



Essays on the economic analysis of discrimination in the labor and the rental housing market

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**Essais sur l'analyse économique des discriminations
dans le marché de l'emploi et du logement locatif**

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Introduction Générale

1. Motivation

Le droit au logement et au travail sont reconnus comme des droits humains universels. L'accès à un logement et à un emploi sont des enjeux cruciaux dans la vie des individus. Entre autres, l'accès au logement et au travail affecte la santé, l'inclusion sociale, la vie familiale, l'éducation ainsi que l'accessibilité aux services publics. L'accès au logement a lui-même un impact sur les opportunités d'emploi et l'accès au travail a un impact sur les opportunités d'achat et de location. Ainsi, leur importance dans la vie des individus est telle qu'ils doivent être protégés. D'un point de vue socio-économique, il est donc réellement important que les individus, quels qu'ils soient, soient traités de manière égale dans leur accès au travail et au logement. De nos jours, le principe de non-discrimination concernant l'appartenance ethnique, la religion, le handicap, l'orientation sexuelle, l'âge ou le sexe, est garanti par de nombreux textes internationaux, dont des textes européens (par exemple le Traité sur le fonctionnement de l'Union européenne). Malgré cela, des expériences sur le terrain ont démontré l'existence de discriminations sur le marché du logement et de l'emploi dans les pays de l'OCDE, entraînant de nombreuses conséquences économiques et sociales préjudiciables pour les groupes cibles, telles que l'aggravation de la ségrégation résidentielle dans des quartiers moins attrayants (Denton, 1999 ; South et Crowder, 1998), un accès plus difficile aux services publics, à l'éducation et à l'emploi (Yinger, 1995, Angrist et Lang, 2004, Hardman et Ioannides, 1999) et une diminution manifeste du bien-être des personnes appartenant aux groupes victimes de discrimination.

Il existe de nombreuses raisons pour lesquelles des individus appartenant à un groupe sont plus susceptibles d'obtenir un logement ou un emploi que ceux appartenant à un autre groupe, mais cela ne peut pas toujours être considéré comme de la discrimination. La charte des droits fondamentaux de l'Union Européenne tout comme le droit Français définissent la discrimination de la manière suivante : « Constitue une discrimination

directe la situation dans laquelle, sur le fondement de son appartenance ou de sa non-appartenance, vraie ou supposée, à une ethnie ou une race, sa religion, ses convictions, son âge, son handicap, son orientation ou identité sexuelle, son sexe ou son lieu de résidence, une personne est traitée de manière moins favorable qu'une autre ne l'est, ne l'a été ou ne l'aura été dans une situation comparable, et constitue une discrimination indirecte une disposition, un critère ou une pratique neutre en apparence, mais susceptible d'entraîner, pour l'un des motifs mentionnés, un désavantage particulier pour des personnes par rapport à d'autres personnes, à moins que cette disposition, ce critère ou cette pratique ne soit objectivement justifié par un but légitime et que les moyens pour réaliser ce but ne soient nécessaires et appropriés » (Art. 1, Loi n° 2008-496 du 27 mai 2008).

La discrimination sur le marché du travail et du logement peut prendre diverses formes et doit être combattue en conséquence. Elle peut être liée à l'offre sur le marché, affecter directement l'occupation dudit logement ou emploi, ou enfin nuire au processus d'embauche ou d'attribution de logements loués ou vendus. La discrimination liée à l'offre sur le marché se réfère aux circonstances dans lesquelles les caractéristiques mêmes du logement ou du travail disponible les rendent inappropriés pour certaines catégories de personnes, qui en sont donc exclues. La discrimination peut survenir au travail ou durant l'occupation du logement, une fois que la personne est entrée dans les lieux (les personnes harcelées par leur patron ou par le propriétaire du logement, un règlement interne défavorisant indirectement un groupe d'individus...). Enfin, la discrimination peut affecter le processus d'embauche ou d'attribution de logements lorsqu'un propriétaire privé ou un agent immobilier refuse de louer ou de vendre une propriété à un particulier, ou lorsqu'un employeur refuse d'embaucher un individu pour des motifs discriminatoires.

La discrimination peut provenir de deux sources communément présentées dans la littérature. La discrimination « fondée sur le goût » fait référence à la discrimination qui provient des préférences des individus. Ainsi, les individus qui font preuve de

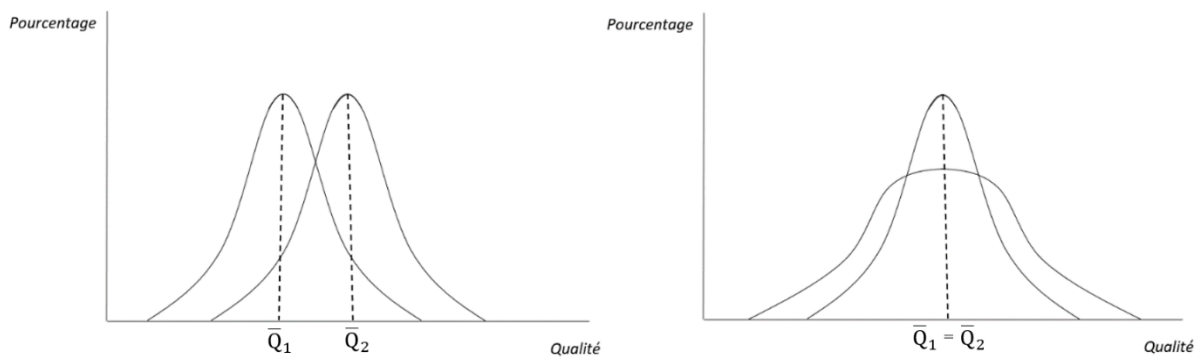
discrimination par le « goût » ont une attitude hostile à l'égard d'individus faisant partie d'un autre groupe (xénophobie, homophobie, racisme, sexisme, ou des préférences d'autres types) ou bien se complaisent avec l'attitude négative du groupe d'individus auquel ils sont rattachés (Becker, 1957). Dans les décisions d'embauche ou de location, cela correspondrait au cas où les employeurs ou agents immobiliers discriminent par exemple les candidats homosexuels ou étrangers en raison de leurs préférences personnelles ou n'acceptent pas les individus d'une autre orientation sexuelle ou d'une autre ethnie afin de ne pas déplaire à leurs autres employés ou clients. Au-delà d'un point de vue moral, ce type de discrimination est très problématique car elle ne repose pas sur la « qualité » ou la « performance » des candidats. Le marché a dans ce cas des défaillances et les ressources ne sont pas toujours allouées aux individus les plus efficaces ou ayant la qualité la plus élevée. Les entreprises perdent ainsi des candidats potentiellement talentueux (Equality Challenge Unit, 2009) et les propriétaires ou agents immobiliers renoncent ainsi à louer leur appartement à des locataires de « qualité » (capable de payer le loyer, de prendre soin de l'appartement etc.).

D'autre part, la discrimination peut provenir de considérations « statistiques », à savoir de la qualité ou performance présumée des candidats (Phelps, 1972 ; Aigner and Cain, 1977). Cette discrimination statistique, un peu moins intuitive mais pourtant souvent très présente, se produit à cause du manque d'information sur les candidats. En effet, en l'absence d'information directe sur la productivité, la fiabilité, ou encore l'engagement au travail du candidat, un employeur peut substituer des moyennes et variances de groupe (réelles ou imaginaires/biaisées par des stéréotypes) pour combler le manque d'information (Schwab, 1986). Le même parallèle peut être fait pour les agents immobiliers ou propriétaires privés qui manqueraient d'information sur la « qualité » du futur locataire, se traduisant essentiellement dans ce cas par un manque d'information sur la capacité à honorer les paiements pour la location, à prendre soin de l'appartement etc. Ainsi, si des individus appartenant à un premier groupe sont supposés être de moins

bonne « qualité » que des candidats appartenant à un deuxième groupe, alors une discrimination statistique dite de « premier ordre » pourrait survenir et les individus du second groupe pourraient être préférés. Il est aussi possible que les candidats des deux groupes soient considérés comme d'aussi bonne « qualité » en moyenne, mais que la variance de leurs caractéristiques propres soit perçue comme différente. Ainsi, si les agents sont averses au risque, les candidats du groupe ayant la plus faible variance perçue devraient être préférés. On parle dans ce cas de discrimination statistique de « second ordre » (Klumpp et Su, 2013). Cette dernière forme de discrimination dépend donc entièrement de l'aversion au risque des agents et ne peut donc pas avoir lieu si ces derniers sont neutres aux risques. La Figure 1 illustre ces deux types de discrimination statistique.

Bien que rationnelle, ce type de discrimination pose un problème d'équité. En effet, même s'il est de qualité, un individu du second groupe aura moins de chances d'obtenir le travail ou le logement qu'il désire qu'un candidat du premier groupe car les membres de son groupe sont considérés de moins bonne qualité ou comme plus risqués.

Fig. 1 : Discrimination statistique de premier et de second ordre



où \bar{Q}_1 est la qualité moyenne supposée des candidats du groupe discriminé et \bar{Q}_2 la qualité moyenne supposée des candidats du groupe favorisé. Source : Flage (2019b)

De plus, les croyances des agents sur la variance et la moyenne des performances des candidats sont très souvent éloignées de la réalité, incertaines voire biaisées par des stéréotypes. Ainsi, il n'est pas rare que les individus d'un groupe soient considérés à tort

comme étant plus risqués ou de moins bonne qualité à cause d'un manque d'information correcte. Dans ce cas ce type de discrimination amène aussi à des défaillances et les emplois et logements ne sont encore une fois pas toujours alloués aux individus les plus performants ou de meilleure qualité. Cependant, cette fois, il est possible de lutter directement contre ce type de discrimination en fournissant une meilleure information sur les caractéristiques du groupe discriminé, ce qui permet de réduire l'incertitude des agents et d'atténuer l'impact de stéréotypes négatifs. Il est plus difficile de combattre la discrimination par le « goût » car elle ne découle pas d'un manque d'information, mais de préférences individuelles profondément ancrées. Il peut alors être nécessaire d'agir dès le plus jeune âge, dans l'éducation des enfants, notamment grâce à l'école.

Enfin, même si les croyances des agents sont parfaitement en accord avec la réalité, ce qui est relativement rare, discriminer maximise en effet le profit espéré de l'agent mais reste inefficace à long terme pour la société. En effet, cela n'améliore pas et même détériore la situation des minorités discriminées qui se retrouvent ainsi enfermées dans un cercle vicieux. Par exemple, il est relativement reconnu que les personnes ayant un handicap souffrent d'une stabilité financière plus faible que les personnes valides (ex. Batavia et Beaulaurier, 2001 ; Elwan, 1999), ce qui peut amener à une discrimination à leur égard dans le marché du logement locatif. Si leur accès au logement est limité par cette faible stabilité financière, cela a un impact sur tout le reste (inclusion sociale, santé...), et notamment sur leur accès à l'emploi et donc leur stabilité financière. Cela renforce ainsi les inégalités avec toutes les conséquences néfastes que cela peut impliquer pour le bien-être social.

Bien entendu, la discrimination provenant des préférences et celle provenant de considérations statistiques ne sont pas exclusives et il n'est pas rare qu'elles soient toutes deux présentes sur un marché. Il est important de les discerner car comme précisé

précédemment, elles ne peuvent pas être combattues de la même manière.¹ Bien que les procédures et les conditions d'embauche et d'attribution de logements soient en partie régies par la loi, les décisions judiciaires restent relativement rares en cas de discrimination. Il semble qu'il soit plus efficace de combattre ce phénomène à la source en luttant contre le racisme, l'homophobie, le sexisme, etc. ou en fournissant plus d'information correcte sur les minorités afin de rassurer les employeurs, les agents immobiliers, les propriétaires, sur les performances et la qualité des potentiels candidats appartenant à une minorité (Riach et Rich, 2002 ; Bertrand et Mullainathan, 2004 ; Ahmed et al., 2010). Dès lors, il est essentiel de clarifier le niveau mais surtout le type de discrimination auquel les différentes minorités font face dans le processus d'embauche et d'attribution de logement. Il existe de nombreuses preuves de la présence de discrimination statistique et par le goût à l'encontre des minorités ethniques, de genre ou encore en fonction de l'orientation sexuelle dans le marché du logement (ex. Baldini et Federici, 2011 ; Hanson et Hawley, 2011 ; Mazziotta et al., 2015 ; Le Gallo et al., 2018 ; Bunel et al., 2019) et de l'emploi (ex. Carlsson, 2010 ; Drydakis, 2011 ; Oreopoulos, 2011 ; Bonoli and Hinrichs, 2012 ; Larribeau et al., 2013 ; Duguet et al., 2018 ; Challe et al., 2018) dans les pays de l'OCDE. Cependant toutes ces études ont leur propre protocole, leur propre façon de rendre compte des résultats, leur propre échantillon (de caractéristiques et tailles différentes) et ont testé la discrimination dans un contexte qui leur est propre. Enfin, la discrimination à l'égard de certaines minorités comme les

¹ Cependant, il existe un lien non rétroactif entre la discrimination par le goût et la discrimination statistique : combattre la discrimination par le goût permet aussi de combattre la discrimination statistique à plus long terme, alors que l'inverse n'est pas vrai. En effet, une discrimination provenant d'un certain racisme à l'égard d'individus appartenant à une minorité ethnique dans le marché de l'emploi rend ces derniers moins attractifs dans le marché du logement locatif en cas d'asymétrie d'information (discrimination statistique) dû à leur plus faible stabilité financière, réduisant ainsi par la même occasion leur accès à l'emploi etc. Le même parallèle peut être fait avec la discrimination indirecte (dû à l'offre d'emploi ou de logements). Lutter contre la discrimination indirecte permet aussi de lutter contre la discrimination directe (statistique).

personnes ayant un handicap physique ou un handicap mental n'a été que rarement étudiée (notamment dans le marché du logement locatif).

L'objectif principal de cette thèse est donc de clarifier le niveau et les sources de la discrimination directe à l'égard des différentes minorités lors du processus d'attribution des logements locatifs et lors du processus d'embauche. Pour ce faire, nous utilisons à la fois des données expérimentales de terrain et de laboratoire. Nous pouvons ainsi répondre qualitativement et quantitativement aux problématiques suivantes :

A quel point la discrimination est-elle répandue sur le marché de la location et de l'emploi dans les pays de l'OCDE ? Quelles sont les causes et sources de la discrimination directe dans ces deux marchés ? Certaines minorités sont-elles plus affectées que d'autres par un ou plusieurs types de discrimination ? Les intermédiaires professionnels sont-ils par nature moins prompts à discriminer que les individus directement concernés ? Quelles sont les conséquences socio-économiques sur les minorités concernées et même sur la société dans son ensemble ? Comment réduire les différentes discriminations et toutes les inefficacités qu'elles engendrent ?

Pour répondre à ces questions il est tout d'abord essentiel de comprendre comment la discrimination est mesurée dans ces différentes expériences de terrains.

2. Le « Testing »

Depuis de nombreuses années, de nombreuses expériences de terrain, que l'on appelle plus communément « testings », ont été menées dans les pays de l'OCDE dans le but de détecter, de mesurer, et de lutter contre la discrimination à l'égard des différentes minorités dans le marché du logement et du travail. Trois approches principales ont été utilisées lors de ces testings. Nous les présentons dans les sous-sections suivantes.

2.1 L'approche en personne

Avec l'approche en personne, deux acteurs (mais parfois plus) sont entraînés de manière à formuler des demandes équivalentes lorsqu'ils rencontrent un employeur lors d'un entretien d'embauche ou un propriétaire/agent immobilier lors de la visite d'un appartement. Le seul élément qui différencie ces deux acteurs est la caractéristique cible, ou variable d'intérêt (couleur de peau, âge, sexe, présence d'un handicap etc.). Un traitement différent des acteurs de la part des agents est considéré comme une discrimination en fonction de la caractéristique cible. Le principal atout de cette approche est qu'elle permet de tester la discrimination lors de la décision finale des agents, à la dernière étape du processus d'embauche et d'attribution de logements.

Malheureusement, cette approche a des défauts non négligeables qui peuvent grandement biaiser les résultats de l'étude. Tout d'abord, il est très difficile pour les testeurs de ne se distinguer que par la variable cible lors de l'entretien. En effet, ce type d'expérience nécessite que les acteurs soient identiques sur toutes les autres caractéristiques visibles, telles que la convivialité, le dialecte, la beauté, le charme etc. (Siegelman et Heckman, 1993; Heckman, 1998). De plus, même si tous ces facteurs sont bel et bien identiques, il est possible que l'agent choisisse un individu plutôt qu'un autre pour une raison autre qui échappe à l'expérimentateur. Enfin, après une formation et toutes les

recommandations fournies par l'expérimentateur aux acteurs, l'expérimentateur n'est pas en mesure d'observer ce qui se passe lors de la réunion entre les deux parties. Pourtant, les acteurs sont parfois informés du but de l'étude (ou l'ont facilement deviné), ce qui peut les inciter, implicitement ou non, à se comporter de manière à produire des résultats conformes à leur propre croyance sur la discrimination à l'égard de leur groupe. Ainsi, le principal problème de cette approche est le manque de contrôle de l'expérimentateur. La plupart des tests en personne utilisent cependant plusieurs paires d'acteurs pour atténuer les biais pouvant être liés à un acteur en particulier ou à une paire d'acteurs (ex. Neumark et al., 1996 pour le marché du travail ; Turner et al., 2002 ; pour le marché du logement locatif).

2.2 L'approche par téléphone

Avec cette approche, des acteurs sont encore une fois entraînés de manière à formuler des demandes équivalentes, mais cette fois cette demande se fait par téléphone. L'expérimentateur peut être présent lors de l'entretien téléphonique, ce qui permet un contrôle plus élevé que dans l'approche précédente. Cependant, cette approche ne capte la discrimination qu'à la première étape, c'est-à-dire lors de la demande de rendez-vous, et pas lors du rendez-vous lui-même. En plus d'annoncer un nom correspondant à une ethnie, un âge ou un genre spécifique, la voix ou l'accent des acteurs est un attribut facilement identifiable permettant encore dans une certaine mesure de détecter l'âge, le sexe, ou encore l'ethnie des acteurs (Purnell et al. 1999; Massey et Lundy 2001). Encore une fois, les acteurs ne doivent se distinguer que par la variable cible, et avoir exactement les mêmes autres caractéristiques, telles que l'attitude, la répartie, le dialecte etc. Une faiblesse des appels téléphoniques (partagée avec les approches en personne) est que la nature des réponses orales de la part des employeurs ou agents immobiliers/propriétaires privés est vraiment sujette à interprétation. En effet, une réponse très courtoise peut très bien cacher un racisme profond. Lorsqu'un agent rencontre un acteur ou répond au téléphone, il n'osera pas nécessairement afficher son hostilité lors de la conversation si

l'appartenance ethnique, le sexe, l'âge de l'acteur ne lui convient pas (Heylen et Van den Broeck, 2016; Verhaeghe et al., 2017).

2.3 L'approche par écrit, ou « test de correspondance ».

Une solution à ces problèmes consiste à utiliser l'approche par écrit. Avec l'expansion rapide du marché de l'emploi et du logement sur Internet, le courrier électronique est devenu l'un des moyens les plus courants pour déposer son cv afin d'obtenir un éventuel entretien d'embauche ou pour faire une demande de visite d'un logement. Ainsi, dans cette approche plus communément appelée « test de correspondance », l'expérimentateur crée un certain nombre de candidats fictifs, qui ne diffèrent que par la variable d'intérêt (le nom pour signaler l'ethnie ou le sexe, la date de naissance pour signaler l'âge, etc.) puis envoie des demandes écrites par courrier électronique en réponse à des offres d'emplois ou des annonces de logements. Pour éviter que les agents ne détectent la présence du testing, les demandes ne doivent pas être strictement identiques (quand plus d'une demande est envoyée au même agent), mais toutes les caractéristiques essentielles, telles que l'expérience, la qualification, etc. doivent être étroitement liées pour ne différer réellement que par la variable d'intérêt. En utilisant cette méthode, l'expérimentateur a l'avantage de pouvoir travailler sur des données réelles tout en gardant un contrôle des variables proches d'une expérience de laboratoire. De plus, cette méthode est également plus facile et moins coûteuse à mettre en œuvre (elle ne nécessite pas l'embauche et le coaching d'acteurs) que les entretiens en face à face ou par téléphone. Enfin, la possibilité d'envoyer un très grand nombre de requêtes dans un délai très court permet de mesurer très précisément les pratiques discriminatoires d'employeurs ou d'agents immobiliers/propriétaires privés grâce à une puissance statistique très élevée. Cependant, cette méthode n'est pas exempte de faiblesses : la principale difficulté de cette dernière est que la caractéristique cible peut être non perçue ou mal perçue de la part des agents, ce qui peut biaiser les résultats, surestimant ou sous-estimant le niveau de discrimination réel subi par les minorités. Par exemple, le nom des candidats fictifs est utilisé comme

proxy pour signaler l'origine ethnique. Cependant, il se peut que les noms choisis ne soient pas perçus comme signalant une ethnie particulière, ou encore qu'ils reflètent autre chose que l'origine ethnique, telle qu'une certaine classe sociale, un certain âge, que l'expérimentateur n'avait pas prévu (Bertrand et Mullainathan, 2004; Pager, 2007). L'un des principaux enjeux de cette méthode est donc de signaler la caractéristique cible de manière à ce que le signal soit assez fort pour être perçu, mais sans induire de biais. Enfin, comme l'approche par téléphone, l'approche par écrit permet de tester le niveau de discrimination uniquement à la première étape du processus de location ou d'embauche, et non lors de l'entretien avec l'employeur ou le propriétaire privé/agent immobilier, lors duquel une discrimination supplémentaire peut se produire.

Les tests de correspondance peuvent être effectués de deux manières. La procédure dite « assignation aléatoire » ou « requête unique » consiste à n'envoyer qu'une seule requête à chaque agent. Le principal avantage de cette procédure est qu'il est pratiquement impossible que les agents détectent la présence du testing. Cependant, cette méthode ne contrôle pas l'effet des variables non observables sur le taux de réponse et nécessite un échantillon plus élevé pour obtenir la même puissance statistique que la méthode dite en « appariée » (qui est la plus utilisée et la plus intuitive). L'approche en « appariée » consiste à envoyer plusieurs requêtes au même agent, ce qui permet bien entendu de contrôler l'effet de caractéristiques non observables de l'agent ou de l'environnement, mais surtout de tester la présence de discrimination « absolue », c'est-à-dire les cas où un candidat fictif reçoit une réponse positive de la part des agents, tandis que l'autre candidat n'en reçoit pas. Comme pour toutes les expériences « within », il n'est pas nécessaire d'avoir un échantillon aussi élevé que dans les expériences « between » pour obtenir la même puissance statistique, mais cela peut introduire un biais, comme par exemple ici un risque de détection.

3. Résumé des travaux

Dans cette thèse, nous exploitons à la fois des données expérimentales et des données de terrain pour détecter, mesurer, et étudier la discrimination à la première étape de l'attribution d'emplois et de logements locatifs dans les pays de l'OCDE. Les quatre chapitres de cette thèse consistent en quatre articles de recherche rédigés de manière indépendante.

Dans le premier chapitre, nous analysons le niveau et les sources de la discrimination à l'encontre des hommes et des minorités ethniques dans le marché du logement locatif dans les pays de l'OCDE à travers une méta analyse de tests de correspondance. Dans le second chapitre, nous nous intéressons cette fois à la discrimination en fonction de l'orientation sexuelle dans le marché du travail. Nous utilisons aussi la méthode de la méta-analyse et une analyse de régression multivariée pour étudier et connaître les causes de la discrimination à l'égard des personnes gays et lesbiennes dans le marché de l'emploi. Dans le troisième chapitre, nous réalisons une expérience en laboratoire pour étudier les différences de comportement entre les participants qui prennent des décisions pour le compte d'autrui et les participants qui prennent des décisions pour leur propre compte dans un contexte discriminatoire. Pour ce faire, nous utilisons une variante du jeu de la confiance à trois joueurs avec identités de groupe. Dans le dernier chapitre de cette thèse, nous réalisons un « testing » avec la méthode du test de correspondance afin de détecter la présence de discrimination à l'égard des personnes handicapées moteur, visuelle ou mentale sur le marché du logement locatif en France.

3.1 Chapitre 1 : Discrimination ethnique et de genre dans le marché du logement locatif dans les pays de l'OCDE. Une méta analyse.

Depuis que Carpusor et Loges (2006) ont réalisé le premier test de correspondance destiné à détecter une discrimination sur le marché du logement locatif, cette approche s'est

généralisée en raison de son aspect pratique et de son efficacité. Dans le premier chapitre de cette thèse², nous présentons une vaste revue de toutes les études ayant testé la présence de discrimination à l'égard des groupes ethniques minoritaires sur le marché du logement locatif par la méthode du test de correspondance, permettant à la littérature sur le sujet d'être à jour. De plus, nous réalisons une méta-analyse de 25 études indépendantes conduites dans les pays de l'OCDE entre 2006 et 2017, contenant plus de 300 tailles d'effets et représentant un total de plus de 110 000 e-mails envoyés à des propriétaires privés et des agents immobiliers, permettant de fournir à la littérature la première analyse quantitative du niveau et des sources de la discrimination à l'égard des minorités ethniques dans le marché du logement locatif dans le pays de l'OCDE³. En plus de présenter les résultats globaux d'études récentes, nous nous concentrons sur des sous-groupes de tests de correspondance spécifiques par l'intermédiaire d'une analyse de régression multivariée afin de mettre en évidence les différences de résultats en fonction de différentes caractéristiques : l'ethnie testée et le sexe des candidats, le type d'agents (particulier ou agent immobilier), la procédure utilisée, la localisation, et le type d'information fourni dans les requêtes. A la première étape du processus de location, nous trouvons que les individus appartenant à la majorité ethnique ont presque deux fois plus de chances d'être choisis (de recevoir une réponse positive alors que l'autre candidat n'en reçoit pas) par des agents immobiliers ou des propriétaires privés par rapport aux candidats des groupes minoritaires. Si l'on concentre notre analyse sur le groupe Arabo-Musulman pour lequel nous disposons de la plus grande base de données, la discrimination est encore plus élevée car ces derniers ont cette fois plus de deux fois moins de chance d'être choisis. Les résultats montrent aussi la présence d'un effet de genre significatif car les femmes ont presque 30% de chances de plus que les hommes

² Ce chapitre est associé à un article publié dans la revue *Journal of Housing Economics* (Flage, 2018).

³ Une autre méta analyse sur le sujet a été réalisée l'année suivante par Auspurg et al. (2019). Cette analyse complète celle effectuée dans cette thèse car elle ne s'intéresse non pas au niveau de discrimination actuel mais plutôt à l'évolution de la discrimination au cours du temps.

d'être choisies par les agents immobiliers et les particuliers dans le marché du logement locatif. Cependant ce résultat est différent en fonction du groupe testé : les femmes appartenant à une minorité ethnique ont 34% de chances de plus d'être choisies que les hommes appartenant à la même minorité et ce résultat est encore plus élevé si l'on se concentre seulement sur le groupe Arabo-Musulman pour lequel les femmes ont 50% de chance de plus d'être choisies que les hommes. Enfin, les femmes appartenant à la majorité ethnique ont « seulement » 20% de chances de plus d'être choisies que les hommes appartenant à la majorité. Ainsi, la discrimination ethnique et de genre interagissent : la discrimination de genre est plus élevée pour les individus appartenant à une minorité ethnique que pour les individus appartenant à la majorité. Ainsi, les femmes appartenant à la majorité sont les plus favorisées, tandis que les hommes ayant un nom à consonance étrangère sont les plus défavorisés (surtout les hommes Arabo-Musulmans). Finalement, les agents immobiliers discriminent significativement moins les individus appartenant à une minorité ethnique que ne le font les particuliers. Il semblerait que cela vienne en partie du fait que les particuliers font preuve de discrimination statistique significative alors que les agents immobiliers non. En effet, fournir plus d'informations positives dans les e-mails envoyés aux particuliers à propos de la stabilité financière et l'éducation du candidat diminue significativement le traitement différentiel entre les individus. Ainsi, la discrimination dans le marché du logement locatif dans les pays de l'OCDE n'est pas seulement un problème de préférences. Il semblerait que les particuliers aient un manque d'informations correctes sur les individus appartenant aux minorités ethniques et qu'ils aient besoin d'être « rassurés » à leur égard.

Les individus appartenant à une minorité ethnique ne sont cependant pas seuls à subir une lourde discrimination synonyme de nombreuses inefficacités économiques et sociales dans le marché du travail et du logement. La littérature sur les autres minorités est cependant moins conséquente, impliquant un plus grand flou sur le niveau et les sources de la discrimination à leur égard.

3.2 Chapitre 2 : Discrimination à l'encontre des gays et des lesbiennes à l'embauche : une méta analyse.

Bien que l'embauche soit l'une des parties les plus cruciales de toute relation de travail, il s'agit sans doute de l'un de ces aspects les moins bien compris (Petersen et al., 2000 ; Pager et Karafin, 2009). Après avoir fait l'objet d'une attention marginale, comme si elle était taboue, la recherche sur la situation des gays et des lesbiennes dans le marché du travail a récemment pris de l'ampleur. Ces dernières années, de nombreux tests de correspondance réalisés dans les pays de l'OCDE ont ainsi examiné si les candidats homosexuels étaient confrontés à un traitement différent lors du processus d'embauche. Cependant, toutes ces études ont leurs propres protocoles, leurs propres façons de rendre compte des résultats et différentes tailles d'échantillons. Dans le deuxième chapitre⁴ de cette thèse, nous avons ainsi mis en œuvre une seconde méta analyse pour évaluer le niveau ainsi que les déterminants de la discrimination à l'encontre des minorités sexuelles dans le marché du travail, permettant de fournir à la littérature la première analyse quantitative du niveau de discrimination à l'égard des minorités sexuelles dans le marché du travail.

Par l'intermédiaire d'une analyse de 18 études représentant une base de données de 50.000 CVs envoyés à des employeurs, nous avons pu déterminer que les candidats ouvertement homosexuels avaient presque 40% de chances de moins d'être choisis que les candidats hétérosexuels, à informations égales fournies dans les CV. Il semblerait ainsi que le préjudice à l'égard des homosexuels soit similaire à celui subi par les minorités ethniques dans le processus d'embauche (voir la méta analyse de Zschirnt and Ruedin, 2016). Cependant, ce résultat doit être nuancé car il dépend grandement du type d'emploi testé ainsi que du sexe des candidats. En effet, les résultats indiquent que les candidats homosexuels sont significativement moins discriminés dans les emplois à haute

⁴ Ce chapitre est associé à un article publié dans la revue *International Journal of Manpower* (Flage, 2019a).

qualification (ex. postes analytiques, comptabilité, banque, finance, gestion, enseignement, ingénierie...) que dans les emplois peu qualifiés (ex. emplois dans l'industrie, dans la vente et la restauration). De plus, on peut voir que les lesbiennes semblent globalement faire face à un préjudice moins grand que les gays, sauf dans les emplois considérés comme plutôt « féminins » (ex. infirmière, femmes de ménages, ergothérapeute...) ce qui est cohérent avec la littérature sur les stéréotypes sur la féminité/masculinité des hommes et des femmes homosexuels. Les hommes gays semblent d'ailleurs moins discriminés dans les emplois féminins que dans les emplois masculins (ex. mécanicien, conducteur de différents véhicules etc.), alors qu'il n'y a pas d'effet significatif pour les lesbiennes. Nous avons pu déterminer que cette discrimination n'était pas entièrement dû à un problème de préférences des employeurs (homophobie) mais était aussi dû à un manque d'information sur les demandeurs, ce dernier pouvant amener à des croyances erronées, parfois biaisées par des stéréotypes négatifs, sur les caractéristiques des candidats homosexuels. En effet, fournir plus d'informations dans le contenu des candidatures sur la motivation et les compétences réduit considérablement l'écart de traitement entre les candidats homosexuels et hétérosexuels, ce qui indique que les employeurs ont besoin d'être « rassurés » sur les caractéristiques de productivité des candidats homosexuels. Enfin, il semble que la manière de signaler l'orientation sexuelle dans le CV influe sur le niveau de discrimination.

La discrimination à l'embauche à l'égard des minorités sexuelles est donc très présente dans le marché de l'emploi. Cependant, il est à noter que cette méta-analyse ne présente que le niveau de discrimination provenant des employeurs. A l'inverse des testings sur le marché du logement locatif, il n'existe pas de données sur le comportement discriminatoire des intermédiaires sur le marché du travail, à savoir les cabinets de recrutements. Pourtant, étant moins concernés et pouvant diversifier leur risque, les cabinets de recrutements pourraient être moins à même de discriminer de manière statistique que les employeurs.

3.3 Chapitre 3 : Intermédiation et discrimination dans un jeu de la confiance : une étude expérimentale

La méta analyse réalisée dans le premier chapitre et de nombreuses études empiriques récentes montrent que les intermédiaires sont moins enclins à discriminer que les individus directement impliqués (Ahmed et Hammarstedt, 2008; Bosch et al., 2010; Bosch et al., 2015; Bunel et al., 2017; Le Gallo et al., 2018). Les implications pour la société sont importantes : il semble que la présence d'intermédiaires pourrait aider à lutter contre les discriminations et toutes les inefficacités économiques et sociales qu'elles impliquent. Cependant, de nombreux facteurs ne peuvent pas être réellement contrôlés lors de ces testings ; par conséquent, les causes de ce comportement ne sont toujours pas bien définies. Est-ce vraiment dû à la simple présence d'un intermédiaire ou plutôt à des facteurs spécifiques aux marchés testés lors de ces expériences de terrains (meilleure information, effet de réputation etc.) ? Les intermédiaires professionnels ne sont-ils pas plus aptes à détecter la présence d'un testing, sous-estimant ainsi leur comportement discriminatoire réel ? Pour élaborer des politiques efficaces, il est nécessaire de déterminer les raisons qui ont conduit à ce comportement. Dans le troisième chapitre de cette thèse⁵, nous réalisons ainsi une expérience de laboratoire avec identités de groupe basée sur le cadre bien établi du jeu de la confiance (Berg et al., 1995) afin d'étudier le comportement d'intermédiaires purs dans un contexte discriminatoire.

Dans la forme la plus simple du jeu de la confiance proposée par Berg et al. (1995), un premier joueur, le « propriétaire » (ou « investisseur »), doit choisir d'envoyer ou non une certaine somme d'argent à un second joueur, le « receveur », sachant que la somme envoyée est triplée et que le receveur a ensuite la possibilité de restituer au propriétaire toute somme provenant de ce gain. Dans ce cadre simple, nous introduisons un troisième joueur (que nous appelons l'« intermédiaire »), qui joue le jeu de la confiance avec la

⁵ Ce chapitre est associé à un article accepté sous révisions mineures dans la revue *Journal of Economic Behavior and Organisation* (Cochard et al., 2019).

dotation du propriétaire. Enfin, des groupes identitaires sont générés préalablement au jeu de la confiance lors d'un quizz mis en place expressément pour favoriser la coopération. Des couleurs sont aussi assignées à chaque groupe pour renforcer ce lien entre les joueurs. Ainsi, le propriétaire et l'intermédiaire sont parfois du même groupe que le receveur, parfois non. Comme l'investissement de l'expéditeur (qu'il soit le propriétaire ou l'intermédiaire) dans le jeu de la confiance peut être motivé à la fois par ses préférences à l'égard du receveur mais également par ses croyances sur sa réciprocité, ce jeu s'avère être un environnement très propice pour étudier si une discrimination potentielle à l'égard des receveurs provient de préférences basées sur le goût ou de considérations statistiques.

Nous fournissons la preuve que les intermédiaires purs (les individus qui prennent des décisions au nom des « propriétaires » et pour lesquels la rémunération ne dépend pas de leur décision) sont au contraire plus aptes à la discrimination que les « propriétaires » (les individus qui prennent leurs propres décisions). Cependant, la cause de discrimination que nous observons n'est pas de l'hostilité à l'égard des receveurs du groupe extérieur ; elle provient principalement d'un favoritisme à l'égard des receveurs du même groupe. Il semble qu'en raison de leur position, les intermédiaires puissent exprimer leurs préférences pour les membres de leur groupe plus facilement que les « propriétaires », bien qu'ils se sentent responsables de l'argent avec lequel ils jouent. Conformément à la méta analyse réalisée dans le chapitre 1, nos données suggèrent également que les « propriétaires » sont plus sujets aux considérations statistiques que les intermédiaires. Finalement, nous observons un effet de genre parmi les intermédiaires : les femmes envoient significativement moins aux receveurs que les hommes. Cela pourrait provenir d'un plus grand sens des responsabilités à l'égard de la dotation des « propriétaires » (Mestre et al., 2009, Toussaint and Webb, 2005, Macaskill et al., 2002).

Nos résultats semblent donc indiquer que le plus faible comportement discriminatoire observé empiriquement chez les intermédiaires n'est pas un pur effet de l'intermédiation. Ainsi, ce sont les facteurs propres aux marchés testés mais qui ne sont pas présents en laboratoire qui semblent conduire à ce phénomène (par exemple, le fait que les intermédiaires professionnels disposent d'un meilleur niveau d'information sur les caractéristiques des minorités, qu'ils soient soumis à des restrictions plus sévères en matière pénale ou civile, ou encore qu'ils aient des préoccupations plus graves en matière de réputation).

3.4 Chapitre 4 : Discrimination à l'encontre des personnes handicapées mentales, moteurs ou visuelles sur le marché du logement locatif. Une expérience de terrain en France

Un grand nombre d'expériences de terrain ont été menées ces dernières années afin de détecter la présence de discriminations dans les pays de l'OCDE. Cependant la plupart des études se sont concentrées sur l'ethnie, le sexe ou encore l'orientation sexuelle comme variable d'intérêt. Très peu d'études se sont penchées sur la discrimination en fonction du handicap, notamment dans le marché du logement locatif. Pourtant, des millions de personnes sont touchées par un ou plusieurs handicaps dans les pays de l'OCDE (Krahn, 2011) et pourraient faire face à une discrimination génératrice de nombreuses inefficacités économiques et sociales.

Dans le dernier chapitre de cette thèse, nous avons ainsi utilisé la méthode du test de correspondance pour détecter la présence de discrimination à l'encontre des personnes handicapées dans le marché du logement locatif en France. En envoyant 1750 e-mails par la méthode dite « appariée » à des agents immobiliers et des propriétaires privés par l'intermédiaire d'adresses e-mails fictives, nous avons démontré qu'une lourde discrimination à l'encontre des aveugles avec un chien guide, des personnes ayant un handicap mental, ou encore des personnes handicapées moteur avait lieu. Ils sont en effet

significativement moins susceptibles d'être invités à visiter un appartement ou même d'être invités à fournir des informations supplémentaires sur leurs conditions que les personnes sans handicap. La discrimination à leur égard semble d'ailleurs augmenter en fonction du prix du loyer des appartements testés, ce qui pourrait indiquer que cette discrimination provient en partie de considérations statistiques basées sur les moyens financiers des potentiels locataires. Il semble que la zone géographique testée ait aussi un impact sur le taux de réponses des agents et le niveau de discrimination à l'égard des personnes handicapées. En effet, la discrimination est significativement plus faible dans les zones isolées et les demandeurs reçoivent significativement moins de réponses de la part des agents immobiliers et des particuliers dans les zones où le marché du logement est particulièrement « tendu ».

Si l'on s'attarde sur chaque type de handicap, nos résultats indiquent que les personnes ayant un handicap mental (mais pourtant autonomes) font face au plus haut niveau de discrimination directe dans le processus de location. Ils ont en effet plus de deux fois moins de chances d'être invités à visiter un appartement qu'une personne sans handicap. Les aveugles ayant un chien guide, quant à eux, ont presque deux fois moins de chances d'être invités que les personnes valides ; cependant, nous montrons, comme Fumarco (2017) en Italie et Verhaegue et al. (2017) en Belgique, que cette discrimination est purement due à la présence du chien guide, et non au statut d'invalidité. De plus, et une fois encore en cohérence avec ces deux autres études sur la discrimination fondée sur la déficience visuelle, nous constatons également que les agents immobiliers discriminent moins les candidats aveugles avec un chien-guide que ne le font les propriétaires privés. Enfin, le préjudice total le plus élevé est subi par les handicapés moteur. Ils subissent premièrement une discrimination indirecte sur l'offre de logement : moins de 20% des appartements leur sont réellement accessibles, ce qui limite bien entendu leur choix. De plus, parmi ces appartements accessibles, tous ne sont malheureusement pas répertoriés comme tels sur Internet et nécessitent donc des recherches plus avancées et plus

coûteuses. Enfin, parmi les annonces bel et bien répertoriées comme accessibles aux personnes à mobilité réduite, les personnes ayant une déficience motrice ont un tiers moins de chances de recevoir une réponse positive de la part des agents immobiliers et des propriétaires privés. Par conséquent, ils font face à une forte discrimination directe et indirecte sur le marché du logement locatif en France.

Ainsi, il semble que les personnes handicapées soient confrontées à un niveau de discrimination plus sévère que celui auquel sont confrontées les autres minorités dans le marché du logement locatif en France (Challe et al., 2018 ; Le Gallo et al., 2018 ; Bunel et al., 2017 ; Acolin et al., 2016).

4. Apports principaux

Notre thèse propose plusieurs innovations par rapport à la littérature existante. Nous avons ainsi réalisé la première méta-analyse sur la discrimination à l'égard des minorités ethniques dans le marché du logement locatif, permettant d'apporter une grande clarté sur le niveau et les sources de cette dernière. En réalisant cette méta-analyse dans le premier chapitre, nous avons aussi pu mettre en évidence, grâce à une très grande puissance statistique, un effet de genre qui était jusqu'à présent presque inconnu. De plus, nous avons effectué dans le deuxième chapitre la première analyse quantitative de la discrimination à l'égard des minorités sexuelles dans le marché du travail, permettant ainsi de dissiper le flou existant sur le niveau et les déterminants de cette dernière. Les résultats indiquent que le niveau de discrimination subi par les individus ouvertement homosexuels est similaire au niveau de discrimination subi par les minorités ethniques à l'embauche dans les pays de l'OCDE. A travers une variante originale du jeu de la confiance réalisée dans le troisième chapitre, nous avons pu démontrer que le plus faible niveau de discrimination observé empiriquement de la part

des intermédiaires n'était pas dû à l'intermédiation mais plutôt aux facteurs propres aux marchés testés. L'élaboration de politiques efficaces nécessite donc une meilleure compréhension de ces facteurs. Finalement, dans le dernier chapitre de cette thèse, nous avons effectué le premier testing sur la discrimination en fonction du handicap dans le marché du logement locatif en France. Nous sommes aussi les premiers à tester et comparer le niveau de discrimination selon trois types de handicap. De manière cohérente avec la littérature sur le marché du travail, il semble que les personnes handicapées soient aussi la minorité la plus discriminée dans le marché du logement locatif. Cela soulève la question de la discrimination provenant du handicap dans les autres pays de l'OCDE.

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Chapter 1

Ethnic and Gender Discrimination in the Rental Housing Market: Evidence from a Meta-Analysis of Correspondence Tests, 2006-2017

Note: This chapter is associated to an original paper, published in Journal of Housing Economics (Flage, 2018).

1. Introduction

Many field experiments have been carried out with the intention of detecting ethnic discrimination in rental housing markets in OECD countries. The results converge in one direction: there is significant discrimination against ethnic minority groups in the rental housing market. Since Carpusor and Loges (2006) performed the first correspondence test to detect discrimination in the rental housing market, this approach has become widespread because of its practicality and efficiency. No correspondence tests have been conducted exclusively to determine gender discrimination in the housing market, but of the 29 studies conducted to determine ethnic discrimination, 14 of them reported their results by gender too. A qualitative review of a dozen of these 29 studies was produced by Rich (2014) and Oh and Yinger (2015). Since the last review, many studies on the subject have emerged, especially in 2017. There are now more than twice as many studies covering almost three times as many countries, hence the importance of providing a new review, and a quantitative one.

In this chapter, we construct a database of correspondence tests from 25 separate studies containing more than 300 estimates of effect sizes conducted in OECD countries since 2006 in order to detect discrimination against ethnic minorities in the rental housing market, representing a total of over 110,000 e-mails sent to private landlords or real-estate agents.

Our contribution to the field is threefold: first, we present a wide review of studies that have tested for discrimination against minority ethnic groups in the rental housing market by the correspondence testing method, thereby bringing the literature really up to date. Then, we present a quantitative analysis of both ethnic and gender discrimination in OECD countries through meta-analyses in order to measure the extent of discrimination. In addition to presenting the overall results of recent studies, we focus

on subgroups of specific correspondence tests in order to highlight the differences across ethnic background, gender, type of landlords, procedure, continent, and type of information provided in applications. Our conclusions are robust with random effects (R-E), fixed-effects (F-E), and unrestricted weighted least squares (WLS) models.

Our aim is to answer those three simple questions: how, how much, and why?

To our knowledge, no study has performed meta-analyses in order to compare and examine the results in the studies reported. Indeed, to really compare studies, one must be able to code their relative differences. For example, it is not relevant to give as much weight to a study with a very small sample as to a study with a very large sample, even if they come from different countries. Furthermore, very few studies have compared the correspondence tests conducted in the rental housing market by separating the types of responses provided by real-estate agents or private landlords. Two main types of responses exist and are noted by authors and it is not necessarily good to mix ratio of them where this can be avoided. Yet, this has often been the case in literature. That is why we have chosen to present a meta-analysis for each type of response provided by private landlords or real-estate agents.

At the initial stage of the rental process, we find that majority candidates are almost twice as likely to be chosen (receiving a positive response while the other applicant does not) by real-estate agents or private landlords compared to applicants from minority groups. Moreover, individuals belonging to the majority are more than twice as likely to be chosen as Arab/Muslim applicants. Female applicants are almost 30% more likely to be chosen than male applicants. However, this result differs depending on the group of applicants: women belonging to an ethnic minority are 34% more likely to be selected by an agent than men belonging to the same minority. This result is even higher when we compare Arab/Muslim women with Arab/Muslim men: women are 50% more likely than men to be favored. Finally, a woman belonging to the majority has “only” 20% more

chance of being chosen than a man belonging to the majority. Therefore, ethnic and gender discrimination interact: gender discrimination is greater for minority-sounding names than for majority-sounding names. Thus, female majority-sounding names are the most favored, while male minority names are the most disadvantaged (especially Arab/Muslim males). Finally, it seems that real-estate agents discriminate significantly less against minority applicants than private landlords do. We were able to determine that this was at least in part because private landlords display significant statistical discrimination while real-estate agents do not.

The remainder of this chapter is as follows. In the first section, we present an up to date literature review of the 29 studies that tested for discrimination against minority ethnic groups in the rental housing market by the correspondence test method. In the second section, we present the method and data used to carry out the meta-analysis considering publication bias. In the third section, we set out the overall results and discuss them: we present results for all minorities and then focus on ethnic and gender discrimination against Arabs/Muslims only. Moreover, we propose a quantitative analysis of statistical discrimination. In the fourth part, we present a multivariate meta-regression analysis with R-E, F-E, and unrestricted WLS econometric models in order to examine the impact of explanatory variables on the level of discrimination. We conclude the chapter in the final section.

2. Literature review

Since Carpusor and Loges (2006), many correspondence tests have been conducted in order to detect discrimination against ethnic groups in the rental housing market place in OECD countries. We count 29 of them covering 15 countries. We present each of them by continent and country chronologically.

There are two ways of conducting a correspondence test: in 13 studies, the authors used the “single inquiries” procedure. As said previously, in this type of correspondence test, each landlord or real-estate agent receives only one inquiry from a randomly selected applicant. This type of test dispenses with revealing the purpose of the experiment. However, this method does not control the effect of unobservable fixed variables on the response rate and therefore requires many more applications to obtain the same statistical significance as the “matched” procedure, which is used in 16 studies. In this latter procedure, a number of applications (often two, but sometimes more) are sent to the same agent. From now on, we use the terms matched paired, triplet, quadruplet, and quintuplet, when two, three, four, and five applications are sent to the same agent.

This literature is very widespread in **North America**, and particularly in the **USA**. In this country, five fields experiments have been performed in recent years.

Carpusor and Loges (2006) were the first to perform a correspondence test in this market, replying over ten weeks in 2003 to 1115 adverts for rental properties in Los Angeles by sending single inquiries in order to test for discrimination against applicants with African-American and Arab/Muslim-sounding names. According to the results, African-American and Arab/Muslim names received significantly fewer simple and positive responses than applicants with White American-sounding names. The response rate for these two minority ethnic groups was respectively 20 and 30 percentage points lower than for White Americans. Moreover, the tests did not find any differential treatment related to the type of agents: real-estate agents discriminated as much as private landlords.

Between January and May 2009, Friedman et al. (2010) used a matched triplet procedure in order to test for discrimination against Hispanic and African-American groups in Dallas and Boston. In this first correspondence test studying discrimination against Hispanic people, almost 1500 e-mails were sent out in total. By comparing simple response rates between ethnic groups, they found significant discrimination in Dallas

only. However, by comparing positive response rates, African-American and Hispanic applicants were significantly less likely than White Americans to be invited to inspect the units in both cities, with a greater difference for African-Americans. Again they did not report any differential treatment by type of agents.

Discrimination towards African-American applicants was also reported by Hanson and Hawley (2011). During three months in 2009, they sent almost 10,000 e-mail inquiries in a matched pair procedure to landlords in the 10 largest US cities. In addition to ethnic background, the four fictitious applicants also differed by social class. This was signaled in inquiries by syntax and varying degrees of financial stability. Across most cities in their sample, the response rate for applicants with African-American-sounding names was 4–6% points lower than for White Americans. Discrimination was higher in neighborhoods near to “tipping points” (when the majority share is between 80% and 95%) and for units advertised as part of a larger building. Finally, the authors reported statistical discrimination: when the content of the e-mail inquiry suggested an applicant of high social standing, ethnic discrimination was small and not significant.

Another large-scale experiment was conducted at the same time by Ewens et al. (2014) and also provides evidence for discrimination against Black people in the USA. By sending 14,000 single inquiries to private landlords in 34 major US cities between September and October 2009, they found a level of discrimination close to that reported by Hanson and Hawley (2011): the positive response rate for applicants with Black-sounding names was 9.3% points lower than for Whites when no other signals were included in the inquiries sent. However, even if providing positive information had a favorable impact on response rates for both groups, they showed that the racial gap widens in the switch from negative to positive signals, maybe suggesting that agents attribute more weight to signals provided by White than Black candidates. It seems that women applicants received slightly more responses than men, but that result was not significant.

The latest correspondence test conducted in the United States was by Hanson and Santas (2014) between February and March 2011 in 21 major cities. Like Friedman et al. (2010), they used written tests in order to study discrimination against Hispanic people. They designed different fictitious applications by separating candidates into three groups by names: Whites, Hispanics who appeared to be assimilated into American culture, and Hispanics who appeared to be recent immigrants. By sending more than 6000 e-mails by a matched pair procedure, they did not find significant evidence of discrimination against Hispanics with assimilated names while they report discrimination against non-assimilated names: they received less favorable treatment with margins of net discrimination as large as more than 4% of landlords, reminding us of the importance of names in correspondence tests. They even highlighted the fact that Hispanics with assimilated names received significantly more responses than White applicants when the proportion of White residents in neighborhoods surrounding housing units is less than 28%.

According to these studies, it appears that discrimination against Hispanics is significantly lower than discrimination against Blacks in the US rental housing market.

Hogan and Berry (2011) carried out between late March and early June 2007 the only experiment testing for ethnic discrimination in the rental housing market in **Canada** by correspondence tests. They created 10 fictitious groups of applicants: White, Black, Asian, Arab/Muslim, and Jewish, varying by gender and sent more than 5000 single e-mail inquiries to private landlords and real-estate agents in Toronto. They reported relatively severe discrimination against Arab/Muslim men: their response rate was 10 percentage points lower than for White men. They also found modest but significant discrimination against men with Asian and Black-sounding names and against Arab/Muslim women. Moreover, it seems that women tended to receive more responses than men (especially for Arab/Muslim and Asian applicants).

Throughout **Europe**, more than 20 field studies have been conducted recently.

The “earliest” experiments were conducted in the Nordic countries, especially **Sweden**. In this country, four known studies using correspondence tests have been carried out.

The first experiment, by Ahmed and Hammarstedt (2008) between February and March 2007, in which 1500 e-mails were sent to private and corporate landlords in Stockholm, Gothenburg, and Malmö, and in non-metropolitan areas revealed the presence of both ethnic and gender discrimination in the Swedish rental housing market. They created three fictitious applicants: one Swedish male, one Swedish female, and one Arab/Muslim male. Hence, they tested gender discrimination on majority applicants only and ethnic discrimination on male applicants only. Using a matched pair procedure, the results indicate that Arab/Muslim-sounding names were more than half as likely to get a simple and positive response from agents than Swedish candidates and women applicants were almost twice as likely to be invited to view an apartment as men. This result was significantly more pronounced in metropolitan than in non-metropolitan areas. Moreover, it seems that real-estate agents discriminated less against Arab/Muslim candidates than private landlords and they did not discriminate against male applicants.

The following year and at the same period, Ahmed et al. (2010) conducted another field experiment in Sweden. In order to study the impact of providing more information in applications sent to landlords and thus test the presence of statistical discrimination, they created four fictitious male applicants: one Swedish and one Arab/Muslim making a simple request without any further information, and one Swedish and one Arab/Muslim providing detailed information about their employment, education, marital status, and income. They responded to a little over 1000 ads by single inquiries and still found strong discrimination against Arab/Muslim applicants. Finally, adding more correct information about applicants did not reduce discrimination, suggesting that discrimination against

Arabs/Muslims in Sweden's rental housing market was based on preferences rather than lack of information.

Between March and May 2010, Bengtsson et al. (2012) tested gender and ethnic discrimination against male and Arab/Muslim applicants of high social status by sending more than 1200 e-mails to private landlords in Stockholm. By testing the gender effect for minority applicants as well, they extended the study by Ahmed and Hammarstedt (2008). They found gender discrimination especially for majority candidates and ethnic discrimination was only present in the suburbs of Stockholm. Interestingly, the results did not confirm whether men with Arab/Muslim-sounding names face discrimination.

During a six-months period from late 2010 to early 2011, Carlsson and Eriksson (2014) provided the latest field experiment in this country by sending more than 5800 single inquiries to corporate or private landlords in response to ads for apartments throughout Sweden. They tested ethnicity, age, gender, and employment status. Consistently with Ahmed and Hammarstedt (2008), Arab/Muslim-sounding name applicants received significantly fewer responses from agents than Swedish candidates and female applicants were more likely to be invited to view the apartment than male applicants. Moreover, providing more positive information in applications did not reduce the level of discrimination and the positive effect of having a job was greater if the landlord was a company. Finally, ethnic discrimination was mainly concentrated outside the metropolitan areas.

A very high level of discrimination against Arab/Muslim people has also been highlighted by Andersson et al. (2012) in **Norway**. Using the single inquiries procedure, they tested for discrimination related to status, gender, and ethnic background throughout Norway between December 2009 to March 2010 by responding to 950 adverts for rental apartments. Arab/Muslim applicants were almost 13 percentage points less likely to receive a response than Norwegian applicants and women applicants tended to receive

more responses from agents than men (statistically significant only for majority applicants). Moreover, it seems that providing positive information in inquiries had more impact for Arabs/Muslims than for Norwegians, thereby reducing the gap between them, which is an indication that it was not only a matter of taste-based discrimination.

Between September and October 2014, 1367 single inquiries e-mails were sent from all over **Denmark** by Herby and Nielsen (2015) in order to test discrimination related to status, gender, age, and ethnic background. A significant discrimination against Arab/Muslim applicants was detected. They were 8 percentage points less likely to receive a response than applicants with Danish names. Moreover, the authors highlighted that Arab/Muslim men faced more discrimination than women: 9% against 6%. Within ethnic groups, it seems that women received more responses than men (especially for minority applicants). As reported by Andersson et al. (2012), providing positive information about applications increased the response rates for both ethnic groups, but significantly more for minority applicants, indicating that discrimination did not arise exclusively from the agents' preferences. Finally, and surprisingly enough, individuals aged 25 received significantly more responses than individuals aged 45.

A high level of discrimination against Arabs/Muslims was also recently revealed by Öblom and Antfolk (2017) in **Finland**. By sending almost 1500 inquiries in a matched pair procedure during December 2015 and April 2016 to private landlords in Helsinki, Turku, Tampere, and other selected locations around Finland, they showed evidence of both gender and ethnic discrimination. Arab/Muslim applicants were almost half as likely to get a positive response as Finnish applicants and Arab women were twice as likely to get a positive response as Arab men. This gender effect was significantly lower for majority applicants, thus the authors reported an interaction of ethnic and gender discrimination: gender discrimination was greater for minority than majority applicants.

The first study in **Iceland** was carried out very recently by Kopsch et al. (2017), who directly and explicitly addressed discrimination against the largest minority group of labor-immigrants in Iceland: Polish people. Four fictitious applicants differing by gender and ethnicity each applied for 127 apartments in the eight largest Icelandic cities. The results suggest that both ethnic and gender discrimination occur against Eastern European men in the Icelandic rental housing market: they received significantly fewer responses from agents than men with Icelandic-sounding names while Eastern European women were clearly favored compared to them. Finally, it seems that the gender effect is not significant for the ethnic majority.

Note that discrimination against Arab/Muslim people in the Nordic countries is extremely high.

In **Western Europe**, this method has recently become popular, especially in France and Germany.

In **France**, four studies have been conducted recently. Acolin et al. (2016) sent 1800 single inquiries to landlords in six broad regions (Northwest France, Northeast France, Southeast France, Southwest France, Central and Western Paris, and Eastern Paris) over eight weeks in the spring of 2014 in order to detect any discrimination against five immigrant groups: Arabs/Muslims, Sub-Saharan Africans, Turks, Eastern Europeans, and Hispanics. They found that Eastern European and Hispanic groups were not discriminated against while Arabs/Muslims, Sub-Saharan Africans, and Turks were 16 to 22 percentage points less likely to receive a response than applicants with French names. Female Eastern European applicants reportedly received more responses from agents than male applicants did. The gender difference was not significant for the other ethnic groups.

Between October 2015 and February 2016, Bunel et al. (2019) tested for discrimination against Kanaks in New Caledonia (under French administration) using a matched

quadruplet procedure by sending 1368 e-mails in response to 342 real-estate rental ads in Greater Nouméa, the capital of New Caledonia. They found that Kanaks were 13 percentage points less likely to receive a response than applicants with European names. Moreover, Bunel et al. highlighted the presence of statistical discrimination against Kanaks: an employment stability signal significantly reduced the gap between Kanaks and European applicants by nine points (from 13 to 4). Finally, they showed that discrimination against Kanaks was greater with private landlords than professionals.

Bunel et al. (2017) used the same protocol to test for discrimination in access to housing against Arabs/Muslims in Paris. Between April and May 2016, they sent 2016 messages in response to 504 rental ads. The results suggested that Arab/Muslim applicants were one-third less likely to receive a favorable response to their request to view than applicants with French-sounding names. However, this time, a signal of professional and financial stability greatly increased the chances of access to housing for candidates of French origin only, increasing the gap between fictitious applicants (from one-third to almost two-thirds). This result suggests a strong taste-based discrimination against Arab/Muslim applicants in Paris.

Finally, Le Gallo et al. (2018) carried out the most extensive experiment so far anywhere on access to rental housing by sending 25,040 applications in response to 5008 ads in the 50 largest French urban areas in order to detect discrimination against Arabs/Muslims and Sub-Saharan Africans in France.⁶ The results indicated that applicants with French-sounding names received a response to 14% of their requests while Arab/Muslim and Sub-Saharan applicants received respectively a response to only 10% and 9.5% of their requests, that is, in relative terms, almost a third less. They highlighted the presence of

⁶ Some data in this article are omitted from the meta-database, so as not to add unnecessary heterogeneity. Indeed, two fictitious applicants clearly indicated in the e-mail that they currently lived in “rent-controlled housing” in a deprived area while none of the other fictitious profiles in the meta-database indicated their current place of residence.

statistical discrimination: providing a signal of financial stability reduced discrimination (but did not eradicate it). Moreover, the testing did not reveal the existence of discrimination related to age (41 to 22 years old) and address (deprived neighborhood or not). Finally, they showed that real-estate agents discriminated significantly less against minority applicants than private landlords did.

Note that according to these studies, discrimination against Arab/Muslim and Sub-Saharan applicants is very high in France.

In **Germany**, three studies have been carried out over the last three years.

Using the matched pair procedure, Auspurg et al. (2017) tested for ethnic discrimination against Turkish people in the Munich rental housing market between December 2006 and January 2008 by responding to 637 adverts for rental properties. They created fictitious applicants: German and Turkish males, who differed in terms of occupation (low, medium, and high social status). They found that applicants with Turkish-sounding names were 9 percentage points less likely to receive a response than applicants with German names, and providing a signal of high status significantly reduced the difference in treatment between Turkish and German applicants. This last result was only confirmed for real-estate agents; it seems that private landlords tended to discriminate by ethnic background only (taste-based discrimination).

Mazziotta et al. (2015) conducted two investigations on discrimination based on ethnic and sexual orientation in 12 large German cities (6 per study) in June 2013 and June 2014 responding by single inquiries to almost 800 adverts for rental properties. Four profiles of fictitious applicants were created: two German and two Turkish couples,⁷

⁷ Because its protocol was too different from other studies and so as not to create unnecessary additional heterogeneity, we chose to omit data from this article.

varying by sexual orientation. They revealed evidence of discrimination based on ethnic background alone.

More recently, two teams of data journalists (BR Data and Spiegel Online) conducted a large-scale experiment in June and September 2016, sending around 20,000 inquiries in response to almost 7000 rental ads in the 10 largest German cities to test discrimination against Arab/Muslim, Turkish, Italian, and East European applicants. Using the matched triplet procedure with applicants varying by ethnicity and gender, they first found that individuals belonging to minorities were discriminated against in the rental housing market to varying extents depending on their ethnic background. Arab/Muslim and Turkish applicants were invariably those suffering most from discrimination (respectively 27 and 24 %), but discrimination against East European and Italian candidates remained significant (12 and 8 %). Moreover, in addition to ethnicity, gender also played a significant role: the authors highlighted an important gender effect between groups for Turkish and Arab/Muslim applicants: men with Turkish and Arab/Muslim-sounding names were at more of a disadvantage compared to German men than Turkish/Arab/Muslim women were compared to German women. Although the authors did not highlight it in this article, a gender effect within groups existed too: female applicants received more responses from landlords than male applicants, for each group. Finally, private landlords discriminated against foreign applicants more strongly than real-estate agents did.

Thus, all studies conducted in Germany have reported a high degree of discrimination against Turkish applicants.

Between March and July 2010, Baldini and Federici (2011) sent more than 3676 single inquiry e-mails to private landlords or real-estate agents in 41 cities of **Italy** in order to detect discrimination against Arab/Muslim and Eastern European applicants (varying by gender and socio-economic information). About 3000 e-mails were sent in due form

while the remainder were deliberately poorly worded.⁸ Applicants with Italian-sounding names received the highest positive response rate from agents (62%) while Arab/Muslim applicants received the lowest (44%). E-mail inquiries signed using typical East-European-sounding names showed a lower level of discrimination than Arab/Muslim ones (12% compared to 18%). Moreover, discrimination was higher against applicants with male rather than female-sounding names, in particular for the Arab/Muslim group, and women were more likely to receive a positive response than men, for any groups. They found that discrimination seemed much higher in Northern than in Central or Southern Italy and they did not find any difference between private landlords' and real-estate agents' behavior. Providing more correct information in the content of the e-mail inquiry slightly reduced the gap between minority and majority applicants, suggesting the presence of some statistical discrimination. Finally, grammatical errors in the content of the e-mail did not seem to reduce the probability of receiving a positive response, for either of the minority groups considered.

Two separate experiments were carried out by Bosch et al. (2010, 2015) in **Spain** between January and March 2009 in 20 of the largest Spanish cities and between December 2009 and June 2010 in Madrid and Barcelona. Using different matched procedures in the first experiment in response to 1809 rental ads and sending 1186 single inquiries in the second, Bosch et al. tested for the existence of discrimination against men and women with Arab/Muslim-sounding names. In the first experiment, they investigated whether providing a greater amount of information in e-mails indicating professional and financial stability increased the chances of access to housing (test for statistical discrimination) whilst they tested the existence of discrimination related to neighborhoods in the second. They found very similar results in both experiments: Arab/Muslim applicants faced significant discrimination in the Spanish rental housing market, they were 15 to 18 percentage points less likely to receive a response than Spanish applicants. The results

⁸ We have included only the data related to e-mails written in proper form.

indicated the presence of statistical discrimination: providing positive information about the status of applicants significantly narrowed the gap between majority and minority applicants. Moreover, real-estate agents seemed to discriminate significantly less than private individuals. In neighborhoods of Madrid and Barcelona with a scarce presence of Arabs/Muslims, the response rate was 30 percentage points lower for Arab/Muslim-sounding names compared to Spanish applicants, while this differential decayed towards zero as the proportion of Arab/Muslim residents increased. Finally, both studies indicated that Arab/Muslim women were favored compared to Arab/Muslim men.

In **Belgium**, Heylen and Van den Broeck (2016) focused on discrimination against ethnicity, disability, and gender combined with financial means (single mother) and only on financial means with a written and phone approach. Almost 700 tests were conducted by telephone by the matched pair procedure and almost 1800 single e-mail inquiries were sent to private landlords in the three Belgian regions between February and May 2013. Regarding ethnic background, Arab/Turkish men were discriminated against in both methods and the level of discrimination was higher in the e-mail approach (by five percentage points). The results confirmed discrimination based on disability, gender combined with financial means, and on financial means too, and these results were consistently more pronounced with the e-mail approach. “Possibly, landlords feel more comfortable when they can discriminate by e-mail than by phone, where a direct contact takes place with the person”. They also tested simple gender discrimination but only by the telephone approach and found that Arab/Turkish men received significantly fewer invitations to view properties than Arab/Turkish women.

In order to test discrimination toward similar characteristics with other ethnic groups, a large scale experiment was conducted more than three years later by Verhaeghe et al. (2017) in the Brussels Capital Region by means of correspondence tests and in-person tests conducted over the phone. More than 20,000 messages were sent to real-estate agents and 1542 successful calls were made by phone in a matched pair procedure,

yielding results consistent with Heylen and Van den Broeck (2016). The results of the written approach indicate that Eastern European applicants were not discriminated against while men with Sub-Saharan African and Arab/Muslim-sounding names were 21 to 23% less likely to receive a response than men with Belgian names. Arab Muslim women faced significantly less discrimination than men while there was no difference for Sub-Saharan applicants. Once again, the authors found that real-estate agents were more likely to discriminate in the case of written applications than by telephone.

In **Eastern Europe**, only two field studies have been completed.

Between December 2009 and August 2010, Bartoš et al. (2016) responded to 1800 rental ads mostly distributed in Prague by the single inquiries procedure in order to test for discrimination against Roma and Asian minorities in the rental housing market in the **Czech Republic**. Applicants with minority-sounding names faced severe discrimination: they were almost half as likely to get a positive response as those belonging to the majority. Moreover, the authors reported statistical discrimination: providing more correct information in the content of the e-mail inquiry narrowed the gap between minority and majority applicants.

For **Slovakia**, Sacherová (2016) sent almost 400 e-mail requests in the matched pair procedure to private and real-estate agents between November 2015 and January 2016 in order to test for discrimination against Roma in the sale and rental housing market.⁹ The results indicated that applicants with Roma-sounding names were 8 to 10% less likely to receive a positive response than applicants with Slovakian names. Moreover, the rate of positive responses to applications for rental offers was on average 16 to 23% lower than in case of ads for sale.

⁹ We have taken the data for the rental housing market only.

Finally, in **Israel**, one study indirectly investigated ethnic discrimination in the rental housing market by the correspondence testing approach. Sansani (2017) studied discrimination against the religiously observant in the Israeli rental market. However, to get a sense of the magnitude of the discrimination against religious applicants relative to discrimination based on other characteristics, such as ethnic background, he also tested for discrimination against individuals with Arab and Eastern European sounding names. Four male fictitious groups were created: Jew signaling religion, Jew with no signal, applicants with Arab and Eastern European-sounding names.¹⁰ More than 1800 single inquiries were sent to private landlords in most major cities in Israel. A significant discrimination against applicants reporting their religion was found: Jews reporting religion receive almost 10 percentage points fewer responses than Jews with no signal. Discrimination against ethnic minorities was also very high: East European-sounding names faced similar discrimination to religious Jew applicants while Arab candidates were more than half as likely to get a response than non-religious Jewish candidates.

3. Method and data

As all of these correspondence tests were conducted in very similar ways, we used meta-analysis to provide a quantitative summary of the existing literature in a systematic manner. In order to increase comparability, we decided to exclude phone call audits and personal approaches and focus instead on the correspondence tests carried out on the rental housing market in OECD countries.

¹⁰ We omitted the profile “Jews signaling religion” from the meta-analysis, in order to compare ethnic backgrounds alone and not to add religion as a characteristic.

We chose this method as it has been the one most commonly used over the past few years, targeting countries with similar levels of development, lifestyles, living standards, the same type of democratic governance, and similar market economies. It makes sense to exclude correspondence tests made in the shared housing market, (e.g. Carlsson and Eriksson, 2015; Diehl et al., 2013; Ghoshal and Gaddis, 2015) given that it would otherwise involve taking into account owners' decisions whether or not to share their homes, which is very different from the concept of the standard tenancy.

To search for the data, we used *Google scholar*, Econlit, and Elsevier's *ScienceDirect*, with the following keywords: "discrimination", "housing", "rental housing", "correspondence test", "ethnic discrimination", "field experiment". We also relied on the review by Rich (2014) and by Oh and Yinger (2015) and attended seminars. Finally, we also included data from the DALTON project (France) in which we participated.¹¹

We included data from 25 studies, covering 14 countries: France (Acolin et al., 2016; Bunel et al., 2017; Le Gallo et al., 2018; Bunel et al., 2019), Canada (Hogan and Berry, 2011), the Czech Republic (Bartoš et al., 2016), Slovakia (Sacherová, 2016), Finland (Öblom and Antfolk, 2017), Denmark (Herby and Nielsen, 2015), Germany (Auspurg et al., 2017; Team BR Data and Spiegel Online, 2017), Italy (Baldini and Federici, 2011), Norway (Andersson et al., 2012), Spain (Bosch et al., 2010; Bosch et al., 2015), Sweden (Ahmed et al., 2010; Ahmed and Hammarstedt, 2008; Carlsson and Eriksson, 2013), Iceland (Björnsson et al., 2017), Israel (Sansani, 2017) and finally the USA (Carpusor and Loges, 2006; Ewens et al., 2014; Friedman et al., 2010; Hanson and Hawley, 2011; Hanson and Santas, 2014), which are very similar countries in terms of their human and economic development.

¹¹<https://anr.fr/Project-ANR-15-CE28-0004>

We could not get enough data to include Heylen and Van den Broeck (2016), Verhaeghe et al. (2017), and Bengtsson et al. (2012) in the meta-analysis.¹² For most analyses, almost every study can be divided into several subgroups, depending on the relevant ethnic group, gender, information provided in applications, procedure used by the experimenter to carry out the correspondence test, etc. As decisions are taken by different private landlords or real-estate agents and are therefore not very likely to influence each other, each subgroup can be treated to some extent as an independent experiment. Our main variable of interest is origin/ethnicity, but subgroups also allow us to evaluate discrimination by gender, type of landlord, type of response, type of information provided in the applications, type of procedure, and so on.

Note that the type of apartment tested in these correspondence tests was very similar in the different studies and the corresponding rent accounted for a similar share of median net income of individuals (around 50%), so we could not test for discrimination according to the type of flat, which is one of the methods of testing statistical discrimination.¹³ However, this makes for a more homogeneous database.

We were able to obtain data about 10 minority groups from these studies: African, Arab/Muslim, Asian, East European, Hispanic, Italian, Jewish, Kanak, Roma, and Turkish.

All of these authors have their very own way of reporting the results: in terms of net discrimination rates, risk ratios, odds ratios, etc. Unfortunately, there is no established standard on this subject. To clarify matters, we present the results of meta-analyses on

¹² The response rates for the baseline (majority men) are not provided and we are unable to calculate them from the information provided.

¹³ We hypothesized that if real-estate agents or landlords do not have sufficient information about minority applicants, and consider foreign ethnicity as a proxy for lower income (due to higher unemployment), they may avoid spending time answering applications from applicants they perceive as being poorer than others, so they can be expected to discriminate more against minority applicants when the rental price is high.

the same basis, in terms of *absolute discrimination*, through the odds ratio, which is the ratio of two odds: the odds of getting a response for the *minority group* over the odds of getting a response for the *majority group*. Put differently, it is the probability of being chosen/favored for an individual belonging to the minority group over the probability of being chosen for a majority applicant.

For example, if only 5% of the minority applicants and 10% of the majority applicants received a response from an agent, we compute the odds as following: $0.05/0.95$ is the odds for minority applicants (share of individuals for whom the event occurs divided by the share of individuals for whom it does not occur) and $0.1/0.9$ is the odds for majority applicants. Thus the odds ratio is 0.47: minority applicants have 53% lower odds of receiving a response from a private landlord or a real-estate agent, compared to majority candidates. A majority applicant in this case is slightly more than twice as likely as a minority applicant to be chosen by a real-estate agent or a private landlord.

The odds ratio is therefore another way of calculating the risks, although a little less intuitive than a simple risk ratio at first glance, but we follow Borenstein et al. (2009): “Many people find this effect size measure less intuitive than the risk ratio, but the odds ratio has statistical properties that often make it the best choice for a meta-analysis”. Note however that we reach similar conclusions with risk ratios.

4. Consideration of publication bias

Studies reporting significant results are much more likely to be published than studies reporting negative (non-significant) results. Publishing only those results that report a significant discovery disrupts the balance of results.

The most obvious way to avoid this publication bias is to try to find as many studies as possible having used correspondence tests in order to determine whether there is discrimination in the housing market but that have not been published. To do this, we analyzed posters and congress abstracts and we participated in seminars on the topic. We were able to find a large number of unpublished studies. We note, however, that they also report high discrimination.

The statistical evaluation of the existence of publication bias can be implemented in different ways. The most common one is through a funnel plot (a graph in the form of an inverted funnel). This type of graph shows, according to the precision of studies (or the sample size) on the y-axis, and the effect size on the x-axis, that some publications seem to be missing: the distribution of the dots is not homogeneous around the true value, not filling an image of an inverted funnel. However, this simple method is not powerful enough to determine with certainty whether a publication bias is present. For example, the heterogeneity observed between studies may be another explanation for possible funnel plot asymmetry. Moreover, a limited number of studies does not allow this test to detect a publication bias (Egger et al., 1997). Statistical tests have been performed to provide a more advanced assessment of publication bias than inspection of funnel diagrams can. The best known of these is Egger's test.

When we present our results in term of odds ratios, we get a somewhat asymmetrical funnel plot (see Figure 1.3 in the appendix). In the absence of heterogeneity (or publication bias), 95% of the studies should lie within the funnel defined by the straight lines. Statistical heterogeneity refers to differences between study results beyond those attributable to chance. The relative asymmetry of the funnel plot (checked using Egger's test: $z = -2.9393$, $p = 0.0033$) suggests that there are some missing studies, reporting even more discrimination than the average.

The “trim and fill” method (Duval and Tweedie, 2000) is a possible “correction” process if a significant publication bias is observed: the results of the “missing” studies in the mirror image are calculated as being strictly opposite those of the identified studies. Adding these fictitious missing studies provides a new summation of results. Using this method, we find that no study is missing on the right side of the funnel plot but there are three studies missing on the left side, suggesting that even greater ethnic discrimination exists in the rental housing market in OECD countries.¹⁴

However, whether the bias is corrected or not, the main results are of the same order of magnitude (see figure 1.4 in the Appendix). So, as the results reported after correcting for bias are very similar and as we cannot affirm the presence of a publication bias, we choose to present the uncorrected results.

Even if “random effects” (R-E) seems to be the most appropriate method in this case, we have also detailed the results of the MRA with the “unrestricted weighted least squares” (WLS) method (Stanley and Doucouliagos, 2015, 2017), which is a more suitable method in the case of publication bias. We have also detailed the results with the “fixed effects” (F-E) model. The effect sizes found with these three models are very similar and lead us to the same conclusions.

5. Meta-analysis results

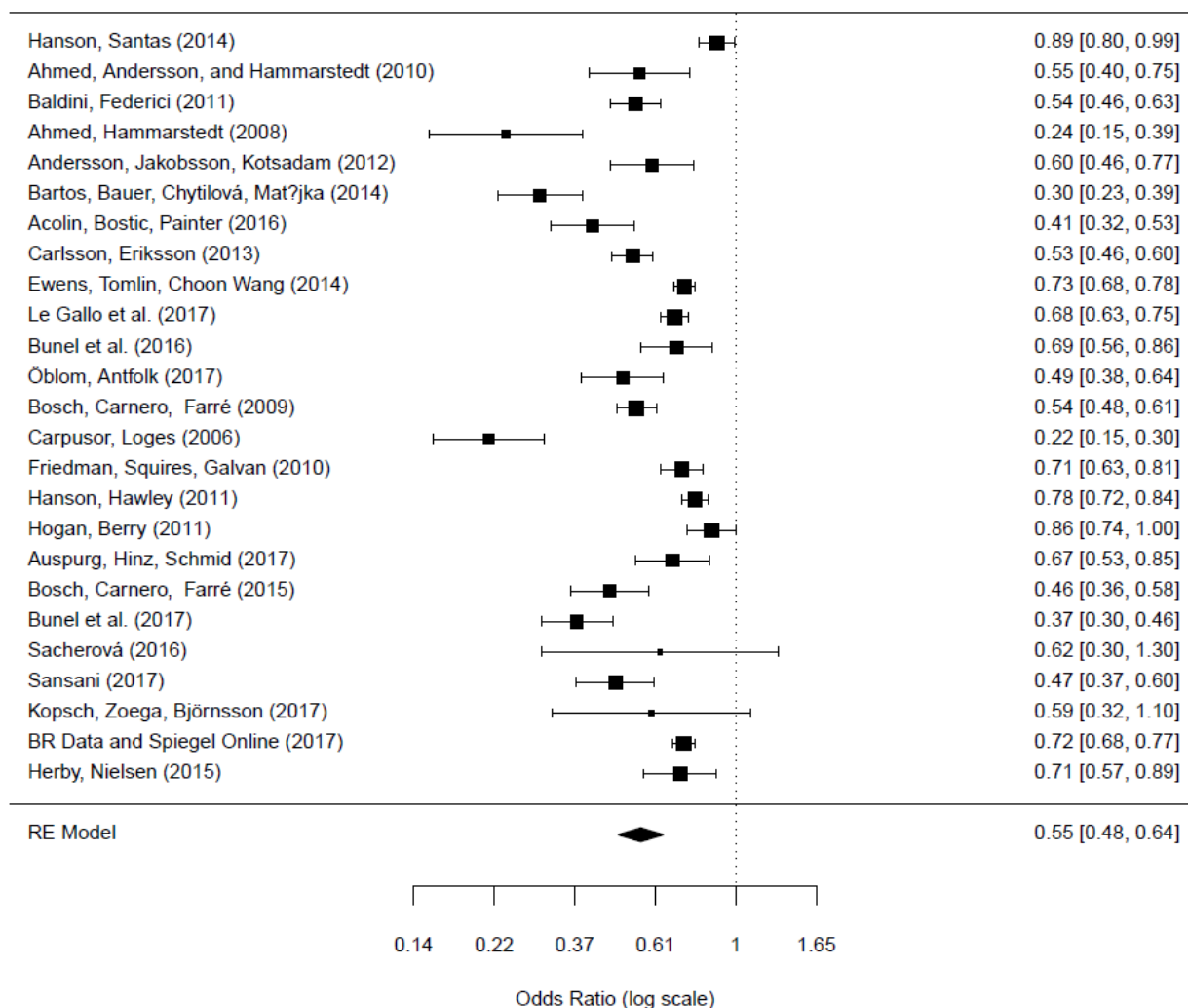
First, we present the overall results of a meta-analysis that takes into account the discrimination reported in all studies. To present the overall results, we use a random

¹⁴ If we report results for gender discrimination only, the “trim and fill” method does not detect any missing studies and the funnel plot is symmetrical (Egger’s test: $z = 0.0201$, $p = 0.9840$)

effect model,¹⁵ as it seems reasonable enough to assume that the real effect size is not exactly the same for every study (presence of between-studies heterogeneity).

5.1 Ethnic and racial discrimination

Figure 1: Ethnic and Racial Discrimination in Rental Decisions



Note: This forest plot (figure 1) displays the odds ratios in log scale of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 25$: study level).

¹⁵ Our conclusions with a fixed-effect model run along the same lines, but as this model assumes that all studies share the same real variable of interest, it places far too much emphasis on the three studies with the largest sample size, and virtually ignores the others. Yet these latter studies, although having a smaller sample, capture an effect that these three large studies do not.

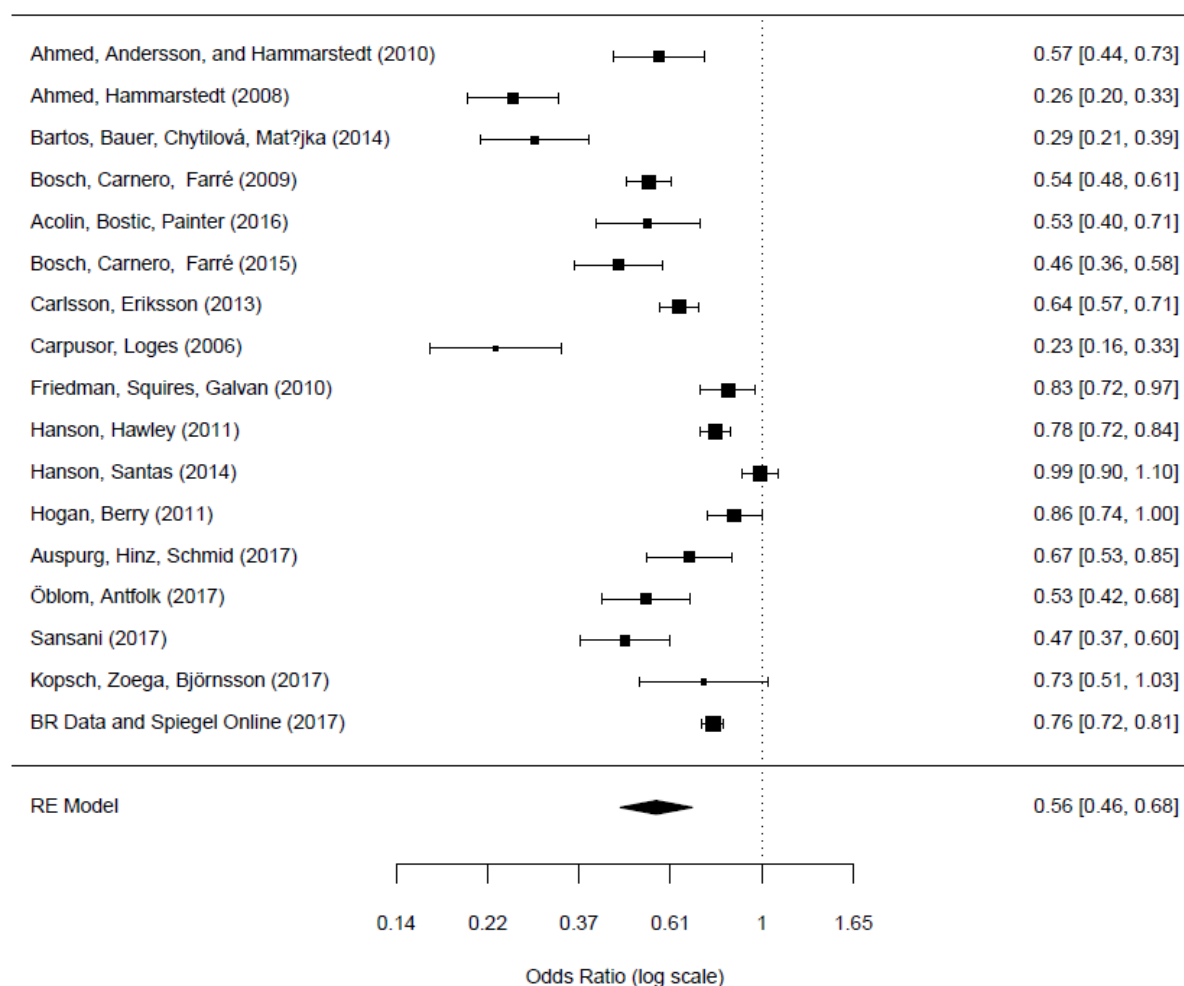
The left-hand column lists the names of the authors of these correspondence tests. The right-hand side of the forest plot indicates the odds ratio of each study and their 95% confidence interval. The weight given to each study is represented by the size of the boxes. The dotted vertical line (y-axis) indicates equal treatment (no discrimination). The lozenge indicates the global effect size.

The odds ratio is 0.55: minority applicants have 45% lower odds of receiving a response from a private landlord or a real-estate agent, for equal information provided in the applications, compared to majority candidates. Each study includes a number of subgroups, and, in order to gain robustness, we can use them to make this global meta-analysis. We find at this level an odds ratio of 0.57 ($N = 268$: subgroup level), which is the same order of magnitude. Thus, a majority candidate is almost twice as likely to be favored as a minority candidate in OECD countries. There is a substantial ethnicity-based discrimination in the housing market in OECD countries. Interestingly, our results are similar to those of Zschirnt and Ruedin (2016) relating to hiring decisions in the labor market. As often in the literature (e.g. Bartoš et al., 2016), we can separate responses provided by private landlords or real-estate agents into two categories: the “simple response”, that results in a contact, regardless of whether it is a positive or a negative response and the “positive response”, meaning that the landlord or real-estate agent requested further information or directly invited the applicant to a showing. Thus “positive response” is a subgroup of “simple response”, but, as some studies have reported only one type of response and as these different types of response do not represent the same thing, it may be wise not to mix corresponding odds ratios. Indeed, despite its practicality, comparing *simple response* rates may not be the best way to determine discrimination and might tend to underestimate it, “to the extent that majority rates could include more positive responses than minority rates” (Ewens et al., 2014). Fortunately, our database has more *positive response* ratios than simple ones because it seems to have become the norm in this literature although it is easier for authors to

record *simple response* rates for each ethnic group rather than *positive response* rates that require considerable sorting. Some of these authors even reported the rate at which the landlord invited the applicant to a showing without any further inquiries, which is even more accurate (but requires even more tedious sorting).

Thus, we present a meta-analysis of each type of response provided by real-estate agents or private landlords in order to determine whether the type of response has an impact of the level of discrimination. We show in Figure 1.1 the level of discrimination reported by “simple response”.

Figure 1.1: Ethnic and Racial Discrimination in Rental Decisions (Simple response)

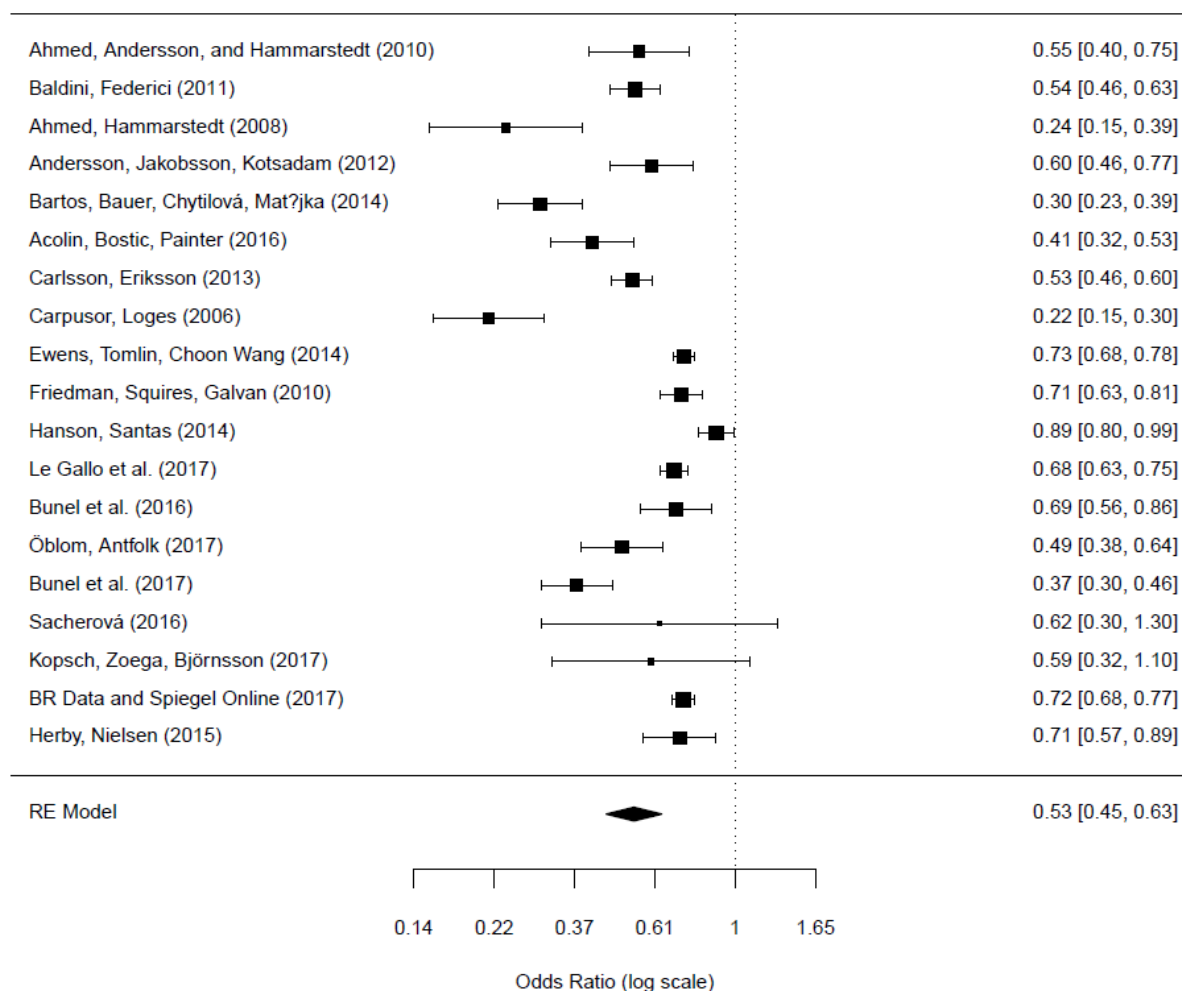


Note: This forest plot (figure 1.1) displays the odds ratios in log scale of each study (point estimate as square, two standard errors as lines) by simple response. The lozenge at the bottom indicates the effect size across studies ($N = 17$: study level).

We can see that the effect size is 0.56 at study level ($N = 17$): minority applicants have 44% lower odds of receiving a response from a private landlord or a real-estate agent, compared to majority candidates. At the subgroup level we find an odds ratio of 0.62 ($N = 109$: level of subgroups), which is similar to the study-level result. Thus, a majority candidate is almost twice as likely to be favored as a minority candidate in OECD countries.

Finally, Figure 1.2, which is also a forest plot, presents the results of all studies composed of positive response rates.

Figure 1.2: Ethnic and Racial Discrimination in Rental Decisions (Positive response)



Note: This forest plot (figure 1.2) displays the odds ratios in log scale of each study (point estimate as square, two standard errors as lines) by positive response. The lozenge at the bottom indicates the effect size across studies ($N = 19$: study level).

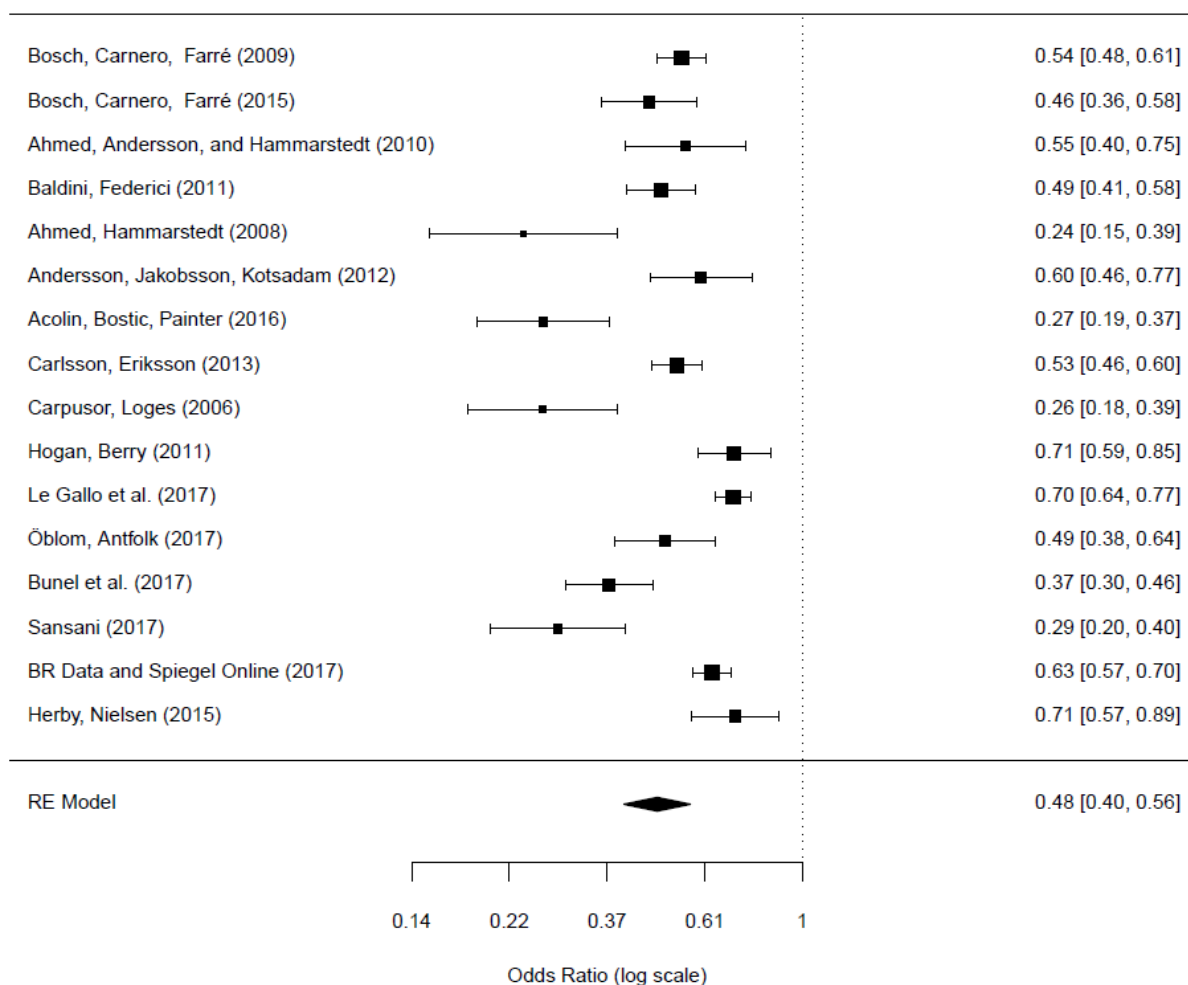
The effect size across studies noted by *positive responses* is 0.53: minority applicants have 47% lower odds than majority applicants of receiving a positive response or being invited to provide further information from a private landlord or a real-estate agent. At subgroup level ($N = 159$), the odds ratio is 0.55, around the same order of magnitude. Hence, a majority applicant is almost twice as likely to be chosen by real-estate agents or private landlords as a minority applicant in OECD countries. It seems that the type of response has a small negative impact on the level of discrimination, suggesting the occurrence of positive responses is a little higher in *simple response* majority rates than in minority rates.

We now choose to focus our meta-analysis on only one minority: the Arab/Muslim group, for which we have a lot of data and we know it is a minority facing wide discrimination. Figure 2 present the results of a meta-analysis that takes into account the discrimination reported in all studies against Arabs/Muslims.

The effect size across studies is 0.48: Arab/Muslim applicants have 52% lower odds of receiving a response from a private landlord or a real-estate agent, compared to majority candidates. The odds ratio is 0.52 at subgroup level ($N = 119$). Individuals belonging to the majority are twice as likely to be favored by private landlords or real-estate agents as Arabs/Muslims in OECD countries. We have also carried out meta-analyses with simple response rates and positive response rates. The corresponding forest plots are presented in the Appendix (figure 2.1 and figure 2.2). The effect size across studies noted by the *simple responses Meta-analysis* is 0.48 ($N = 11$: study level, confidence interval: [0.39: 0.59]): Arab/Muslim applicants have 52% lower odds than majority candidates of receiving a response from a private landlord or a real-estate agent. At subgroup level ($N = 40$), the odds ratio is 0.52. Finally, the effect size across studies noted by *the positive response meta-analysis* is 0.47 ($N = 12$: study level, confidence interval: [0.38: 0.58]): Arab/Muslim applicants have 53% lower odds than majority candidates of receiving a positive response from a private landlord or a real-estate agent (subgroup level: $N = 79$,

odds ratio = 0.52). Thus, majority candidates are more than twice as likely to be chosen as Arab/Muslims in OECD countries.

Figure 2: Discrimination against Arabs/Muslims in Rental Decisions



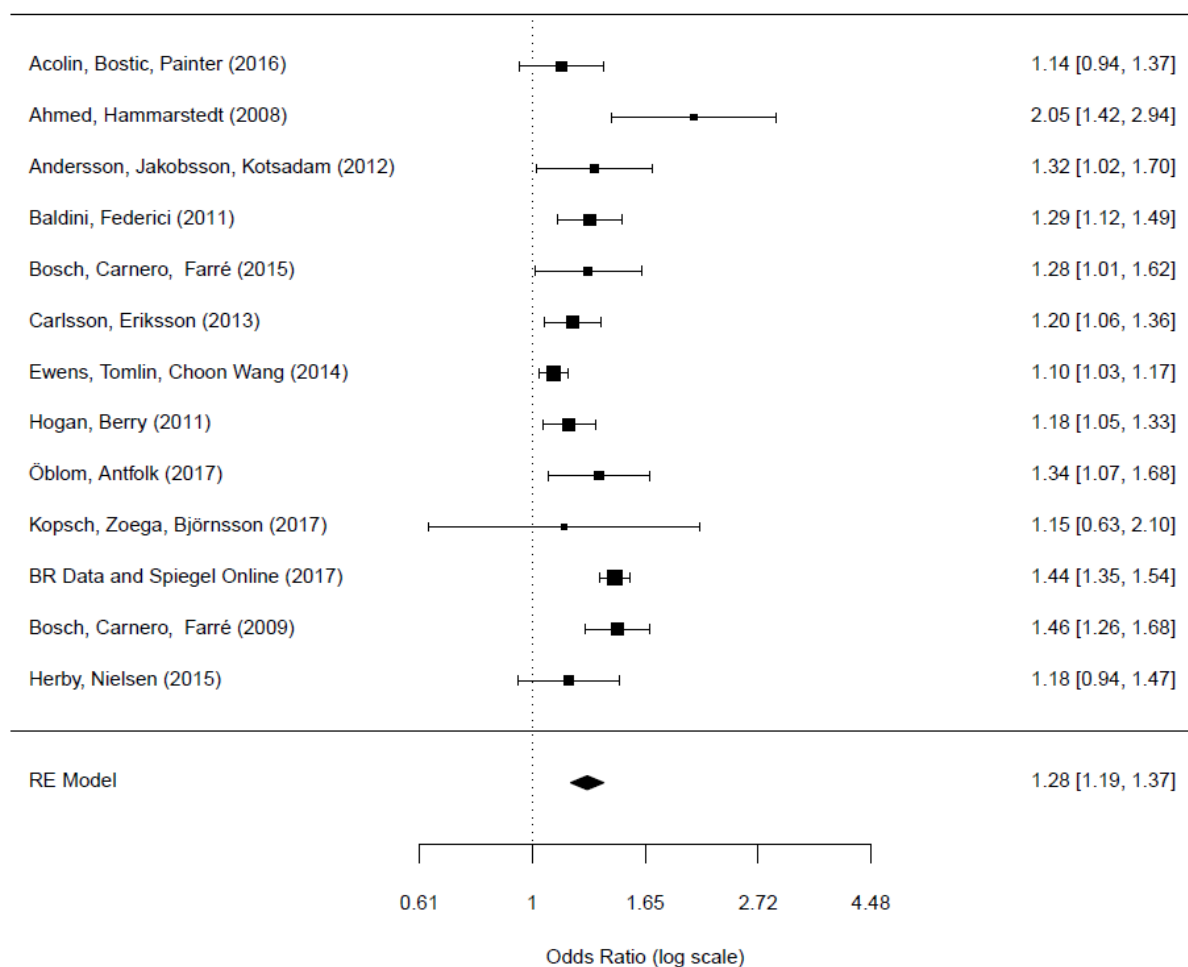
Note: This forest plot (figure 2) displays the odds ratios in log scale of each study deferring discrimination against Arabs/Muslims (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 16$: study level).

5.2 Gender Discrimination

In 13 out of 25 studies, the authors also listed the results by gender of applicants. Gender discrimination is invariably associated with ethnic discrimination in housing: to the best of our knowledge, there is no correspondence test that has been made to study

the effect of gender alone in the housing market and we are the only ones to present a quantitative analysis of gender discrimination in the housing market in OECD countries. Again we present the results (Figure 3) in terms of odds ratio, the ratio of two odds: the odds of getting a response for the *Female group* over the odds of getting a response for the *Male group*.

Figure 3: Gender Discrimination in Rental Decisions



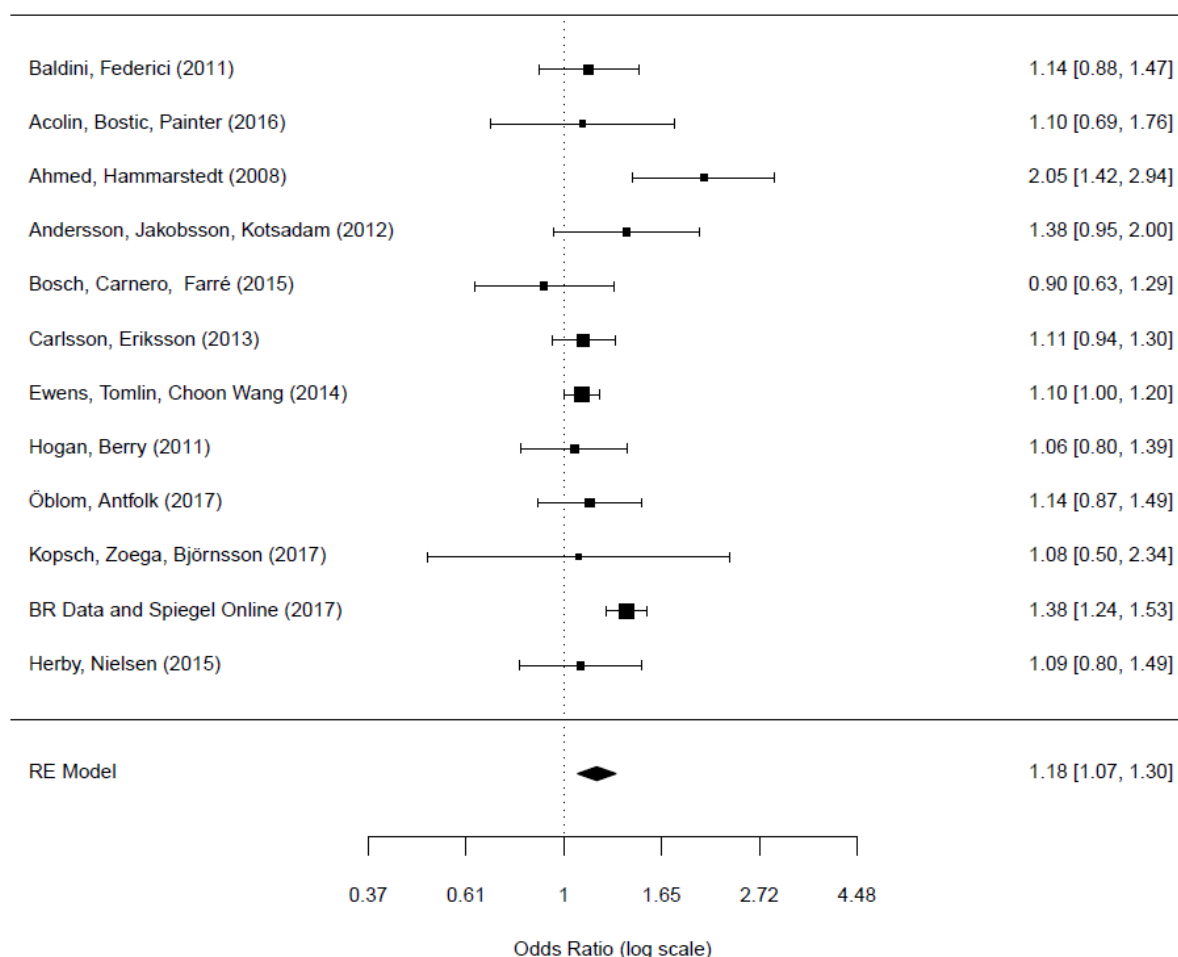
Note: This forest plot (figure 3) displays the odds ratios between male and female applicants (in log scale) of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 13$: study level).

The odds ratio is 1.28: female applicants have 28% higher odds than male candidates of receiving a response from a private landlord or a real-estate agent. The result at subgroup

level ($N = 65$) is 1.30. Therefore, some gender-based discrimination is apparent in the housing market in OECD countries, male names apparently receiving fewer responses than female names. The fact that males are discriminated against in the housing market contrasts starkly with the pattern of discrimination documented in the labor market (Altonji and Blank, 1999).

Is this gender effect different between majority and minority applicants? If we analyze the results by group of applicants, the findings are not the same; Figure 3.1 presents the results by majority applicants and Figure 3.2 shows the results by minority applicants.

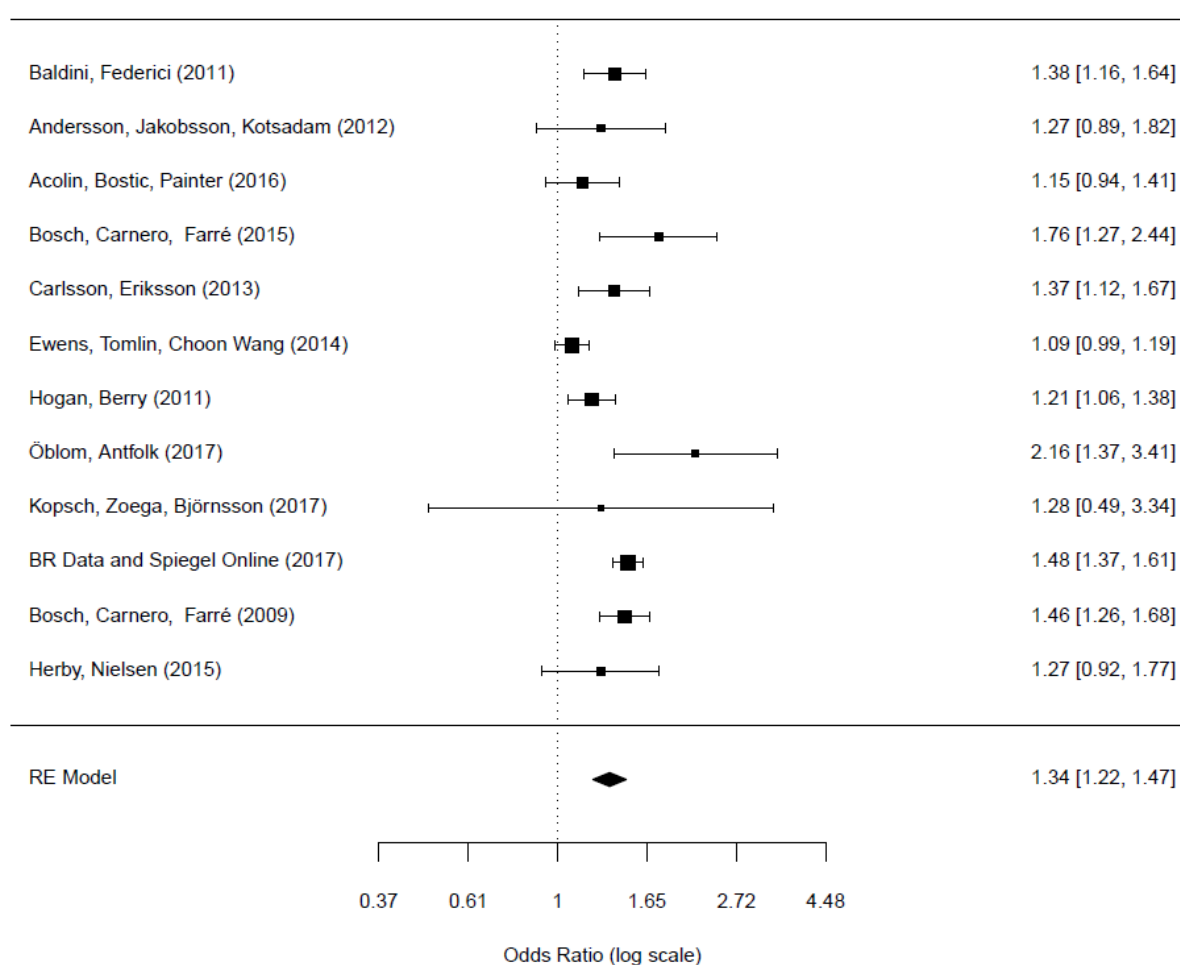
Figure 3.1: Gender Discrimination in Rental Decisions (majority applicants)



Note: This forest plot (figure 3.1) displays the odds ratios between male and female majority applicants (in log scale) of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 12$: study level).

We can easily see from this forest plot that the gender effect is smaller for individuals belonging to the majority. The effect size across studies is only 1.18: female majority applicants have 18% higher odds than male candidates of being favored by agents. At subgroup level ($N = 23$), the odds ratio is 1.22, which is around the same order of magnitude.

Figure 3.2: Gender Discrimination in Rental Decisions (minority applicants)



Note: This forest plot (figure 3.2) displays the odds ratios between male and female minority applicants (in log scale) of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 12$: study level).

For minority applicants, the gender effect is much more striking (Figure 3.2), the odds ratio is 1.34 at study level ($N = 12$), and 1.35 at subgroup level ($N = 42$), which implies that female minority applicants are more than 30% more likely than male applicants to be chosen. Moreover, if we concentrate our analysis on Arab/Muslim applicants only, this gender effect is even higher: 50% (see Figure 3.3 in Appendix): the odds ratio is 1.48 at study level ($N = 10$) and 1.47 at subgroup level ($N = 18$). Separating analysis by identity groups allows us to show that there is an interaction of ethnic and gender discrimination: gender discrimination is greater for minority-sounding names than for majority-sounding names.

Thus, female majority-sounding names are the most favored, while male minority names are the least often chosen (especially Arab/Muslim males). In the collective imagination foreign women would seem to be more trustworthy than foreign men, who are often thought suspicious, and maybe in extreme case associated with terrorists for Arabs/Muslims.

5.3 Statistical or preference-based discrimination?

In order to combat this discrimination, it is essential to know its origins. As mentioned before, this discrimination may be preference-based or statistical. In the literature, a very common method of testing the source of discrimination consists in comparing the level of discrimination between majority and minority applicants when no information is sent to the agents except the names of applicants versus the level of discrimination when detailed information is sent to agents. “Detailed information” indicates that the applications sent to real-estate agents or private landlords provided positive information about, among other things, employment, education, and marital

status of the applicant, indicating a stable situation.¹⁶ Therefore, this allows to study whether discrimination against applicants can be reduced by increasing the information given about them.

We assume that providing more correct information about the candidates should not affect the level of discrimination against minorities if discrimination is taste-based, but it should reduce the discrimination against minorities if part of the discrimination is statistical. In other words, if the positive effect of the information on the response rates is stronger for individuals belonging to the minority than to majority, then some of the discrimination is statistical, otherwise discrimination is preference-based.

In 10 studies, the authors tested for the occurrence of statistical discrimination by this method. We present the first quantitative analysis of statistical discrimination in the housing market in OECD countries through meta-analysis. Once again, we present the results in term of odds ratio, the ratio of two odds: the odds of getting a response for the *Detailed information* group over the odds of getting a response for the *No information* group. The results are reported in the Appendix in Figure 4.1 for majority applicants and Figure 4.2 for minority applicants.

Our findings suggest that providing more correct information in the applications increases the probability of being chosen for both minority and majority applicants by almost 40% (respectively 37 and 39%).¹⁷ Therefore, the overall effect of information is of the same order of magnitude for both ethnic groups and it seems that more correct information

¹⁶ Consider examples of e-mails sent to test statistical discrimination (Bosch et al., 2010): “No-information” applicant: “Hello, I am interested in renting this apartment. I would be very grateful if you contacted me. Thank you. NAME” compared to “Detailed information”: “Hello, I am interested in this flat. I work for an important commercial bank. I have recently moved to (city) and I am looking for a flat where to live for at least a couple of years. I would be happy to provide a financial guarantee. Please contact me if interested. Many thanks. NAME”. Other examples are provided in Appendix.

¹⁷ Recall that the information provided by the two ethnic groups is exactly the same because it comes from the same studies.

would not tend to narrow the gap between majority and minority applicants. In the majority of cases, providing more information slightly reduces discrimination, but in two cases, it greatly increases discrimination. Thus we cannot provide evidence, by the meta-analysis method, that significant statistical discrimination is at work in OECD countries.

The meta-analysis allowed us to test the overall effect of information, but we need to test this effect *ceteris paribus* in various contexts and on different types of agents to determine whether or not there is indeed no statistical discrimination and only preference-based discrimination. We will address these issues in the next part, using a meta-regression.

6. Meta-regression analysis

In this section, we present a multivariate regression analysis in order to examine the determinants of the level of discrimination (in log odds ratio) with three econometric models: fixed-effects (F-E), unrestricted weighted least squares (WLS), and random effects (R-E). Meta-analysis focuses on the value of the variable of interest while meta-regression focuses on the variables that influence this variable.

We now present all the explanatory variables that we have chosen to use for our regression and explain the way in which we code variables.

6.1 Explanatory variables

The coding of variables is a crucial issue. It allows different characters identified in the literature to be transformed into testable elements. However, this procedure is not

without its problems, the main one being the loss of information. This occurs when the literature only reported data on key determinants (e.g. data sources, study samples, econometric techniques). However, as we have been able in our case to report response rates for almost every type of application,¹⁸ we did not need to transform many different characters into testable elements.

Indeed, because of the control that correspondence testing provides, most elements were already well coded in the primary literature and were directly testable without the need for transformation. We only made one real transformation, and that concerned the first variable, *Company*.

We separated the variables into three broad categories: type of renter, characteristics of the e-mails sent, and characteristics of the applicant.

Company is a dummy variable which takes the value “1” when the applications were sent to real-estate agents while it takes the value “0” if applications were sent to private landlords. However, only eight studies reported response rates by type of agent. Nonetheless, most studies reported the proportion of each type of agent in the experiment. Therefore, we chose, when the separate response rates were not reported, to code these variables as follows: when the proportion of real-estate agents was greater than the proportion of private landlords in a study, we coded this latter as “*Company*”, and conversely, when the proportion of real-estate agents was lower than the proportion of private landlords, we coded this study as “*Private landlords*”.¹⁹

¹⁸ Although we have reported the results by age and geographical environment whenever possible, we do not have enough data to include these in the meta-analysis.

¹⁹ The results are robust to other codifications of this variable, such as coding half of the studies (those with the lowest proportion of real-estate agents) as “*Private landlord*” and half of the studies (those with the largest proportion of real-estate agents) as “*Company*” or only coding studies with more than 70% of real-estate agents as “*Company*” and only coding studies with less than 30% of real-estate agents as “*Private landlords*” (involving the deletion of some data).

As said before, there are two ways of conducting a correspondence test: in 11 studies, the authors used the *single inquiries* procedure while the *matched* procedure was used in 14 studies. We chose the *single inquiries* procedure as a reference.

We clustered the countries of the database by continents, using a dummy variable to distinguish between applications sent in North America or in Europe²⁰ (reference).

Meta-analysis allows us to determine the overall effect of information on applicants, but meta-regression allows us to test this effect *ceteris paribus*. Once again, *Detailed information* indicates that the applications sent to real-estate agents or private landlords provided positive information about employment, education, marital status of the applicant, implying a stable situation. Our reference is *No information*, which indicates that no information was sent to the agents except the name of applicants. So, this allows us to study whether discrimination against applicants can be reduced by increasing the information given about them.

Female minority is a dummy variable which takes the value “1” if the minority applicant is a woman and “0” when the minority applicant is a man (reference).

Female majority is a dummy variable which takes the value “1” if the majority applicant is a woman and “0” when the majority applicant is a man (reference).

The last variable is ethnicity and we took as a reference the Arab/Muslim group.

Our baseline model for the MRA is specified as follows:

$$y_j = \beta_0 + \beta_1 x_{1j} + \beta_2 x_{2j} + \dots + \varepsilon_j$$

where y_j is the odds ratio (in log) on the correspondence test (a subgroup of a study) j and β_0 is the intercept. The variables x_i specify different characteristics of the correspondence test, such as detailed information provided in the applications, type of

²⁰ For convenience, Israel is coded as “Europe”.

agents, gender of applicants, etc. ε_j in this baseline model specifies the between-subgroup variation.

Several methods can be used to estimate this model:

A fixed effect (FE) estimator assumes that all subgroups share the same real effect size. Because of possible unobserved protocol differences and unobserved differences in the population tested in these correspondence tests, we must be very careful when interpreting the results. This type of estimator allows for within-subgroup variability but ignores between-subgroup variation. As a result, parameter estimates are biased if between-subgroup variation cannot be ignored.

On the other hand, the random effects (RE) estimator allows the real variables of interest to vary from one subgroup to the other but this method may be sensitive to possible publication bias.

Finally, Stanley and Doucouliagos (2015, 2017) propose estimating the baseline model using an unrestricted least squares (WLS) model, which consists in estimating this equation using weighted least squares with $1/\text{se}^2(\mathbf{y}_j)$ (where se is the standard error of log odds ratio) as the weights. When there is publication selection bias, the WLS-MRA estimates invariably have a smaller bias than random effects estimates (Stanley and Doucouliagos, 2015, 2017).

For the sake of thoroughness, we also follow Stanley and Doucouliagos (2012), by clustering standard errors at the study level in all specifications, to make them robust to intra-study dependence. Clustering does not affect the estimated coefficients, only their statistical significance in a more conservative way.

Moreover, we pay great attention to multicollinearity problems in our regressions because a meta-regression analysis is more prone to multicollinearity than classical econometrics. Indeed, most explanatory variables are dummies. In our case, all explanatory variables

present a variance inflation factor (VIF) less than 3. Small VIF values indicate low correlation among variables. A limit value of 10 (or sometimes 5) is a rule of thumb commonly used in the literature (Hair et al., 1998).

We have only taken into account in this meta-regression the subgroups where all information was provided (detail of information in the e-mail is specified, gender is specified, type of landlord is specified, etc.). As *positive response* is a subgroup of *simple response*, we take into account only *positive response* in the meta-regression to avoid using the same data multiple times and also because *positive response* is a better estimator of discrimination.

Unfortunately, we do not have enough data to calculate the correct effect of some ethnic variables. Descriptive statistics of variables used in the MRA are provided in Table 1.

Table 1: Descriptive statistics of variables used in the MRA (odds ratios minority against majority)

<i>Variable</i>	<i>Dummy</i>	<i>Frequency=1</i>	<i>Frequency=0</i>
<i>Matched</i>	<i>1 if matched procedure</i>	<i>51</i>	<i>56</i>
<i>Company</i>	<i>1 if Company</i>	<i>30</i>	<i>77</i>
<i>Detailed information</i>	<i>1 if Detailed information</i>	<i>42</i>	<i>65</i>
<i>Female Minority</i>	<i>1 if Female</i>	<i>46</i>	<i>71</i>
<i>Female Majority</i>	<i>1 if Female</i>	<i>47</i>	<i>70</i>
<i>North America</i>	<i>1 if North America</i>	<i>8</i>	<i>99</i>
<i>African</i>	<i>1 if African</i>	<i>10</i>	<i>49</i>
<i>East European</i>	<i>1 if East European</i>	<i>20</i>	<i>49</i>
<i>Hispanic</i>	<i>1 if Hispanic</i>	<i>9</i>	<i>49</i>
<i>Turkish</i>	<i>1 if Turkish</i>	<i>8</i>	<i>49</i>

6.2 Results

The results are reported in Table 2 in terms of log odds ratio. The odds ratio is the ratio of two odds: the odds of getting a response for the *Minority group* over the odds of getting a response for the *Majority group*. We reported the results of the three regression models by blocks, starting with (1) the characteristics of the protocol and agents, then (2) adding characteristics of applicants (type of information and gender), and finally (3) adding the ethnic background.

In addition, we test three interactions with the variable Company (see Table 3 in Appendix) to investigate (1) whether private landlords respond differently to added information than do real-estate companies and (2) whether gender effects differ depending on the type of landlord.

Table 2. Results of the meta-regression (all variables)

	<i>RE Eqn. (1)</i>	<i>WLS Eqn. (1)</i>	<i>FE Eqn. (1)</i>	<i>RE Eqn. (2)</i>	<i>WLS Eqn. (2)</i>	<i>FE Eqn. (2)</i>	<i>RE Eqn. (3)</i>	<i>WLS Eqn. (3)</i>	<i>FE Eqn. (3)</i>
<i>Intercept</i>	-0.782*** (0.05)	-0.741*** (0.07)	-0.741*** (0.03)	-0.891*** (0.07)	-0.807*** (0.08)	-0.807*** (0.03)	-0.869*** (0.07)	-0.790*** (0.10)	-0.790*** (0.04)
<i>Company</i> ^(a)	0.456*** (0.11)	0.380*** (0.07)	0.380*** (0.04)	0.349*** (0.11)	0.317*** (0.09)	0.317*** (0.05)	0.351*** (0.10)	0.330*** (0.09)	0.330*** (0.06)
<i>Matched</i> ^(b)	0.003 (0.10)	0.059 (0.09)	0.059 (0.05)	0.1119 (0.10)	0.113 (0.09)	0.113** (0.05)	0.088 (0.09)	0.064 (0.10)	0.064 (0.06)
<i>North America</i> ^(c)	-0.089 (0.14)	0.007 (0.11)	0.007 (0.04)	0.043 (0.13)	0.081 (0.13)	0.081* (0.05)	-0.232* (0.14)	-0.159 (0.18)	-0.159** (0.07)
<i>Detailed information</i> ^(d)				0.107 (0.08)	0.043 (0.07)	0.043 (0.03)	0.107 (0.07)	0.026 (0.11)	0.026 (0.04)
<i>Female Minority</i> ^(e)				0.341*** (0.07)	0.351*** (0.04)	0.351*** (0.03)	0.297*** (0.06)	0.331*** (0.05)	0.331*** (0.03)
<i>Female Majority</i> ^(f)				-0.200*** (0.07)	-0.274*** (0.05)	-0.274*** (0.03)	-0.253*** (0.06)	-0.296*** (0.04)	-0.296*** (0.03)
<i>African</i> ^(g)							-0.145 (0.11)	-0.058 (0.07)	-0.058 (0.06)
<i>East European</i> ^(g)							0.216*** (0.08)	0.149*** (0.05)	0.149*** (0.04)
<i>Hispanic</i> ^(g)							0.489*** (0.14)	0.398** (0.15)	0.398*** (0.08)
<i>Turkish</i> ^(g)							-0.162 (0.10)	-0.078 (0.09)	-0.078* (0.05)

Notes. Robust standard errors in parentheses. ***p < 0.01, **p < 0.05, and *p < 0.1. Reference: ^(a) Private Landlords, ^(b) Single inquiries, ^(c) Europe, ^(d) No information, ^(e) Male Minority, ^(f) Male Majority, ^(g) Arab/Muslim.

Positive values indicate a lower level of discrimination (less differential treatment) between ethnic majority and minority applicants and negative values denote a higher level of discrimination between ethnic majority and minority applicants.

For instance, the positive value for “*Female Minority*” suggests that the difference in treatment between majority and minority candidates is lower when the minority candidate is a woman rather than a man, *ceteris paribus* (i.e. whatever the gender of the majority candidate). In other words, as the numerator of the odds ratio is higher for women than for men, this result implies that minority women receive more responses than minority men. The meta-regression therefore reveals a preferential treatment of minority women with respect to minority men.

On the other hand, the negative value for “*Female Majority*” suggests that the differential treatment between majority and minority candidates is higher when the majority candidate is a woman rather than a man, *ceteris paribus*. So, majority women are favored compared to majority men. Indeed, the negative sign does not mean that majority women are disadvantaged compared to men. Since the odds of getting a response for the majority group is in the denominator of the odds ratio, then, as majority women receive more responses than men, the differential treatment with minority applicants increases, hence the negative sign.

Therefore, consistently with meta-analysis results, a significant gender effect within groups exists: female applicants receive more responses from landlords than male applicants do, whether they are majority or minority.²¹

Moreover, it seems that the coefficient for *Female Minority* is higher than the absolute value of the coefficient for *Female Majority*, suggesting that the gender discrimination is higher for minorities than for majorities. Interaction terms (Table 3 in the Appendix)

²¹ Note that we do not have enough data to test the difference in gender-based discrimination for each minority group.

show that this difference is really significant only when the applications are sent to private landlords.²²

In addition to these results, we draw from this analysis four new interesting results that are robust to the three estimation methods used.

Finally, it seems that real-estate agents discriminate significantly less against minority applicants than private landlords do. Many factors could explain this lower discrimination by real-estate agents: agents could be subject to rules (laws) or at least to prevention against discrimination, while private landlords are not. Their seniority in the trade could make them more confident about minority applicants (while private landlords might be more afraid of the unknown). In addition, it is riskier for professionals not to comply with legislation. Finally, the fact that agents often handle a larger portfolio of clients allows them to spread their risk, thereby reducing statistical discrimination.

By controlling the effect of the other variables, there still does not seem to be any significant effect of information on the level of discrimination. However, interaction terms (Table 3 in the Appendix) show that this is primarily due to the difference in response to added information between private landlords and real-estate companies. Indeed, the coefficient for *Detailed information* (in Table 3) is positive, which indicates that discrimination decreases when detailed information is provided to private landlords. Conversely, the coefficient for *Detailed information* \times *Company* is negative, which indicates that the effect of information is significantly lower for real-estate agents than for private landlords. Moreover, it seems that there is no positive effect of information over the level of discrimination when applications are sent to real-estate agents (the total

²² The difference in significance between *Female Minority* and the absolute value of *Female Majority* is then significant at the 5% level for the three estimation methods used. Conversely, for real-estate agents, the difference in significance between (*Female Minority* + *Female Minority* \times *Company*) and the absolute value of (*Female Majority* + *Female Majority* \times *Company*) is not significant at the 5% level.

marginal effect is *Detailed information + Detailed information \times Company*), which indicates that real-estate agents do not display significant statistical discrimination.²³ Thus, it seems that private landlords show significant statistical discrimination while real-estate agents do not. This result is very intuitive; as mentioned before, we suppose that real-estate agents have more correct information about individuals belonging to minorities and can more easily spread their risk than private landlords (not to mention the fact that private landlords are dealing with their personal assets but not so do real-estate agents). Thus, private landlords need to be “reassured” more than real-estate agents.

Thus, real-estate agents discriminate significantly less than private landlords, and this is due (at least in part) to the fact that they differentiate less, in a statistic manner, individuals by their ethnic groups. Indeed, providing more correct information in the applications significantly narrows the gap in discrimination between private landlords and real-estate agents.²⁴

Except in FE Eqn. (2), it seems that the way of conducting correspondence tests does not have a significant impact on the results. Indeed, the discrimination reported with the *single inquiries procedure* is similar to the discrimination reported with the *matched procedure*.

Except in FE Eqn. (3), discrimination does not seem to be significantly higher in North America than in Europe.

²³ When the “Company” variable interacts with three other variables, the effect of information sometimes even seems to increase the level of discrimination of real-estate agents (see Table 3 in the Appendix), but this effect is weak and not robust to the three estimation methods.

²⁴ However, the gap between real-estate agents and private landlords remains significant at the 1% level for all three models. In the presence of detailed information, private landlords continue to discriminate more than real-estate agents.

Finally, and as we easily deduced from the literature review, Hispanic and Eastern European applicants face significantly less discrimination than Arab/Muslim, Turkish, and African applicants in the rental housing market in OECD countries.²⁵

7. Conclusion

In this meta-analysis, we provide evidence of the occurrence of both ethnic and gender discrimination in the OECD rental housing market. At the initial stage of the rental process, we find that majority candidates are almost twice as likely as applicants belonging to the minority to be chosen by real-estate agents or private landlords. Moreover, individuals belonging to the majority are more than twice as likely to be favored as Arab/Muslim applicants. Female applicants are almost 30% more likely than male applicants to be chosen. However, this result is different between the group of applicants: women belonging to an ethnic minority are more than 30% more likely than men belonging to the same minority to be chosen by an agent. This result is even higher when we compare Arab/Muslim women with Arab/Muslim men: women are 50% more likely than men to be chosen. Finally, a woman belonging to the majority has “only” 18% more chance than a man belonging to the majority of being chosen. Therefore, there is interaction between ethnic and gender discrimination: gender discrimination is greater for minority-sounding names than for majority-sounding names. Female majority-sounding names are the most favored in the OECD rental housing market, while male minority names are the most disadvantaged. Moreover, it seems that real-estate agents

²⁵ The difference in significance between Hispanic and Eastern European applicants is not significant at the 5% level. So Eastern Europeans do not face more discrimination than Hispanics. There are two main levels of discrimination, the Hispanic/Eastern European level and the Arab/African/Turkish level.

discriminate significantly less against minority applicants than private landlords do. We were able to determine that this difference was at least in part because private landlords show significant statistical discrimination while real-estate agents do not. Thus, discrimination in this market is not only a matter of preferences. It seems that private landlords have a lack of information about ethnic minorities and discrimination could be significantly reduced by the provision of more correct information about the economic and social conditions of discriminated ethnic groups. These conclusions are robust with random effects (R-E), fixed-effects (F-E), and unrestricted weighted least squares (WLS) models. We hope that our results provide important information for the future development of non-discrimination and equal housing opportunities in the rental housing market in OECD countries.

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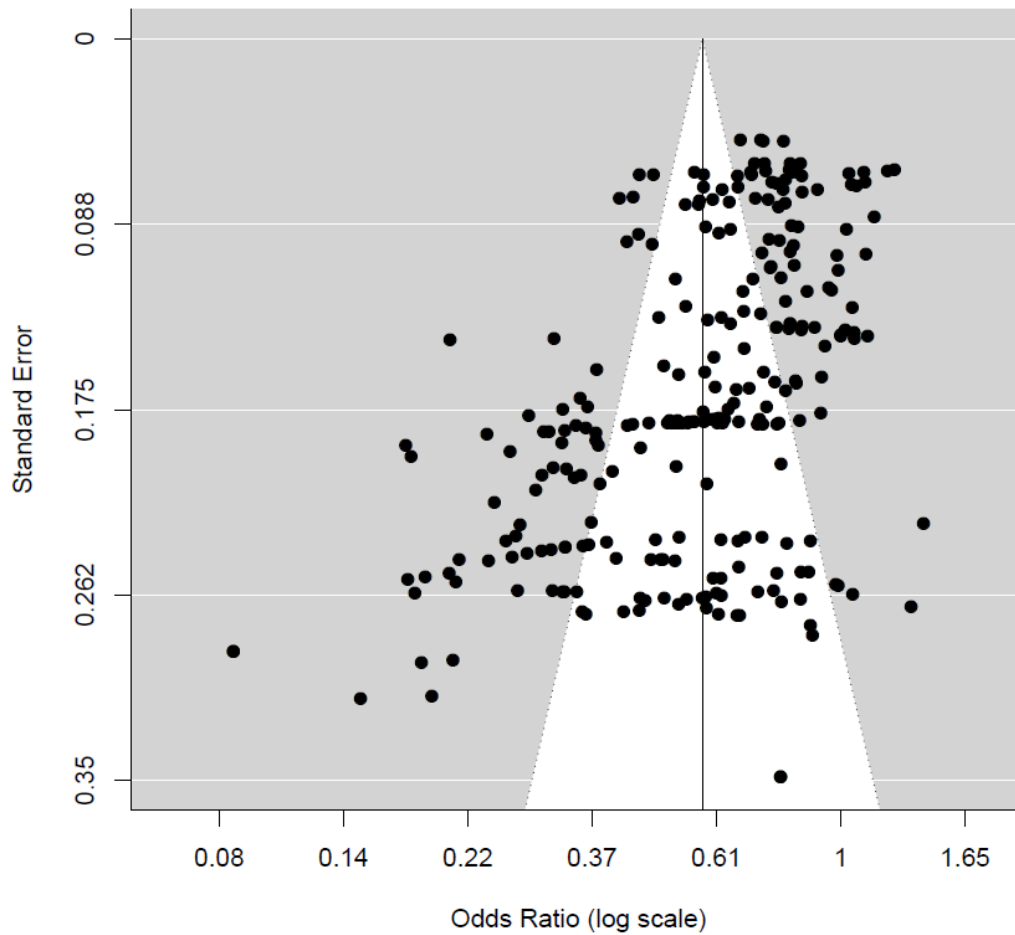
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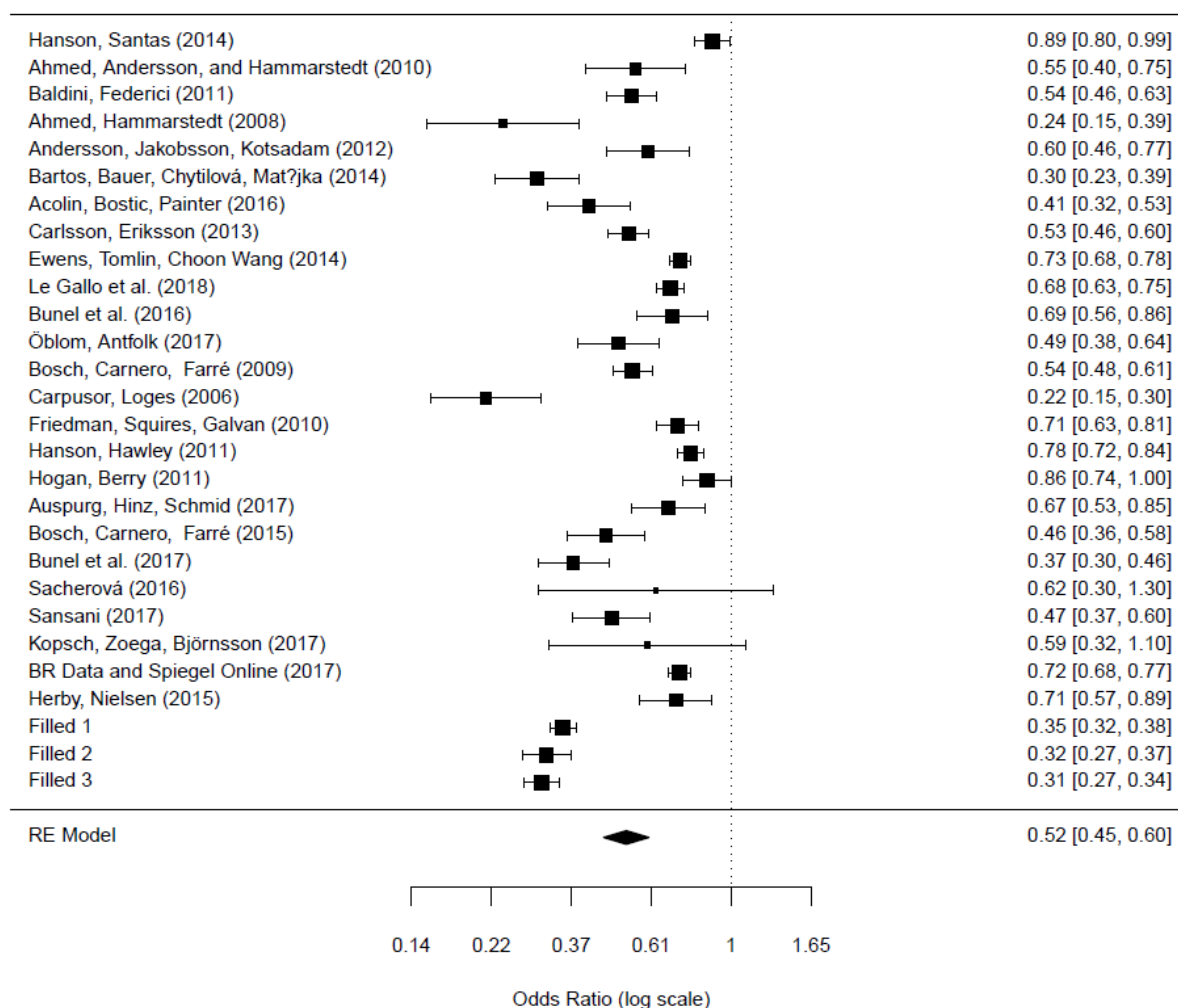
Appendix: Additional figures

Figure 1.3: Funnel plot



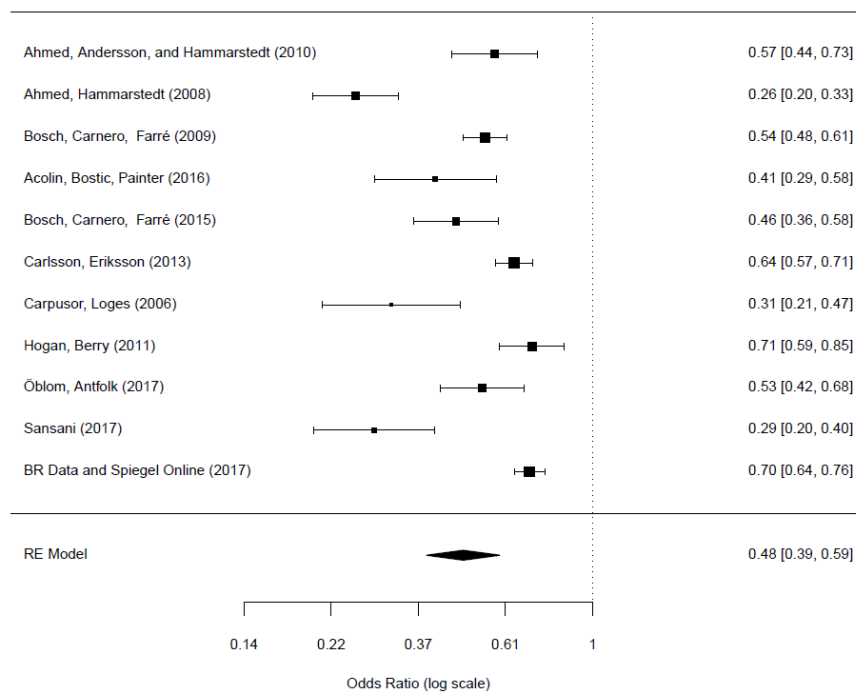
Note: Each dot represents an odds ratio estimated from a test against the standard error of the odds ratio (in log scale), with a reversed scale that places the larger, most powerful studies toward the top.

Figure 1.4: Ethnic and Racial Discrimination in Rental Decisions (correction bias)



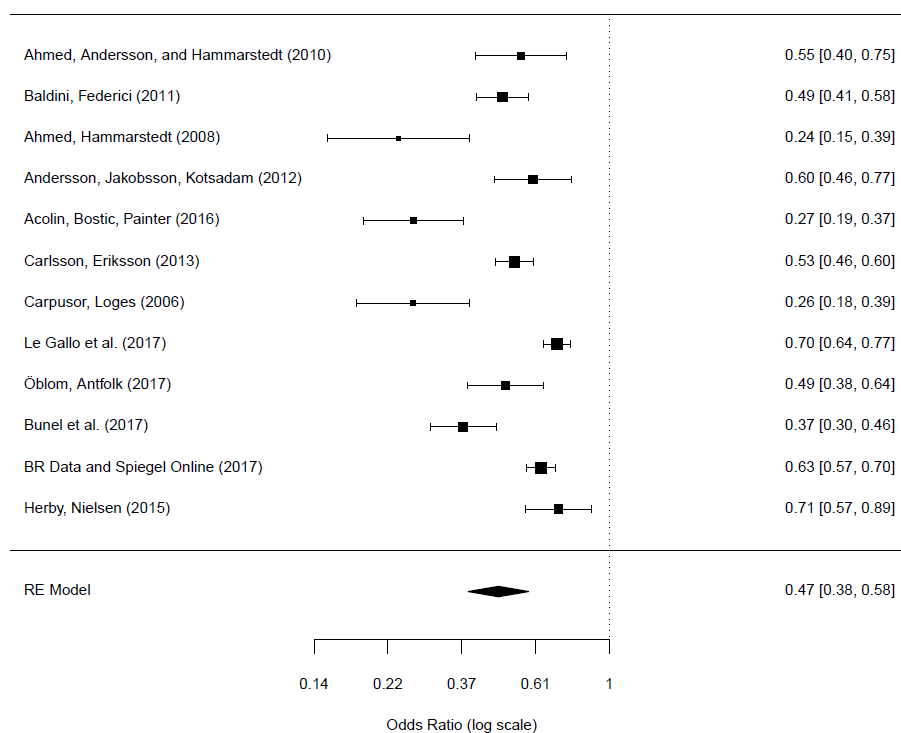
Note: This forest plot (figure 1.3) displays the odds ratios in log scale of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 28$: study level). Three fictitious studies are generated by the trim-fill method in order to correct publication bias.

Figure 2.1: Discrimination against Arab/Muslim in Rental Decisions (Simple response)



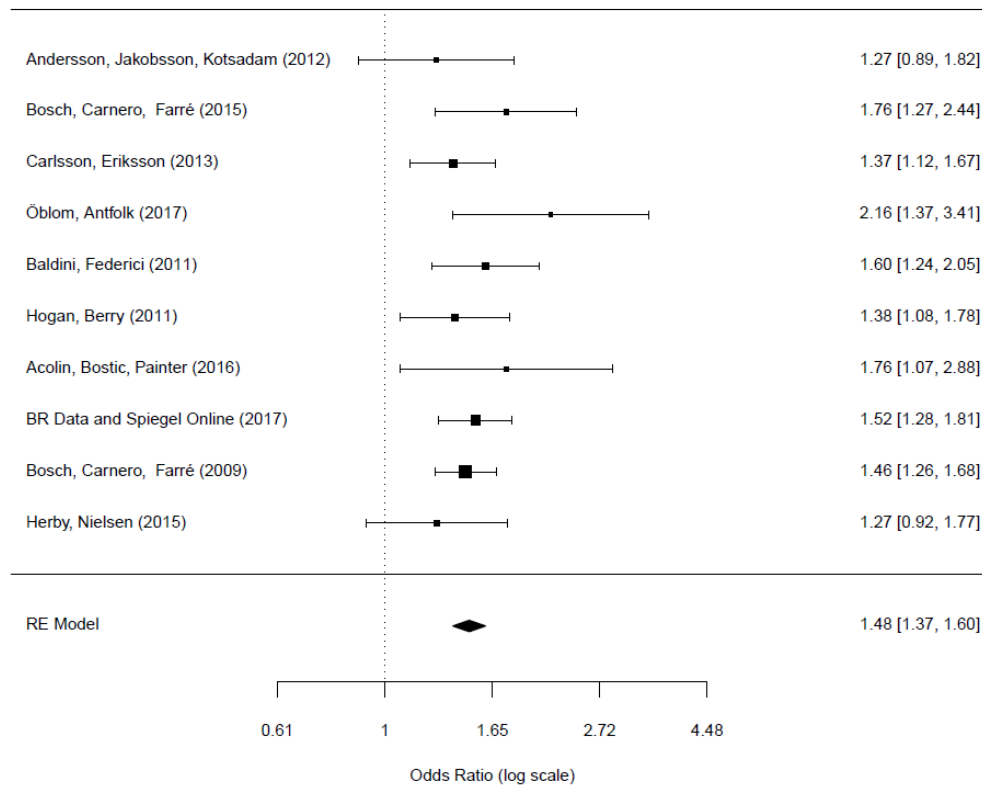
Note: This forest plot (figure 3.1) displays the odds ratios in log scale of each study deferring discrimination against Arab-Muslims by simple response (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 11$: study level).

Figure 2.2: Discrimination against Arab/Muslim in Rental Decisions (Positive response)



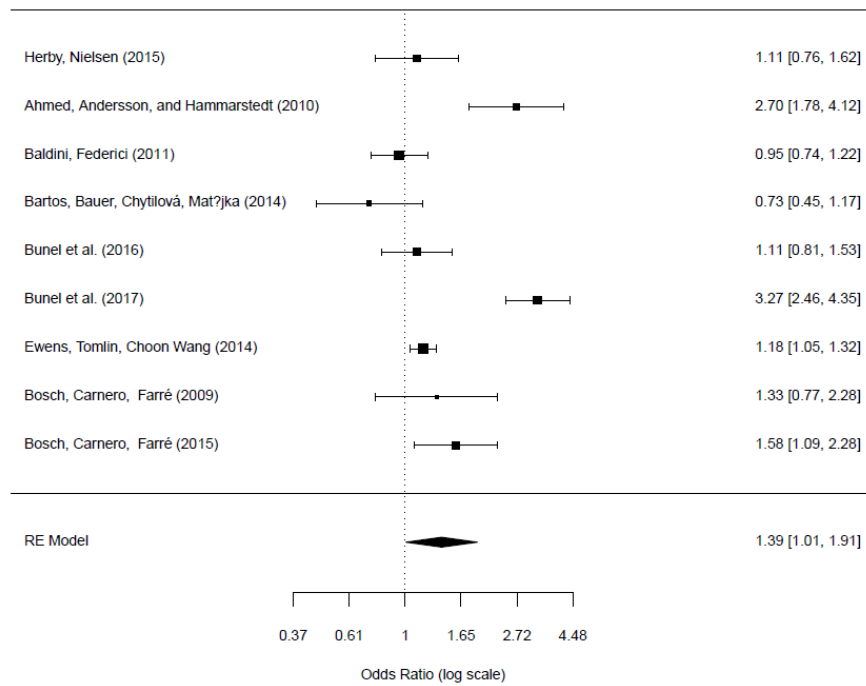
Note: This forest plot (figure 3.2) displays the odds ratios in log scale of each study deferring discrimination against Arab-Muslims by positive response (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 12$: study level).

Figure 3.3: Gender Discrimination in Rental Decisions (Arab/Muslim applicants)



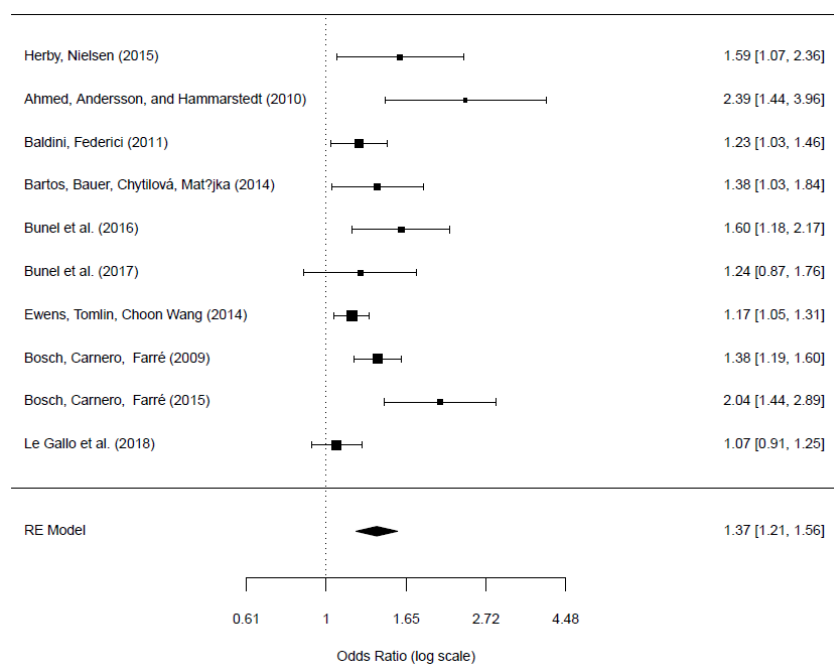
Note: This forest plot (figure 4.3) displays the odds ratios between male and female Arab/Muslim applicants (in log scale) of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 10$: study level).

Figure 4.1 Effect of providing correct information on majority candidates



Note: This forest plot (figure 4.3) displays the odds ratios between majority applicants with “detailed information” and majority applicants with “no information” (in log scale) of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 9$: study level).

Figure 4.2: Effect of providing correct information on minority candidates



Note: This forest plot (figure 4.3) displays the odds ratios between minority applicants with “detailed information” and minority applicants with “no information” (in log scale) of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 10$: study level).

Structure of Emails sent to test statistical discrimination

«**No information applicant** »: 1) an introductory hello statement. 2) a statement of interest in the apartment. 3) a closing that thanks the landlord and is always followed with the applicant’s name. (and sometimes an inquiry statement regarding the availability of the unit).

Examples:

“Hello, I am interested in renting this apartment. I would be very grateful if you contacted me. Thank you. NAME” (Bosch et al., 2010)

Hello, my name is NAME and I am writing in response to your ad. If it is still available, I would be interested in fixing a meeting to see the apartment. Hoping in an answer from you, Thanks for your time, NAME (Baldini and Federici, 2011)

Compared to

«**Detailed information**»: 1) an introductory hello statement. 2) a statement of interest in the apartment. 3) detailed positive information about, among other things, employment, education, and marital status of the applicant, involving a certain stable situation 4) a closing that thanks the landlord and is always followed with the applicant’s name. (and sometimes an inquiry statement regarding the availability of the unit).

Examples:

“Hello, I am interested in this flat. I work for an important commercial bank. I have recently moved to (city) and I am looking for a flat where to live for at least a couple of years. I would be happy to provide a financial guarantee. Please contact me if interested. Many thanks. NAME” (Bosch et al., 2010)

“Good Morning, I am interested in renting the apartment in the ad. My name is NAME, I am married without children, and have an unlimited job contract as a clerk. Good references are available. Is it possible to make a date to visit the apartment? Looking forward to hearing from you, Sincerely, NAME (Baldini and Federici, 2011)

Table 3. Results of the meta-regression involving interaction effects with the “Company” variable

		<i>RE Eqn. (1)</i>	<i>WLS Eqn. (1)</i>	<i>FE Eqn. (1)</i>	<i>RE Eqn. (2)</i>	<i>WLS Eqn. (2)</i>	<i>FE Eqn. (2)</i>	<i>RE Eqn. (3)</i>	<i>WLS Eqn. (3)</i>	<i>FE Eqn. (3)</i>
<i>Variables</i>	<i>Intercept</i>	<i>-0.855***</i> (0.06)	<i>-0.813***</i> (0.07)	<i>-0.813***</i> (0.03)	<i>-0.850***</i> (0.06)	<i>-0.798***</i> (0.08)	<i>-0.798***</i> (0.03)	<i>-0.917***</i> (0.06)	<i>-0.859***</i> (0.10)	<i>-0.859***</i> (0.03)
	<i>Company^(a)</i>	<i>0.590***</i> (0.11)	<i>0.552***</i> (0.09)	<i>0.552***</i> (0.04)	<i>0.536***</i> (0.09)	<i>0.489***</i> (0.08)	<i>0.489***</i> (0.04)	<i>0.654***</i> (0.10)	<i>0.598***</i> (0.12)	<i>0.598***</i> (0.04)
	<i>Detailed information^(b)</i>	<i>0.229**</i> (0.09)	<i>0.199***</i> (0.07)	<i>0.199***</i> (0.04)				<i>0.215**</i> (0.08)	<i>0.175**</i> (0.08)	<i>0.175***</i> (0.04)
	<i>Company × Detailed information</i>	<i>-0.315**</i> (0.15)	<i>-0.256***</i> (0.09)	<i>-0.256***</i> (0.05)				<i>-0.367**</i> (0.16)	<i>-0.283***</i> (0.10)	<i>-0.283***</i> (0.06)
	<i>Female Minority^(c)</i>				<i>0.342***</i> (0.09)	<i>0.326***</i> (0.08)	<i>0.326***</i> (0.04)	<i>0.329***</i> (0.09)	<i>0.304***</i> (0.08)	<i>0.304***</i> (0.04)
	<i>Female Majority^(d)</i>				<i>-0.122</i> (0.08)	<i>-0.132**</i> (0.05)	<i>-0.132***</i> (0.04)	<i>-0.131</i> (0.08)	<i>-0.150**</i> (0.06)	<i>-0.150***</i> (0.04)
	<i>Company × Female Minority</i>				<i>0.018</i> (0.15)	<i>0.028</i> (0.08)	<i>0.028</i> (0.05)	<i>0.098</i> (0.16)	<i>0.091</i> (0.08)	<i>0.091</i> (0.06)
	<i>Company × Female Majority</i>				<i>-0.232</i> (0.15)	<i>-0.219***</i> (0.06)	<i>-0.219***</i> (0.05)	<i>-0.156</i> (0.16)	<i>-0.162***</i> (0.06)	<i>-0.162***</i> (0.06)
<i>Marginal effects</i>	<i>Effect of Information on Company</i>	<i>-0.086</i> (0.12)	<i>-0.056</i> (0.04)	<i>-0.056</i> (0.03)				<i>-0.152</i> (0.14)	<i>-0.107**</i> (0.05)	<i>-0.107**</i> (0.04)
	<i>Gender minority effect for Company</i>				<i>0.360***</i> (0.13)	<i>0.355***</i> (0.02)	<i>0.355***</i> (0.03)	<i>0.427***</i> (0.13)	<i>0.396***</i> (0.11)	<i>0.396***</i> (0.04)
	<i>Gender majority effect for Company</i>				<i>-0.354***</i> (0.13)	<i>-0.351***</i> (0.02)	<i>-0.351***</i> (0.03)	<i>-0.287**</i> (0.13)	<i>-0.313***</i> (0.11)	<i>-0.313***</i> (0.04)

Notes. Robust standard errors in parentheses. ***p < 0.01, **p < 0.05, and *p < 0.1. Reference: ^(a) Private Landlords, ^(b) No Information, ^(c) Male Minority, ^(d) Male Majority. Marginal effects refer to the total effect of the variable on Company (e.g. the marginal effect of information on Company is equal to: Detailed information + Company × Detailed information)

Chapter 2

Discrimination against Gays and Lesbians in Hiring Decisions: A Meta-Analysis

Note: This chapter is associated to an original paper, published in International Journal of Manpower (Flage, 2019).

1. Introduction

After being a subject of marginal attention as if it was taboo, research into the position of lesbian and gay applicants in the labor market has recently gained momentum. These last years, a significant literature has investigated whether homosexual people face differential treatment in hiring decisions in OECD countries. These studies have often demonstrated the presence of sexual orientation discrimination against openly homosexual job-seekers. However, as said in the General Introduction, all these studies have their own protocols, their own ways of reporting the results, and different sample sizes. It is therefore essential to carry out a meta-analysis to clarify the true level of discrimination against homosexual applicants in OECD countries and to identify the explanatory variables of that discrimination.²⁶ The meta-analysis method has been widely used to assess discrimination in areas such as the rental housing market (Auspurg et al., 2019) and ethnic discrimination in hiring (Zschirnt and Ruedin, 2016). However, as far as we know, no study has ever performed meta-analyses to compare and examine the findings of studies on hiring discrimination towards homosexual individuals. In this chapter, we therefore construct a database of correspondence tests from 18 separate studies containing more than 70 estimates of effect sizes conducted in OECD countries in order to detect discrimination against homosexual applicants in hiring decisions, representing a total of more than 50,000 resumes sent to employers.

Our contribution to the field is threefold: first, we present an overview of 19 studies that have tested for discrimination against homosexual job-seekers in the labor market by the

²⁶ Note that there is also an emerging literature on the position of transgender people in the labor market (e.g. Van Borm and Baert, 2018; Winter et al., 2018; Drydakis, 2017; Leppel, 2016; Bardales, 2013) but to date, there is not enough data to make a proper meta-analysis of gender-identity based discrimination. Therefore, we opted to concentrate our analysis on sexual orientation.

correspondence testing method, thereby aggregating all the literature we are aware of on the subject.²⁷ Next, the extent of discrimination on the grounds of sexual orientation is analyzed quantitatively for OECD countries using meta-analyses. As well as setting out the overall findings, we focus on subgroups of specific correspondence tests in order to highlight the differences across gender, type of jobs, procedure, continent, and type of information provided in applications.

At the initial stage of the hiring process, we find that openly homosexual candidates have almost 40% lower odds of receiving a positive response from an employer, for equal information provided in the applications, compared to straight candidates. Thus, it seems that the level of discrimination faced by homosexual applicants is close to the level of discrimination faced by ethnic minority applicants (Zschirnt and Ruedin, 2016) in hiring decisions. However, this result needs to be nuanced: indeed, discrimination is significantly higher in job offers for low-skilled jobs than in high-skilled jobs. Moreover, there is a significant difference in discrimination between gays and lesbians when they apply for low-skilled jobs: straight males are twice as likely to be chosen by employers as gays while lesbians have “only” 31% lower odds than straight women of receiving a positive response from an employer. We were able to determine that this discrimination is not only a matter of preferences (e.g. xenophobia, homophobia) but is also due to a lack of correct information about homosexual applicants. Indeed, providing more correct information in the content of the applications significantly reduced the gap between straight and homosexual applicants. Finally, discrimination is significantly higher in

²⁷ A qualitative review of 12 of these 19 studies was produced by Valfort (2017) and Baert (2018). We found 19 of them because we also looked for unpublished articles, non-scientific journals, doctoral dissertations, or even abstracts. Having data from many unpublished articles is essential for a good meta-analysis due to the possibility of publication bias.

Europe than in North America and it seems that the way of signaling sexual orientation could influence the level of discrimination.

In the first section, we present an up to date literature overview of the 19 studies that tested for discrimination against homosexuals in the labor market by the correspondence testing method. In the second section, we present the data and method used to carry out the meta-analyses. In the third section, we set out the overall results: we present results for all openly homosexual applicants and then focus on the extent of discrimination against only gays and against only lesbians, by type of occupation. Section four describes a multivariate meta-regression analysis with unrestricted WLS econometric models (Stanley and Doucouliagos, 2015, 2017). This is used to investigate how explanatory variables affect the level of discrimination. The fifth section concludes the chapter.

2. Literature overview

Since Adam (1981), many correspondence tests have been conducted in order to detect discrimination against homosexual applicants in hiring in OECD countries. We count 19 of them covering 11 countries. This literature is very widespread in North America, and particularly in the USA. In this country, seven field experiments have been performed while only one was made in Canada. Next to this, correspondence studies were also conducted in France, Italy, Sweden, United Kingdom, Greece, Cyprus, Germany, Austria and Belgium. We present an overview of these studies in Table 1.

A total of 51,249 applications were sent to employers in OECD countries: 27,317 to test discrimination against gay candidates and 23,932 to test discrimination against lesbian candidates.

Table 1: Overview of correspondence studies conducted in order to test discrimination against Gays and Lesbians

<i>Treatment</i>	<i>Country of analysis</i>	<i>Study</i>	<i>Collection year</i>	<i>Resume sent</i>	<i>Procedure</i>	<i>Effect</i>
<i>Gays (versus straight males)</i>	<i>USA</i>	<i>Acquisti and Fong (2016)</i>	<i>2013</i>	<i>2091</i>	<i>Single</i>	<i>0</i>
		<i>Bailey et al. (2013)</i>	<i>2010</i>	<i>2304</i>	<i>Matched</i>	<i>0</i>
		<i>Berger and Kelly (1981)</i>	<i>1979</i>	<i>230</i>	<i>Matched</i>	<i>0</i>
		<i>Gorsuch (2015)</i>	<i>2014</i>	<i>661</i>	<i>Single</i>	<i>Sign.</i>
		<i>Tilcsik (2011)</i>	<i>2005</i>	<i>3538</i>	<i>Matched</i>	<i>Sign.</i>
		<i>The Equal Rights Center and Freedom to Work (2012)</i>	<i>2012</i>	<i>100</i>	<i>Matched</i>	<i>Sign.</i>
	<i>Canada</i>	<i>Adam (1981)</i>	<i>1979</i>	<i>81</i>	<i>Single</i>	<i>Sign.</i>
	<i>France</i>	<i>Amadieu (2014)²⁸</i>	<i>2014</i>	<i>500</i>	<i>Matched</i>	<i>Sign.</i>
		<i>Challe et al. (2018)</i>	<i>2017</i>	<i>772</i>	<i>Matched</i>	<i>0</i>
	<i>Italy</i>	<i>Patacchini et al. (2015)</i>	<i>2012</i>	<i>1163</i>	<i>Matched</i>	<i>Sign.</i>
	<i>Sweden</i>	<i>Ahmed et al. (2013)</i>	<i>2010</i>	<i>1975</i>	<i>Single</i>	<i>Sign.</i>
	<i>United Kingdom</i>	<i>Drydakis (2015)</i>	<i>2013-2015</i>	<i>5628</i>	<i>Matched</i>	<i>Sign.</i>
<i>Lesbian (versus straight females)</i>	<i>USA</i>	<i>Bailey et al. (2013)</i>	<i>2010</i>	<i>2304</i>	<i>Matched</i>	<i>0</i>
		<i>Gorsuch (2015)</i>	<i>2014</i>	<i>673</i>	<i>Single</i>	<i>0</i>
		<i>Mishel (2016)</i>	<i>2014</i>	<i>1550</i>	<i>Matched</i>	<i>Sign.</i>
		<i>The Equal Rights Center and Freedom to Work (2012)</i>	<i>2012</i>	<i>100</i>	<i>Matched</i>	<i>Sign.</i>
	<i>Canada</i>	<i>Adam (1981)</i>	<i>1979</i>	<i>82</i>	<i>Single</i>	<i>Sign.</i>
	<i>France</i>	<i>Challe et al. (2018)</i>	<i>2017</i>	<i>534</i>	<i>Matched</i>	<i>0</i>
	<i>Germany</i>	<i>Weichselbaumer (2015)</i>	<i>2011-2012</i>	<i>1339</i>	<i>Matched/Single</i>	<i>Sign.</i>
	<i>Austria</i>	<i>Weichselbaumer (2003)</i>	<i>1998-2000</i>	<i>1226</i>	<i>Matched</i>	<i>Sign.</i>
	<i>Belgium</i>	<i>Baert (2014)</i>	<i>2012-2013</i>	<i>1152</i>	<i>Matched</i>	<i>0</i>
	<i>Italy</i>	<i>Patacchini et al. (2015)</i>	<i>2012</i>	<i>1157</i>	<i>Matched</i>	<i>0</i>
	<i>Sweden</i>	<i>Ahmed et al. (2013)</i>	<i>2010</i>	<i>2015</i>	<i>Single</i>	<i>Sign.</i>
	<i>United Kingdom</i>	<i>Drydakis (2015)</i>	<i>2013-2015</i>	<i>5470</i>	<i>Matched</i>	<i>Sign.</i>
	<i>Greece</i>	<i>Drydakis (2011)</i>	<i>2007-2008</i>	<i>2114</i>	<i>Matched</i>	<i>Sign.</i>
	<i>Cyprus</i>	<i>Drydakis (2014)</i>	<i>2010-2011</i>	<i>4216</i>	<i>Matched</i>	<i>Sign.</i>

Notes. "Sign."(respectively "0") indicates an overall significant discrimination (no discrimination) against homosexual applicants.

²⁸ <https://www.20minutes.fr/societe/2188255-20171214-lgbt-comment-lutter-contre-discriminations-travail>

In 5 studies, the authors used the “*Single*” inquiries procedure. As said before in the first chapter, in this type of experiment, each employer receives only one resume from a randomly selected job-seeker. This type of test allows the researcher to eliminate the probability of detection and hence revealing the purpose of the experiment. However, this method does not control the effect of unobservable fixed variables on the response rate and therefore requires more applications to obtain the same statistical significance as the “*Matched*” procedure, which is used in 15 studies (Weichselbaumer, 2015, used both).²⁹

As we can see from this table, only seven studies reported no significant discrimination against homosexuals. Gorsuch (2015) and Patacchini et al. (2015) find the presence of significant discrimination against gay male applicants only. It is important to note that no study has reported preferential treatment for homosexual candidates.

3. Method and data

All these correspondence tests were conducted along similar lines. Accordingly, meta-analysis was used to provide a quantitative summary of the existing literature in a systematic manner (Zschirnt and Ruedin, 2016). So as to improve comparability among the studies, face-to-face interviews (e.g. Hebl et al., 2002; Barron and Hebl, 2013) and laboratory experiments (e.g. Baert, 2018; Van Hove and Lievens, 2003) were omitted and

²⁹ For a thorough discussion on the best procedure to choose when making testing, see Vuolo et al. (2018).

we concentrated instead on correspondence tests performed in OECD country labor markets.³⁰

To search for the data, we used *Google Scholar*, Econlit, and Elsevier's *ScienceDirect*, with the following keywords: “discrimination”, “hiring”, “labor market”, “correspondence test”, “sexual orientation”, “field experiment”, “testing”, “LGBTI”, “gay”, “lesbian”. We also drew on the reviews by Valfort (2017) and Baert (2018).

We have included all the data from the studies presented in Table 1, except for the study by The Equal Rights Center and Freedom to Work (2012). We choose not to include it in the meta-analyses because this study compares response rates for homosexual applicants and straight applicants when the homosexual is more skilled than the straights. The purpose of this study was more to prove the existence of discrimination by highlighting the cases where a less-qualified straight applicant was chosen compared to a more-qualified homosexual applicant, and not to calculate the extent of the phenomenon. We thus included data from 18 studies, covering 11 OECD countries with similar levels of human and economic development.

As rental decisions in the first chapter, hiring decisions are taken by different agents (recruiters) and are therefore unlikely to influence each other. Therefore, each subgroup can be treated to some extent as an independent experiment. Sexual orientation is the variable of prime interest here, but subgroups also allow us to evaluate discrimination by gender (discrimination against lesbians or gays), type of jobs, procedure, continent, type of signal determining sexual orientation, and type of information provided in applications.

³⁰ Because of the control that correspondence testing provides, the studies carried out by this method really are comparable, unlike in-person tests.

The studies all report the levels of discrimination faced by homosexual applicants in their own ways: e.g. in terms of risk ratios, odds ratios, net discrimination rates. Unfortunately, there is no established standard in the literature. To be consistent with the first chapter, and following Zschirnt and Ruedin (2016), we present the results of meta-analyses on the same basis, in terms of *absolute discrimination*, through the odds ratio, which “has statistical properties that often make it the best choice for a meta-analysis” (Borenstein et al., 2009).³¹

The odds ratio is in our case the ratio of the following two odds: the odds of getting a positive response (receiving an invitation for an interview) for the *homosexual group* over the odds of getting a positive response for the *straight group*. Put differently, it is the probability of being chosen/favored (receiving a positive response while the other applicant does not) for a homosexual applicant over the probability of being chosen for a straight applicant.

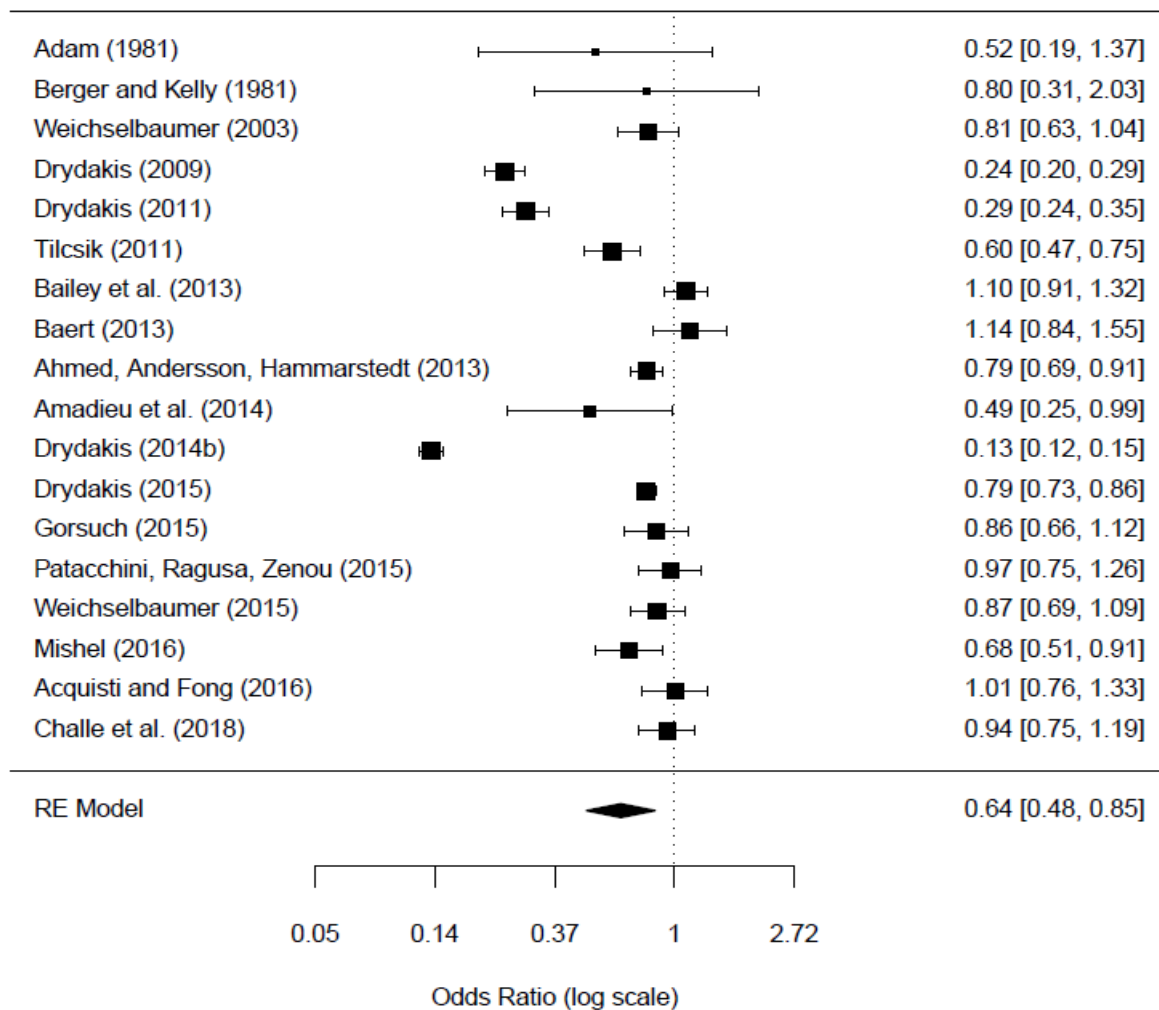
4. Meta-analysis results

We begin by setting out the overall results of a meta-analysis that takes into account the discrimination reported against homosexuals in all studies. To present the overall results, we use a random effect model, as it seems reasonable enough to assume that the real effect size is not exactly the same for every study (presence of between-studies heterogeneity³²). Figure 1 is a forest plot and presents the overall results.

³¹ Note, however, that we reach similar conclusions with simple risk ratios.

³² Note that, as in the first chapter, similar conclusions are reached using a fixed-effect model, but as this model assumes that all studies share the same real variable of interest, excessive importance is given to the two studies with the biggest samples, while the others are largely

Figure 1: Sexual Orientation Discrimination in Hiring Decisions



Note: This forest plot (figure 1) displays the odds ratios in log scale of each study (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 18$: study level; 51,049 applications, $I^2 = 61\%$)

Discrimination against homosexuals in hiring decisions in OECD countries is significant (p -value = 0.002) and the odds ratio is 0.64: homosexual applicants have 36% lower odds of receiving a positive response from an employer, for equal information provided in the resumes, compared to straight candidates. If we take into account only the studies that

ignored. Yet these latter studies, although having a smaller sample, capture an effect that these two large studies do not.

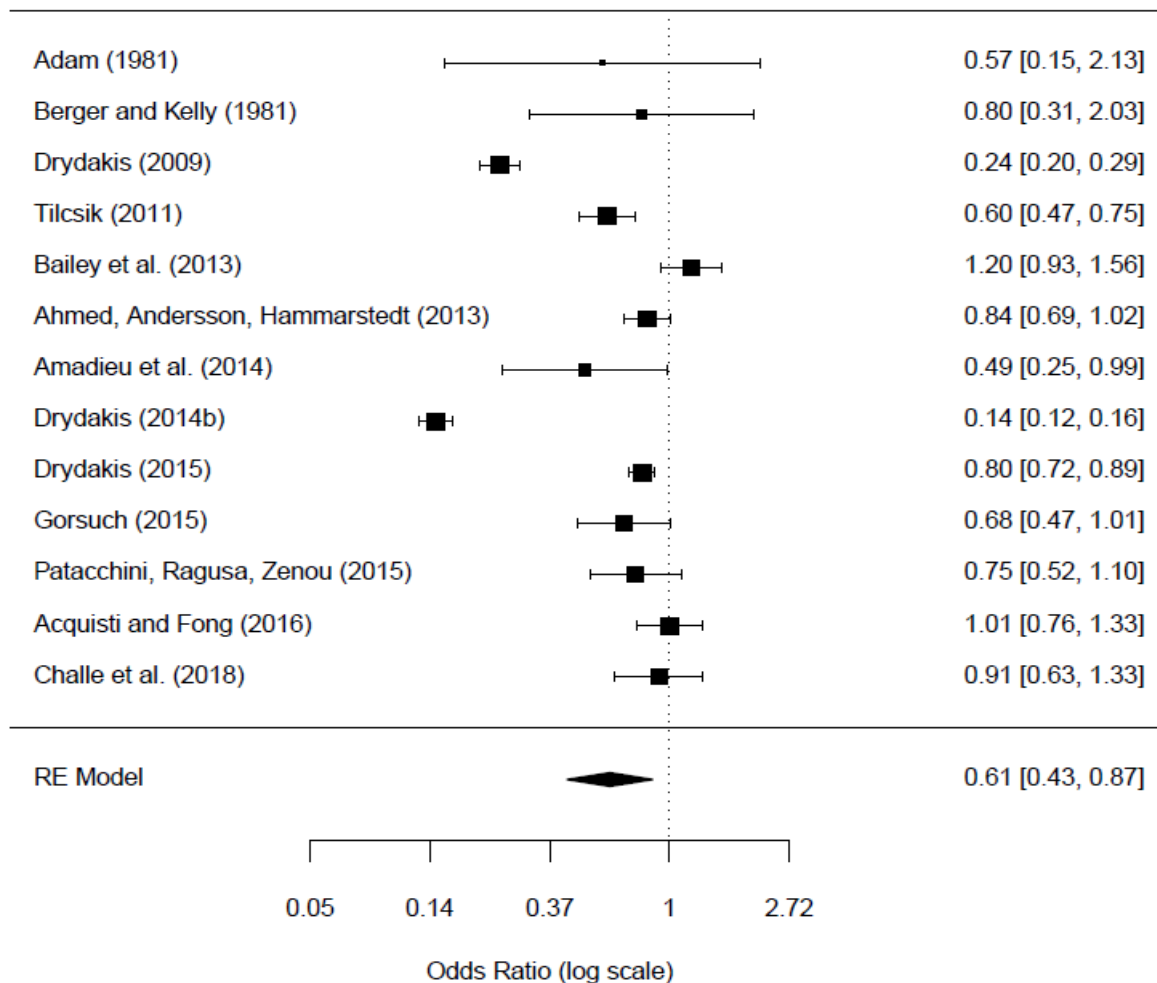
have been carried out in the last decade (more representative of the current level of discrimination), we find an odds ratio of 0.65 ($N = 13$), which is of the same order of magnitude. Thus, homosexual applicants face substantial discrimination in OECD countries. It is worth noting that these findings are similar to the results reported by Zschirnt and Ruedin (2016) for discrimination on ethnic grounds when hiring. This suggests that openly homosexual applicants face a level of discrimination similar to ethnic minorities in hiring decisions.

As often in the literature (e.g. Drydakis, 2014, 2015; Bailey et al., 2013; Patacchini et al., 2015; Ahmed et al., 2013), we can separate responses provided by employers according to the applicant's gender to investigate if there is differential treatment between gay and lesbian applicants. Indeed, there are many reasons to believe that discrimination against homosexual applicants varies by gender. For example, research on beliefs, perceptions, and stereotypes about male and female characteristics shows that men are considered to be more ambitious, independent, assertive, competitive, and therefore are perceived as more productive compared to women, who are seen as more generous, affectionate, and sensitive, which are characteristics not linked to labour market success (Broverman et al. 1972; Deaux and Lewis 1984; Heilman 2001; Connell 2005; Ellemers 2018).³³ Conversely, it seems that gays and lesbians are perceived to have the stereotypical characteristics of the opposite sex (Haddock et al., 1993; Kite and Deaux 1987; Jackson and Sullivan 1989; Blashill and Powlishta 2009; Niedlich et al., 2015). Therefore, we might expect for gay men to be more discriminated, compared to lesbian women.

³³ Note that Drydakis et al. (2018) have recently demonstrated the impact of masculine and feminine traits on the hiring response rates in a correspondence testing study: "In both male and female-dominated occupations, women with masculine personality traits have an occupational access advantage."

First, we present in Figure 1.1 the results of a meta-analysis that takes into account the discrimination reported in all studies against gay applicants. Results are also reported in terms of odds ratio, the ratio of two odds: the odds of getting a positive response for the *gay group* over the odds of getting a positive response for the *straight male group*.

Figure 1.1: Discrimination against Gays in Hiring Decisions



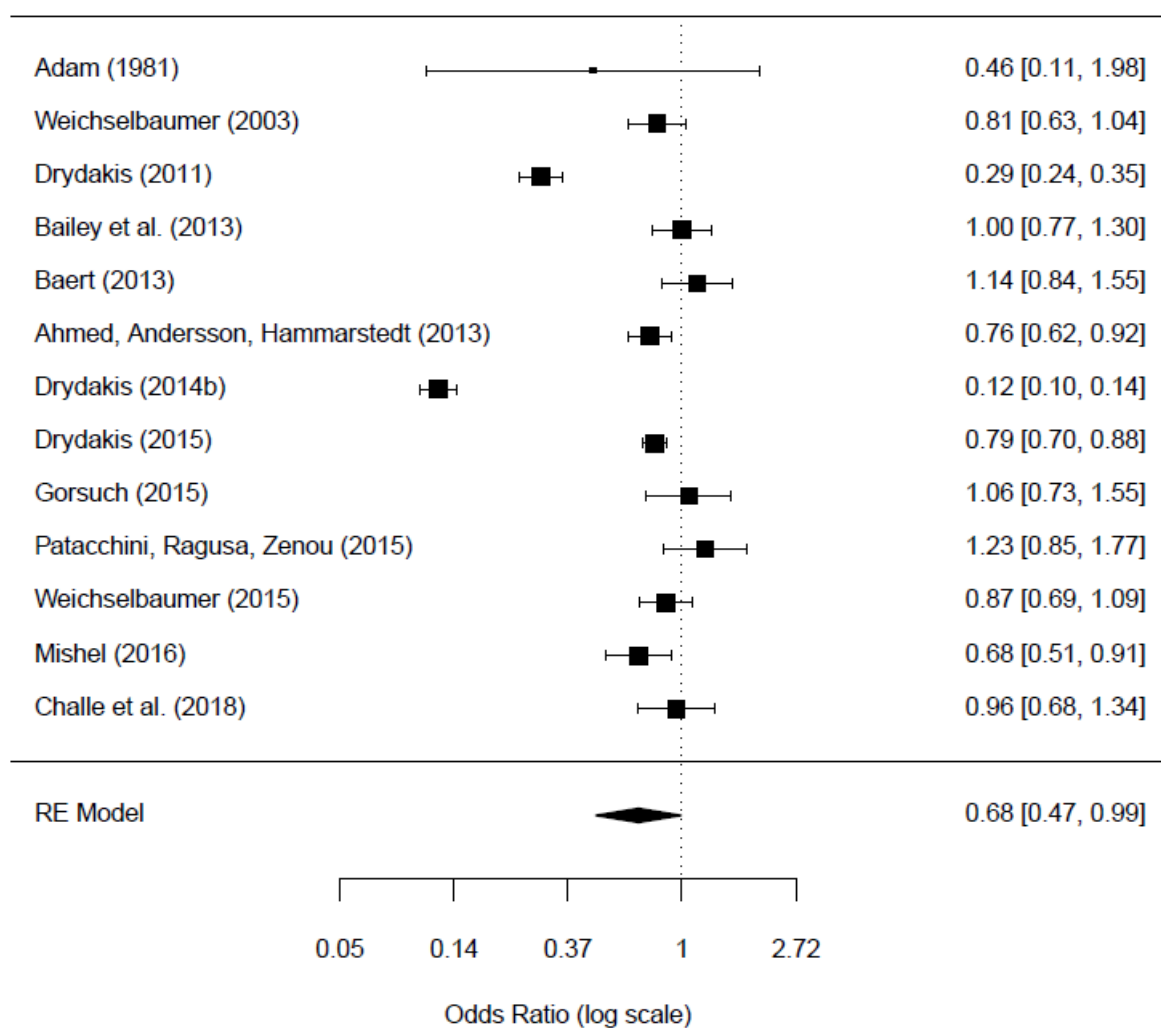
Note: This forest plot (figure 1.1) displays the odds ratios in log scale of each study deferring discrimination against gay applicants (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 13$: study level, 27.317 applications, $I^2 = 61\%$)

Discrimination against gay applicants in hiring decisions in OECD countries is significant (p -value = 0.006). We can see that the effect size is 0.61: for equal information provided

in the resumes, gay applicants have 39% lower odds of receiving a response from an employer than straight males.

Lastly, Figure 1.2 shows the results for women applicants, again as a forest plot. Here too we present the results in terms of odds ratio, the ratio of two odds: the odds of getting a positive response for the *lesbian group* over the odds of getting a positive response for the *straight female group*.

Figure 1.2: Discrimination against Lesbians in hiring decision



Note: This forest plot (figure 1.2) displays the odds ratios in log scale of each study deferring discrimination against lesbian applicants (point estimate as square, two standard errors as lines). The lozenge at the bottom indicates the effect size across studies ($N = 13$: study level, 23.832 applications, $I^2 = 70\%$)

The effect size across studies noted by *positive responses* is 0.68 (p-value = 0.045): lesbian applicants have 32% lower odds than straight women of receiving a positive response from an employer. From these last forest plots, it seems that there is an interaction between sexual orientation discrimination and the gender of the applicants. Sexual orientation discrimination seems slightly greater for male than for female applicants.

However, these results need to be nuanced. Indeed, there are many reasons to believe that discrimination against homosexual applicants varies by the type of occupation. For example, due to stereotypes about homosexual men and women's femininity/masculinity (Haddock et al., 1993; Kite and Deaux 1987; Jackson and Sullivan 1989; Blashill and Powlishta, 2009; Mize and Manago, 2018), we can expect that the degree of discrimination depends on whether an occupation is predominantly female or predominantly male. Moreover, there are reasons to believe that discrimination against minority applicants (whether by ethnic origin, gender, sexual orientation, etc.) also depends on whether applications are sent for high-skilled or for low-skilled jobs. Indeed, among other reasons, employers focus more on participants' skills for positions of responsibility and/or occupations where special skills are required (e.g. banker, manager) while for low-skilled jobs (e.g. waiter in a cafe, salesperson in a shop), employer selection criteria are less related to skills (e.g. physical criteria, sexual orientation). Moreover, it might be that a job offer for a position as a mechanic, a waiter in a café, or a sales assistant in a shop will receive on average more applications than for a commercial director or managerial position. An employer can more easily express discriminatory preferences when the number of candidates is high. Moreover, the employer is less likely to be caught in the act of discrimination. Thus, low-skilled jobs can be expected to be more at risk in terms of sexual orientation discrimination (Badgett et al., 2007; Eurobarometer, 2007; Drydakis, 2012).

Therefore, we implemented 10 additional meta-analyses that determine the level of discrimination according to the type of occupation.³⁴ Discrimination in office jobs has been the most tested in the literature with almost 15,000 applications sent to employers.

Table 2: Meta-analyses of sexual orientation discrimination by type of jobs and gender (Random effect model)

<i>Type of jobs</i>	<i>N (study level)</i>	<i>Resumes sent</i>	<i>Gays (1)</i>	<i>Lesbians (2)</i>	<i>Total</i>	<i>P-value T-Test (1)=(2)</i>
<i>Low-skilled</i>	<i>13</i>	<i>31288</i>	<i>0.50</i>	<i>0.69</i>	<i>0.61</i>	<i>0.03</i>
<i>High-skilled</i>	<i>7</i>	<i>19531</i>	<i>0.80</i>	<i>0.79</i>	<i>0.80</i>	<i>0.98</i>
<i>Masculine</i>	<i>9</i>	<i>15533</i>	<i>0.45</i>	<i>0.55</i>	<i>0.50</i>	<i>0.17</i>
<i>Feminine</i>	<i>5</i>	<i>9372</i>	<i>0.90</i>	<i>0.78</i>	<i>0.83</i>	<i>0.12</i>
<i>Office</i>	<i>9</i>	<i>14293</i>	<i>0.36</i>	<i>0.59</i>	<i>0.52</i>	<i>0.25</i>
<i>Industrial</i>	<i>5</i>	<i>4507</i>	<i>0.28</i>	<i>0.48</i>	<i>0.41</i>	<i>0.74</i>
<i>Shop sales</i>	<i>5</i>	<i>5612</i>	<i>0.37</i>	<i>0.41</i>	<i>0.38</i>	<i>0.56</i>
<i>Social care, social services, nurses</i>	<i>5</i>	<i>8892</i>	<i>0.86</i>	<i>0.78</i>	<i>0.81</i>	<i>0.15</i>
<i>Restaurant and café services</i>	<i>4</i>	<i>3778</i>	<i>0.29</i>	<i>0.32</i>	<i>0.31</i>	<i>0.80</i>
<i>Accountancy, banking, finance and management</i>	<i>4</i>	<i>7359</i>	<i>0.83</i>	<i>0.85</i>	<i>0.84</i>	<i>0.60</i>
<i>Others</i>	<i>4</i>	<i>6378</i>	<i>NA</i>	<i>NA</i>	<i>NA</i>	<i>NA</i>

Notes. The last column reported the p-value of a T-test of the null hypothesis that discrimination faced by gays equals discrimination faced by lesbians.

³⁴ We use the same classification as Drydakis (2009, 2011, 2014, 2015) for low skilled, high skilled, masculine and feminine jobs: industrial jobs, sales, restaurant and café services and office jobs are considered as low-skilled jobs. Moreover, we also included customer service, cleaner and nurse in this category. High-skilled jobs regroup analytical positions, accountancy, banking, finance, management but also education and teaching, engineering. accountancy, banking, finance, management, engineering, industrial, and jobs like motor vehicle driver are considered as “masculine” jobs while education and teaching jobs, social care, social services, charity, and some jobs like occupational therapist, nurse, cleaner are considered as “Feminine”.

The detail of jobs used by authors are provided in Appendix (Table 1.1). Table 2 presents the results of these meta-analyses by controlling for the applicant's gender.

Once again, results are presented in terms of odd ratios. For example, the coefficient 0.50 for gay candidates in *low-skilled* jobs suggests that straight males are twice as likely to be chosen by employers as gays in the selection process for low-skilled jobs and the coefficient 0.69 for lesbians in *low-skilled* jobs suggests that they have 31% lower odds than straight women of receiving a positive response from an employer.

First of all, we can see that lesbians seem to face less discrimination than gays (except in jobs considered as “feminine”, which is consistent with the literature on stereotypes about homosexual men and women's femininity/masculinity). Clustering standard errors at the study level, we only find a significant difference in discrimination between gays and lesbians at the 5% level for the *low-skilled* jobs category (T-test, p-value: 0.033).³⁵

Consistent with our expectations, discrimination in the selection process for *low-skilled* jobs is significantly higher than discrimination in the selection process for *high-skilled* jobs (T-test, p-value: 0.049).

Therefore, in job offers for low-skilled jobs, homosexual applicants face substantial discrimination, and there is a significant difference in discrimination between women and men, while in job offers for high-skilled jobs, discrimination is significantly lower, and there is no difference between men and women.

Finally, we can note that the odds ratios are always below 1. Therefore, lesbians are on average not preferred to straight women, even in “masculine” jobs, and gay men are not preferred to straight men, even in “feminine” jobs. This would suggest the presence of

³⁵ Indeed, in jobs considered as feminine (masculine), gay (lesbian) applicants seem to face less discrimination than lesbian (gay) applicants, but the difference in discrimination is not significant at the 10 percent level.

discrimination from pure homophobia (taste-based) and/or the presence of statistical discrimination not based on feminine/masculine traits, such as discrimination due to the perception of higher level of health issues from homosexual individuals (e.g. Worthen, 2013; Jorm et al., 2002; Saunders and Valente, 1987).

5. Meta-regression analysis

In this section, we present a multivariate regression analysis in order to examine the determinants of the level of discrimination (in log odds ratio) against gay and lesbian applicants with the most recent and conservative econometric model: unrestricted weighted least squares (WLS). Meta-analysis focuses on the value of the variable of interest while meta-regression focuses on the variables that influence this variable.

We now present all the explanatory variables that we have chosen to use for our regression and explain the way in which we code variables.

5.1 Explanatory variables

In order to fight discrimination, it is essential to know its origins. As mentioned before, this discrimination may be preference-based or statistical. Following Drydakis (2014), and as statistical discrimination is mainly due to a lack of information about the job candidates, a method of testing the source of discrimination consists in the following: comparing the level of discrimination between straight and homosexual applicants when little information is sent to the employers versus the level of discrimination when more positive information is sent to employers. Therefore, this allows to study whether

discrimination against homosexuals can be reduced by increasing the information given about them. We code the dummy variable as follows: “*Detailed information*” takes the value “1” when the application sent to employers consisted of more than a resume (such as a detailed cover letter, a reference letter, etc.) and “0” when just a resume and a statement of interest were sent (“*Standard information*”).³⁶

We assume that providing more correct information about the candidates should not affect the level of discrimination against homosexuals if discrimination is taste-based, but it should reduce the discrimination if part of the discrimination is statistical.

As indicated, correspondence testing can be done in either of two ways: five studies used the *single inquiries* procedure and 14 used the *matched* procedure (Weichselbaumer, 2015, used both). We took the *single inquiries* procedure as our reference.

The countries in the database were grouped by continents and a dummy variable used to distinguish applications made in Europe from those made in North America (reference).

Low-skilled jobs is a dummy variable which takes the value “1” if applications are sent in response to ads for low-skilled jobs and “0” if applications are sent in response to ads for high-skilled jobs.

Feminine jobs is a dummy variable which takes the value “1” if applications are sent in response to ads for “feminine” jobs and “0” if applications are sent in response to ads for “masculine” jobs.

³⁶ Consider examples of different cover letters sent to test statistical discrimination (Drydakis, 2014): “Standard information” applicant: “Dear Sir/Madam, please find attached my Curriculum Vitae for your kind consideration for the vacancy as was advertised in.... Yours sincerely, Name and surname.” compared to “Detailed information” applicant: Dear Sir/Madam, please find attached my Curriculum Vitae for your kind consideration for the vacancy as was advertised in.... I have ten years of relevant working experience (Job task / Job Specific Skills / Firm). I provide a reference letter from my previous employer. Yours sincerely, Name and surname”

Female is a dummy variable which takes the value “1” if applicants are women and “0” when applicants are men (reference).

The last variable is how the sexual orientation is signaled. The different signals are *Partner* (i.e. mention of the partner’s gender in the cover letter), *Online social networks* (i.e. mention of the sexual orientation on social media), and finally a reference to involvement in a *LGBT association*. We chose the latter as our reference. The detail of signals used by authors are provided in Appendix (Table 1.1).

Our baseline model for the MRA is specified as follows:

$$y_j = \beta_0 + \beta_1 x_{1j} + \beta_2 x_{2j} + \dots + \varepsilon_j$$

where y_j is the odds ratio (in log) on the correspondence test (a subgroup of a study) j and β_0 is the intercept. The variables x_i specify different characteristics of the correspondence test, such as type of jobs, detailed information provided in the applications, gender of applicants. ε_j in this baseline model specifies the between-subgroup variation. Stanley and Doucouliagos (2015, 2017) propose estimating this baseline using an unrestricted least squares (WLS) model, which consists in estimating this equation using weighted least squares with $1/\text{se}^2(y_j)$ (where se is the standard error of log odds ratio) as the weights. When there is publication selection bias, the WLS-MRA estimates are more suitable than random effects estimates.³⁷

³⁷ We did not find any evidence of publication bias with Egger’s test ($z = 0.3743$, $p = 0.7081$) but, as we can never affirm with certainty the absence of a publication bias, we choose to present estimates from the WLS-MRA model. “Simulations and statistical theory show that WLS-MRA provides satisfactory estimates of meta-regression coefficients that are practically equivalent to random effects when there is no publication bias. When there is publication selection bias, WLS-MRA always has smaller bias than mixed effects or random effects.” (Stanley and Doucouliagos, 2015, 2017)

In the interest of thoroughness, standard errors were grouped at the study level in all specifications, to make them robust to intra-study dependence. Finally, it is important to pay attention to multicollinearity in our regressions because a meta-regression analysis is more subject to this problem than classical econometrics. In our case, all explanatory variables present a variance inflation factor (VIF) less than 3 which indicate low correlation among them.

In this meta-regression, we have only taken into account the subgroups for which all information was provided (e.g. details of information in the e-mail are specified, gender is specified, type of jobs is specified). Descriptive statistics of variables used in the MRA are provided in Appendix (Table 3.1).

5.2 Results

The results are reported in Table 3 in terms of the log odds ratio. Once again, the odds ratio is the ratio of the following two odds: the odds of getting a positive response for the *homosexual group* over the odds of getting a positive response for the *straight group*.

First, we test three interactions with the gender variable “*Female*” to investigate (1) whether discrimination against gay applicants in the selection process for low-skilled jobs appear to be higher than discrimination against lesbian applicants, (2) whether discrimination against gays/lesbians appear to be lower in the selection process for feminine/masculine jobs and finally (3) if the effect of adding positive information in applications is different between gay and lesbian applicants. Then, we add control variables (type of procedure, continent, type of information provided in applications).

Table 3. Results of the unrestricted weighted least squares meta-regression

<i>Intercept</i>	-0.732** (0.33)	-0.240*** (0.03)	-0.239*** (0.05)	-0.614** (0.26)	-1.074 *** (0.35)	-1.173*** (0.37)	-0.781** (0.33)	-0.289 (0.39)	-0.146 (0.43)
<i>Female</i> ^(a)	0.121 (0.15)		0.001 (0.06)	0.056 (0.25)		0.204 (0.19)	0.200 (0.19)	0.061 (0.12)	0.048 (0.13)
<i>Low-skilled jobs</i> ^(b)		-0.760** (0.38)	-0.972** (0.38)						
<i>Female × Low-skilled jobs</i>			0.378** (0.17)						
<i>Feminine jobs</i> ^(c)				0.456* (0.28)					
<i>Female × Feminine jobs</i>				-0.085 (0.23)					
<i>Detailed information</i> ^(d)					0.595** (0.28)	0.660** (0.32)	0.608* (0.35)	0.723** (0.33)	0.765** (0.34)
<i>Female × Detailed information</i>						-0.136 (0.22)	-0.142 (0.21)	0.174 (0.24)	0.113 (0.20)
<i>Matched</i> ^(e)							-0.429 (0.36)	-0.219 (0.31)	-0.310 (0.32)
<i>Europe</i> ^(f)								-0.947** (0.38)	-1.028** (0.43)
<i>Partner</i> ^(g)									0.688* (0.36)
<i>Online social networks</i> ^(g)									-0.613 (0.38)
<i>Number of subgroups</i>	71	71	71	32 ⁽¹⁾	71	71	71	71	71
<i>R²</i>	0.01	0.23	0.28	0.15	0.13	0.13	0.18	0.42	0.48

Robust Standard errors in parentheses. ***p < 0.01, **p < 0.05, and *p < 0.1. ⁽¹⁾ In this case, we focus our analysis on feminine and masculine jobs only to avoid additional interaction terms.

Ref: ^(a) Male, ^(b) High-skilled, ^(c) Masculine, ^(d) Standard, ^(e) Single inquiries, ^(f) North America, ^(g) Member of LGBT association.

Positive values indicate a lower level of discrimination (less differential treatment) between homosexual and straight applicants and negative values denote a higher level of discrimination between homosexual and straight applicants.

For example, and consistently with meta-analysis results, the negative value for “*Low-skilled jobs*” suggests that the gap between homosexual and straight applicants is broader when candidates apply for low-skilled jobs rather than “*High-skilled jobs*”. On the other hand, the positive value for “*Female × Low-skilled jobs*” suggest that discrimination in job offers for low-skilled jobs is significantly lower when applicants are women rather than men.

We draw from this meta-regression four new interesting results.

When the information provided in applications is high and likely to reassure the employer about the candidate (such as a reference letter attesting to the candidate’s skill, a detailed cover letter that shows strong motivation and determination of the candidate), the differential treatment between homosexual and straight candidates is significantly lower than when just a resume and a statement of interest are sent.³⁸ This means that employers need more reassurance about the homosexual candidate than about the heterosexual candidate and that discrimination does not come from preferences only³⁹(due to a lack of information provided by the candidates). Note that the positive effect of information on the level of discrimination is not significantly higher for women than for men.

We can legitimately ask why employers need to be reassured more about homosexual candidates. There is however some research track: employers’ uncertainty regarding

³⁸ The rate of positive responses increases significantly more for homosexuals than for heterosexuals by the provision of positive information.

³⁹ Indeed, in such a case, increasing the information would not affect the level of discrimination.

sexual orientation minorities' average productivity, work commitment etc. could simply be really high (Drydakis, 2014; Altonji and Pierret, 2001). Moreover, it is possible that there is a lack of information not only on the average productivity but also on the whole productivity distribution. Indeed, as (mostly heterosexual) employers are less used to work with homosexual workers, perceived *variance* of their productivity-related characteristics could potentially be higher than among heterosexual workers. Therefore, risk-averse employers might be reluctant to hire homosexuals because of their allegedly higher productivity variance, although their average productivity might not differ from their heterosexual counterparts. (see Baert, 2018 for a good discussion about second order statistical discrimination against homosexual workers). Finally, we know that homosexual individuals are subject to many stereotypes: about their masculinity/femininity (e.g. Blashill and Powlishta, 2009), about their presumed higher risks of mental health issues, suicide and HIV infection (e.g. Worthen, 2013; Jorm et al., 2002; Saunders and Valente, 1987).

Individuals therefore have sometimes misinformation about the true characteristics of homosexuals. So, when employers receive applications from homosexual job seekers, they may base their evaluation not only on the (limited) information provided by the candidate, but also on stereotypes that exist about homosexuality. For example, with equal qualifications, a homosexual may be considered as in poorer health condition (whether physical or mental), and a fortiori considered as a less productive candidate than a heterosexual, and even by a recruiter who is not homophobic.

Thus, providing more information on the real characteristics of gays and lesbians could reduce employers' uncertainty and mitigate the impact of such negative stereotypes.

However, even when higher information is provided in applications, discrimination remains significant (coefficient for "*Intercept + Detailed information*" = -0.479*), suggesting the presence of pure preference-based discrimination. This type of

discrimination is harder to counter because it does not stem from lack of information but from deep-seated individual preferences. Such a mindset cannot be changed overnight.

Interestingly, and as the overview might suggest, it seems that discrimination against openly homosexual applicants in hiring decisions is significantly higher in studies made in Europe than in North America.

Moreover, the variable “*Feminine jobs*” is positive and significant at 10 percent. So, gay men seem less discriminated against in feminine than in masculine jobs, which is consistent with the literature on stereotypes about homosexual men ‘femininity’. However, there is no significant effect for lesbians (coefficient for “*Feminine jobs* + *Female* × *Feminine jobs*” is not significant).

The coefficient for the variable *Female* is positive and the coefficient for the variable (*Female* + *Female* × *Feminine jobs*) is negative. Thus, and consistently with meta-analysis results, it seems that lesbians are less discriminated against than gays in masculine jobs and that gays are less discriminated against than lesbians in feminine jobs. However, and consistently with meta-analysis, the difference in discrimination is not significant at the 10 percent level.

Finally, it seems that the way of signaling sexual orientation influences the level of discrimination. Indeed, discrimination against homosexual applicants is lower when they indicate their sexual orientation by registering the gender of their partner rather than being a member of an LGBT association.⁴⁰ However, we must be very careful with this result and especially its causes. Indeed, each of these signals has its weaknesses and its qualities. Reporting sexual orientation through the gender of the partner has two main advantages: it announces without ambiguity that the applicant is homosexual, while being a member of an LGBT association is only taken as a proxy of different sexual

⁴⁰ Note that this result is not significant at the 5 percent level: $p = 0.063$.

orientation: an individual can defend the rights of homosexuals without being a homosexual. Finally, reporting sexual orientation through the gender of the partner indicates nothing but the applicant's sexual orientation, while applicants' voluntary involvement in an LGBT association might suggest they are activists. Therefore, the extent of the discrimination found with this last signal should be interpreted with care because it runs the risk of it being overestimated.⁴¹

However, reporting sexual orientation through the gender of the partner is not without its weaknesses. It is already very unconventional to talk about sexual orientation in applications, but mentioning it so openly can be perceived as truly disturbing by employers and may alert them to the fact that they are being tested (especially in the matched procedure). One may think it is a lot more trivial to report having been in an LGBT association ten years ago at the end of a resume.

Thus, we hypothesize that the smaller gap found between homosexual and straight candidates when reporting sexual orientation through the gender of the partner rather than by indicating membership of an LGBT association may also be due to a higher risk of detection. In this case, the level of discrimination may be underestimated.

In addition, the way correspondence tests are performed does not seem to significantly affect the results. The levels of discrimination reported with single inquiries and matched procedures are similar.

⁴¹ Nevertheless, this problem can be mitigated by emphasizing the managerial or financial tasks an applicant holds in the organization that are not in the political realm but relevant from a human capital perspective (e.g. Weichselbaumer, 2003; Tilcsik, 2011; Patacchini et al., 2015; Drydakis, 2015) or by indicating that membership of this group is long outdated (e.g. Drydakis, 2011; Drydakis, 2014).

Due to a lack of data, we have been unable to find any significant differences in discrimination between applicants who report their sexual orientation through social media and individuals who indicate they are members of an LGBT association.

6. Conclusion

By means of this meta-analysis, we provide evidence of substantial hiring discrimination based on sexual orientation in the OECD labor market. At the initial stage of the hiring process, we find that openly homosexual applicants face similar discrimination as ethnic minority applicants (Zschirnt and Ruedin, 2016): homosexual applicants have almost 40% lower odds of receiving a positive response from an employer, for equal information provided in the resumes, than straight candidates. However, this result needs to be nuanced because discrimination varies according to the type of occupation and gender of the applicant: more concretely, discrimination is significantly lower in the selection process for high-skilled jobs than in the selection process for low-skilled jobs. Moreover, there is a significant difference in discrimination between gays and lesbians when they apply for low-skilled jobs: straight males are twice as likely to be chosen by employers as gay males are, while lesbians have “only” 31% lower odds than straight women of receiving a positive response from an employer. We were able to determine that this discrimination is not only a matter of preferences (e.g. xenophobia, homophobia) but is also due to a lack of information on the applicants. Indeed, providing more (positive) information in the content of the applications significantly reduces the gap between straight and homosexual applicants, which implies that employers need to be “reassured” about homosexual applicants. Understanding the nature of discrimination is a necessary step in setting the right policies to limit it. Reducing uncertainty regarding

gay and lesbian characteristics and mitigate the impact of negative stereotypes through a better knowledge of sexual minorities could be part of it, as discrimination does not appear to stem entirely from the homophobic preferences of recruiters. Finally, discrimination found by studies conducted in Europe is significantly higher than discrimination reported by studies performed in North America and it seems that the way of signaling sexual orientation could influence the level of discrimination. We hope that our results provide important information for the future development of non-discrimination and equal hiring opportunities for homosexuals in the labor market in OECD countries.

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Appendix:

Table 1.1 Overview of correspondence studies conducted in order to test discrimination against Gays and Lesbians (all variables)

<i>Treatment</i>	<i>Country of analysis</i>	<i>Study</i>	<i>Resume sent</i>	<i>Procedure</i>	<i>Type of Signal</i>	<i>Type of information</i>	<i>Type of occupation</i>	<i>Effect</i>
<i>Gays (versus straight males)</i>	<i>USA</i>	<i>Acquisti and Fong (2016)</i>	<i>2091</i>	<i>Single</i>	<i>Social Media</i>	<i>Detailed</i>	<i>Technical, Managerial, and Analytic positions</i>	<i>0</i>
		<i>Bailey et al. (2013)</i>	<i>2304</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Administrative/clerical, Warehouse</i>	<i>0</i>
		<i>Berger and Kelly (1981)</i>	<i>230</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Social care, social services</i>	<i>0</i>
		<i>Gorsuch (2015)</i>	<i>661</i>	<i>Single</i>	<i>LGBT</i>	<i>Standard</i>	<i>Office, Retail, Food, Industry</i>	<i>Sign.</i>
		<i>Tilcsik (2011)</i>	<i>3538</i>	<i>Matched</i>	<i>LGBT</i>	<i>Detailed</i>	<i>White-collar jobs at entry level</i>	<i>Sign.</i>
		<i>The Equal Rights Center and Freedom to Work (2012)</i>	<i>100</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Administrative assistant</i>	<i>Sign.</i>
	<i>Canada</i>	<i>Adam (1981)</i>	<i>81</i>	<i>Single</i>	<i>LGBT</i>	<i>Standard</i>	<i>Articling position at legal firm</i>	<i>Sign.</i>
	<i>France</i>	<i>Amadieu (2014)</i>	<i>500</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Commercial in the food industry</i>	<i>Sign.</i>
		<i>Challe et al. (2018)</i>	<i>772</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Administrative manager</i>	<i>0</i>
	<i>Italy</i>	<i>Patacchini et al. (2015)</i>	<i>1163</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Administrative/clerical, Shop sales</i>	<i>Sign.</i>
	<i>Sweden</i>	<i>Ahmed et al. (2013)</i>	<i>1975</i>	<i>Single</i>	<i>Partner</i>	<i>Detailed</i>	<i>Shop sales, Industrial, Teacher, Motor Vehicle Driver, Cleaner, Restaurant and café, Nurse</i>	<i>Sign.</i>
	<i>United Kingdom</i>	<i>Drydakis (2015)</i>	<i>5628</i>	<i>Matched</i>	<i>LGBT</i>	<i>Detailed</i>	<i>Accounting, banking, finance and management, education and teaching, Social care, social services and charity</i>	<i>Sign.</i>
	<i>Greece</i>	<i>Drydakis (2009)</i>	<i>3428</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard</i>	<i>Office, Industrial, Restaurant/Café services, Shop sales</i>	<i>Sign.</i>
	<i>Cyprus</i>	<i>Drydakis (2014b)</i>	<i>4846</i>	<i>Matched</i>	<i>LGBT</i>	<i>Standard/Detailed</i>	<i>Office, Industrial, Restaurant/Café services, Shop sales</i>	<i>Sign.</i>

Lesbian (versus straight females)	USA	Bailey et al. (2013)	2304	Matched	LGBT	Standard	Administrative/clerical, Warehouse	0
		Gorsuch (2015)	673	Single	LGBT	Standard	Office, Retail, Food, Industry	0
		Mishel (2016)	1550	Matched	LGBT	Detailed	Office	Sign.
		The Equal Rights Center and Freedom to Work (2012)	100	Matched	LGBT	Standard	Administrative assistant	Sign.
	Canada	Adam (1981)	82	Single	LGBT	Standard	Articling position at legal firm	Sign.
	France	Challe et al. (2018)	534	Matched	LGBT	Standard	Caregiver	0
	Germany	Weichselbaumer (2015)	1339	Matched/Single	Partner	Detailed	Office	Sign.
	Austria	Weichselbaumer (2003)	1226	Matched	LGBT	Detailed	Office	Sign.
	Belgium	Baert (2014)	1152	Matched	Partner	Detailed	Secretary, Nurse, Industrial, Management, Ergotherapist, Engineer	0
	Italy	Patacchini et al. (2015)	1157	Matched	LGBT	Standard	Administrative/clerical, Shop sales	0
	Sweden	Ahmed et al. (2013)	2015	Single	Partner	Detailed	Shop sales, Industrial, Teacher, Motor Vehicle Driver, Cleaner, Restaurant and café, Nurse	Sign.
	United Kingdom	Drydakis (2015)	5470	Matched	LGBT	Detailed	Accounting, banking, finance and management, education and teaching, Social care, social services and charity	Sign.
	Greece	Drydakis (2011)	2114	Matched	LGBT	Standard	Office, Industrial, Restaurant/Café services, Shop sales	Sign.
	Cyprus	Drydakis (2014b)	4216	Matched	LGBT	Standard/Detailed	Office, Industrial, Restaurant/Café services, Shop sales	Sign.

Notes. “Sign.” (respectively “0”) indicates an overall significant discrimination (no discrimination) against homosexual applicants.

In 15 studies, subjects signal their sexual orientation by highlighting involvement with an association for lesbian, gay, bisexual and transgender (LGBT) rights in the resume. In 3 studies, there is a mention of the partner’s gender in the application. Finally, in Acquisti and Fong (2016), there is no mention of different sexual orientation in the application and homosexuality is signaled on social medias.

In 9 studies, the applications sent to employers consisted of more than a resume (such as a detailed cover letter, a reference letter...) while in 11 studies, just a resume and a statement of interest was sent (Drydakis, 2014, did both).

Table 3.1 Descriptive statistics of variables used in the MRA (odds ratios homosexual vs. straight applicants)

<i>Variable</i>	<i>Masculine/Feminine</i>	<i>Frequency = 1</i>	<i>Frequency = 0</i>
<i>Matched</i>	<i>1 if Matched procedure</i>	<i>50</i>	<i>21</i>
<i>Detailed information</i>	<i>1 if Detailed information</i>	<i>44</i>	<i>27</i>
<i>Female</i>	<i>1 if Female</i>	<i>39</i>	<i>32</i>
<i>Europe</i>	<i>1 if Europe</i>	<i>61</i>	<i>10</i>
<i>Partner</i>	<i>1 if Partner</i>	<i>22</i>	<i>49</i>
<i>Online social networks</i>	<i>1 if Online social networks</i>	<i>1</i>	<i>49</i>
<i>Low-skilled jobs</i>	<i>1 if Low-skilled jobs</i>	<i>56</i>	<i>15</i>
<i>Feminine jobs</i>	<i>1 if Feminine jobs</i>	<i>13</i>	<i>19</i>

Chapter 3

Intermediation and Discrimination in an Investment Game: An Experimental Study

Note: This chapter is associated to an original paper under minor revisions in Journal of Economic Behavior & Organization (Cochard et al., 2019).

1. Introduction

Many everyday life decisions with uncertain outcomes are made by one party where another party bears the principal consequences. For example, investment managers are in charge of other people's money, CEOs make decisions on behalf of shareholders, and we rely on many intermediaries to make decisions that engage our own wealth and welfare (e.g. bankers, lawyers, real estate agents, recruitment agencies, doctors). Several laboratory experiments and empirical studies have shown that people behave differently when they have to make decisions on behalf of other than for themselves (see among others Charness and Jackson, 2009; Reynolds et al., 2009; Eriksen and Kvaløy 2010; Chakravarty et al., 2011; Agranov et al., 2014; Kvaløy and Luzuriaga, 2014; Füllbrunn and Luhan, 2015; Batteux et al., 2017; and the recent meta-analysis by Polman and Wu, 2019).

To our knowledge, among these papers, only Kvaløy and Luzuriaga (2014) investigates the behavior of an intermediary in the well-known trust (or investment) game (Berg et al., 1995). The trust game is a particularly relevant environment to study the behavior of intermediaries. Indeed, a lot of commercial transactions involving intermediaries require a significant amount of trust among parties. As an example, we rely on bankers to invest our wealth in profitable investment projects; we rely on real estate agents to find trustworthy tenants for our house or apartment, etc.

One might expect intermediaries to have different risk attitudes and cognitive and affective involvement since they do not use their own resources. This could result in different decisions between the intermediary and the party directly involved. In this chapter we focus mainly on the discriminatory behavior. Indeed, recent testing studies on the housing market show that real estate agents discriminate against minority groups to a lesser extent than owners (see Ahmed and Hammarstedt, 2008; Bosch et al., 2010;

Bosch et al., 2015; Bunel et al., 2017; Le Gallo et al., 2018; see also Flage, 2018, for a meta-analysis). This kind of behavior could have important economic and social implications for the society and particularly for the well-being of discriminated minority groups. However, the causes of this behavior are still not well defined. Why can we expect intermediaries to adopt a different discriminatory behavior than the party directly involved?

As said in other chapters, economic theories of discrimination are generally divided between two main concepts. “Taste-based” discrimination refers to the discrimination which occurs due to preferences. This means that agents who discriminate have personal hostile attitudes towards a foreign group or comply with the negative attitude of the group of individuals to which they are attached (Becker, 1957; Yinger, 1986). To follow-up with our example on the rental housing market (note that it also applies to other situations, such as hiring decisions), this could be illustrated by a private landlord or a real estate agent preferring to rent an apartment to an individual of the same group as her (for example from the same ethnic group) than to an individual from another group simply because she has a taste for in-group members (and/or a distaste for out-group members). “Statistical discrimination” (Phelps, 1972; Arrow, 1973) refers to the rejection of an individual because members of her own group are believed (rightly or wrongly) to often have undesirable characteristics. An individual would discriminate a person belonging to an outside group in favor of an individual from her own group because of a lack of information regarding individual characteristics. In the rental housing market, this would be illustrated by private landlords or real estate agents preferring to rent to a member of their own group (say ethnic) because they believe that members of their own groups are on average more trustworthy. They would for instance hold the belief that there are thereby better chances that the rent is paid, and/or that the tenant does not deteriorate the apartment. Both types of discrimination have been observed in past studies. For example, Baldini and Federici (2011), Hanson and Hawley (2011), Bunel et

al. (2017) and Le Gallo et al. (2018) were able to distinguish the presence of both taste-based and statistical discriminations in the rental housing market. Other empirical studies (e.g. Carlsson, 2010; Oreopoulos, 2011; Bonoli and Hinrichs, 2012) explore the nature of the discrimination in the labor market.

The first intuitive reason for which intermediaries could adopt a different discriminatory behavior than the party directly involved is that she does not put her own assets at risk. Regarding the housing market, the transaction is of course less risky for the professional than for the owner, to the extent that the professional does not invest her own property. In addition, she is likely to have more opportunities to diversify her risks. Moreover, professionals have a better ability to estimate the “quality” of the renter and are therefore less subject to prejudice against minority candidates. For these reasons, they are in consequence less likely to be subject to “statistical” discrimination. It seems less easy to conjecture a different level of taste-based discrimination. However, it is probably riskier for professionals to be convicted for discriminatory practices, both in terms of penal and civil sanctions and in terms of reputation. Of course, all these potential sources of differences in discriminatory behavior between intermediaries and the party directly involved may be at work in other markets. This includes for example recruitment agencies in the labor market or all other cases where an intermediary has to deal with non-personal assets in contexts where discrimination may be an issue.

Identifying a difference in discriminatory behavior between intermediaries and the party directly involved seem to have important implications for society: the presence of intermediaries could help to fight against discriminations and all the economic and social inefficiencies they imply. Efficient policy making therefore requires a better understanding of each of these potential factors. Hence, we believe that the first step is to investigate the behavior of a “pure” intermediary, i.e. an agent who takes no personal financial risks, has no superior diversification opportunities, no better information, who is not restricted by stronger judicial rules and who does not face reputational issues.

Our main contribution is therefore to control for all external factors and focus on a pure intermediary, i.e. who simply does not risk her own resources but the resources of somebody else—the “owner”. Is such an intermediary intrinsically less prone to discriminate in a trust game, given that she takes no personal risks? Or will the intermediary feel some responsibility towards the owner, maybe leading her to discriminate more? To answer these questions, we need to use an environment in which we control the potential factors of decisions, in order to abstract from external factors. Implementing an experiment in the laboratory satisfies this objective. Focusing on “pure” intermediation also allows us analyzing the effect of intermediation in general and not only in a specific market. To the best of our knowledge, no laboratory study has ever investigated whether intermediaries discriminate to a different degree than the party directly involved in games with strategic interaction.

Another important contribution of our study is that by using laboratory methods we are not only able to analyze the effect of intermediation on discrimination, but also to investigate the causes of this effect, by cleanly disentangling between taste-based and statistical discriminations. Empirical studies exploring the nature of discrimination in the presence of intermediation are seldom. To our knowledge, the only study to address this issue is the meta-analysis carried out in the first chapter, which concludes that real estate agents display less statistical discrimination than owners. This could explain in part the difference in discrimination between real estate agents and owners observed in most field experiments on the housing market (e.g. Ahmed and Hammarstedt, 2008; Bosch et al., 2010; Bosch et al., 2015; Bunel et al., 2017; Le Gallo et al., 2018). However, those findings may reflect specific properties of the housing market (e.g. legislation, organization of real estate agencies, nature of the interaction), and might not apply to other contexts where intermediation takes place. Last but not least, it is quite possible that real estate agents detect being tested more often than private owners (especially in matched paired procedure), thus underestimating their real discriminatory behavior.

To summarize, we chose to conduct a decontextualized laboratory experiment to (i) isolate the role of intermediary from other environmental variables captured in field studies and (ii) implement treatments which directly aim at disentangling between statistical and taste-based discrimination.

In this study, we run an incentivized laboratory experiment that builds on the well-established trust game framework. As the trustor's investment in the trust game can be motivated both by his preferences towards the trustee but also by his beliefs in trustee's reciprocation, it turns out to be a very suitable environment to study whether potential discrimination comes from taste-based (preferences) or statistical (beliefs) considerations. In this simple setting, we introduce a third player, the "intermediary", who plays the trust game with the owner's endowment. It should be noticed however that in this study, we consider a "pure intermediary" to the extent that this subject receives a flat payoff, so that she will take no personal risk in this game (and thus does not have any monetary incentives to care about the owner's payoff). This is in line with Kvaløy and Luzuriaga (2014), who run an experiment on the trust game with a pure intermediary and observe no difference in the average amount sent by intermediaries and owners. In addition to this three-player trust game, we create social groups (social identities) to investigate what happens if the owner and the intermediary are (or not) in the same group as the recipient. The use of artificial identity group to study issues related to discrimination is widespread in the identity economics literature (Eckel and Grossman, 2005; Charness et al., 2007; Chen and Li, 2009; Chen and Chen, 2011; Chen et al. 2014; Currarini and Mengel, 2016).

We refer to two main strands of the literature. The first one is the literature on intermediation and on "risk-taking on behalf of others". There is a growing literature addressing risk-taking on behalf of others: does an individual take more or less risk when she plays with other people's money? The evidence is mixed, as some studies tend to show that she takes less risk (e.g. Charness and Jackson 2009; Reynolds et al. 2009;

Eriksen and Kvaløy 2010, Füllbrunn and Luhan, 2015), while others tend to find that she takes more risk (e.g. Chakravarty et al., 2011; Agranov et al., 2014; Pollmann et al., 2014; Batteux et al., 2017).⁴² A potential explanation for the former effect might be that the intermediary feels some form of “moral responsibility” towards the resources with which she plays, preventing her from taking too much risk. We shall also consider this behavioral hypothesis in our study since it may have a direct impact on discriminatory behavior. Our study also relates to the experimental literature on discrimination. Some studies find discriminatory behavior while others do not (concerning more particularly trust game environment, see for example Fershtman and Gneezy, 2001; Bouckaert & Dhaene, 2004; Güth et al., 2008; Haile et al., 2008; Hargreaves and Zizzo, 2009; Falk and Zehnder, 2013; and the meta-analyses of Balliet et al., 2014 and Lane, 2016).

Our main results can be summarized as follows: we provide evidence that pure intermediaries (individuals who make decision on behalf of “owners” and whose payoffs do not depend of their decisions) are more prone to discrimination than “owners” (players who make decisions for themselves). The cause of the discrimination we observe is not hostility toward out-group members but is mainly triggered by preferences for in-group members. It seems that because of their position, intermediaries can express their preferences for in-group members more easily than the owners, although they feel responsible for the endowment with which they play. Consistent with the results of the meta-analysis carried out in the first chapter, our data suggest that owners (dealing with personal assets) are more prone to statistical considerations than intermediaries. Finally, we observe gender effects among intermediaries: women invest significantly less than men.

⁴² Strictly speaking, Batteux et al. (2017) show that risk preferences are closer to risk-neutrality when making decisions on behalf of others. More complex effects are found in Pahlke et al. (2015). For an exhaustive survey, see also the recent meta-analysis by Polman and Wu (2019).

The remainder of this chapter is structured as follows. Section 2 presents the experimental design and procedure. Section 3 describes our behavioral predictions. Section 4 reports the results and discuss them while Section 5 concludes the chapter and provides directions for further research. Instructions for the experiment are relegated in Appendix.

2. The experiment

2.1. Experimental design

In this experiment, we aim at investigating whether a pure intermediary is less or more prone to discriminate than the owner in the trust game. To do so, we require the presence of group identities in the laboratory. We have decided to create the identity artificially in the lab because discrimination has been shown to be stronger when identity is artificially induced in the laboratory than when the subject pool is divided for instance by ethnicity or nationality (Lane, 2016). It was essential for a strong group identity to be created in order to compare the discriminatory behavior when a participant play with his own money with the discriminatory behavior when a participant play with another person's money.

When designing our experiment, we wanted to ensure that identity would be salient enough to matter.⁴³ Many experimental studies have addressed the salience of group identity in experimental designs relying on induced identity, such as group colors (e.g. Eckel and Grossman, 2005; Charness et al., 2007; Chen and Li, 2009; Chen et al. 2014; Currarini and Mengel, 2016). In particular, it is often argued that a minimal group

⁴³ A group is commonly defined as salient when both members and non-members of the group recognize the existence of the group.

paradigm setup is rarely enough for participants to be significantly affected by the existence of group identity. For that reason, we have decided to introduce a quiz task at the beginning of the experiment. Implementing social interactions beforehand to raise identity salience has successfully been done before (see Chen and Chen, 2011).

Each session has twelve subjects. In all treatments, subjects are separated in two groups of six. In our identity treatments, one group is randomly attributed the Red color and the other group the Blue color. Then each group participates in a cooperative quiz aiming at generating social links. A variety of questions is asked in order to ensure that everybody can contribute to the group effort. A chat window allows the subjects to discuss and help each other. In order to avoid the formation of negative social links, questions are rather easy ones. Every correct answer is rewarded by a €0.20 payoff to each member of the group.

Then subjects are matched in three-player groups and play a game derived from the standard Berg et al.'s (1995) trust game. One subject takes the role of owner (the standard 'sender' or 'proposer' in the literature), another subject takes the role of intermediary, and the last subject takes the role of recipient. The owner and the recipient are endowed with 10 euros. The intermediary is endowed with 15 euros.⁴⁴

The owner and the intermediary first decide simultaneously how much they want to send to the recipient, knowing that the recipient will get three times the amount sent, and that the recipient will then have the opportunity to return any amount from this earning to the owner. The intermediary plays with the owners' endowment (10 euros). Then,

⁴⁴ We chose an endowment of 15 for the intermediary because we did not want to penalize the latter compared to other players on the final payoff (indeed the average payoff of this central part of the experience is 15 euros, see Johnson and Mislin, 2011) but also and above all because choosing the same endowment as the other subjects (10 euros) could have created a problem of disadvantageous inequity aversion. Indeed, the intermediary cannot influence / increase her gain during this experiment while the other participants can do it. This inequality in possibilities might have induced the intermediary to refuse to send anything in order to reduce her disadvantageous position compared to the other participants.

either the decision of the owner or the decision of the intermediary is selected randomly for implementation, with equal probabilities, and the recipient receives the corresponding amount. We use this procedure in order to avoid informing the recipient of the role (owner or intermediary) of the sender. Indeed, as shown by Kvaløy and Luzuriaga (2014), recipients tend to return less to the owners when decisions are made by intermediaries rather than by the owners ourselves. When designing our experiment, we have prioritized our ability to answer properly our research question over realism. Our main purpose is to investigate whether owners and intermediaries discriminate to the same extent and through the same mechanisms (preferences and/or expectations). Informing the recipient about the source of the donation could have had an undesirable effect on the intermediary's decision. Indeed, the intermediary may anticipate that the recipient would not display reciprocity to the owner if she learns that the owner is not the one who trusted her. This difference in the decision environment of the owner and the intermediary would prevent us to properly isolate the role of "pure intermediary", as we intend. By concealing the source of the donation to the recipient, we ensure that owner and intermediary face the same sending decision, with the sole exception that owners use their own endowment whereas intermediaries use the endowment of the owner.

In the Baseline treatment, group identity is not introduced to participants. Subjects take part in the cooperative quiz, but no mention is done of group color at any point in the experiment.⁴⁵ In the IDentity-Informed Recipient treatment (IDIR), identity groups are common knowledge within the three-player groups. In the IDentity-Uninformed Recipient treatment (IDUR), only the owner and the intermediary are informed of the

⁴⁵ Indeed, if a quiz is introduced in the Identity treatments, we must also introduce it in the baseline treatment, to avoid confounding effects: participants may be more likely to exhibit trust or being generous after cooperating in the quiz game. If we had omitted the quiz in the baseline treatment, one could have argued that observed difference between baseline and identity treatments could be driven by the mere presence of the quiz (and the payment thereof), rather than the introduction of identity.

other players' identity. The recipient is not, and they know it. In the IDIR and IDUR treatments, we only consider situations in which the owner and the intermediary have the same identity (either both from the Blue group or both from the Red group). In half of the cases, the recipient has a different identity from the senders ("out-group" condition) and in the other half, she has the same identity as the senders ("in-group" condition).

Our three treatments are summarized in table 1.

We created these two different identity treatments to highlight the impact of the recipient's level of information on the senders' offer, the recipient's returned amount, and also to identify the nature of the discrimination we could potentially observe. Indeed, as discussed in the previous section, discrimination can be either taste-based or statistical (Becker, 1957; Phelps, 1972).

Table 1: Summary of the three treatments

	Sender (Owner and Intermediary)	Recipient
No identity (Baseline)	No group identity.	Does not know the role of the Sender. No group identity.
IDentity – Uninformed recipient (IDUR)	Knows the group of the recipient. Knows that the recipient does not know his/her group.	Does not know the role of the Sender. Does not know the group of the Sender.
IDentity – Informed recipient (IDIR)	Knows the group of the recipient. Knows that the recipient knows his/her group.	Does not know the role of the Sender. Knows the group of the Sender.

In the IDUR treatment, the recipient is not informed of the identity group of her counterparts. This treatment leaves only little room for statistical discrimination, as owner and intermediary have no legitimate reason to expect a different degree of

trustworthiness between in-group and out-group members. If discrimination is observed in this treatment, it may be due to preferences or other-regarding concerns for in-group members. It is unlikely driven by a belief that the amount returned will differ on average between in-group and out-group recipients. In contrast, in the IDIR treatment, the recipient is informed of the identity group of her counterparts. If one believes that reciprocity will be stronger from in-group members (belonging to the same group may indeed foster cooperation and trustworthiness), then there are legitimate reasons to discriminate between in-group and out-group members. This discrimination would be statistical, as it is driven by expectation in the absence of information rather than preferences. Some discrimination could still be based on preferences in the IDIR treatment, but any treatment difference observed between IDUR and IDIR may be interpreted as statistical discrimination.

To summarize, statistical discrimination can only occur in the IDIR treatment and is due to a lack of information regarding the recipient's reaction to the sender's group identity. Senders have to base their decision on their beliefs regarding this reaction. A natural belief is that in-group recipients will return more than out-group recipients.

2.2. Experimental procedures

A total of 168 students participated in the laboratory experiment in November 2017 and March 2018. All participants were drawn at random from a pool of 700 undergraduate French students from all disciplines of the university. Subjects were recruited thanks to public announcement in classes and ads at the campus.

14 sessions of 12 subjects were run (2 Baseline, 6 IDIR, 6 IDUR) in between-subjects design. The experiment was conducted and programmed with the software oTree (Chen et al., 2016). At the beginning of the experiment, subjects were randomly attributed a place in the lab. All interactions were fully anonymous. Subjects were given a copy of

the instructions (see Appendix). An experimenter read aloud the instructions to ensure common knowledge and informed the participants that before starting the experiment, they would be asked to answer a questionnaire to verify their understanding of the instructions. Then, the experiment started: first the quiz, and second the trust game. The games were played only once. At the end of the experiment, we asked the participants to provide a few explanations for their decisions and to answer a socio-demographic questionnaire. Finally, subjects received individually the total payment earned during the two games in cash. The average duration of an experimental session was about one hour. The average earning was €21.13 Euros, to which we added a €3 show-up fee.

3. Behavioral predictions

If we consider the “standard” economic model involving purely self-interested subjects, recipients will not return anything neither in Baseline nor in ID treatments in order to maximize their payoff. By anticipation, owners will not send anything. As intermediaries have a fixed payoff, they will be indifferent to the amount sent and returned. Thus, this model predicts no treatment effect among players. Moreover, according to this model, senders do not discriminate.

Nevertheless, typical results show a significant proportion of trust and trustworthiness of the players (Johnson and Mislin, 2011), indicating that many subjects have other regarding preferences such as altruism, fairness, efficiency and reciprocity concerns. We must therefore consider social preferences in our predictions.

First of all, let us start briefly with the recipient’s behavior. In all treatments, the recipient may return a certain amount if she shows reciprocity (Rabin, 1993; McCabe et

al., 2003; Dufwenberg and Kirschsteiger, 2004; Falk and Fischbacher, 2006) and/or inequity aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). In IDIR, the recipient will return more to an owner of her own group than to an owner of the other group if she shows taste-based discrimination.⁴⁶ For a detailed analysis of the recipient's behavior in the presence of an intermediary, see Kvaløy and Luzuriaga (2014).

We will mainly focus on the sender's behavior and compare the behavior of the intermediary and that of the owner. Clearly the trust game involves some risk for the owner since his payoff depends on the behavior of the recipient. The intermediary is hence assigned the responsibility of taking risk with the owner's money.

We consider the possibility of "moral responsibility" from the intermediary towards the owner. The intermediary feels responsible towards the owner if she is not willing to take more risk with the owner's money than she would do with her own⁴⁷. This may be related to the guilt aversion described by Charness and Dufwenberg (2006): if the recipient does not return anything, the intermediary may feel guilty that the owner is wronged by her fault. Anticipating this, the intermediary does not send anything or at least less than she would if she was only responsible for herself. Even though this type of norm of behavior has been observed in some studies (see among others Charness and Jackson 2009; Reynolds et al. 2009; Eriksen and Kvaløy 2010, Füllbrunn and Luhan, 2015), other articles have failed to confirm it (see for example Chakravarty et al. 2011; Agranov et al. 2014; Pollmann et al., 2014; Batteux et al., 2017). A recent meta-analysis by Polman and Wu (2019) on 71 articles concludes in a "risky shift" when people play for others, which is consistent with a relatively low level of responsibility even though the effect size

⁴⁶ Recall that in IDUR, the recipient is uninformed on the sender's identity, implying that she cannot discriminate.

⁴⁷ See Charness (2000) and Charness and Jackson (2009) for a detailed discussion about "responsibility alleviation". This theory suggests that an agent who bears the responsibility for another person's welfare will behave in a more 'pro-social' manner, which can result in conservative risk taking.

is small and large heterogeneity is observed. Since in addition the low responsibility assumption is closer to the standard “homo economicus” benchmark, this is the assumption we take into account in our hypotheses. However, we keep discussing the alternative hypothesis throughout the chapter.

Consider first the Baseline treatment. The intermediary is expected to send more than the owner: since the intermediary's payoff is fixed, his betrayal aversion is expectedly reduced or even nil. Moreover, assume that the owner and the intermediary give the same relative weight to efficiency (i.e., the desire to maximize total payoff) with respect to their own payoff in their utility function. Then, on average, the intermediary would tend to send more than the owner. Indeed, for the intermediary, sending can never have a negative impact on his own payoff, whereas for the owner, sending may have a negative impact on his own payoff in particular if the expected return is low or if the preference for efficiency is insufficient. Therefore, we make the following hypothesis:

***H1:** In the Baseline treatment, given low responsibility feelings and a preference for efficiency, the intermediary is expected to send more than the owner.*

However, the intermediary will be sensitive to the risk of the investment if he feels responsibility towards the owner. In this case, the intermediary's beliefs regarding the recipient behavior will play a crucial role. As the recipient does not know who the sender is, we have no reason to think that an intermediary has different beliefs from the owner about the recipient's return. The intermediary will send less if he feels strong responsibility towards the owner:

Next let us move to the identity treatments IDIR and IDUR. To disentangle between the notions of favoritism and discrimination/prejudice, we have chosen to directly compare sending decisions between the baseline and the identity treatments. Through this approach, out-group discrimination would correspond to lower amounts sent to out-group members compared to a situation where no identity is present. In-group favoritism

would correspond to higher amount sent to in-group members compared to a situation where no identity is present. Favoritism/prejudice refer to the social identity theory developed by Turner and Tajfel (1986). They argued that the reason why we favor our in-group over the out-group is that group membership is vital to our self-esteem. According to the theory, our identity is derived from the groups we belong to, and we feel good about ourselves if we can maximize the status, prestige and success of the groups we identify with.

Consider first the IDUR treatment, in which it is common knowledge that recipients are uninformed of the group identity of senders (whereas the senders know the identity of the recipient). Senders' offers may be affected both by their preferences for in-group and out-group recipients and by their beliefs on the behavior of recipients. Let us assume for simplicity's sake that senders believe that in-group and out-group recipients will behave (reciprocate) similarly. This implies that there cannot be any statistical discrimination in IDUR. Then, the extent of discrimination will mostly depend on *preferences* for in-group vs. out-group members (and not on *beliefs* on their behavior), which pertains to taste-based discrimination. Hence, given that the intermediary does not take personal risk, and if she shows low responsibility feelings towards the owner, she can afford to favor/disfavor (based only on her preferences) more than the owner. Then, we can make the following hypothesis:

H2: *In the IDUR treatment, we expect that given low responsibility feelings towards the owner, the intermediary will discriminate on average more than the owner. More precisely, we expect for the intermediary relative to the owner:*

H2a: more in-group favoritism,

H2b: more out-group prejudice.

However, if the intermediary feels sufficiently responsible for the owner, she will be willing to avoid taking too much risk with the owner's money. This will lead her to mitigate the

prejudice/favoritism she would have been willing to implement in the absence of such responsibility feelings.

Finally let us consider treatment IDIR, in which all group identities are common knowledge. As noted previously, statistical discrimination is unlikely to be observed in the IDUR treatment. This is because owners and intermediaries have no reason to expect a different degree of trustworthiness between in-group and out-group recipients who are uninformed of the sender's group identity. In contrast, in treatment IDIR, senders may expect the recipients to react to this identity, although they are uncertain about this reaction. They have therefore to rely on their beliefs on the recipients' behavior to make their decisions. This may alter the amounts sent with respect to the IDUR treatment, not because of a change in preferences but for strategic reasons, in line with the concept of statistical *discrimination*. Hence, any treatment difference observed between IDUR and IDIR may be interpreted as statistical discrimination.

We assume that senders believe that informed in-group recipients tend to be more reciprocal than uninformed recipients (and a fortiori than informed out-group recipients). In the same way, we assume that senders believe that informed out-group recipients tend to be less reciprocal than uninformed recipients (and a fortiori than informed in-group recipients). Given that the intermediary does not take personal risk as opposed to the owner, she should be less concerned by the response of the recipient than the owner in the IDIR treatment if she shows low responsibility feelings towards the owner. Therefore, we expect less statistical discrimination from the intermediary than from the owner. Given that the difference in behaviors between IDIR and IDUR can only be interpreted by statistical discrimination, there should be less difference in behavior between IDUR and IDIR for an intermediary than for an owner. Thus we can formulate the following hypothesis:

H3: *If the intermediary feels weakly responsible for the owner, she will typically show less statistical discrimination than the owner. Therefore, we expect less difference in discrimination between IDIR and IDUR for the intermediary than for the owner. More precisely, we expect for the intermediary relative to the owner:*

H3a: less difference in in-group favoritism between IDIR and IDUR (or equivalently, the difference in in-group offers between IDIR and IDUR is lower for the intermediary than for the owner),

H3b: less difference in out-group prejudice between IDIR and IDUR (or equivalently, the difference in out-group offers between IDIR and IDUR is lower for the intermediary than for the owner).

However, if the intermediary feels sufficiently « responsible » for the owner, she may take into account the response of the receiver as much or even more than if she invested her own money, thus she may discriminate *statistically* as much or even more than the owner.

4. Experimental Results

We first investigate how intermediaries and owners may differ in their sending decision, in the absence of identity groups. We then explore how intermediaries and owners react to the identity group of recipients. Finally, we examine the returning decisions of recipients.

As stated in the previous section, our analyses of discrimination are declined in “in-group favoritism” on the one hand and “out-group prejudice” on the other hand. Another more standard approach could have been to directly investigate the difference between the amounts sent to in-group members and to out-group members. Interested readers can

find in the Appendix a brief description of our findings when we directly compare behavior towards in-group members and out-group members.

To support our results, we perform Wilcoxon Mann-Whitney (henceforth WMW) at the independent observation level. Note however that the sending decision is the product of multiple determinants. The econometric analysis may therefore be more adapted to test the hypotheses. We also exploit the post-experimental questionnaire to support our interpretations.

4.1 Intermediation and trust

Table 2: Average offers across roles and treatments

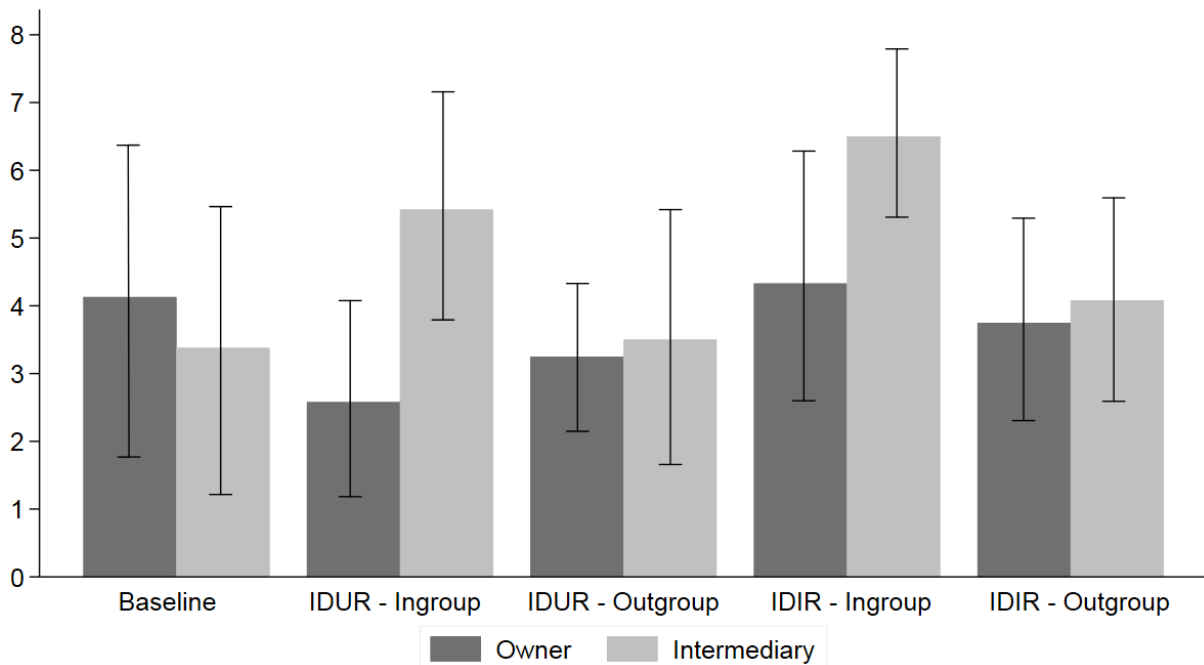
	Owner	Intermediary
Baseline	4.13 (3.39)	3.38 (3.37)
IDUR	2.92 (2.47)	4.46 (3.45)
In-group	2.58 (2.84)	5.42 (3.08)
Out-group	3.25 (2.09)	3.50 (3.65)
IDIR	4.04 (3.30)	5.29 (3.07)
In-group	4.33 (3.70)	6.50 (2.74)
Out-group	3.75 (2.99)	4.08 (3.00)

Note: standard deviations are reported in parentheses. Offers are expressed in euros.

We first focus on the baseline treatment, where no identity is introduced. Table 2 and Figure 1 reports descriptive statistics regarding the average amount of money sent by role and by treatment. Our data report that owners send on average €4.13, i.e. slightly above 40% of their endowment. This figure is in line with previous experimental findings (e.g. the meta-analysis performed by Johnson and Mislin, 2011, suggests that the average proportion sent in the standard trust game in France is 43% of the endowment).

Hypothesis H1 states that given low responsibility feeling and a preference for efficiency, intermediaries are expected to send more than owners. On average, participants in the role of intermediary send €3.38. Although lower than the average trust displayed by owners (€4.13), the difference between intermediaries and owners is not statistically significant (WMW, $p = 0.71^{48}$). This finding contrasts with hypothesis H1, which may suggest that participants in the role of intermediary demonstrate some form of moral responsibility for the owner's money. Responses to the post-experimental questionnaire appear to be consistent with this interpretation: a large majority of intermediaries justify low donation by the fact that they did not really trust the recipient to return something to the owner and justify high donation by the fact that they trusted that the recipient would be trustworthy. This indicates that intermediaries are indeed worried about the risk taken with the owner's money and feel responsible for it. Hence, we get the following result:

Figure 1: Average offers across roles and treatments



Note: Exact figures are reported in table 2. 10% confidence intervals are displayed.

⁴⁸ All tests are two-sided.

Result 1: In the absence of identity groups, owners and intermediary send similar amounts to the recipient.

This finding is consistent with Kvaløy and Luzuriaga (2014) according to which senders who take decision with other people’s money do not behave differently from senders who manage their own money in the trust game. However, our study provides an extension of this result in a situation where recipients are unaware of whether the sender is an owner or an intermediary. In their experiment, recipients are aware about the role of the sender and return significantly less when they know that the decision was made by an intermediary (see Dufwenberg and Kirchsteiger, 2004 and Falk and Fischbacher, 2006 for a discussion regarding the power of intention). Thus it was possible that an anticipation of lower reciprocity from recipients induced intermediaries to invest less. In our setting, in contrast, there is no reason to think that an intermediary has different belief from the owner regarding the recipient’s return.

4.2 Intermediation and discrimination

Table 2 reports no out-group discrimination from either the owner or the intermediary. Senders send as much in the Baseline treatment (where groups are absent) than to an out-group recipient. This finding is observed in both environments where the recipient is not informed on the identity of the sender (IDUR treatment, WMW, owners: $p = 0.31$; intermediaries: $p = 0.38$) and environments where the recipient receives this information (IDIR treatment, WMW, owners: $p = 0.89$; intermediaries: $p = 0.21$). The average amount sent to out-group recipients in the IDUR treatment being similar to the

amount sent in the Baseline treatment implies that senders do not exhibit a distaste towards the opposite group. Hypothesis H2b is therefore rejected.

When focusing on the amount sent to out-group recipients, informing the recipient of the sender's group does not appear to have any effect on donations. The owner sends on average €3.25 in IDUR treatment and €3.75 in IDIR treatment (WMW, $p = 0.63$). The intermediary sends on average €3.50 in IDUR treatment and €4.08 in IDIR treatment (WMW, $p = 0.45$). This suggests that senders, regardless of their role, do not believe that an out-group recipient would discriminate in their decision to return. We will discuss the returning decision in subsection 4.3. Hypothesis H3b is thus rejected.

It is when comparing the amounts sent to in-group members to amounts sent in the Baseline treatment that we observe the effect of including identity in our trust game. While owners do not show in-group favoritism in the IDUR treatment (WMW, $p = 0.26$) or the IDIR treatment (WMW, $p = 0.97$), intermediaries tend to favor in-group recipients in their decision to invest the owner's money. In the Baseline treatment, intermediaries send on average €3.38. This amount increases to €5.42 in the IDUR treatment (WMW, $p = 0.19$) and to €6.50 in the IDIR treatment (WMW, $p = 0.06$).

Considering all treatments, we observe some gender differences in trust, only in the role of intermediary. Female intermediaries send on average €3.77, whereas male intermediaries send on average €5.43 (WMW, $p = 0.07$). There are no gender differences in the sending decision of owners. female owners send on average €3.08, whereas male owners send on average €3.97 (WMW, $p = 0.31$).

Simple non-parametric tests may not perform well in the context our data analysis, as the decision to return must be comprised between 0 and 10 and therefore results in censored data.⁴⁹ We therefore perform a Tobit model to capture this issue and to control

⁴⁹ Overall in our data, around 17% of sending decisions take the minimum value of 0 and around 14% of sending decisions take the maximum value of 10.

for demographics (age and gender of the decision maker). Table 3 reports those Tobit estimates. Those regressions confirm our previous results. No out-group discrimination appears from neither the owner, nor the intermediary. In contrast, the intermediary tends to favor in-group members, while the owner does not favor in-group members on average. We even observe that an in-group member would receive less from an owner compared to a setting where no identity is included (Baseline treatment). In a general way, it appears that the simple introduction of identity groups in the owner's environment (Baseline vs. identity treatments) affects his sending decision. As we will discuss later, not only the trust of the owner is affected by the presence of identity groups, but the trustworthiness of the recipient too.

Result 2: Intermediaries show more in-group favoritism than owners, whereas no out-group discrimination is displayed from neither the owner, nor the intermediary.

This result supports hypothesis H2a, but discards hypothesis H2b. Furthermore, the recipient knowing the identity group of the sender does not affect significantly the amount sent by the intermediary. Indeed, the coefficients associated to an in-group recipient are not significantly different between the IDUR and the IDIR treatments (linear combination test, $p = 0.33$). This suggests that intermediaries have indeed preferences for their own group, making this favoritism taste-based rather than statistical. Intermediaries would therefore seek to maximize the overall in-group gain by investing the owner's money.

The fact that owners do not send more to in-group member does not mean that identity did not influence their choices. Indeed, there is no reason to believe that the feeling of belonging to a group is stronger for players who play with other people's money than for those who play with their own. However, we believe that the fact that owners are dealing with personal assets does not allow them to favor members of their group as much as intermediaries, as they have their own interest at stake. This is largely different for

intermediaries, who can express their preferences for in-group members much more easily, as they do not risk any monetary loss.

Table 3: Tobit estimates of the effects of treatment and identity of the recipient on the amount sent

	Offer from the owner	Offer from the intermediary
Baseline	<i>Ref.</i>	<i>Ref.</i>
IDUR		
In-group	-1.944** (0.953)	3.598*** (0.839)
Out-group	-0.832 (0.925)	0.710 (1.396)
IDIR		
In-group	0.211 (1.114)	4.740*** (0.839)
Out-group	-0.832 (0.925)	1.405 (1.771)
Age	0.012 (0.089)	-0.157 (0.110)
Female	-1.124 (1.184)	-2.282** (0.927)
Intercept	4.132** (2.034)	3.375*** (0.476)
# Observations	56	56

Note: The Tobit model is performed on a support ranging from 0 euros to 10 euros

Standard errors are clustered at the session level

*Significance level: * 10%, ** 5%, *** 1%*

It may be surprising that we find a large amount of in-group favoritism for intermediaries but no out-group discrimination. It should be noticed however that the two effects have no reason to be symmetrical; a priori, the willingness to increase the welfare of in-group members does not necessarily imply the willingness to decrease the welfare of out-group members. Furthermore, the absence of out-group discrimination might be caused by the design features we chose to generate identity in the lab. While the cooperative quiz is

likely to give rise to positive feelings among group members, it may be insufficient to create resentment against out-group members. We did not make the groups compete during the quiz. Apart from the presence of group colors, nothing else separates a recipient from the baseline from an out-group recipient. Apparently, this “minimal group” identity (Tajfel, 1974) is not enough in our case to induce discriminatory behavior toward out-group members.

The fact that hypothesis H2a (*higher in-group favoritism for intermediary compared to owners in IDUR*) is confirmed whereas H1 (*intermediary send more than owners in the baseline*) might appear somehow contradictory. Indeed, rejecting H1 suggests that the intermediary feels rather strong responsibility towards the owner, whereas H2a would be more consistent with lower responsibility feelings. An explanation could be that the moral responsibility of the intermediary extends to the whole group (including the in-group recipient) in the IDUR treatment, whereas it is restricted to the owner only in the Baseline. Hence, it would not be that the intermediary feels less responsible for the owner in the IDUR treatment, but rather that he cares more for the recipient (so that he cares *relatively* less for the owner).

Informing the recipient could induce statistical discrimination. Indeed, in IDIR, senders could expect more reciprocity from in-group recipients and less reciprocity from out-group recipients. Hypotheses H3 state that informing the recipient of the sender’s identity group should have a stronger impact on the owner’s decision compared to the intermediary’s decision. The reason for that is that owners, potentially more concerned about the investment of their own money, would be more inclined toward statistical discrimination. In other words, the strategic considerations associated to revealing the sender’s identity may trigger for owners a larger increase in the amount sent to in-group recipients (Hypothesis H3a) and a larger decrease in the amount sent to out-group recipients (Hypothesis H3b).

Our findings are in line with hypothesis H3a. While the amount sent by intermediaries to in-group members only slightly increases with the introduction of information (from €5.42 to €6.50 on average), the amount sent by owners increases much more (from €2.58 to €4.33 on average). Estimates reported in Table 3 tend to support these results; while we observe that in-group members receive less in the IDUR treatment than recipients in the baseline treatment, this penalty disappears in the IDIR treatment, where information is introduced to the recipient. This finding suggests that owners show more statistical considerations than intermediaries when dealing with an in-group recipient. We do not however observe any significant impact of the information on the amounts sent to out-group recipients by either the owner or the intermediary. Hypothesis H3b is therefore rejected.

Result 3: Introducing information about the sender’s identity group leads more the owner than the intermediary to send more to in-group members. It does not affect the amount sent to out-group recipients.

Altogether, those findings suggest that owners and intermediaries approach identity groups in different manners. Intermediaries’ decisions to favor in-group members appear to be motivated mainly by in-group preferences, whereas owners may be more affected by statistical considerations.

Finally, we also observe some gender differences in the intermediary behavior. Table 3 reports that female intermediaries invest on average a lower amount of the owner’s money than males. This finding indicates that women in the role of intermediary may be more risk averse than men, potentially because they have a stronger feeling of responsibility toward the owner’s money.

4.3 Returning decisions

We now focus on the returning decisions of recipients. Table 4 reports the proportions of the received amount returned by the recipient to the owner. We do observe that recipients send back a significantly larger proportion of the amount received in the Baseline treatment, where identity is absent, compared to both identity treatments (baseline vs. IDUR, $p = 0.02$; baseline vs. IDIR, $p < 0.01$). As we have already seen for the owner, it appears that entering an environment where identity groups are present affects the behavior of decision makers; trustworthiness is lower in identity conditions.

Table 4: Proportion of the received amount returned by the recipient to the owner

Baseline	0.436 (0.065)
IDUR	0.231 (0.182)
IDIR	0.215 (0.171)
In-group	0.214 (0.165)
Out-group	0.216 (0.185)

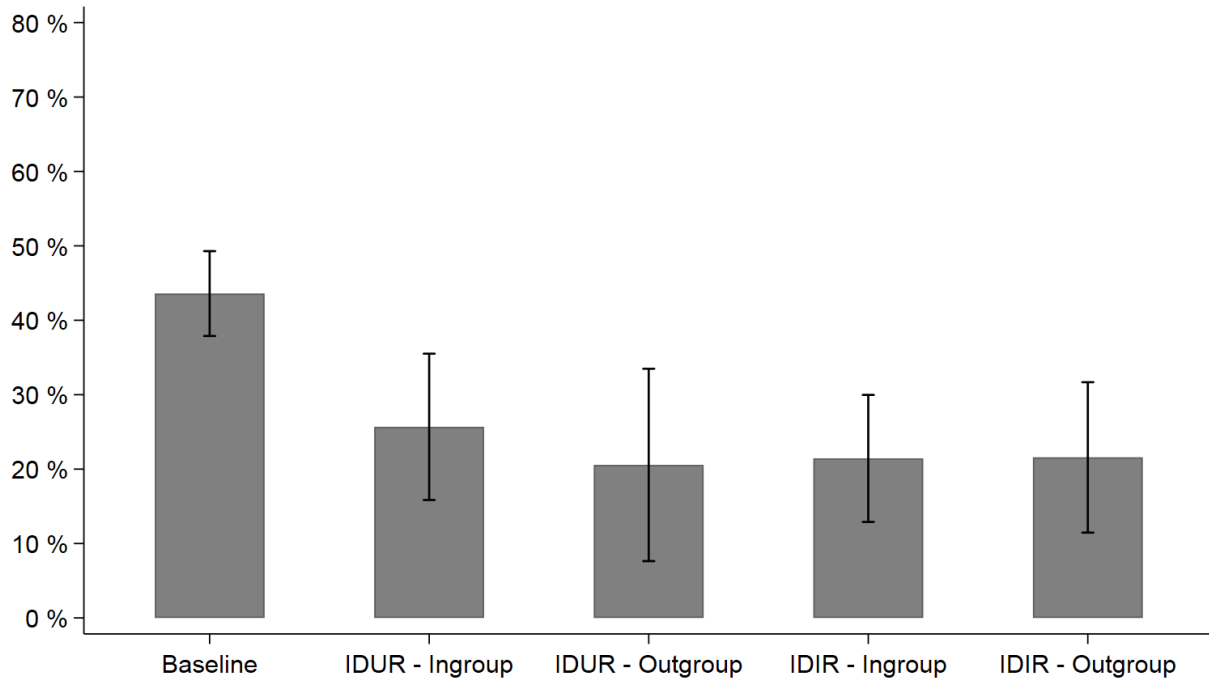
Note: standard deviations are presented in parentheses.

Instances where the recipient did not receive anything are removed from the subsample.

Contrasting with our expectation, we do not observe any difference between the amount sent to an in-group sender and the amount sent to an out-group sender. It appears that recipients do not discriminate in our experiment, and all statistical considerations for identity groups in the role of sender is therefore irrelevant. A graphical representation of those findings is reported in figure 2. Finally, we do not observe any gender difference in trustworthiness. Female recipients send back on average 27.40% of the received amount,

whereas men recipients send back on average 22.37% of the received amount (WMW, $p = 0.32$).

Figure 2: Average proportion of the received amount returned



Note: Exact figures are reported in table 4. 10% confidence intervals are displayed.

5. Conclusion

Recent empirical studies have shown that intermediaries are less prone to discriminate than the party directly involved (e.g. Ahmed and Hammarstedt, 2008; Bunel et al., 2017; Le Gallo et al., 2018; Bosch et al., 2010; Bosch et al., 2015). There are important implications for society: it seems that the presence of intermediaries could help to fight against discriminations and all the economic and social inefficiencies they imply. However, there are many factors that cannot really be controlled in field experiments; hence the causes of this behavior are still not well defined. Does it really stem from the mere presence of an intermediary or rather from factors specific to the tested markets?

It may also be that intermediaries (often professionals) are more prone to detect that they are being tested than owners, resulting in an underestimation of their true discriminatory behavior. Efficient policy making therefore requires a good understanding of the reasons leading to this behavior.

By using an original three-player's variant of the investment game (Berg et al., 1995) with group identities, we provide evidence that pure intermediaries (individuals who make decision on behalf of "owners" and for which own payoff does not depend of their decision) are on the contrary more prone to discrimination than "owners" (players who make decisions for themselves). However, the cause of discrimination we observe is not hostility toward out-group members; discrimination is mainly triggered by preferences for in-group members. It seems that because of their position, intermediaries can express their preferences for in-group members more easily than the owners, although they feel responsible for the wealth with which they play. Consistent with the meta-analysis of Flage (2018), our data also suggest that owners (dealing with personal assets) are more prone to statistical considerations than intermediaries. Finally, we observe a gender effect among intermediaries: women send significantly less than men. Indeed, higher empathy from women (see Mestre et al., 2009, Toussaint and Webb, 2005, Macaskill et al., 2002, Gault and Sabini, 2000, Lennon and Eisenberg, 1987) could bring a greater sense of responsibility towards the owner's endowment. These results complement the experimental literature on delegation and discrimination.

To summarize, our results seem to indicate that the smaller discriminatory behavior of intermediaries that is observed empirically is not a pure effect of intermediation. Hence, factors present in reality but not in the laboratory are likely to drive this phenomenon (for example, the fact that professionals have a better level of information on minority characteristics, face harsher penal or civil restrictions, or have stronger reputation concerns). Thus, it will be informative for future researchers to examine the effect of these factors in the laboratory.

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Appendix - Experimental Instructions

INSTRUCTIONS

*Note: These are the instructions (translated in English) for the Baseline treatment. For the Identity treatments (IDUR-IDIR), a few sentences have been inserted in italics into brackets [*** ... ***].*

You will participate in an experiment whose objective is to study individual decision-making. You will have to make a series of decisions that will be explained in the following. These instructions are simple. If you follow them carefully, you may earn a significant amount of money.

Your earnings will depend on your decisions and in some situations, on other participants' decisions. Therefore, it is very important to read these instructions with attention. Your final payoff will be equal to the sum of your payoffs in each part of this experiment, plus a show-up fee of 3 euros.

All your answers will be processed anonymously (with respect to the other participants and to the experimenters). We insist on the fact that communication among participants is forbidden. If you do not comply with this rule, you may be excluded from the experiment and earn nothing.

*[*** Treatments IDUR-IDIR—Before starting the experiment, please open the envelop which is on your table. In that envelop is a colored card: red or blue. That color represents the group of 6 participants with which you will interact in part 1 of the experiment. You will stay in that group (the red group or the blue group) for the whole experiment. ***]*

Part 1.

In this game, you will collaborate with five other players in order to answer to general culture questions. *[*** Treatments IDUR-IDIR—The six of you are all in the same color group (red or blue group) ***]*. For each correct answer your team will give, **each group member will earn €0.20** (including you). This first part will last 15 minutes.

Here is a screen shot example of the software that you will use:

Question 1

Temps restant avant la fin de la phase 1: ⌚ 1:43

Merci de répondre à la question?
 Dans la mythologie, combien de travaux Hercule a-t-il accompli?

Suivant

Player 2 (Me)
Je ne sais pas, quelqu'un à la réponse?

Send

[translation]

Remaining time for phase 1

Please answer the question:

“In mythology, how many labors did Hercules accomplish?”

Next

Player 2 (Me): “I don’t know, who knows the answer?”

The question is displayed in the middle of the screen. You will have to type your answer on keyboard and then click on “Next” [“Suivant”] in order to enter it. If your answer is correct, you will be notified of it and then move to the next question. If your answer is incorrect, an error message will be displayed, and then you will have the opportunity to enter another answer. There is no penalty for wrong answers, so that you may propose as many answers as you wish.

At the bottom of the screen, a chat window will allow you to discuss with the five other members of your group. The messages will only be readable by your group members. *[*** Treatments IDUR-IDIR—Hence, blue group members will only see the messages from blue group members. Red group members will only see the messages from red group members. ***]* A pseudo (player 1, 2, 3, 4 or 5) will be randomly assigned to each of you for this part of the experiment only. You are free in the use of this chat window. For example, you may ask the other group members the answer of the current question. You may also tell them the answer of the question if you know it. The only restriction is that you may not reveal your identity or even give hints on your identity to the other group members. If you do so, you will be excluded from the experiment and will not earn anything.

When the correct answer has been entered, the following screen is displayed:

Question

Votre réponse est correcte !
Lorsque les six participants de votre groupe auront répondu à la question, vous passerez à la question suivante.

Player 2 Je ne sais pas, quelqu'un à la réponse?

C'est 12! Send

[translation]

Your answer is correct!

When the six members of your group will have answered the question, you will move to the next question.

Player 2: "I don't know, does anybody know the answer?"

"It's 12!"

This screen informs you that your answer is correct. As long as the other members of your group have not correctly answered the current question, you will stay on this waiting screen. The chat window remains active, and you can send the other members the correct answer in order to enable them answer the question.

When the six group members have correctly answered the question, everybody moves to the next question. A new question screen similar to the previous one will be displayed, and you will have to find the correct answer again.

Please note that the answers are always numbers between 0 et 9999.

When the 15 minutes of part 1 will be completed, the game will stop automatically. The instructions of part 2 will be distributed to participants.

Your payoff for part 1 of the experiment:

€0.20 x Number of correct answers provided by your group.

Part 2.

In this part of the experiment, you will play with two other participants picked up at random. One of you will be designated as "participant A", another one as "participant B" and the last one as "participant C". Hence, there are three different roles in this part of the experiment. You will be informed of your role at the beginning of part 2.

At the beginning of this part, participants A et C receive 10 euros each, while participant B gets 15 euros. In the first step, only participants A and B make a decision. Participant C makes his decision in a second step. Let us describe first the decision of participant A and then the decision of participants B. Only one of these decisions will be implemented in the experiment, with equal probability.

Participant A may use his/her endowment of 10 euros to send any amount he/she wishes to participant C. The remaining amount is kept by participant A. The amount sent to C is **tripled**.

For instance, if the amount sent to C is of 2 euros, then C receives 6 euros. If the amount sent is of 8 euros, then C receives 24 euros. Participant C may then use the received amount to return any amount he/she wishes to participant A. The returned amount is not tripled.

[*** This screen is for treatment Baseline only: ***]

<div style="border: 1px solid #ccc; padding: 10px;"> <h3 style="margin: 0;">Votre choix</h3> <p style="margin: 10px 0;">Vous êtes le participant A. Vous disposez d'une dotation de 10,00 Euro. Combien souhaitez-vous envoyer au participant C ?</p> <p style="margin: 10px 0;">Veuillez entrer un montant compris entre 0 et 10 :</p> <div style="display: flex; align-items: center; margin: 10px 0;"> <input style="width: 80px; border: 1px solid #ccc;" type="text"/> <div style="border: 1px solid #ccc; padding: 2px 5px; margin-left: 5px;">Euro</div> </div> <div style="display: flex; justify-content: flex-start; margin-top: 10px;"> <div style="background-color: #0056b3; color: white; padding: 5px 15px; border-radius: 3px; cursor: pointer;">Suivant</div> </div> </div>	<p>[translation]</p> <p>YOUR CHOICE</p> <p>You are participant A. You are endowed with 10 euros. How much are you willing to send to participant C?</p> <p>Please enter an amount between 0 and 10:</p>
--	---

[*** Treatments IDUR-IDIR—When participant A makes his/her decision, he/she is informed of the groups (red or blue) of participants B and C.

<div style="border: 1px solid #ccc; padding: 10px;"> <h3 style="margin: 0;">Votre choix</h3> <p style="margin: 10px 0;">Vous êtes le participant A. Vous disposez d'une dotation de 10,00 Euro.</p> <p style="margin: 10px 0;">Le participant B fait partie du groupe ROUGE.</p> <p style="margin: 10px 0;">Le participant C fait partie du groupe BLEU.</p> <p style="margin: 10px 0;">Combien souhaitez-vous envoyer au participant C ?</p> <p style="margin: 10px 0;">Veuillez entrer un montant compris entre 0 et 10 :</p> <div style="display: flex; align-items: center; margin: 10px 0;"> <input style="width: 80px; border: 1px solid #ccc;" type="text"/> <div style="border: 1px solid #ccc; padding: 2px 5px; margin-left: 5px;">Euro</div> </div> <div style="display: flex; justify-content: flex-start; margin-top: 10px;"> <div style="background-color: #0056b3; color: white; padding: 5px 15px; border-radius: 3px; cursor: pointer;">Suivant</div> </div> </div>	<p>[translation]</p> <p>YOUR CHOICE</p> <p>You are participant A. You are endowed with 10 euros.</p> <p>Participant B is in the RED group.</p> <p>Participant C is in the BLUE group.</p> <p>How much are you willing to send to participant C?</p> <p>Please enter an amount between 0 and 10:</p>
--	---

***]

Participant B is placed in a similar situation as participant A, except that **he/she does not use his/her endowment but that of participant A**. Thus, participant B may use participant A's endowment of 10 euros to send any amount to participant C. The remaining amount is kept by participant A. The amount sent to C is **tripled**. Participant C may then use the received amount to return any amount to participant A. The returned amount is not tripled.

[*** This screen is for treatment Baseline only: ***]

<div style="border: 1px solid #ccc; padding: 10px;"> <h2 style="margin: 0;">Votre choix</h2> <p style="margin: 10px 0;">Vous êtes le participant B. Le participant A dispose d'une dotation de 10,00 Euro. Combien souhaitez-vous envoyer au participant C ?</p> <p style="margin: 10px 0;">Veuillez entrer un montant compris entre 0 et 10 :</p> <div style="display: flex; align-items: center; margin: 10px 0;"> <input style="width: 80px; border: 1px solid #ccc;" type="text"/> <div style="border: 1px solid #ccc; padding: 2px 5px; margin-left: 5px;">Euro</div> </div> <div style="text-align: center; margin-top: 10px;"> <div style="background-color: #0056b3; color: white; padding: 5px 15px; border-radius: 3px; cursor: pointer;">Suivant</div> </div> </div>	<p><i>[translation]</i></p> <p>YOUR CHOICE</p> <p><i>You are participant B. Participant A is endowed with 10 euros. How much are you willing to send to participant C?</i></p> <p><i>Please enter an amount between 0 and 10:</i></p>
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*[*** Treatments IDUR-IDIR—When participant B makes his/her decision, he/she is informed of the groups (red or blue) of participants A and C.*

<div style="border: 1px solid #ccc; padding: 10px;"> <h2 style="margin: 0;">Votre choix</h2> <p style="margin: 10px 0;">Vous êtes le participant B. Le participant A dispose d'une dotation de 10,00 Euro.</p> <p style="margin: 10px 0;">Le participant A fait partie du groupe ROUGE.</p> <p style="margin: 10px 0;">Le participant C fait partie du groupe BLEU.</p> <p style="margin: 10px 0;">Combien souhaitez-vous envoyer au participant C ?</p> <p style="margin: 10px 0;">Veuillez entrer un montant compris entre 0 et 10 :</p> <div style="display: flex; align-items: center; margin: 10px 0;"> <input style="width: 80px; border: 1px solid #ccc;" type="text"/> <div style="border: 1px solid #ccc; padding: 2px 5px; margin-left: 5px;">Euro</div> </div> <div style="text-align: center; margin-top: 10px;"> <div style="background-color: #0056b3; color: white; padding: 5px 15px; border-radius: 3px; cursor: pointer;">Suivant</div> </div> </div>	<p><i>[translation]</i></p> <p>YOUR CHOICE</p> <p><i>You are participant B. Participant A is endowed with 10 euros</i></p> <p><i>Participant A is in group RED.</i></p> <p><i>Participant C is in group BLUE.</i></p> <p><i>How much are you willing to send to participant C?</i></p> <p><i>Please enter an amount between 0 and 10:</i></p>
---	--

***]

Once both participants A and B have made their decision, only one of the two decisions is selected at random by the computer to be implemented.

- If participant A's decision is selected, then the amount chosen by participant A is subtracted from participant A's endowment and participant C receives this amount ($\times 3$).
- If participant B's decision is selected, then the amount chosen by participant B is subtracted from participant A's endowment and participant C receives this amount ($\times 3$).

Participant C does not know whether the received amount comes from participant A or from participant B. He/she observes the amount sent and which is multiplied by 3. He/she may then return any amount **to participant A**. This amount lies between 0 and the received amount, multiplied by 3.

*[*** Treatments IDUR—When participant C makes his/her decision, he/she is **not informed** of the groups (red or blue) of participants A and C. ***]*

*[*** This screen is for treatments Baseline and IDUR only: ***]*

Votre choix

Vous êtes le participant C. Le participant A et le participant B ont décidé d'une somme à vous envoyer. Une de ces décisions a été aléatoirement appliquée.

Le participant qui a été aléatoirement sélectionné vous a envoyé 10,00 Euro. Vous recevez donc 30,00 Euro. Combien souhaitez-vous envoyer au participant A ?

Euro

Suivant

[translation]

YOUR CHOICE

You are participant C. Participant A and participant B have decided to send you an amount of money. One of these decisions has randomly been implemented.

The participant who was randomly selected sent you 10 euros. Hence, you receive 30 euros. How much are you willing to return to participant A?

*[*** Treatments IDIR—When participant C makes his/her decision, he/she is informed of the groups (red or blue) of participants A and C.*

Votre choix

Vous êtes le participant C. Le participant A et le participant B ont décidé d'une somme à vous envoyer. Une de ces décisions a été aléatoirement appliquée.

Le participant A fait partie du groupe ■■■■

Le participant B fait partie du groupe ■■■■

Le participant qui a été aléatoirement sélectionné vous a envoyé ■■■■ Euro. Vous recevez donc ■■■■ Euro. Combien souhaitez-vous envoyer au participant A ?

Euro

Suivant

[translation]

YOUR CHOICE

You are participant C. Participant A and participant B have decided to send you an amount of money. One of these decisions has randomly been implemented.

Participant A is in group ■■■■

Participant B is in group ■■■■

The participant who was randomly selected sent you ■■■■ euros. Hence, you receive ■■■■ euros. How much are you willing to return to participant A?

***]

To summarize, participant B will always have a fixed payoff of 15 euros whereas participants A and C's payoffs depend on other participants' decisions.

Your payoff for part 2 of the experiment:

If you are a participant A:

If your decision is taken into account:

$$€10 - (\text{Amount sent by you}) + (\text{Amount returned by participant C})$$

If participant B's decision is taken into account:

$$€10 - (\text{Amount sent by participant B}) + (\text{Amount returned by participant C})$$

If you are a participant B:

$$15€$$

If are a participant C:

$$€10 + (\text{Amount send by A or B}) \times 3 - (\text{Amount returned to participant A})$$

Post-experimental questionnaire.

The following questions, along with standard questions asking for demographic information (gender, age, level of study), were addressed to the participants at the end of the experiment.

Please rate from 1 (totally disagree) to 10 (absolutely agree) the following affirmations:

- 1- I tend to take risks in my everyday life.
- 2- I tend to trust others in my everyday life.
- 3- I care about other' welfare and I do not act only for myself.

The following questions were addressed to participants in roles A and B. The formulation of the question automatically adapted to (i) the role of the participant and (ii) the decision of the participant to send a positive amount or not. We only display the questions addressed to a participant in role A, who decided to send a positive amount to the recipient.

- 1- What reasons lead you to send a positive amount to player C?
 - a. I trusted player C to send back more than the amount I sent.
 - b. I wanted to increase the profit of player C.
 - c. I wanted to increase our global payoff, as the amount sent was multiplied by 3.
 - d. None of the above (please write down your specific reason).
- 2- Would you have sent another amount to player C if you were in the role of intermediary?
 - a. I would have sent more.
 - b. I would have sent less.
 - c. My choice would not have been different.

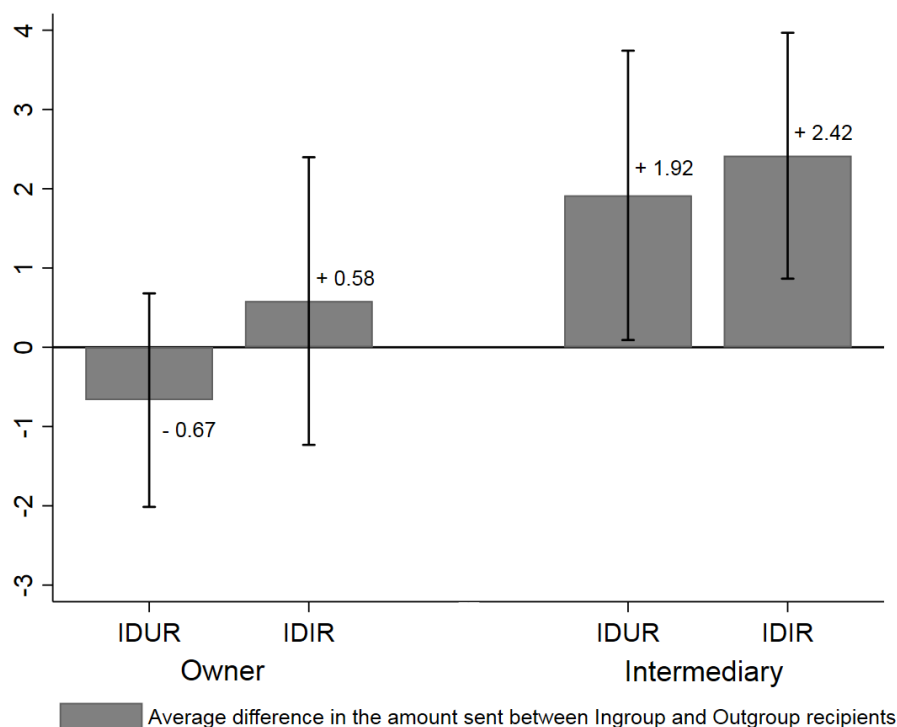
- 3- *Your choice had a fifty percent chance of being implemented. Did this random component affect your decision?*
 - a. *Yes*
 - b. *No*
- 4- Did you feel closer to the members of your group (Red/Blue) than members of the other group?
 - a. *Yes*
 - b. *No*
- 5- How did belonging to a group (Red/Blue) affect your decision
Participants were asked to write a couple of sentences.

The following questions were addressed to participants in role C. The formulation of the question automatically adapted to the decision of the participant to send back a positive amount or not. We only display the questions addressed to a participant in role C, who decided to send a positive amount back to the owner.

- 1- *What reasons lead you to send back a positive amount to player A?*
 - a. *It is normal to send back when one receives something.*
 - b. *I wanted to increase the profit of player A.*
 - c. *I wanted to even the payoffs in the group out.*
 - d. *None of the above (please write down your specific reason).*
- 2- You were not aware of which decision (player A or player B) was randomly implemented. Did this lack of information affect your decision?
 - a. *Yes*
 - b. *No*
- 3- Did you feel closer to the members of your group (Red/Blue) than members of the other group?
 - a. *Yes*
 - b. *No*
- 4- How did belonging to a group (Red/Blue) affect your decision
Participants were asked to write a couple of sentences.

Appendix - Additional figure

Figure A1: Difference between amounts sent to in-group and amounts sent to out-group members



*Note: Reported differences are calculated by deducting figures from table 2.
10% confidence intervals are displayed.*

In this chapter, we identify discrimination by comparing behaviors between the baseline treatment and the identity treatments, according to the role of the sender (owner vs. intermediary) and the information displayed to the recipient. What we define as “favoritism” and “discrimination” are deviations in behavior when identities are displayed, compared to a situation where no identity group exist. An alternative approach to discuss discrimination would have been to explore the difference in the sending decisions when the recipient is an in-group member and the sending decision when the recipient is an out-group member. It would not consider the baseline treatment and it would not allow to disentangle between the notions of favoritism and discrimination. However, it may be useful to report findings from this approach, as it could allow the reader to compare those results with previous and future findings in the

experimental literature addressing the issue. We decide to report this analysis in the Appendix, as it is in most part redundant with the analysis provided in the manuscript.

Figure A1 reports the difference between the amount sent to an in-group recipient and the amount sent to an out-group recipient. We calculate this difference for each role and each treatment (original numbers used to compute this difference are reported in Table 2). The observations we can draw from this figure are in line with the results presented in this chapter. First, owners do not appear to send different amounts to in-group and out-group recipients, neither in the IDUR treatment (WMW, $p = 0.25$) nor in the IDIR treatment (WMW, $p = 0.84$). Intermediaries however appear to send more to in-group members than out-group members. In the IDUR treatment, this difference amounts to €1.92 (WMW, $p = 0.10$), whereas it amounts to €2.42 (WMW, $p = 0.06$) in the IDIR treatment. Thus, it appears that intermediaries do consider the group of the recipient in their sending decision. To understand the mechanisms underlying this discrimination, it is useful to explore whether this difference comes from a discrimination against out-group members or favoritism for in-group members. This is what we do in the results section of the third chapter, by comparing identity treatments to the baseline treatment.

Chapter 4

Discrimination against people with mental, physical or visual disabilities in the rental housing market. A field experiment in France

1. Introduction

During the last years, a large number of field experiments have been conducted in order to detect the presence of discrimination in the rental housing market in OECD countries. One of the most used methods of “testing” in recent years is the correspondence testing approach, which consists in sending emails to real estate agents or private owners to determine whether unequal treatment takes place in the first stage of access to housing between several fictitious profiles of candidates. Most analyzes were conducted to study discrimination by ethnicity (e.g. Carpusor and Loges, 2006; Ahmed and Hammarstedt, 2008; Ahmed et al., 2010; Bosch et al., 2010; Hanson and Hawley, 2011; Hogan and Berry, 2011; Baldini and Federici, 2011; Bunel et al., 2016, Bunel et al., 2017, Le Gallo et al., 2018). Some tests were also implemented to test discrimination based on sexual orientation (e.g. Ahmed and Hammarstedt, 2008, Mazziotta et al., 2015, Levy et al., 2017, Murchie and Pang, 2018, Schwegman, 2018) or by gender (e.g. Ahmed and Hammarstedt, 2008; Andersson et al., 2012; Öblom and Antfolk, 2017). Surprisingly, it is only recently that studies have focused on discrimination based on disability (Heylen et al., 2015, Fumarco, 2017; Verhaegue et al., 2016; Verhaegue et al., 2017). Yet, millions of people are affected by at least one disability in OECD countries (e.g. Krahn, 2011).

People with disabilities are protected against discrimination in the labor and housing markets by law. Indeed, Article 21 of the Charter of Fundamental Rights of the European Union, 2000, states that any discrimination based on disability shall be prohibited. In the facts, many studies have found that people with disabilities face severe discrimination in the labor market (for an overview, see Baert, 2018; Jones, 2008). In France, despite the national law of February 11, 2005 "for equal rights and opportunities, participation and citizenship of people with disabilities", disability is still one of the main source of discrimination in the labor market, sometimes even before ethnic origin (for empirical

evidence, see for example Mbaye, 2018). However, as in many other countries, there is still no quantitative evidence of the presence of discrimination based on disability in the housing market. A quarter of the workforce is nevertheless affected by disability in France (Tableaux de l'économie française, 2011, INSEE) and could be subject to discrimination in the housing market. This issue deserves a lot of attention: discrimination in the rental housing market affects social inclusion, health, job opportunities, education, and availability of public services (Riach and Rich, 2002).

By means of correspondence tests, this chapter provides an estimate of the level of discrimination against people with disabilities by private landlords and real estate agents in the first stage of the rental housing application process in France. In the experiment, five fictitious testers, one valid person (control group), a blind person with a guide dog, a person with a mental disability, a person with a physical disability and a valid person with a dog applied for vacant rental apartments advertised by private landlords or real estate agents. Between March and July 2019, 1,750 applications have been sent in a matched-paired procedure (in response to 875 ads for apartments). In addition to providing the level of discrimination faced by blind people, individuals with mental disabilities and individuals with physical disabilities in the rental housing market, our goal is to disentangle the sources of the discrimination.

Indeed, in order to combat discrimination, it is essential to understand its origins. Discrimination against disabled people could also come from the two sources commonly presented in this thesis. “Taste-based” discrimination (Becker, 1957; Yinger, 1986) occurs when agents have personal hostile attitudes towards people with disabilities (i.e. they prefer to be in the presence of a valid person rather than a disabled person) or comply with the negative attitude of the group of individuals to which they are attached. For example, in the rental housing market, this corresponds to the case where private landlords or real-estate agents discriminate because of their distaste towards disabled people or do not accept individuals with disabilities, so as not to displease their other

“valid” clients. Therefore, this type of discrimination comes from preferences, unlike “statistical” discrimination (Phelps, 1972; Arrow, 1973), which comes from the supposed performance (or “quality”) of candidates. This second type of discrimination could only occur in the presence of a lack of correct information about the candidate. Indeed, in the absence of direct information about an individual’s reliability (e.g. ability to pay the rent, ability to take care of the apartment, and having a civilized behavior with the neighborhood as well as with the owner etc.), a landlord may substitute group averages/variances (either real or imagined/biased by stereotypes) to fill the information void. For example, if disabled people are perceived as being less financially stable on average (first order statistical discrimination), or as stable but with more variance regarding their financial stability (second order statistical discrimination), landlords or real estate agents could prefer to choose a valid candidate.

By using the same control group, we are the first to test and compare the level of discrimination according to three types of disability. We are thus able to determine if one type of disability is more conducive to discrimination than another. Moreover, although one study analyzes the presence of discrimination according to motor impairment (Levy et al., 2015 for USA), we are the first to analyze not only the presence but also the extent of discrimination according to this type⁵⁰. We are also the first to explicitly control for geographical characteristics in our analysis of the discrimination against disabled people in the rental housing market. Finally, to the best of our knowledge, we are the first to study the presence of discrimination based on disability in the rental housing market in France.

⁵⁰ Indeed, this study compares response rates for disabled applicants and valid applicants when the disabled applicant is more qualified for the housing—for example, having a higher income level or credit score—than the valid applicant. The purpose of this study was more to prove the existence of discrimination by highlighting the cases where a less-qualified valid applicant was chosen compared to a more-qualified disabled applicant, and not to calculate the extent of the phenomenon.

In this chapter, we provide evidence that blind people, individuals with mental disabilities and individuals with physical disabilities are discriminated against in the French rental housing market. People with mental disabilities (yet autonomous) face the highest level of direct discrimination in the process of allocation of rentals. They are more than two times less likely to be invited to visit an apartment than a person without disabilities in the French rental housing market. Blind people with a guide dog are almost two times less likely to be invited to visit the apartment than the valid ones; however, we show that this discrimination comes mainly from the presence of the guide dog, not to the disability status. Finally, in addition to strong indirect discrimination regarding the housing supply in the market, people with motor disabilities face direct and significant discrimination during the process of allocation of housing rented, thus making individuals with motor disabilities the minority with the greatest prejudice in the rental housing market in France. Finally, results indicate that real estate agents discriminate significantly less against disabled applicants than private landlords do.

The chapter is organized as follows: Literature review is presented in Section 2; the experimental design of our study is described in Section 3; Section 4 contains the results; our conclusions follow in the last Section.

2. Literature review

To the best of our knowledge, only four scientific studies have examined the level of discrimination based on disability in the rental housing market by means of correspondence test. The authors have often looked at other types of discrimination in their experience (sexual orientation, gender, ethnicity), we present here only what is in relation to disability.

The first correspondence test carried out in order to study the presence of discrimination based on disability was conducted by Heylen et al. (2015) in Belgium between February and April 2013. In this study, the authors used the “single inquiries” (also known as “random assignment”) procedure. In this approach, each landlord or real-estate agent receives only one inquiry from a randomly selected applicant. The main advantage of this type of test is that it is almost impossible for the testing to be detected. However, this method does not control the effect of unobservable fixed variables on the response rate and moreover requires more applications to obtain the same statistical power as the matched-paired procedure. They sent 653 e-mails in order to test the presence of discrimination against blind tenants in the Belgian rental housing market. Disability is signaled in this experiment by a male tenant who asks the landlord whether it is possible to describe the dwelling in detail. They find that the likelihood of getting an appointment was 7.2 per cent point lower than for the male control group.

Almost at the same time, Fumarco (2017) performed a correspondence test in Italy. Between April and June 2013, 1,000 emails were sent in single inquiry procedure to detect once again discrimination against blind tenants, but with the presence of a guide dog this one. The experiment took place as follows: a fictitious normal-sighted male sends an application for him and his wife and indicates whether his wife owned a dog or not and whether this dog was a guide dog. Results indicate that married tenants with a blind wife assisted by a guide dog are 12 per cent less frequently invited by apartment owners, compared to married tenants without dog. Finally, the results seem to suggest that this lower quantity of invitations is due to the presence of the guide dog alone, not to the disability status. Therefore, discrimination against blind tenants seems entirely indirect in the Belgian rental housing market (neither tasted-based nor statistical, i.e. it depends neither on the preferences of the agents nor on the supposed quality of the candidates).

Next, Verhaegue et al. (2016) carried out another correspondence testing to detect the presence of discrimination against blind tenants with a guide dog. This time, authors

used the standard “matched-paired” procedure and thus two similar applications (except for the target characteristic) were sent to the same agent. This procedure makes it possible to detect discrimination for a sole agent as well as between two agents and control for fixed unobservable variable. Moreover, this makes it possible to study absolute discrimination (where one individual is chosen by one agent to the detriment of the other) in addition to relative discrimination. 268 properties were tested on the Belgian rental housing market, more precisely in the city of Ghent. Disability was signaled in this experiment as follows: the test person presented himself as a friend of a visually impaired person. He asked whether the dwelling is still available and whether it is possible for him and his friend with assistance dog to visit the dwelling. The control person makes the same request but only for him. They find that blind candidate with a dog are discriminated by more than one in three lessors. Moreover, they revealed a differential treatment related to the type of agents: real-estate agents discriminated significantly less than private landlords.

Finally, between November 2016 and March 2017 in the Brussels Capital, Verhaegue et al. (2017) proceeded to the last correspondence testing we are aware of in order to measure discrimination against persons with disabilities. They responded to 898 rental ads in a matched-paired procedure to test discrimination against blind tenants with and without a guide dog and to 514 ads to test discrimination against people with mental disabilities. This time, disability was signaled by the father of a disabled person who explains that he is looking for rental housing for his son (mentally handicapped or blind) and would like to visit the apartment with him. Once again, the control person is just a male candidate who would like to visit the apartment too. Results indicate that people with a mental disability and blind candidates with a guide dog are indeed discriminated against by agents but that blind people are discriminated only due to the presence of the dog: the father with a blind son (when no mention of any assistance dog) receives even a little more (not significant) positive responses than the (control) person alone.

There are also two others studies (Levy et al., 2015; Hammel, 2017) from the U.S. Department of Housing and Urban Development (commonly known as ‘HUD’) where e-mails have been sent to real estate agents or private owners for the purpose of testing the presence of discrimination against people with disabilities in the U.S rental housing market, but which are not strictly speaking correspondence testing. Indeed, in these studies, real trained and hired testers (not fictitious) send e-mails and/or phone to owners in matched-paired procedure to detect the presence of discrimination based on motor, hearing (Levy et al., 2015) or mental (Hammel, 2017) impairments. If the first stage is positive (invited to visit the apartment), they also participate to the face-to-face interview. Authors find a significant presence of discrimination for both types of disability. However, these studies tested the presence of discrimination towards disabled applicants when the valid applicants are less qualified for the housing—for example, have a lesser income level or credit score— than the handicapped. The purpose of these two studies was more to prove the existence of discrimination by highlighting the cases where a less-qualified non-handicapped applicant was preferred compared to a more-qualified disabled applicant, and not to calculate the extent of the phenomenon.

3. Experimental design

To test for discrimination based on disability in the rental housing market using the correspondence testing method, we used the largest buy and sell site in France, leboncoin.fr and four other sites specifically dedicated to the housing market (pap.fr; seloger.com; logic-immo.com; avendrealouer.fr). We were forced to use multiple sites because beyond a certain message threshold sent per day for each addresses (we believe 10), our created email addresses were banned and unusable for reasons of spamming. For each site, responding to an ad is free of charge and if someone is interested in an

apartment, he/she can easily send a message to the publisher of the ad through an email and the only information that an applicant is required to fill in is name, email address, and a short message.

To conduct this study, we had to solve the “disclosure problem”, because disabilities, like sexual orientation, is not an observable trait in written application. The presence of a disability has to be clearly disclosed in a way that does not seem unnatural and thus does not raise suspicions (Pager, 2007, Ahmed and Hammarstedt, 2008, Fumarco, 2017). We decided to use a similar person test as Verhaeghe et al., 2017 in order to signal disability: the father of a disabled person explains that he is looking for rental housing for his son (mentally handicapped, motor handicapped or blind). The fact that a father indicates the handicap of his son can easily be justified by the fact that he prefers to indicate this issue as soon as possible because it will necessarily end up being known. Indeed, unlike sexual orientation, disability (whether mental, visual or physical) is not something that can easily be hidden during the visit or even during the occupation of the property. If this could be a problem later, it is logical for the father to decide to prevent before going for many visit. We make a similar reasoning regarding the disclosure of the presence of a dog, if the father of the blind applicant “discloses the presence of the guide dog only upon visiting the accommodation, they might eventually receive a rejection and end up wasting time and energy. Moreover, although not illegal, failure to disclose the presence of the guide dog prior to signing the rental contract could cause future friction with neighbors and the landlord” (Fumarco, 2017). This approach makes it possible to test discrimination against a single disabled person, not a household (as in Fumarco, 2017). Making the decision to rent his apartment to one couple where only one person is disabled does not have the same implication and determinants as renting one’s apartment to one single disabled person. The prejudice can be diluted in the first case. However, we decided not to take the same control group as Verhaeghe et al., 2017 (namely a male candidate who would like to visit the apartment). Indeed, the fact that

it is the father who asks for his son strongly suggests the presence of a guarantor in addition to signaling the presence of a disability, it is necessary that the control group also suggests the presence of a guarantor so that the only variable that differs between these two groups is the presence of a disability (and not the presence of a guarantor plus presence of a disability). Therefore, the control person is also a father who indicates that he is looking for an apartment for his son, but no mention of any disability is done.

The first step in our experimental design was to create fictitious applicants. The choice of names is fundamental in correspondence testing. In an experiment on disability discrimination, names cannot be used to signal the target characteristic (unlike experiments on gender or ethnicity). Therefore, names and surnames were chosen among the most standard and common names and surnames of the French population in order to not point to a particular age or social status. In order not to add some gender effect, our experiment is entirely composed of fictitious male candidates: the application is always a dad looking for an apartment for his son. The name of the fictitious dad with a son with no disability is Vincent Meunier and the name of the fictitious dad with a son with disability is Clément Rivière. For each of them, four email accounts were created (one by treatment). The handicapped son is named Benoit and the non-handicapped is named Thomas.

These mails were sent exclusively to “F2” (2 rooms with only one bedroom) by controlling all the characteristics of the apartment (e.g. amount of rent, surface, location by zip code). It is motivated by the fact that the choice of apartments tested has an impact on the agents’ beliefs about the characteristics of the candidates. F2 is the most common apartment and the most suitable for this type of testing. On the contrary, F3 and F4 are not suitable as it would be very surprising to ask for an apartment of this size for one person, where there are several bedrooms. This may increase the probability of detection, and may suggest that the person will not live alone in the apartment so that the analysis would no longer capture discrimination against one person and would add heterogeneity

that cannot be controlled. Conversely, applying for a studio (one room) may very often suggest that the candidate is a student and there may be specific discrimination against students. F2 is a good compromise because it is suitable for a single person and does not indicate any particular age or status. We did not send mails to ads asking for applicants to call or stating that the advertiser wished to receive responses from a particular gender.

To test for discrimination against motor disabled people, we sent emails only to the apartments where it was explicitly noted on the ad: "disabled access". It is difficult to know the real proportion of apartments that actually display disabled access. There does not seem to be an official estimate, this is still relatively vague, subject to debate and creates many tensions.⁵¹ However, we know that most of apartments in the living areas, as in the city center, are part of older buildings, and are therefore rarely accessible to people with motor disabilities. Concerning new construction, The Elan bill (Evolution of housing, development and digital) plans for 20% of housing accessible to disabled people. The remaining 80% are intended to be "evolutive", that is to say made accessible after construction work. Note that we had great difficulty sending these mails as very few apartments indeed have a disabled access, and when they have, the owners of the apartment do not always indicate it in the advertisement. The difficulty we have encountered already gives us a good indication of the difficulty encountered by people with motor disabilities to only look for an apartment on internet.

All landlords were tracked during the experiment in order to avoid that a landlord was contacted more than once by each applicant. However, we could not control for the possibility that one landlord might have several ads under different names. We recorded the date and the heading of the ad, the zip code of the apartment, whether the landlord was a private person or a company, the name of the landlord and thus his gender (when

⁵¹ https://www.lemonde.fr/les-decodeurs/article/2018/06/05/le-grand-flou-des-logements-accessibles-aux-handicapes_5309881_4355770.html

available), if the apartment is furnished or not, the surface, and the rental cost per month.

Conducting a correspondence testing (like other type of testing) involves observing people's behavior without their knowledge or consent. Therefore, it is essential to take into account ethical considerations when conducting the experiment. After receiving a response and in order to limit the prejudice of the agents or private landlords, the fictitious applicant indicated as soon as possible having already found another apartment. Moreover, we did not ask any additional questions after the agent's or owner's response to obtain more details, we use only the information that could be learned from the ads or emails that we received from the landlords.

We sent email responses to advertisements published on these five website between March and July 2019 in a matched-paired procedure. The total sample includes 1,750 responses. 750 mails were sent in order to test for discrimination against blind applicants, 500 mails to test for discrimination against people with a mental disability and 500 mails to test for discrimination against people with motor impairment. Emails were sent only to advertisements that were published the same day. It allows to increase the response rates and more importantly to avoid many cases where agents do not respond simply because they have not seen the demand. In our case, since the ad has just been posted on the site, if a person does not answer, it is most certainly because he/she deliberately chooses not to answer. We thus mitigate a fairly common bias in correspondence testing where non-responses are often coded in the same way because it is difficult to determine why an agent did not answer. Yet, agents who have not seen the applications have not been tested and should not be included in the database.

We alternate sending order and structure of the message. Here is an example coded order 1, structure 1, sentences in square brackets indicate how disability is disclosed (for each type) (translated in English, see the appendix for the original version):

“Hello,

This ad corresponds to what I am currently looking for. Is it possible to make an appointment for a future visit? It’s for my son, Benoît. I allow myself to point out that [he is blind. His guide dog accompanies us] [he has a mental handicap but is able to have his own apartment] [he is motor handicapped].

Thanking you.

Clément Rivière

Dear Sir or Madam,

My son, Thomas, is looking for an apartment in the area and your ad seems to match. Would it be possible to visit it?

Yours truly,

Vincent Meunier”

Although we believe that the mails are different enough so that the testing is not detected, we allow 24 hours’ interval between sending the first and second email for more safety. Half the time it is the father of the disabled person who sends the mail first, then the father of the valid person (order 1), the other half it is the father of the valid who sends first, then the father of the handicapped person the next day (order 2). The structures of the messages are also alternated, once in two it was the father of the disabled person who sent the mail with structure “1” (“Hello, this ad etc.”), and the father of the valid person who sent the mail with structure “2” (“Dear Sir or Madam, my son etc.”), once in two it was the opposite. Therefore, there are four sets/paired of messages for each treatment.

As we use the matched paired procedure, we are able to compare the response rates from individuals with exactly the same characteristics, whereas the random assignment procedure is very dependent on the heterogeneity of the advertisements or the websites on which the ads are tested (all things being equal, there are fewer answers on some sites than on others).

Like Fumarco (2017) we use two control groups instead of one to test discrimination against blind tenants in order to check whether discrimination is due to the presence of a guide dog or rather due to the disability status. For that purpose, we compared the profile "Valid with a dog" (we just add "and his dog" after "Thomas" in applications) with the profile "Blind with a dog" so that only the presence of a disability differs. We assume it's better to do this rather than compare "Valid" with "Blind" without dog, because the "Blind" variable implies a dog's presence more than "Valid". We sent 250 e-mails only to private landlords in this treatment. This is justified from the results found by real estate agents with the treatment "Valid vs Blind with dog" (see below).

We are aware that adding the part "able to have his own apartment" makes the mental disability more benign. However, we wanted to test discrimination against alone and autonomous people so that we could compare the prejudice associated with each type of disability.

4. Results

4.1 Invitation to visit or to further contact

In total, Vincent (valid applicant) and Clément (disabled applicant) each applied for 875 apartments. Table 1 displays descriptive statistics regarding mean call back rates and share of applications that resulted in invitations to further contacts with the

landlords or to showings for Vincent and Clément respectively, in total, by type of disability and by type of landlords. We implemented mean difference T-tests with statistical inference based on a bootstrap procedure to test statistically the differences in the invitation rates for fictitious applicants by type of responses.

Table 1 shows the positive call backs divided into two categories. The category *Non-Negative* includes responses that invited the applicant either to further contacts, for example to provide the landlord with additional information about the applicant, or to a showing of an apartment. The category *Positive response* includes responses that directly invited the applicant to a showing. Thus, the category *Non-Negative* is a broader definition of a positive call back than the category *Positive response*. Studying both types of responses is necessary to understand discrimination against applicants. Indeed, comparing only non-negative responses might tend to underestimate discrimination, to the extent that Vincent's rates could include more real positive responses than Clément's rates. However, although purer, it is not sufficient either to only use the positive response rates because it may not capture the level of discrimination homogeneously (real estate agents are more likely to ask for vouchers than private landlords before booking an appointment for a visit, and the more expensive the rents, the more justifying pieces will be asked by the owners etc.) and because of a possible lack of power that could underestimate discrimination. Finally, using both types of responses allows to compare the rate of positive responses with the rates of request for vouchers and for more information, and therefore allow to a certain extent to test the presence of statistical discrimination.

Chapter 4: Discrimination against people with mental, physical and visual disabilities in the rental housing market.

Table 1: Mean call back rates, share of applications that led to invitations to further contacts or to showings and share of applications that led to invitation to showings

	Vincent (Valid)		Clément (Disabled)		<i>Bootstrap T-test</i>	
Call back rate (per cent) <i>N</i> = 875	Non-Negative	Positive	Non-Negative	Positive	Non-Negative	Positive
Total (per cent) <i>N</i> = 875	51.2	34.1	34.7	20.0	7.05***	6.70***
Private landlords (per cent) <i>N</i> = 515	61.9	43.8	40.3	25.8	7.08***	6.18***
Company (per cent) <i>N</i> = 360	35.8	20.0	26.7	11.6	2.66***	3.08***
Valid Vs Blind/Dog (per cent) <i>N</i> = 250	57.2	41.2	39.6	23.2	3.99***	4.38***
Valid Vs Mental Imp. (per cent) <i>N</i> = 250	56.8	37.6	36.4	16.8	4.66***	5.36***
Valid Vs Motor Imp. (per cent) <i>N</i> = 250	42.8	26.8	28.0	17.2	3.50***	2.60***
Valid Vs Blind/Dog (per cent / P.landlords) <i>N</i> = 130	73.8	58.5	43.8	28.4	5.14***	5.10***
Valid/Dog Vs Blind/Dog (per cent / P.landlords) <i>N</i> = 125	44.8	27.2	35.2	25.6	1.55	0.29
Valid Vs Mental Imp. (per cent / P.landlords) <i>N</i> = 130	70.0	49.2	43.8	23.8	4.40***	4.39***
Valid Vs Motor Imp. (per cent / P.landlords) <i>N</i> = 130	58.5	40.0	38.5	25.4	3.28***	2.53**
Valid Vs Blind/Dog (per cent / Company) <i>N</i> = 120	39.2	22.5	35.0	17.5	0.66	0.97
Valid Vs Mental Imp. (per cent / Company) <i>N</i> = 120	42.5	25.0	28.3	9.2	2.31**	3.32***
Valid Vs Motor Imp. (per cent / Company) <i>N</i> = 120	25.8	12.5	16.7	8.3	1.74*	1.05

Note: *p < 0.1; **p < 0.05; ***p < 0.01

The descriptive statistics and bilateral tests show that there is a large discrimination based on disability ground in the French rental housing market. Indeed, there are strong differences between the test person with a non-handicapped son (Vincent) and the test person with a handicapped son (Clément) as regards the numbers of call backs with invitations to further contacts with the landlords/real estate agents and with invitations to view apartments. We find that about 51% of the 875 applications sent by Vincent led to positive call backs in the sense that he was invited to either further contacts or to showings of the apartments while the corresponding figure for Clément is only 35%. Moreover, Vincent was directly invited to visit the apartment in 34% of the cases while Clément was invited in only 20% of the cases. These differences are statistically significant at 1%. However, these figures need to be nuanced: there are some differences depending on the type of disability and the type of agent tested.

In general, real estate agents have responded less than private owners to requests from the two fictitious candidates but figures indicate that both type of agents discriminated people with disabilities. As regards private landlords, we find that about 62% of applications sent by Vincent results in invitation to further contacts or to showings of the apartments whereas this figure was only 40% for Clément, and 44% of applications sent by Vincent results in direct invitations against only 26% for Clément. The corresponding figures for applications sent to real estate agents were 36% versus 27% for non-negative responses and 20% versus 12 for direct invitation to see the apartment.

Each type of disability faces significant discrimination, but figures indicate that the prejudice is higher for some than for others. The profile Clément, when his son has a mental impairment (yet totally autonomous), received less than half as many positive responses as Vincent (17 against 38%). Differential treatment is lower when comparing the rate of non-negative responses for these two fictitious applicants (36 against 56), which could indicate the presence of statistical discrimination: there are more positive

responses in the Vincent's rate than in the Clément's rate (54% of replies received by Clément were requests for additional information and/or vouchers, whereas this figure was only 36% for Vincent). Clément's penalty, although still very high, was a bit weaker when his son was this time blind with a dog: 23% against 42% of positive responses. Once again, differential treatment is lower when focusing on non-negative responses (40% against 57%). Finally, figures indicate that people with motor disabilities received one-third fewer positive responses than valid people (16% for Clément and 27% for Vincent). The corresponding figures for non-negative responses were 28% and 43%, respectively. This reveals that in addition to strong indirect discrimination related to access to housing, people with motor disabilities also face direct discrimination in the few apartments available to them in the French rental housing market.

Table 1 also reveals that agents respond differently to the different types of disabilities. Discrimination against blind people is very low or even non-existent (non-significant) by real estate agents (17% of positives responses for Clément against 22% for Vincent) while it is very high from private owners (28% versus 5%⁸). This is consistent with the study carried out in Italy by Fumarco (2017), which concludes that discrimination against blind tenants is driven by private landlords, whereas real estate agents do not discriminate. On the contrary, it seems that discrimination against people with mental or motor disabilities is as high from real estate agents than from private landlords. Finally, and consistently with previous studies in other countries (Fumarco, 2017; Verhaegue et al., 2017), figures indicate that Vincent, when his son has a dog, does not receive significantly more positive answers from private landlords than Clement, when his son is blind with a guide dog (27% against 26%), which seems to indicate that discrimination against blind people in the French rental housing market is purely indirect and mainly stems from the presence of the guide dog, not to the disability status. However, even if the landlords do not invite more the valid with his dog than the blind with his dog to a future visit of the apartment, it would seem that they are still more inclined to further contacts with the

dad of the valid one (about 45% of applications sent by Vincent results in non-negative responses whereas this figure was 35% for Clément), but this result is not significant at 10%.

4.2 Apartment and advertisement characteristics

Table 2: Descriptive Statistics on the structural characteristics of apartments and location

Variable	Observations	Mean	Std. Dev.	Min.	Max.
Continuous					
Rent per m2	1750	19.17	9.52	5.2	67.5
Pop density	1750	7558.2	8561.2	21.1	40059.1
Share of active population	1750	0.484	0.051	0.345	0.675
Dummies					
Furnished	1750	0.317			
Sent in second	1750	0.500			
Professional	1750	0.411			
Higher education	1750	0.930			
Tense zone	1750	0.633			
Large pole	1750	0.908			
Ring of urban pole	1750	0.034			
Multipolarized	1750	0.010			
Small or intermediate pole	1750	0.039			
Isolated	1750	0.008			

We collected extensive information from each advertisement: rent per m², whether it is furnished or not (dummy *furnished*) and whether the advertiser is a private agent or a real estate agent (dummy *professional*). For the analysis on the determinants of call back rates, we also added a dummy indicating whether the mail was sent in second. Moreover, we collected the municipality where the housing unit is located, which allowed to match this information with several socio-economic variables. Population density and the share

of active population was retrieved from the 2016 census conducted by INSEE⁵². We also included a dummy (*Higher education*) indicating whether the municipality is located in a broader area (*bassin de vie*) where a higher education establishment is present. In these municipalities indeed, there might be a prior from agents that the applicant might be a student. We finally included in turn two different variables capturing the urban/rural character of the municipality. First, in France, some areas are classified as “Tense zones”, they are particular areas where special rules apply. From a legal point of view, tense zones are “areas of continuous urbanization of more than 50,000 inhabitants where there is a marked imbalance between the supply and demand for housing, leading to serious difficulties of access to housing on all the existing housing stock, characterized in particular by the high level of rents, by the high level of acquisition prices of old housing or the high number of applications for housing compared to the number of annual dwellings in the social rental housing stock” (Art. 6, ALUR law). We therefore included a dummy indicating whether the ad is located in such a tense zone. Second, we used the official zoning of INSEE. In particular, in the 2010 zoning, INSEE identifies 3 categories of urban areas (large, medium and small) according to the number of jobs in the urban centre. A large urban area is a group of municipalities, in one piece and without enclave, constituted by an urban pole of more than 10 000 jobs (resp. 5000-10000 jobs for an intermediate pole and 1500-5000 jobs for a small pole) and a suburban ring composed of the municipalities of which at least 40% of the resident population works in the centre or in municipalities attracted by it. From there, we constructed a set of dummies describing the class in which the municipality belongs: large pole, small or intermediate pole, ring of urban pole, multipolarized municipality (i.e. a municipality in the ring of various poles) and isolated municipalities, corresponding to the most rural municipalities. Table 2 reports the main descriptive statistics for all of these variables.

⁵² Institut National de la Statistique et des Etudes Economiques (French Institute of Statistics and Economic Studies).

4.3 Regression analysis on the determinants of call back rates

We further investigate the data using linear probability models. We create two dependent variables, *Positive answer_i* and *Non negative answer_i*, which are dummy variables equaling 1 if the applicant received a positive answer, resp. a non-negative answer, to visit the apartment, and equaling 0 otherwise. This variable is regressed against a dummy indicating the disability (Clément, Blind with dog, Mental impairment, Motor impairment⁵³). The control variables in X_i include the apartment or advertisement characteristics and the location characteristics. The linear probability model can be written as:

$$(1) y_i = \beta_0 + \beta_1 \text{disability}_i + \beta_2 X_i + \varepsilon_i$$

where y_i is “*Positive answer_i*” or “*Non negative answer_i*”, disability_i is the dummy variable representing the specific disability and ε_i is the error term. First we estimate equation (1) with only the control variables pertaining to the ad or the apartment included, then we further include the location variables, either (2) “*tense zone*” or (3) the dummies describing the INSEE zoning. In all cases, we interact the dummy disability_i with some of the control variables in order to check whether the difference in the responses (positive or non-negative) between the valid and the disabled applicants vary according to other control variables.

⁵³ “Clément” is a variable, which groups all disabilities. We didn’t test for the profile “valid with a dog” because it was not matched with the control group (reference in our Tables), namely “Valid (without a dog)”. Thus data from the treatment Valid/dog vs Blind/dog are not taken into account in this regression and the sample consists of 1500 matched emails.

Chapter 4: Discrimination against people with mental, physical and visual disabilities in the rental housing market.

Table 3. OLS estimation results for the linear probability model of receiving a positive or a non-negative answer; Valid vs Disabled applicants

	<i>Dependent variable:</i>					
	Positive answer			Non negative answer		
	(1)	(2)	(3)	(1)	(2)	(3)
Clément	-0.179*** (0.043)	-0.187*** (0.043)	-0.168*** (0.046)	-0.158*** (0.046)	-0.166*** (0.046)	-0.144*** (0.051)
Sent in second	-0.055*** (0.016)	-0.056*** (0.016)	-0.054*** (0.016)	-0.075*** (0.018)	-0.076*** (0.018)	-0.074*** (0.018)
Professional	-0.275*** (0.034)	-0.262*** (0.034)	-0.264*** (0.035)	-0.296*** (0.036)	-0.289*** (0.036)	-0.290*** (0.037)
Rent per m2	-0.003* (0.002)	0.005* (0.002)	0.0002 (0.002)	-0.006** (0.002)	0.001 (0.003)	-0.002 (0.002)
Furnished	0.090** (0.040)	0.064 (0.040)	0.080* (0.041)	0.106*** (0.041)	0.083** (0.041)	0.094** (0.041)
Ring of a large pole			0.096 (0.130)			0.083 (0.103)
Multipolarized			0.149 (0.235)			-0.051 (0.231)
Small and intermediate pole			0.098 (0.141)			0.130 (0.094)
Isolated			0.067 (0.236)			-0.157 (0.232)
Higher education		0.019 (0.075)	0.016 (0.114)		0.035 (0.071)	0.018 (0.093)
Share of active population		-0.529 (0.340)	-0.832** (0.341)		-0.832** (0.368)	-1.009*** (0.373)
Tense zone		-0.210*** (0.044)			-0.153*** (0.046)	
Clément x Professional	0.135*** (0.032)	0.129*** (0.032)	0.130*** (0.033)	0.148*** (0.037)	0.141*** (0.037)	0.140*** (0.039)
Clément x Rent per m2	-0.001 (0.002)	-0.004* (0.002)	-0.002 (0.002)	-0.004* (0.002)	-0.006** (0.003)	-0.004* (0.002)
Clément x Furnished	-0.069* (0.038)	-0.060 (0.038)	-0.068* (0.038)	-0.061 (0.044)	-0.052 (0.045)	-0.061 (0.044)
Clément x Tense zone		0.087** (0.039)			0.091* (0.047)	
Clément x Ring of a large pole			-0.054 (0.105)			-0.137 (0.112)
Clément x Multipolarized			0.169 (0.271)			0.158 (0.265)
Clément x Small and intermediate pole			-0.151 (0.108)			-0.156 (0.108)
Clément x Isolated			0.223*** (0.042)			0.423** (0.198)
Constant	1.550*** (0.045)	1.776*** (0.162)	1.862*** (0.192)	1.794*** (0.043)	2.130*** (0.170)	2.187*** (0.186)
Observations	1,500	1,500	1,500	1,500	1,500	1,500
F Statistic	23.532***	19.993***	11.446***	26.255***	19.85***	12.632***

Note:

*p<0.1; **p<0.05; ***p<0.01

We use Ordinary Least Squares (OLS) and cluster-robust inference, where the error terms are clustered at the ad level. The estimation results are displayed below in Table 3 (Vincent vs Clément). We further investigate the discrimination by treatment in Tables 3.1 (Valid VS Blind with dog), 3.2 (Valid vs Mental Impairment) and 3.3 (Valid Vs Mobile Impairment). These corresponding three tables are provided in Appendix.

Consistent with results from Table 1, Table 3 indicates that disabled applicants receive fewer positive and non-negative responses than valid applicants (“Clément” is negative and significant for all specifications). As expected, the rate of positive and non-negative responses of applicants is significantly lower for fictitious applicants who sent an email in second position. Moreover, and as we could easily see it in Table 1, real estate agents return significantly fewer positive and non-negatives responses than private owners to applicants (“Professional” is negative and significant for all equations). Finally, it would seem that people with disabilities are significantly less discriminated against by real estate agents than by private landlords (coefficient for “Clément x Professional” is positive and significant for all equations).

Regarding the other variables collected from each advertisement, the rent per m2 does not seem to have a clear impact on the response rate of applicants while it seems that applicants receive more positive and non-negative responses from owners with furnished apartments than from owners with unfurnished apartments. Interestingly, the coefficient for “Clément x Rent per m2” is negative and significant for non-negative responses, which indicates that the differential treatment between Vincent and Clément grows when the rent increases, which would be consistent with the presence of some statistical discrimination based on financial means. Indeed, we hypothesized that if real-estate agents or landlords do not have sufficient information about the candidate, and consider disability as a proxy for lower income, they may avoid spending time answering applications from applicants they perceive as being poorer than others, so they can be

expected to discriminate more against disability applicants when the rental price is high. Finally, there is a tendency to more discrimination against disabled applicants when the apartment is furnished, but this tendency is only weakly significant or even non-significant.

With respect to the variables associated with the location of the apartment, the presence or absence of a higher education university in the area of the ad does not seem to have an impact on the response rate. On the contrary, it seems that the higher the proportion of the active population in the area, the less the candidates receive responses from agents or owners. Not surprisingly, the rate of positive and non-negative responses is lower in tense areas (“Tense zone” is significant and negative for all equations). Interestingly, the negative effect of tense areas is lower for Clément than for Vincent (coefficient for “Clément x Tense zone” is positive and significant). Finally, results indicate that there are no differences in rates of positive and non-negative responses from the dummies describing the INSEE zoning, except for isolated area. People with disabilities are significantly less discriminated against in isolated areas (coefficient for “Clément x Isolated” is positive and significant for all equations), which is relatively intuitive: landlords receive far fewer requests for visits in isolated areas and therefore have much less choice concerning the future tenants. They therefore allow themselves less to refuse a visit or not to answer the father of the disabled person.

All these results must however be nuanced, there are still some differences depending on the type of disability (see Table 4, 5 and 6 in the Appendix). For example, the fact that real estate agents discriminate less disabled applicants mainly comes from their non-discriminatory behavior towards blind people with dogs, while their discriminatory behavior towards people with mental and motor disabilities is little or no different from the behavior of private owners, which is in agreement with the results found by bilateral tests in Table 1. Moreover, blind people with guide dogs are more discriminated against

in terms of direct invitation to visit the apartment when they respond to ads for furnished apartments rather than unfurnished apartments (coefficient for “Blind x Furnished” is negative and significant) while there is no effect for other types of disabilities. Indeed, the presence of the dog is supposed more problematic in furnished apartments than in unfurnished apartments.

4.4 Regression analysis on absolute discrimination

Finally, we explicitly make use of our matched-paired design by investigating the determinants of absolute discrimination. For that purpose, we estimate a linear probability model where as a dependent variable, we include two dummies: “*I prefer the valid one*” which equals 1 if the valid applicant has received a positive answer and not the disabled applicant and “*I prefer the valid one2*” which equals 1 if the valid applicant has received a non-negative answer and not the disabled applicant. These dummies are then regressed against the same set of control variables using OLS and clustered-robust inference. As previously, we first present the results for absolute discrimination against Clement (see below Table 4), then we focus on the absolute discrimination by type of disabilities (Table 4.1, 4.2 and 4.3 in the Appendix) without the inclusion of variables describing the INSEE zoning but with a variable controlling for the population density.

Table 4 indicates that private landlords are more prone to absolute discrimination (choosing the valid one and not the disabled applicant) than real estate agents (“Professional” is negative and significant for all equations). However, this result may be partly explained by the fact that real estate agents simply return fewer answers to candidates, leaving less opportunity for which Vincent is favored. Results also indicate that the higher the rent per m², the more Vincent will be chosen to visit the apartment while Clément will not, which is consistent with the presence of some statistical discrimination based on financial stability, but this result is only weakly significant. Moreover, we find that Vincent is more often chosen (receives an answer while Clement

is not) to visit the apartment when the apartment is furnished than when it's not. Finally, it would appear that the location of the apartments tested (whether tense zones or variables describing the INSEE zoning) does not have a significant impact on the level of absolute discrimination against people with disabilities. Finally, there does not appear to be a significant difference in the determinants of absolute discrimination according to the type of disability (see table 4.1, 4.2 and 4.3 in the Appendix)

Table 4. OLS estimation results for the linear probability model; Valid vs Disabled applicants (Absolute Discrimination)

	<i>Dependent variable:</i>					
	Positive answer			Non negative answer		
	(1)	(2)	(3)	(1)	(2)	(3)
Professional	-0.121*** (0.029)	-0.117*** (0.030)	-0.115*** (0.030)	-0.143*** (0.031)	-0.144*** (0.032)	-0.139*** (0.032)
Rent per m2	0.002 (0.002)	0.004* (0.002)	0.003* (0.002)	0.003 (0.002)	0.003 (0.002)	0.003 (0.002)
Furnished	0.068** (0.034)	0.059* (0.034)	0.063* (0.034)	0.046 (0.036)	0.043 (0.037)	0.045 (0.037)
Tense zone		-0.053 (0.038)			-0.023 (0.041)	
Ring of a large pole			0.072 (0.104)			0.160 (0.112)
Multipolarized			-0.002 (0.191)			0.012 (0.206)
Small and intermediate pole			0.131 (0.113)			0.147 (0.122)
Isolated			-0.234 (0.191)			-0.203 (0.206)
Share of active population		-0.331 (0.357)	-0.455 (0.354)		-0.102 (0.385)	-0.205 (0.382)
Higher education		0.001 (0.069)	0.019 (0.100)		0.035 (0.075)	0.102 (0.108)
Constant	1.197*** (0.035)	1.340*** (0.163)	1.358*** (0.179)	1.237*** (0.038)	1.253*** (0.176)	1.215*** (0.192)
Observations	750	750	750	750	750	750
F Statistic	10.007***	5.604***	3.990***	10.359***	5.257***	4.046***

Note:

*p<0.1; **p<0.05; ***p<0.01

5. Conclusion

In this chapter, we use the well-known correspondence testing method to detect the presence of discrimination against people with disabilities in the rental housing market in France. By sending 1,750 emails in matched-paired procedure, we show that the prejudice of people with visual, motor and mental disabilities is very high in the rental housing market in France. They are very significantly less likely to be invited to visit an apartment or even invited to providing further information from real estate agents or private landlords. Discrimination increases with the level of rent per m², and the percentage of response received which are requests for additional information and/or vouchers is much higher for the disabled than for the non-disabled, which could suggest that this discrimination comes in part from statistical considerations based on financial means. It seems that agents need to be “reassured” about disabled candidates. Fictitious applicants are significantly less likely to receive a response in areas where the rental housing market is “tense”, leading to lower differential treatment between the disabled and non-disabled applicants. Results indicate also that discrimination is as high in large poles as in ring of urban poles, multipolarized municipality or even in small or intermediate poles. Discrimination is, however, significantly lower in isolated areas.

People with mental disabilities (yet autonomous) face the highest level of direct discrimination in the process of allocation of rented housing. They were more than two times less likely to be invited to visit an apartment than a person without disabilities in the French rental housing market. Blind people with a guide dog, as for them, are almost two times less likely to be invited to visit the apartment as the valid ones; however, we show, as in Fumarco (2017) and Verhaegue et al. (2017), that this discrimination is purely indirect and mainly comes from the presence of the guide dog, not from the

disability status. Moreover, and once again consistent with these two other studies on discrimination based on visual impairment, we also find that real estate agents discriminate to a lesser extent (or do not discriminate at all) blind candidates with a guide dog than private landlord do.

The strongest total prejudice is faced by people with motor disabilities. Indeed, there is firstly an indirect discrimination in regards to the housing supply in the market for people with motor disabilities: less than 20% of the apartments are really accessible to them, which of course limits their choice. Moreover, of these accessible apartments, all are unfortunately not listed as such on the internet, and therefore require more advanced and costly research⁵⁴. Finally, among the ads well listed as being accessible to people with motor disabilities, people with motor disabilities are one third less likely to receive a positive response from real estate agents and private landlords or even being asked to provide more details about their situation than valid people. Therefore, they face strong direct and indirect discrimination in the rental housing market in France.

Consistently with the literature on labor market, it seems that people with disabilities also face a level of discrimination harder than the level of discrimination faced by other minorities in the rental housing market. Indeed, correspondence tests on ethnic discrimination (which is one of the most conducive factors of discrimination) in France in the rental housing market (Le Gallo et al., 2018; Bunel et al., 2017; Acolin et al., 2016) shows that applicants with foreign sounding names are 16 to 33 percent less likely to receive a response than applicants with French names.

In France, as in the other OECD countries, Internet is the principal channel used for a rental housing search. Our results should therefore represent well the prejudice suffered by people with a disability to find a rental apartment in France. However, correspondence

⁵⁴ In this regard, we strongly advise people with motor disabilities to turn to ads from real estate agencies that report much more systematically the disabled access than private owners.

testing, certainly allows to have a great control on the tested variables, but does not allow to test the discrimination in the final attribution of the housing. Indeed, it only captures discrimination at the first stage of the process while it is quite possible that a second layer of discrimination against people with disabilities occurs during the visit of housing. Moreover, for all the benefits it can provide, we have used the matched paired procedure. However, it is not impossible either that some agents have detected the presence of testing, in which case the level of discrimination that we found, yet already very strong, underestimates the true discrimination faced by disabled applicants. Thus, the large level of discrimination revealed in this study can be taken as a low benchmark of the level of actual discrimination experienced by disabled candidates in the rental housing market.

This is really meaningful and shows how important it would be for more studies to address the issue of disability in the rental housing market, and not only in France. A lot of people are concerned by these three types of disabilities. In France, as in many other countries, millions of people are affected by a disability (almost 5 million of individuals are concerned by visual, mental or especially motor impairment in France, INSEE, 2007). Hence, our results show that people with disabilities face a large prejudice in the French rental housing market, but can to a certain extent be generalized and raises the question of disability in other OECD countries.⁵⁵

⁵⁵ Regarding discrimination against blind people with a guide dog, we find exactly the same conclusions as previous studies carried out in Italy and Belgium (Fumarco, 2016; Verhaegue et al., 2017)

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Appendix:

Examples of messages uses in French (sending order 1, structure 1)

Bonjour,

Cette annonce correspond à ce que je recherche actuellement. Est-il possible de prendre rendez-vous pour une visite future ? C'est pour mon fils, Benoît. Je me permets de signaler [qu'il est aveugle. Son chien guide nous accompagne] [il a un handicap mental mais est en mesure d'avoir son propre appartement] [il est handicapé moteur].

En vous remerciant.

Clément Rivière

Madame, Monsieur

Mon fils, Thomas, cherche un appartement dans la zone et votre annonce semble tout à fait correspondre. Serait-il possible de le visiter ?

Bien à vous,

Vincent Meunier

Table 3.1 OLS estimation results for the linear probability model of receiving a positive or a non-negative answer; Valid vs Blind with guide dog

	<i>Dependent variable:</i>					
	Positive answer			Non negative answer		
	(1)	(2)	(3)	(1)	(2)	(3)
Blind	-0.180*** (0.030)	-0.155** (0.066)	-0.085 (0.076)	-0.176*** (0.032)	-0.113 (0.070)	-0.058 (0.080)
Sent in second		-0.074*** (0.028)	-0.069** (0.028)		-0.111*** (0.030)	-0.107*** (0.030)
Professional		-0.327*** (0.063)	-0.257*** (0.070)		-0.300*** (0.064)	-0.228*** (0.071)
Rent per m2		0.002 (0.003)	0.014*** (0.005)		-0.004 (0.003)	0.007 (0.005)
Furnished		0.128* (0.077)	0.118 (0.075)		0.156** (0.073)	0.147* (0.072)
Population density			-0.00001** (0.00000)			-0.00001** (0.00000)
Higher education			0.068 (0.110)			0.121 (0.102)
Share of active population			-0.209 (0.715)			0.095 (0.723)
Tense zone			-0.143* (0.084)			-0.201** (0.084)
Blind x Professional		0.221*** (0.061)	0.179** (0.069)		0.241*** (0.065)	0.188*** (0.072)
Blind x Rent per m2		-0.005 (0.003)	-0.012*** (0.005)		-0.008** (0.003)	-0.016*** (0.005)
Blind x furnished		-0.125* (0.070)	-0.131* (0.070)		-0.091 (0.076)	-0.087 (0.077)
Blind x Population density			0.00001** (0.00000)			0.00001 (0.00001)
Blind x Tense zone			-0.017 (0.075)			0.073 (0.083)
Constant	1.412*** (0.031)	1.538*** (0.075)	1.503*** (0.324)	1.572*** (0.031)	1.810*** (0.069)	1.619*** (0.326)
Observations	500	500	500	500	500	500
R ²	0.055	0.134	0.161	0.031	0.137	0.163
F Statistic	28.773***	9.485***	6.664***	15.936***	9.769***	6.767***

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 3.2 OLS estimation results for the linear probability model of receiving a positive or a non-negative answer; Valid vs Mental impairment

	<i>Dependent variable:</i>					
	Positive answer			Non negative answer		
	(1)	(2)	(3)	(1)	(2)	(3)
Mental imp.	-0.208*** (0.028)	-0.243*** (0.081)	-0.220*** (0.083)	-0.204*** (0.033)	-0.187** (0.083)	-0.153* (0.090)
Sent in second		-0.072*** (0.027)	-0.075*** (0.027)		-0.092*** (0.032)	-0.096*** (0.032)
Professional		-0.216*** (0.061)	-0.209*** (0.062)		-0.251*** (0.063)	-0.256*** (0.065)
Rent per m2		-0.006* (0.004)	0.006 (0.005)		-0.007* (0.004)	0.001 (0.006)
Furnished		0.112 (0.071)	0.106 (0.072)		0.102 (0.069)	0.099 (0.070)
Population density			-0.00000 (0.00001)			-0.00000 (0.00001)
Higher education			0.076 (0.123)			0.101 (0.149)
Share of active population			-0.861 (0.710)			-1.011 (0.761)
Tense zone			-0.199*** (0.076)			-0.133* (0.078)
Mental imp. x Professional		0.094* (0.056)	0.080 (0.055)		0.119* (0.064)	0.099 (0.064)
Mental imp. x Rent per m2		-0.0004 (0.003)	-0.007 (0.005)		-0.004 (0.004)	-0.014** (0.006)
Mental imp. x furnished		-0.007 (0.067)	-0.005 (0.067)		-0.011 (0.080)	-0.006 (0.080)
Mental imp. x Population density			0.00000 (0.00000)			0.00001 (0.00001)
Mental imp. x Tense zone			0.128* (0.070)			0.169** (0.084)
Constant	1.376*** (0.031)	1.596*** (0.083)	1.867*** (0.335)	1.568*** (0.031)	1.828*** (0.076)	2.159*** (0.340)
Observations	500	500	500	500	500	500
R ²	0.055	0.132	0.159	0.042	0.125	0.138
F Statistic	28.773***	9.336***	6.561***	21.730***	8.766***	5.559***

Note:

* p<0.1; ** p<0.05; *** p<0.01

Table 3.3 OLS estimation results for the linear probability model of receiving a positive or a non-negative answer; Valid vs Motor impairment

	<i>Dependent variable:</i>					
	Positive answer			Non negative answer		
	(1)	(2)	(3)	(1)	(2)	(3)
Motor imp.	-0.096*** (0.025)	-0.065 (0.078)	-0.097 (0.086)	-0.148*** (0.031)	-0.123 (0.090)	-0.124 (0.096)
Sent in second		-0.021 (0.025)	-0.021 (0.025)		-0.025 (0.031)	-0.025 (0.031)
Professional		-0.276*** (0.054)	-0.292*** (0.054)		-0.328*** (0.060)	-0.330*** (0.059)
Rent per m2		-0.001 (0.003)	0.007* (0.004)		-0.004 (0.003)	0.004 (0.005)
Furnished		0.021 (0.061)	-0.029 (0.059)		0.060 (0.071)	0.021 (0.070)
Population density			-0.00000 (0.00000)			-0.00000 (0.00000)
Higher education			-0.071 (0.156)			-0.129 (0.141)
Share of active population			-0.491 (0.553)			-1.513** (0.677)
Tense zone			-0.268*** (0.072)			-0.116 (0.081)
Motor imp. x Professional		0.092* (0.050)	0.102* (0.053)		0.095 (0.064)	0.096 (0.065)
Motor imp. x Rent per m2		-0.003 (0.003)	-0.003 (0.004)		-0.002 (0.003)	-0.0003 (0.004)
Motor imp. x furnished		-0.055 (0.065)	-0.046 (0.064)		-0.074 (0.074)	-0.080 (0.075)
Motor imp. x Population density			-0.00000 (0.00000)			-0.00000 (0.00000)
Motor imp. x Tense zone			0.073 (0.063)			-0.028 (0.081)
Constant	1.268*** (0.028)	1.435*** (0.083)	1.787*** (0.289)	1.428*** (0.031)	1.667*** (0.083)	2.463*** (0.333)
Observations	500	500	500	500	500	500
R ²	0.013	0.098	0.159	0.024	0.121	0.165
F Statistic	6.777***	6.676***	6.574***	12.218***	8.449***	6.822***

Note:

* p<0.1; ** p<0.05; *** p<0.01

Table 4.1 OLS estimation results for the linear probability model; Valid vs Blind with dog
(Absolute discrimination)

	<i>Dependent variable:</i>			
	Positive answer		Non negative answer	
	(1)	(2)	(1)	(2)
Professional	-0.175*** (0.054)	-0.161*** (0.059)	-0.186*** (0.055)	-0.164*** (0.061)
Rent per m2	0.005* (0.003)	0.010** (0.005)	0.007* (0.003)	0.009* (0.005)
Furnished	0.080 (0.063)	0.088 (0.063)	0.044 (0.064)	0.052 (0.065)
Population density		-0.00001* (0.00000)		-0.00001 (0.00000)
Tense zone		0.035 (0.072)		0.026 (0.074)
Share of active population		-0.318 (0.704)		0.327 (0.727)
Higher education		0.084 (0.097)		-0.022 (0.100)
Constant	1.183*** (0.063)	1.203*** (0.311)	1.177*** (0.064)	1.020*** (0.321)
Observations	250	250	250	250
R ²	0.079	0.100	0.079	0.087
F Statistic	6.987***	3.852***	7.001***	3.283***

Note:

* p<0.1; ** p<0.05; *** p<0.01

Table 4.2 OLS estimation results for the linear probability model; Valid vs Mental impairment (Absolute Discrimination)

	<i>Dependent variable:</i>			
	Positive answer		Non negative answer	
	(1)	(2)	(1)	(2)
Professional	-0.114** (0.054)	-0.088 (0.056)	-0.149*** (0.057)	-0.127** (0.059)
Rent per m2	0.0002 (0.003)	0.007 (0.005)	0.002 (0.003)	0.007 (0.005)
Furnished	0.022 (0.061)	0.012 (0.061)	0.032 (0.064)	0.025 (0.064)
Density		-0.00001 (0.00001)		-0.00000 (0.00001)
Tense zone		-0.087 (0.068)		-0.078 (0.071)
Share of active population		0.182 (0.736)		0.336 (0.773)
Higher education		-0.207 (0.177)		-0.167 (0.186)
Constant	1.265*** (0.066)	1.344*** (0.355)	1.289*** (0.070)	1.274*** (0.373)
Observations	250	250	250	250
R ²	0.021	0.042	0.034	0.047
F Statistic	1.764	1.520	2.917**	1.704

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 4.3 OLS estimation results for the linear probability model; Valid vs Motor impairment (Absolute Discrimination)

	<i>Dependent variable:</i>			
	Positive answer		Non negative answer	
	(1)	(2)	(1)	(2)
Professional	-0.079*	-0.086*	-0.103**	-0.125**
	(0.043)	(0.045)	(0.052)	(0.054)
Rent per m2	0.002	0.003	0.001	0.001
	(0.002)	(0.003)	(0.003)	(0.004)
Furnished	0.080	0.066	0.048	0.048
	(0.052)	(0.052)	(0.062)	(0.063)
Population density		0.00000		0.00000
		(0.00000)		(0.00000)
Tense zone		-0.029		0.032
		(0.061)		(0.074)
Share of active population		-0.952		-1.060
		(0.602)		(0.728)
Higher education		-0.099		0.257
		(0.130)		(0.157)
Constant	1.090***	1.617***	1.211***	1.452***
	(0.057)	(0.292)	(0.069)	(0.353)
Observations	250	250	250	250
R ²	0.021	0.042	0.034	0.047
F Statistic	1.764	1.520	2.917**	1.704

Note:

*p<0.1; **p<0.05; ***p<0.01

Conclusion Générale

Depuis plusieurs décennies, de nombreuses expériences de terrain ont démontré la présence de discrimination dans le marché du travail et du logement locatif à l'égard de différentes minorités. La discrimination, surtout dans ces deux marchés, est génératrice de nombreuses inefficacités économiques et sociales. En effet, en plus d'aggraver la ségrégation résidentielle, de réduire l'accès aux services publics, à l'éducation ou encore d'affecter la santé et l'inclusion sociale des individus, la discrimination ne permet pas l'allocation des logements et des emplois aux individus les plus « efficaces ». De plus, l'accès au travail est très étroitement lié à l'accès au logement, et vice versa. Ainsi, les individus discriminés à l'embauche ont un accès au logement locatif réduit de par leur plus faible stabilité financière, réduisant par la même occasion leur accès au travail, etc.

Pour combattre la discrimination, il est nécessaire d'en connaître les déterminants. En effet, la discrimination peut provenir de deux sources communément présentées dans la littérature. La discrimination par le « goût » (Becker, 1957) fait ainsi référence à la discrimination qui provient des préférences des agents (racisme, xénophobie, homophobie, sexisme, etc.) tandis que la discrimination dite « statistique » (Phelps, 1972 ; Arrow, 1973) provient de la performance supposée (ou « qualité ») des candidats en présence d'asymétrie d'information. Grâce à l'investigation réalisée par plusieurs expériences de terrains, de nombreuses preuves existent quant à la présence de discrimination statistique et par le goût à l'égard des différentes minorités dans le marché du logement locatif et dans le marché du travail. Cependant, ces études ont leur propre protocole, leur propre façon de rendre compte des résultats, leur propre échantillon et ont testé la discrimination dans un contexte qui leur est propre. Ainsi, la littérature regorge d'expériences similaires aux résultats parfois différents. Enfin, certaines minorités n'ont été que peu ou pas du tout testées.

Dans cette thèse, nous avons ainsi utilisé une large panoplie de méthodes empiriques et expérimentales pour clarifier le niveau, les sources, ainsi que les déterminants de la discrimination à l'encontre de différentes minorités sur le marché du logement locatif et de l'emploi, permettant d'apporter des éléments essentiels pour l'élaboration de politiques anti-discrimination efficaces. Pour répondre à nos différentes questions de recherche, plusieurs outils et méthodes tels que la méta-analyse, le Testing ou encore l'expérience en laboratoire sont utilisés dans les quatre essais que compose ce manuscrit.

Dans le premier chapitre de cette thèse, nous avons présenté une vaste revue de toutes les études ayant testé la présence de discrimination à l'égard des groupes ethniques minoritaires sur le marché du logement par la méthode du test de correspondance dans les pays de l'OCDE, permettant à la littérature sur le sujet d'être plus à jour. De plus, nous avons réalisé une méta-analyse de ces études, contenant plus de 300 tailles d'effets et représentant ainsi une base de données totale de plus de 110 000 e-mails envoyés à des propriétaires privés et des agents immobiliers. En plus de présenter les résultats globaux de ces études récentes, nous avons réalisé une analyse de régression multivariée en nous concentrant sur des sous-groupes de tests de correspondance spécifiques afin de mettre en évidence les différences de résultats en fonction de différentes caractéristiques telles que l'ethnie testé, le sexe des candidats, le type d'agents testé, la procédure utilisée, la localisation ou encore le type d'information fourni dans les requêtes. Grâce à la puissance statistique conférée par la méta-analyse ainsi qu'aux nombreuses variables récoltées, nous avons clarifié le niveau, les sources et les déterminants de la discrimination à l'égard des différentes minorités ethniques dans le marché du logement locatif dans les pays de l'OCDE.

A la première étape du processus de location, nous trouvons que les individus appartenant à une minorité ethnique ont presque deux fois moins de chances d'être choisis par les agents immobiliers et les particuliers que les individus appartenant à la majorité ethnique,

à information égale fournie dans les e-mails. Cependant, ce résultat doit être nuancé : la méta-analyse permet en effet de comparer le niveau de discrimination subi par les différentes minorités, en contrôlant toutes les variables observables qui peuvent différencier les études. Ainsi, les résultats montrent que certaines minorités ethniques sont plus affectées que d'autres dans le marché du logement locatif dans les pays de l'OCDE et qu'il existe deux niveaux principaux de discrimination subis par les minorités. Le niveau le plus élevé est subi par les individus ayant un nom à consonance Arabo-musulmane, Turque ou d'Afrique sub-saharienne, tandis que les individus ayant un nom à consonance Hispanique ou d'Europe de l'est sont certes discriminés mais significativement moins que ces derniers. En réalisant cette méta analyse, nous avons aussi pu mettre en évidence un effet de genre. En effet, les e-mails signés d'un nom féminin reçoivent significativement plus de réponses de la part des agents immobiliers et des propriétaires privés que les e-mails signés d'un nom masculin. De plus, il semble y avoir une interaction entre la discrimination de genre et la discrimination ethnique : cette différence de traitement entre les femmes et les hommes est plus élevée parmi les individus appartenant à une minorité ethnique que parmi les individus appartenant à la majorité. Fait très intéressant, les agents immobiliers, qui sont des intermédiaires professionnels, discriminent significativement moins que les propriétaires privés. Cela provient en partie du fait que ces derniers font preuve de discrimination statistique significative à propos de la stabilité financière et/ou l'éducation des futurs locataires alors que les agents immobiliers non. Il semblerait que les particuliers, de par leur position plus risqué et/ou à cause d'un manque d'information correcte sur les individus appartenant aux minorités ethniques, aient besoin d'être « rassurés » à leur égard.

La méta-analyse nous a ainsi permis de déterminer l'ampleur de la discrimination actuelle dans le marché de la location dans les pays de l'OCDE. Cependant, il est tout à fait possible que ces résultats, bien que montrant une discrimination déjà élevée, sous-estiment le niveau réel de préjudice subi par les minorités ethniques. En effet, même si

la première étape permet par nature de capter la majeure partie de la discrimination⁵⁶ (voir Riach et Rich, 2002), il est malheureusement probable qu'un second niveau de discrimination ait lieu à la seconde étape du processus (lors de la visite réelle du logement). Quoi qu'il en soit, cela montre que les individus ayant un nom à consonance étrangère (surtout Arabo-musulmane, Turque et d'Afrique Subsaharienne) subissent de grandes difficultés dans le marché du logement locatif dans les pays de l'OCDE et qu'il est plus coûteux pour eux en temps et en argent d'obtenir le logement qu'ils désirent (proche des zones d'emplois, des zones de vie, des services).

Il est cependant difficile de savoir l'ensemble des répercussions que cette discrimination peut avoir sur les minorités discriminées ainsi que sur la société dans son ensemble. Elle a évidemment un impact sur de nombreux autres facteurs, en premier lieu l'accès à l'emploi. La méta-analyse de Zschirnt et Ruedin (2016) réalisée dans le marché du travail montre ainsi que les individus appartenant à une minorité ethnique ont 40% de chances de moins d'être choisis à la première étape du processus d'embauche dans les pays de l'OCDE. Ce traitement différentiel lors du processus d'embauche a lui-même un impact sur la stabilité financière des candidats minoritaires et les rends donc moins attractifs sur le marché de la location, pouvant ainsi amener à une discrimination statistique sur ce marché, réduisant encore leur accès à l'emploi, etc.

Ainsi, il est urgent de combattre la discrimination dans l'un ou l'autre marché afin de rentrer dans un cercle vertueux. En effet, réduire la discrimination (qu'elle soit statistique ou par le goût) dans un marché a un effet multiplicateur car permet de diminuer les inégalités et l'inefficacité dans ce même marché et conduit également à de nombreuses

⁵⁶ Les agents qui acceptent à la première étape les candidats du groupe discriminé pour une visite future sont plus rares à discriminer à la deuxième étape. Les agents qui à l'inverse discriminent à la première étape ne rencontreront bien évidemment pas de candidats appartenant au groupe discriminé lors de la visite.

externalités positives dans plusieurs autres marchés, ce qui permet ainsi de réduire la discrimination dans le marché initial etc.

Notre deuxième chapitre fut consacré au traitement des individus homosexuels à la première étape du processus d'embauche. En effet, après avoir fait l'objet d'une attention marginale, la littérature sur la situation des gays et des lesbiennes dans le marché du travail a récemment pris de l'ampleur. Cependant, le niveau, les sources et les déterminants de la discrimination à leur égard n'était toujours pas bien défini. Nous avons ainsi réalisé une seconde méta-analyse pour fournir à la littérature la première analyse quantitative de la discrimination à l'égard des individus ouvertement homosexuels dans le marché du travail.

Nous avons ainsi déterminé, par l'analyse de 18 études représentant une base de données de plus de 50 000 requêtes (CV plus lettre de motivation et parfois lettre de recommandation) envoyées à des employeurs, que les candidats ouvertement homosexuels avaient presque 40% de chances de moins d'être choisis pour un éventuel entretien d'embauche que les candidats hétérosexuels, à information égale fournie dans les requêtes. Ainsi, les résultats montrent que le préjudice à l'égard des individus ouvertement homosexuels est similaire au préjudice subi par les minorités ethniques à la première étape du processus d'embauche (Zschirnt and Ruedin, 2016).

Les résultats montrent aussi que la discrimination est plus élevée dans les emplois à faible qualification que dans les emplois très qualifiés. De plus, et de manière cohérente avec la littérature sur les stéréotypes de féminité/masculinité des homosexuels, les lesbiennes font face à un préjudice moins grand que les gays, sauf dans les emplois considérés comme plutôt féminins, et les hommes gays sont significativement moins discriminés dans les emplois féminins que dans les emplois considérés comme masculins.

De prime abord, cette investigation pourrait être perçue comme ne reflétant pas la réalité du préjudice subi par les homosexuels, car, à l'inverse de l'ethnie étudiée dans le premier

chapitre, l'homosexualité peut tout à fait être cachée et il n'y a que peu de raison de la signaler, même de manière détournée, lors de l'envoi d'un CV ou lors d'un entretien d'embauche. Cependant, étudier la discrimination à l'égard d'individus ouvertement homosexuels, pouvant être d'ailleurs perçus comme activistes, permet d'étudier indirectement le préjudice subi par les individus homosexuels dans le marché du travail, car ces derniers sont comparés à des individus hétérosexuels eux aussi activistes. Ainsi, le montant et les sources de la discrimination révélés dans cette méta-analyse sont bien liés à l'homosexualité et pas à l'activisme. Cela permet surtout d'étudier le comportement des employeurs face à l'homosexualité dans le marché du travail. Etudier le comportement des employeurs ex-ante par la méthode du test de correspondance est surtout très pratique pour des raisons de puissance statistique et de contrôle des variables.

Comprendre la nature de la discrimination est une étape nécessaire pour définir de bonnes politiques pour la limiter. Réduire l'incertitude sur les caractéristiques des gays et des lesbiennes et atténuer l'impact des stéréotypes négatifs grâce à une meilleure connaissance des minorités sexuelles pourrait en faire partie, car la discrimination ne semble pas résulter entièrement des préférences homophobes des recruteurs.

Comme nous avons pu le constater dans le premier chapitre et lors de l'étude de nombreuses expériences de terrain dans le marché du logement locatif, les intermédiaires semblent moins enclins à discriminer que les individus directement concernés. Les implications pourraient être grandes pour d'éventuelles politiques publiques : les intermédiaires professionnels pourraient aider à lutter contre les discriminations et toutes les conséquences négatives qu'elles impliquent.

Dans le troisième chapitre de cette thèse, nous avons, par l'intermédiaire d'une variante originale du jeu de la confiance (Berg et al., 1995), testé si ce comportement provenait de la nature même de l'intermédiation ou plutôt de facteurs externes propres aux marchés

du logement locatif. Nos résultats indiquent qu'un intermédiaire pur (qui ne risque absolument rien, ne peut augmenter son profit, et est épuré de tous les facteurs propres au marché) est au contraire plus apte à favoriser les membres de son groupe que les individus directement concernés (« propriétaire » de la dotation mise en jeu). Cela provient du fait qu'en raison de leur position non risquée, les intermédiaires peuvent se permettre plus facilement d'exprimer leurs préférences, bien qu'ils se sentent responsables de la dotation avec laquelle ils jouent. De manière cohérente avec la méta-analyse réalisée dans le chapitre 1, nous trouvons que les propriétaires de la dotation mise en jeu, de par leurs choix plus risqués, sont plus sujets aux considérations statistiques que les intermédiaires. De plus, nous avons observé un effet de genre parmi les intermédiaires : les participantes prennent moins de risques avec la dotation du propriétaire que les participants.

Ainsi, nos résultats semblent indiquer que cette plus faible discrimination observée empiriquement de la part des agents immobiliers n'est pas un pur effet de l'intermédiation mais provient plutôt de facteurs propres au marché testé qui ne sont pas présents en laboratoire : les intermédiaires professionnels sont par exemple soumis à des restrictions plus sévères en matière pénale ou civile, ont des soucis de réputation ou encore disposent d'un niveau d'information plus élevé sur les caractéristiques des minorités. Il serait ainsi très intéressant que de futures études se penchent sur l'effet de ces facteurs pour une meilleure compréhension permettant l'élaboration de politiques anti-discrimination efficaces.

Finalement, dans le dernier chapitre de cette thèse, nous avons mis en place le premier test de correspondance permettant de détecter, mesurer, et comparer la discrimination à l'encontre de personnes ayant un handicap moteur, mental ou visuel dans le marché du logement locatif. En envoyant 1750 e-mails appariés à des agents immobiliers et des particuliers, nous avons pu constater que les aveugles avec un chien guide, les personnes

ayant un handicap mental et les handicapés moteur étaient très discriminés dans le marché du logement locatif en France. Il semble que la discrimination provienne en partie de considérations statistiques concernant la stabilité financière des personnes handicapées. De manière cohérente avec les résultats de la méta-analyse réalisée dans le premier chapitre sur la discrimination à l'égard des minorités ethniques, ils semblent que les agents immobiliers discriminent moins que les propriétaires privés, cependant ce résultat n'est vraiment significatif qu'à l'égard des aveugles.

Notre testing indique que les personnes ayant un handicap mental (mais autonomes) sont celles qui subissent la discrimination directe la plus élevée : elles ont plus de deux fois moins de chances de recevoir une invitation à visiter l'appartement demandé. Les aveugles avec un chien guide, quant à eux, subissent une discrimination presque aussi lourde, mais celle-ci provient entièrement de la présence du chien guide, et non du statut d'invalidité, ce qui est en accord avec les résultats trouvés par les tests de correspondance réalisés en Italie (Fumarco, 2017) et en Belgique (Verhaegue et al., 2017) sur la discrimination à l'égard des aveugles. Finalement, le préjudice total le plus élevé est subi par les handicapés moteurs car ces derniers, en plus d'avoir un tiers moins de chances de recevoir une réponse positive de la part des agents immobiliers et des propriétaires privés, font aussi face à une forte discrimination indirecte sur l'offre de logements accessibles.

Ces résultats sont sans appel, les personnes ayant un handicap sont, de manière cohérente avec la littérature sur le marché du travail, la minorité la plus discriminée dans le marché du logement locatif en France (Challe et al., 2018 ; Le Gallo et al., 2018 ; Bunel et al., 2017 ; Acolin et al., 2016). Des millions d'individus sont ainsi concernés par cette forte discrimination, impactant leur accès à l'emploi, leur inclusion sociale, leur accès aux services, leur santé etc. Ces résultats alarmants posent aussi la question du handicap dans les autres pays de l'OCDE. Il serait important que davantage d'études abordent la question du handicap sur le marché du logement locatif, et pas seulement en France. En

effet, malgré les nombreuses conséquences néfastes que la discrimination peut impliquer, il n'existe à ce jour que très peu d'études de terrain sur la discrimination à l'égard des personnes handicapées dans les autres pays de l'OCDE.

Comme pour la discrimination à l'égard des autres minorités, il est important de lutter contre cette discrimination dans le marché du travail ou le marché du logement locatif pour rentrer dans une forme de cercle vertueux. Certaines mesures ont déjà été prises en France pour lutter contre la discrimination à l'égard des personnes handicapées dans le marché du travail. Par exemple, la loi « pour l'égalité des droits et des chances, la participation et la citoyenneté des personnes handicapées » du 11 février 2005 renforce l'obligation pour toute entreprise dont l'effectif dépasse 20 personnes d'embaucher un quota minimum de 6 % de personnes handicapées. Cela permet indirectement de baisser la discrimination statistique dans le marché du logement locatif grâce à une meilleure stabilité financière. Nos résultats montrent cependant qu'il reste encore un long chemin à parcourir.

De manière générale, même si certaines études récentes montrent que la discrimination à l'égard de différentes minorités semble plutôt décroître au fil du temps (ex. Auspurg et al., 2019), nous avons pu déterminer dans cette thèse que la discrimination était encore très présente dans les pays occidentaux. Il serait intéressant que de futures études de terrains se penchent aussi sur la discrimination en fonction d'autres critères, comme par exemple l'âge des candidats, notamment dans le marché du logement locatif. En effet, plusieurs lois, en France notamment (ex. loi ALUR), protègent les personnes âgées lors de l'occupation du logement. Malheureusement, il n'est pas impossible que cette protection puisse créer des barrières à l'entrée, les propriétaires préférant choisir des candidats moins protégés lors de l'attribution du logement.

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Résumé

La discrimination dans le marché de l'emploi et du logement locatif est génératrice de nombreuses inefficacités économiques et sociales. Pour combattre la discrimination à l'égard des minorités, il est essentiel d'en connaître l'ampleur ainsi que ses différentes sources. Dans cette thèse, nous exploitons à la fois des données expérimentales et des données de terrain pour détecter, mesurer, et étudier la discrimination à la première étape de l'attribution d'emplois et de logements locatifs dans les pays de l'OCDE.

Dans un premier temps, nous analysons le niveau et les sources de la discrimination à l'encontre des minorités ethniques dans le marché du logement locatif dans les pays de l'OCDE à travers une méta analyse de tests de correspondance. Les résultats de notre analyse montrent la présence d'une discrimination substantielle à l'encontre des minorités ethniques mais aussi à l'encontre des candidats masculins. Consécutivement à cela, nous réalisons une méta-analyse sur la discrimination à l'égard des candidats homosexuels dans le marché de l'emploi. En plus de révéler un niveau de discrimination similaire à celui subi par les minorités ethniques, notre étude permet de montrer des différences de traitement en fonction du genre des individus et du type d'emploi testé. Dans un troisième temps, nous réalisons une expérience de laboratoire pour étudier l'effet de l'intermédiation dans un contexte propice à la discrimination. Nos résultats indiquent que le plus faible niveau de discrimination observé empiriquement de la part des agents immobiliers n'est pas dû à l'intermédiation mais plutôt aux facteurs propres au marché. Enfin, nous réalisons une expérience de terrain afin de détecter la présence de discrimination à l'égard des personnes ayant un handicap mental, moteur ou visuel dans le marché du logement locatif en France. De manière cohérente avec la littérature sur le marché du travail, il semble que les personnes handicapées soient aussi la minorité la plus discriminée dans le marché du logement locatif.

Mots clés : Discrimination, testing, méta-analyse, marché du logement locatif, marché du travail, économie expérimentale

Abstract

Discrimination in the labor and rental housing market generates many economic and social inefficiencies. In order to combat discrimination against minorities, it is essential to know its extent and its different sources. In this thesis, we use both experimental and field data to detect, measure, and investigate discrimination at the first stage of the rental or the hiring process in OECD countries.

As a first step, we analyze the level and the sources of discrimination against ethnic minorities in the rental housing market in OECD countries through a meta-analysis of correspondence tests. Results show the presence of substantial discrimination against ethnic minorities and also against male tenants. Subsequent to this, we perform a meta-analysis on discrimination against openly homosexual candidates in the labor market. In addition to revealing a level of discrimination similar to that experienced by ethnic minorities, our study shows differences in treatment according to the gender of the individuals and the type of job tested. Thirdly, we carry out a laboratory experiment to study the effect of intermediation in a context conducive to discrimination. Our results indicate that the smaller level of empirically observed discrimination by real estate agents is not due to intermediation but rather to market-specific factors. Finally, we perform a field experience to detect the presence of discrimination against people with mental, motor or visual disabilities in the rental housing market in France. Consistent with the literature on the labor market, it appears that people with disabilities are also the most discriminated minority in the rental housing market.

Keywords: Discrimination, testing, meta-analysis, rental housing market, labor market, experimental economics