grains, pulses, tofu, seeds, nuts.</i></p>
✓	Vitamin D	<p>Vitamin D levels mainly depend on sun exposure and not so much on food intakes. The recommendations for vegetarians and vegans are the same as the general population.</p>	<p><i>Sun, supplements.</i></p>
✓	Iodine	<p>The ANSES recommends 150µg of iodine daily for adults. Vegetarian patients ensure their intake through the consumption of dairy products. Vegans risk deficiencies if they do not consume marine products regularly.</p>	<p><i>Iodized salt, seaweed, supplements.</i></p>
✓	Omega 3	<p>Omega-3 represent one of the two families of essential polyunsaturated fats and compete with omega-6. Vegetarians and vegans can optimize their intakes by consuming products rich in omega-3 and by limiting their consumption of omega-6 (sunflower oil or peanut oil).</p>	<p><i>Rape oil, flax oil, hemp oil ; nuts, soy, margarine.</i></p>
✗	Vitamin B12	<p>Vitamin B12 is not available in any plant-based products. It is essential for vegetarians and vegans to take supplements, preferably cyanocobalamin and daily intakes of 25µg or weekly intakes of 2000µg.</p> <p>Attention, seaweed contains vitamin B12 analogs that can distort blood tests (urinary tests are preferred).</p>	<p><i>Supplements</i></p>
<p>To bear in mind for clinical practices: Patients with diversified diets do not present higher deficiency risks as long as they take a vitamin B12 supplement.</p>			



SPECIFIC POPULATIONS

+ Infants from 0 to 6 months

- Supplements of **vitamin D2 or D3** during the nursing period.
- In case of breastfeeding, the nutritional intake of the vegetarian or vegan mother must be optimized.
- In case of artificial feeding, it is essential to use **plant-based infantile preparations specifically designed for infants**.

+ Children from 6 months to adolescence

- **Vitamin D, B12 and iodine** require special attention, as is the case for the adult vegetarian and vegan population. Care should be taken about over supplementation due to the intake of other supplements (apart from B12).
- **In case of doubts regarding a well-balanced diet**: conduct a biological test with CBC (complete blood count and platelets), ferritin levels, phosphate, calcium and parathormone test, and zinc. A vitamin B12 screening may be relevant according to the context.

+ Pregnant women or breastfeeding

- Recommendations are the same as those for the general population (cyanocobalamin).
- **A supplement of iodine** should be prioritized. Pay particular attention to excess dosage: limit the intake of seaweed to one portion weekly.
- During the periconceptional period: supplement of B9.

+ Patients aged 50 and older

- Daily consumption should be **rich in omega-3, calcium and proteins**.
- No specific recommendations for the other nutrients.

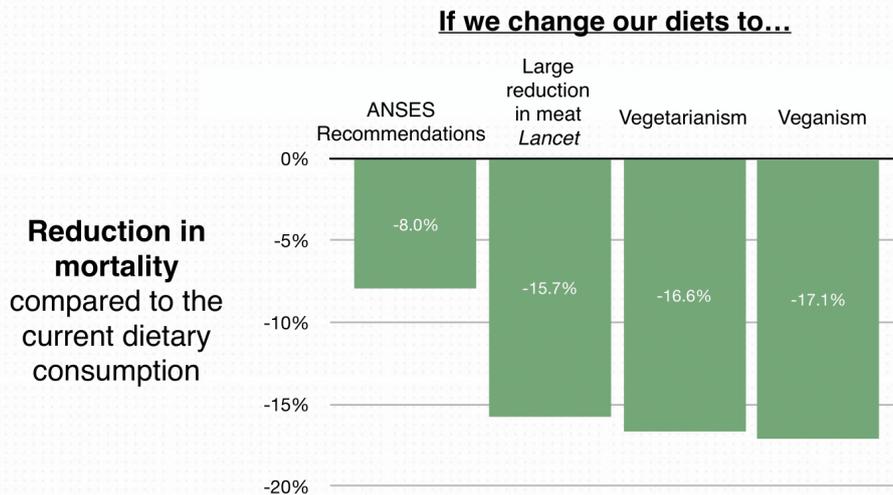
RECENT SCIENTIFIC ADVANCES

A number of meta-analyses and cohort studies have highlighted the co-benefits of well-balanced vegetarian and vegan diets.

- A lower prevalence of **cardiovascular risk factors** (overweight, obesity, hypertension, dyslipidemia)
- A lower incidence of **ischemic cardiopathy**
- A lower incidence of **ischemic stroke**
- A lower incidence of **type 2 diabetes**
- A lower incidence of **colorectal cancers**
- Reduction of **gestational diabetes**

Simulations on the impact of **dietary change** in France.

Springmann et al. (BMJ, 2020), *The healthiness and sustainability of national and global food based dietary guidelines: modelling study*.



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*Platform created and managed by Paco Maginot and
Hermeline Jarson, doctors in general medical practice.*

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B.3.3 Pilot Session

Pilot questionnaire – Translated version (original version in French)

Screen #1:

We present here a case study. We invite you to respond as sincerely as possible so as to reflect what you would practice in your office.

A 27-year-old man, presenting no symptoms or previous illness history, comes to you for a consultation. He currently consumes animal-based products regularly but wishes to become vegan (no red meat, white meat, fish, dairy products or eggs). He asks your opinion about the potential health-risks associated with this diet.

How likely are you to give them the following advice? On a scale from 0% (Not at All) to 100% (Absolutely).

	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
Advise against this diet.	<input type="radio"/>										
Explain to him that there are significant risks of nutritional deficiency.	<input type="radio"/>										
Explain to him that there are no problems as long as he eats a well-balanced	<input type="radio"/>										
Tell him that there are health benefits.	<input type="radio"/>										
Tell him that it is a bad decision that could have serious consequences for	<input type="radio"/>										

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Screen #2:

The same patient returns after four years as a vegan (no consumption of animal-based products – meat, fish, eggs, dairy products etc.) with no supplements and ask if he should have a blood test to look for any deficiencies. He reports eating fruit, vegetables, pulses, seaweed and grains regularly. He does not have any particular symptoms.

Would you prescribe biological tests?

- Yes
- No

If yes, what tests would you prescribe?

- Methylmalonic acid urine test
- Phosphate, calcium and parathormone test
- Vitamin B9 (folic acid test)
- Zinc test
- Albuminemia
- Ferritinemia
- 25-hydroxy vitamin D test
- TSH (thyroid-stimulating-hormone)
- Vitamin B12 blood test
- Complete blood count, platelets

Removing barriers to plant-based diets: assisting doctors with vegan patients

Screen #3:

Imagine that we gave you €2. Imagine that you now have the possibility to give a portion of this endowment to develop an **information campaign** aiming to **promote vegan diets** to the general population. This campaign, developed by the CNRS and doctors, would emphasize the importance of a well-balanced vegan diet and the expected health benefits from such diet.

Please note that the remaining amount, if any, would be yours to keep.

Out of the €2 received, how much would you like to donate to the information campaign project?

You must choose an amount between €0 and €2. The amounts are divided in increments of €0.20.

€0	€0.20	€0.40	€0.60	€0.80	€1	€1.20	€1.40	€1.60	€1.80	€2
<input type="radio"/>										

B.3.4 Code for power analysis

The following code was run in R to obtain the power analysis.

```

1 #Import libraries
2 library(censReg)
3
4 #Set general parameters
5 N=400 #Number of total observations
6 J=1000 #number of simulations for each effect size
7 bonferroniAdj=3 #Three hypotheses to be tested in the paper
8 alpha=0.05 #Significance level
9
10 #-----#
11
12 ### TEST 1 - VDI ###
13 #Vegan disapproval (takes values between 0 and 1 included).
14 #The intervention is projected to decrease the level of VDI.
15 #SESOI (smallest effect size of interest) = 0.1 and defined
    according to Espinosa and Stoop (2021)
16 #Based on the pilot data, we assume: MEAN=0.3768 (H1) and SD=0.2185
    (H2)
17 #We assume data are normally distributed among each group but are
    censored at 0 and 1. (H3)
18
19 #Set seed for replication
20 set.seed(123456789)
21
22 #Set parameters for simulation (defined according to SESOI)
23 minimumEF=0.1
24
25 #Vector to store the results of the simulations
26 vectorResults=rep(NA,J)
27
28 #Simulation
29 for(j in 1:J){
30   t=ifelse(rnorm(N)>0,1,0) #Random Group generation
31   y=rnorm(N, mean=0.3768, sd=0.2185)-minimumEF*t #Data simulation
32   y=ifelse(y>0,y,0) #Censorship below 0
33   y=ifelse(y<1,y,1) #Censorship above 1
34   df=data.frame(cbind(t,y))
35

```

```
36 estResult <- censReg(y ~ t, left=0, right=1, data = df ) #Tobit
   model
37 me=margEff(estResult) #Compute marginal effects
38 s=summary(me)
39
40 #Test H0: t<=0
41 vectorResults[j]=ifelse(s[1]/s[2]<qnorm(alpha/bonferroniAdj),1,0)
42
43 }
44
45 #Statistical power:
46 powerForVDI=mean(vectorResults)
47 powerForVDI
48
49 #-----#
50 ### TEST 2 ###
51 #Proper Medical Practice Index (takes values between 0 and
52 #1 included).
53 #It is made of the sum of separate biological tests.
54 #The intervention is projected to increase the level of PMPI.
55 #SESOI (smallest effect size of interest) = 1/n = 1/8 = 0.125, and
   defined as an improvement by at least one additional test
56 #in doctors prescriptions (either a useless test is abandoned,
   or an additional useful test is prescribed).
57 #It follows a binomial distribution normalized between 0 and 1.
58 #Based on the pilot data, the probability of a positive event is
   0.625.
59 #The standard deviation in the pilot data is 0.220.
60
61 #Set seed for replication
62 set.seed(123456789)
63
64 #Average proba of positive event (before normalization)
65 predicted_p=0.625
66
67 #Set parameters for simulation
68 minimumEF=0.125
69
70 #Vector to store the results of the simulations
71 vectorResults=rep(NA,J)
72
73 #Simulation
```

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```
74 for(j in 1:J){
75
76   y=rbinom(N/2, size=8, prob=predicted_p)/8 #Simulate data in the
       control
77   y=c(y,rbinom(N/2, size=8, prob=predicted_p+minimumEF)/8) #
       Simulate data with treatment effect
78   t=c(rep(0,N/2),rep(1,N/2))
79   df=data.frame(cbind(t,y))
80
81   estResult <- censReg(y ~ t, left=0, right=1, data = df ) #Tobit
       model
82   me=margEff(estResult) #Compute marginal effects
83   s=summary(me)
84
85   #Test H0: t<=0
86   vectorResults[j]=ifelse(s[1]/s[2]>qnorm(1-alpha/bonferroniAdj)
       ,1,0)
87
88 }
89
90 #Statistical power:
91 powerForPMPI=mean(vectorResults)
92 powerForPMPI
93
94 #-----#
95 ### TEST 3 - VPI ###
96 #Vegan promotion index (takes values between 0 and 1 included).
97 #The intervention is projected to increase the level of VPI.
98 #SESOI (smallest effect size of interest) = 0.12 and defined
       according to Espinosa and Treich (2021).
99 #Based on the pilot data, we assume: MEAN=1.2064 (H1) and SD
       =0.8703151 (H2)
100 #We assume data are normally distributed among each group but are
       censored at 0 and 1. (H3)
101
102 #Set seed for replication
103 set.seed(123456789)
104
105 #Set parameters for simulation
106 minimumEF=0.12
107
108 #Vector to store the results of the simulations
```

```
109 vectorResults=rep(NA,J)
110
111 #Simulation
112 for(j in 1:J){
113   t=ifelse(rnorm(N)>0,1,0) #Random Group generation
114   y=rnorm(N, mean=1.2064, sd=0.8703151)+minimumEF*t #Simulate data
115   y=ifelse(y>0,y,0) #Censorship below 0
116   y=ifelse(y<1,y,1) #Censorship above 1
117   df=data.frame(cbind(t,y))
118
119   estResult <- censReg(y ~ t, left=0, right=1, data = df ) #Tobit
120   model
121   me=margEff(estResult) #Compute marginal effects
122   s=summary(me)
123
124   #Test H0: t<=0
125   vectorResults[j]=ifelse(s[1]/s[2]>qnorm(1-alpha/bonferroniAdj)
126     ,1,0)
127
128 #Statistical power:
129 powerForVPI=mean(vectorResults)
130 powerForVPI
131
132 #-----#
133 #Show results:
134 powerForVDI #Predicted power is 0.996 or 99.6%.
135 powerForPMPI #Predicted power is 1 or 100%.
136 powerForVPI #Predicted power is 0.157 or 15.7%.
```

Listing B.1: Power analysis for the VDI, PMPI, and VPI.

Chapter 3

A Practical Guide to Registered Reports for Economists¹

Contents

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¹The paper corresponding to this chapter is published in the Journal of the Economic Science Association and is coauthored with Romain Espinosa (CIRED, CNRS, Nogent-sur-Marne, France).

1 Introduction

In recent years, a growing number of researchers have discussed how research practices can influence the quality of evidence published in scientific journals. It is now well-established that the current publication system has contributed to the inflation of positive (i.e. statistically-significant) results in published research. Franco et al. (2014) find that strong results are 40 percentage points more likely to be published than negative (i.e. statistically-insignificant) results and 60 percentage points more likely to be written up.² Fanelli (2010) shows that papers in the social sciences are 2.3 times more likely to report positive results as compared to the physical sciences, leading some researchers to call for the retirement of statistical significance (Amrhein et al., 2019). Economists are no exception, and may engage in controversial research practices (Ferraro and Shukla, 2020) either intentionally or/and unintentionally due to publication pressure (Necker, 2014). Publication biases (journals' greater propensity to publish positive over negative results and authors' greater propensity to submit positive results) and citation biases (more citations for positive than null results) are widespread in economics (Christensen and Miguel, 2018), and promote manuscripts that contain statistically-significant results.

As a result, researchers tend to over-report positive results, either by actively looking for statistical specifications that reject null hypotheses (p-hacking) or by interpreting unpredicted positive results ex-post (Hypothesizing After the Results are Known - HARKing).³ For instance, Bruns et al. (2022) estimate that 56% to 71% of significance published in economics is inflated. This bias towards false statistically-significant findings (Brodeur et al., 2016) has contributed to the replication crisis (Schooler, 2014, Loken and Gelman, 2017), undermining the credibility of scientific evidence.⁴ Significance inflation can be particularly problematic for laboratory ex-

²Franco et al. (2014) analyze the results of survey-based experiments funded by a NSF-sponsored program and run on nationally representative samples between 2002 and 2012. They compare the results of the experiments that got eventually published with the results of the experiments that remained unpublished.

³See for instance John et al. (2012), Agnoli et al. (2017), Fanelli (2009), Fiedler and Schwarz (2016), LeBel et al. (2013), O'Boyle Jr et al. (2017)

⁴This effect is worsened by non-replicable analyses being cited more than replicable analyses (Serra-Garcia and Gneezy, 2021), and by the fact that a failure to replicate a work does not lead to fewer citations (Schafmeister, 2021). Note that, in economics, Camerer et al. (2016) find a replication rate of 61% in a sample of 18 experiments published in the *American Economic Review* and the *Quarterly Journal of Economics*, although the low replication rate might result from imperfect replication conditions (Chen et al., 2021).

periments where limited costs may encourage researchers to abandon experiments with null results (i.e., drawer effect, Page et al. (2021)).

The over-representation of statistically-significant results is harmful in two ways. First, for a given study, the strength of the statistical evidence depends on the hidden statistical evidence that is not reported in the manuscript. For instance, listing a statistically-significant result after having explored two null hypotheses is much more informative than after having explored a dozen null hypotheses. The incentive to report statistically-significant results blurs the quality of the evidence provided in manuscripts, which is harmful for long-run knowledge-accumulation in science. Second, the under-representation of statistically-insignificant results prevents policy-makers from having access to the entire range of scientific evidence, which may lead them to overestimate the effect of one variable on another as only statistically-significant findings are reported. This overall leads to suboptimal policy-making and a mis-perception of the world by researchers who only have access to biased or blurred knowledge.

A growing number of scientists have called for the use of pre-registration in empirical work to tackle these issues (Nosek and Lakens, 2014, Swanson et al., 2020, Miguel, 2021). In pre-registered studies, researchers pre-specify the analysis to be carried out before examining (or even collecting) the data (Olken, 2015). This includes listing (i) the outcome variables, (ii) the control variables, (iii) the cleaning procedure (e.g., exclusion rules), (iv) the statistical models that will be used in the analysis. The pre-registration also describes the significance level that will be used as the decision criterion to reject null hypotheses, how multiple-hypothesis testing will be addressed (e.g., via a Bonferroni adjustment), a description of the sample size, and when data collection will be terminated. By limiting the *researcher's degree of freedom* (Bakker et al., 2020), pre-registration, when well implemented, substantially reduces the risks of p-hacking (Heyes et al., 2022), HARKing, and forking (i.e., choosing a statistical model conditional on the data, but in an environment where a different model would have been chosen given different data (Gelman and Loken, 2013)).

Over the past decade, a number of economic journals have become aware of the necessity to pre-register empirical analyses. For instance, all RCT submissions to the

American Economic Association's journals must now be pre-registered.⁵ Platforms such as the Social Science Registry (AEA RCT Registry), As Predicted (Wharton Credibility Lab) and OSF Pre-registration enable researchers to easily store online, under embargo, their research design and analysis plans. An increasing number of economists are using these platforms. For instance, the number of pre-registrations on the Social Science Registry more than quintupled between 2014 and 2021 (from 223 to 1,169 pre-registrations per year).

However, pre-registration, even when meticulously executed, only solves part of the issue of the misreporting of statistical findings. First, pre-registration does not preclude publication bias (the greater likelihood of journal publication for positive results), which can still distort the distribution of evidence. Second, researchers can still erroneously anticipate publication bias even if it is absent (incorrect beliefs) or expect positive results to be better-cited (citation bias). Researchers can still then be more likely to submit manuscripts with positive results and drop work with null results, again leading to the biased reporting of scientific evidence.

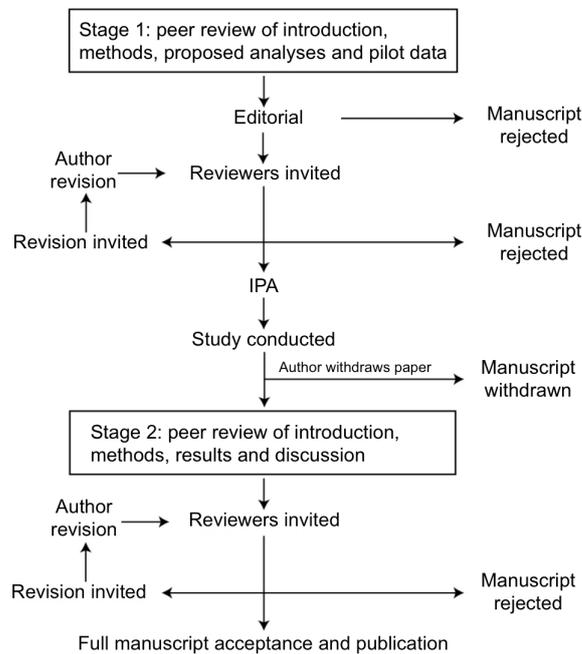
Registered Reports (RRs) are a new submission format that has been intensively discussed over the past decade as a way of improving credibility in empirical work (Page et al., 2021). RRs, also known as Pre-Results Reviews, focus on the scientific process rather than the outcomes. A review of a paper is carried out before any research outcomes are known. Chambers and Tzavella (2022) describe the process as follows (as summarized in Figure 3.1):

In the first stage, authors submit their research question(s), theory, hypotheses, detailed methods and analysis plans and any preliminary data as needed. Following detailed review and revision—usually according to specific criteria—proposals that are favourably assessed receive in principle acceptance (IPA), which commits the journal to publishing the final paper regardless of whether the hypotheses are supported, provided that the authors adhere to their approved protocol and interpret the results in line with the evidence. Following IPA, authors then typically register their approved protocol in a repository, either publicly or under a temporary embargo. Then, after completing the research, they submit a stage-2 manuscript that includes the approved protocol plus the results and dis-

⁵This only applies to field experiments. Laboratory experiments have no pre-registration requirements for the moment.

cussion, which may include clearly labelled post hoc analyses in addition to the preregistered outcomes (that is, findings from both confirmatory and exploratory analyses). The reviewers from stage-1 and/or newly invited reviewers then assess the completed stage-2 manuscript, focusing on compliance with the protocol and whether the conclusions are justified by the evidence. Crucially, reviewers do not relitigate the theory, hypotheses or methods, thereby preventing knowledge of the results from influencing recommendations. (Chambers and Tzavella (2022), page 29)

Figure 3.1: Summary of the publication process of Registered Reports (from Chambers and Tzavella (2022))



Note: We added the word "peer" in "peer review" compared to the original figure in Chambers and Tzavella (2022).

An increasing number of scientific journals have adopted RRs as a valid submission format over the past few years (from 3 in 2013 to over 300 in 2022⁶). Economic outlets have also showed a growing interest in RRs. For instance, the Journal of Development Economics, Q-open, the Journal of Behavioral and Experimental Eco-

⁶According to the Center for Open Science <https://www.cos.io/initiatives/registered-reports>.

nomics, and the Review of Finance accept RRs on a regular basis. In addition, the Journal of Political Economy Microeconomics plans to accept RRs in the near future, and Experimental Economics recently published a special issue dedicated to RRs. Last, the Journal of the Economic Science Association now accepts RRs for replication studies.

This growing interest in RRs reflects their substantial advantages. First, RRs provide the same benefits as pre-registration (PR) when meticulously implemented. Researchers commit to the way in which they will carry out their research (the research question, theory, hypotheses, statistical models, and outcome and control variables) before the data collection, which eliminates the risk of data mining (p-hacking or HARKing). Second, RRs are preferable to pre-registration as they allow researchers to improve their study design and analytic approach based on feedback from peers. Stage-1 reviews allow referees to make suggestions about the research design that can be implemented before data collection, unlike standard ex-post reviews. Third, RRs also improve pre-registration. There is currently little control over the quality of pre-registrations, and researchers may omit important information (e.g., multiple-hypothesis adjustment), which undermines the very purpose of pre-registration. Bakker et al. (2020) find, for instance, that unstructured pre-registration is much less effective in increasing research transparency than structured pre-registration, and argue that RRs would help to clarify the real degrees of freedom. Ofosu and Posner (2021) show that pre-analysis plans are often not written or used in a way that allow them to solve the issues they are aimed to address. Similarly, Abrams et al. (2020) analyze pre-registrations in experimental economics and conclude that the majority of these pre-registrations are not detailed enough to address the concerns about inference. Fourth, RRs create better incentives for researchers as compared to pre-registration: in-principle acceptance (IPA) increases the likelihood of innovative approaches, as researchers know that high-risk, high-reward protocols will be published when they receive an IPA, regardless of the outcome.⁷ Researchers therefore feel more comfortable in proposing ground-breaking resource-

⁷The current publication system might lead some researchers to avoid high-risk, high-reward protocols that might however be beneficial for science. The pressure for positive results might indeed make risk-averse researchers invest in several small-scale experiments rather than in a large-scale high-risk intervention to ensure that they have at least some positive results to publish. In-principle acceptance could help mitigate this issue by reducing the publication risk associated with high-risk studies.

intensive studies, and less pressure to publish positive results.⁸ As a result, Heckeleei et al. (2022) suggest that RRs could play a growing role in funding decisions, with research funders potentially being willing to fund pre-accepted studies that provide them with a more-secure research outcome. Overall, RRs can be considered the most advanced form of PR because all the elements that should be included in a PR are not only present in the RR but also peer-reviewed and because they guarantee the publication to researchers. Table 3.1 summarizes the advantages of registered reports over unregistered and pre-registered studies, assuming high quality of peer review.⁹

Table 3.1: Bias limitations between unregistered studies, pre-registered studies and registered reports.

	Unregistered studies	Pre-registered studies	Registered reports
High statistical power¹	Unannounced ex-ante or unanticipated	If power analysis adequately pre-registered	✓
Eliminates p-hacking	✗	If pre-analysis plan adequately pre-registered	✓
Eliminates HARKing	✗	If pre-analysis plan adequately pre-registered	✓
Low Family-Wise Error Rate (FWER)	✗	If multiple hypothesis correction adequately pre-registered	✓
Limits citation bias	✗	✗	✓
Eliminates publication bias	✗	✗	✓
Clear distinction between confirmatory and exploratory analyses	✗	If pre-analysis plan adequately pre-registered	✓

¹High statistical power may depend on the journal's required statistical threshold.

The aim of the current paper is to provide practical guidance about the way in which to write and manage RRs in experimental economics. We discuss the

⁸Scheel et al. (2021) select papers in psychology that include hypothesis testing, and find that 96% report a positive significant result for their first hypothesis, as compared to a figure of only 44% in registered reports.

⁹We focus here on the statistical advantages of RRs. Henderson (2022) proposes a similar table where she also discusses the benefits for researchers, such as the reduced stress associated with the publication process.

important steps of a RR, including determining the number of hypotheses, writing an analysis plan, carrying out a power analysis or defining sample size more generally, and correcting the level of significance. We show examples of code for the implementation of the statistical analyses in R and Stata (the latter in the Appendix). We also provide advice about how to improve a pre-registered study, e.g., by reducing the dimensionality of the outcome variables, distinguishing statistical and economic significance (the smallest effect size of interest), and discriminating between confirmatory and exploratory analyses. We focus here on frequentist approaches to statistical inference, as these are dominant in economics, but RRs can also benefit from Bayesian methods (e.g., stopping rules: see Dienes (2011)). Last, we provide practical advice for authors, editors, and referees for the writing and evaluation of RRs.

The remainder of the paper is organized as follows. First we illustrate the credibility challenge in empirical work, and how Registered Reports can help. We then present guidelines for the writing of Registered Reports. The last section concludes. All of the codes appear in the Supplementary Materials.

2 The credibility of non-registered studies

We illustrate the credibility issue for non-registered empirical work via the following example. Imagine that researchers have access to a dataset where an exogenous event affects a subset of the sample (a laboratory, field, or quasi-natural experiment). The researchers wish to evaluate whether the event (called the treatment) significantly affected the individuals who were exposed, by comparing them to a non-exposed control group (a between-subject design). We assume that the researchers have J outcome variables that could be considered as relevant with respect to the literature, and K potentially-relevant control variables (e.g., socio-demographics). We in addition assume that the researchers identify L possible exclusion rules for outliers in the sample (inconsistent answers to a question, failure to pass attention tests, etc). Last, imagine that the researchers identify M possible statistical models that could be used to estimate the treatment effect. For instance, if the outcome variables are all positive and bounded, a linear regression on levels, a linear regression on logs, a Poisson regression, a Tobit regression, a binary regression model (e.g., below/above

the middle of the scale) or an ordered regression model (decomposing the scale into subcategories) could all be used.

The researchers here can then explore up to $J \times 2^K \times 2^L \times M$ specifications in order to determine whether the event had a significant impact on the treated group. This analysis space, called the multiverse, includes the range of equally-legitimate analyses that can be carried out to answer the research question. With five outcome variables ($J = 5$), ten socio-economic variables ($K = 10$), ten potential exclusion rules ($L = 10$) and six econometric models ($M = 6$), the researchers can investigate up to 31 million combinations. It can be argued that these numbers are overstated, as the researchers' degree of freedom might be much more limited. For instance, researchers can anticipate editors' and referees' concerns, and therefore consider a smaller number of dimensions. Even so, if the researchers have only $K = 5$ control variables (as the referees will always have strong opinions about the other control variables), $L = 5$ decision rules and $M = 4$ models, they can still explore up to 20 thousand specifications.

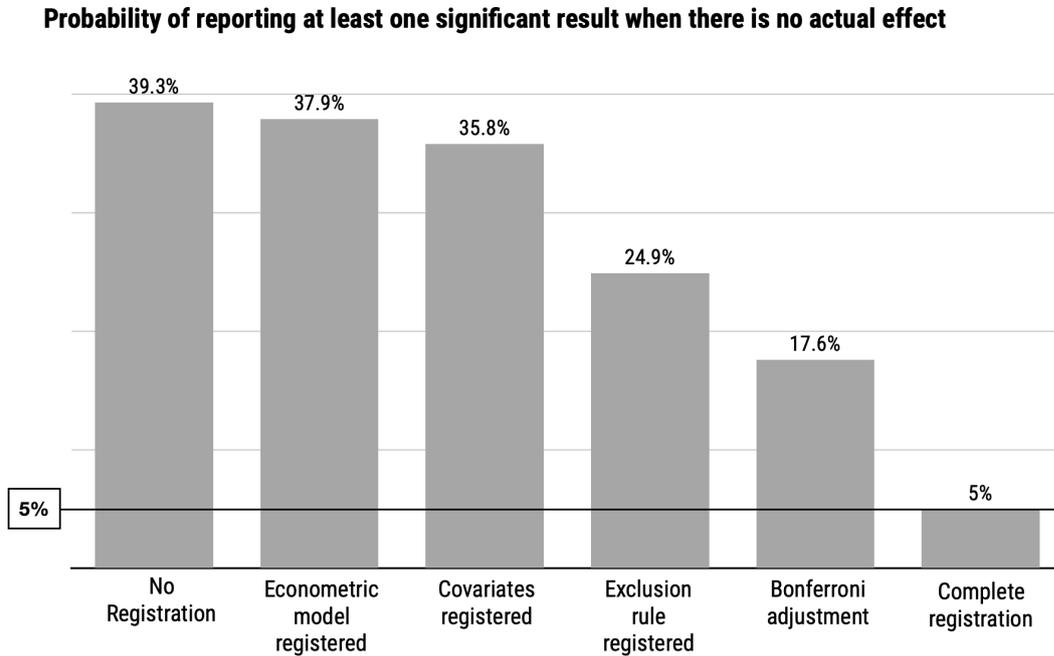
To illustrate the benefits of pre-registration, consider the following example. Imagine that the treatment has no effect on $J = 3$ outcome variables. Imagine now that the researchers did not pre-register their analysis, and anticipated that statistically-significant findings will be more likely to be published (publication bias) or cited (citation bias), so that they only report significant results, using $\alpha = 5\%$ as their decision rule (i.e. they reject a null hypothesis whenever the associated (uncorrected) p-value is below 5%). Given that the treatment has no effect, what is the probability that they will be able to report at least one significant result?

Figure 3.2 displays the simulation results (the code appears in the Supplementary Materials). We assume $K = 3$ control variables, $L = 3$ exclusion rules and $M = 3$ statistical models (OLS, Poisson, Probit), i.e. a multiverse of 576 specifications. We consider here independent outcome variables that are normally-distributed ($\mu = 10$, $\sigma^2 = 10$) and censored between 0 and 20 (e.g., contributions in a public-good game). Without a minimal form of pre-registration, researchers can erroneously report at least one significant result in 39.3% of the cases. In other words, if the same experiment is run by 10 research teams, on average four of them will be able to write a manuscript with at least one significant result. It may be that researchers will also be required to produce robustness checks, which would reduce the probability of reporting a false positive result. However, the simulations show that robustness

checks are likely to be of only limited help. Whenever researchers are able to reject at least one of the three null hypotheses (as $J=3$ in these simulations), they are able to provide on average 74.3 specifications where the treatment is found to have a significant effect. In unregistered studies, researchers can therefore strategically report robustness tests to support their findings (Young and Holsteen, 2017).

Pre-registering parts of a study significantly reduces the risk of erroneously reporting a statistically-significant treatment effect, with all elements playing a role: registering the econometric model (-1.4 percentage points), the set of covariates (-3.5 pp) and the exclusion rule (-14.4 pp), and correcting the significance threshold via a Bonferroni adjustment (-21.7 pp). Complete pre-registration, as would be found in a RR, has the largest impact. In this case, researchers only have a 5% chance of erroneously reporting a statistically-significant treatment effect, which is precisely the significance level of 5% that they targeted. From a statistical perspective, it is obvious that pre-registrations can help mitigate the inflation of false positive results only if all aspects are covered, but it is often not the case. Registered Reports are an effective way of reviewing (and adjusting) a study's analysis plan before it becomes too late, i.e. prior to data collection.

Figure 3.2: Illustration of the risks of non-registration in inflating the number of studies with positive results.



Notes = These are the results from 20,000 simulations when researchers have three potential outcome variables, three alternative econometric models, three potential exclusion rules, three covariates for additional inclusion, and a 5% significance level for the decision rule.

3 How to write a Registered Report

Registered Reports are a mix of a standard paper and a pre-registration. On the one hand, RRs are written as a standard manuscript regarding the abstract, the introduction (the relevance of the research question and the contribution to the literature), and the theoretical background. On the other hand, the manuscript must include a data-analysis plan, a sampling plan and a detailed research-design section. Similar to well-executed pre-registrations, RRs must be specific (a detailed description of all of the steps from hypothesis to final report), precise (only one possible interpretation), and exhaustive (exclusion of other steps / deviations from the analysis plan) (Wicherts et al., 2016). The discussion and conclusion sections can be left blank for Stage-1 submission, or a number of versions can be written

conditional on the results in Stage-2.¹⁰ Various templates are proposed online to help researchers write their RR (e.g., PCI-RR, JDE, and Nature Human Behavior).

In what follows, we discuss the most technical issues of writing a RR: the analysis plan, the sampling plan, the correction of the significance level, and the smallest effect size of interest. We mostly provide examples taken from experimental set-ups, but the discussion can be applied to any analysis involving primary data collection. We also discuss how to include exploratory results in the Stage-2 manuscript, how to deal with ethics requirements, and how to choose the appropriate journal for submission.

3.1 Analysis Plan

The analysis plan is the core of RRs and has to discuss the following elements, all of which aim to reduce the researcher's degree of freedom and so limit p-hacking and HARKing.

Dataset. First, researchers must describe how the data will be collected (e.g., either by the researchers themselves or a third-party, either through interviews or self-administrated questionnaires), where (e.g., either in a laboratory, on the field or online), and when (time window). They must commit to a certain number of observations (preferably determined by a power analysis). The researchers can also decide on the number of observations conditional on quality checks, by for example continuing data collection up to the point where a given number of observations pass the quality checks. Researchers also have to explain how they will tackle potentially missing data.

Exclusion rules. At this stage, researchers should indicate their inclusion/exclusion rules. In particular, these exclusion rules include quality selection via the use of attention checks. For example, researchers could decide that participants in an experiment who spend less than 30 seconds reading the instructions will be excluded from the analysis. Similarly, they could decide that only participants who successfully answer comprehension questions after reading the instructions will be included in the analysis. In the case of RCTs, researchers could for example decide

¹⁰Henderson et al. (2019) is an example of a Stage-1 manuscript with conditional results. <https://osf.io/8rq7k>

to exclude from their analysis individuals who changed location during the intervention. These exclusion/inclusion rules can be identical across groups or specific to some of them (e.g., when the comprehension questions differ for the treatment and control groups). As in unregistered studies, the researchers must ensure that these inclusion/exclusion rules do not bring about selection effects.

More globally, the exclusion rules should concern all possible types of exclusion decisions. For instance, researchers should pre-specify, when relevant, how to deal with equipment errors (e.g., a software crashes for some participants during an experiment in which participants interact), or with partial participation (e.g., a participant who leaves the room before the end of the session). Running a pilot experiment is a good way to identify the major exclusion risks in the main experiment and to decrease the risks of adverse events (e.g., software crash). While the exclusion rule cut-offs might involve some arbitrariness, researchers should seek to justify them when it is possible.

Construction of the variables of interest. The construction of the outcome and independent variables has to be explained in detail. These may be taken directly from the dataset (e.g., the number of tokens given to a public good). Alternatively, they may be a transformation of a variable directly obtained from the participants (e.g., the log of the contribution) or composite variables (e.g., the average contribution). For the latter composite variables, it can be important to check with pilot data whether these have the desired properties (e.g., via Cronbach's alpha). Alternatively, researchers can make hypothesis-testing conditional on these desired properties in the final sample using outcome-neutral tests (see below). It is important to report the nature of all variables (ordinal, nominal etc.) and the relevant characteristics for data analysis (e.g., the range). It is also important to prepare for outliers (for instance in the case of open numeric fields) that would need to be corrected, for example via winsorization above a certain threshold. For simplicity, the description of the control variables and/or their construction can be put in an Appendix.

Statistical method. The analysis plan, i.e which statistical method to be used, needs to be specified. For instance, the authors can commit to analyzing contributions in a public-good game via a Tobit regression with individual random effects, sandwich-robust standard errors, correcting for age, gender, and political

self-placement. Ideally, the statistical code that will be used to analyze the data after data collection can be provided. Note that the statistical method should be the same as that used in the power analysis (see below).

Dealing with outliers. An important challenge for RRs is to define the appropriate method that one will implement for outliers. Beaumont and Rivest (2009) recall that outliers can be considered either representative (i.e., large observations that occur in the population) or non-representative (e.g., large values resulting from reporting errors). Similarly, we can see representative outliers as legitimate and non-representative outliers as illegitimate using the terminology of Leys et al. (2019).

In our view, legitimate outliers should not be excluded from the dataset as they naturally occur in the population. However, it might be that the presence of legitimate/representative outliers significantly impacts the fit of the statistical model. For instance, the presence of large values might create a substantial gap between the mean and the median. We identify two strategies to deal with legitimate outliers. First, researchers can reduce the likelihood of outliers by changing the design of their survey/experiment before data collection. For instance, asking closed questions instead of open numeric fields significantly reduces the risks of outliers. Second, researchers can set up in their RR a protocol about the way they will deal with outliers. For instance, they can explain that they will winsorize the data above a given threshold and that they will use a censored statistical model to account for winsorizing. As Leys et al. (2019) underline, keeping or removing outliers always comes at a cost (either it risks decreasing the quality of the statistical estimation, or we lose some information).

As far as illegitimate/non-representative outliers are concerned, researchers should seek to minimize the risks of occurrence (when relevant) or detail the exclusion/recoding rules. As to minimize the risks, researchers must test their software before data collection to ensure that only the expected range of values can be entered. Alternatively, researchers can explain their exclusion/recoding rules. For instance, they can describe for each variable the possible range of values (e.g., variable X takes values between 1 to 7 with increments of one), and detail how they will deal with outliers (e.g., values above 7 will be recoded as 7, values outside of the pre-specified range will be recoded as missing).

If researchers fear the emergence of unexpected outliers, Leys et al. (2019) also suggest asking external judges (e.g., other researchers), who are blind to the research

hypotheses, to make a decision about the way to deal with the outliers. In our view, this strategy can be appealing as it is not possible to foresee all potential events that could generate outliers. However, we see three risks associated with this type of procedure. First, editors must ensure that the external judges are indeed blind to the research hypotheses. For instance, asking an external anonymous reviewer appointed by the editor might help solve this issue but it would increase the costs of dealing with RRs for journals. Second, one of the main ideas of pre-registration in general (and, thus, for RRs as well) is to limit the risks of ex-post justification. HARKing is an important risk because there are always good reasons ex-post for choosing one approach rather than another. If an external judge is called to decide on a case, the authors might always come up with convincing reasons for doing so. Last, another risk is *forking*: given the observed sample, researchers might seek to change their estimation/method, which makes the estimation method contingent on the sample.

Hypothesis testing. Last but not least, the hypotheses to be tested (e.g., H_0 : the treatment has no impact on the contribution to the public good), and which statistical tests will be carried out must be stated clearly. RRs have two advantages here, as they avoid any suspicion of data-mining. First, researchers are free to use one-sided tests, which are more appropriate when theory predicts the direction of the effect. One-sided tests have the advantage of requiring fewer observations for the same statistical power. Second, researchers can also rely on less-frequently used statistical tools that may seem more appropriate but can be suspected of p-hacking in unregistered analyses (e.g., polychoric correlations). As Dienes (2020) notes, RRs can help in choosing the most appropriate test of the theory.

It is important to count the number of hypotheses tested so as to adjust the significance level (see below). Each additional hypothesis increases the probability of obtaining at least one positive result (when there is no correction). Sub-group analysis, which postulates a statistically different effect across groups, also increases the researcher's degree of freedom. However, testing a variety of null hypotheses for the same parameter of interest (e.g., $H_0^1 : \theta = \theta_1$ and $H_0^2 : \theta = \theta_2$) does not increase the researcher's degree of freedom. Null-hypothesis testing can be carried out by calculating the confidence interval of the relevant parameter, so that a single confidence interval can be used to rule out multiple hypotheses without increasing the risk of false positives.

We will illustrate the writing of an analysis plan via the following example. Consider that researchers wish to test whether a treatment affects the contribution and punishment decisions in a public-good game. They have a between-subject design that they described in a previous section of their manuscript. The analysis plan could be presented as follows:

Example: Based on previous work and the theory discussed above, we expect the intervention to increase contributions and reduce punishment. To test these two hypotheses, we will run the experiment described above at the experimental laboratory of the University of Rennes. Based on our power analysis (see below), we will obtain observations from 240 participants with equal-probability random assignment between the control and treatment conditions (6 sessions of 20 participants in each condition). The experiment will take place between February and March 2023. Participants who do not correctly answer the three comprehension questions (described in the design section) after reading the instructions will be excluded from the statistical analysis.

The two hypotheses will be tested by OLS estimations with individual fixed effects and standard errors clustered at the group level (partner-matching). In total, given that each player plays ten rounds, we will have $10 \times N$ observations, where N is the number of participants who answered the comprehension questions correctly. The control variables include self-reported variables (age, gender, and political self-placement on a 1-to-7 Likert scale) as well as a Global Trust score that is the sum of the six items presented in the design section (Yamagishi and Yamagishi, 1994, Jasielska et al., 2021).

We will first test whether the treatment increases public-good contributions. We will regress the contribution levels (that take on values between 0 and 20) on the treatment dummy and the control variables above. We will perform a one-sided test using the estimates from the linear model ($H_0^1 : \theta_1 \leq 0$). Second, we will similarly regress the number of punishment points (that take on values between 0 and 6) on the treatment dummy and control variables and run a one-sided test ($H_0^2 : \theta_2 \geq 0$). For each of the two outcome variables, we will instead apply a random-effects Tobit estimation if the share of observations at the lowest or highest possible values exceeds 50% of the total number of observations. Last, we will consider a significance level of $\alpha = 0.05$, and will correct for multiple-hypothesis testing (with two hypotheses) using Holm-adjusted p-values.

Exceptions. There are two exceptions to this general framework. First, Registered Reports do not necessarily impose the inclusion of hypotheses. When there is no prediction about the sign or size of a coefficient, authors can simply propose

a method to estimate the coefficient without any hypothesis testing. In this case, authors should still correct their confidence intervals taking into account the number of parameters of interest they estimate, as if they were testing hypotheses.

Second, authors can decide to perform blinded analysis. Dutilh et al. (2021) argue that blinded analysis protects research from hindsight and confirmation biases but still allows some form of flexibility to deal with unexpected peculiarities in the data. The authors distinguish six types of blinded analysis. First, a data manager can give the analyst access to only part of the data (calibration set), and the statistical analysis is then run on the remaining data (test set) once an appropriate analysis plan is produced. This method is however costly in data as part of the dataset is used for calibration. Second, the data manager can add random noise to all values of the outcome variable(s) such as to hide the relationships between variables. Researchers would develop their analysis plan on the contaminated data and would then run their analysis on the original data. However, this method requires determining the appropriate amount of noise to add to the data. Third, the data manager can shuffle the level labels of an experimental factor in the data she gives to the researchers to develop their analysis plan. Fourth, the data manager can add a random score to the original outcome variables, which would be identical for all observations that have the same original value. By doing so, the data manager can equalize the means (for instance between a treated and a control group) such that the difference in means in the contaminated data is by construction equal to zero, which prevents p-hacking. Fifth, the data manager can also shuffle one or several key variables, while leaving the rest untouched, which will keep the overall distribution intact (e.g., for dealing with outliers) but will distort the relationships observed in the original data. Last, the data manager could also provide the analyst with decoy datasets (MacCoun and Perlmutter, 2017). The analyst would work with several datasets (e.g., six) to produce his data analysis plan and would then run his test on the real dataset.

In case the authors want to rely on blinded analysis, they must provide detailed instructions in the Stage-1 manuscript about the procedure they want to implement. Importantly, they must describe in detail how the data manager and the analyst will proceed, and the types of interaction they will have. There are three important criteria in this procedure: editors must ensure that the analyst is effectively blind to the hypotheses that the authors wish to test, the data manager must be indepen-

dent from the data analyst, and authors must report the analyst's procedures in reproducible details in the Stage-2 manuscript.

While blinded analysis can be relevant in very specific cases (e.g., it is not possible to obtain pilot data, there is high uncertainty about the data collection, and conditional analysis is not feasible), we do not recommend it when it is not necessary. It is indeed difficult, if not impossible, to control for the interactions between the data manager and the analyst, and unobserved interactions would undermine the very purpose of RRs. We thus recommend blinded analysis in RRs as a last resort solution that necessitates very strong guarantees about the interactions between the data manager and the data analyst.

3.2 Sampling plan: power analysis and sample size

The objective of the power analysis is to determine the number of observations necessary to estimate a treatment effect. Type-I errors, i.e. the probability of incorrectly rejecting the null hypothesis, is by definition equal to the significance level of α when the test assumptions are met. Type-II errors, i.e. the probability of not rejecting an incorrect null hypothesis, are usually labeled β . Statistical power is the probability of successfully rejecting an incorrect null hypothesis, and therefore equals $1 - \beta$. In an experimental setting, high statistical power indicates that the study is likely to conclude that a treatment has an effect when it actually does (i.e. a low risk of Type-II errors). In practice, analyses with $1 - \beta \geq 80\%$ are usually considered as sufficiently powered, as they have an at least 80% chance of concluding that the treatment has an effect when it actually does. However, note that some journals require higher statistical power (ex: 95% for Nature Human Behavior).

Reporting the statistical power of a study is a central element in empirical research, especially in confirmatory analyses. In the case of null results (H_0 not rejected), readers may not know whether the lack of statistical significance results from low statistical power (i.e. too few observations) or from a true null hypothesis (no treatment effect). This follows from statistical power being a function of the number of observations: more observations lead to more-precise estimates (i.e. smaller standard errors) and thus to tighter confidence intervals. A greater number of observations increases the probability of successfully rejecting the null hypothesis, i.e. statistical power. As the number of observations rises, not rejecting the null hypothesis becomes stronger evidence for the absence of a treatment effect.

The power-analysis section aims to estimate the statistical power ($1 - \beta$). Some statistical software proposes packages and functions that directly estimate the statistical power of simple tests. For instance, the R function *pwr.t.test()* reports the statistical power of a t-test. The STATA package *powerBBK* developed by Bellemare et al. (2016) can also be used to calculate power for linear, binary, and censored models. However, researchers may need to use specific statistical models for which there are no available statistical power functions. We here present the general method that will allow researchers to calculate the statistical power of their model.

The general idea of power analysis is to simulate data assuming that we know the data-generating process, and to estimate the probability that this statistical model successfully rejects the null hypothesis given the assumed effect size. The process can be summarized as follows:

1. Set the seed, so that the same results are produced every time we run the code.
2. Set the parameters of interest: the parameters necessary to generate the data (ϕ ; e.g., the standard deviation of the outcome variable), the number of simulations S , the sample size N , and the significance level α .
3. For each simulation from 1 to S :
 - (a) Generate a dataset using the assumed data-generating process with θ and N .
 - (b) Calculate the test statistics from the statistical model under consideration (e.g., a t-value from a linear regression).
 - (c) Report whether the model rejects the null hypothesis (e.g., no difference in means between the treated and control groups).
4. Calculate the average frequency of rejecting the null hypothesis, which is an estimate of statistical power.

We show in the Supplementary Materials an example of power analysis using the above algorithm for a public good game.

Power Analysis from pilot data Note that the data-generating process can sometimes be difficult to infer (e.g., messy data). For instance, in some cases the distribution of the pilot data does not correspond to any standard statistical distribution. In these cases, power can be simulated by bootstrapping the empirical pre-test data (e.g., data from previous papers or pilot data). This method is valid as long as the pre-test data can be assumed to be representative of the population of interest. This is however unlikely to hold for data from published works if we assume some form of publication bias (i.e., we have access to a distorted distribution of the data). Importantly, researchers should not use the effect size observed in the pilot data to run the statistical power analysis. Indeed, this would lead to a *follow-up bias*, i.e., only studies with pilot data that report a sufficiently large effect size (and thus a sufficiently low sample size requirement) would be implemented (Albers and Lakens, 2018). The researchers should use the pilot data to simulate the data generating process but must set the effect size of interest with another approach (see the smallest effect size of interest below).

Statistical model. The statistical model used in the power analysis has to be that which will effectively be used in the data analysis. The structure of the data therefore has to be anticipated: the nature of the outcome variable (e.g., binary, censored or ordered), unobserved heterogeneity or interdependence (robust or clustered standard errors), and so on. The statistical model can be conditional if there is uncertainty about the distribution of the data to be collected (see the outcome-neutral tests below).

Design feedback. Power analyses can help researchers improve the design of their experiment. For example, continuous variables contain more information than binary variables, but continuous decisions might be less intuitive for participants or might have less external validity. Researchers who face a trade-off between the complexity (or external validity) of an experiment and statistical power can assess the benefits of continuous over dummy variables by calculating the statistical power and looking at the number of observations that they are able to collect (for example due to budget considerations). Similarly, researchers might be tempted to have their participants play a game several times in a row (e.g., a repeated public-good game) to produce more observations (that will however not be independent). Nevertheless, participants who stay longer in the laboratory are likely to be paid more, which

reduces the number of participants. Researchers might therefore hesitate between having more repeated observations or more participants with fewer observations per participant. Running a statistical power analysis with a clustered linear regression can help to establish the best design in terms of statistical power.

Calibration. A key condition for power analysis is the anticipation of the structure of the data that will be collected and the correct estimation of the data-generating process (DGP) for the targeted sample. As discussed above, power analysis requires some information about the distribution of the variables of interest or about the control group at least. The best way to calibrate the simulations is to collect pilot data using the same experimental design and population as in the upcoming experiment. When this is not possible, a second-best solution is to look at similar situations in the literature (e.g., similar country and cohort). If no data is available, a third-best solution is to infer how participants are expected to behave from similar games. A last-resort solution is to calibrate the DGP arbitrarily, to carry out the power analysis under different scenarios, and take the most conservative estimate from these analyses. Last, it should be noted that the calibration is used to define the expected effect size in the DGP, which is different from the effect size that is tested under the null hypothesis. For instance, researchers may anticipate an effect size of 0.5, but be interested in testing whether the actual effect size is above 0.2. In this case, they would simulate the data assuming $\theta = 0.5$, and would then test whether the estimated coefficient $\hat{\theta}$ is statistically larger than 0.2 ($H_0 : \theta \leq 0.2$).

Outcome-neutral tests. The validity of power analyses is affected when there is sufficient uncertainty about the way participants will behave. For instance, an ex-ante power analysis may largely underestimate the number of censored observations in the baseline condition. In this case, an ex-post power analysis might show that the study is underpowered for the detection of the originally-assumed effect size with sufficiently-high probability (e.g., $1 - \beta = 80\%$).

If researchers anticipate this risk, they can propose *outcome-neutral tests*. These tests have no economic value regarding the research questions of the paper, and only check whether the statistical tests used to address the research questions are relevant given the data collected. For instance, a decision rule could be to run a regression of a binary variable on a treatment dummy only if the share of ones is below 90% of the observations. Similarly, for Likert-scale questions, a researcher can

state ex-ante that she will run the planned statistical tests only if under 40% of the data is censored at the higher limit (*ceiling effect*) or the lower limit (*floor effect*).

One convenient solution for outcome-neutral tests is to condition the statistical test on an ex-post power analysis. Once the data has been collected, statistical power can be calculated using the same method as prior to data collection but calibrating the data-generating process to the data collected. In their ex-ante power analysis, researchers might have for example estimated a statistical power of 90%, but, as they misperceived the structure of the data, the ex-post statistical power might be only 70%. To ensure sufficient statistical power, researchers might therefore commit to run a statistical test only if the ex-post statistical power to estimate the pre-registered effect is above 80%. Importantly, ex-post statistical power should be estimated using the effect size assumed in the registered sampling plan and not the observed effect size.¹¹ Note that if an outcome-neutral test fails and the researchers cannot run their main test of interest, one fewer hypothesis is tested, reducing the statistical stringency required for the other tests (e.g., via a less-conservative Bonferroni adjustment). Based on this, researchers must define the most appropriate statistical tests. One of the advantages of Registered Report is to allow researchers to use more uncommon statistical tests that fit the best their analysis but that would be rejected.

More generally, outcome-neutral tests can be included in the Stage-1 manuscript to ensure a good internal validity of the experiment. For instance, researchers might want to run their statistical analysis only if their treatment successfully affected participants in the way they expected. Conditioning statistical analysis on the result of a manipulation check might be a way to ensure that the experiment successfully generates the conditions necessary to estimate the treatment effect the authors have in mind. The only limit is that the outcome-neutral test should remain neutral to the main hypotheses the researchers are willing to test.

Alternative methods for sample size definition. In some cases, it might not be possible for the researchers to define an appropriate sample size with an *a priori* power analysis or to define a stopping rule for data collection. In these cases,

¹¹Calculating statistical power based on the observed effect size would indeed be a form of tautology: observed effect sizes that are not statistically significant are indeed more likely to have low statistical power. See Althouse (2021) for a brief discussion.

researchers can justify the sample size in other ways. Lakens (2022) describes five alternative ways of justifying sample size. First, the author notes that, whenever researchers can access the entire population under consideration (or almost all of it), they require no sample size justification. Ultimately, when the researchers have population data, they do not do statistical tests, as there is no uncertainty about the population's parameters (i.e., they are observed). Second, researchers might face constrained resources, which can limit the amount of data they can collect. Lakens suggests that this happens much more often than it is usually discussed, as researchers are reluctant to spend all their budget on one study, and thus face a trade-off between the value of the information the data can provide and the costs of collecting these data. Collecting a few observations can still be preferable to collecting no data as (i) it might still do better than a simple coin flip, and (ii) these data might contribute to a later meta-analysis. In this case, researchers might state clearly their resource constraints and report the statistical power of their sample size. Third, researchers might seek a given level of precision for their estimates. In this case, they can justify the sample size by looking at the confidence interval of their estimates. For instance, we know that the standard error is a function of the square root of the sample size, such that for any assumed standard deviation, researchers can compute the sample size that will yield a desired confidence interval for the mean. Fourth, the author mentions that some authors might be willing to use heuristics like rules of thumb to justify sample size. However, these heuristics are often “based on weak logic, and not widely applicable”, which is why we do not recommend it. Last, researchers might not necessarily have an inferential goal, and might therefore not have a good sample size justification. The author recommends here to be honest about it and to state it clearly in the paper. They can still discuss the smallest effect size of interest (see below), the minimal statistically detectable effect, or other statistical measures associated with the chosen sample size.

Sequential Analysis. Alternatively, researchers might be willing to adopt another approach to data collection. Instead of defining a given sample size (either with a power analysis or with other justifications), researchers might want to collect data up to the point where the sample fulfills pre-specified statistical properties. For instance, researchers that are interested in estimating an elasticity might want to have a sufficiently high pre-specified precision level and might want to stop data collection once the confidence interval is sufficiently small. In this case, researchers

might want to implement a sequential analysis which consists in repeatedly analyzing results while data collection is still in progress and stopping when the sample satisfies the desired pre-specified properties.

The main advantage of sequential analysis is that researchers might need to collect less data than originally planned. For instance, if researchers are able to reject the null hypothesis after half of the data are collected, they do not need to gather additional observations to increase statistical power (as they already reject the null), which can reduce costs. However, the associated challenge consists in correcting the statistical analysis to account for the increased risks of Type-1 errors. Analyzing the data at several stages of the data collection increases the chances of wrongfully rejecting the null hypothesis. Intuitively, the more researchers analyze the data at intermediary points, the higher the chances of Type-1 errors. Lakens (2014) provides rationales and examples for implementing sequential analyses and correcting the statistical analyses.

3.3 Significance-level correction

A central requirement for RR analyses is the correction of the significance level for multiple-hypothesis testing. As shown in Section 2, with multiple statistical tests we need to correct for the number of hypotheses to obtain a Family-Wise Error Rate (FWER) of α (where α is the significance level for one single hypothesis test). We discuss below the question of *families* of hypotheses.

Three general methods have been proposed to correct for excessive Type-I errors under multiple-hypothesis testing. We show in the Supplementary Materials an example of implementation using a public good game, a dictator game, and a money burning game. The first and best-known is the Bonferroni adjustment. With this adjustment a researcher who wants to run L statistical tests with the standard $\alpha = 5\%$ significance threshold should reject the null hypothesis if the associated p-value is less than or equal to $\frac{\alpha}{L}$. The implementation of the Bonferroni correction in the power analysis and the final analysis is straightforward.

The Bonferroni adjustment has however been criticized for being too conservative, as it increases Type-II errors (Clarke et al., 2020). A second method, the Holm or Holm-Bonferroni correction, is considered to be more powerful as it keeps the FWER weakly below α but produces fewer Type-II errors. The intuition behind the Holm-correction is to reduce the Bonferroni adjustment according to the number of

remaining hypotheses to be tested. We compare the lowest p-value to the threshold $\frac{\alpha}{L}$, the second-lowest to $\frac{\alpha}{L-1}$, and so on up to the last p-value.¹²

An easier implementation of the Holm-correction is to adjust the p-values. Imagine that we have a series of L p-values that we order from the lowest to the highest (p_1, \dots, p_L). The Holm-adjusted p-values are given by:

$$\tilde{p}_i = \max_{j \leq i} \{ \min\{(L - j + 1)p_j, 1\} \}$$

Consider the following example of the test of the effect of an intervention on pro-social behavior. An experiment is designed with three games: a public-good game, a dictator game, and a money-burning game. In each game, the researchers predict that the intervention will increase pro-social behavior: increase the contribution in the public-good game ($H_{0,PGG} : \theta_{PGG} \leq 0$), increase donations in the dictator game ($H_{0,DG} : \theta_{DG} \leq 0$), and reduce money burning ($H_{0,MB} : \theta_{MB} \geq 0$). The researchers thus have $L=3$ hypotheses to test.

Imagine that the following p-values are obtained: $p_{PGG} = 0.022$, $p_{DG} = 0.01$ and $p_{MB} = 0.12$. With the Bonferroni correction with a significance level of $\alpha = 0.05$, the significance threshold becomes $\alpha/L = 0.0167$. In this case, the null hypothesis would be rejected only for the dictator game. With the Holm correction, the p-values become:

$$\begin{aligned} \tilde{p}_{DG} &= \max\{\min\{(3 - 1 + 1)p_{DG}, 1\}\} = 0.03 \\ \tilde{p}_{PGG} &= \max\{\tilde{p}_{DG}, \min\{(3 - 2 + 1)p_{PGG}, 1\}\} = 0.044 \\ \tilde{p}_{MB} &= \max\{\tilde{p}_{PGG}, \min\{(3 - 3 + 1)p_{MB}, 1\}\} = 0.12 \end{aligned}$$

With the Holm-correction for multiple-hypothesis testing, the researchers would reject the null hypothesis (with $\alpha = 5\%$) for the dictator and public-good games, but not for the money-burning game.

Third, the Romano-Wolf multiple-hypothesis testing correction has gained in popularity in recent years (Romano and Wolf, 2005). This has been shown to provide greater power (i.e. a greater probability of successfully rejecting the null hypothesis) as compared to the Bonferroni and Holm corrections. The main idea behind the

¹²The iterative process stops as soon as the researchers are not able to reject a null hypothesis.

Romano-Wolf correction is that the Bonferroni and Holm corrections assume a worst-case dependence structure among the p-values, which is close to the individual p-values being independent of each other (Clarke et al., 2020). However, if there is dependence between the p-values, the Bonferroni and Holm corrections are too conservative.

The intuition is as follows: if a treatment affects pro-social behaviors, the p-values of the treatment effects in the public-good and dictator games are very likely to be correlated as they (partially) result from the same underlying attitudes. In this case, it is natural for the probability of rejecting a second null hypothesis to be larger after having rejected the first. The Bonferroni and Holm corrections are here too conservative as they do not consider this conditional probability. The Romano-Wolf correction takes the dependence between the p-values into account by resampling the data and estimating the dependence structure of the p-values.

Romano and Wolf (2016) provide the following method to calculate the adjusted p-values. In the original dataset, first calculate the L t-statistics to test the L hypotheses ($t_l = \frac{\hat{\theta}_l}{\hat{\sigma}_l}$). Then rank the t-statistics from the largest to the smallest (t_1, \dots, t_L). Second, resample the dataset B times. For each resampling, calculate a standardized statistic $\tilde{t}_l^b = \frac{\hat{\theta}_l^b - \hat{\theta}_l}{\hat{\sigma}_l^b}$, and then the value $T_l^b = \max\{\tilde{t}_l^b, \dots, \tilde{t}_L^b\}$ for each $l = 1, \dots, L$. The adjusted p-value for Hypothesis 1 (i.e. the hypothesis that has the largest absolute t-statistic in the original sample) is $\tilde{p}_1 = \frac{\#\{T_1^b \geq t_1\} + 1}{B + 1}$. The other adjusted p-values are also corrected for monotonicity: $\tilde{p}_l = \max\{\frac{\#\{T_l^b \geq t_l\} + 1}{B + 1}, \tilde{p}_{l-1}\}$.

Last, note that the Bonferroni, Holm, and Romano-Wolf corrections aim to guarantee a FWER of α . In other words, a FWER-correction with $\alpha = 0.05$ ensures that, out of 100 studies, on average, 5 will report at least one incorrect rejection of a null hypothesis (i.e. 5 studies with at least one Type-I error). Alternatively, researchers may wish to minimize the False Discovery Rate (FDR), which is the expected proportion of false rejections out of all rejections. Some methods, such as the Benjamini-Hochberg correction, focus on the FDR rather than the FWER (Thissen et al., 2002). We here focus on FWER-correction methods as they are more conservative than FDR-correction methods, but some researchers may instead target the FDR.

The price of hypotheses. The correction methods show the statistical costs of multiple-hypothesis testing. The more hypotheses to be tested, the lower the p-values needed to reject the null hypothesis, and, thus, the more observations are

needed to obtain sufficient statistical power. In other words, every additional hypothesis to test (or, more globally, every additional parameter of interest to estimate) comes at a price, whether it be a new outcome variable, a new treatment, or a new subgroup analysis. Consider again the example given in Section 3.1, in which researchers wish to establish whether a treatment affects contributions and punishment in a public-good game. Here, the researchers have to take into account that they are testing two hypotheses. Suppose now that they want to test the effect of an additional treatment on the two outcome variables, and to explore the heterogeneity of the treatment effect with respect to political preferences (e.g., Independent, Left-wing and Right-wing). Exploring every possible combination yields 12 hypotheses to test (2 outcome variables \times 2 treatments \times 3 subgroups), requiring harsher statistical correction (e.g., $\frac{\alpha}{12}$ for the Bonferroni correction).¹³ It then follows that researchers ultimately face a trade-off between the number of hypotheses to test and their physical constraints (financial or organizational constraints on sample size). Research that does not correct for multiple-hypothesis testing will not reveal the true statistical costs of testing multiple hypotheses (by concealing the costs of increased Type-I errors).

Reduction of dimensionality. The cost of multiple-hypothesis testing can be limited by reducing the dimensionality of the data and thus the number of hypotheses. For instance, researchers sometimes expect a treatment to affect a latent attitude that they capture through multiple outcome variables. In this case, they can aggregate outcomes into one single composite variable and test the null hypothesis on this variable only.

Espinosa and Treich (2021) asked whether moderate and radical discourses of animal-advocacy NGOs significantly affect people's willingness to engage in animal welfare. There were four outcome variables: a donation to an animal charity, signing two petitions (one against intensive farming, and one for vegetarian meals at school), and a subscription to a newsletter to help the adoption of plant-based diets. Here, a Bonferroni adjustment with $\alpha = 0.05$ would imply the rejection of the null hypothesis if the p-value is below 0.0125. However, the authors instead decided in

¹³List et al. (2019) propose a novel and less-restrictive approach to deal with the simultaneous testing of null hypotheses. The results show improvements over the Holm and Bonferroni corrections, but continue to indicate the price of testing an additional hypothesis.

the pre-registered plan to run a principal component analysis (PCA) on these four dimensions and then carry out the statistical tests on this composite indicator. As such, the null hypothesis is rejected if the p-value is 0.05 or below.

Reducing the dimensionality of the outcome variables is an attractive remedy to the cost of multiple-hypothesis testing. The challenge is to ensure that all of the results of the composite index (which can be any pre-specified transformation of the outcome variables) successfully capture the latent dimension over which we expect the treatment to have an effect. In the case of a PCA, we can check from pilot data that the first dimension does indeed correspond to the latent factor we have in mind. We can also pre-specify the dimensions that will be retained based on their correlation with the original outcome variables (e.g., to be positively correlated with X and Y, and negatively correlated with Z). Alternatively, we can set up an outcome-neutral test (see above) and commit to run the statistical test only if the PCA yields results that are consistent with the expectations set out in the analysis plan.

Families of hypotheses. The above correction methods (Bonferroni, Holm and Romano-Wolf) aim to produce a Family-Wise Error Rate of α . The correction must therefore take into account the number of hypotheses within each *family* of hypotheses/tests. The challenge is then how to define statistical families. As Dienes (2022) underlines, it could be said that a family consists of all of the tests relevant to a theory, but theories actually appear in hierarchies. For instance, we could run several series of tests, but if the series come from the same general theory, should we consider all of the tests as part of the same family?

List et al. (2019) argue that dependence between null hypotheses arises for at least three reasons: when a treatment can affect several outcomes, when a treatment can affect one outcome but differently so across subgroups (a heterogeneous treatment effect), and when there are multiple treatments of interest that can affect the same outcome. Taken separately, each refers to a family of hypotheses that are dependent, as they share the same treatments or outcome variables. At one extreme, all of the hypotheses in a study are in the same family. This is the case for most experiments in economics. For instance, if we look at the effect of introducing cheap talk into a public-good game on both punishment and contribution decisions, the two hypotheses are in the same family. Equally, if we further ask whether introducing centralized sanctions could also affect the same punishment and contribution

decisions via an additional treatment, all four hypotheses will also be in the same family.

At the other extreme, we can think of analyses with distinct families of hypotheses. Consider for example an experiment with four samples (two control and two treatment groups) and two separate treatments, and which analyzes distinct outcome variables. In this case, the two series of hypotheses would not be in the same family, and the number of hypotheses in each family should be controlled for separately. Ultimately, the question is whether the two series of hypotheses stem from the same theory (i.e. the effect of social norms on pro-social behaviors). As Dienes (2022) underlines, if the two series of hypotheses aim to test the same theory of how social norms affect contribution decisions, they are part of the same family.

3.4 The smallest effect size of interest

Statistical significance is not the same as economic significance (McCloskey and Ziliak, 1996a, Ziliak and McCloskey, 2004). Many interventions may have a statistically-significant effect on behavior, but with an effect size that is economically negligible. As we can see in big-data analysis, many variables will have statistically-significant impacts with a large-enough number of observations, as confidence intervals shrink as a result.

Researchers must therefore not only focus on the statistical significance of their results but also their expected social impact, i.e. their economic significance. For instance, if an intervention significantly increases charity donations by 1 dollar per individual but costs 1.2 dollars to implement it is not economically viable. However, were the intervention to increase donations by over 1.2 dollars it becomes economically attractive. More generally, economists should identify interventions for which the benefits outweigh the costs (Glennerster and Takavarasha, 2013).

Previous work has proposed to consider the *Smallest Effect Size Of Interest* (SESOI) in RRs (Lakens, 2014), i.e. the minimum effect size s below which an intervention is not economically significant. Researchers then wish to evaluate not only whether an intervention has an effect that is different from 0 ($H_0 : \theta \leq 0$), but also whether this estimated effect is significantly larger than the SESOI ($H_0 : \theta \leq s$).

Defining the SESOI. This is an important step in the RR analysis, as it produces the set of hypotheses to test. If the SESOI is not set before data collection,

it becomes vulnerable to p-hacking or HARKing in the same way as standard hypotheses. The difficulty is to define ex-ante the appropriate SESOI. A number of procedures have been proposed in the literature to define the SESOI. Lakens et al. (2018) distinguish objective justifications (e.g., theory-driven hypothesis, minimal clinically important difference) and subjective justifications (e.g., using benchmarks, related studies, or smallest observed effect size that could have been significant in a previous study) for choosing the SESOI. Dienes (2021) presents four heuristics for the choice of SESOI. Researchers can first use the opinion of end users or experts to determine the minimal effect that is of interest. For example, a firm might be willing to implement a nudge on its platform only if it increases sales by over 1 percent. Second, researchers can determine the SESOI on an outcome variable by looking at its impact on a third variable. For example, we might want to support employment by reinforcing job training. Imagine that previous work had shown that a two percentage-points rise in job training increased the probability of employment by one percentage point. Further assume that the job-training policy under consideration is economically worthwhile if it increases the probability finding a job by at least 5 percentage points. In this case, the SESOI for the intervention on job training probability would be 10 percentage points. Third, if previous work has considered the same outcome variable (as in replication studies or meta-analyses), we can take the lower bound of the 95% Confidence Interval in these analyses as the SESOI. Last, we can take into account the economic importance of an effect. For instance, a policy-maker may wish to implement a policy only if it increases social welfare. The benefits of the policy must therefore outweigh the costs, implying a SESOI that is equal to the expected costs of the policy.

Power analysis with SESOI. The use of a SESOI can be twofold. First, it can be used to discuss whether the observed effect size is economically relevant. Second, it can also be used in the power analysis. On the one hand, researchers might indeed be willing to design their study so that they have at least 80% chance of rejecting the null hypothesis of no effect when the true effect size is equal to the SESOI. On the other hand, the researchers might be interested in rejecting the null hypothesis of an effect smaller than the SESOI, like in the Sequential Unilateral Hypothesis testing (SUHT) process described below. In this case, the authors compute the probability to successfully claim that the effect size is significantly larger than the SESOI given an expected effect size (that is different from the SESOI).

Consider again the public-good game where the intervention is expected to increase contributions. First, researchers could be willing to have a 80% chance of detecting an effect size statistically larger than zero ($H_0 : \theta \leq 0$), and would use the SESOI as a true effect size in the simulations. Second, researchers could seek to evaluate whether the intervention is economically attractive, and would like to ensure that they have at least 80% chance of detecting an effect statistically larger than the SESOI ($H_0 : \theta \leq s$). They would use here their expected effect size as the true effect size in their simulations.

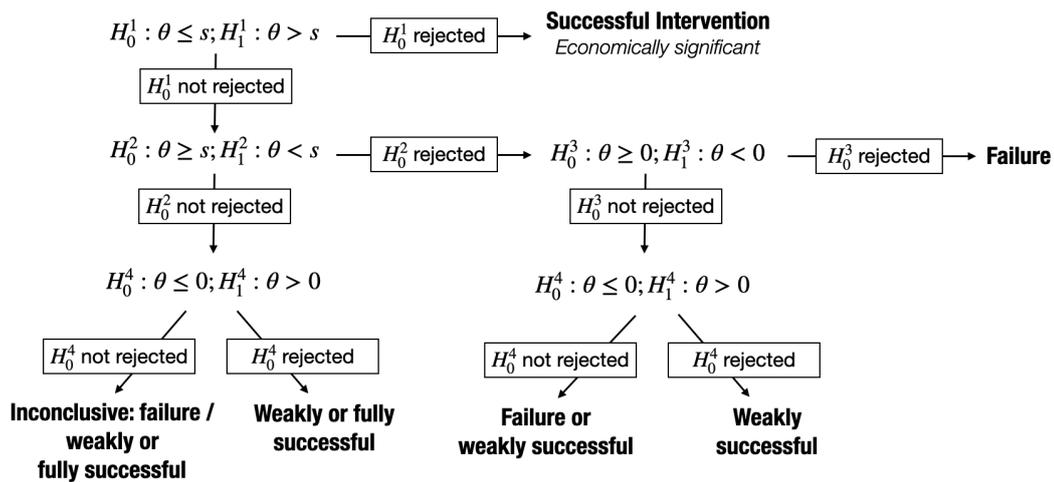
Equivalence testing. It is well-known that the absence of evidence is not evidence of absence (Altman and Bland, 1995). However, when researchers fail to reject the null hypothesis of no effect, they can still learn from the data and find some evidence for no effect (Dienes, 2021). Researchers willing to use all the information conveyed by the data can use *equivalence testing*. As Lakens (2017) notes, researchers who fail at rejecting a null hypothesis might be willing to report to which extent their findings tend to support the null. In equivalence tests, researchers are able to define an upper and lower equivalence bound using the SESOI. For instance, with the two one-sided tests (TOST) procedure, researchers can run two successive one-sided tests to figure out whether the observed effect size is between two boundaries (i.e., whether the effect size is significantly larger or smaller the SESOI). Several resources are available for equivalence testing. Lakens (2017) presents several equivalence tests for t-test, correlations, and meta-analyses. Lakens et al. (2018) distinguish different possible outcomes following equivalence testing, and discuss the statistical and practical significance.

Sequential Unilateral Hypothesis testing. More generally, successive unilateral testing can provide important information to the researchers even in case of positive results. We provide here an example of a process, which we call the sequential unilateral hypothesis testing (SUHT) taken from Espinosa et al. (2021). Imagine that researchers want to estimate the effect of an intervention (θ) and have pre-defined a SESOI of $s > 0$. We can distinguish between a number of scenarios, as summarized in Figure 3.3. We can first determine whether the intervention has an economically-significant impact on the outcome variable, i.e. whether $\theta > s$, by testing $H_0^1 : \theta \leq s$. If we reject the null hypothesis, we can conclude that the intervention does have an economically-significant impact. If we do not reject H_0^1 , we

can still learn whether the data supports a negative result, a zero result, or whether the statistical evidence is too weak to draw a conclusion.

If H_0^1 is not rejected, we can ask whether the intervention has an effect significantly below the SESOI ($H_0^2 : \theta \geq s$). By rejecting H_0^2 , we know that the intervention has an effect that is under s . We can then further determine whether the intervention is a failure (reducing the outcome variable, $H_0^3 : \theta \geq 0$), weakly successful (increasing the outcome variable by between 0 and s , $H_0^4 : \theta \leq 0$), or is one or the other of the two scenarios (failure or weakly successful) but we do not have sufficient statistical power to determine which. If we do not reject H_0^2 , we cannot exclude the possibility that the intervention is economically/fully successful (an effect size above s). We can still however determine whether we can reject that the intervention is a failure (with a negative effect on the outcome variable, H_0^4).

Figure 3.3: Illustration of the Sequential Unilateral Hypothesis Testing (SUHT)



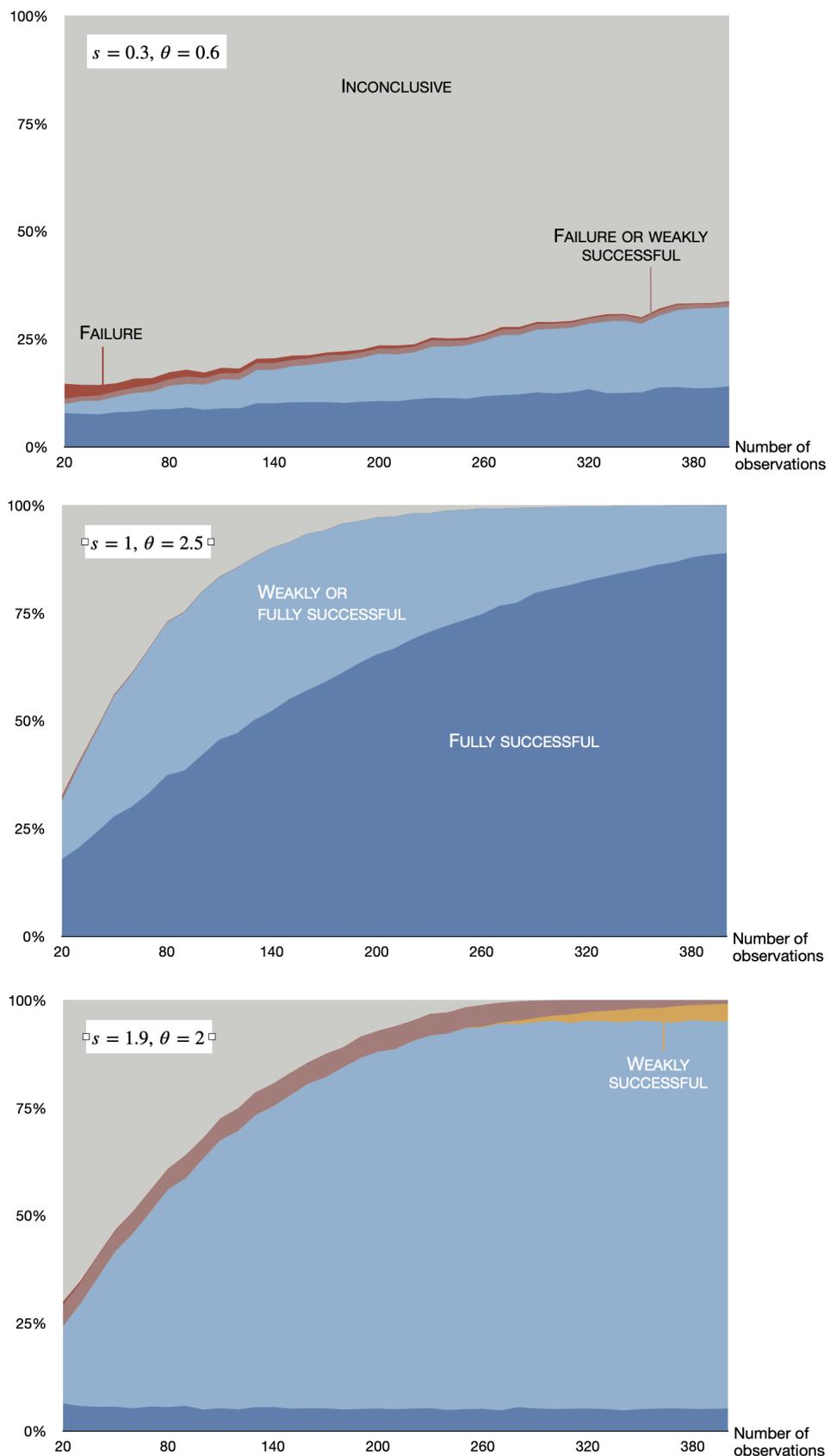
Note that all of these tests have to be run with the appropriate significance level, via Bonferroni or Holm corrections if more than one outcome variables are considered. Although we explore several null hypotheses for the SESOI analysis (from H_0^1 to H_0^4), all of the tests apply to the same outcome variable and are therefore counted as a unique hypothesis in terms of multiple-hypothesis testing. This process is equivalent to evaluating the confidence interval of the estimated coefficient. The decision rule shown in Figure 3.3 can also be expressed in terms of confidence intervals, as in Figure C1 in the Appendix. The decision rule with successive null

hypotheses is easier to implement, given that we are interested in one-sided tests (i.e. whether the treatment effect is *larger* than the SESOI).

To illustrate how power analysis can be used with the SUHT process, consider again the public-good game where the intervention is expected to increase contributions. We assume that the outcome variable is normally-distributed in the control group with mean of 8 and standard deviation of 5, and is bounded between 0 and 20. Our objective here is to compute the probability to detect an effect size larger than the SESOI (i.e., to claim that the intervention is fully successful). Our power analysis thus considers the null hypothesis $H_0 : \theta \leq s$. Whenever we fail to reject this hypothesis, we go through the SUHT process and report our conclusion. We can then compute the average probability of each conclusion given our assumed effect size, SESOI, DGP and sample size. We consider here three scenarios: (1) $s = 0.3$ and $\theta = 0.6$; (2) $s = 1$ and $\theta = 2.5$; and (3) $s = 1.9$ and $\theta = 2$.

Figure 3.4 displays the results for the three scenarios. First, the probability of rejecting a null effect rises with the number of observations, as expected. This corresponds to the dark blue (fully-successful treatment), light blue (weakly- or fully-successful treatment) and orange (weakly-successful treatment) areas on the graph. On the contrary, the share of inconclusive results (grey), failed treatments (dark red) and failed or weakly successful treatments (light red) falls with the number of observations. Interestingly, the share of cases corresponding to weakly- or fully-successful treatments (light blue) is not necessarily a monotonic function of the number of observations ($s = 1$ and $\theta = 2.5$). Third, the closer the SESOI is to the expected effect, the smaller the probability to conclude that the treatment is fully successful.

Figure 3.4: An example of a power analysis in a public-good game with a Smallest Effect Size Of Interest (SESOI)



3.5 Exploratory analysis and deviations from Stage-1

The main benefit of RRs is to clarify the distinction between exploratory and confirmatory analyses. Exploratory analyses are useful for science, as they enable researchers to look for potential associations that they had not expected or uncover the most appropriate way of estimating the associations given the distribution of the data. In confirmatory analyses, researchers commit to the test of a hypothesis that is derived from theory or previous exploratory analyses. The two types of analyses have different objectives: exploratory analyses look for unexpected associations between variables while minimizing the risk of false negatives, while confirmatory analyses aim to establish an expected relationship between variables while minimizing the risk of false positives. Confirmatory analyses limit the extent of innovation but offer stronger statistical evidence than exploratory analyses.

The two types of analyses can both contribute to science if there is a clear distinction between them. RRs do not prevent researchers from learning from the data, but are mostly designed for confirmatory analyses. When researchers detect unexpected and interesting results in their data, they have two options. The first is to report their findings in a dedicated section in the manuscript that clearly notes that the analyses were not included in Stage-1 (Chambers et al., 2014). Alternatively, if the data contain numerous findings that were not in Stage-1, researchers can write a separate article based on these additional findings. In this case, the findings must be transparently reported as exploratory or post-hoc results, and the original RR can still be published. Ideally, the original RR is published before, but in some cases it might be acceptable if the follow-up study is published first (e.g., original Stage-1 RR or preprint is available online). The original RR must be cited to ensure that readers know upfront that the study was not registered and will take into account the weaker statistical strength of the evidence.

In RRs, authors should primarily discuss the registered results of their work in the abstract and in the conclusion to avoid any confusion between the two types of statistical results. Exploratory and confirmatory analyses can then be seen as the two extreme points on a range reflecting the researcher's degree of freedom. Deviations from Stage-1 can be seen as a shift in this dimension, i.e. a departure from the perfect restriction of the researcher's degree of freedom. Deviations from Stage-1 can of course be justified, as researchers cannot necessarily anticipate all of the aspects of the data collection and analysis. However, they do introduce ex-post

arbitrariness into the analysis and, therefore, reduce the statistical strength of the evidence. Researchers should limit any deviations from Stage-1, but should be open to doing so when a central element of the study is at stake.

In all cases, researchers must contact the editor as soon as possible. If the deviations from Stage-1 are discussed before data collection but after the Stage-1 was accepted, researchers should explain these deviations in a footnote in the Stage-2 manuscript and should mention that the agreement of the editor was obtained before data collection. If deviations are discussed after data collection, the editor can decide either to accept the deviation (which must be explicitly mentioned in the Stage-2) or to ask the authors to stick to their commitment. Generally, we recommend to use the second solution and to present the alternative analysis in the exploratory result section.

3.6 Levels of Registered Reports

Registered reports follow a set of rules and procedures that have to be respected to ensure the quality of results. Even though it is optimal to write a registered report without any existing data prior to In-Principle Acceptance (IPA), there may be scenarios where the authors have already collected or have access to some form of existing data and wish to commit to the registered-report format. Here, the quality of the results is affected, but a registered report is still possible. To address this issue and reduce the bias from prior data observation, PCI-RR¹⁴ proposes a scale to assess the quality of a registered report prior to IPA. Authors self-select the level that applies to their study on a scale from 1 (the lowest quality) to 6 (the highest).

Any registered report based on uncollected data (i.e. data that will be generated after the IPA) is automatically given the highest grade, as the authors cannot see the data prior to the IPA. This criterion only applies to data that will be analyzed in Stage-2 and not to any pilot data (e.g., that collected to calibrate the power analysis). The other five levels refer to existing data and reflect the authors' data access prior to the IPA. Level 5 applies if the authors rely on existing data but do not have current access to it (e.g., the data is guarded and will only be accessible post-IPA). In Level 4, the data is accessible but the authors certify that they have not downloaded or

¹⁴Peer Community In Registered Reports (PCI RR) is a researcher-run, non-profit and non-commercial platform that reviews and recommends pre-prints RRs.

accessed it in any form. Level 3 is an extension of Level 4, with the difference that the authors have access to the data but certify that they have not observed it yet. In Level 2, the authors have access to the data and have already observed most of it, but not enough to answer their research question. Level 1 is the same as Level 2, except that the authors have observed enough data to be able to answer their research question but have not carried out their pre-registered analyses. Levels 1 through 4 are subject to bias and asymmetric information, as they rely mostly on the researchers' honesty. The PCI-RR thus recommends that authors in these categories adopt multiple steps to minimize the bias (e.g., conservative thresholds, blinded analysts, and robustness testing). Table 3.2 sums up these registered-report levels and provides specific examples for each level.

Assessing the level of a registered report is important for both researchers and journals. For the former, this allows the self-identification of the degree of familiarity with the data and the decision of whether a registered report is the appropriate format. For example, if a researcher has already collected data and wishes to publish in a journal that only accepts Level-6 submissions, a registered report might not be appropriate. This step allows for self-selection and ensures the quality of the results post-IPA. For journals, this reduces the risk of bias from prior data observation and guarantees ex-ante hypotheses formulation. Requiring a minimum level of registered reports acts as a quality signal for journals, and a number have set minimum quality levels. Journals such as *Nature Human Behaviour* and *Experimental Psychology* only accept RRs for which prior data observation is impossible (Level-6 submissions), while *Cortex* and *Royal Society Open Science* accept submissions as low as Level 2. In economics, the *Journal of Development Economics* (JDE) only accepts Level-6 submissions.¹⁵

¹⁵All of the information for the submission of registered reports to the JDE are available on the dedicated website: <http://jde-preresultsreview.org/>. The *Journal of Political Economy* and *Q-Open* have not specified any submission level for RRs at present.

Table 3.2: PCI-RR levels of Registered Reports with examples.

Level of registered report	Data availability	Data do not already exist prior to IPA	Data are not accessible by researchers	Data have not been accessed by researchers	Data have not been observed by researchers	Key variables have not been observed by researchers	Researchers have not already analyzed key variables	Risk of bias due to data observation before IPA	Additional steps to minimize bias
6	No	✓	✓	✓	✓	✓	✓	Very low	No
	<i>Example:</i> A researcher wants to run an RCT. She collected pilot data for power analysis but none of the final data were collected before In-Principle Acceptance (IPA).								
5	No	✗	✓	✓	✓	✓	✓	Very low	No
	<i>Example:</i> A researcher ran an experiment at the university's lab. The data are kept by the lab manager and are not accessible by the researcher before IPA (e.g., secured access with codes not yet granted).								
4	Yes	✗	✗	✓	✓	✓	✓	Low	No
	<i>Example:</i> A researcher ran an experiment at the university's lab. The data are kept by the lab manager on an internal server and are available for download. The researcher certifies that she has neither downloaded nor accessed the data before IPA.								
3	Yes	✗	✗	✗	✓	✓	✓	Moderate	No
	<i>Example:</i> A researcher ran an experiment at the university's lab. She has access to the data but certifies that she has not opened them before IPA.								
2	Yes	✗	✗	✗	✗	✓	✓	High	Needed
	<i>Example:</i> A researcher ran an experiment at the university's lab. She has access to the data and took a look at some of the <u>secondary</u> variables that will help her answer the research question. She certifies that she has not sufficiently explored the key variables before IPA.								
1	Yes	✗	✗	✗	✗	✗	✓	Very high	Needed
	<i>Example:</i> A researcher ran an experiment at the university's lab. She has access to the data and took a look at some of the <u>key</u> variables that will help her answer the research question. She certifies that she has not yet performed any of the preregistered analyses before IPA.								

3.7 Checklist

The above discussion has focused on the most technical aspects of the RRs. In Table 3.3, we provide a checklist of the elements that must be included in a Registered Report, adapted from Olken (2015). This list can be used by authors when writing their RR or by referees when reviewing a paper. Each category of the checklist is important, as leaving one category unaddressed can substantially increase the researcher's degrees of freedom and, thus, the risk of the inflation of positive results. The advantage of RR over pre-registration without review is that referees and editors can make sure that all of the aspects are covered prior to data collection. It is also beneficial for researchers, who can ensure that they have not omitted an important aspect that could otherwise lead to rejection after data collection.

3.8 Study-design table

Last but not least, some journals require authors to submit a study-design table together with their paper. This table summarizes the different elements of the analysis plan. A number of variants of the study-design table exist, but this must at least include for each research question the associated hypothesis (prediction), sampling plan, analysis plan, and interpretation given to the different outcomes. Study-design tables are very valuable for referees and editors to clearly identify the key elements of the registered reports.

In Section 3.1, we gave an example of researchers who wish to test whether an intervention had an impact on contributions and punishment in a public-good game. The associated study-design table could be presented as in Table 3.4.

Table 3.3: Checklist for Registered Reports

Item	Check
Outcome variables	Do the authors explain their outcome variables and how they will be constructed?
Hypothesis testing	Do the authors describe which tests they will run and the total number of tests?
Sample size	Do the authors provide a good rationale for the sample size?
Covariates	Do the authors explain which covariates they plan to include in a multivariate analysis (if relevant)?
Exclusion rule	Do the authors explain how the final sample will be constructed by setting out the exclusion and inclusion rules?
Statistical model specification	Do the authors explain in detail which statistical model they plan to use (e.g., a linear model, clustered standard errors, estimation by ML or GMM, etc.)?
Power analysis	Do the authors provide a power analysis that shows their tests' expected statistical power (in the statistical model that they committed to use)?
Outcome-neutral tests	Do the authors make some tests conditional and, if so, do they describe the conditions?
Subgroup analysis	If the authors plan to explore the heterogeneity of the treatment effect, do they set out how they plan to do so? Do they also provide a statistical power analysis?
Significance adjustment	Do the authors explain how they plan to account for multiple-hypothesis testing (e.g., a Bonferroni, Holm, or Romano Wolf adjustment)?
Smallest effect size	If the authors discuss the economic significance of their results, do they adequately explain the smallest effect size of interest?
Exploratory analyses	At Stage-2, if the authors discuss unregistered results, do they clearly state that these results were not registered?

Table 3.4: Example of a study-design table

Question	Hypothesis	Sampling plan	Analysis Plan	Interpretation
Does the intervention affect contributions in the public-good game?	The intervention increases contribution levels.	240 observations from the experimental laboratory of the University of Rennes (6 sessions of 20 participants in each condition). Collected between February and March 2023. Exclusion of participants who did not pass the comprehension questions. The sample size was determined using power analysis simulations calibrated with data from a previous study (see Appendix).	OLS estimation with individual fixed effects and clustered at the group level (random effect Tobit if the number of highest or lowest possible values exceeds 50%). Controls include: age, gender, political self-placement, and the Global Trust Scale. We test $H_0^1 : \theta_1 \leq 0$ with $\alpha = 0.05$ and Holm-adjusted p-values for two hypotheses. If we fail to reject H_0^1 , we will run a equivalence test with a SESOI of a 1 point increase of contributions.	If we reject H_0^1 , we will consider that the intervention increases contributions to the public good. If we fail to reject H_0^1 and run the equivalence test, we will conclude that the intervention has no or a negligible impact on contributions if the estimated effect size is within the boundaries, and will conclude that the effect on contributions is uncertain if we cannot reject that the effect size is outside the boundaries.
Does the intervention affect punishment in the public-good game?	The intervention reduces punishment points.	240 observations from the experimental laboratory of the University of Rennes (6 sessions of 20 participants in each condition). Collected between February and March 2023. Exclusion of participants who did not pass the comprehension questions. The sample size was determined using power analysis simulations calibrated with data from a previous study (see Appendix).	OLS estimation with individual fixed effects and clustered at the group level (random effect Tobit if the number of highest or lowest possible values exceeds 50%). Controls include: age, gender, political self-placement, and the Global Trust Scale. We test $H_0^2 : \theta_2 \geq 0$ with $\alpha = 0.05$ and Holm-adjusted p-values for two hypotheses. If we fail to reject H_0^2 , we will run a equivalence test with a SESOI of a 0.5 point increase of punishment.	If we reject H_0^2 , we will consider that the intervention reduces punishment. If we fail to reject H_0^2 and run the equivalence test, we will conclude that the intervention has no or a negligible impact on punishment if the estimated effect size is within the boundaries, and will conclude that the effect on punishment is uncertain if we cannot reject that the effect size is outside the boundaries.

3.9 Ethics approval for stage-1

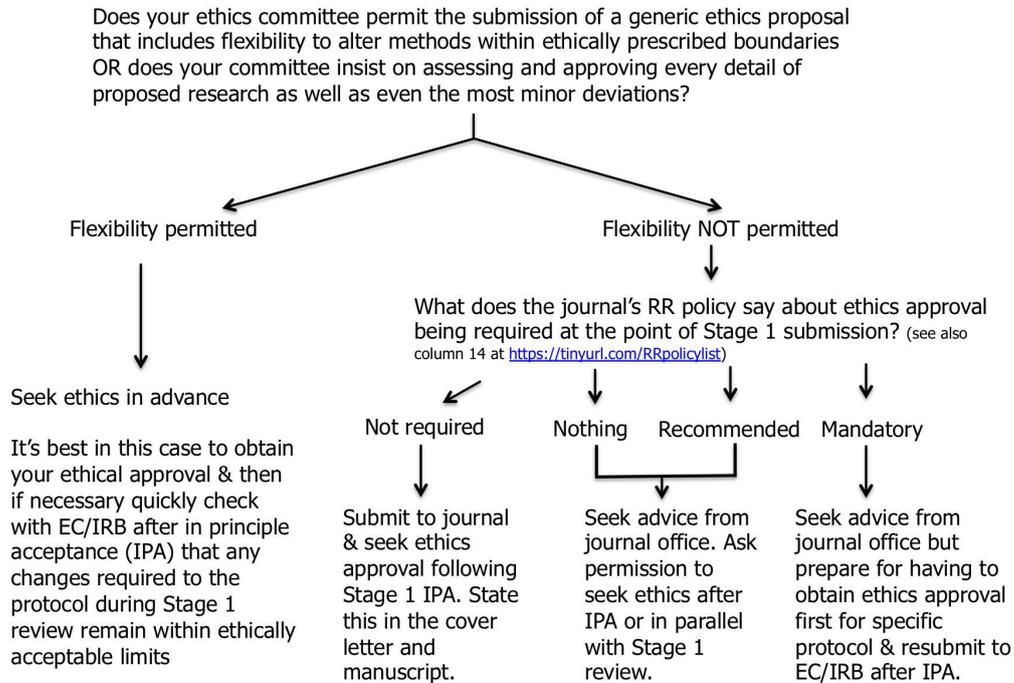
Researchers may need to take into account the flexibility of their ethics committee before submitting a Stage-1 RR. Ethics committees have different approval methods, and journals have different ethics requirements that may influence the flow of a Stage-1 submission. Researchers need to check whether their ethics committee allows for deviations from the original proposal and if deviations must be re-evaluated and re-approved. If the ethics committee is flexible and allows for minor deviations, researchers may seek ethics approval before Stage-1 submission so that any (reasonable) suggestion from the reviewers can be taken into account without an ethics proposal resubmission. Researchers who obtained ethics approval for a Stage-1 submission need to ensure that any changes to the experimental protocol made during the Stage-1 reviewing process stay within the ethics approval and should report any major changes.

Whenever the ethics committee is not flexible, researchers should refer to the journal's ethics approval policy for Stage-1 submissions and proceed accordingly. If the journal does not require ethics approval, researchers can submit the Stage-1 and seek ethics approval after receiving the IPA. In case a journal policy does require ethics approval, researchers can seek it after IPA or while the Stage-1 review is ongoing. However, researchers should also contact the journal to request information on the best way to proceed.

Finally, if the journal requests ethics approval, researchers may want to obtain a protocol validation from the ethics committee prior to the Stage-1 submission. Researchers can then proceed to the Stage-1 submission and, if any changes occurred after the Stage-1 review process, seek a second ethics approval once the IPA is secured. All this information is summarized in Figure 3.5 from OSF. As with most RRs procedures, researchers must plan ahead and take into account the specificity of the ethics committee before moving along in the publication process.

Figure 3.5: RRs ethics approval flowchart from the Open Science Framework

Should I seek ethics approval for my project before or after submitting my Stage 1 Registered Report to the journal?



3.10 Choosing the appropriate journal

Researchers might consider several criteria to choose the appropriate journal to submit their RR. First, economists are currently limited by the number of economic journals that accept RRs, although it is very likely to increase in the coming years as it happened in other fields. To this date, we are only aware of the following journals accepting RRs: the Journal of Development Economics, Q-Open, the Journal of Behavioral and Experimental Economics, Entrepreneurship Theory and Practice, Review of Finance, Quality of Life, and the Journal of the Economic Science Association (JESA). The Journal of Political Economy: Micro and Energy Economics further announced that they plan to accept RRs soon. Second, economists might also consider here the type of RRs that are accepted by these journals. For example, JESA has announced that it accepts RRs for replication studies only at this time. It is very likely that journals will develop their own guidelines and will accept only specific types of RRs (e.g., experiments, surveys, meta-analysis, replications).

Third, journals will also differ in the strength of evidence they will require. This relates for instance to the "levels" of RR that we presented in Table 3.2. For instance, *Advances in Methods and Practices in Psychological Science* accepts Level-1 RRs (and above), while *Experimental Psychology* accepts Level-6 RRs only. Another dimension relating to the strength of evidence is statistical power. For instance, *Nature Human Behavior* requires an *a priori* power of 0.95 or higher. The Center for Open Science (COS) offers on its website a useful resource to keep track of some journals that accept RRs.¹⁶

Alternatively, researchers can also submit their RR to PCI-RR. The *Peer-Community-In Registered Reports* is a free and transparent community of researchers that review RRs. Some researchers serve as recommenders, i.e., they act as editors during the peer-review process and can recommend the final manuscript to a list of journals that take part in the initiative. The PCI-RR 'friendly' journals commit to accepting without further peer review any manuscript that achieves a positive final recommendation from PCI RR while also meeting any additional procedural requirements that do not require further scientific evaluation by the journal. Unlike submissions to specific journals, PCI-RR allows for all kinds of RRs (e.g., meta-analysis, replications, novel studies). The level of the RR is clearly mentioned in the recommendation of the manuscript. It allows for incremental RRs and qualitative research. Once the Stage-2 manuscript is accepted, researchers are free to submit their work to a journal that does not participate in PCI-RR. Submitting to PCI-RR presents several advantages (peer-review by experts in RRs, multiple participating journals, larger range of RRs considered, open science) but researchers, especially young scholars, might seek In-Principle-Acceptance of a prestigious journal that is not part of the initiative.

4 Conclusion

The current production of scientific knowledge is subject to the artificial inflation of statistically-significant results. This inflation results from incorrect practices by researchers (p-hacking, HARKing) who anticipate, correctly or incorrectly, publication and citation biases. The inflation of positive results undermines the quality of the scientific evidence produced in economics, as a considerable share of the pub-

¹⁶<https://www.cos.io/initiatives/registered-reports>

lished results are actually statistical noise. New methods have been introduced to improve the robustness of statistical findings and are becoming more popular (pre-registration, registered reports, and replication studies). Page et al. (2021) discuss the merits and blind spots of these methods in addressing the replication crisis. In our view, Registered Reports outperform standard pre-registration: they retain all of the advantages of pre-registration (and even improve them through peer review) while eliminating the pressure to find positive results and ensuring that all steps of the pre-registration are carried out. While some editors might worry about the risks of moral hazard (i.e., reducing the efforts in data collection after in-principle acceptance or withdrawing RRs with positive results), we believe that the reputational costs will prevent this type of practice.¹⁷

In this paper, we have discussed the main elements of registered reports and provided specific examples adapted to experimental economics. Some elements of the RR analysis plan are fairly similar to well-known pre-registered practices in economics (dataset description, exclusion rules, and power analysis). Others are less common but greatly improve the ex-ante statistical specifications (statistical correction for multiple-hypothesis testing, smallest effect size of interest, and outcome-neutral tests). RRs also take an additional step by drawing a clear line between the conclusions derived from the hypothesis testing set out in the analysis plan (confirmatory analysis) and those that were unanticipated and came about during the data-exploration phase (exploratory analysis). Finally, RRs provide a unique revision system with a two-stage procedure and In-Principle Acceptance that allows changes to be made before data collection and guarantees publication regardless of the results.

This paper has aimed to cover all of the materials related to RRs that are relevant in experimental economics. We have on purpose omitted certain materials that are popular in other fields but are less common in economics (e.g., Cohen's d). We have focused here on a frequentist approach, which is the dominant approach in economics, while other fields prefer Bayesian statistical models for hypothesis testing (e.g., the Bayes Factor). A number of journals accept both types of analyses in RR.¹⁸

¹⁷Some journals like *Nature Human Behavior* require authors to sign a statement confirming that if they withdraw their paper after in-principle acceptance, they agree to the journal publishing a short summary of the pre-registered study under a dedicated section.

¹⁸*Nature Human Behavior* and *Cortex* accept Bayes factor analysis (Dienes, 2020). We do not know of any specific journal in economics policy regarding Bayes factor analysis.

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CHAPTER 3

Young, C. and Holsteen, K. (2017). Model uncertainty and robustness: A computational framework for multimodel analysis. Sociological Methods & Research, 46(1):3–40.

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Appendix C

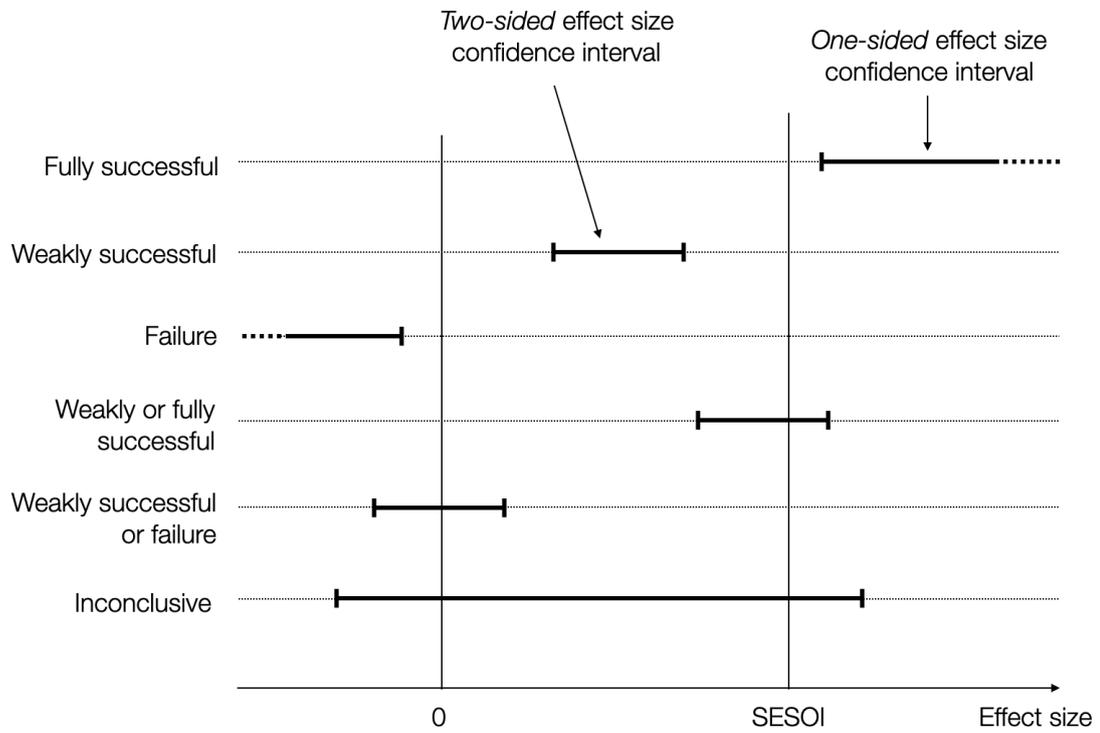
A Practical Guide to Registered Reports for Economists

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C.1 Additional figures

Figure C1: The decision rule with a Smallest Effect Size of Interest (SESOI) with confidence intervals



C.2 Supplementary Materials

C.2.1 The risks of non-registered research in inflating the number of analyses with positive results.

```

1 library(MASS)
2 library(doParallel)
3 S=10000 #Number of simulations
4 alpha=0.05 #Significance level
5 N=300 #Sample size
6
7 J=3 #Number of outcome variables
8 K=3 #Number of control variables
9 L=3 #Number of exclusion rules
10
11 #Define a function for simulations
12 simulatedAnalysis=function(K_func,L_func,J_func,alpha_func,N_
    funct,model_func="all",excl_func="all",covariateSet_func="all
    "){
13
14 #Get all possible combinations for the control variables
15 k_list=rep(list(0:1),K_func)
16 kk=expand.grid(k_list)
17 kk=ifelse(kk==1,TRUE,FALSE)
18 if(covariateSet_func=="one"){
19   kk=matrix(kk[sample(1:K_func,1),],nrow=1)
20 }
21
22 #Get all possible combinations for exclusion rules
23 l_list=rep(list(0:1),L_func)
24 ll=expand.grid(l_list)
25 ll=ifelse(ll==1,TRUE,FALSE)
26 if(excl_func=="one"){
27   ll=matrix(ll[sample(1:L_func,1),],nrow=1)
28 }
29
30 #Counter for number of null rejections
31 numberOfRejections=0
32
33 Y=round(mvrnorm(N_func, mu=rep(10,J_func), Sigma=10*diag(J_
    funct))) #outcome variables

```

```

34 Y=ifelse(Y<0,0,ifelse(Y>20,20,Y))
35 Y_binary=ifelse(Y<10,0,1)
36 #Y_ordered=ifelse(Y<5,0,ifelse(Y<10,1,ifelse(Y<15,2,3)))
37 X=mvrnorm(N_funcnt, mu=rep(0,K_funcnt), Sigma=diag(K_funcnt)) #
   control variables
38 R=matrix(data=rbinom(n=N_funcnt*L_funcnt, size=1,p=0.95), ncol=L_
   funcnt)
39 treatment=ifelse(rnorm(N_funcnt)<0,0,1)
40
41 for(j in 1:J_funcnt){
42
43   if(excl_funcnt=="all") dimLL=dim(l1)[1]
44   if(excl_funcnt=="one") dimLL=1
45
46   for(cond_loop in 1:dimLL){
47
48     inSampleRule=rep(TRUE,N_funcnt)
49     for(sub_loop_cond in 1:L_funcnt){
50       if(l1[cond_loop,sub_loop_cond]==TRUE) inSampleRule=ifelse(R
[,sub_loop_cond]==0,FALSE,inSampleRule)
51     }
52
53     #Select subsample based on the decision rule
54     Y_j_subset=Y[inSampleRule,j]
55     Y_binary_j_subset=Y_binary[inSampleRule,j]
56     #Y_ordered_j_subset=Y_ordered[inSampleRule,j]
57     X_j_subset=X[inSampleRule,]
58     treatment_j_subset=treatment[inSampleRule]
59
60     if(covariateSet_funcnt=="all") dimKK=dim(kk)[1]
61     if(covariateSet_funcnt=="one") dimKK=1
62
63     for(control_loop in 1:dimKK){
64
65       #Select control variables
66       subX=rep(1,dim(X_j_subset)[1])
67       for(sub_loop_control in 1:K_funcnt){
68         if(kk[control_loop,sub_loop_control]==TRUE) subX=cbind(
subX,X_j_subset[,sub_loop_control])
69       }
70
71       #Model selection

```

```

72     if(model_funct=="all") M_funct=4
73     if(model_funct=="one") M_funct=sample(1:3, 1)
74
75     #Linear model
76     if(M_funct==1 | M_funct==4){
77         res=summary(lm(Y_j_subset ~ treatment_j_subset + subX))$
coef[2,]
78         pvalue=res[4]
79         #print(pvalue)
80         if(pvalue<=alpha_funct) numberOfRejections=
numberOfRejections+1
81     }
82
83     #Poisson model
84     if(M_funct==2 | M_funct==4){
85         res <- summary(glm(Y_j_subset ~ treatment_j_subset + subX
, family = "poisson"))$coef[2,]
86         pvalue=res[4]
87         #print(pvalue)
88         if(pvalue<=alpha_funct) numberOfRejections=
numberOfRejections+1
89     }
90
91     #Binary model
92     if(M_funct==3 | M_funct==4){
93         res <- summary(glm(Y_binary_j_subset ~ treatment_j_subset
+ subX, family = binomial(link = "probit"))$coef[2,]
94         pvalue=res[4]
95         #print(pvalue)
96         if(pvalue<=alpha_funct) numberOfRejections=
numberOfRejections+1
97     }
98
99     }
100 }
101 }
102
103 return(numberOfRejections)
104 }
105
106 #Matrix to store the results
107 Results=matrix(nrow=S, ncol=1, data=NA)

```

APPENDIX C

```
108
109
110 #####
111 #STANDARD PROCESS#
112 #####
113
114 #Parallel simulations
115 cores=detectCores()
116 cl <- makeCluster(cores[1]-1) #not to overload your computer
117 registerDoParallel(cl)
118 results_1 <- foreach(i=1:S, .combine='c', .packages='MASS') %dopar%
    {
119     set.seed(i) #Set seed here for reproductibility
120     tempRes=simulatedAnalysis(K,L,J,alpha,N)
121     tempRes
122 }
123 stopCluster(cl)
124 results_1
125
126 #Percentage of cases with at least one rejection
127 #mean(ifelse(results_1>0,1,0))
128
129 #Average number of specifications that reject H0 if at least once
    rejected
130 #mean(results_1[ifelse(results_1>0,1,0)==1])
131
132
133 #####
134 #ONLY ONE ECONOMETRIC MODEL (randomly selected in the simulation)#
135 #####
136
137 #Parallel simulations
138 cores=detectCores()
139 cl <- makeCluster(cores[1]-1) #not to overload your computer
140 registerDoParallel(cl)
141 results_2 <- foreach(i=1:S, .combine='c', .packages='MASS') %dopar%
    {
142     set.seed(i) #Set seed here for reproductibility
143     tempRes=simulatedAnalysis(K,L,J,alpha,N,model_func="one")
144     tempRes
145 }
146 stopCluster(cl)
```

```

147 results_2
148
149
150 #####
151 #ONLY ONE PRE-REGISTERED COMBINATION OF EXCLUSION RULE
152 #####
153
154 #Parallel simulations
155 cores=detectCores()
156 cl <- makeCluster(cores[1]-1) #not to overload your computer
157 registerDoParallel(cl)
158 results_3 <- foreach(i=1:S, .combine='c', .packages='MASS') %dopar%
  {
159   set.seed(i) #Set seed here for reproducibility
160   tempRes=simulatedAnalysis(K,L,J,alpha,N, excl_funct="one")
161   tempRes
162 }
163 stopCluster(cl)
164 results_3
165
166
167 #####
168 #ONLY ONE PRE-REGISTERED COMBINATION OF COVARIATES
169 #####
170
171 #Parallel simulations
172 cores=detectCores()
173 cl <- makeCluster(cores[1]-1) #not to overload your computer
174 registerDoParallel(cl)
175 results_4 <- foreach(i=1:S, .combine='c', .packages='MASS') %dopar%
  {
176   set.seed(i) #Set seed here for reproducibility
177   tempRes=simulatedAnalysis(K,L,J,alpha,N, covariateSet_funct="one"
  )
178   tempRes
179 }
180 stopCluster(cl)
181 results_4
182
183
184 #####
185 #With Bonferroni adjustment

```

APPENDIX C

```
186 #####
187
188 #Parallel simulations
189 cores=detectCores()
190 cl <- makeCluster(cores[1]-1) #not to overload your computer
191 registerDoParallel(cl)
192 results_5 <- foreach(i=1:S, .combine='c', .packages='MASS') %dopar%
  {
193   set.seed(i) #Set seed here for reproductibility
194   tempRes=simulatedAnalysis(K,L,J,alpha/L,N)
195   tempRes
196 }
197 stopCluster(cl)
198 results_5
199
200
201 #####
202 #COMPLETE PRE-REGISTRATION
203 #####
204
205 #Parallel simulations
206 cores=detectCores()
207 cl <- makeCluster(cores[1]-1) #not to overload your computer
208 registerDoParallel(cl)
209 results_6 <- foreach(i=1:S, .combine='c', .packages='MASS') %dopar%
  {
210   set.seed(i) #Set seed here for reproductibility
211   tempRes=simulatedAnalysis(K,L,J,alpha/L,N, model_func="one",
     excl_func="one", covariateSet_func="one")
212   tempRes
213 }
214 stopCluster(cl)
215 results_6
216
217
218 #####
219 #Save results
220 #####
221
222 setwd("/Users/espinosaromain/Dropbox/Recherche/Guide for RR for
     economists/")
223
```

```

224 fileConn<-file("output.txt")
225 str=paste0("S=",S,", K=",K,", L=",L," ,J=",J,", alpha=", alpha, ",
      M=",3,"\n \n")
226
227 str=paste0(str,"NO PRE-REGISTRATION: ")
228 str=paste0(str,"Share where at least one H0 is rejected: ", round(
      mean(ifelse(results_1>0,1,0))*100,1), "% \n")
229 str=paste0(str,"Average number of rejections if at least one
      rejection: ", round(mean(results_1[ifelse(results_1>0,1,0)==1]
      ,1))
230
231 str=paste0(str,"\n \n ECONOMETRIC MODEL PRE-REGISTERED: ")
232 str=paste0(str,"Share where at least one H0 is rejected: ", round(
      mean(ifelse(results_2>0,1,0))*100,1), "% \n")
233 str=paste0(str,"Average number of rejections if at least one
      rejection: ", round(mean(results_2[ifelse(results_2>0,1,0)==1]
      ,1))
234
235 str=paste0(str,"\n \n EXCLUSION RULE PRE-REGISTERED: ")
236 str=paste0(str,"Share where at least one H0 is rejected: ", round(
      mean(ifelse(results_3>0,1,0))*100,1), "% \n")
237 str=paste0(str,"Average number of rejections if at least one
      rejection: ", round(mean(results_3[ifelse(results_3>0,1,0)==1]
      ,1))
238
239 str=paste0(str,"\n \n COVARIATE PRE-REGISTERED: ")
240 str=paste0(str,"Share where at least one H0 is rejected: ", round(
      mean(ifelse(results_4>0,1,0))*100,1), "% \n")
241 str=paste0(str,"Average number of rejections if at least one
      rejection: ", round(mean(results_4[ifelse(results_4>0,1,0)==1]
      ,1))
242
243 str=paste0(str,"\n \n WITH BONFERRONI ADJUSTMENT: ")
244 str=paste0(str,"Share where at least one H0 is rejected: ", round(
      mean(ifelse(results_5>0,1,0))*100,1), "% \n")
245 str=paste0(str,"Average number of rejections if at least one
      rejection: ", round(mean(results_5[ifelse(results_5>0,1,0)==1]
      ,1))
246
247 str=paste0(str,"\n \n COMPLETE PRE-REGISTRATION: ")
248 str=paste0(str,"Share where at least one H0 is rejected: ", round(
      mean(ifelse(results_6>0,1,0))*100,1), "% \n")

```

```
249 str=paste0(str,"Average number of rejections if at least one
      rejection: ", round(mean(results_6[ifelse(results_6>0,1,0)]=1])
      ,1))
250
251 writeLines(str, fileConn)
252 close(fileConn)
253
254 Results=cbind(results_1,results_2,results_3,results_4,results_5,
      results_6)
255 write.csv(Results,file="ResultsSimulation.csv",row.names=F)
```

Listing C.1: Example of the risks from non-registered studies

C.2.2 Example of power analysis with a public good game

We illustrate this process in the following example. Consider an experiment with two conditions (baseline and treatment) with equal random assignment. Imagine that we are interested in an outcome variable Y that is normally distributed with zero mean and standard deviation of one in the baseline condition. Imagine then that the treatment increases the outcome variable by 0.5 points in the treatment group (i.e. $\theta = 0.5$). We simulate $S = 1,000$ datasets with sample size of N . For each simulation s , we estimate the treatment effect $\hat{\theta}_s$ using a linear regression and report whether we reject the null hypothesis $H_0 : \theta = 0$. The average rejection rate corresponds to the statistical power, i.e. the probability of correctly rejecting the null (as the true parameter θ is different from zero). In Listing C.2 below, we calculate the statistical power for $N = \{50, 100, 150, 200\}$ (see Listing C.7 for the code in Stata). The statistical power when $N = 50$ is 0.423, which means that the probability of successfully rejecting $H_0 : \theta = 0$ is only 42%. In this case, not rejecting the null is not very informative, and the study is said to be underpowered. Statistical power increases to 71.4% when $N = 100$, and 86% when $N = 150$.

```

1 set.seed(123) #Set seed to replicate results
2 vectorN=c(50,100,150,200) #List of sample sizes
3 S=1000 #Number of simulations per sample size
4 alpha=0.05 #Significance level
5 statPower=rep(NA,length(vectorN)) #Vector to store the results
6 for(k in 1:length(vectorN)){ #Loop over sample sizes
7   N=vectorN[k] #Sample size
8   rejectionVector=rep(NA,S) #Vector to store the rejection decision
9   for(s in 1:S){
10    t=rep(c(0,1),N/2) #Random treatment assignment
11    y=rnorm(N,mean=0,sd=1)+0.5*t #Generate data
12    results=summary(lm(y~t)) #Estimate statistical model
13    #Store the rejection decision:
14    rejectionVector[s]=ifelse(results$coefficients[2,4]<=alpha,1,0)
15  }
16  statPower[k]=mean(rejectionVector) #Compute the overall rejection
17  #rate:
18 }
19 statPower #Results: 0.423 0.714 0.860 0.947

```

Listing C.2: Example of a statistical-power estimation in R

This process can be adapted to create a function that returns the statistical

power for any given sample size N and any given value of θ . We show how to transform the code to generate such a function from the previous example.

```

1 #Script for simulation - Public Good Game
2 powerAnalysisSimulation=function(N_funcnt,theta_funcnt,S_funcnt=1000,
3   alpha_funcnt=0.05){
4   set.seed(123) #Set seed to replicate results
5   rejectionVector=rep(NA,S_funcnt) #Vector to store the rejection
6     rate
7   for(s in 1:S_funcnt){
8     t=rep(c(0,1),N_funcnt/2) #Random treatment assignment
9     y=rnorm(N_funcnt,mean=0,sd=1)+theta_funcnt*t #Generate data
10    results=summary(lm(y~t)) #Estimate statistical model
11    #Store the rejection decision:
12    rejectionVector[s]=ifelse(results$coefficients[2,4]<=alpha_
13      funct,1,0)
14  }
15  #Compute the overall rejection rate:
16  return(mean(rejectionVector))
17 }
18
19 #To test the function
20 powerAnalysisSimulation(N_funcnt=200,theta_funcnt=0.10)
21
22 #Graph
23 vectorN=seq(0,600,10)
24 vectorTheta=c(0.15,0.30,0.45)
25 vectorAlpha=c(0.01,0.05)
26 grid=expand.grid(N=vectorN, theta=vectorTheta,alpha=vectorAlpha)
27
28 #Loop
29 grid$power=NA
30 for(i in 1:dim(grid)[1]){
31   if(grid[i,"N"]>0){
32     grid[i,]$power=powerAnalysisSimulation(N_funcnt=grid[i,"N"],
33       theta_funcnt=grid[i,"theta"],
34       alpha_funcnt=grid[i,"alpha"],
35       S_funcnt=10000)
36   }else{
37     grid[i,]$power=0
38   }
39 }

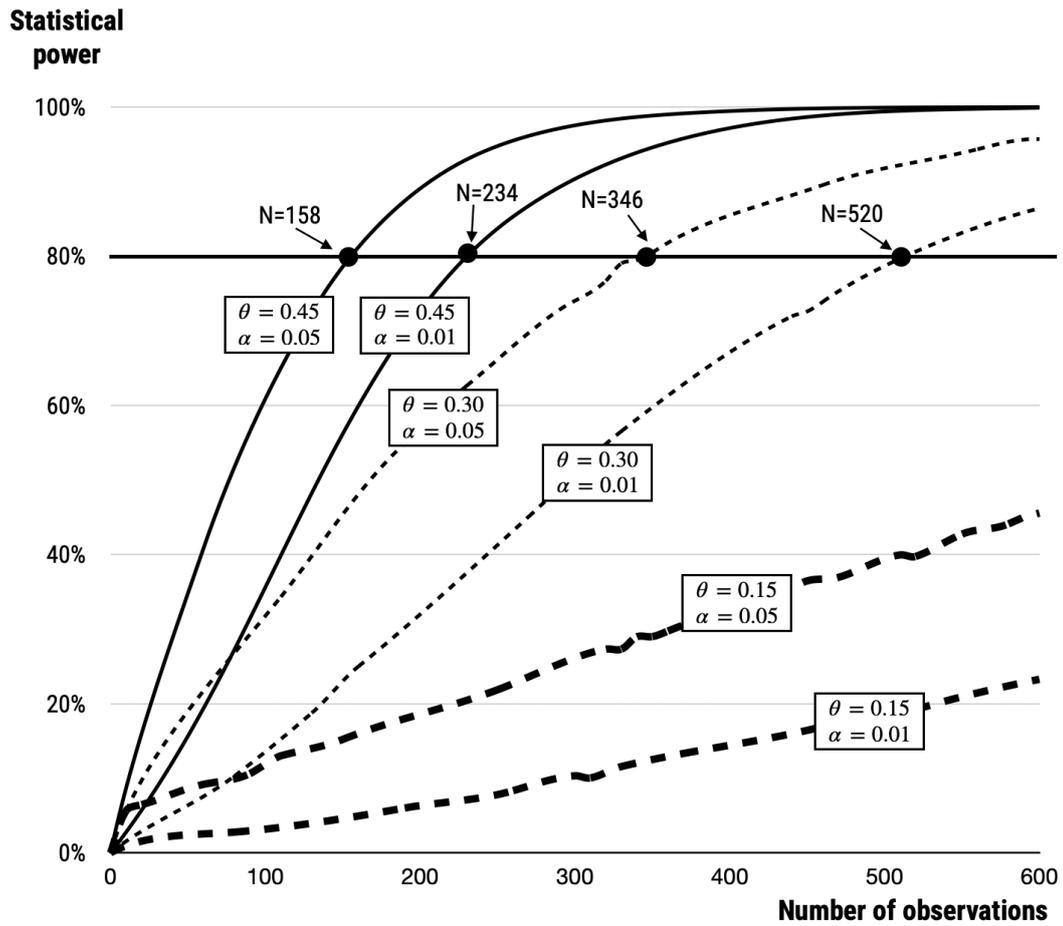
```

```
37
38 #Matrix Results
39 resultsMat=matrix(data=NA,nrow=61,ncol=6)
40 for(k in 1:6){
41   i=61*(k-1)+1
42   j=61*k
43   resultsMat[1:61,k]=grid[i:j,"power"]
44 }
45 resultsMat
46 View(resultsMat)
```

Listing C.3: Example of a function of a power analysis for an experiment

The simulation results are depicted in Figure C2. Using this approach, we can retrieve the minimal number of observations to obtain statistical power of above 80% for various specifications. As expected, the larger the expected treatment effect or the larger the significance level, the fewer observations are needed.

Figure C2: Power analysis of an experimental design



This graph plots an example of a power analysis estimation in an experiment: two conditions (baseline and treatment), equal probability assignment, and a two-sided test after OLS estimation. The outcome variable has mean 0 and standard deviation 1, the treatment effect size is θ , and the significance level is α . The number of simulations is $S=10,000$ and the hypothesis tested is $H_0 : \theta = 0$.

C.2.3 Example of power analysis with multiple-hypothesis testing correction

We now look the way to deal with multiple-hypothesis testing in ex-ante power analysis. With the Bonferroni adjustment, the process is simple as we only need to replace α by $\frac{\alpha}{L}$ in the decision rule to reject the null hypotheses. With the less-conservative Holm approach, we need to rank the p-values for each simulation and apply the decision rule.

Consider the example above of a treatment and pro-social behavior in public-good, dictator and money-burning games. The researchers have directional predictions, and so only run one-sided tests. We consider the following elements:

- The one-shot public-good game: we assume that contributions in the control group have a normal distribution with mean of 8 and standard deviation of 5, and are bounded between 0 and 20. We assume a treatment effect of a 1-point increase. We estimate the treatment effect via a rank-sum test.
- The dictator game: we assume that the share of money given to the receiver in the control group follows a normal distribution with mean of 0.2 and standard deviation of 0.2, and is bounded between 0 and 1. We assume a treatment effect of a 10 percentage-point increase in the share given to the receiver. We estimate the treatment effect via a Tobit model.
- The money-burning game: we assume a binary decision, with a probability of money burning of 35% in the control group. The treatment is expected to reduce the burning of the other participant's money by 10 percentage points. We estimate the treatment effect using a proportion test.

The R code (see Listing C.8 for the code in Stata) for the power analysis with 300 observations (random assignment with equal probability) is as follows:

```

1 library(censReg) #Library for tobit regression
2 set.seed(123) #Set seed to replicate results
3 S=1000 #Number of simulations
4 alpha=0.05 #Significance level
5 N=300 #Sample size
6 rejectionMatrix=matrix(data=NA,nrow=S,ncol=3) #Vector to store the
   rejection rate
7 colnames(rejectionMatrix)=c("PGG","DG","MB")

```

```

8
9 for(s in 1:S){
10   #Generate data
11   t=rep(c(0,1),N/2) #Random treatment assignment
12   y_pgg=rnorm(N,mean=8,sd=5)+t #PGG data
13   y_pgg=round(ifelse(y_pgg<0,0,ifelse(y_pgg>20,20,y_pgg)),0)
14   y_dg=rnorm(N,mean=0.2,sd=0.2)+0.1*t #DG data
15   y_dg=round(ifelse(y_dg<0,0,ifelse(y_dg>1,1,y_dg)),1)
16   y_mb=rbinom(n=N,size=1,prob=0.35-0.1*t) #MB data
17
18   #Statistical tests and pvalues
19   p_pgg=wilcox.test(y_pgg~t, alternative="less")$p.value
20   res=summary(margEff(censReg(y_dg ~ t, left=0, right=1)))
21   p_dg=1-pnorm(res[1]/res[2])
22   p_mb=prop.test(x=c(sum(y_mb[t==0]),sum(y_mb[t==1])),n=rep(N/2,2),
23     alternative = "greater")$p.value
24
25   #Vectors of pvalues
26   vectorPvalues=c(p_pgg,p_dg,p_mb)
27   rankPvalues=rank(vectorPvalues)
28   sortedPvalues=sort(vectorPvalues)
29
30   #Adjusted pvalues
31   adjustedSortedPvalues=rep(NA,length(vectorPvalues))
32   adjustedSortedPvalues[1]=3*sortedPvalues[1]
33   adjustedSortedPvalues[2]=max(adjustedSortedPvalues[1],min(2*
34     sortedPvalues[2],1))
35   adjustedSortedPvalues[3]=max(adjustedSortedPvalues[2],min(1*
36     sortedPvalues[3],1))
37   adjustedPvalues=rep(NA,length(vectorPvalues))
38   adjustedPvalues[1]=adjustedSortedPvalues[rankPvalues[1]]
39   adjustedPvalues[2]=adjustedSortedPvalues[rankPvalues[2]]
40   adjustedPvalues[3]=adjustedSortedPvalues[rankPvalues[3]]
41
42   #Store rejection decisions
43   rejectionMatrix[s,]=ifelse(adjustedPvalues<=alpha,1,0)
44 }
45 #Look at the statistical power
46 colMeans(rejectionMatrix)
47 #PGG    DG    MB

```

```
46 #0.483 0.988 0.494
```

Listing C.4: An example of power analysis using the Holm correction for multiple-hypothesis testing

The power analysis indicates that researchers have a 48.1% chance of successfully rejecting the null hypothesis for the public-good game, a 98.8% change in the dictator game, and a chance of only 49.4% in the money-burning game.

We also show how to estimate power using the Romano-Wolf correction. We use the same data-generating process as above. We now estimate the treatment effect on the public-good and money-burning games with a linear model (OLS) as the Wilcoxon and proportion tests do not produce t-statistics. The R code is displayed in Listing C.5. We here reject the null hypotheses with the following probabilities: 49.7%, 98.8% and 54.1% for the public-good, dictator and money-burning games respectively.

```
1 library(censReg) #Library for tobit regression
2 library(matrixStats) #For rowMaxs
3 library(doParallel)
4 set.seed(123) #Set seed to replicate results
5 S=1000 #Number of simulations
6 alpha=0.05 #Significance level
7 N=300 #Sample size
8 rejectionMatrix=matrix(data=NA,nrow=S,ncol=3) #Vector to store the
   rejection rate
9 colnames(rejectionMatrix)=c("PGG","DG","MB")
10 B=1000 #Number of Bootstraps
11 cores=detectCores() #Number of cores
12 cl <- makeCluster(cores[1]-1, setup_timeout = 0.5) #not to overload
   your computer
13 registerDoParallel(cl)
14
15 for(s in 1:S){
16   #Same Data Generating Process omitted
17
18   #Statistical tests and t-stats
19   est_pgg=summary(lm(y_pgg ~ t))$coef
20   t_pgg=est_pgg[2,1]/est_pgg[2,2]
21   est_dg=summary(margEff(censReg(y_dg ~ t, left=0, right=1)))
22   t_dg=est_dg[1]/est_dg[2]
```

```

23 est_mb=summary(lm(y_mb ~ t))$coef
24 t_mb=-est_mb[2,1]/est_mb[2,2]
25 #Get the opposite value t-stat for money burning
26 #because we test H0: b>0 for this one.
27
28 #Vectors of t-stats
29 vectorTstat=c(t_pgg,t_dg,t_mb)
30 rankTstat=rank(-vectorTstat) #From largest to smallest
31 sortedTstat=sort(vectorTstat, decreasing=TRUE)
32 t1=sortedTstat[1] #Largest t-stat
33 t2=sortedTstat[2]
34 t3=sortedTstat[3] #Smallest t-stat
35
36 matTstat_star_b <- foreach(i=1:B, .combine='rbind', .packages='
  censReg') %dopar% {
37   set.seed(i) #Set seed here for reproductibility
38
39   #Bootstrap dataset
40   data_boot=data_loop[sample(nrow(data_loop), N, replace=TRUE), ]
41
42   #Get t-stats
43   est_pgg_b=summary(lm(data_boot$y_pgg ~ data_boot$t))$coef
44   t_pgg_star_b=(est_pgg_b[2,1]-est_pgg[2,1])/est_pgg_b[2,2]
45   est_dg_b=summary(margEff(censReg(data_boot$y_dg ~ data_boot$t,
left=0, right=1)))
46   t_dg_star_b=(est_dg_b[1]-est_dg[1])/est_dg_b[2]
47   est_mb_b=summary(lm(data_boot$y_mb ~ data_boot$t))$coef
48   t_mb_star_b=-(est_mb_b[2,1]-est_mb[2,1])/est_mb_b[2,2]
49
50   #Store values
51   results_parallel=c(t_pgg_star_b,t_dg_star_b,t_mb_star_b)
52
53   results_parallel
54 }
55
56 #Get the maxima
57 max1=rowMaxs(matTstat_star_b)
58 max2=rowMaxs(matTstat_star_b[,-rankTstat[1]])
59 max3=matTstat_star_b[,-c(rankTstat[1],rankTstat[2])]
60
61 #Vector of adjusted pvalues

```

```

62 adjustedPvalues_sorted=adjustedPvalues_unsorted=rep(NA,length(
    vectorTstat))
63 adjustedPvalues_sorted[1]=(count(max1>t1)+1)/(B+1)
64 p_2_init=(count(max2>t2)+1)/(B+1)
65 p_3_init=(count(max3>t3)+1)/(B+1)
66 adjustedPvalues_sorted[2]=max(p_2_init,adjustedPvalues_sorted[1])
67 adjustedPvalues_sorted[3]=max(p_3_init,adjustedPvalues_sorted[2])
68 for(k in 1:3) adjustedPvalues_unsorted[k]=adjustedPvalues_sorted[
    rankTstat[k]]
69
70 #Store rejection decisions
71 rejectionMatrix[s,]=ifelse(adjustedPvalues_unsorted<=alpha,1,0)
72
73 }
74 stopCluster(cl)
75
76 #Look at the statistical power
77 #PGG    DG    MB
78 #0.497 0.988 0.541

```

Listing C.5: An example of power analysis using the Romano-Wolf correction for multiple-hypothesis testing

C.2.4 Power analysis for SESOI

```

1 library(censReg) #Library for tobit regression
2 set.seed(123) #Set seed to replicate results
3 S=10000 #Number of simulations
4 alpha=0.05 #Significance level
5 N=300 #Sample size
6 rejectionMatrix=matrix(data=NA,nrow=S,ncol=3) #Matrix
7
8 for(s in 1:S){
9   #Generate data
10  t=rep(c(0,1),N/2) #Random treatment assignment
11  y_pgg=rnorm(N,mean=8,sd=5)+t #PGG data
12  y_pgg=round(ifelse(y_pgg<0,0,ifelse(y_pgg>20,20,y_pgg)),0)
13
14  #Statistical tests and pvalues
15  res=summary(margEff(censReg(y_pgg ~ t, left=0, right=1)))
16  p_pgg=1-pnorm(res[1]/res[2])
17
18  #Store rejection decisions
19
20
21  rejectionMatrix[s,1:3]=c(0,0,0)
22
23
24 }
25 #Look at the statistical power
26 colMeans(rejectionMatrix)

```

Listing C.6: An example of power analysis with a SESOI

C.2.5 Code replication with Stata

C.2.5.1 Replication of Listing C.2

```

1 set seed 123 //Set seed to replicate results
2 local S=1000 //Number of simulation per sample size
3 local alpha=0.05 //Significance level
4 set matsize `S' //Set matrix size to store results
5 mat statPower=J(1,4,.) //Vector to store the results
6
7 capture program drop my_sim
8 program my_sim, rclass
9     version 14.2
10    args N_sim alpha_sim
11    tempname b_sim V_sim zscore_sim pvalue_sim rejection_sim
12    tempname y t id
13    drop _all
14    set obs `N_sim'
15    gen `id'=_n
16    gen `t'=`cond(`id'<`N_sim'/2,0,1) //Treatment assignment
17    gen `y'=`rnormal(0,1)+0.5*`t' //Generate data
18    reg `y' `t' //Estimate the linear model
19    mat `b_sim'=`e(b) //Vector of coefficients
20    mat `V_sim'=`e(V) //Var-Covar matrix
21    scalar `zscore_sim'=`b_sim'[1,1]/sqrt(`V_sim'[1,1])
22    scalar `pvalue_sim'=2 * normprob(-abs(`zscore_sim'))
23    scalar `rejection_sim'=`cond(`pvalue_sim'<`alpha_sim',1,0)
24    return scalar reject=`rejection_sim' //Return rejection decision
25 end
26
27 local j=1
28 forvalues N=50(50)200{
29     simulate rejectResults=r(reject), reps(`S') nodots: my_sim `N' `
        alpha '
30     qui su rejectResults
31     mat statPower[1,`j']=`round(`r(mean)',0.001)
32     local j=`j'+1
33 }
34 mat list statPower
35 // .434 .721 .87 .938

```

Listing C.7: The replication of Listing C.2 with Stata

C.2.5.2 Replication of Listing C.4

```

1 set seed 123 //Set seed to replicate results
2 local S=1000 //Number of simulation per sample size
3 local alpha=0.05 //Significance level
4 set matsize `S' //Set matrix size to store results
5 mat statPower=J(1,4,.) //Vector to store the results
6
7 capture program drop my_sim
8 program my_sim, rclass
9     version 14.2
10    args N_sim alpha_sim
11    tempname b_sim SE_sim zscore_sim pvalue_sim
12    tempname y_pgg y_dg y_mb t id
13    tempname pvalue_pgg pvalue_dg pvalue_mb
14    tempname adjustedp_pgg adjustedp_dg adjustedp_mb
15    tempname rejection_pgg rejection_dg rejection_mb
16    drop _all
17
18    //Generate data
19    set obs `N_sim'
20    gen `id'=_n
21    gen `t' = cond(`id' < `N_sim'/2, 0, 1) //Treatment assignment
22    gen `y_pgg' = rnormal(8,5) + `t' //PGG Data
23    replace `y_pgg' = cond(`y_pgg' > 20, 20, `y_pgg')
24    replace `y_pgg' = cond(`y_pgg' < 0, 0, `y_pgg')
25    gen `y_dg' = rnormal(0.2, 0.2) + 0.1 * `t' //DG Data
26    replace `y_dg' = cond(`y_dg' < 0, 0, `y_dg')
27    replace `y_dg' = cond(`y_dg' > 1, 1, `y_dg')
28    gen `y_mb' = rbinomial(1, 0.35 - 0.1 * `t') //MB data
29
30    //Statistical tests and pvalues
31    ranksum `y_pgg', by(`t')
32    scalar `pvalue_pgg' = normprob(r(z))
33    tobit `y_dg' `t', ll(0) ul(1)
34    mfx
35    mat `b_sim' = e(Xmfx_dydx)
36    mat `SE_sim' = e(Xmfx_se_dydx)
37    scalar `zscore_sim' = `b_sim'[1,1] / `SE_sim'[1,1]
38    scalar `pvalue_dg' = 1 - normprob(`zscore_sim')
39    prtest `y_mb', by(`t')
40    scalar `pvalue_mb' = 1 - normal(r(z))

```

```

41
42 //Compute adjusted `pvalues
43 if(`pvalue_pgg` < `pvalue_dg` & `pvalue_pgg` < `pvalue_mb` & `
  pvalue_dg` < `pvalue_mb`){
44   scalar `adjustedp_pgg` = 3*`pvalue_pgg`
45   scalar `adjustedp_dg` = max(`adjusted_pgg`, min(2*`pvalue_dg`, 1))
46   scalar `adjustedp_mb` = max(`adjustedp_dg`, min(`pvalue_mb`, 1))
47 }
48 if(`pvalue_pgg` > `pvalue_dg` & `pvalue_pgg` < `pvalue_mb` & `
  pvalue_dg` < `pvalue_mb`){
49   scalar `adjustedp_dg` = 3*`pvalue_dg`
50   scalar `adjustedp_pgg` = max(`adjustedp_dg`, min(2*`pvalue_pgg`, 1))
51   scalar `adjustedp_mb` = max(`adjustedp_pgg`, min(`pvalue_mb`, 1))
52 }
53 if(`pvalue_pgg` < `pvalue_dg` & `pvalue_pgg` < `pvalue_mb` & `
  pvalue_dg` > `pvalue_mb`){
54   scalar `adjustedp_pgg` = 3*`pvalue_pgg`
55   scalar `adjustedp_mb` = max(`adjusted_pgg`, min(2*`pvalue_mb`, 1))
56   scalar `adjustedp_dg` = max(`adjustedp_mb`, min(`pvalue_dg`, 1))
57 }
58 if(`pvalue_pgg` > `pvalue_dg` & `pvalue_pgg` > `pvalue_mb` & `
  pvalue_dg` < `pvalue_mb`){
59   scalar `adjustedp_dg` = 3*`pvalue_dg`
60   scalar `adjustedp_mb` = max(`adjustedp_dg`, min(2*`pvalue_mb`, 1))
61   scalar `adjustedp_pgg` = max(`adjustedp_mb`, min(`pvalue_pgg`, 1))
62 }
63 if(`pvalue_pgg` < `pvalue_dg` & `pvalue_pgg` > `pvalue_mb` & `
  pvalue_dg` > `pvalue_mb`){
64   scalar `adjustedp_mb` = 3*`pvalue_mb`
65   scalar `adjustedp_pgg` = max(`adjustedp_mb`, min(2*`pvalue_pgg`, 1))
66   scalar `adjustedp_dg` = max(`adjustedp_pgg`, min(`pvalue_dg`, 1))
67 }
68 if(`pvalue_pgg` > `pvalue_dg` & `pvalue_pgg` > `pvalue_mb` & `
  pvalue_dg` > `pvalue_mb`){
69   scalar `adjustedp_mb` = 3*`pvalue_mb`
70   scalar `adjustedp_dg` = max(`adjustedp_mb`, min(2*`pvalue_dg`, 1))
71   scalar `adjustedp_pgg` = max(`adjustedp_dg`, min(`pvalue_pgg`, 1))
72 }
73
74 scalar `rejection_pgg` = cond(`adjustedp_pgg` <= `alpha_sim`, 1, 0)
75 scalar `rejection_dg` = cond(`adjustedp_dg` <= `alpha_sim`, 1, 0)
76 scalar `rejection_mb` = cond(`adjustedp_mb` <= `alpha_sim`, 1, 0)

```

```
77
78   return scalar reject_pgg=`rejection_pgg' //Return rejection
      decision
79   return scalar reject_dg=`rejection_dg' //Return rejection
      decision
80   return scalar reject_mb=`rejection_mb' //Return rejection
      decision
81 end
82
83 simulate rejectResults_pgg=r(reject_pgg) /*
84    */ rejectResults_dg=r(reject_dg) rejectResults_mb=r(reject_mb) /*
85    */ , reps(`S') nodots: my_sim 300 `alpha'
86
87 su rejectResults_pgg rejectResults_dg rejectResults_mb
88
89 //0.432 0.992 0.523
```

Listing C.8: The replication of Listing C.4 with Stata

General Conclusion

Experimental economics is a powerful tool to study individual deviations from rationality. Since seminal work by Tversky and Kahneman (1974), the field of experimental economics has expanded its scope by studying heuristics and biases that distort the predictability of individual decisions. More recently, experimentalists have started applying this framework to study individual food choices. Individuals make, on a daily basis, food-related choices that are affected by economic, physiological, emotional and cultural factors, as well as individual preferences, heuristics, and cognitive biases, making food choices an ideal framework for the experimentalist to study individual patterns. With the accumulation of scientific evidence questioning the sustainability of animal-based consumption in terms of environmental, health, and animal welfare impact (Willett et al., 2019), apprehending the determinants of food choices is becoming indispensable. In particular, meat consumption is affected by the meat paradox and entails an array of cognitive biases and social constructs that prevent individuals from reducing meat and switching to more plant-based foods. Experimental economics studies provide a new set of evidence on the determinants of meat consumption that can be used to define relevant policy recommendations to guide individuals toward healthier and more sustainable diets. While this area of study is rapidly growing, many mechanisms remain unclear. This dissertation intends to make a contribution by providing experimental evidence on the individual tenants of meat-related behaviors.

Chapter 1 analyzes the consequences of adopting a plant-based diet on group interactions. In Western societies, consuming meat is considered natural, normal, necessary, and nice (Piazza et al., 2015). Therefore, adopting a vegetarian or vegan diet is a deviation from the norm and leads to group-identity conflicts. While growing scientific evidence points toward the necessity of switching to plant-based diets, vegetarians and vegans incur a social cost in the form of out-group biases known as vegophobia. This bias might prevent individuals from adopting more sustain-

able diets. Chapter 1 uses an economic experiment with an incentive-compatible environment and social preferences to quantify the presence of vegephobia.

Vegephobia is assessed using an online experiment and a modified version of the dictator game. Omnivore dictators are matched with either an omnivore, vegetarian, or vegan recipient, and are provided with their dietary identity along with additional identity information. Dictators must allocate money between themselves and the recipient. Recipients are provided with the dietary identity of the dictators alongside additional information and must guess the dictators' allocation decisions. Vegephobia is quantified by comparing the sensitivity of the dictators' inequity aversion parameters (Fehr and Schmidt, 1999) contingent to the recipients' dietary identity. Perceptions of vegephobia are quantified by comparing the recipients' expectations of discriminatory behaviors. Two hypotheses on the dictators' inequity aversion parameters are pre-registered and additional results are left for exploratory analyses.

The confirmatory results reject the presence of vegephobia but the exploratory results suggest that vegephobia is driven by individual characteristics. Females have more positive attitudes towards vegans than males. Vegephobia also appears to be driven by ethnicity, education, and political preferences. Contrary to previous evidence on vegephobia, dictators who declare having a vegetarian or vegan friend in their social circle display greater out-group bias. Vegan recipients declare higher levels of experienced discrimination due to their dietary choices but do not expect vegephobia from dictators in the experiment. The results of the study are overall inconclusive. While vegephobia is reported by vegans, it is not captured by the dictators' choices, nor reflected in the vegetarian and vegan participants' expectations. This result is possibly due to the economic tools employed that fail at capturing the bias. The results are also possibly mitigated by the dictators' friendliness towards plant-based diets.

Chapter 2 investigates the information acquisition of French doctors on plant-based diets. While doctors are an immediate source of information for patients, their lack of specialized knowledge on plant-based diets and cognitive biases might lead to inadequate care and prevent switches to more sustainable diets. To explore this issue, 400 French doctors answer a questionnaire with case studies involving a patient who desires to adopt a vegan diet. Using a randomized controlled trial, half of the sample answers the questionnaire and the other half is first exposed to an information campaign before answering the questionnaire. The information

campaign is a booklet designed by the research team with medical experts that contains information on vegan diets.

The information campaign positively impacts the doctors' opinions about vegan diets and to a smaller extent their practices. The active support for the diffusion of the information campaign is not analyzed due to insufficient statistical power. Additional results suggest more significant impacts for doctors who spent more time reading the booklet and no individual heterogeneous effects. Further data explorations also reveal that doctors tend to overestimate their nutrition knowledge while being aware of their colleagues' lack of specific knowledge on the subject. The results indicate that targeted information campaigns can potentially improve doctors' opinions and to some extent practices with vegan patients at relatively low financial and time costs.

Chapter 1 and 2 provide new evidence on the behavioral mechanisms surrounding plant-based diets. The results from Chapter 1 indicate that social identities derived from plant-based diets are not enough to induce out-group discrimination in an experimental economic environment. This result does not follow previous evidence in economics demonstrating out-group bias in the presence of salient group identities, which suggests that plant-based diet identities might react to different out-group mechanisms. The absence of out-group bias also contradicts findings from the sociology and psychology literature, giving hints that vegophobia might respond differently when financial incentives are involved. Additionally, the results bring new evidence on the methodology employed. Specifically, the economic tools appear to fail at capturing vegophobia as vegans report discrimination in their everyday life but do not expect discrimination in the economic game, despite the financial incentives. The findings from Chapter 1 suggest that the meat-related social constructs and interactions remain unclear and highlight the limits of some economic tools. Chapter 2 underlines the importance of effectively informing medical professionals on plant-based diets. The results show that doctors' opinions can be influenced by providing short and reliable information, but that changing practices may require longer interventions. This suggests that doctors' practices rely heavily on their initial knowledge and beliefs and that one short information intervention on plant-based diets is not enough to induce a change. Further research involving different approaches and methods will bring a more comprehensive understanding, which will help adequately guiding individuals towards more sustainable diets.

The expansion of the meat consumption research agenda comes with challenges.

The implications behind research on meat consumption are substantial as it might lead to positive impacts on the environment, health, and animal welfare. Researchers will have to report reliable evidence to provide relevant policy recommendations. This will ensure cost-efficient policy-making and a smooth transition towards plant-based diets. The first pieces of evidence, as demonstrated in Chapters 1 and 2, also outline that meat consumption responds to unique mechanisms that might lead to counterintuitive results. Researchers will have to provide robust and credible results to validate the findings. Finally, researchers and scholars are affected, to some extent, by information resistance and confirmation bias. Thus, it is likely that the evidence on meat consumption will be questioned and rejected on the basis of contradictory beliefs. To address this issue, studies will have to produce transparent and replicable processes that leave no doubt about the quality of the results. To this end, new developments in Open Science could be beneficial.

Chapter 3 emphasizes novel methods in open science by offering a guide to the implementation of Registered Reports in economics. The publication system in economics is pressuring researchers to publish original and positive results. This bias towards positive results is leading to an overrepresentation of statistically significant results that blurs the spectrum of evidence and is pushing researchers to engage in questionable research practices. To counter this trend, researchers can use preregistration to separate prediction from postdiction. Preregistration consists in depositing a document on an online dedicated platform to specify the data collection process and analyses ex-ante. Preregistration is useful to diminish ex-post malpractices but is inefficient against publication bias. The lack of agreement and common rules also leave researchers with an important degree of freedom.

Registered Reports are a new article format designed to improve the scientific process by using a two-stage procedure, which allows for improving the experimental design, data collection, and analysis plan ex-ante. Before the data collection, researchers write the introduction, research question, hypotheses, data collection process, and analysis plan. The paper is then sent to a journal for a first round of revision (i.e., Stage-1). Once Stage-1 is approved, the paper receives an In-Principle Acceptance, which secures the publication, regardless of the results but conditional on the respect of the Stage-1 analysis plan. Researchers then collect the data, run their analyses, and write the results section. The paper is re-submitted for Stage-2 approval to verify the conformity of the analysis and is then published. Registered Reports' popularity is increasing due to their efficiency in eliminating publication

bias, improving research processes ex-ante, and limiting questionable research practices.

Chapter 3 is dedicated to providing a guide to Registered Reports for economists. The guide distinguishes Registered Reports from preregistration and demonstrates how the former outperforms the latter. Specific elements of Registered Reports are then described such as the analysis plan, statistical power, multiple hypothesis testing, smallest effect size of interest, exploratory analyses and deviations for the analysis plan, levels of Registered Reports, design table, ethics approvals, and optimal journal choice. The guide also provides specific examples for experimental economists with R and Stata codes.

Chapter 3 contributes to improving the methodology in economics by providing tools to accompany researchers who desire to undertake a Registered Report. The implementation of Registered Reports in economics comes with challenges. Questionable research practices are deeply rooted in the scientific process and many researchers are not aware of their negative effects. Worse, many questionable research practices are taught as appropriate, which ultimately legitimates them. Eliminating such practices will require extensive evidence of their negative effects on scientific results, as well as a better understanding of the researchers' biases. It will also require structural changes. First, changes in the education system will be necessary in order to highlight malpractices and train future researchers to make use of more virtuous methods. Second, a better emphasis of the publish or perish culture in economics will help relieving pressure from researchers. This will favor the development of new research methods such as Registered Reports, encourage replication initiatives, and ultimately benefit scientific evidence. Lastly, changes will have to come from peer-reviewed journals. As of today, ten journals accept Registered Reports in economics. A greater number of journals accepting Registered Reports and replication studies will encourage researchers to undertake such studies, which will mechanically increase the number of clean evidence and drift the field away from a replication crisis. Different barriers are yet to be removed, but the growing interest from economists will be the driving force towards cleaner research practices.

In conclusion, the first section of this dissertation contributes to the economic research on meat consumption and the second section aims at improving the methodology in experimental economics. Overall, I believe that this dissertation only scratches the surface of the two subject matters and that many opportunities are ahead. Chapter 1 and 2 tackle meat-related behaviors from the social identity and

GENERAL CONCLUSION

information processing angles. These approaches could be extended by studying the role of social norms in relation to meat consumption and attitudes towards vegetarians and vegans in an environment where following a plant-based diet is the norm. The group dynamics remain uncertain and extensions of the settings proposed in Chapter 1 would bring a more comprehensive understanding. Regarding the information channel, the next steps will be to recover more evidence on the mechanisms with different sub-populations. This will enable to conceive and convey information more efficiently, which will lead to a smoother transition toward plant-based diets. In my opinion, future research on the individuals tenants of meat consumption should also focus on nudging individuals into reducing meat consumption and extend the methods at a larger scale. For Registered Reports, spreading the information on the benefits and training researchers will be the next key steps. Future studies should concentrate on outlining researchers' practices in economics, which will give a better understanding and broader picture of some of the malpractices in place. Examining the role of scientific social norms and the effects of nudges on researchers' practices could bring some interesting insights. For example, future research could attempt to assess the impact of incentivizing or rewarding researchers for the publication of Registered Reports, as well as look into the effects of facilitating the implementation process of Registered Reports in journals.

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Titre : Trois essais en économie expérimentale : alimentation végétale et *Registered Reports*

Mots clés : Économie expérimentale, Alimentation végétale, Préférences sociales, Registered Reports

Résumé : Les preuves scientifiques sur les effets positifs de l'alimentation végétale (environnement, santé et bien-être animal) s'accumulent. Malgré ces avancées, la consommation individuelle de viande reste à des niveaux très élevés. La première partie de la thèse utilise les méthodes d'économie expérimentale pour identifier et lever les freins à la végétalisation de l'alimentation. Le chapitre 1 vise à mesurer expérimentalement le stigma social associé à l'alimentation végétale. Les résultats ne permettent pas de conclure à la présence de végéphobie dans un cadre économique, mais soulignent les limites des méthodes économiques utilisées. Le chapitre 2 rapporte les résultats d'une expérience portant sur l'impact d'une brochure d'information relative à l'alimentation végétale auprès

d'un échantillon représentatif de médecins généralistes français. L'exposition à la brochure améliore l'avis général des médecins et dans une moindre mesure leurs pratiques. La deuxième partie de la thèse vise à répondre au biais de publication et aux pratiques scientifiques inadéquates en économie qui menacent la répliquabilité des résultats. Le chapitre 3 propose un guide à la rédaction des *Registered Reports* pour les économistes. Le guide développe les apports des *Registered Reports* et explicite les principaux concepts nécessaires à leur rédaction (plan d'analyse, analyse de pouvoir statistique, taille d'effet minimal d'intérêt, etc.). Des exemples spécifiques adaptés à l'économie expérimentale ainsi que des codes R et Stata sont détaillés pour faciliter la mise en oeuvre des *Registered Reports*.

Title: Three essays in experimental economics: plant-based diets and Registered Reports

Keywords: Experimental economics, Plant-based diets, Social preferences, Registered Reports

Summary: Scientific evidence on the benefits of plant-based diets (environmental, health and animal welfare) is growing. Yet, individual meat consumption remains high. The first part of this dissertation uses experimental economics methods to identify and remove some of the barriers that limit the transition towards plant-based diets. Chapter 1 presents an experimental approach to measure the social stigma associated with following a plant-based diet. The results do not conclude to the presence of vegophobia in the economic environment but underline the limits of the employed tools. Chapter 2 reports the results from an experiment on the impact of an information campaign about plant-based diets on a

representative sample of French doctors. The information campaign has a positive impact on the doctors' views and, to a smaller extent, improves their practices. The second part of the dissertation is dedicated to reduce publication bias and questionable research practices in economics in order to reduce the likelihood of a replication crisis. Chapter 3 provides a practical guide to Registered Reports for economists. The guide describes the benefits of Registered Reports and details the necessary methodological components (analysis plan, power analysis, smallest effect size of interest, etc.). Specific examples for experimental economics as well as R and Stata codes are provided to facilitate the implementation of Registered Reports.

