Essays in economics of crime prevention and behavior under uncertainty

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Essays in Economics of Crime Prevention and Behavior Under Uncertainty

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Proefschrift

Proefschrift ter verkrijging van de graad van doctor aan Tilburg University op gezag van de rector magnificus, prof. dr. W.B.H.J. van de Donk, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de aula van de Universiteit op vrijdag 3 februari 2023 om 10:00 uur door Gülbike Mirzaoğlu geboren te Konya, Turkije.

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To my mom and dad...

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[1]You might notice that the acronym I have used for this study (Chapter 1) was "LGBTQ+" since it was the commonly accepted term for the community as of 2015-2016. As society's understanding has grown and provided more room to represent more communities from diverse sexual and gender identities/expressions, so has its acronym.

[2]No offense, mom and dad, I love you. I leave to thank you at the end of this section.

[3]And many more.

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[4]I wish I could attach our childhood photo here, you know that I love it.

[5]Does this make you a hero? I really can't say... but, yes.

[6]And definitely much more fun.

[7]See: The Cambridge Notebooks (Mirzaoglu and Sahin, 2009, 2010, 2011) and any given conversation of us.

[8]Literally. If Google Search was a person, it would be you. I still do not know how you can do that.

[9]Notoriously the most challenging year for Ph.D. student in Economics.

Mom and dad, I know it was very hard for you to be miles apart from me for years. Even though you still do not know what exactly my research is about,[10] thanks for always encouraging and believing in me and yet reminding me that life is much more than a doctoral thesis. My sister Umay, my brother İlteriş, and my sister-in-law Ayşegül, and my brother-in-law Muzaffer, thanks for your support and understanding, even in the hardest times for you. And my lovely nieces and nephews: Bengisu, Mercan, Yusuf, Eren, and Erdem.[11] Thank you for your unconditional love. Thanks to you, I got a Ph.D. in being an aunt way before I got this doctorate diploma. I know we will all unite soon. I love you.

Finally, if you were expecting to see your name here, but I forgot, I am sorry for that, for whomever you are.

[10] Or somehow do not connect it with "Economics."

[11] The order of the names goes in descending order with respect to their ages, not with respect to how much I love them. I love them equally, but that's another topic of discussion.

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Introduction

This dissertation consists of a collection of three essays on topics in experimental economics and the economics of crime. Chapter 1 examines gender and sex differences behavior under uncertainty. Chapter 2 studies contagion in crime preventive behavior. Chapter 3 studies the crime-reducing effect of private crime prevention.

Are there any gender and sex differences when we decide under uncertainty? To what extent do gender and sex differ in this context? Do cisgenders behave differently than non-cisgenders? How does our reported behavior differ from our actual behavior? Chapter 1 studies a reconsideration of gender and sex differences in behavior under uncertainty. Uncertainty preferences govern many aspects of economic behavior, and extensive research has been carried out on understanding gender differences in risk and ambiguity. Previous studies in economics have divided individuals into binary categories in terms of gender and used "gender" and "sex" interchangeably. However, sex, as a biological category, differs from the very concept of gender, which is related to an imposed or adopted social and psychological condition. Therefore, the previous approach towards understanding behavior under uncertainty regards gender as a fixed, inevitable concept and does not consider its social aspects. More importantly, as this approach neglects gender minorities, the behavior of individuals who are not cisgender or do not fall into the binary category cannot be addressed. In this chapter, my aim was to study this important topic with a step toward having a more inclusive sample in terms of gender. For this, I have conducted an online experiment to elicit the risk and ambiguity preferences of the participants and analyze the concepts of sex, gender, and gender expression separately. Moreover, I have also analyzed the predictive power of the self-reported questions with regard to the incentivized tasks. This is the first study on uncertainty preferences that takes gender from a more inclusive perspective to the best of my knowledge. The experiment's findings vary for sex and gender in different domains and according to whether the task was incentivized or self-reported. No sex difference was found in ambiguity preferences; however, some gender difference was documented. Overall, individuals who did not conform to traditional gender/sex norms were less ambiguity averse than those who conformed. In addition, ceteris paribus, the results show that even though males reported themselves as being less averse to risk compared to females, no significant difference was evident in the incentivized risk task. Contrary to the current literature, it is not possible to conclude that women are more risk averse than men. The findings of this paper support that we need a more refined analysis of the gender concept in economics. Even though the small sample size of this study is one of the limitations in understanding the full dynamics of gender, it is important to see that when gender is controlled for in addition to sex, results can differ. Lastly, although obtaining single parameters for risk and ambiguity preferences can be practical from an interpretation or policy implementation perspective, those parameters should be interpreted with caution since individuals can act differently in different domains, and the results may depend on whether the situation is incentivized or self-reported.

Why certain visible crime prevention measures are highly popular in some areas but rarely used in other areas? Can the adoption of crime preventive measures socially contagious? Chapter 2 studies the extent to which the behavior of individuals is interrelated in terms of crime prevention. Explaining victim behavior is as important as explaining offender behavior to understand crime patterns. Similar to other contexts in daily life, individuals' decisions on how to prevent themselves from crime depend on other people's decisions, and individuals within a locality can feature common precautionary measures. Even though social contagion can provide one explanation for why certain crime prevention measures are highly popular in some areas but rarely used in other areas, its identification has received only limited attention in empirical work to date, most probably due to its notorious challenges. Using a detailed crime survey in the Netherlands, I study a striking geographical pattern in the adoption of specific crime preventive measures by households, roll-down windows and door shutters. This conspicuous measure against domestic burglary in the Netherlands is common in the south but rare in the north of the country. I analyze the extent to which the rate of adoption of roll-down shutters in one neighborhood is affected by the rate of adoption in bordering neighborhoods. To achieve identification, I impose an interaction structure between neighborhoods to examine how the adoption rate of roll-down shutters in a neighborhood is a function of the adoption in other neighborhoods. The results show that the rate of adoption of roll-down shutters in a neighborhood goes up by 0.39 percentage points when the adoption rate in surrounding neighborhoods increases by one percentage point. This indicates a strong presence of spatial spillover of roll-down shutters.

How can we prevent crime? What is the role of the "potential" victims and to what extent do their actions deter crime? Chapter 3[12] studies the crime-reducing effect of private crime preventive measures. Even though the reduction of criminal activities has been studied extensively, the evidence for a crime-reducing effect of private crime control by households has remained limited. This is important because the quality and quantity of the potential victims' self-protection measures

[12]This chapter is jointly written with Ben Vollaard.

can increase the likelihood of being targeted by an offender. In turn, potential victims can respond to crime by changing their self-protective measures. This interaction influences the volume and distribution of criminal activities. In this chapter, I investigate the burglary-reducing effect of a specific situational crime prevention measure, roll-down window and door shutters in the Netherlands. Building on Chapter 2, I exploit social contagion in victim preventive behavior as a source of exogenous variation. The results show that a one-percentage-point increase in the presence of roll-down window shutters lowers the rate of burglary victimization by an estimated 0.1 percentage point. To the best of my knowledge, this is the first study on the crime-reducing effect of window and door securities, and the first on exploiting social contagion in victim behavior to identify the effect of a situational crime-preventive measure. This is important because if situational components can discourage offenders, then some changes in the immediate environment can alter the proximal causes of crime. Then, this approach can be more socially progressive and less harmful to the offenders and victims than the traditional reactive 'law and order' justice approaches.

Chapter 1

A Reconsideration of Gender and Sex Differences in Behavior Under Uncertainty

Gülbike Mirzaoğlu[1]

Uncertainty preferences govern many aspects of economic behavior. In this study, we conducted an online experiment to elicit information on the risk and ambiguity preferences of the participants in different domains using both incentivized and self-reported tasks. Contrary to the previous literature, sex and gender differences in uncertainty attitudes were analyzed separately. This is important for being able to address the behavior of individuals who are not cisgender or do not fall into the binary category. Additionally, the predictive power of the self-reported questions with regard to the incentivized tasks was analyzed. No sex difference was found in ambiguity preferences; however, some gender difference was documented. In addition, ceteris paribus, the results show that even though males reported themselves as being less averse to risk compared to females, no significant difference was evident in the incentivized risk task. Non-cisgender individuals reported themselves as being more willing to take risk than cisgender individuals. To the best of our knowledge, this is the first study on uncertainty preferences that separates the concepts of sex and gender and looks at gender from a more inclusive perspective.

Risk Attitudes . Ambiguity Attitudes . Gender

[1]This paper is a single-authored paper. I confirm sole responsibility for the following: study conception and design, data collection, analysis and interpretation of results, and manuscript preparation. I greatly appreciate Gijs van de Kuilen for his supervison and feedback.

1.1. Introduction

It is widely recognized that choice under uncertainty can be understood by studying attitudes towards risk and ambiguity (Ellsberg, 1961). An uncertain environment is called "risky" when it can be represented by numerical probabilities and is called "ambiguous" if either the set of outcomes or the probability distributions over the event are unknown or only partially known (Knight, 2012).[2] Ambiguity and risk attitudes exhibit strong links to important economic phenomena such as preventive behavior, career advancement, the wage gap, financial decisions, precautionary saving, insurance decisions, criminal activities, and gambling (Ahn et al., 2014; Bonin et al., 2007; Croson and Gneezy, 2009; Powell and Ansic, 1997).

When it comes to the importance of the topic, decades have been devoted to understanding how gender roles determine attitudes toward uncertainty, with most of the lab and field studies in economics documenting that women avoid uncertain situations more than men do (Croson and Gneezy, 2009; Harris et al., 2006; Lindquist and Säve-Söderbergh, 2011). Notable exceptions include Nelson (2016), Filippin and Crosetto (2016), and Nelson (2015). Researchers also have a history of speculating on the underlying reasons for the documented findings. While some recent studies have speculated that the differences are caused by "natural" factors (i.e., biological or evolutionary factors),[3] a sizeable body of research has put forward that they originate in the contrasting distributions of gender roles in patriarchal society, social learning, and sociocultural constructs of gender.[4] These different explanations can have different policy implications: for example, in the suggestion that women and men be treated in categorically different ways since women are presumed to "naturally" refrain from uncertainty or in policies aimed at modifying social factors as a means of erasing the gender gap.

Within the scope of analyzing gender differences, most of the economics literature simply divides individuals into two categories and uses the terms "gender" and "sex" interchangeably and co-extensively: positing women versus men or females versus males (Krieger, 2003), or considering women as human females and men as human males (Mikkola, 2019).[5] This approach in previous

[2]A person can be classified as risk averse if she/he prefers the certain outcome prospect of any risky prospect with the same expected value (Kahneman and Tversky, 1979). A person can be classified as ambiguity averse if she/he prefers situations with known probabilities over options with vague probabilities (ambiguity) (Ellsberg, 1961).

[3]See Buss (2019); Harris et al. (2006); Tregenza and Wedell (2002).

[4]See Andersen et al. (2013); Booth and Nolen (2012); Hill and Lynch (1983); Rose and Rudolph (2006); Vandello et al. (2008).

[5]In addition to these categorizations, some studies have examined the relationship between hormones and behavior under uncertainty (Apicella et al., 2015; Rosenblitt et al., 2001). However, as will be explained below, measuring hormones and concluding that this indicates gender

studies that have analyzed "gender" differences in uncertainty preferences merits further investigation since it can be regarded as reductive and exclusionary for at least two reasons.

First, sex and gender are two different concepts, and they have been distinguished as such by scores of important social scientists, feminists, and other scholars across the humanities.[6] Using them interchangeably or equating the two can lead to bio-deterministic conclusions. Sex is a physiological classification that emphasizes the biological differences between males, females, and intersex individuals.[7] The markers of biological sex include the genotypes and phenotypes of individuals. On the other hand, gender is a complex and multidimensional construct. It is a psycho-sociocultural category that is related to an imposed or adopted social and psychological condition (Dea, 2016; Diamond, 2002). It is regarded as a social construct that goes beyond sex differences (Butler, 1986). It is also considered an "intended or unintended product of a social space" (Haslanger, 1995). It is not nature but society or culture that makes women, men, or others who they are (Beauvoir, 1952). It is a set of social standards and a continuum of gender identities and expressions. This complex structure encompasses characteristics of appearance, speech, movement, and other factors not solely limited to biological sex. All these determine how individuals are expected to think and act like women or men and can drive people to different social realities, life expectations, and economic circumstances. For example, a female with a feminine appearance, attire, and speech faces different social realities than a female who has a relatively masculine style. Therefore, the binary approach in identifying gender and equating it with sex can overlook socially given roles, responsibilities, expectations, activities, and the like.

Second, the binary classification of "gender" can exclude individuals who do not fall into the cisgender category,[8] that is, transgender,[9] genderqueer, or none-of-the-above individuals. These people can live their lives in a social gender that

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differences assumes that gender is an exclusively individual characteristic. According to this viewpoint, gender can seem like a set of traits or behavioral dispositions that people come to possess based on their assignment to a particular sex category (Wharton, 2009).

[6]One of the main feminist motivations for constructing a distinction between sex and gender was to counter the view that biology is destiny, or biological determinism (Holmes, 2007; Mikkola, 2019).

[7]Intersex refers to the individuals who are born with ambiguous genitalia, with sex chromosome abnormalities, or with some misalignment between their sex chromosomes and their anatomy (Dea, 2016).

[8]Cisgender refers to individuals whose gender identity matches their sex assigned at birth. For example, a person who is assigned male at birth and who identifies as male is cisgender (Schilt and Westbrook, 2009). This terminology can be helpful for emphasizing the conceptual symmetry of the One/Other relationship (Beauvoir, 1952) between cis and trans individuals (Dea, 2016).

[9]Transgender is the term for an individual whose gender identity does not align with their gender assignment at birth (Dea, 2016). Some might choose to change their physical appearance by undergoing cosmetic procedures/surgeries or using hormones. Others might not go

is not the sex they were assigned at birth (Schilt and Westbrook, 2009). If gender is analyzed solely from a binary perspective, "a subject who does not self-identify as male or female would simply be dropped from the investigation, which is an obvious lacuna in the research" (Nelson, 2016). Accordingly, non-conformist individuals fail to be addressed, and their needs and experiences will remain unknown.[10] Therefore, policies aimed at developing strategies to improve the standards of their lives will also presumably be insufficient.

Concerning the aforementioned points, this paper aims to measure individuals' risk and ambiguity preferences by following a broader and more inclusive concept of gender. Accordingly, an online experiment was conducted to examine uncertainty attitudes. The subjects were recruited by means of convenience sampling, with the help of several major LGBTQ+ organizations in the Netherlands and Belgium. This produced a rich data set in terms of including non-cisgender individuals in the research. Moreover, what is different from previous studies is that participants were able to reflect their gender identities and expressions in addition to their sex. To my knowledge, there has not yet been any study on uncertainty preferences that has explicitly considered gender from such a broader perspective.

Another question this paper addresses is whether self-reported survey data can predict the actual risk and ambiguity attitudes in the incentivized task. Individuals tend to report differently on their behavior when considering a hypothetically uncertain situation than when considering how they would act in an incentive-compatible experiment.[11] However, experiments with monetary rewards are costly, can be difficult to understand compared to straightforward survey results, and can be infeasible in a large representative sample. Given this, it is important to identify whether greater willingness to engage in uncertainty in the hypothetical parts maps into a greater risk/ambiguity seeking behavior in the incentivized experiments. The literature has focused on understanding risk preferences in

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through a physical transition but still decide to express their chosen gender identity in how they live(Lombardi and van Servellen, 2000).

[10] A systematic review of Collin et al. (2016) shows that the prevalence of gender identity that differed from the binary sex categories assigned at birth ranges from 4.5% to 0.01%. However, these numbers should be interpreted with caution because, to this date, very little is known about the number of people who identify as transgender or gender non-conforming individuals. One reason is that it is very difficult to know who fits this potentially vast category. Differences in methodology and variable definitions of transgender are greatly affected by reported prevalence estimates (Coleman et al., 2012; Lombardi, 2001). Another reason may be the understandable reluctance of stigmatized individuals to come out of the closet. It is also possible that these individuals have simply been ignored in research to date. For example, Schönpflug et al. (2018) examined thirty European national statistics institutes' web pages to see if and how "queers" are being "counted." They report that transgender and intersex persons are not even identified in national public statistics. Therefore, given the nature of the problem, we may never be able to come up with accurate numbers.

[11] See Camerer and Hogarth (1999) for a discussion.

this respect (Dohmen et al., 2011; Donkers et al., 2001; Vieider et al., 2015), while overlooking ambiguity preferences (Cavatorta and Schröder, 2019). Our study employs both monetarily incentivized tasks and hypothetical questions to reveal risk and ambiguity attitudes in different domains, whereby the extent to which the self-reported ambiguity and risk questions can predict the incentivized ambiguity and risk tasks is analyzed. Furthermore, people can differ in how they address risk in different domains (Hanoch et al., 2006; MacCrimmon et al., 1988; Weber and Johnson, 2009). For example, a person who avoids risky financial situations might engage in risky situations concerning their health. Therefore, instead of depending on a single parameter, the risk attitudes are measured in five content domains (Dohmen et al., 2011).

The findings for sex and gender vary in different domains and according to whether the task was incentivized or self-reported. On the ambiguity tasks, there was no significant sex difference. Transgender individuals were relatively less ambiguity seeking than others, and individuals with more feminine gender expression reported that they were willing to face more ambiguous situations. On the incentivized risk task, there was also no significant sex difference; but on the self-reported risk task, males reported being more willing to undertake risk than females, especially in the financial and health/safety domains. Cisgender individuals reported themselves as being less risk taking compared to others. Transgender individuals reported that they can take less risk in the social domain but more in the ethical and financial domains.

The rest of the paper is organized as follows. The next section presents the experimental design, the data, and the descriptive statistics. Results are provided in Section 1.3, followed in Section 1.4 by an analysis of the prediction incentivized tasks from self-reports. Section 1.5 discusses the findings, and Section 1.6 presents some conclusions. The self-reported survey questions and additional tables are presented in Appendix A.

1.2. Data and experimental design

The experiment was conducted in July 2017 in the Netherlands and April 2018 in Belgium through an online questionnaire presented in three languages: Dutch, English, and French. A total of 161 subjects participated in the study. They were recruited using convenience sampling, with the help of several major LGBTQ+ organizations in the Netherlands and Belgium in order to ensure an inclusive sample in terms of gender.[12] These organizations agreed to send the survey link

[12]These organizations include Transgender Netwerk Nederland, COC Nederland, Vereniging Genderdiversiteit, ÇAVARIA, and TIP. As the survey was conducted anonymously, we do not know which respondents were reached via LGBTQ+ organizations. Therefore, it is not possible to analyze these subjects separately and compare their responses.

to their members via email and/or post it on their social media accounts. That means the participants were not necessarily members of the organizations.

The estimation sample includes 146 respondents.[13] The sample of participants is diverse in terms of age, income, employment status, education, citizenship, and gender identity.[14] It contains 72 (49%) male and 74 (51%) female subjects, with an average age of 31. Of these, 90 participants identified as cisgender (62%), 33 as transgender (21%), and 23 as genderqueer/gender non-conforming (18%).[15] The subjects with a college degree have been classified as higher educated, and otherwise lower educated. The majority of the participants had a college degree, either a bachelor's (32%) or a master's (45%); 25% were employed either full or part-time; and 59% were students. Most of the subjects were Belgian citizens (60%), with some 35 Dutch citizens (24%) and the remainder from other parts of the world. The average household income, before taxes, was €20,000-€29,999. Table 1.1 provides the sample statistics in detail.

The survey consisted of five parts. The first part was designed to elicit information on the subjects' ambiguity preferences, and the second on their risk preferences. For these parts, participants were incentivized with a monetary reward. The third and fourth parts measured self-reported ambiguity and risk preferences. The last part of the survey was filled with demographic questions.

Individual ambiguity preferences were measured using a well-established incentivized decision task similar to the study by Ellsberg (1961). Participants were first asked to choose the color they wanted to bet on, either red or black. This ensures that subjects have no reason to believe that the experimenter has a strategic incentive to manipulate the color of the balls in the ambiguous urn (Charness et al., 2013b; Chow and Sarin, 2002). After choosing a color, the subjects were presented with a decision table containing 11 choices. Figure 1 shows the first three of 11 questions.[16] Each choice shows two urns containing exactly 10 balls, either red or black. One of the urns is a risky urn, that is, the composition of the balls is known and changes from one situation to the next. While the number of

[13] 15 participants completed the survey very quickly, i.e., in less than two minutes. The responses of these participants might not be reliable as they might not have read the instructions well or/and understood the questions correctly. Therefore, we drop these participants from the estimation sample.

[14] Due to the aim of this study, our sample cannot be considered a completely random sample. This is mainly because of two reasons. First, as discussed in Footnote 10, the estimated prevalence of gender identity that differed from the binary sex categories assigned at birth ranges from 4.5% to 0.01%. Since this study aims to focus on and include non-conforming individuals, we had to oversample them compared to a representative sample. Second, non-conforming individuals in our study might not be entirely representative as they can be different from other non-conforming individuals who do not/cannot come out. Considering the purpose of our study, these are inevitable limitations of the sample. Therefore, the results need to be interpreted with caution.

[15] Six respondents selected the "other" option; they are classified together with the genderqueer/gender non-conforming participants.

[16] The full set of questions is presented in Appendix A.

red balls increases incrementally from 0 to 10, the number of black balls decreases accordingly. The other urn is an ambiguous urn, that is, the composition of the balls is not known but identical in each situation.

For each of the 11 situations, the subjects were asked to indicate which urn they would prefer to draw a ball from. The attractiveness of the lottery changes monotonically from one situation to the other. The last point before they switch from the ambiguous urn to the risky urn, called the "switching point," reveals their ambiguity preferences. Earlier switching points indicate being relatively more ambiguity averse than later switching points. The participants who prefer the ambiguous urn over the risky urn when the winning probability of the risky urn is 50% or less are classified as "ambiguity averse" subjects; those who do not are considered "ambiguity seekers." It is typically expected that participants will choose the ambiguous urn over the risky urn when there is no chance of winning anything in the risky urn. This design allows for ambiguity preferences to be measured independent of the subject's utility function and risk preferences (Trautmann and van de Kuilen, 2015). In order to ensure the participants reveal their actual preferences, they are told that they can only win a monetary reward if the color they picked is drawn. The typical finding in the literature is that the subjects are ambiguity averse (Trautmann and van de Kuilen, 2015).

For the second part of the survey, aimed at eliciting information on the risk preferences of the participants, the preferred option was to use a binary choice list because this presents a simple method that subjects can easily understand (Wakker, 2010). Participants were presented with a decision table containing 11 situations, each of which offered either a chance to win a lottery or a guaranteed sum of money. While the lottery amount remained the same from one situation to the next, the sure payoff that was offered decreased each time. Subjects were asked to choose between the certain outcome and the risky prospect. If the participants chose the lottery, they had a 25% chance of winning €20 and a 75% chance of getting nothing. If they chose the guaranteed sum, they would get the amount of money indicated for that situation. The median of the lowest sure payoff for which a subject chooses the certain outcome and the highest sure payoff for which they choose the lottery is their certainty equivalent (Akay et al., 2012; Cubitt et al., 2020), with a higher certainty equivalent indicating less risk averse behavior. For example, since the expected value of the lottery was €5, weakly risk-averse subjects should start preferring the safe option over the lottery for payments of less than €5. Only risk seekers should opt for the lottery when the safe option being offered is higher than €5. The increments in the safe payouts and maximum value of €7 were chosen to keep the length of the choice table manageable and to allow for a finer grid for categorizing different degrees of risk aversion.

The third part consisted of 17 survey questions and was intended to measure the participants' self-reported ambiguity preferences. Measuring ambiguity preferences with attitudinal questions faces the challenge that the connotation of the

Table 1.1: Sample statistics

			Panel A: Sex & gender expression					
				Appearance			Mannerism	
	Observations	Percentage	very/mostly fem.	equally/somewhat fem./masc.	masc.	very/mostly fem.	equally/somewhat fem./masculine	masc.
female	74	50.68 %	30	30	14	30	33	11
male	72	49.32 %	18	17	37	18	20	34
Panel B: Gender identity			Panel C: Education			Panel D: Income		
	Observations	Percentage		Observations	Percentage		Observations	Percentage
Cisgender	90	61.64 %	Lower education	66	45.21 %	Less than €10,000	53	36.30%
Transgender	33	22.60%	Higher education	80	54.79 %	€10,000-€19,999	20	13.70%
Genderqueer/non-conf.	23	15.75%				€20,000-€29,999	20	13.70%
						€30,000-€39,999	19	13.01%
						€40,000-€49,999	11	7.53%
						€50,000 or more	23	15.75%
Panel E: Age			Panel F: Employment			Panel G: Nationality		
	Observations	Percentage		Observations	Percentage		Observations	Percentage
18-21	51	34.93%	Employed full time	19	13.01 %	Dutch	35	23.97%
22-29	50	34.25%	Employed part time	18	12.33 %	Belgian	88	60.27%
30-40	15	10.27%	Unemployed	14	9.58%	International	23	15.75 %
41-73	30	20.55%	Student	86	58.90 %			
			Retired/self-employed	9	6.16%			
Total	146	100.00%	Total	146	100.00%	Total	146	100.00%

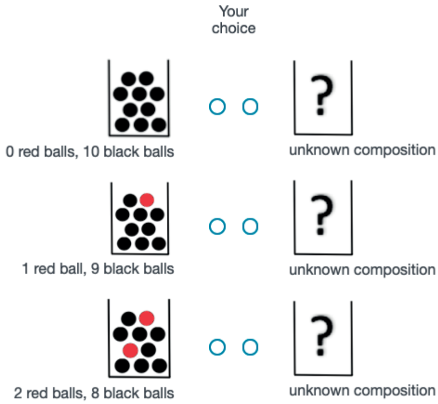

Figure 1.1: Incentivized ambiguity task (first 3 of 11 questions)

word "ambiguity" in everyday language is rather different from the notion of ambiguity in economics. The study by Cavatorta and Schröder (2019) used a set of questionnaires from various validated and renowned scales on ambiguity attitudes in psychology.[17] To keep the survey in the present experiment short and simple, the 17 questions used were randomly selected from the 46 self-reported attitudinal survey questions in Cavatorta and Schröder (2019).[18] These questions define ambiguity as the absence of the exact probabilities used in the economics literature. Ambiguity attitudes are related to optimism, pessimism, and self-esteem (Chateauneuf et al., 2007; Heath and Tversky, 1991), so the questions also aimed to measure those attitudes. Participants were asked to indicate the extent to which they agreed or disagreed with a given statement on a 7-point scale. In the scale, 1 corresponds to "Strongly disagree," 4 to "Neither agree or disagree," and 7 to "Strongly Agree." Statements participants were asked to measure included "When a situation is uncertain, I generally expect the worst to happen"; "When uncertain, I act very cautiously until I have more information about the situation"; and "I

[17]Since yet no study in economics explicitly uses questionnaires to measure ambiguity preferences, Cavatorta and Schröder (2019) resort to survey questions from self-reported ambiguity intolerance attitudinal scales in the psychology literature. Therefore, they selected questions from the Intolerance of Ambiguity Scale by Kirton (1981); the Ambiguity Tolerance Scales by Norton (1975); Stanley Budner (1962), and McLain (2009); the Uncertainty Response Scale by Greco and Roger (2001); the Extended Life Orientation Test by Chang et al. (1997); and a self-esteem measure by Robins et al. (2001), and also added some questions of their own.

[18]Cavatorta and Schröder (2019) examine whether the attitudinal questions can predict the incentivized ambiguity preferences. As the concept of ambiguity intolerance in psychology is more general than in economics, the attitudinal questions are not separately weighted or sub-classified in their analysis. Therefore, we resort to selecting them randomly to avoid any selection bias.

would like to live for a while in a foreign country that is new to me." To rule out potential order effects, the 17 questions were presented in random order.[19]

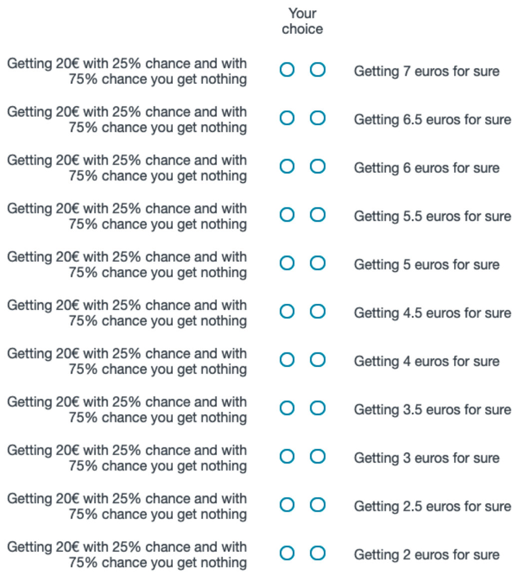

Figure 1.2: Incentivized risk task

Part four consisted of 16 questions to measure the subjects' self-reported and context-specific risk preferences. These were obtained from the domain-specific risk-taking (DOSPERT) scale in a 2006 paper by Blais and Weber (2006)[20] and were presented randomly. Of the 16 items, three each covered the domains of ethical, social, recreational, and health and safety risks and four covered the financial domain (Dohmen et al., 2011). Subjects were asked to indicate the likelihood that they would engage in a certain activity or behavior given the opportunity, using a 7-point scale ranging from "Extremely unlikely" (1) to "Extremely likely"

[19]We acknowledge that one disadvantage of this approach may be that if order effects exist, we can get additional noise, so the power of the study can decrease. Ideally, we could check whether there is an order effect by conducting a similar experiment on a different set of subjects. However, this would go beyond the scope of this study and therefore remains a limitation.

[20]Blais and Weber (2006) uses 30 questions to measure respondents' likelihood of engaging in risky behaviors originating from five domains. Ideally, we would use all of these questions, but we had to choose a subset from these questions to keep our experiment shorter and more straightforward. For example, the six financial items in the study of Blais and Weber (2006) can be split into three gambling and three investment items, resulting in narrower constructs. To keep our questionnaire comparable with theirs, we chose two from gambling and two from investment items and constructed four questions for the financial domain.

(7).[21] This part of the survey therefore evaluates the domain-specific nature of risk-taking behavior in an attempt to contravene the prevailing notion that risk taking is a stable trait whereby individuals show consistent risk-averse/risk-taking behavior across domains. This method makes it possible to observe, for example, whether a person who is risk averse in financial decisions is also risk averse in decisions affecting health outcomes or not. Lastly, the survey presented the third and fourth parts in a random order to control for potential order effects.

The fifth part consisted of demographic questions. Participants were asked about their age, highest level of education completed, citizenship, employment status, and estimated household income in the previous year, excluding taxes. We used the method proposed by the UCLA Williams Institute (Badgett et al., 2014) to ensure that measures for gender were included. These questions advance the development of sex and gender-related measures and include measurements of sex, gender identity, and transgender status. Following this method, participants were asked what sex they were assigned at birth (female or male) and then asked about their gender identities, that is, how they describe themselves: man, woman, trans-male/transman, trans-female/transwoman, genderqueer/gender non-conforming, or by some different identity. The genderqueer/gender non-conforming category refers to individuals whose gender expression does not fully conform to sex-linked social expectations like a masculine woman or feminine man. This category also includes people who identify as non-binary (e.g., gender-fluid) or may have no self-concept related to their gender identity. Transgender describes individuals whose current gender identity is not entirely congruent with their assigned sex at birth (Feinberg, 1996). Trans-male/transman refers to individuals who are assigned female at birth but identify as men, regardless of whether they have physically transitioned from female to male. Similarly, trans-female/transwoman refers to people who are assigned male at birth but identify as women, regardless of a physical change. On the other side, if an individual's gender identity matches their sex assigned at birth, that person is classified as cisgender (Badgett et al., 2014). Therefore, if a participant is a female and identifies as a woman, that participant is categorized as cisgender. Similarly, a person assigned male at birth who identifies as a man is cisgender. However, if a female subject identifies as a man, then that person is categorized as trans-male/transman. Lastly, in order to analyze their gender expression, the respondents were asked two additional questions: "On average, how do you think people would describe your appearance, style, or dress?" and "On average, how do you think people would describe your mannerisms (such as the way you walk or talk)?" They were asked to rate their responses on a 7-point scale from very feminine to very masculine. This provided us with complete information about the participants' sexes, gender identities, and gender expressions.

[21] The list is provided in Appendix A.

1.3. Results

1.3.1. Ambiguity tasks

Incentivized ambiguity task

Panels A and C of Table 1.2 show the sample statistics and the switching points of the participants. Although the subjects were expected to indicate only one switching point, 15% of the participants,[22] whom we call "inconsistent decision makers," switched from one choice to the other more than once. Following Falk et al. (2016) and Cavatorta and Schröder (2019), their average switching points are averaged and presented in Panel C. As Charness et al. (2013a) suggest, "such inconsistent behavior is difficult to rationalize under standard assumptions on preferences." That is, multiple-switching behavior suggests that a subject did not understand the task. Therefore, we report the summary statistics of the observations with multiple-switching behavior in Panel C, but we drop them from the main analysis.

As Table 1.2 makes evident, 40% of the participants chose the ambiguous urn over the risky urn when there was up to a 40% chance of winning from the risky urn. These subjects only switched to the risky urn on their next decision, when there was a 50% chance of winning. The mean switching point is 3.92, and 73% of the participants switched to the risky urn when the winning probability was less than 50%, indicating that the majority is ambiguity averse.[23] This is in line with the typical findings in the literature (Trautmann and van de Kuilen, 2015). Moreover, the average matching probability is 0.44, which is similar to Dimmock et al. (2016) and Cavatorta and Schröder (2019).[24]

To analyze the determinants of ambiguity aversion, we ran probit regressions of related demographic variables.[25] The dependent variable takes a value of 1 if the participant is an "ambiguity seeker (AS)" and 0 otherwise. Columns 1-3 of Table 1.3 present the results as marginal effects.

In order to distinguish between sex and gender effects, the first column shows the results when only controlling for the sex variable, not gender. Columns 2 and 3 present the regression results when controlling for gender identity and gender

[22]This proportion of multiple-switching behavior is in line with Filippin and Crosetto (2016).

[23]Cavatorta and Schröder (2019) found the mean switching point to be 4.90 in their study. As their experimental design is the same as ours, we compare our study with theirs. Using a 5%-level two-sided test, we find the power of our study to be 0.96.

[24]The matching probability is defined as the subjective probability that the decision maker will be indifferent about the two options. If the matching probability is larger than 0.5, the subject is ambiguity-seeking (Dimmock et al., 2015).

[25]These variables include sex, gender, employment status, income, education, age, and citizenship.

expression in addition to sex.[26] For ease of interpretation, the specification in Column 2 controlled for being cisgender, so that the comparison group is non-cisgender individuals (transgenders and genderqueers), and the one in Column 3 controlled for being transgender, hence the comparison group is non-transgender individuals (cisgenders and genderqueers). In addition to cisgender and transgender individuals, we also control for genderqueer individuals and present the results in Table 1.3A in Appendix A.

The main results suggest that there was no significant sex or gender difference in ambiguity-seeking behavior. However, the results in Table 1.3A in Appendix A show that genderqueer individuals are more willing to face ambiguous situations.[27] Moreover, highly educated individuals engaged in significantly more ambiguous decisions than less educated ones.

As a robustness check, the model was estimated with different specifications, including OLS regressions, in which the dependent variable is the switching point of the subject, and logistic regressions.[28] The extensive robustness checks show that the effects remain similar.

Until now, we have analyzed gender in two dimensions: gender identity, which consists of three categories, and gender expression. In order to see the dynamics of uncertainty for individuals who do not conform to traditional gender/sex norms more clearly, we have created a non-conformity category. If a respondent conforms to traditional gender/sex expectations, that is, born as female (male), identifies as a woman (man), and has a mostly/very feminine (masculine) appearance and mannerisms, then this respondent is considered in the conformity category, and non-conformity otherwise.[29] Table 1.3B in Appendix A presents the results by controlling for non-conforming individuals. The results suggest that individuals who do not conform to traditional gender/sex norms are more willing to face ambiguous situations, with or without controlling for sex.

[26]As gender and sex variables could be related, we check the potential multicollinearity issue. The Variance Inflation Factor and Condition Number statistics suggest that this is not a concern for our analysis.

[27]We also implement Romano-Wolf multiple hypothesis corrections to control the probability of rejecting at least one true null hypothesis in the hypotheses we test. Since this correction takes into account the dependence structure of the test statistics by resampling from the original data, it is considered more powerful than Bonferroni and Holm corrections Clarke et al. (2020). The results with this correction also suggest no significant sex or gender difference in ambiguity-seeking behavior.

[28]Available upon request.

[29]With this definition, %65 of the respondents fall into the non-conformity category in the estimation sample.

Table 1.2: Descriptive statistics for incentivized tasks

Panel A: Incentivized ambiguity task (Consistent DMs)			Panel B: Incentivized risk Task (Consistent DMs)		
Switching point	Observations	Fraction	Certainty equivalent	Observations	Fraction
0	5	4.03%	2	16	11.4%
1	5	4.03%	2.75	3	2.14%
2	7	5.65%	3.25	5	3.57%
3	24	19.4%	3.75	8	5.71%
4	50	40.3%	4.25	12	8.57%
5	21	16.9%	4.75	38	27.1%
6	7	5.65%	5	1	0.71%
8	1	0.81%	5.25	11	7.86%
10	4	3.23%	5.75	8	5.71%
			6.25	2	1.43%
			6.75	6	4.29%
			7	30	21.4%
Total	124	100.00%	Total	140	100.00%

Panel C: Summary statistics for incentivized ambiguity task

	Observations	Mean	Standard deviation	Lowest	Highest
Switch. point	124	3.92	1.77	0	10
Switch. point (incon.)	146	4.06	1.77	0	10

Panel D: Summary statistics for incentivized risk task

	Observations	Mean	Standard deviation	Lowest	Highest
Cert. equi.	140	4.93	1.57	2	7
Cert. equi. (incon.)	146	4.90	1.55	2	7

Notes: The table presents the descriptive statistics for incentivized ambiguity and risk tasks. Panels A and B show the switching points and certainty equivalences for the consistent subjects, respectively. Panels C and D show the summary statistics of the incentivized tasks for the consistent decision-makers and for all respondents (including the inconsistent decision-makers, who switched from one choice to the other more than once). The switching points of the inconsistent subjects have been averaged.

Table 1.3: Regression results for ambiguity tasks

VARIABLES	(1) AS	(2) AS	(3) AS	(4) ASSR	(5) ASSR	(6) ASSR
Sex	-0.0439	-0.0256	0.0079	-0.1486**	-0.0717	-0.0765
	(0.080)	(0.084)	(0.092)	(0.063)	(0.064)	(0.062)
Transgender			-0.0957			-0.0206
			(0.107)			(0.080)
Cisgender		-0.0936			-0.0803	
		(0.095)			(0.074)	
Gender expression		0.0201	0.0259		0.0382**	0.0393***
		(0.021)	(0.023)		(0.016)	(0.015)
Employed	-0.1337	-0.1718	-0.1188	0.0732	0.0095	0.0245
	(0.133)	(0.134)	(0.133)	(0.127)	(0.136)	(0.132)
Student	-0.0785	-0.0856	-0.0895	-0.1358	-0.1067	-0.1413
	(0.153)	(0.150)	(0.152)	(0.159)	(0.173)	(0.167)
Income	-0.0005	0.0002	0.0007	0.0166	0.0172	0.0189*
	(0.015)	(0.015)	(0.015)	(0.011)	(0.011)	(0.011)
High education	0.2393***	0.2550***	0.2336***	0.0959	0.1261**	0.1125*
	(0.074)	(0.073)	(0.074)	(0.064)	(0.062)	(0.061)
Age	0.0044	0.0037	0.0040	-0.0022	-0.0026	-0.0030
	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Dutch	0.0362	0.0307	0.0366	-0.2255	-0.1824	-0.1843
	(0.126)	(0.125)	(0.123)	(0.150)	(0.139)	(0.141)
Belgian	0.0471	0.0781	0.0479	-0.2635**	-0.2127*	-0.2307*
	(0.103)	(0.111)	(0.103)	(0.132)	(0.129)	(0.127)
Observations	124	124	124	146	146	146

Notes: Probit estimates on ambiguity attitudes. The results are reported in margins. The dependent variable takes a value of 1 if the subject is an "ambiguity seeker" and 0 otherwise. Columns 1-3 report the results for the incentivized task. Columns 4-6 present the results for the self-reported task. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Self-reported ambiguity task

In order to understand the determinants of the self-reported ambiguity task, and for ease of interpretation, a single index was created by totaling the scores for the responses of each participant to the 17 questions. It is called the "Ambiguity Seeking, Self-Reported " (ASSR) index. Since each question can take any value between 1 and 7, when the responses for 17 questions are added up, the minimum value a subject can receive is 17 and the maximum 119. The median of this range is 68. A subject who received a total value higher than 68 can thus be classified as an "ambiguity seeker." The ASSR in such a case was given a value of 1; otherwise, it was assigned a value of 0, for subjects classified as being "ambiguity averse."

Similar to the analysis in the previous section, the ASSR was regressed on demographic variables. Columns 4-6 of Table 1.3 show the probit regression results as marginal effects. When only the sex variable was controlled for, males were less willing to face ambiguous situations than females. However, once we controlled for gender, as well, the sex difference disappeared. The results in Table 1.3A in Appendix A show that genderqueer individuals report themselves as more willing to face ambiguous situations than cisgenders. Furthermore, participants with a more feminine appearance/mannerism tended to engage more in ambiguous situations.[30]; Belgian citizens reported themselves as being more cautious when they are in an ambiguous environment than international citizens; and individuals with higher education were relatively more willing to engage in ambiguity when controlled for gender.

For a robustness check, the analysis was conducted with different OLS and logistic regressions. The extensive robustness checks show that the effects are mainly in line.[31] Lastly, Table 1.3B in Appendix A presents the results by controlling for non-conforming individuals. The results suggest that individuals who do not conform to traditional gender/sex norms report being more willing to face ambiguous situations.

1.3.2. Risk tasks

Incentivized risk task

Panels B and D of Table 1.2 present the descriptive statistics for the incentivized risk task. As happened in the incentivized ambiguity task part, six subjects showed multiple-switching behavior. We follow the same approach as the previous section

[30]These results are robust to Romano-Wolf multiple hypothesis correction.

[31]The results from logit analysis are very similar to the probit analysis. In the OLS regression, even though the effect of gender expression remains robust, the effects of sex and other control variables disappear. This may be because of the polarized distribution of the outcome variable, i.e., most of the respondents' certainty equivalence is concentrated between 60 and 90. These tables are available upon request.

and only report their average switching point in Panel D;[32] we do not include "inconsistent decision makers" in the main analysis.

The results in Table 1.2 indicate that 60% of the subjects preferred a sure bet of less than €5 over the lottery, indicating that more than half of the participants are risk averse.[33] The mean certainty equivalent for the consistent subjects is 4.93. Since the expected value of the lottery is €5, subjects who start preferring the safe option over the lottery for payments of less than €5 can be considered risk averse.

The main regression results in marginal effects are presented in Columns 1-3 of Table 1.4 using probit modeling. The dependent variable takes a value of 1 if the certainty equivalence is more than the expected value of the lottery, indicating that the participant is less averse to risk. It takes a value of 0 otherwise.

The results show that there was no significant sex difference in risk-taking behavior, ceteris paribus. Once gender was also controlled for, the subjects who had more feminine gender expression tended to take less risk, and the transgender individuals were relatively more risk seeking than others.[34][35] Moreover, employed participants and students made more risky decisions in the incentivized risk task, and the willingness to take risk decreased with education. Lastly, Dutch citizens refrained from risk-taking behavior more than internationals.

The model was also estimated with other specifications, similar to the previous section. The results of the variables of interest are mainly in line with the main results.[36] Lastly, 1.4B in Appendix A presents the results by only controlling for non-conforming individuals. In that case, we do not see any effect for individuals who do not conform to traditional gender/sex norms.

Self-reported risk task

The results for this part were analyzed in a similar manner as with the self-reported ambiguity task. A single index, called the "Self-Reported Risk Taking"

[32]For these cases, the average switching point of the participant was calculated following Falk et al. (2016) and Cavatorta and Schröder (2019).

[33]In their well-known study, Akay et al. (2012) found that 76% of the subjects were risk averse. We compare our study with theirs. Using a 5%-level two-sided test, we find the power of our study to be 0.96.

[34]Since we analyze gender identity in three categories, this result shows that transgender individuals were relatively more risk-seeking than cisgender and genderqueer individuals, combined. Table 1.4B in Appendix A presents the results by also controlling for genderqueer individuals. We can see that transgender individuals were more risk seeking than cisgenders.

[35]These results are robust to Romano-Wolf multiple hypothesis correction, except for the results of the transgender variable. It is only robust with using resampled p-values.

[36]The results from logit analysis are very similar to the probit analysis. In the OLS regression, even though the effects of gender expression and being transgender remain robust, the effects of the control variables disappear, except the income variable. This may be because of the polarized distribution of the outcome variable, i.e., most of the respondents' certainty equivalence is concentrated between 4.25 and 5.75. These tables are available on request.

Table 1.4: Regression results for risk tasks

VARIABLES	(1) RT	(2) RT	(3) RT	(4) SRRT	(5) SRRT	(6) SRRT
Sex	-0.0540	-0.1544*	-0.1179	0.1746***	0.1585***	0.1845***
	(0.085)	(0.085)	(0.084)	(0.055)	(0.061)	(0.059)
Transgender		0.1942*			0.1059	
		(0.107)			(0.075)	
Cisgender			-0.0993			-0.1303**
			(0.097)			(0.062)
Gender expression		-0.0535***	-0.0463**		-0.0048	0.0004
		(0.021)	(0.020)		(0.015)	(0.015)
Employed	0.3720***	0.3787***	0.3752**	-0.0900	-0.0781	-0.1192
	(0.142)	(0.144)	(0.155)	(0.129)	(0.123)	(0.118)
Student	0.4015**	0.4494***	0.4530**	-0.0981	-0.0817	-0.0824
	(0.170)	(0.165)	(0.179)	(0.129)	(0.119)	(0.112)
Income	0.0150	0.0115	0.0124	-0.0333**	-0.0336**	-0.0333**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)
High education	-0.1871**	-0.1973**	-0.1837**	0.1314**	0.1418**	0.1546**
	(0.080)	(0.077)	(0.081)	(0.061)	(0.062)	(0.062)
Age	0.0081*	0.0091*	0.0098**	-0.0134***	-0.0151***	-0.0149***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Dutch	-0.2899**	-0.3171**	-0.3076**	-0.2943***	-0.2853***	-0.2894***
	(0.141)	(0.142)	(0.144)	(0.111)	(0.108)	(0.104)
Belgian	-0.1354	-0.1386	-0.1152	-0.1056	-0.1059	-0.0625
	(0.118)	(0.117)	(0.124)	(0.067)	(0.066)	(0.069)
Observations	140	140	140	146	146	146

Notes: Probit estimates on risk attitudes. The dependent variable takes a value of 1 if certainty equivalence is more than the expected value of the lottery, that is, the participant is less averse to risk, and 0 otherwise. Columns 1-3 report the results for the incentivized task. Columns 4-6 present the results for the self-reported task. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(SRRT) index, was created for the risk-taking variable by totaling the scores for the answers by each participant to the 16 questions. The sum can range from 16 to 112, with a median of 64. A subject is considered less averse to risk if she/he scores more than the median, whereby the SRRT is 1; otherwise, the subject is classified as risk averse and the SRRT is 0.

Columns 4-6 of Table 1.4 present the results of the probit model in terms of marginal effects. Ceteris paribus, males reported themselves as being greater risk takers than females. Being cisgender is associated with 13 percentage points less risk taking behavior.[37][38] Being Dutch decreased it by 28 percentage points compared to the international citizens. Older individuals and participants with a higher income also tended to take less risk. The effect of education on risk taking was positive.

As before, the model was also estimated OLS and logit regressions. The results are mainly in line with the main specification, except the cisgender variable in the OLS regression. Even though the size and magnitude of the cisgender variable in the OLS analysis is in line with the main results, it is not significant. Moreover, 1.4B in Appendix A presents the results by only controlling for non-conforming individuals. Similar to the previous section, we do not see any effect for individuals who do not conform to traditional gender/sex norms.

Additionally, since this part covered five different domains, each of them was analyzed separately. Subjects were classified as risk takers for a particular domain if they got a total value that was higher than the median and risk averse otherwise.[39]

The results of the probit regression for the financial, ethical, recreational, health/safety, and social domains are shown in Table 1.5. Males reported themselves as being greater risk takers than females in the financial and health/safety domains. Transgender individuals tended to take less risk in the social domain but more in the ethical and financial domains. Cisgender individuals took less risk in the financial and ethical domains and more in the social domain. And having a more feminine mannerism/appearance is associated with an increased probability of risk taking in the social and health/safety domains. Lastly, as before, we have also conducted analysis by controlling only for non-conforming individuals. Table

[37]As before, Table 1.4A in Appendix A presents the results by also controlling for genderqueer individuals. We can see that cisgender individuals reported to be less risk seeking than transgenders.

[38]These results are robust to Romano-Wolf multiple hypothesis correction, except for the results of the cisgender variable. It is only robust with using resampled p-values.

[39]Similar to the analyses of the previous parts, the binary variables were formed by adding up the responses for each domain and assigning 1 if the participant score was above the median and 0 otherwise. Since an answer to one question can take any value between 1 and 7, a participant's value for every domain except financial ranges from 3 to 21 after her/his responses are added up; in the financial domain, it can range from 4 to 28. The median is 16 for the financial domain and 12 for the rest.

1.5A in Appendix A presents the results. We do not see any significant effect for individuals who do not conform to traditional gender/sex norms.

1.4. Predicting incentivized tasks from self-reports

To what extent can answering questions regarding a hypothetical situation predict what happens in those situations when there are real consequences? This section describes how we used a stepwise selection procedure to separately analyze for both the ambiguity and risk tasks the predictive power of self-reported questions for incentivized behavior.

We used two iterative selection algorithms for this: forward and backward elimination procedures. Forward selection starts with a null model. At every iteration, it adds to the model the predictor that gives the most optimal criterion value when added. This is repeated until no additional predictor can be found to meet a certain criteria (i.e., F-statistic above a certain threshold). Backward selection is also an iterative procedure that starts with the full model composed of all predictors and at every iteration, removes from the model the predictor that performs the worst according to the chosen criteria. This procedure is repeated until no additional predictor is found that can be removed (Draper and Smith, 1998).[40]

1.4.1. Ambiguity tasks

Panel A of Table 1.6 presents the results from probit regressions of the ambiguity seeking variable on the self-reported ambiguity questions. The dependent variable is the incentivized "ambiguity seeking" variable and the independent variables are the selected items from the self-reported questions.[41] The first two columns show results for the questions that pass the significance level of 15%, and Columns 3 and 4 for those that pass 10%, with the results from backward item selection presented in Columns 1 and 3, and from forward item selection in Columns 2 and 4.

The questions that were chosen both by the backward and forwards item selections with the significance level set at 15% yielded three questions for the

[40]One drawback of these approaches is since the order of selection, or elimination greatly influences the process, these two methods might not end up selecting the same final models (Graybill, 1976). Another drawback of these approaches is that not all possible models are evaluated. See Falk et al. (2016), footnote 13.

[41]The full set of questions is presented in Appendix A.

Table 1.5: Parameter estimates on self-reported domain-specific risk task

VARIABLES	(1) F	(2) F	(3) F	(4) E	(5) E	(6) E	(7) H	(8) H	(9) H	(10) R	(11) R	(12) R	(13) S	(14) S	(15) S
Sex	0.2967***	0.3025***	0.2746***	0.0550	0.1023	0.0794	0.1839***	0.2779***	0.3010***	-0.0162	-0.0316	-0.0127	-0.1718**	-0.0768	-0.0225
	(0.059)	(0.060)	(0.061)	(0.054)	(0.063)	(0.065)	(0.065)	(0.071)	(0.069)	(0.072)	(0.073)	(0.077)	(0.070)	(0.072)	(0.075)
Transgender			0.1532*			0.1137*			-0.0889			-0.1254			-0.2192***
			(0.080)			(0.066)			(0.096)			(0.100)			(0.084)
Cisgender		-0.1502**			-0.1048*			0.0975			0.0763			0.1155	
		(0.075)			(0.062)			(0.086)			(0.085)			(0.080)	
Gender expression		0.0039	-0.0011		0.0214	0.0174		0.0450**	0.0491***		-0.0070	-0.0035		0.0595***	0.0703***
		(0.017)	(0.016)		(0.015)	(0.014)		(0.018)	(0.018)		(0.018)	(0.019)		(0.017)	(0.018)
Employed	-0.0074	-0.0606	-0.0420	-0.0894	-0.0893	-0.0574	0.2348	0.3025*	0.2705*	0.1097	0.1301	0.1011	0.0811	0.0921	0.0739
	(0.115)	(0.117)	(0.115)	(0.098)	(0.098)	(0.096)	(0.151)	(0.170)	(0.163)	(0.161)	(0.158)	(0.155)	(0.119)	(0.109)	(0.114)
Student	-0.0730	-0.0521	-0.0661	-0.0672	-0.0389	-0.0335	0.2427	0.2662*	0.2616*	0.0561	0.0455	0.0388	0.0359	0.0099	0.0096
	(0.135)	(0.126)	(0.124)	(0.113)	(0.098)	(0.097)	(0.149)	(0.157)	(0.153)	(0.152)	(0.156)	(0.158)	(0.136)	(0.124)	(0.136)
Income	-0.0123	-0.0129	-0.0139	-0.0318**	-0.0334**	-0.0334**	-0.0064	-0.0045	-0.0046	0.0105	0.0112	0.0121	0.0062	0.0125	0.0146
	(0.013)	(0.012)	(0.012)	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.013)
High education	0.1149*	0.1479**	0.1218*	0.1259**	0.1496**	0.1428**	0.1850***	0.1911***	0.1991***	0.0480	0.0326	0.0358	-0.0352	-0.0384	-0.0284
	(0.068)	(0.072)	(0.068)	(0.055)	(0.058)	(0.057)	(0.070)	(0.071)	(0.069)	(0.076)	(0.077)	(0.075)	(0.077)	(0.073)	(0.071)
Age	-0.0030	-0.0037	-0.0038	-0.0037	-0.0060	-0.0064	-0.0136**	-0.0165***	-0.0163***	-0.0097**	-0.0095**	-0.0084**	-0.0073	-0.0086**	-0.0078*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Dutch	-0.0444	-0.0283	-0.0414	0.0005	0.0153	0.0031	0.0203	0.0260	0.0371	-0.1428	-0.1428	-0.1393	-0.4860***	-0.4392***	-0.4423***
	(0.118)	(0.119)	(0.121)	(0.098)	(0.094)	(0.095)	(0.110)	(0.112)	(0.113)	(0.134)	(0.134)	(0.133)	(0.133)	(0.129)	(0.126)
Belgian	-0.0396	0.0087	-0.0284	0.0938	0.1380	0.1097	0.0187	-0.0061	0.0250	0.0124	-0.0144	0.0042	-0.5173***	-0.5063***	-0.4951***
	(0.091)	(0.098)	(0.093)	(0.080)	(0.089)	(0.085)	(0.090)	(0.091)	(0.089)	(0.101)	(0.103)	(0.100)	(0.101)	(0.102)	(0.100)
Observations	146	146	146	146	146	146	146	146	146	146	146	146	146	146	146

Notes: The table presents the results for the self-reported risk task of a probit regression with respect to different domains. Subjects are classified as risk takers in a particular domain if their total score is above the median and risk averse otherwise. Columns 1-3 show the results for the financial, Columns 4-6 for the ethical, Columns 7-9 for the health/safety, Columns 10-12 for the recreational, and Columns 13-15 for the social domain. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.6: Predicting the incentivized ambiguity task

VARIABLES	(1) BS	(2) FS	(3) BS	(4) FS
Panel A: Ambiguity task				
Q9	0.0448	0.0551*	0.0551*	
	(0.029)	(0.029)	(0.029)	
Q13	-0.0663**	-0.0641**	-0.0641**	
	(0.027)	(0.027)	(0.027)	
Q17	0.0445			
	(0.027)			
Q6	-0.0515**	-0.0405*	-0.0405*	-0.0419*
	(0.024)	(0.024)	(0.024)	(0.024)
Q5	0.0364			
	(0.025)			
Observations	124	124	124	124
Panel B: Risk task				
S1	-0.0703**	-0.0670**	-0.0629**	-0.0629**
	(0.028)	(0.027)	(0.027)	(0.027)
H2	-0.0396*	-0.0334		
	(0.024)	(0.022)		
E1	0.0448			
	(0.029)			
F1	0.0512**	0.0532**	0.0408*	0.0408*
	(0.025)	(0.024)	(0.023)	(0.023)
F2	0.0366*			
	(0.021)			
E3	-0.0463*			
	(0.028)			
Observations	140	140	140	140

Notes: The table presents the probit regressions of the ambiguity seeking (Panel A) and risk taking (Panel B) variables from the incentivized task on the self-reported ambiguity questions. The results are reported in margins. Columns 1 and 3 present the questions from the backward item selection model, and Columns 2 and 4 from the forward item selection model. The significance level for model selection is 15% for Columns 1-2 and 10% for Columns 3-4. Standard errors are robust to heteroskedasticity and reported in parentheses.
** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Q6: "When uncertain, I act very cautiously until I have more information about the situation." Q9: "I generally prefer novelty over familiarity." Q13: "I enjoy unexpected events." S1: "Admitting that your tastes are different from those of a friend." F1: "Investing 10% of your annual income in a moderate growth mutual fund."

Table 1.7: Cross-correlations among risk tasks

	Financial	Ethical	Recrational	Health	Social	Certainty equivalence
Financial	1					
Ethical	0.226**	1				
Recrational	0.0634	0.334***	1			
Health	0.160	0.476***	0.359***	1		
Social	-0.270**	0.123	0.371***	0.211*	1	
Certainty equivalence	0.197*	0.157	0.0323	0.0750	-0.0726	1
Observations	140					

Notes: The table presents pairwise correlations among different risk domains in the self-reported task and certainty equivalence in the incentivized task.
** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

self-reported ambiguity task (Questions 6, 9, and 13). At the 10% significance level, only Question 6 was favored by both methods.[42]

To assess the quality of the "best" predictors, we then analyzed the degree to which the selected questions captured the results on the incentivized tasks, which is to say, the in-sample correlation between the predicted ambiguity preferences using the "best" predictors (i.e., fitted values) and the ambiguity preference benchmark.[43] The correlations range from 15% when only Question 6 is used to 26% when all three questions are used. These correlations are sizable and significant.

1.4.2. Risk tasks

As a first step, the pairwise correlations among the incentivized and self-reported tasks were calculated. For this, the questions were analyzed with respect to the different domains: social (S), health/safety (H), ethics (E), recreational (R), and financial (F). Responses for each participant were totaled for each category.

[42] The study by Cavatorta and Schröder (2019) selected a different set of questions as the best proxy for ambiguity preferences. This is most probably because, as explained in Section 1.2, we used only a randomly selected subset from their complete questionnaire for this study. Therefore, the questions they selected as the best proxy do not necessarily overlap with the 17 questions used in this study.

[43] While adding more predictors improves the explanatory power in-sample, but can worsen the ability to predict preferences for another subject pool Cavatorta and Schröder (2019). Therefore, in addition to the in-sample correlations, ideally, we would test the predictive power of the self-reported questions with an entirely different sample of subjects of similar size. Even though this would enable us to get a more reliable prediction of the incentivized task, this goes beyond the scope of this study and so remains to be a limitation.

Table 1.7 presents the pairwise correlations among the domains and the certainty equivalence, which is measured from the incentivized risk task.[44]

Interestingly, the correlations across the self-reported domains vary from -0.27 to 0.47, giving a strong indicator that risk-taking behavior can be domain specific. Risk taking in the health domain is strongly correlated with the ethical and recreational domains. Financial risk taking is negatively correlated with the social domain but positively correlated with the ethical domain. The correlations between the aggregated self-reported risk-taking domains and the certainty equivalences for the incentivized risk task range from -0.07 to 0.197, suggesting that people might exhibit different behaviors when a task is incentivized versus when it is self-reported.

To explore which questions from the self-reported risk task had the greatest explanatory power, we used an approach similar to that explained in the previous section. Panel B of Table 1.6 presents the results for the questions that were chosen using backward and forward item selection at significance levels of 15% and 10%, with the regression results as marginal effects using probit modeling presented in each column. The dependent variable takes a value of 1 if the certainty equivalence is greater than the expected value of the lottery and 0 otherwise. The independent variables are the items selected from the self-reported questions.

Three questions were chosen that pass the significance level of 15%: from social, health, and financial domains. At the 10% significance level, however, only two questions were chosen ("Admitting that your tastes are different from those of a friend" and "Investing 10% of your annual income in a moderate growth mutual fund").

As before, in order to assess the quality of the "best" predictors, we analyzed the degree to which these questions captured the behavior on the incentivized tasks, which is to say, the in-sample correlation between the predicted risk preferences using the "best" predictors (i.e., fitted values) and the risk preference benchmark.

The correlation of the model for risk preferences is 27% with three selected questions and 24% with only one question. These correlations are sizable and significant. It can be concluded that the best question from the self-reported risk task in terms of explaining the incentivized behavior is from the social domain.

1.5. Discussion

Uncertainty preferences are central to many economic behaviors. Although extensive research has been carried out on understanding gender differences in

[44]The sample used in this table includes only the consistent decision-makers in the incentivized risk task, as the estimation sample for that task excludes the "inconsistent" subjects. Nevertheless, the magnitude and significance of the correlation coefficients are very similar when the inconsistent decision makers are also included.

risk and ambiguity preferences, previous studies have divided individuals into binary categories in terms of gender and used "gender" and "sex" interchangeably. However, the relationships between the biological domain of sex and the pyscho-sociocultural domain of gender are not that tidy or intuitive (Dea, 2016). Sex, as a biological category, differs from the very concept of gender, which is related to an imposed or adopted social and psychological condition. Moreover, the variation in the expression of gender is much broader over time and place than that in sex traits, which would not be the case if sex and gender were the same thing (Dea, 2016). Equating gender and sex not only regards gender as fixed and inevitable, but also neglects the social aspects of gender. More importantly, gender minorities of all backgrounds encounter discriminatory practices in a wide array of settings. When sex and gender are used interchangeably, non-conformist individuals are left out of the research, so their needs and experiences remain undiscovered to the field of academia and policymakers.

This paper offers valuable insights into gender and sex differences in uncertainty attitudes. Contrary to previously published studies, we obtained inclusive data in terms of gender, with respondents given the opportunity to reflect their sex, gender identity, and gender expression. Accordingly, sex and gender differences were analyzed separately in this study. The risk and ambiguity aversion parameters were derived using both monetarily incentivized and self-reported tasks. To the best of my knowledge, this is the first study on uncertainty preferences to treat gender from a more inclusive perspective.

The findings vary according to sex and gender, to whether the task is incentivized or self-reported, and in different domains. There was no significant sex difference documented in the ambiguity tasks. The literature shows mixed evidence on this. While some found no difference in ambiguity aversion between the sexes ((Bianchi and Tallon, 2019; Dimmock et al., 2016; Sutter et al., 2015), others have deduced that females become more ambiguity averse as the ambiguity of a situation increases (Hudgens and Fatkin, 1985; Powell and Ansic, 1997). We found in our study that once gender is controlled for in addition to sex, genderqueer individuals and individuals with more feminine gender expression reported that they were willing to face more ambiguous situations. Overall, individuals who did not conform to traditional gender/sex norms were less ambiguity averse than those who conformed, both in incentivized and self-reported tasks.

In terms of risk aversion, even though there was no significant sex difference in the incentivized risk task, males reported themselves as being more willing to take risks than females in the financial and health domains. Meanwhile, subjects with more feminine gender expression took less risk in the incentivized task. As a whole, it is not possible to conclude, contrary to the current literature Borghans et al. (2009); Holt and Laury (2002), that women are more risk averse than men.

Three self-reported questions were selected through elimination procedures to explain behavior on the incentivized ambiguity task, and the question with the most explanatory power was "When uncertain, I act very cautiously until I have more information about the situation." For the risk tasks, the self-reported questions in the financial and social domains had the greatest explanatory power for the incentivized behavior. However, the results should be taken with caution as the approach has multiple limitations. Therefore, using only these specific self-reported questions would not be enough to understand the behavior under uncertainty. Nevertheless, the results hint at the direction that some questions can have more predictive power, which could help future research when implementing experiments with monetary rewards is not feasible.

1.6. Conclusion

Three main conclusions can be deduced from this paper. First, we need a more refined analysis of the gender concept in economics. This is important for all the reasons outlined above. Authors of previous studies on uncertainty preferences have been unable to include a broader definition of gender in their work because they lacked the kind of rich data we used in our research. Further study focusing more on the fact in question by analyzing gender in a broader sense and targeting a more inclusive set of data is therefore recommended.

Second, the fact that there may not be a sex difference in uncertainty preferences does not mean there are no gender differences or vice versa. Even though the small sample size of this study does not provide sufficient variation for understanding the full dynamics of gender, it is important to see that when gender is controlled for in addition to sex, results can differ. For example, if gender is not controlled for, it becomes impossible to know that transgender individuals are less willing to get involved in ambiguous situations and cisgender individuals report themselves as being less of a risk taker.

Lastly, as Weber and Johnson (2009) suggest, there is no single measure of "risk attitude" that can be inferred from observed levels of risk taking. Thus, although obtaining single parameters for risk and ambiguity preferences can be practical from an interpretation or policy implementation perspective, those parameters should be interpreted with caution, since individuals can act differently in different domains, and the results may depend on whether the situation is incentivized or self-reported. Nevertheless, the results of this paper can be used in future studies when conducting monetarily incentivized experiments might be impractical or infeasible.

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Appendix A

Appendix: A Reconsideration of Gender and Sex Differences in Behavior Under Uncertainty

A.1. Questions from self-reported tasks

A.1.1. Self-reported ambiguity task questions

- Question 1: I would like to live for a while in a foreign country that is new to me.
- Question 2: I find it hard to make a choice when the outcome is uncertain.
- Question 3: I am tolerant of ambiguous situations.
- Question 4: I prefer to stick to tried and tested ways of doing things.
- Question 5: I avoid situations that are too complicated for me to easily understand.
- Question 6: When uncertain, I act very cautiously until I have more information about the situation.
- Question 7: I like movies or stories with definite endings.
- Question 8: I voluntarily accept new challenges.
- Question 9: I generally prefer novelty over familiarity.
- Question 10: When making a decision, I am deterred by the fear of making a mistake.

- Question 11: When a situation is uncertain, I generally expect the worst to happen.
- Question 12: I find the prospect of change exciting and stimulating.
- Question 13: I enjoy unexpected events.
- Question 14: Vague and impressionistic pictures appeal to me more than realistic pictures.
- Question 15: I like parties where I know most of the people more than the ones where all or most of the people are complete strangers.
- Question 16: A person who leads an even, regular life in which few surprises or unexpected happenings arise, really has a lot to be grateful for.
- Question 17: I like to fool around with new ideas, even if they turn out later to be a total waste of time.

A.1.2. Self-reported risk task questions

F: financial, S: social, H: health/safety, R: recreational, E: ethical

- F1: Investing 10% of your annual income in a moderate growth mutual fund.
- F2: Betting a day's income at a high-stake poker game
- F3: Investing 5% of your annual income in a very speculative stock.
- F4: Betting a day's income on the outcome of a sporting event.
- S1: Admitting that your tastes are different from those of a friend.
- S2: Starting a new career in your mid-thirties.
- S3: Disagreeing with an authority figure on a major issue
- H1: Drinking heavily at a social function
- H2: Engaging in unprotected sex.
- H3: Driving a car without wearing a seat belt.
- R1: Going down a ski run that is beyond your ability.
- R2: Taking a skydiving class.
- R3: Going whitewater rafting at high water in the spring.
- E1: Having an affair with a married man/woman.
- E2: Revealing a friend's secret to someone else.
- E3: Passing off somebody else's work as your own.

A.2. Incentivized ambiguity task: The full set of questions

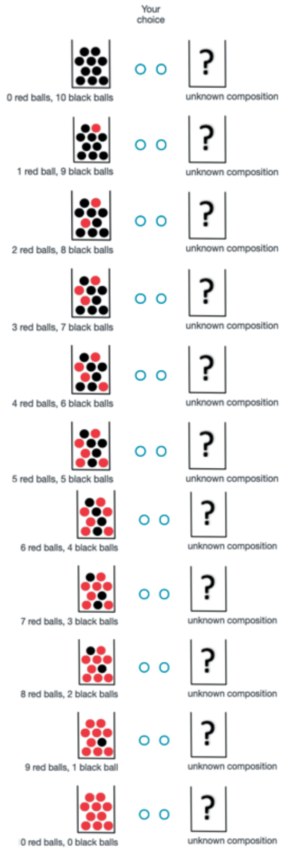

Figure A.1: The full set of questions of the incentivized ambiguity task

A.3. Tables

Table 1.3B: Parameter Estimates of Ambiguity Tasks

VARIABLES	(1) AS	(2) AS	(3) AS	(4) SRAS	(5) SRAS	(6) SRAS
Sex	-0.0439		-0.0048	-0.1486**		-0.1078*
	(0.080)		(0.082)	(0.063)		(0.061)
Non-conformity		0.2180**	0.2169**		0.1628**	0.1207*
		(0.088)	(0.089)		(0.064)	(0.065)
Employed	-0.1337	-0.1740	-0.1745	0.0732	0.0184	0.0348
	(0.133)	(0.129)	(0.129)	(0.127)	(0.121)	(0.124)
Student	-0.0785	-0.0424	-0.0431	-0.1358	-0.0928	-0.1011
	(0.153)	(0.144)	(0.144)	(0.159)	(0.152)	(0.158)
Income	-0.0005	-0.0028	-0.0027	0.0166	0.0128	0.0158
	(0.015)	(0.015)	(0.015)	(0.011)	(0.011)	(0.011)
High Education	0.2393***	0.2801***	0.2800***	0.0959	0.1268**	0.1206*
	(0.074)	(0.074)	(0.074)	(0.064)	(0.064)	(0.064)
Age	0.0044	0.0036	0.0036	-0.0022	-0.0033	-0.0023
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Dutch	0.0362	0.0732	0.0730	-0.2255	-0.1798	-0.1843
	(0.126)	(0.125)	(0.126)	(0.150)	(0.139)	(0.144)
Belgian	0.0471	0.1221	0.1207	-0.2635**	-0.1952	-0.2066
	(0.103)	(0.109)	(0.111)	(0.132)	(0.128)	(0.132)
Observations	124	124	124	146	146	146

Notes: Probit estimates on ambiguity attitudes. The results are reported in margins. The dependent variable takes a value of 1 if the subject is an "ambiguity seeker" and 0 otherwise. Columns 1-5 report the results for the incentivized task. Columns 6-10 present the results for the self-reported task. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.3A: Parameter Estimates of Ambiguity Task

VARIABLES	(1) AT	(2) AT	(3) AT	(4) AT	(5) AT	(6) SRAT	(7) SRAT	(8) SRAT	(9) SRAT	(10) SRAT
Sex	-0.0439	0.0079	0.0439	-0.0256	0.0439	-0.1486**	-0.0765	-0.0591	-0.0717	-0.0591
	(0.080)	(0.092)	(0.085)	(0.084)	(0.085)	(0.063)	(0.062)	(0.062)	(0.064)	(0.062)
Transgender		-0.0957	-0.0282				-0.0206	0.0279		
		(0.107)	(0.113)				(0.080)	(0.082)		
Genderqueer			0.2227*		0.2509**			0.2030*		0.1752
			(0.117)		(0.123)			(0.115)		(0.128)
Cisgender				-0.0936	0.0282				-0.0803	-0.0279
				(0.095)	(0.113)				(0.074)	(0.082)
Gender Expression		0.0259	0.0324	0.0201	0.0324		0.0393***	0.0418***	0.0382**	0.0418***
		(0.023)	(0.022)	(0.021)	(0.022)		(0.015)	(0.014)	(0.016)	(0.014)
Employed	-0.1337	-0.1188	-0.1652	-0.1718	-0.1652	0.0732	0.0245	0.0013	0.0095	0.0013
	(0.133)	(0.133)	(0.130)	(0.134)	(0.130)	(0.127)	(0.132)	(0.125)	(0.136)	(0.125)
Student	-0.0785	-0.0895	-0.0729	-0.0856	-0.0729	-0.1358	-0.1413	-0.0990	-0.1067	-0.0990
	(0.153)	(0.152)	(0.149)	(0.150)	(0.149)	(0.159)	(0.167)	(0.157)	(0.173)	(0.157)
Income	-0.0005	0.0007	-0.0005	0.0002	-0.0005	0.0166	0.0189*	0.0175*	0.0172	0.0175*
	(0.015)	(0.015)	(0.014)	(0.015)	(0.014)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
High Education	0.2393***	0.2336***	0.2691***	0.2550***	0.2691***	0.0959	0.1125*	0.1340**	0.1261**	0.1340**
	(0.074)	(0.074)	(0.074)	(0.073)	(0.074)	(0.064)	(0.061)	(0.063)	(0.062)	(0.063)
Age	0.0044	0.0040	0.0049	0.0037	0.0049	-0.0022	-0.0030	-0.0019	-0.0026	-0.0019
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Dutch	0.0362	0.0366	0.0250	0.0307	0.0250	-0.2255	-0.1843	-0.1938	-0.1824	-0.1938
	(0.126)	(0.123)	(0.121)	(0.125)	(0.121)	(0.150)	(0.141)	(0.137)	(0.139)	(0.137)
Belgian	0.0471	0.0479	0.1107	0.0781	0.1107	-0.2635**	-0.2307*	-0.1944	-0.2127*	-0.1944
	(0.103)	(0.103)	(0.110)	(0.111)	(0.110)	(0.132)	(0.127)	(0.130)	(0.129)	(0.130)
Observations	124	124	124	124	124	146	146	146	146	146

Notes: Probit estimates on ambiguity attitudes. The results are reported in margins. The dependent variable takes a value of 1 if the subject is an "ambiguity seeker" and 0 otherwise. Columns 1-5 report the results for the incentivized task. Columns 6-10 present the results for the self-reported task. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.4A: Parameter Estimates of Risk Task

VARIABLES	(1) RT	(2) RT	(3) RT	(4) RT	(5) RT	(6) SRRT	(7) SRRT	(8) SRRT	(9) SRRT	(10) SRRT
Sex	-0.0540	-0.1544*	-0.1555*	-0.1179	-0.1555*	0.1746***	0.1585***	0.1851***	0.1845***	0.1851***
	(0.085)	(0.085)	(0.086)	(0.084)	(0.086)	(0.055)	(0.061)	(0.062)	(0.059)	(0.062)
Transgender		0.1942*	0.1901*				0.1059	0.1288*		
		(0.107)	(0.112)				(0.075)	(0.073)		
Genderqueer			-0.0117		-0.2018			0.1323		0.0036
			(0.131)		(0.143)			(0.093)		(0.108)
Cisgender				-0.0993	-0.1901*				-0.1303**	-0.1288*
				(0.097)	(0.112)				(0.062)	(0.073)
Gender Expression		-0.0535***	-0.0537***	-0.0463**	-0.0537***		-0.0048	0.0005	0.0004	0.0005
		(0.021)	(0.021)	(0.020)	(0.021)		(0.015)	(0.016)	(0.015)	(0.016)
Employed	0.3720***	0.3787***	0.3810**	0.3752**	0.3810**	-0.0900	-0.0781	-0.1202	-0.1192	-0.1202
	(0.142)	(0.144)	(0.149)	(0.155)	(0.149)	(0.129)	(0.123)	(0.122)	(0.118)	(0.122)
Student	0.4015**	0.4494***	0.4474***	0.4530**	0.4474***	-0.0981	-0.0817	-0.0828	-0.0824	-0.0828
	(0.170)	(0.165)	(0.164)	(0.179)	(0.164)	(0.129)	(0.119)	(0.112)	(0.112)	(0.112)
Income	0.0150	0.0115	0.0115	0.0124	0.0115	-0.0333**	-0.0336**	-0.0333**	-0.0333**	-0.0333**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)
High Education	-0.1871**	-0.1973**	-0.1990**	-0.1837**	-0.1990**	0.1314**	0.1418**	0.1546**	0.1546**	0.1546**
	(0.080)	(0.077)	(0.080)	(0.081)	(0.080)	(0.061)	(0.062)	(0.062)	(0.062)	(0.062)
Age	0.0081*	0.0091*	0.0090*	0.0098**	0.0090*	-0.0134***	-0.0151***	-0.0148***	-0.0149***	-0.0148***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Dutch	-0.2899**	-0.3171**	-0.3181**	-0.3076**	-0.3181**	-0.2943***	-0.2853***	-0.2897***	-0.2894***	-0.2897***
	(0.141)	(0.142)	(0.143)	(0.144)	(0.143)	(0.111)	(0.108)	(0.101)	(0.104)	(0.101)
Belgian	-0.1354	-0.1386	-0.1423	-0.1152	-0.1423	-0.1056	-0.1059	-0.0618	-0.0625	-0.0618
	(0.118)	(0.117)	(0.124)	(0.124)	(0.124)	(0.067)	(0.066)	(0.071)	(0.069)	(0.071)
Observations	140	140	140	140	140	146	146	146	146	146

Notes: Probit estimates on risk attitudes. The dependent variable takes a value of 1 if certainty equivalence is more than the expected value of the lottery, that is, the participant is less averse to risk, and 0 otherwise. Columns 1-3 report the results for the incentivized task. Columns 4-6 present the results for the self-reported task. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.4B: Parameter Estimates of Risk Tasks

VARIABLES	(1) RT	(2) RT	(3) RT	(4) SRRT	(5) SRRT	(6) SRRT
Sex	-0.0540		-0.0654	0.1746***		0.1813***
	(0.085)		(0.085)	(0.055)		(0.058)
Non-conformity		-0.0283	-0.0480		-0.0212	0.0286
		(0.095)	(0.096)		(0.066)	(0.064)
Employed	0.3720***	0.3752**	0.3869***	-0.0900	-0.0759	-0.0982
	(0.142)	(0.149)	(0.149)	(0.129)	(0.134)	(0.129)
Student	0.4015**	0.3924**	0.3974**	-0.0981	-0.0848	-0.0975
	(0.170)	(0.173)	(0.172)	(0.129)	(0.139)	(0.127)
Income	0.0150	0.0131	0.0155	-0.0333**	-0.0284**	-0.0337**
	(0.015)	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)
High Education	-0.1871**	-0.1908**	-0.1961**	0.1314**	0.1268**	0.1395**
	(0.080)	(0.082)	(0.082)	(0.061)	(0.063)	(0.061)
Age	0.0081*	0.0076	0.0084*	-0.0134***	-0.0115**	-0.0138***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Dutch	-0.2899**	-0.2872**	-0.3025**	-0.2943***	-0.2933**	-0.2936***
	(0.141)	(0.142)	(0.141)	(0.111)	(0.117)	(0.112)
Belgian	-0.1354	-0.1367	-0.1541	-0.1056	-0.1356*	-0.0940
	(0.118)	(0.124)	(0.122)	(0.067)	(0.075)	(0.071)
Observations	140	140	140	146	146	146

Notes: Probit estimates on risk attitudes. The dependent variable takes a value of 1 if certainty equivalence is more than the expected value of the lottery, that is, the participant is less averse to risk, and 0 otherwise. Columns 1-3 report the results for the incentivized task. Columns 4-6 present the results for the self-reported task. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.5A: Estimates of Self-Reported Domain Specific Risk Task

VARIABLES	(1) F	(2) F	(3) F	(4) E	(5) E	(6) E	(7) H	(8) H	(9) H	(10) R	(11) R	(12) R	(13) S	(14) S	(15) S
Sex	0.2967***		0.2944***	0.0550		0.0709	0.1839***		0.1653**	-0.0162		-0.0162	-0.1718**		-0.1768**
	(0.059)		(0.060)	(0.054)		(0.055)	(0.065)		(0.068)	(0.072)		(0.074)	(0.070)		(0.074)
Non-conformity		-0.0935	-0.0119		0.0394	0.0610		-0.1254*	-0.0760		0.0049	-0.0001		0.0386	-0.0199
		(0.085)	(0.081)		(0.063)	(0.064)		(0.073)	(0.075)		(0.082)	(0.084)		(0.086)	(0.089)
Employed	-0.0074	0.0210	-0.0038	-0.0894	-0.0971	-0.0999	0.2348	0.2598	0.2519*	0.1097	0.1091	0.1097	0.0811	0.0790	0.0849
	(0.115)	(0.128)	(0.120)	(0.098)	(0.100)	(0.101)	(0.151)	(0.172)	(0.152)	(0.161)	(0.162)	(0.163)	(0.119)	(0.121)	(0.122)
Student	-0.0730	-0.0824	-0.0748	-0.0672	-0.0600	-0.0587	0.2427	0.2227	0.2318	0.0561	0.0572	0.0561	0.0359	0.0634	0.0308
	(0.135)	(0.144)	(0.134)	(0.113)	(0.111)	(0.110)	(0.149)	(0.168)	(0.150)	(0.152)	(0.150)	(0.151)	(0.136)	(0.135)	(0.135)
Income	-0.0123	-0.0027	-0.0121	-0.0318**	-0.0321**	-0.0325**	-0.0064	-0.0018	-0.0054	0.0105	0.0101	0.0105	0.0062	0.0020	0.0063
	(0.013)	(0.014)	(0.013)	(0.015)	(0.016)	(0.015)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)
High Education	0.1149*	0.0910	0.1123*	0.1259**	0.1337**	0.1401**	0.1850***	0.1520**	0.1686**	0.0480	0.0494	0.0480	-0.0352	-0.0211	-0.0396
	(0.068)	(0.074)	(0.068)	(0.055)	(0.060)	(0.058)	(0.070)	(0.074)	(0.074)	(0.076)	(0.079)	(0.079)	(0.077)	(0.079)	(0.077)
Age	-0.0030	-0.0002	-0.0029	-0.0037	-0.0033	-0.0042	-0.0136**	-0.0104*	-0.0129**	-0.0097**	-0.0099**	-0.0097**	-0.0073	-0.0088*	-0.0073
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Dutch	-0.0444	-0.0775	-0.0473	0.0005	0.0036	0.0090	0.0203	-0.0457	0.0003	-0.1428	-0.1407	-0.1428	-0.4860***	-0.4774***	-0.4894***
	(0.118)	(0.125)	(0.117)	(0.098)	(0.101)	(0.100)	(0.110)	(0.118)	(0.111)	(0.134)	(0.135)	(0.135)	(0.133)	(0.133)	(0.136)
Belgian	-0.0396	-0.1083	-0.0444	0.0938	0.0974	0.1145	0.0187	-0.0580	-0.0186	0.0124	0.0163	0.0124	-0.5173***	-0.4966***	-0.5242***
	(0.091)	(0.104)	(0.097)	(0.080)	(0.088)	(0.088)	(0.090)	(0.095)	(0.091)	(0.101)	(0.105)	(0.105)	(0.101)	(0.108)	(0.112)
Observations	146	146	146	146	146	146	146	146	146	146	146	146	146	146	146

Notes: The table presents the results for the self-reported risk task of a probit regression with respect to different domains. Subjects are classified as risk takers in a particular domain if their total score is above the median and risk averse otherwise. Columns 1-3 show the results for the financial, Columns 4-6 for the ethical, Columns 7-9 for the health/safety, Columns 10-12 for the recreational, and Columns 13-15 for the social domain. Standard errors are robust to heteroskedasticity and reported in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Chapter 2

Social Contagion in Victim Precaution

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The adoption of crime preventive measures may be socially contagious. This can be due to the local nature of crime spillovers as well as local social norms and beliefs about the use and effectiveness of such measures. We provide rare evidence of this phenomenon in the case of roll-down shutters, a conspicuous burglary prevention device that is highly popular in some areas but rarely used in others. Our results suggest that the strong geographical variation in the use of this preventive measure is driven by social contagion rather than differences in local conditions or preferences. The rate of adoption in a neighborhood goes up by 0.39 to 0.87 percentage points when the rate of adoption in surrounding neighborhoods increases by one percentage point.

Crime . Preventive Behavior . Social Contagion

2.1. Introduction

The decision of adopting a preventive measure against crime depends on the behavior of others. Individuals within a locality feature common precautionary measures as a result. This notion goes back to the theoretical work by Clotfelter (1978) and Shavell (1990). They work out one channel for the interdependence of preventive behaviors: having less protection than alternative targets makes one a more likely target for offenders. In other words, taking preventive measures may not only deter crime but also displace crime within the locality.

Another possible channel is an upward adjustment in beliefs about the need to adopt certain preventive measures in response to observing their use by others

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(Durlauf, 2004). Then, an individual relies on the judgment of others in their environment, which may be sensible given the uncertainty about the likelihood and consequences of victimization as well as the effectiveness of precautionary measures.[2] A last possible channel is the wish to conform to local social norms about the proper way of securing oneself against crime risk (Nordholm, 1975). An individual may experience disutility from behaving differently from others in their environment. Therefore, ceteris paribus, contagious behavior of adopting a preventive measure against crime can be more likely to occur between spatially closer agents.

The influence of other people's behavior on decision-making has been studied in a wide range of contexts, including student academic performance (Angrist and Lang, 2004), household energy conservation (Wolske et al., 2020), and also offender behavior (Glaeser et al., 1996; Ludwig et al., 2001) but less so in the context of private crime precaution. Two recent studies are notable exceptions. Within the Italian context, Maheshri and Mastrobuoni (2020) provide evidence for one of the channels for social contagion in victim precaution: local displacement of crime. They study the decision of banks to deter robberies by hiring a security guard. Hiring guards not only provides a private deterrent effect but also displaces half of the deterred robberies to nearby, unguarded banks. Another exception is Amodio (2019), who studies the adoption of burglar alarms and security cameras by households in the city of Buenos Aires. Based on variation in investment in security measures induced by the victimization of friends, relatives, and acquaintances living farther away, he provides evidence that neighbors' investment has a positive and significant effect on their own investment. He does not disentangle the three channels that may explain his results.

Social contagion in crime prevention provides one explanation for why certain visible crime prevention measures are highly popular in some areas but rarely used in other areas. Iron or steel grilles in front of windows of homes are a very popular way of securing one's home in Taiwan, for instance, but completely absent in many other countries (Shu, 2009). Tall stonework walls around the home are common in many Latin-American countries, but not elsewhere (Siembieda, 1996). The commonality of preventive measures within a locality may also be driven by similar conditions, including the local crime risk or similar preferences of

[2]In his paper, Amodio (2019) presents a simple model of a frictional market for offenses by building upon on the work of Elrich. A crime event is seen as a trading episode, and the model equilibrium is determined by the equilibrium fraction of active criminals and unprotected individuals. However, in this model, beliefs regarding the effectiveness of a crime preventive measure are not incorporated into the utility function of the potential victims. In our context, one channel for the spread of the roll-down shutters can be people's beliefs regarding the effectiveness of a measure, which can be a function of the popularity of the measure itself. Then, as the measure becomes more popular, the belief in the measure can increase, and the equilibrium amount of unprotected individuals can decrease. This can be seen more clearly by adjusting the model of Amodio (2019) accordingly.

individuals that choose to live within the same locality rather than social contagion, however (Manski, 1993). Identifying peer effects is notoriously challenging. As Maheshri and Mastrobuoni (2020) point out, the scope of the externality emanating from preventive measures[3] is usually ambiguous due to multiple endogeneity and measurement issues. This makes it challenging to identify interdependence in preventive behavior.

In this paper, we contribute to this small but growing literature on social contagion in private victim precaution by studying a striking geographical pattern in the adoption of a specific crime preventive measure by households in the Netherlands that is obvious to even casual observers. We investigate whether social contagion can explain why roll-down window shutters, a conspicuous measure against domestic burglary, are common in the south of the country, but rare in the north of the country. Our primary aim is to conduct an empirical test for the presence of social contagion, and less so to disentangle the three channels mentioned above that are behind the interdependence of the observed behavior. In the concluding section, we provide a qualitative discussion of alternative mechanisms that may be at work within this particular context.

Across the world, roll-down shutters or security shutters are marketed as a means of securing one's home against burglary (Cozens and Davies, 2013).[4][5] Generally, shutters consist of aluminum slats that fit in a steel frame. The construction creates a barrier in front of windows or doors that is meant to delay entry into the home by an intruder.[6] Shutters in front of windows also keep property in the home private. Beckford (2020) provides anecdotal evidence of how roll-down shutters spread across shopping streets in west London at the end of the 1970s, beginning of the 1980s. The impetus was a period of civil unrest, known as the Southall riots. "The natural response was to erect those ugly grey aluminum roller shutters (which attract graffitists) and once one shopkeeper did it, the neighbors followed suit. Soon, nearly every shop along Southall Broadway was shuttered at night, putting an end to nighttime window shopping" (Beckford, 2020).

Roll-down shutters are one of several target-hardening measures that households can take, and it has been included as such in the Netherlands Crime Survey

[3]As explained before, this is one of the channels driving social contagion.

[4]To the best of our knowledge, hard evidence of the burglary preventive role of the roll-down shutters for the residential buildings is still missing. Therefore, even though they are marketed for burglary prevention, it is questionable to what extent this claim is supported by evidence.

[5]Next to the supposed crime-reducing effect, roll-down shutters are also bought for reasons of climate control (Almusaed, 2011), noise reduction (Díaz et al., 2013), and storm protection (Peacock, 2003). It is pretty common for crime preventive measures to serve other purposes as well. Double glazing, for instance, helps for climate control but also keeps out burglars (Tilley et al., 2015).

[6]In several countries, standards for the burglar retardant quality of roll-down shutters have been developed. Examples are LPS 1175 maintained by BRE Global and NEN 5096 maintained by Nederlands Normalisatie-Instituut.

(Veiligheidsmonitor) that we use in this study. The survey provides household-level data for 2012-2017 on both victimization of crime and crime preventive behavior. It is one of the largest crime surveys in the world, relative to the size of population. By matching the survey data with geo-coded register data, we can trace the neighborhood of a household.

The survey data clearly show the above-mentioned north-south gradient in the presence of roll-down shutters. As stated before, social contagion is only one of the explanations for this gradient. The geographical pattern may also be the outcome of co-located individuals, each making their own independent decisions that happen to be similar. The north and south of the country differ in many ways, with cultural differences going back centuries (Byrne et al., 2020). The adoption of roll-down shutters may be related to this variation in household characteristics. In that case, there is something about the people in the south that makes them more likely to adopt this preventive measure. The north-south gradient may also be the result of differences in local conditions, including the burglary risk. If that is the case, then people in the south are not necessarily different from people in the north in any way, but just face conditions that ask for the adoption of roll-down shutters.

We analyze the extent to which the rate of adoption of roll-down shutters in one neighborhood is affected by the rate of adoption in bordering neighborhoods. As in any study estimating spillover effects, we need to make assumptions about how households in one neighborhood influence the decisions of households living in other neighborhoods. In other words, to achieve identification, we have to impose an interaction structure between neighborhoods. We assume that spillovers are a function of spatial proximity. Thus households simply need to be co-located for social contagion to occur; they do not need to be acquaintances or even have any contact in order to be affected by each other.

We look at how the adoption rate of roll-down shutters in a neighborhood[7] is a function of the adoption in other neighborhoods. Specifically, the rate of adoption of roll-down shutters in a neighborhood is assumed to be a linear function of the spatially weighted average of roll-down shutter rates in nearby neighborhoods, which is called 'spatial lag' variable. Following the 'Generalized spatial two stage least squares (GS2SLS)' approach developed by Kelejian and Prucha (1998,9, 2004), we account for endogeneity in the adoption of roll-down shutters by using predetermined variables of the model and their spatial lags as instrumental variables.

To adjust for the selection of households into neighborhoods based on characteristics that drive the adoption of roll-down shutters, we include a number of known and observed household characteristics, namely demographic char-

[7]In this paper, we define the adoption rate (or rate of adoption) of roll-down shutters in a location to be the average shutter adoption in that location.

acteristics, the ethnic composition of the neighborhood, and views about the neighborhood.

Moreover, we allow for the presence of spatial interaction effects among the error terms. This can account for the determinants of the dependent variable that are omitted from the model to be spatially auto-correlated, the possibility that a random shock occurring in one neighborhood may affect its bordering neighborhoods, or the unobserved latent factors such as local amenities, culture, or neighborhood prestige may have spatial dependence.

We find that the rate of adoption of roll-down shutters in a neighborhood goes up by 0.39 to 0.87 percentage points when the adoption rate in surrounding neighborhoods increases by one percentage point. To adjust for variation in time-invariant local conditions that affect the rate of adoption of roll-down shutters and exploit the limited time variation in our survey data, we also estimate spatial panel models with neighborhood fixed effects in a sensitivity analysis. Such fixed effects may capture factors such as crime conditions. We find qualitatively similar results.

The remainder of the paper is structured as follows. In Section 2.2, we discuss the data and descriptive statistics. Section 2.3 presents the empirical approach. In Section 2.4, we present our findings and in Section 2.5 we conduct a number of sensitivity tests. Section 2.6 concludes.

2.2. Data

Our primary data source is the Netherlands Crime Survey (Veiligheidsmonitor, VM), an annually repeated cross-sectional survey among individuals. It is one of the largest crime surveys in the world, relative to the size of the population. Every year from 2012 to 2017, around 270,000 randomly selected respondents aged 15 years or older are approached to conduct the interview. The interview is conducted between August and December 31, by internet or paper questionnaires. The response rate is around 39 percent.

Our data contains information regarding household, neighborhood, and municipality characteristics. These include household size, gender, education level (low, medium, and high), number of children in the household, being immigrant or not, age, employment status, disposable income of the household (in 100,000 euros),[8] whether their house is rented or owned, type of residence (apartment, detached house, and other types), personal views towards the municipality, degree of urbanization of the municipality, liveability of the neighborhood, satisfaction with locally provided police services, and feelings of cohesion in the vicinity

[8]This is the gross income minus paid income transfers, insurance premiums, health premiums, and income tax. Therefore, it is possible that the disposable income to be negative.

within the last five years. In addition, our data provide information on crime victimization experience and private crime preventive measures of the household.[9]

We treat the answer 'don't know' or 'refuses to answer' as missing on the questions. Therefore, the estimation sample includes 586,363 respondents,[10] consists of 388 municipalities and covers 11,001 out of the 13,150 neighborhoods as defined in 2017. On average, we have 53 participants per neighborhood in the estimation sample.[11] Table 2.1 presents the summary statistics for our sample.[12] Participants are 15 years old or older. Around half of the respondents are women, middle-educated, and have a paid-work. Most of the respondents are native, and they own the house they are living in. Twenty percent of the respondents have roll-down window or door shutters.

To explore geographic concentration in the adoption of crime preventive measures across the country, we present choropleth maps for five different measures to prevent domestic burglary in the municipalities in Figure 2.1: roll-down shutters for window and door, burglar alarms, outdoor lights, additional door locks, and leaving lights on when not at home. The rate of adoption of roll-down shutters among households in a municipality is geographically clustered, with a much higher rate in the south than in the north of the country. The other four prevention measures do not show a similarly clear geographical pattern, although the adoption rate of burglar alarms and additional door locks seems to be relatively low in the northeast of the country.

Figure 2.2 further illustrates the difference in take-up of roll-down shutters between the north vs. the south of the country and the absence of this particular geographical pattern for other preventive measures.[13] For each preventive measure, we plot the kernel densities based on the average adoption rates across municipalities, separately for the north and south parts of the Netherlands. The distribution is markedly different for roll-down shutters but fairly similar for the other measures.

[9]We do not include crime victimization experience and other private crime preventive measures of the household in the analysis specified in Section 2.3 as they can be potentially endogenous to the roll-down shutter adoption.

[10]As our variable of interest is roll-down shutter adoption, we use list-wise deletion, so the estimation sample excludes missing observations of roll-down shutter adoption.

[11]The area of a four-digit zip code is greater than the area of a neighborhood. On average, the number of residents per neighborhood as of 2017 is 1,298 in the Netherlands. The definition of neighborhoods is fairly similar to how US census tracts are defined. In the US, block groups generally contain between 600 and 3,000 residents.

[12]The minimum and maximum values of the disposable household variable are not revealed so as not to risk the confidentiality of our data.

[13]We split the country into two parts, with the major rivers Waal and Lek being the horizontally dividing line.

Table 2.1: Summary statistics

	Mean	SD	Min.	Max.
Private Crime Prevention Measures				
Roll-down shutters	0.202	0.402	0	1
Burglary alarm	0.153	0.360	0	1
Additional door lock	0.751	0.432	0	1
Outdoor lights	0.835	0.371	0	1
Leaving lights on when away	0.753	0.431	0	1
Neighborhood Characteristics				
Liveability in the neighborhood	7.456	1.283	1	10
Contact with neighbors	3.120	0.997	1	5
Social cohesion score	6.248	1.858	0	10
Satisfied with neighborhood composition	3.663	0.920	1	5
Functioning of the police	3.345	0.875	1	5
Functioning of the municipality	3.334	0.836	1	5
Degree of urbanization	3.334	1.252	1	5
Household Characteristics				
Woman	0.524	0.499	0	1
Age	51.840	17.929	15	106
Native	0.735	0.441	0	1
Non-western immigrant	0.154	0.361	0	1
Education	2.026	0.828	1	3
Working	0.528	0.499	0	1
Unemployed	0.023	0.149	0	1
Retired	0.275	0.446	0	1
Student	0.072	0.259	0	1
Household size	2.518	1.184		
Disposable household income (in 1.000)	44.065	37.603		
Presence of children in the household	0.249	0.432	0	1
Rental house	0.278	0.448	0	1
Own house	0.722	0.448	0	1
Apartment	0.221	0.415	0	1
Detached house	0.751	0.432	0	1
Observations	586,363			

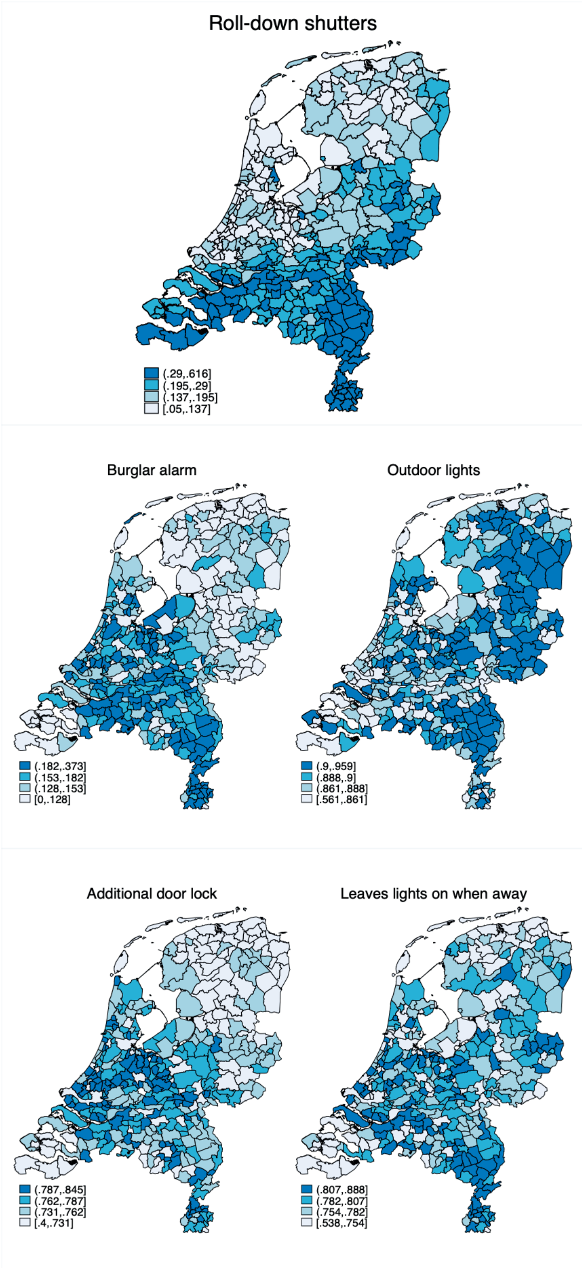

Figure 2.1: Geographic spread of adoption of crime preventive measures, the Netherlands, 2012-2017. Data aggregated at the level of municipalities.

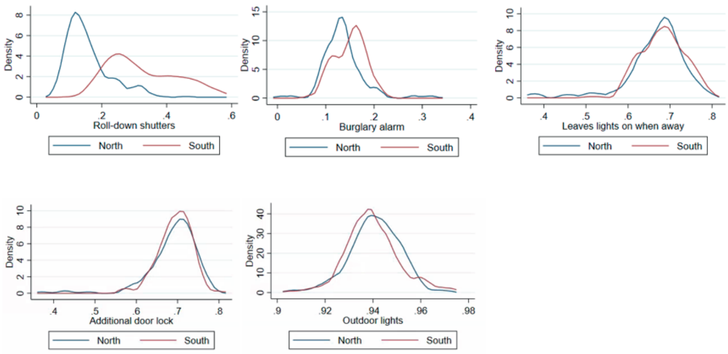

Figure 2.2: Kernel density plots for five victim precaution measures. The estimates are based on the average adoption rates across municipalities.

Both Figure 2.1 and Figure 2.2 suggest that the geographical clustering in the use of roll-down shutters is unlikely to be driven by a difference in crime rates. If that were the case, then we would expect to see a similar geographical clustering for the other crime prevention measures as well.

Geographical clustering of roll-down shutters can also be investigated by way of a Moran scatter plot. In such a plot, the standardized rate of adoption in a place (on the horizontal axis) is set against the standardized[14] adoption rate in neighboring places (on the vertical axis). Here, we choose the municipality as geographical unit.[15] The scatter plot is displayed in Figure 2.3.

The upper-right quadrant of Figure 2.3 includes municipalities where the rate of adoption, as well as the rate of adoption in neighboring municipalities, are above-average. The lower-left quadrant includes municipalities where the rate of adoption, as well as the rate of adoption in neighboring municipalities, are below-average. The slope of the line is defined as Moran's I coefficient. Overall, adoption rates in a municipality are strongly positively associated with adoption rates in neighboring municipalities.

We formally test for the presence of non-random variation in the use of roll-down shutters across space using Moran's I statistic (Moran, 1950). Moran's I is a correlation coefficient that measures the overall spatial auto-correlation in the data. In other words, it measures the overall degree to which one object, in our

[14]In this plot, the variables are in the standardized forms with mean zero and standard deviation equal to one.

[15]We choose municipality as geographical unit rather than neighborhood in this scatter plot because the number of observations per neighborhood is small, so we do not release it to protect the confidentiality of our microdata.

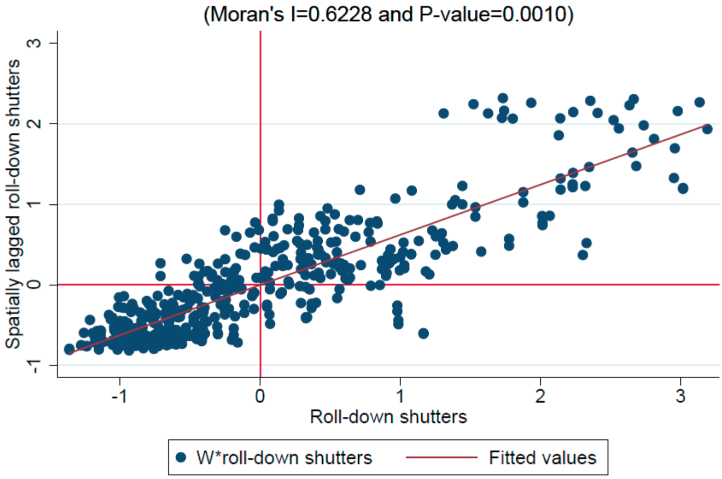

Figure 2.3: Moran scatter plot for the adoption of roll-down shutters. The horizontal axis stands for the standardized adoption percentage of roll-down shutters in a municipality; the vertical axis shows the standardized average shutter adoption percentage in its neighboring municipalities.

case, a municipality, is similar to others surrounding it. We can reject the null hypothesis of no spatial correlation with 99 percent confidence.

2.3. Empirical approach

In this section, we discuss our approach to identifying the presence of social contagion in our data. Social contagion is present if a household's decision to adopt roll-down shutters is affected by the shutter adoption decision of other households living nearby. As discussed in the introduction, social contagion can work by way of three channels: negative externalities from victim precaution, updates in beliefs about the need to take a preventive measure in response to observing measures taken by others, and preferences for conforming to local norms about the proper way of protecting oneself against crime risk.

2.3.1. Traditional model and identification problems

The traditional model for studying social interactions is Manski (1993)'s linear-in-means model. At the level of households, this model can be described as follows:

$$y_{ir} = \phi_0 \, \mathrm{E}[y_{\mathrm{r}}|r] + \beta_1 x_{ir} + \beta_2 \, \mathrm{E}[x_{\mathrm{r}}|r] + \epsilon_{ir} \tag{2.1}$$

where $\mathbf{y_{ir}}$ denotes the outcome for household i living in neighborhood r. In our context, the outcome is the decision whether to adopt roll-down shutters or not. The outcome not only depends on exogenous characteristics of households, namely $\mathbf{x_{ir}}$, but also on peer group behavior, with the peer group defined as other households within the same neighborhood. That is, neighborhoods *interact in groups*: each member of a neighborhood is equally affected by other members of the neighborhood, but not by households outside their neighborhood. Specifically, peer effects are group-specific and the same for all members of the group. This is denoted by the mean *outcome* of their peer group, $\mathrm{E}[y_{\mathrm{r}}|r]$, and commonly referred to as *endogenous effects*, i.e., the influence of the peer's outcome. Each household in a neighborhood is also equally affected by the mean *characteristics* of other households in their neighborhood, $\mathrm{E}[x_{\mathrm{r}}|r]$, known as *contextual effects*. Since ϕ_0 denotes how much a household's outcome is affected by the outcome of their peer group, it is our parameter of interest. In other words, ϕ_0 reflects the degree of social contagion.[16]

Identification of Equation 2.1 is not straightforward. This can be gleaned more easily by substituting $\mathrm{E}[y_{\mathrm{r}}|r]$ into Equation 2.1 and obtaining the reduced form of Equation 2.1:

$$y_{ir} = \beta_1 x_{ir} + \left(\frac{\beta_1 \phi_0 + \beta_2}{1 - \phi_0}\right) \mathrm{E}[x_i|r] + \epsilon_{ir} \tag{2.2}$$

[16] For example, in the context of crime prevention, there will be *endogenous effects* if a household's crime prevention decision is affected by their peer's crime preventive decision.

We face three main challenges in terms of identification of the parameters in Equation 2.2 (Manski, 1993). Firstly, as shown in Equation 2.2, only β_1 and the composite parameter vector $(\beta_1\phi_0+\beta_2)/(1-\phi_0)$ are identified. We cannot separately identify *endogenous effects* (ϕ_0) from exogenous effects (β_2) without additional information. This is known as the *reflection problem* (Manski, 1993); the expected mean outcome of the peers and their mean characteristics are perfectly collinear because of simultaneity. It is not possible to know whether group behavior affects household behavior or whether group behavior is simply the aggregation of household behaviors, without making strong assumptions.[17] Secondly, there can be common conditions faced by the same group members that affect the outcome variable. The geographical gradient might reflect the effects of similar environments or shocks instead of social contagion.

Lastly, households may sort themselves into neighborhoods in a non-random manner, i.e., based on characteristics that affect the decision of whether to adopt roll-down shutters. Then, the similarity in outcomes for the people who live in the same area is driven by non-random neighborhood formation.

2.3.2. Our model

We confront the reflection problem by relaxing the interaction structure assumption of the linear-in-means model, where individuals interact only in groups. However, social relationships among households in transmitting information and channeling behavioral change can be tied to a larger web of connections. In our framework, we rely on a spatial econometric model with a specific partially overlapping interaction structure.

We use pooled cross-sectional data as described in Section 2.2.[18] We aggregate our data up to the neighborhood level as the neighborhood is the smallest geographic identifier in our data. We do not know the distance between respondents living in the same neighborhood; we do know whether neighborhoods are adjacent or not. We construct the social interaction structure assuming that only neighbors sharing a border affect each other. That is, households in one neighborhood do not need to be acquaintances or even have any contact to be affected by households in surrounding neighborhoods; they are only directly affected by households in bordering neighborhoods and indirectly through households

[17] The reflection problem is commonly addressed by assuming only one type of effect and ignoring the other one (Trogdon et al., 2008).

[18] We do not use panel data framework in the baseline specification because the spread of roll-down shutters is a relatively slow phenomenon. The CBS crime surveys (ERV, POLS, VMR, IVM, and VM) show that the percentage of households with roll-down shutters has been increasing slowly since the 1990s. Figure B.1 in Appendix B shows the long-term trend of the roll-down shutters adoption over time in the Netherlands. Therefore, given our data, we do not have enough variation for the time frame considered. Nevertheless, as a robustness check, we also employ panel data in Section 2.5 for the reasons that will be discussed at the end of this section.

further away.[19] The network of interactions between neighborhoods is denoted by the interaction matrix **W**.[20]

Each element of **W** shows the degree of spatial proximity between the pair of neighborhoods. This is a n x n spatial weighting matrix which is assumed to be known and non-stochastic. It is a binary symmetric matrix with elements w_{ij} for locations i and j. The value of w_{ij} is 1 if there is a link between two row-column unit, and zero for those having no links. By definition, diagonal entries are zero because a unit is not a neighbor to itself. For ease of interpretation and also estimation, the matrix **W** is row-normalized, such that each row sums up to one.

Under the assumption of this social interaction structure, our baseline model can be specified as follows:

$$Y = \alpha\iota + \delta WY + X\beta_1 + \epsilon \tag{2.3}$$

where **Y** denotes the rate of adoption of roll-down shutters in neighborhood i. Household, neighborhood, and municipality characteristics are denoted by exogenous matrix **X**. **W** is the weighting matrix described above and ϵ is the disturbance term.

Equation 2.3 includes the various reasons for why the rate of adoption of roll-down shutters in a neighborhood can be related to the rate of adoption in adjacent neighborhoods that we discussed previously (Elhorst, 2014). Multiplying W with Y gives the spatial lag of Y, which is the linear combination of the outcome variable constructed from regions bordering that neighborhood. Meaning, it is the average rate of adoption in adjacent neighborhoods. Parameter of interest, δ, therefore reflects the *endogenous effects*; it shows how the rate of adoption of roll-down shutters in one neighborhood depends on **WY**. In other words, it is the causal effect of rates of adoption in adjacent neighborhoods conditional on explanatory characteristics of the neighborhood and of its adjacent neighborhoods, under the assumptions stated above. It can theoretically take on values between -1 and 1. Social contagion is absent if δ=0.

It is likely that the rate of adoption of roll-down shutters in a neighborhood is directly influenced by the characteristics of the adjacent neighborhoods. This can be included in the model through spatially lagged explanatory variables. Moreover, there can be spatial dependence in the error terms. Then, the extended model can be specified as follows:

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[19]Another common approach to defining neighborhood relations is based on the distance between locations. Intrinsically, this is most appropriate for geographic data expressed as point data, whereas our approach is suited for areal units (Anselin and Rey, 2014), i.e., neighborhoods.

[20]'W' is the most commonly used notation for the so-called spatial weights matrix in spatial econometrics literature. That is the reason we adopt this notation.

$$Y = \alpha\iota + \delta WY + X\beta_1 + WX\beta_2 + \epsilon \tag{2.4a}$$
$$\epsilon = \lambda W\epsilon + \mu \tag{2.4b}$$

Parameter β_2 reflects the exogenous interaction effect, where the roll-down shutters in one neighborhood depend on the independent explanatory characteristics of the adjacent neighborhoods, **WX**. Specifically, we allow for the possibility that the roll-down shutters are also directly affected by the household characteristics, neighborhood and municipality perceptions of the surrounding neighborhoods.

Equation 2.4b reflects the spatial dependence in the disturbance term, ϵ. Parameter λ denotes the spatial interaction effect among the disturbance terms, $\mathbf{W}\epsilon$, and μ is a vector of error term. $\mathbf{W}\epsilon$ accounts for auto-correlation in the determinants of the dependent variable that are omitted from the model or for situations in which an unobserved shock follows a spatial pattern. Bordering neighborhoods can share idiosyncratic characteristics. For example, if unobserved latent factors such as local amenities and culture are spatially dependent, then $\lambda \neq 0$ (LeSage and Pace, 2009). In that case, inclusion of this term is necessary to get consistent estimation of the standard errors of the parameters (Kelejian and Prucha, 2010).[21]

In the presence of *endogenous effects*, estimation of Equations 2.3 and 2.4 by OLS is biased and inconsistent as the error term is correlated with the spatial lag term of the dependent variable (Anselin, 2013). We estimate our model by way of an instrumental variable procedure that accounts for both the endogeneity problem and the spatial interaction effect among the error terms. This method is called 'Generalized spatial two stage least squares (GS2SLS)' (Kelejian and Prucha, 1998,9, 2004).[22]

Kelejian and Prucha (1998,9) show that the "best instruments"[23] for the endogenous variable is its conditional mean. That is,

$$\mathrm{E}[WY|X] = W\,\mathrm{E}[Y|X] = W(I - \delta W)^{-1}X\beta_1 \tag{2.5}$$

[21]For consistency of the estimator, the matrices $I - \delta$W and $I - \lambda$W should be non-singular. For this to hold, Kelejian and Prucha (1998,9) assume that δ and λ are restricted to (-1,1). Elhorst (2010); Kelejian and Prucha (1998); Kelejian et al. (2006); Lee and Yu (2010); LeSage and Pace (2009) discuss the conditions for identification, i.e., stationary conditions that are imposed on the spatial weight matrix W, λ, δ, and the parameter spaces of λ and δ.

[22]Various number of approaches have been proposed for estimating the models that include spatial interaction effects (LeSage, 1999). The advantage of the GS2SLS approach is that it does not depend on distributional assumptions and provides consistent estimates if innovations μ are heteroskedastically distributed, where the heteroskedasticity is of unknown form. See Arraiz et al. (2010); Kelejian and Piras (2017).

[23]To avoid issues associated with the computation of the inverse of the matrix $(I - \delta W)$, Kelejian and Prucha (1998,9) suggest the use of an approximation of the best instruments.

Using the assumptions specified above:

$$(I - \delta W)^{-1} = I + \delta W + \delta^2 W^2 + ... \tag{2.6}$$

Therefore,

$$\mathrm{E}[WY|X] = W(I + \delta W + \delta^2 W^2 + ...)(X\beta_1) \tag{2.7}$$

Equation 2.7 suggests that the conditional mean of the endogenous variable is linear in **WX, W^{2}X,...** This is because as **Y** is determined by **X, WY** is also determined by **WX, W^{2}X,...** Moreover, since **X** is uncorrelated with the disturbance term, then **WX** should also be uncorrelated with the disturbance term. Therefore, given this specification, our instruments include **X, WX,** and **W^{2}X**, which are used standard in the literature. The identification is possible only if **I, W**, and **W^2** are linearly independent (Bramoullé et al., 2009). The neighborhood interaction structure we utilize meets this condition as it makes the networks partially overlapping (Bramoullé et al., 2009).

The intuition behind the instrumental variables can also be seen through this example.[24] Let's assume that there are only three neighborhoods: i, j, and k. Neighborhood i shares a border with j, j shares with i and k, and k only shares with j. In our framework, households in neighborhood i are directly affected by j but not directly affected by k. This implies that, the characteristics of the households in neighborhood k (i.e., x_k) affects the roll-down shutter adoption of neighborhood i only indirectly, through their effect on y_j.[25] Therefore, the characteristics of a further away neighborhood, (**W^{2}X**), can serve as instruments for the roll-down shutter adoption of a neighborhood's adjacent neighborhoods, (**WY**).

The estimation consists of three steps. In the first step, 2SLS is applied to estimate regression parameters using the instruments. This only accounts for the endogenous spatial lag, not for the spatial correlation of the disturbances. In the second step, based on 2SLS residuals, the spatial interaction effect among the disturbance terms, λ, is estimated using a GMM procedure. In the last step, the estimate of λ is used to transform the model and estimate the parameters using an IV procedure with the same instruments (Drukker et al., 2013; Kelejian et al., 2013).

The literature on neighborhood formation shows that selection depends on multiple neighborhoods and household characteristics which are assessed simultaneously or in combination (Hedman and van Ham, 2012). These factors include preferences, socioeconomic characteristics of the household (i.e., income,

[24]This example is taken from Bramoullé et al. (2009).

[25]For example, in our case, a household's decision to adopt roll-down shutters could be affected by both their characteristics and (indirectly through or directly by) their neighbor's characteristics, i.e., being an immigrant, employment status, social cohesion score, and being in contact with neighbors.

age, education, employment status), availability of affordable houses, dwelling types, ease of access to the housing market, and ethnic composition of the neighborhood population (Boschman and Van Ham, 2015; Clark and Ledwith, 2007; Doff, 2010). Households may be more likely to choose neighborhoods where the composition of the inhabitants matches their own backgrounds. For example, higher-income households are more likely to live in a high-income neighborhood, and poor households can only afford to live in a poor neighborhood. While individuals with children are more likely to move to a neighborhood with a high proportion of households with children, young adults and singletons prefer to live in city centers, and older people can favor living in the suburbs (Clark and Dieleman, 1996). The ethnic composition of the neighborhood can also be one of the significant determinants of neighborhood selection. Ethnic minorities in the Netherlands exemplify this statement as they lean towards living with residents of the same ethnic group, and this preference is even stronger for native Dutch people (Doff, 2010). There can also be barriers to access to the housing market, making it difficult for immigrants to get a dwelling in a neighborhood with high socioeconomic status (Aalbers, 2005; Bolt et al., 2008).

In our study, we should be concerned about selection into neighborhoods if it happens on the grounds that are also determinants of the use of crime preventive measures. Ideally, we would have panel data at the level of households so that we can rule out the observable and unobservable household level characteristics in the absence of such data. What we can do is to adjust for a number of known determinants of selection into neighborhoods for which we have data, namely demographic characteristics, the ethnic composition of the neighborhood, and views about the neighborhood. Moreover, specifying a neighborhood fixed effect in Equation 2.3 may also deal with the concern of self-selection into neighborhoods in case this choice depends on the characteristics of the neighborhoods that do not vary within our time frame. For instance, neighborhood choice may depend on some unobserved factors related to that neighborhood, such as reputation, and determine the type of households who prefer these neighborhoods. We will investigate this further in the robustness section. Not being able to adjust for other, unobserved factors related to self-selection is a limitation of our approach. The resulting bias is likely to be limited, given the presumably limited overlap of factors driving neighborhood selection and adoption of crime preventive measures.

2.4. Results

First, we estimate the baseline model; Equation 2.3. We control for a number of variables mentioned in Section 2.2, e.g., household, neighborhood, and municipality characteristics. The parameter of interest, δ is denoted as the coefficient of the spatial lag. Then, we extend our model and estimate Equation 2.4 which is a more general framework that includes spatial interaction effects among error terms and spatially lagged explanatory variables. Both equations are estimated by way of Generalized spatial two stage least squares (G2SLS). The results are presented in Table 2.2.

Each column presents the results for the variable of interest, δ, with different specifications of spatial spillover structures. In the first column, we estimate the baseline equation, and we only take *endogenous effects* into account. The estimated endogenous effect is 0.39, which is significant. That is, the effect of a one percentage point increase in the mean shutter adoption of surrounding neighborhoods of a neighborhood on that neighborhood is 0.39.

In Column 2, we allow for *endogenous effects* and exogenous interaction effects. That is, $\delta \neq 0$ and $\beta_2 \neq 0$ but λ=0 in Equation 2.4, which is denoted as the Spatial Durbin Model (SDM). Finally, the results of the model that takes into account *endogenous*, exogenous and spatial interaction effects within the error terms are presented in Column 3, denoted as the General Nesting Spatial Model (GNSM). In that case, $\delta \neq 0$, $\beta_2 \neq 0$, and $\lambda \neq 0$.[26] In these specifications, we allow that, for example, perceptions regarding the liveability of the vicinity or satisfaction with the police functioning in the surrounding neighborhoods can affect the roll-down shutter adoption of that neighborhood as the households can change their perceptions accordingly. The results are much larger than the baseline specification and provide evidence for the presence of social contagion by way of spatial spillovers. These findings have further strengthened our confidence in the results from the baseline specification.

Even though the control variables can partly address the common conditions the same neighborhood members face and the self-selection into neighborhood issues that affect the outcome variable, the parameter of interest can still reflect the unobserved common factors in a neighborhood. We deal with this concern and conduct additional robustness checks in Section 2.5.

Lastly, as mentioned, Table 2.2 only presents the results for the coefficient of interest for the whole country. Even though the results for the independent variables are not of our primary concern, this can be worth mentioning. We show these in Appendix B in Table B.1. We also present the results for the north and south parts separately in Appendix B in Table B.2.

[26]We also conduct the analysis of a model that only controls for endogenous and spatial interaction effects within the error terms, which is called the Spatial Autoregressive Combined (SAC) model. That is, $\delta \neq 0$, $\lambda \neq 0$, but β_2=0. However, the Wald test for λ=0 is insignificant. Therefore, we conclude that this model does not fit the data.

Table 2.2: Parameter estimates of roll-down shutters

VARIABLES	(1) SLM	(2) SDM	(3) GNSM
Spatial lag of roll-down shutters (δ)	0.3872***	0.7370***	0.8732***
	(0.032)	(0.047)	(0.028)
(λ)			-0.6477***
			(0.027)
Observations	10,428	10,428	10,428

Notes: The table shows results from estimating Equations 2.3 and 2.4. Based on data by neighborhood. Not shown are estimated coefficients for covariates. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.5. Robustness

In this section, we conduct a number of sensitivity tests to see how robust the estimated effect δ is to alternative specifications.

Until now, we defined two locations to be neighbors if they share immediate borders, i.e., a line segment or a vertex, i.e., a corner, in common. We can change this assumption to see how robust our results are to alternative specifications. For example, another way of changing the matrix **W** would be considering two locations as neighbors if and only if they share a border (line segment), not a corner. The results are presented in Panel A of Table 2.3 with this alternative formulation of the weighting matrix.

2.5.1. Different neighborhood definitions

In addition to the first-order borders, we can also consider the second-order bordering neighbors of a location as its neighbor. Meaning, the roll-down shutters in a neighborhood are not only affected by its bordering neighbors but also by the ones bordering its' neighbors. In this variant of the weighting matrix, w_{ij} is 1 if i and j are first order bordering neighborhoods, 0.5 if they are second order bordering neighborhoods, and 0 otherwise. Panel B of Table 2.3 presents the results according to this specification. Lastly, Panel C presents the results by changing the weighting matrix, w_{ij} to 1 if i and j are either first or second order bordering neighborhoods, and 0 otherwise. That is, a neighborhood is equally affected by its first and second order neighbors. The coefficients of the estimated endogenous effect are preserved with respect to changing the matrix **W**.

Table 2.3: Parameter estimates of roll-down shutters

VARIABLES	(1) SLM	(2) SDM	(3) GNSM
Panel A: line segment			
Spatial lag of roll-down shutters (δ)	0.3667***	0.7230***	0.8569***
	(0.032)	(0.049)	(0.030)
(λ)			-0.6195***
			(0.026)
Panel B: second order borders (0.5)			
Spatial lag of roll-down shutters (δ)	0.5262**	0.8380***	0.9185***
	(0.030)	(0.042)	(0.022)
(λ)			-0.8617***
			(0.064)
Panel B: second order borders (1)			
Spatial lag of roll-down shutters (δ)	0.5321***	0.8420***	0.9210***
	(0.030)	(0.040)	(0.022)
(λ)			-0.8455***
			(0.061)
Observations	10,498	10,498	10,498

Notes: The table shows results from estimating Equations 2.3 and 2.4. Based on data by neighborhood. Not shown are estimated coefficients for covariates, i.e., household, neighborhood, and municipality characteristics. In Panel A, two locations are considered neighbors if they only share a line segment border. In Panel B, two locations are considered neighbors if they share first and second-order borders, the weight of the second-order border being 0.5. In Panel B, two locations are considered neighbors if they share first and second-order borders; the first and second-order borders are weighted equally. Standard errors are robust to heteroskedasticity and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.5.2. Omitted common factors

Even though the reflection problem has been addressed by the intransitive interaction structure, one source of concern may be regarding the existence of unobserved variables, due to which δ might be biased. These are the unobserved variables that can directly affect the adoption of roll-down shutters and are spatially correlated because they are common to a group of adjacent neighbors. Then, δ might partly reflect the spatial correlation between unmeasured characteristics. Following Kelejian et al. (2013), we refer to these variables as omitted common factors.

As an example, it can be that people in some neighborhoods are less likely to install shutters because of a greater sense of community, their culture, or due to the difference in their norms and values regarding crime or religion. These differences, in turn, can influence a household's decision to adopt crime preventive measures. Kelejian et al. (2013) show that under reasonable conditions, the estimated parameter of δ is still consistent in the presence of the above described omitted common factors. This is because the set of instruments used in the model is orthogonal to the disturbance term and its spatial lag, by construction. Therefore, the 2SLS estimator of δ will be consistent.[27]

Nevertheless, we employ panel data structure as a robustness check. Introducing a variable intercept γ to our model can account for the (unobserved) omitted variables that are time-invariant and specific to each neighborhood (Elhorst, 2014). For example, neighborhoods can differ in their background characteristics, which are usually location-specific time-invariant variables that can affect the dependent variable. Neighborhoods located in a rural area, in the center, or the periphery of the country can be different in various aspects (Elhorst, 2014). These differences, in turn, can influence household's decision to adopt crime preventive measures. For this, we employ panel data structure and include neighborhood-fixed effects in our estimation equation to rule out the unobservable effects that do not vary with time. The model and the results are presented in the next section.

Panel data analysis

In the panel data framework, we estimate the following equation:

$$Y_t = \alpha\iota + \gamma + \delta W Y_t + X_t\beta_1 + W X_t \beta_2 + \epsilon_t \tag{2.8a}$$

$$\epsilon_t = \lambda W \epsilon_t + \mu_t \tag{2.8b}$$

[27] The same conclusion does not hold for the parameter estimates of other covariates. See Kelejian et al. (2013) for the theoretical proof. See Kelejian (2014) for the proof in the panel data framework and the conditions under which the estimator of the spatial lag parameter is consistent; both in the case of exogenous and endogenous omitted common factors.

where γ encompasses the effect of unobserved time-invariant neighborhood heterogeneity. The rest of the variables are defined similarly to the cross sectional analysis. Moreover, for identification, it is assumed that the interaction structure, W, is constant over time.[28] This implies that the panel data must be balanced.

At the neighborhood level, utilizing all relevant variables[29] does not provide sufficient variance for each neighborhood for six years. Therefore, to have balanced panels at the level of neighborhoods, we can only use part of them.[30] What we can do is to utilize all relevant variables but at the level of municipalities.

We estimate Equation 2.8 by way of Quasi-Maximum Likelihood (QML) estimator derived by Lee and Yu (2010). The estimator provides consistent estimates in the presence of *endogenous effects*. It addresses the endogeneity in the adoption of roll-down shutters by conducting a Jacobian transformation that ties the unobserved errors to the observable dependent variable.[31] This allows us to disentangle *endogenous effects* from exogenous effects (Bramoullé et al., 2009; Kelejian and Prucha, 1998; Lee, 2007).

The demeaning procedure that is used to estimate fixed effect models can yield biased estimates in the spatial model (Baltagi, 2008). To get consistent results, instead of demeaning, Lee and Yu (2010) uses an orthogonal transformation approach to remove the fixed effects without inducing dependence in the transformed error terms. This estimation does not suffer from the incidental parameter problem (Lee and Yu, 2010).

The results are presented in Table 2.4. Each column presents the results for the variable of interest, γ, with different specifications of spatial spillover structures. That is, we impose varying restrictions on Equation 2.8 as described in Section 2.4. The results are presented in Panel A. Moreover, similar to Section 2.5.1, we also consider the second-order bordering neighbors of a location as its neighbor. Panel B presents results according to this neighborhood definition. Depending on the specification, the estimated coefficient for spatial spillover varies between 0.20 and 0.6. This is broadly similar to the results in Section 2.4. The slight difference can be due to the smaller sample size we are left with as a result of the demeaning procedure. Nevertheless, the results still provide evidence for the presence of social contagion by way of spatial spillovers.

As mentioned above, we excluded a number of related independent variables that have missing observations for some years at the level of neighborhoods. We want to include these characteristics because, first, a household's decision to adopt

[28] The stationary conditions and the normalization procedure of the weighting matrix and the parameter spaces of the coefficients discussed in the previous section also apply here. See Footnote 21.

[29] These are the variables presented in Table 2.2.

[30] These include household size, disposable household income, gender of the respondent, being immigrant or not, age, and the degree of urbanization of the municipality.

[31] See Elhorst (2010); LeSage and Pace (2009).

a specific crime preventive measure can be affected by their view towards the municipality. Second, these variables can also partly control for the self-selection problem discussed in Section 2.3.

To be able to include these additional characteristics, we aggregate our data to the municipality level and re-estimate Equation 2.8. In addition, to have a baseline comparison with the main analysis, we run the regression with the same covariates as the analysis at the level of neighborhoods. Table 2.4 presents the results for the variable of interest: the regression results obtained by using the same covariates as in the main regression are presented in Panel C and the results obtained by including the additional covariates are shown in Panel D. The estimated endogenous effect, which is positive and significant, ranges between 0.7 and 0.9. The effect is even stronger now.[32]

[32] Since the channels of the social contagion primarily function locally, this higher estimate might be due to the aggregation of the local neighborhood-level effect.

Table 2.4: Parameter estimates of roll-down shutters, panel data

VARIABLES	(1) SLM	(2) SDM	(3) GNSM
Panel A: Neighborhoods			
Spatial lag of roll-down shutters (δ)	0.2008***	0.2510***	0.6047***
	(0.007)	(0.007)	(0.008)
(λ)			-0.4809***
			(0.012)
Observations	32,650	32,650	32,650
Number of groups	6,530	6,530	6,530
Panel B: Neighborhoods; second order borders (0.5)			
Spatial lag of roll-down shutters (δ)	0.3003***	0.3604***	0.6504***
	(0.008)	(0.008)	(0.008)
(λ)			-0.5228***
			(0.016)
Observations	32,650	32,650	32,650
Number of groups	6,530	6,530	6,530
Panel C: Municipalities; same covariates			
Spatial lag of roll-down shutters (δ)	0.7188***	0.7082***	0.9175***
	(0.015)	(0.016)	(0.010)
(λ)			-0.7917***
			(0.044)
Observations	2,328	2,328	2,328
Number of groups	388	388	388
Panel D: Municipalities; additional covariates			
Spatial lag of roll-down shutters (δ)	0.7084***	0.7027***	0.9077***
	(0.017)	(0.019)	(0.011)
(λ)			-0.7884***
			(0.044)
Observations	1,940	1,940	1,940
Number of groups	388	388	388

Notes: The table shows results from estimating Equation 2.8. Based on data by neighborhood in Panel A and Panel B, municipalities in Panel C and Panel D. In Panel B, two locations are considered neighbors if they share first and second-order borders. The analysis in Panel C uses the same covariates as the analyses in Panels A and B. The analysis in Panel D uses the same covariates as the analysis in Section 2.4. Not shown are estimated fixed effects and coefficients for covariates, i.e., household, neighborhood, and municipality characteristics. Standard errors are reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.6. Conclusion

Potential victims of crime are not passive recipients of the actions of offenders but can take preventive action. Thus, understanding victim behavior is at least as important as understanding offender behavior to explain crime patterns. One aspect of private victim precaution that has received only limited attention in empirical work to date is the extent to which the behavior of potential victims is interrelated. As discussed in the introduction, it has been noticed many times that victims that can be considered to be peers, neighboring households, in particular, show similarity in preventive behavior. Social contagion is one explanation for this similarity in behavior. Given our data, we are in an excellent position to distinguish social contagion from two alternative explanations, namely common conditions that related victims face, including the crime rate and common characteristics of related victims.

We contribute to the literature on private victim precaution by studying whether peer effects can explain the striking geographical clustering in the use of roll-down shutters in the south of the Netherlands. We find that the estimated coefficient for the variable of interest is 0.39, which indicates a strong presence of spatial spillover of roll-down shutters. Namely, the roll-down shutter adoption rate of a neighborhood increases by 0.39 percentage points when the adjacent neighborhoods increase their roll-down shutter adoption by one percentage point. Extensive robustness tests further support the results.

The spread of roll-down shutters from 'hotbed' Belgium further northwards resembles a very slow epidemiological spread of disease. Several factors can explain why this can be so and why it is not yet prevalent in the north part of the country: first, one of the channels behind the contagion, beliefs about the effectiveness of preventive measures, can be endogenous to the preventive measure's popularity. If a measure is not yet widespread enough in a location, people might think it is ineffective in preventing crime. Similarly, people might believe more in its effectiveness in the areas where roll-down shutters are more popular. That is, if the adoption of roll-down shutters increases slowly, so would the speed of adjustment in people's beliefs regarding its benefits, resulting in the slower spread of the roll-down shutters. Therefore, it might not be yet popular enough in the north for people to believe in its effectiveness. Second, the other possible channel fueling the spread, the displacement of burglary, can respond endogenously to the adoption of roll-down shutters. If the adoption of roll-down shutters is already low, as in the northern parts, the displacement due to the presence of roll-down shutters can be limited and, therefore, the spread might be very slow and limited. Besides, even though the adoption percentage is high in a neighborhood, it is almost never 100 percent. Meaning, there will most probably be some unprotected households without shutters left in that neighborhood. Changing the target from a 'familiar' location to a further away 'unfamiliar' location will be highly costly for an offender since prior knowledge regarding the opportunities of crime is central to criminal activity (Curtis-Ham et al., 2020). Therefore, moving to a further away vicinity due to the presence of roll-down shutters seems unlikely for offenders, and if any, we expect it to be limited and slow, resulting in the very slow spread of roll-down shutters.

Finally, a number of potential limitations need to be considered. First, we have specified the group interaction structure ex-ante and assumed that households are only affected by that spatial network. Thus, further study is required to extend the model by making use of an endogenous network structure and incorporating information with regard to social relationships between households. Second, for prospective research, longitudinal data at the level of households should be of interest to completely rule out the self-selection problem.

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Appendix B

Appendix: Social Contagion in Victim Precaution

B.1. Descriptives

In Figure B.2, we zoom in on the north-south divide in the adoption of one particular burglary prevention measured that became apparent in Figure 2.1. Each dot represents a municipality. On the horizontal axis is latitude of the location of the municipality; on the vertical axis, the adoption rate of a prevention measure, ranging from 0 to 1. We observe a stark geographic discrepancy in terms of the adoption of shutters, but not for other measures.

B.2. Variation over time in roll-down shutters

Figure shows the percentage of households with roll-down shutters over time in the Netherlands. Figure B.1 shows how the adoption of roll-down shutters has grown from 10 percent by the 1990s to around 20 percent currently.

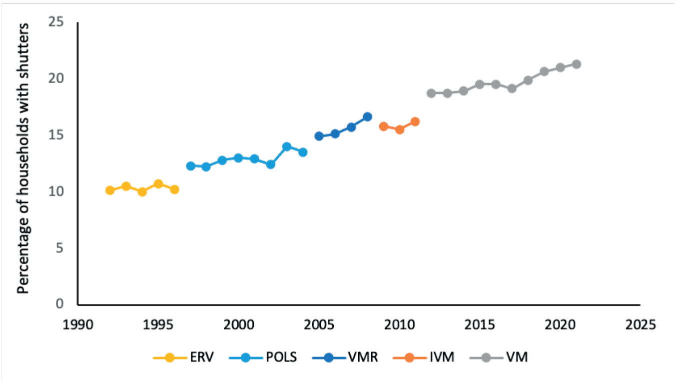

Figure B.1: Households with roll-down shutters. Source: CBS.

B.3. Results

In this section, we present the results of the main analysis including the estimates for the independent variables. It should be noted that in the spatial regression specifications, drawing conclusions from the point estimates of the partial derivatives approach would be misleading for the following reason. When an explanatory variable in a neighborhood changes, not only will the shutter adoption rate in that neighborhood change (direct effect), but the shutter adoption rate in other neighborhood will change as well (indirect effect) (LeSage and Pace, 2009).[1] The sum of direct and indirect effects is denoted as the total effect, which is the average effect of a change in the independent variables on the shutter adoption in all neighborhoods.

The total effects are presented in Table B.1. Since total effects are different for different spatial units in the sample, following LeSage and Pace (2009), we report one summary indicator. Different from Table 2.2, in the first column we do not allow for the presence of spillovers as a baseline comparison. We only include neighborhood-level fixed effects. In this specification, Equation 2.3 simplifies to the conventional linear regression model. In that case: β_2=0, λ=0 and $\delta \neq 0$. The second, third and forth columns are SLM, SAC, and GNSM specifications, respectively.

[1]The direct effect is the average impact from a change in neighborhood characteristics, β_1 in Equation 2.3. In other words, it can be explained as the average effect of the independent variables of households in a neighborhood on the rate of shutter adoption in that neighborhood. The indirect effect (or the contextual effect) refers to the average effect of a change in an independent variable of households in a neighborhood on the adoption of roll-down shutters of households in bordering neighborhoods, is represented by β_2 in Equation 2.3.

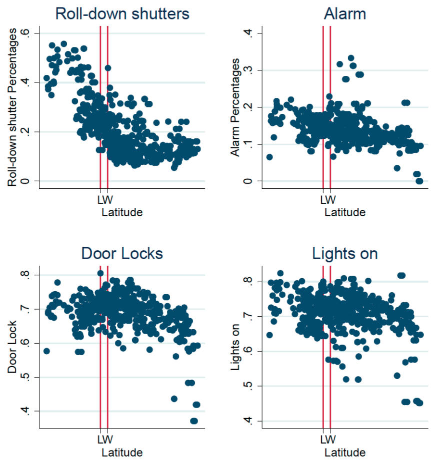

Figure B.2: The north-south variation in the adoption of burglary prevention measures. Each dot represents a municipality. L and V stand for Lek river and W river, respectively. Source: Netherlands Crime Survey 2012-2017.

Table B.1: Parameter estimates of roll-down shutters

VARIABLES	(1) OLS	(2) SLM	(3) SDM	(4) GNSM
Spatial lag of roll-down shutters (δ)		0.3872***	0.7370***	0.8732***
		(0.032)	(0.047)	(0.028)
(λ)				-0.6477***
				(0.027)
Total Effects				
Household size	0.0119	0.0117	-0.0780	-0.1219*
	(0.008)	(0.013)	(0.048)	(0.059)
Disposable household income (in 1.000)	-0.0003	-0.0003	0.0013	0.0021*
	(0.000)	(0.000)	(0.001)	(0.001)
Woman	-0.0161	-0.0136	-0.0856	-0.2407*
	(0.017)	(0.027)	(0.097)	(0.116)
Education	-0.0952***	-0.1122***	-0.2047***	-0.2763***
	(0.009)	(0.013)	(0.043)	(0.054)
Presence of children in the household	-0.0597***	-0.0749*	-0.1308	-0.2067
	(0.020)	(0.031)	(0.115)	(0.136)
Native	-0.0524***	-0.0825***	-0.0422	-0.0801
	(0.015)	(0.023)	(0.062)	(0.070)
Age	-0.0010**	-0.0015*	-0.0090**	-0.0143***
	(0.000)	(0.001)	(0.003)	(0.004)
Working	0.0045	0.0026	-0.1782	-0.2252*
	(0.017)	(0.026)	(0.108)	(0.130)
Rental house	-0.0925***	-0.1191***	-0.3377***	-0.5283***
	(0.015)	(0.023)	(0.094)	(0.134)
Apartment	-0.1100***	-0.1515***	-0.0262	0.0316
	(0.012)	(0.019)	(0.062)	(0.079)
Functioning of the municipality	-0.0082	-0.0025	-0.0442	-0.0537
	(0.008)	(0.013)	(0.040)	(0.046)
Liveability in the neighborhood	-0.0207***	-0.0290**	-0.0382	-0.0624
	(0.006)	(0.010)	(0.035)	(0.045)
Functioning of the police	-0.0194***	-0.0278**	-0.0467	-0.0715
	(0.007)	(0.010)	(0.037)	(0.044)
Social cohesion score	-0.0053	-0.0060	-0.0102	-0.0177
	(0.004)	(0.006)	(0.021)	(0.026)
Degree of urbanization	-0.0075***	-0.0091**	-0.0167*	-0.0219*
	(0.002)	(0.003)	(0.009)	(0.011)
Observations	10,428	10,428	10,428	10,428

Notes: The table shows results from estimating Equations 2.3 and 2.4. Based on data by neighborhood. Standard errors are robust to heteroskedasticity and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.2: Parameter estimates of roll-down shutters, north and south

VARIABLES	(1) SLM	(2) SDM	(3) GNSM
North			
Spatial lag of roll-down shutters (δ)	0.3324***	0.6172***	0.7992***
	(0.042)	(0.087)	(0.058)
(λ)			-0.5710***
			(0.039)
Observations	7,621	7,621	7,621
South			
Spatial lag of roll-down shutters (δ)	0.3579***	0.6132***	0.7967***
	(0.036)	(0.056)	(0.038)
(λ)			-0.6587***
			(0.052)
Observations	2,807	2,807	2,807

Notes: The table shows results from estimating Equations 2.3 and 2.4. Based on data by neighborhood. Not shown are estimated coefficients for covariates. Standard errors are robust to heteroskedasticity and reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

References

J. LeSage and R. K. Pace. *Introduction to spatial econometrics*. Chapman and Hall/CRC, 2009.

Chapter 3

The Crime-reducing Effect of Window Security

Gülbike Mirzaoğlu and Ben Vollaard[1]

We study the deterrent effect of a crime prevention measure, roll-down window shutters. We use peer effects in the adoption of this measure as the source of exogenous variation. We instrument the presence of roll-down shutters in a neighborhood with the average level of adoption in adjacent neighborhoods. Based on rich survey data for more than half a million households in the Netherlands, we find evidence for a substantial deterrent effect of this targeting hardening measure. A one-percentage point increase in the presence of roll-down window shutters lowers the rate of burglary victimization by about 0.1 percentage point.

Crime . Deterrence . Crime Prevention

3.1. Introduction

Property crime can only occur when an offender is aware of a potential target and when that target is not well guarded (Cook, 1986; Cornish and Clarke, 1986). As such, property crime is the outcome of an interaction. Potential offenders find out about criminal opportunities, on purpose or by chance, and exploit those that are sufficiently attractive. Potential victims, in their turn, expose themselves to crime risk but may conceal and protect their properties. Victim precaution has been shown to have a real deterrent effect, with sometimes positive spillovers to

[1]This is a joint paper with Ben Vollaard. Author contributions: B.V. conceived the study. B.V. and G.M. designed the analysis. G.M. collected the data and performed the analysis. All authors contributed to the final version of the manuscript. We gratefully appreciate Jan van Ours for his feedback.

other potential victims and negative spillovers at other times (Ayres and Levitt, 1998; Gonzalez-Navarro, 2013; Vollaard and Van Ours, 2011).

Of late, the crime-reducing effect of private crime control has received renewed interest because of its role in explaining the crime drop in industrialized countries since the 1990s (van Winden and Ash, 2012). Measures as diverse as track-and-trace systems in passenger cars (Ayres and Levitt, 1998; Gonzalez-Navarro, 2013) and burglary proof windows and doors (Vollaard and Van Ours, 2011) have been found to have a deterrent effect. Private crime prevention has greatly increased over the past decades. The evidence mounts that the broad scale at which these measures closed off criminal opportunities has contributed to the crime decline (Farrell, 2021).

The evidence base for a crime-reducing effect of private crime control is limited though, given that it is rare to find plausibly exogenous variation in the adoption of preventive measures. For instance, to the best of our knowledge, no study has provided causal evidence for a deterrent effect of burglar alarms.[2] One source of exogenous variation in victim precaution that has been used in a number of studies is changes in regulations. Ayres and Levitt (1998) and Gonzalez-Navarro (2013) exploit variation across cities or states in the timing of regulatory approval of a track-and-trace system that requires police follow-up. Vollaard and Van Ours (2011) exploit revised building codes for homes; van Ours and Vollaard (2016) a mandate for engine immobilizers in passenger cars.

In this paper, we exploit a new and hitherto unexploited source of exogenous variation in victim precaution: peer effects in the adoption of crime prevention measures.[3] One of the central tenets of crime models is that preventive behavior is the outcome of a trade-off between the costs and the benefits of preventive measures, with the benefits being dependent on the supposed decline in perceived crime risk (Salm and Vollaard, 2021). This seems an innocuous assumption of how potential victims arrive at decisions, but it is, in fact, questionable given the circumstances. This is because of the uncertainty around two primary inputs of this decision: the perceived risk of victimization, first, and the supposed deterrent effect of preventive measures, second. The victimization risk is highly heterogeneous across individuals, places and time, and learning about this risk by way of personal experience is not only slow but can also be exceedingly costly. The deterrence value of preventive measures is also largely unknown, primarily due to the highly endogenous nature of variation in the level of prevention – when comparing with other individuals – and the absence of feedback on the

[2]Even after adjusting for observable characteristics of homes and their dwellers, burglar alarms tend to be positively related with prevalence rates of domestic burglary (Tilley et al., 2015).

[3]Unlike market-channeled effects, peer effects represent how an individual's decision or outcome is directly influenced by her peers' outcomes or characteristics (Lin, 2010). In our case, individuals on the demand side of the market for crime coordinate their behavior, without interacting by way of the supply side of the market for crime.

returns to crime precaution – when making before-after comparisons within individuals. For example, if a burglar does not attempt to break into a home, then the potential victim does not know whether that is the result of precautionary measures or simply of a burglar not even considering the home as a target. Thus, both perceived risk and the deterrent effect of precaution are anyone's guess (Salm and Vollaard, 2021). Given this uncertainty, a potential victim may look at peers, in this case other potential victims, for guidance on how to best response to the crime risk (Amodio, 2019; Pressman, 2008).

We focus on a particular prevention measure for which we know the nature of the peer effects: roll-down window shutters. Next to privacy and climate control, roll-down shutters are meant to provide protection against burglary. It is not only a target-hardening measure, but it also conceals valuable properties, both during the day and at night, whenever the shutters are drawn. Roll-down window shutters are fairly expensive and not exactly an embellishment to the façade of a home, but surprisingly popular in some areas in the Netherlands. Based on detailed survey data for more than half a million households, Mirzaoglu (2021) shows that peer effects play a role in the adoption of this measure, with peers being neighbors and the effect declining with distance to the neighbors. Peer effects partly explain the spread of this prevention measure from the deep south further northwards in the Netherlands.

We rely on the same data as Mirzaoglu (2021) in this paper. Given the presence of peer effects, we use the average rate of roll-down shutters adoption in adjacent neighborhoods as an instrument for the presence of roll-down shutters in a neighborhood. We conduct the analysis at the level of neighborhoods as this is the lowest geographical identifier in our data – and also lowers the possibility of crime displacement, which would violate the exclusion restriction. As the southern parts are already fairly saturated with roll-down shutters, adoption of roll-down shutters may have a lower marginal effect. Therefore, we focus our empirical analysis on the northern parts of the Netherlands.

Our results can be summarized as follows. We find evidence for a substantial deterrent effect of roll-down shutters on the prevalence of domestic burglary. A one-percentage point increase in the presence of roll-down shutters in a neighborhood lowers burglary victimization by an estimated 0.1 percentage point in that neighborhood.

Our paper is structured as follows. The next section describes the data. In Section 3.3, we discuss our identification strategy. Results are provided in Section 3.4, followed in Section 3.5 by robustness checks. Section 3.6 concludes.

3.2. Data

Our primary source of data is the Netherlands Crime Survey (Veiligheidsmonitor), an annually repeated cross-sectional survey among households. It is one of the largest crime surveys in the world, relative to the size of population. We use six annual waves of the survey from 2012 to 2017. In each of these years, between August and December, on average some 270,000 randomly selected respondents aged 15 years or older were approached to complete the survey – either online or on paper. The response rate is 39 percent; providing us with a sample of 687,395 respondents.

Our data provide information on victimization of crime and private crime preventive measures, first of all. Burglary is a rare event. In our data, about one percent of households have been victim over the last 12 months. Given its rare occurrence, we use the five-year prevalence rate, which is also reported. This measure does not make a distinction between successful and unsuccessful burglary attempts. Thus our outcome measure combines burglary and attempted burglary attempts. Respondents are asked about five types of burglary prevention measures in the household: roll-down window shutters, alarm systems, extra door locks, outdoor lights, and leaving indoor lights on when not at home. The year-on-year variation in the presence of roll-down shutters is minimal, which is why we analyze the pooled survey data and do not exploit the time dimension of our data.

In addition, the survey provides a number of individual and neighborhood characteristics. Individual characteristics include gender, education level (low, medium, and high), being immigrant or not, age, employment status, household size, number of children in the household, disposable income of the household,[4] type of residence (apartment, detached house, and other types), and whether the house is rented or owned. Neighborhood characteristics include a measure of liveability of the neighborhood, satisfaction with locally provided police services, feelings of cohesion, views towards the functioning of the municipality, and degree of urbanization. Social cohesion is a score based on responses to four statements: "Residents hardly know each other," "Residents interact in a pleasant manner," "This is a cozy neighborhood where people help each other and do things together," "I feel at home with the residents of this neighborhood." A confirmatory response is assumed to reflect higher levels of social cohesion. The score is scaled from 0 to 10. Similarly, liveability is a score based on the response to five statements: "The neighborhood has good outdoor lighting," "There are good playgrounds for children in the neighborhood," "In the neighborhood, flowerbeds, parks, and gardens are well maintained," "In the neighborhood, roads, paths, and squares are well-maintained," "There are good facilities for youth in the neighborhood."

[4]Disposable household income reflects gross income minus paid income transfers, insurance premiums, health premiums, and income tax: it is possible that disposable income is negative. Minimum and maximum values are not shown for reasons of confidentiality.

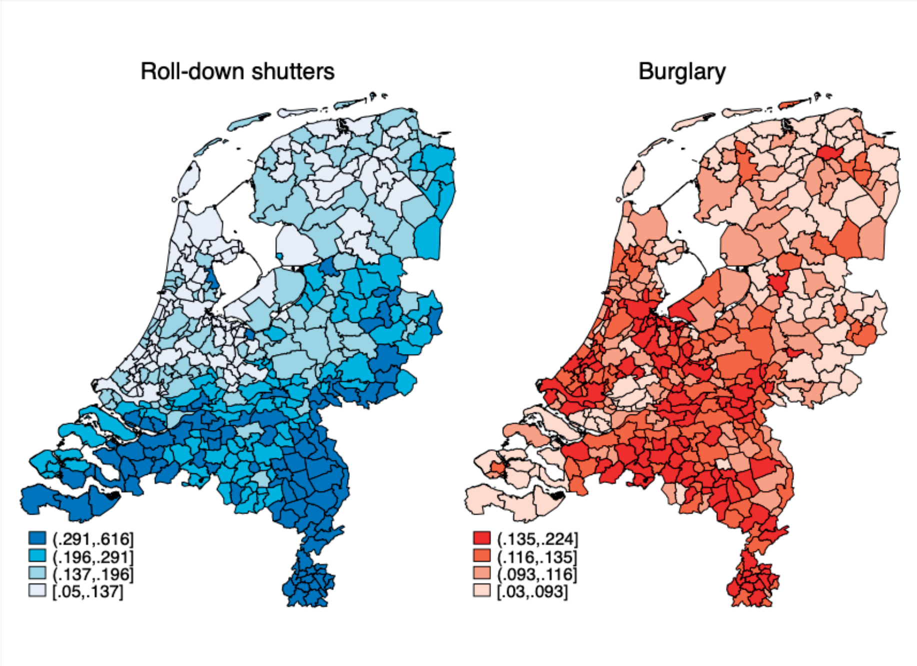

Figure 3.1: Geographic spread of roll-down shutters and victimization of burglary, the Netherlands, 2012-2017. Data aggregated at the level of municipalities.

We exclude respondents that do not answer or answer 'don't know' or 'refuses to answer' to questions about burglary victimization and the presence of roll-down shutters, resulting in an estimation sample of 582,522 respondents.

We match the survey data with register data on the place of residence of each respondent. The lowest geographical identifier in our data is the neighborhood ('buurt'). Neighborhoods are part of districts ('wijk'), which are part of municipalities. On average, the number of residents per neighborhood in 2017 was 1,298. The definition of neighborhoods is fairly similar to US census tracts: in the US, block groups generally contain between 600 and 3,000 residents. Our sample consists of 388 municipalities and covers 10,946 out of the 13,150 neighborhoods as defined in 2017. On average, we have 53 respondents per neighborhood, which amounts to about four percent of the number of neighborhood residents.

Figure 3.1 shows geographical variation in the presence of roll-down shutters and the rate of victimization of burglary at the level of municipalities. The

Table 3.1: Summary statistics

	North		South		All			
	Mean	SD	Mean	SD	Mean	SD	Min.	Max.
Crime Victimization								
Burglary and attempted burglary	0.123	0.329	0.136	0.343	0.126	0.332	0	1
Private Crime Prevention Measures								
Roll-down shutters	0.155	0.362	0.353	0.478	0.202	0.402	0	1
Burglary alarm	0.149	0.357	0.164	0.370	0.153	0.360	0	1
Additional door lock	0.750	0.433	0.755	0.430	0.751	0.432	0	1
Outdoor light	0.827	0.378	0.860	0.347	0.835	0.371	0	1
Leaving lights on when away	0.744	0.436	0.779	0.415	0.753	0.432	0	1
Neighborhood Characteristics								
Liveability in the neighborhood	7.452	1.280	7.474	1.290	7.457	1.283	1	10
Social cohesion score	6.229	1.859	6.311	1.853	6.248	1.858	0	10
Functioning of the police	3.355	0.872	3.308	0.886	3.344	0.875	1	5
Functioning of the municipality	3.343	0.834	3.303	0.842	3.334	0.836	1	5
Degree of urbanization	3.421	1.264	3.057	1.172	3.335	1.252	1	5
Individual Characteristics								
Woman	0.525	0.499	0.519	0.500	0.523	0.499	0	1
Age	51.504	17.944	52.602	17.768	51.765	17.908		
Native	0.723	0.447	0.774	0.418	0.735	0.441	0	1
Non-western immigrant	0.165	0.371	0.120	0.325	0.154	0.361	0	1
Education	2.045	0.826	1.977	0.831	2.029	0.828	1	3
Working	0.533	0.499	0.516	0.500	0.529	0.499	0	1
Unemployed	0.023	0.151	0.021	0.144	0.023	0.150	0	1
Retired	0.270	0.444	0.284	0.451	0.273	0.446	0	1
Student	0.074	0.262	0.068	0.252	0.073	0.260	0	1
Household size	2.527	1.194	2.502	1.152	2.521	1.184		
Disposable household income (in 1.000)	44.335	39.860	43.451	29.564	44.125	37.664		
Presence of children in the household	0.255	0.436	0.229	0.420	0.249	0.432	0	1
Rental house	0.283	0.451	0.256	0.437	0.277	0.447	0	1
Own house	0.717	0.451	0.744	0.437	0.723	0.447	0	1
Apartment	0.236	0.424	0.174	0.379	0.221	0.415	0	1
Detached house	0.736	0.441	0.801	0.399	0.752	0.432	0	1
Observations	443,758		138,764		582,522			

Notes: See Section 3.2 for a definition of social cohesion and liveability.

adoption of roll-down shutters is geographically clustered, with a much higher rate in the south than in the north of the country. Given the very high rate in the south – possibly reflecting close to full saturation given that many homes are unlikely to the fitted with roll-down shutters because they have monumental status or fall under rent control – we expect the marginal effect of roll-down shutters to lower. Therefore, we focus the analysis on the north.[5] We split the country into two parts, with the major rivers Waal and Lek being the dividing line. In Section 5, we discuss the results for the south of the country.

Table 3.1 presents the summary statistics for the north, the south, and all of the country. As can be inferred from the number of observations, what we define to be the north is much larger than the south. The five-year prevalence rate of burglary (attempts) for the north of the country is 12 percent. Except for the presence of roll-down window shutters, individual and neighborhood characteristics are roughly similar for both parts of the country. Around half of the respondents are women, have paid-work, and have a medium level of education. Most of the respondents are native and homeowners. While in the north the rate of adoption of roll-down shutters is around 16 percent, it is 35 percent in the south. On average, about one out of five homes features roll-down shutters.

3.3. Empirical approach

Our aim is to identify the effect of roll-down shutters on victimization of burglary. Our estimation equation looks as follows:

$$Burglary_h = \gamma_0 + \gamma_1 Shutters_h + \gamma_2 X_h + \epsilon_h \tag{3.1}$$

where $\mathrm{Burglary_h}$ denotes victimization of burglary of household h as well as attempted burglary. γ_1, the parameter of interest, presents the direct effect of roll-down shutters on victimization of burglary (attempts). $\mathrm{X_h}$ denotes the individual and neighborhood characteristics discussed in Section 3.2. The error term is denoted by ϵ_h.

We conduct our analysis at the level of neighborhood, which is the smallest geographical identifier in our data. Thus, for household h living in neighborhood i populated with N households, we transform the variables in Equation 3.1 as follows:

$$\sum_{h=1}^{N} \frac{Burglary_{hi}}{N} = \alpha_0 + \beta_1 \sum_{h=1}^{N} \frac{Shutters_{hi}}{N} + \beta_2 \sum_{h=1}^{N} \frac{X_{hi}}{N} + \sum_{h=1}^{N} \frac{\epsilon_{hi}}{N} \tag{3.2}$$

Then, our estimation equation becomes:

[5]Then, our estimation sample in the main analysis consists of 443,758 respondents.

$$Burglary_i = \alpha_0 + \beta_1 Shutters_i + \beta_2 X_i + \epsilon_i \tag{3.3}$$

All variables are defined similarly to Equation 3.1, but now denote average levels for neighborhood i. Then, β_1 is the weighted average of γ_1. As the number of observations per neighborhood varies greatly (see the Appendix C) we use frequency weights based on the number of observations per neighborhood when estimating Equation 3.3.

A primary challenge for identifying β_1 in Equation 3.3 is potential endogeneity in the presence of roll-down shutters with respect to the burglary rate. Households may adopt roll-down shutters for reasons that are likely to be related to the burglary risk. Therefore, estimating Equation 3.1 by means of ordinary least squares (OLS) may provide us with a biased estimate of γ_1. As discussed in the introduction, we employ an instrumental variable (IV) approach to address endogeneity in the presence of roll-down shutters. We use the average rate of shutter adoption in adjacent neighborhoods as our instrument. The first-stage equation can be specified as follows:

$$Shutters_i = \alpha_1 + \beta_3 WShutters_i + \beta_4 X_i + \epsilon_{1i} \tag{3.4}$$

where $WShutters_i$ denotes our instrument, with W being a nxn row-normalized binary symmetric matrix. The element w_{ij} of W is 1 if the locations i and j are adjacent and 0 otherwise.[6]

Our identification strategy provides a Local Average Treatment Effect (LATE) for the neighborhoods affected by the instrument. The validity of our instrument depends on three conditions. First, it leads to sufficient variation in the adoption of roll-down shutters. Second, the instrument only affects the outcome, victimization of burglary, by way of the adoption of roll-down shutters and not in other ways. Third, the instrument should be as good as randomly assigned. Below, we address each of these assumptions.

3.3.1. Instrument relevance

The relationship between a locality's presence of roll-down shutters $Shutters_i$ and that of its adjacent localities $WShutters_i$ is shown in Figure 3.2. In this plot, known as a Moran scatter plot, the rate of roll-down shutter adoption in a locality (on the horizontal axis) is set against the adoption rate in neighboring localities (on the vertical axis). The variables are standardized, with mean zero and standard deviation equal to one. Municipalities are chosen as geographical units for ease

[6] By definition, diagonal entries are zero because a unit is not a neighbor to itself. Since W is row-normalized, multiplying W with the presence of roll-down shutters in neighborhood i, $Shutters_i$, gives the weighted average of the presence of roll-down shutters of the neighbors of i.

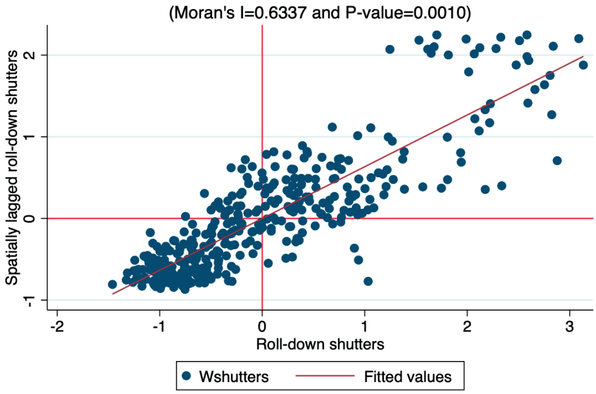

Figure 3.2: Moran scatter plot for the presence of roll-down shutters, the Netherlands, 2012-17. The horizontal axis shows the standardized adoption percentage of roll-down shutters in a municipality; the vertical axis shows the standardized average shutter adoption percentage in neighboring municipalities.

of presentation. Overall, roll-down shutter adoption rates in a municipality are strongly positively associated with adoption rates in neighboring municipalities.

Next, we formally test this relationship based on the first stage equation. Based on data for the north of the country, the F-statistic for the excluded instrument is reported in the table with estimation results in the following section, Table 3.2, in Columns 2 and 4. Irrespective of whether we include covariates in the first stage, the F-statistic is highly statistically significant and well above the threshold for weak instruments (Stock and Yogo, 2005). We conclude that our instrument is sufficiently strong to estimate the effect of roll-down shutters on burglary.

3.3.2. Exclusion restriction

The assumption that our instrument only affects the outcome by way of a neighborhood's rate of adoption of roll-down shutters is not testable. Below, we qualitatively discuss channels that could violate the exclusion restriction.

One channel that is of concern is crime displacement in response to the adoption of roll-down shutters in adjacent neighborhoods. A relatively high rate of adoption of roll-down shutters in a neighborhood could prompt burglars to shift their activities to neighborhoods with a relatively low rate of adoption. If that happens to be the neighborhood for which we want to identify the causal effect of roll-down shutters on crime, then our instrument has a direct effect on the outcome, and the exclusion restriction is violated.[7]

Displacement of burglary to another neighborhood in response to the presence of roll-down shutters is not a given. Burglars end up at a certain location for a reason. As in other social contexts, offenders are intentional decision-makers with (bounded) rationality who respond to economic incentives. They happen to know of an opportunity specific to a time and location, and they know of a way of exploiting this opportunity. For example, offenses are more likely to occur in places that the offender visits more frequently, longer, and recently (Alessandretti et al., 2018; Bernasco, 2019; Menting et al., 2016). Changing targets is not straightforward. Information on opportunities in a particular neighborhood, for instance, may not easily transfer to another neighborhood. Reliable information on alternative opportunities is of great importance to the potential offender since committing a crime is a risky proposition (Curtis-Ham et al., 2020). Thus, displacement, if it occurs, is unlikely to be complete (Cook, 1986). The empirical evidence suggests that negative spillovers in response to situational crime prevention are limited. Spillovers may well be positive rather than negative, but in that case they are also limited (Bowers et al., 2011).

Given that our unit of analysis is a neighborhood, with on average more than 600 households, displacement, if present, may well be within the same neighborhood rather than to other, adjacent neighborhoods. Penetration rates of roll-down shutters may well be such that alternative, unprotected targets within the neighborhood remain available, particularly in the north of the country. For instance, roll-down shutters are less likely to be installed in social housing and monumental buildings. In any case, displacement tends to decline with distance, limiting the displacement from one neighborhood to another. To see how sensitive our results are to the possibility of spatial spillovers, we conduct our analysis also at larger geographical units in Section 3.5.2.

3.3.3. Independence assumption

The third assumption is that our instrument, $WShutters_i$, should also be independent of potential outcomes, conditional on covariates. This assumption may fail if there are omitted common causes of $Burglary_i$ and $WShutters_i$, for

[7] A second possible channel is the effect of our instrument on other crime preventive measures. Households may interpret the prevalence of roll-down shutters in surrounding neighborhoods as a signal of a high burglary rate and may take a range of *other* prevention measures in response. Those measures may affect the crime rate, and thus the exclusion restriction is violated. Alternatively, observing the widespread use of roll-down shutters may lead households to believe that they are very effective and also take this particular measure, in which case the exclusion restriction is not violated. We cannot include the presence of other prevention measures as covariates in Equation 3.3 since these variables may well be endogenous. Thus, we cannot exclude that we over-estimate the effect of roll-down shutters on burglary due to spillovers on other preventive measures.

instance, differences in terms of community, culture, and values regarding crime or religion. We include a number of observed household and neighborhood characteristics to address potential non-random selection. Moreover, Mirzaoglu (2021) shows that peer effects in the adoption of roll-down shutters are robust to omitted variables that are time-invariant and specific to each neighborhood.

3.4. Estimation results

We estimate our baseline model, Equation 3.3, and present the results in Table 3.2. The first row of the table shows the estimated effect of roll-down shutters on the victimization of burglary, including unsuccessful burglary attempts. The first and third columns present the results from OLS regressions; the second and fourth columns the 2SLS results. In the first two columns, we do not include any covariates; in the last two columns, we do include covariates.

The OLS results in Columns 1 and 3 show that the roll-down shutters are negatively related to burglary. These estimates are likely to be biased towards zero, as discussed. We do indeed find considerably higher estimated effects when instrumenting the presence of roll-down shutters in Columns 2 and 4. Based on the latter results, a one-percentage point increase in the presence of roll-down shutters lowers victimization of burglary by about 0.17 percentage point without any covariates and by 0.08 percentage point once including covariates.

The estimated effect of roll-down shutters is considerably lower when including covariates, which suggests the presence of some remaining non-random selection into roll-down shutters. Maybe the instrumental variable partly reflects characteristics of residents and their environment that are common to adjacent neighborhoods – and that are related to the outcome variable. The control variables address this apparent non-random variation at least in part.

Table 3.2: Estimated effect of roll-down shutters on burglary

Dep.var.: burglary (attempts)	(1) OLS	(2) IV	(3) OLS	(4) IV
Roll-down shutters	-0.0846***	-0.1734***	-0.0239***	-0.0844***
	(0.009)	(0.016)	(0.008)	(0.018)
Control variables	NO	NO	YES	YES
Observations	443,758	443,758	443,161	443,161
Number of clusters	7,991	7,991	7,621	7,621
F-test excluded instrument		727.1		520.2

Notes: The table shows results from estimating Equation 3.3. Based on data by neighborhood. The sample is restricted to the north of the Netherlands. Not shown are estimated coefficients for covariates. Standard errors clustered at the level of neighborhoods.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.5. Robustness

In this section, we explore the sensitivity of the estimated effect β_1 from Equation 3.3. Table 3.3 presents the 2SLS results, including covariates. For ease of comparison, the baseline estimate is presented in the first column.

3.5.1. Regional differences

Up to now, we have focused the analysis on the north of the country, as discussed in Section 3.2. Next, we compare our baseline estimates with the rest of the country. The second column of Table 3.3 presents the 2SLS results for the whole country. The specification includes a fixed effect for the part of the country. We include this fixed effect because the north and the south can be different in many ways, as explained in Section 3.4, and these differences may be related to both the likelihood to install roll-down shutters and of being burgled. The results show that roll-down shutters are still negatively related to the burglary risk but that the size of the effect is smaller, reflecting a lower effect in the south. The F-statistic shows that the instrumental variable is still sufficiently strong for the sample restricted to the south (not shown). The difference in the estimated effect may be related to another factor: the scale at which this precautionary measure is adopted. Higher levels of adoption of roll-down shutters may go together with a lower marginal effect, i.e. the deterrent effect of this prevention measure may be

Table 3.3: Estimated effect of roll-down shutters, sensitivity analysis

Dep.var.: burglary (attempts)	(1) Baseline specification	(2) Whole country	(3) North, district level	(4) North, municipality level
Roll-down shutters	-0.0844***	-0.0287**	-0.0981***	-0.0996**
	(0.018)	(0.012)	(0.026)	(0.046)
South		0.0247***		
		(0.003)		
Observations	443,161	581,697	443,161	443,161
Number of clusters	7,621	10,428	2021	268
F-test excluded instrument	520.2	1044	270.6	44.22

Notes: The table shows the results from estimating Equation 3.3. Based on data by neighborhood in Columns 1-2, district in Column 3, or municipality in Column 4. Column 2 shows the results for all of the country, and Columns 1, 3, and 4 for the north only. Not shown are estimated coefficients for covariates. Standard errors clustered at the level of neighborhoods in Columns 1-2, districts in Column 3, or municipalities in Column 4.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

subject to decreasing returns to scale. In any case, these results imply that our findings may be conditional on the overall level of adoption of this precautionary measure.

3.5.2. Displacement

Displacement of burglary between neighborhoods in response to installation of roll-down shutters is of concern because it violates the exclusion restriction of our instrument, as discussed in Section 3.3. Displacement tends to decline with distance. Thus, if displacement occurs, then the resulting bias would be smaller at higher geographical levels.

To test the sensitivity of our results, we also conduct the analysis at higher levels of aggregation. First, we look at the level of districts, of which there are a little over 2,000 compared to the 8,000 neighborhoods. As our instrumental variable, we use the average rate of adoption of roll-down shutters in the districts. Second, we look at the level of municipalities, of which there are only 268. In this case, our instrumental variable is the average adoption rate of roll-down shutters in municipalities.

Table 3.3 presents the results. Column 3 shows our findings at the level of districts; Column 4 at the level of municipalities. We find the estimated effect to be similar in magnitude to our baseline results. These findings suggest that we are unlikely to overestimate the effect of roll-down shutters on burglary due to crime displacement.

3.6. Conclusion

We identify a crime-deterrent effect of private victim precaution based on a hitherto unexploited source of plausibly exogenous variation in the use of a specific prevention measure: peer effects. We focus on a fairly popular measure that is meant to deter burglary: roll-down window shutters. To isolate as-good-as-random variation in the rate at which roll-down shutters are installed at the neighborhood level, we use the average presence of roll-down shutters in adjacent neighborhoods as our instrument. Based on this instrumental variable design, we find that the adoption of roll-down shutters does indeed lower the victimization of burglary – at least in areas in the north of the country that are not saturated with roll-down shutters. A one-percentage point increase in the presence of roll-down window shutters lowers the rate of burglary victimization by an estimated 0.1 percentage point. Adoption of roll-down shutters in the north of the country varies between 5 and 25 percent (leaving out some extremes in the tails of the distribution). Thus, taking those two extremes, a 20-percentage points difference in the adoption of roll-down shutters translates into a 2-percentage points difference in the rate of burglary victimization. On average, 12 percent of households have experienced burglary over the last five years (around 54,582 out of 443,758 households in the north). Thus, roll-down shutters can have a substantial effect on the rate of victimization of burglary, a reduction in the order of 10 to 20 percent. Our findings confirm that relatively straightforward changes in the immediate environment of potential targets of crime can have a clear, favorable effect, in line with the premise of situational crime prevention.

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Appendix C

Appendix: The Crime-reducing Effect of Window Security

C.1. Neighborhood size distribution

Below, we show the cumulative distribution of the size of population size by neighborhood in Figure C.1.

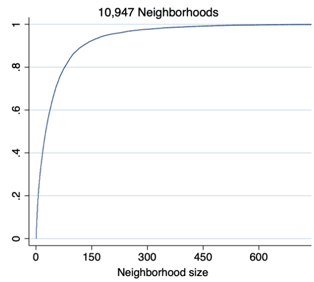

Figure C.1: Cumulative distribution of the neighborhood size (number of households per neighborhood)

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This dissertation contains three chapters on topics in experimental economics and the economics of crime. The first chapter studies gender and sex differences in uncertainty attitudes by following a broader and more inclusive concept of gender instead of the conventional binary approach. The second chapter investigates the extent to which the behavior of individuals is interrelated in terms of crime prevention. Specifically, it studies whether social contagion could explain why some visible crime prevention measures are highly popular in some areas but rarely used in others. The third chapter demonstrates the crime-reducing effect of private crime preventive measures. It studies the role of the "potential" victims and how their actions can deter crime.

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