Three Essays in Applied Microeconomics

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Summary

This thesis focuses on three vulnerable groups in Europe that have recently been highlighted both in media and in the economics literature; and that are policy priorities. Chapter 1 is a joint work with Giovanni Mastrobuoni[1] which focuses on prisoners and peer effects in prison. Studies that estimate criminal peer effects need to define the reference group. Researchers usually use the amount of time inmates overlap in prison, sometimes in combination with nationality to define such groups. Yet, there is often little discussion about such assumptions, which could potentially have important effects on the estimates of peer effects. We show that the date of rearrest of inmates who spend time together in prison signals with some error co-offending, and can thus be used to measure reference groups. Exploiting recidivism data on inmates released after a mass pardon with a simple econometric model which adjusts the estimates for the misclassification errors, we document homophily in peer group formation with regards to age, nationality, and degrees of deterrence. There is no evidence of homophily with respect to education and employment status.

Chapter 2 evaluates a policy in the English county of Essex that

[1]Collegio Carlo Alberto and University of Essex.

aims to reduce domestic abuse through informing high-risk suspects that they will be put under higher surveillance, hence increasing their probability of being caught in case of recidivism, and encouraging their victims to report. Using a Regression Discontinuity Design (RDD), it underlines that suspects that are targeted by the policy are more 9% more likely to be reported again for domestic abuse. Although increasing reporting is widely seen as essential to identify and protect victims, this paper shows that policies to increase reporting will deter crime only if they give rise to a legal response. Moreover, results highlight that increasing the reporting of events of that do not lead to criminal charges may create escalation and be more detrimental to the victim in the long run.

Chapter 3 investigates how migrants in the United Kingdom respond to natural disasters in their home countries. Combining a household panel survey of migrants in the United Kingdom and natural disasters data, this paper first shows, in the UK context, that male migrants are more likely to remit in the wake of natural disasters. Then, it underlines that to fund remittances male migrants also increase labour supply, decrease monthly savings and leisure. By showing how migrants in the UK adjust their economic behaviours in response to an unexpected shocks i.e. natural disasters, this paper demonstrates both how UK migrants may fund remittances and that they have the capacity to adjust their economic behaviours to increase remittances.

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Introduction

Various vulnerable individuals in Europe have been disregarded both by governments and media in the past decades. Resource scarcity, potential unpopular reforms and societal taboos prevented thousands of individuals in Europe from geting enough attention to become a policy priority and see their situation improved. However, recent events shed light on some of these vulnerable groups and they are now at the top of the policy agenda. This thesis focuses on three vulnerable groups in Europe that have recently been highlighted both in media and in the economics literature; and that are policy priorities. The first chapter of this thesis looks at prisoners in Italy and examines how peer groups develop in prisons. The second chapter sheds light on victims of domestic abuse and investigates the impact of a policy to support them in the English county of Essex. Finally, the last chapter looks at migrants from developing countries that live in the UK and shows how natural disasters in their country of origin affect their economic behaviours and financial situations. All chapters use microeconometrics methods to establish causal effects.

The first vulnerable group that this thesis studies is prisoners. More than 10 million people are currently imprisoned around the world (Walmsley, 2013), and rates of incarceration are exhibiting increasing trends. This contributes to the shocking overcrowding conditions that prisoners are facing in European prisons, which were recently revealed by news media in France, Italy and in the UK. Overcrowding leads to negative consequences for inmates privacy, out-of-cell activities, healthcare, peace and safety. One of the main problems to address for policymakers is that time in prison does not seem to effectively reduce crime and a large fraction of inmates re-offend, which contributes further to overcrowding. For instance, in Italy and in the United States more than 2/3 of prisoners are re-incarcerated within three years (National Institute of Justice, 2016). This is a great concern for most governments around the world, including the ones in Italy, the UK, and the United States who spend on average between $30,000 and $42,000 per prisoner each year.

To cope with overcrowding and deal with financial pressure, more and more European countries try to increase the prevalence of non-prison sentences (Council of Europe, European Committee on Crime Problems, 2016). However, prisons also protect societies from dangerous criminals and alternatives to prison sentences cannot apply to all cases. Thus, it is crucial to better understand how prisons may affect post-release behaviours and recidivism. One of the factors that may reinforce criminal activities is peer effects that develop in prison. There is a growing economics literature on the impact of peer effects in prison on recidivism (Bayer et al., 2009; Ouss, 2011; Drago et al., 2009). These studies rely on sociology literature to define peer groups and, then, they analyse the impact of peer effects in prison on recidivism. However, to my knowledge, there is no empirical work that investigates directly how peer groups develop in prison. The objective of the first paper of this thesis is to bridge this gap and shed light on the characteristics that make inmates more likely to form groups in prison that lead to joint recidivism.

To do so, this chapter exploits the 2006 Italian prison pardon that gave rise to a random release of large numbers of inmates across several prisons. More specifically, we study inmates who benefited from the prison pardon and re-offended in order to investigate the determinants that make them more likely to re-offend in group with prisoners with whom they were released. The key feature of our data that makes it unique for the purpose of our study, is that we observe the post-release behaviour of groups of substantial size that were released at the same time from different prisons. The two main factors that influence the likelihood of partnerships are nationality and age. Inmates are more likely to partner up when young and when partners are of similar age. The fact that criminals are more likely to partner up with inmates who previously committed different types of crime suggests that prisons might sometimes allow inmates to learn new skills and move to a different crime.

The second chapter of this thesis focuses on a vulnerable group that, mainly due to social taboos, has suffered silently for decades i.e. victims of domestic abuse. In October 2017, the *Me Too* movement encouraged many victims of domestic abuse to report on social media through using #MeToo as a hashtag. It raised awareness worldwide on the prevalence of domestic abuse and highlighted the necessity to push these victims that stay silent to report. Violence against women and, particularly intimate partner violence and sexual violence, is a major public health problem and a violation of women's human rights. Global estimates published by the World Health Organisation (2017) indicate that about 1 in 3 women worldwide have experienced either physical and/or sexual intimate partner violence or non-partner sexual violence in their lifetime.

One of the issues that researchers and policymakers face to address domestic abuse is that most events go unreported. For instance, in the UK, according to the Office for National Statistics (ONS), only 20% of abuse is reported to the police (ONS, 2016). An important factor that leads to under-reporting is that some victims do not trust the police. In the UK, in 2011-12, one of the main reasons for not reporting abuse was that victims perceived the police would not (or could not) do anything about it (ONS, 2016). In line with this, a survey for the 2014 Her Majesty's Inspectorate of Constabulary finds that 30% of victims do not report because of lack of trust or confidence in the police to take action in the wake of their reporting.

Thus, the second chapter of this thesis analyses the impact of a policy in the English county of Essex that pushes victims of domestic abuse to report to the police through increasing trust in the police. The policy also informs offenders that they will be put under higher surveillance. Using a Regression Discontinuity Design, this chapter underlines that suspects who are targeted by the policy are 9% more likely to be reported again for domestic abuse. Although increasing reporting is widely seen as essential to identify and protect victims, this paper shows that policies to increase reporting will deter crime only if they give rise to a legal response. Moreover, results highlight that increasing the reporting of events that do not lead to criminal charges may create escalation and be more detrimental to victims in the long run. These results are insightful to policymakers designing future policies to protect victims by encouraging them to report violence.

Finally, the last vulnerable group covered in this thesis is migrants who live in Europe and originate from developing countries. The migration crisis in Europe that started in 2015 shed light on the precarity of refugees and, more generally, on obstacles that migrants face. The media coverage of the crisis created vivid debates on how to deal with large flows of migrants and integrate them. Some countries implemented policies to welcome migrants. For instance, in 2015 Germany allowed Syrian refugees who had already registered elsewhere in the European Union (EU) to enter Germany and register there, temporarily suspending an EU law that requires asylum seekers to be returned to the first country they entered (CNN, 2018). Unlike Germany, other countries implemented hostile measures to dissuade refugees from staying in the country. For instance, in March 2017, the Hungarian parliament approved a law allowing all asylum seekers to be detained (Independent, 2017). In France, until July 2018, citizens helping refugees could be prosecuted (RTL, 2018). Political responses to support migrants triggered a wave of scepticism among many European citizens. As a result, political parties and movements using anti-migrant rhetoric have gained increasing support in Europe in the past 3 years. In the UK, the UKIP, which has taken a far-right turn, keeps on attracting new members (The Guardian, 2018). In France, in 2017, Marine Le Pen, the far-right candidate reached the second round of the presidential election (Ministere de l'interieur, 2017), and in 2018, the far-right politician Matteo Salvini became deputy prime minister in Italy (BBC, 2018). To protect migrants from detrimental policies, more work is needed to understand problems they are facing.

In the third chapter I study how migrants in the United Kingdom adjust their economic behaviours in response to unexpected shocks, e.g. natural disasters in their home countries. By combining a household panel survey of migrants in the UK and natural disasters data, I demonstrate that male migrants are more likely to remit in the wake of natural disasters. By investigating mechanisms that are used by migrants to fund remittances, this chapter shows that male migrants increase labour supply, decrease monthly savings and leisure. Results suggest both how UK migrants may fund remittances and that they have the capacity to adjust their economic behaviours to increase remittances. However, results also emphasise that migrants report worse financial situations when their countries are hit by natural disasters. This suggests that sending money back home may make them vulnerable and supportive policies should be designed to support these migrants in such situations.

Chapter 1

Partners in Crime: Evidence from Recidivating Inmates

1.1 Introduction

More than 10 million people are currently imprisoned around the world Walmsley (2013), and rates of incarceration are exhibiting an increasing trend. This is of great concern for most governments around the world, including the ones in Italy, the UK, and the United States who per prisoner each year spend on average between $\$30,000$ and $\$42,000$[1]. These trends lead to overcrowded prisons and, in England and Wales, 71 of the 118 prisons are currently welcoming more prisoners than their actual capacity (Day et al. (2015)), which is putting budgets even more under pressure. Moreover, a large fraction of inmates has been in prison a number of times. For instance, in Italy and in the United States more than 2/3 of prisoners are re-incarcerated within three years National Institute of Justice (2016). At times of resources scarcity, preventing

[1] See the US Federal Bureau of Prisons (https://www.bop.gov/), Ministry of Justice (2013), and the Italian Justice Department (https://www.giustizia.it/)

the curse of recidivism is a major priority.

One important channel that has been shown to worsen post-release recidivism are peer effects that develop behind bars. The criminology literature emphasised decades ago how interactions with other inmates, other peers, can favour crime (Clemmer (1950)). There is now a growing literature in economics on the role of such peer effects, where peers are either cellmates, or inmates spending time in the same facility, and possibly sharing some socioeconomic or criminal characteristics.

Bayer et al. (2009), for example, exploit the plausibly exogenous exposure of individuals to different peers within prison facilities based on the variation in the incarceration spells. They find that inmates who were in contact in prison with criminals incarcerated for the same type of crime are more likely to re-offend. Ouss (2011), using a French administrative dataset, and a similar identification strategy, but this time at the prison cell-level, finds evidence of peer effects for skill-intensive offences. Drago et al. (2009) exploit the 2006 Italian prison pardon in order to demonstrate how peers' incentives not to re-offend (larger future sentences) influence inmates' post-release recidivism behaviour. In this paper peers are inmates who spent time in the same prison and share the same nationality.[2]

It is common in these papers to regress individual recidivism on the average recidivism or the average expected sentence of a peer group. A potential limitation of these empirical studies is that peer status cannot be observed. While it is plausible that inmates who share the same cell or belong to the same ethnic group become peers, it is also plausible to think that peer status

[2]Using more aggregate data, Glaeser et al. (1996) find positive peer effects in criminal behaviour but only for less serious crimes. Ludwig and Kling (2007), instead, find little evidence of peer effects in deprived neighbourhoods.

may be broader or narrower, introducing potential biases in the empirical findings. As pointed out by Manski (1993), in order to infer any causal impact of peer effects, measuring the proper reference groups is the cornerstone of peer effects studies.

Peer effects can occur through different mechanisms such as skills transfers or the elaboration of new partnerships for reoffending purposes. Papers on peer effects in prison are usually not able to distinguish between these different types of peer effects. The purpose of this paper is to show the driven factors of co-offending. By doing so, it will also contribute to improve the understanding of peer formation, which might also help designing better ways to distribute inmates across prison facilities.

The criminology literature has emphasised that criminals may find it attractive to co-offend for many reasons. McCarthy et al. (1998) point out that partnering up enables labour division. Weerman (2003) highlights, among other things, the financial benefits incurred by co-offending, while Felson (2003) emphasises that co-offending can benefit the exchange of knowledge. More related to our study, based on a small but rich panel survey, the Cambridge Study in Delinquent Development, Farrington et al. (1991) list a number of characteristics that make co-offenders more likely to partner up, such as similarity in terms of age, sex, race, and criminal experience. The economics literature is relatively silent on co-offending and little is known on what makes inmates form peer groups and, more specifically, peer groups in prison.

Stevenson (2015), using data from juvenile facilities in Florida, shows that peer criminal experience and gang affiliation of peers in prison only affect recidivism when peers live close by. She interprets these results as a proof that network formation plays a role in recidivism. However, she is not able to clearly identify peers when re-offending. One of the main purposes of our study is to better understand the role of network formation in prison in order to pave the way for more accurate studies on peer effects.

We contribute to the literature on peer effects exploiting information on peer groupings that, in spite of being widely available in prison data, has been widely disregarded: the date of re-incarceration. We show that, for individuals who spent time in prison together, being re-incarcerated on the same day is a strong signal of criminal partnership. We devise a simple identification strategy that allows us to assess the probabilities of type-I and type-II errors when estimating partnerships using same-day re-incarceration.

The two main factors that influence the likelihood of partnerships are nationality and age. Inmates are more likely to partner up when young and when partners are of similar age. They also look for partners who share the same nationality, and thus the same language and culture.

Each additional year of age difference reduces the probability of partnership by 1.8 per cent relative to the mean. Instead, belonging to the same nationality raises such probability by 44 per cent. The fact that criminals are more likely to partner up with inmates who previously committed different types of crime suggests that prisons might sometimes allow inmates to learn new skills and move to a different crime.

The criminal type that sticks out in terms of partnerships is being a mafia member. They are 130 per cent more likely to co-offend, and not just with other mobsters, which justifies the monitoring and the increased care used by the Italian prison system when dealing with mafia convicts.

1.2 Data

For our study, we make use of two datasets from the Italian prison administration. The first one, which is the one we use to run our peer regressions, comes from an internal database maintained by the Italian Department of Prison Administration (DAP) on offenders under its supervision. The dataset covers all individuals released as a result of the July 2006 collective pardon law right after the law was passed. The full sample includes more than 25,000 individuals. The key feature of these data that makes it unique for the purpose of our study is that we observe the post-release behaviour of groups of substantial sizes that were released at the same time from different prisons.

This is ideal, as inmates have an extensive choice of partnerships, choices that are less constrained when compared to the typical gradual release of inmates. Since we are interested in understanding whether individuals re-offended alone or with previous co-inmates, we focus on the 4,135 male inmates who re-offended. For each individual, we have information on whether or not the former inmate re-offended between the date of release from prison and December 2007.

Moreover, the data contain information concerning a wide range of variables at the individual and facility level. The following information is reported for each individual: facility where the sentence was served, official length of the sentence, actual time served, kind of crime committed (i.e., most recent offense in an individual's criminal history before the pardon), age, sex, level of education, marital status, nationality, province of residence, and employment status before being sentenced to prison. As data on successive convictions are not available, we use subsequent criminal charges and imprisonment as our measure of recidivism.

Our main variable of interest is whether two individuals i and j have partnered to re-offend $P_{ij} \in \{0, 1\}$. We define inmates to have co-offended when they spent time in prison together *and* were rearrested and imprisoned on the same day. Table 1 and 2 describe our first and main dataset[3]. In this dataset, there is no information on the new crime the inmates committed, which would possibly lower the degree of misclassification of our partnership variable. Though we can gauge the degree of misclassification using an auxiliary dataset that has precisely that information.

The second dataset covers inmates released from two prisons located in the city of Milan, Bollate and Opera, between 2001 and 2009[4]. For these inmates, we observe the entire prison history, as well as information on the type of crime inmates, commit when they recidivate. The data encompass all observed crimes that an individual has perpetrated in his life as well as individual characteristics such as: age, nationality, prison of incarceration, dates of incarceration. The next section discusses in great detail how such information helps us to uncover the degree of misclassification.

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[3]Regression results are based on this dataset.

[4]We only use this dataset to uncover the degree of misclassification. The descriptive statistics of this dataset are available upon request.

Table 1.1: Summary statistics (for individuals who reoffended)

Variable	Mean	Std. Dev.
Co-offending rate[a]	12%	n/a
Partner in crime[b]	0.251%	n/a
Age	36.194	9.065
Difference in age[c]	9.991	7.825
Years of education	3.675	3.36
Difference in years of education[d]	3.34	3.136
Employed	0.086	n/a
Both employed[e]	0.011	n/a
Same nationality[f]	0.597	n/a
Sentence (in months)	39.215	30.208
Difference in sentence[g]	29.654	29.571
Residual sentence (months)[h]	13.982	10.351
Difference in residual sentence[i]	11.614	8.783
Prison time (months)	25.252	26.525
Difference in prison time[j]	24.228	27.682

$N = 224,328$

[a] *Likelihood that an inmate was rearrested with at least one former co-inmate.*
[b] *Likelihood that two given former co-inmates were rearrested together.*
[c] *Average age difference within each pair of former co-inmates.*
[d] *Average difference in years of education within each pair of former co-inmates.*
[e] *Likelihood that two given former co-inmates were both employed.*
[f] *Likelihood that two given former co-inmates have the same nationality.*
[g] *Average difference in sentence within each pair of former co-inmates.*
[h] *Time in prison not served due to the prison pardon.*
[i] *Average difference in residual sentence within each pair of former co-inmates.*
[j] *Average difference in prison within each pair of former co-inmates.*

From Table 1, we observe that about 12 per cent of our pardoned inmates re-offended on the same day as inmates they were in prison with[5]. In order to analyse the partnership variable, we construct inmate ij pairs for all

[5] To do so, we look at whether a given inmate who was released in the wake of the prison pardon was rearrested alone on a or with former co-inmates given date. Note that it implies that the pairs are only based on prisoners released from the same prison.

inmates who spent time in the same prison, recidivated, and could therefore have recidivated together. We end up with more than 220,000 ij-pairs. The summary statistics are shown in Tables 1 to 2.

We notice that if we take two recidivating inmates from the same prison, there is a probability of 0.251% that they reoffended in partnership. This number is small but on average recidivating inmates are exposed to about 43 recidivating co-inmates, which reduced the chances of matching with any given inmate.

Inmates are on average 36 years old, and there is an average 10-year age difference with the former co-inmates they were released with. Education levels are very low and below 3.6 years of education, and the average difference with respect to other inmates is 3.34. It appears that only a few of them are employed and the probability that two inmates are both employed is around 1%. We also observe that almost 60% of the prisoners in our sample have the same nationality, mainly the Italian one. Moreover, before being released, inmates accomplished on average 25 months in jail for an initial sentence of more than 39 months. This suggests that the prison pardon enabled them on average to spend 13.9 months less in custody. Differences in sentences and residual sentences are respectively 30 months and 12 months.

Table 2 exhibits the different types of crime that individuals committed. Please notice that individuals can be included in several categories. More than 18% committed a crime against the public administration, about 22% against the justice system, more than 2% against public order, about 2% against public safety, 7.6% against the public belief, about 2% against their family, about 30% against the person and 69% against property. In addition, 2% were involved in mafia crimes, more than 34% were arrested for drug-related crimes, almost

8% for traffic violation, about 10% for illegal detention of weapons and 4.9% for illegal immigration.

Table 1.2: Summary statistics (for individuals who reoffended)

Variable	Mean	Std. Dev.
Against public admin. (tit.2)	0.184	n/a
Against the justice system (tit.3)	0.219	n/a
Against public order (tit.5)	0.021	n/a
Against public safety (tit.6)	0.021	n/a
Against the public belief (tit.7)	0.076	n/a
Against family (tit. 11)	0.019	n/a
Against the person (tit. 12)	0.295	n/a
Against property (tit. 13)	0.695	n/a
Mafia crime	0.02	n/a
Drugs	0.343	n/a
Traffic violation	0.079	n/a
Illegal detention of weapons	0.103	n/a
Illegal immigration	0.049	n/a
Undefined crime	0.021	n/a
Other crime	0.095	n/a

$N = 224,328$

1.3 Empirical Strategy

1.3.1 Econometric model

The objective of this paper is to isolate the drivers of group formation of former inmates for re-offending purposes. In order to do so, we exploit the 2006 Italian prison pardon that gave rise to a random release of large numbers of inmates across several prisons in Italy. More specifically, we study inmates who benefited from the prison pardon and re-offended in order to investigate the determinants that make them more likely to re-offend in group with prisoners they were released with.

Conditional on recidivating, we model the criminals' decision to recidivate with or without previous inmates. For a given individual i, we regress directly his likelihood to re-offend with inmate j, who has spent time in prison with him, on his characteristics, the characteristics of his potential peer as well as potential interactions between own and peer characteristics. For continuous variables X such as age, sentence length, and residual sentence length we take the absolute difference between i and j. For categorical variables Z, like nationality, education, employment, or crime type, we define a variable that is equal to one when one when the two inmates share the same category.

We use a linear probability model,

$$P_{ij} = \alpha + \beta_X'(X_i + X_j) + \gamma_X'|X_i - X_j| + \beta_Z'(Z_i + Z_j) + \gamma_Z' Z_i Z_j + \epsilon_{ij}, . \quad (1.1)$$

where the error term may contain a series of fixed effects (including prison facility) and is clustered at the prison facility level (there are a total of 120 different facilities in our final sample). β_X and β_Z measure the direct effect on the likelihood of partnering up versus acting alone, while $-\gamma_X$ and γ_Z measure how homophily influences partnerships. When interacting crime types a positive γ_Z would mean that inmates specialise and look for inmates with similar skills, while a negative γ_Z would mean that inmates are more likely to partner up with inmates with different skills, potentially complementary skills. The partnership variable is subject to miss-classification, meaning that the true partnership between i and j, P^*_{ij}, is subject to error. The next Section discusses how such bias can be adjusted for.

1.3.2 Misclassification of Criminal Partnerships

In the data on pardoned inmates two individuals (i and j) who spent time in prison together (in the same facility F) and re-offended on the same day D are assumed to be partners (P) in crime:

$$P_{ij} = 1 \text{ if } D_i = D_j \text{ and } F_i = F_j. \tag{1.2}$$

The main empirical challenge we face is that our observed criminal partnership measure between individual i and j, $P_{ij} \in \{0, 1\}$ may be subject to misclassification. If P is the observed partnership and P^* the true one, we can distinguish two types of misclassification (disregarding the ij subscript):

$$\alpha_1 = \Pr(P = 1|P^* = 0), \tag{1.3}$$

is the probability of observing that two individuals re-offended in partnership whereas they were not together, and

$$\alpha_0 = \Pr(P = 0|P^* = 1), \tag{1.4}$$

the probability of observing that two individuals re-offended alone whereas they were partnering up.

Misclassification can come from different sources. On one hand, we might not observe all partnerships if some co-offenders are arrested on different dates or if some manage to avoid an arrest altogether. On the other hand, two individuals who spent time in prison together might end up being rearrested on the same date even if both were acting on their own.

Misclassification in a limited dependent setting is not as innocuous as classical measurement error in the dependent variable, as it attenuates all coefficients towards zero Meyer and Mittag (2013). Estimates are conservative and need be inflated by a factor that depends on α_1 and α_0.

Hausman et al. (1998) show that as long as misreporting is conditionally "random" and constant across individuals, which is what we assume, the marginal effects in the observed data are proportional to the true marginal effects

$$\frac{\partial \Pr(Y = 1|X)}{\partial X} = (1 - \alpha_0 - \alpha_1) f(X'\beta)\beta;$$

where $f(X'\beta)$ is the link function of a limited dependent variable model, which in the case of a linear probability model is simply equal to one.

In order to uncover the degree of adjustment $(1 - \alpha_0 - \alpha_1)^{-1}$ we use an auxiliary data set which contains additional information about recidivating offenders. In particular, it contains information on the new type of crime committed. This information allows us to get estimates of α_0 and α_1 under fairly mild conditions.

The definition of peer groups that we use when running regressions with our first dataset carries the following signal[6]:

$$\Pr(P^* = 1 | D_i = D_j, F_i = F_j) = \Pr(P^* = 1 | P = 1). \tag{1.5}$$

Since our second dataset includes crime type (C) when re-offending, we can use this information to quantify the measurement error caused by the definition of peer groups we use in our regressions. In order to do so, we make the following assumption:

Assumption 1.

$$\Pr(P^* = 0 | D_i = D_j, F_i = F_j, C_i \neq C_j) = 1,$$

meaning that two individuals who are arrested on the same day and spent time in prison together cannot be peers if they have not committed the same type of crime. This appears to be true by definition when considering criminal responsibility and the full set of crimes committed. For example, during a robbery one criminal might be the sole responsible for having used violence against a victim, though all criminals are going to be responsible for the robbery, even those who might have just been on the watch.

Assumption 2.

$$\Pr(D_i = D_j, C_i = C_j | F_i = F_j) = \Pr(P^* = 1 | F_i = F_j),$$

meaning that on average the observed probability of observing a peer based on same crime type and same day of arrest is equal to the true probability of observing a peer.

[6]Note that we only consider two individuals as being peers if they were released from the same prison.

Let us first focus on the likelihood that we classify two individuals as being peers whereas they are not re-offending together (α_1). Since we are always considering inmates who spent time in prison together we simplify the notation by omitting that $F_i = F_j$ From Assumption 1, it follows that[7]:

$$\Pr(P^* = 0|P = 1) = 1 - [\Pr(P^* = 1|D_i = D_j, C_i = C_j)] \Pr(C_i = C_j|D_i = D_j) \tag{1.6}$$

Using Assumption 2 and Equation (1.6), we obtain:

$$\Pr(P^* = 0|P = 1) = 1 - \Pr(C_i = C_j|D_i = D_j). \tag{1.7}$$

Using our second dataset, we estimate $\Pr(C_i = C_j|D_i = D_j)$ to be 0.44. Moreover, such estimate can be computed for various thresholds of $|D_i - D_j|$, allowing us to assess the importance of false positive in the auxiliary dataset: $\Pr(P^* = 1|D_i = D_j, C_i = C_j)$. Figure 1.1 shows the estimated likelihood of co-offending $\Pr(C_i = C_j| \ |D_i - D_j|)$ depending on the time distance between the arrest of two peers (conditional on having spent time in prison together). Identifying co-offenders when $|D_i - D_j|$ is larger then zero immediately lowers the likelihood that the criminals committed the same type of crime. This suggests that the day of rearrest of two inmates who spent time in the same prison is a strong signal for partnership. Plugging the estimated probability $\Pr(C_i = C_j|D_i = D_j)$ into Equation (1.7), we obtain $\Pr(P^* = 0|P = 1) = 0.66$.

To find the probability of observing that two individuals re-offended in partnership whereas they were not together, we use the Bayes formula and

[7]We express our main definition of partnership as a function of crime type $\Pr(P^* = 0|D_i = D_j) = \Pr(P^* = 0|D_i = D_j, C_i = C_j) \Pr(C_i = C_j|D_i = D_j) + \Pr(P^* = 0|D_i = D_j, C_i \neq C_j) \Pr(C_i \neq C_j|D_i = D_j)$

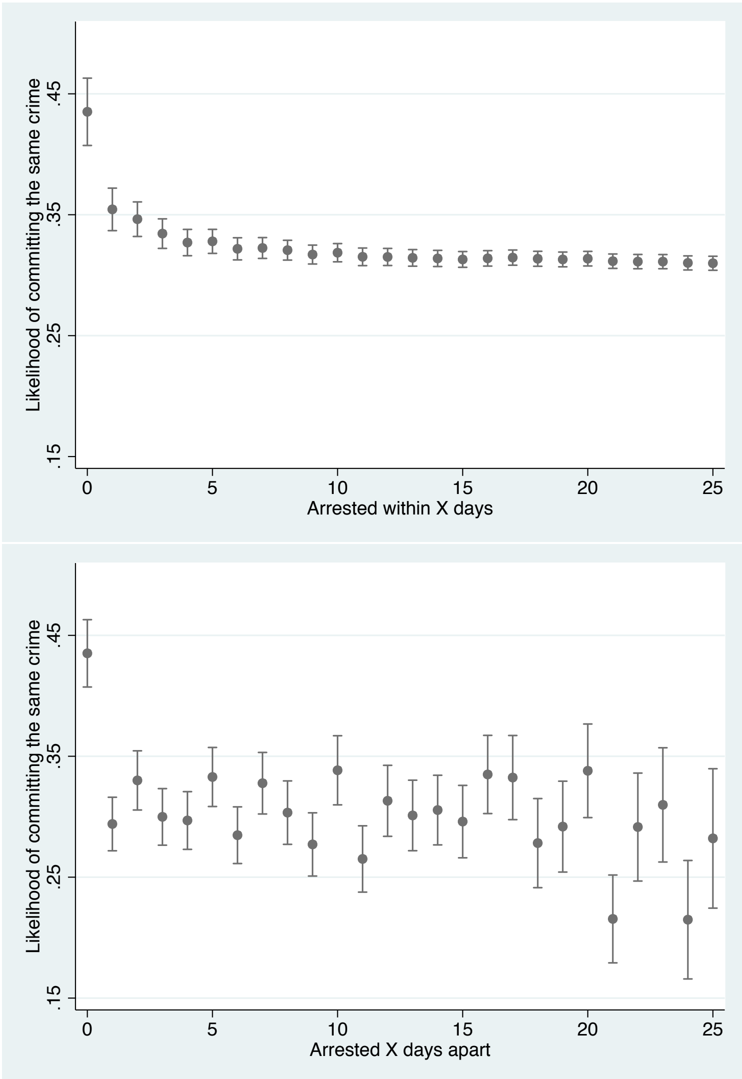

Figure 1.1: Probability of Committing the Same Crime Type

Notes: The first graph represents the probability that two inmates who spent time in prison together were rearrested for the same crime within X days. The second graph represents the probability that two inmates who spent time in prison together were rearrested for the same crime X days apart.

Assumption 2 and we obtain:

$$Pr(P=1|P^*=0) = \frac{Pr(P^*=0|P=1)Pr(P=1)}{1-Pr(D_i=D_j, C_i=C_j)} \tag{1.8}$$

Using our second dataset we estimate $Pr(P=1)=0.043$ and $Pr(D_i = D_j, C_i = C_j) = 0.013$. Plugging these numbers into Equation (8) and using $\Pr(P^*=0|P=1)=0.66$, we find $\alpha_1 = Pr(P=1|P^*=0)=0.03$.

The intuition here is that there are few inmates who commit the same crime in the same day that if they are not peers it is unlikely to observe such combinations. This confirms that observing criminals released from the same prison and then rearrested on the same day is a very strong signal for being partners in crime.

Next, we have to consider the likelihood that someone is not classified as a peer when in reality he or she is, α_0. In our definition of partnerships, individuals can only be peers if they were re-incarcerated on the same day and met in prison before. We note three cases where we could have defined individuals as not being peer whereas, in reality, they were. We enumerate these three cases below and try to find the corresponding probabilities:

1. $\Pr(D_i = D_j, F_i \neq F_j|P^*=1)$ and $\Pr(D_i \neq D_j, F_i \neq F_j|P^*=1)$

2. $\Pr(D_i \neq D_j, F_i = F_j|P^*=1)$

There are 120 different prison facilities in our data, or slightly more than one facility per Italian province. Since prison rules dictate to serve time in prison close to one's residence, inmates from different facilities have a chance of just 1 over 119, or less than 1 per cent, to have spent time in different prisons residing in the same province. We assume that inmates that live in different provinces have a chance that is indistinguishable from zero to be partners in crime. Moreover, since our focus is on partnerships that develop in prison, partnerships developed outside of prison would be of little use.

Next we discuss $\Pr(D_i \neq D_j, F_i = F_j | P^* = 1)$, the possibility that criminals are co-offending but end up in prison at different times. Again we can use the evidence from Figure 1.1. The right panel shows that there is a clear jump in the probability that two offenders are committing the same crime when moving from offenders arrested on the same day to offenders arrested one day apart. After that first jump, there is no evidence that the probability keeps on decreasing as we increase $|D_i - D_j|$, suggesting that a negligible fraction of peers are arrested on different days, driving $\Pr(P = 0|P^* = 1)$ to zero.

Summing up, in order to adjust our estimates we need to inflate them by $1/(1 - 0.03) \approx 1.03$. Considering the low magnitude of misclassification, we present regression results without inflating coefficients and consider them as lower bound estimates.

1.4 Results

We start by documenting how the likelihood of criminal partnership varies with respect to age, total sentence, residual sentence, and the corresponding difference in these variables between inmate pairs. The top left panel of Figure 1.2 shows an inverted U-shaped relationship between the probability of partnering up and age, with a peak that is close to the average age. The right panel shows that the inverted U-shaped relationship is likely to be driven by the fact that inmates of average age are more likely to find partners of crime of similar age, as the likelihood of partnership decreases monotonically when the age difference between potential partners increases. The evidence with respect to age implies a clear pattern of homophily.

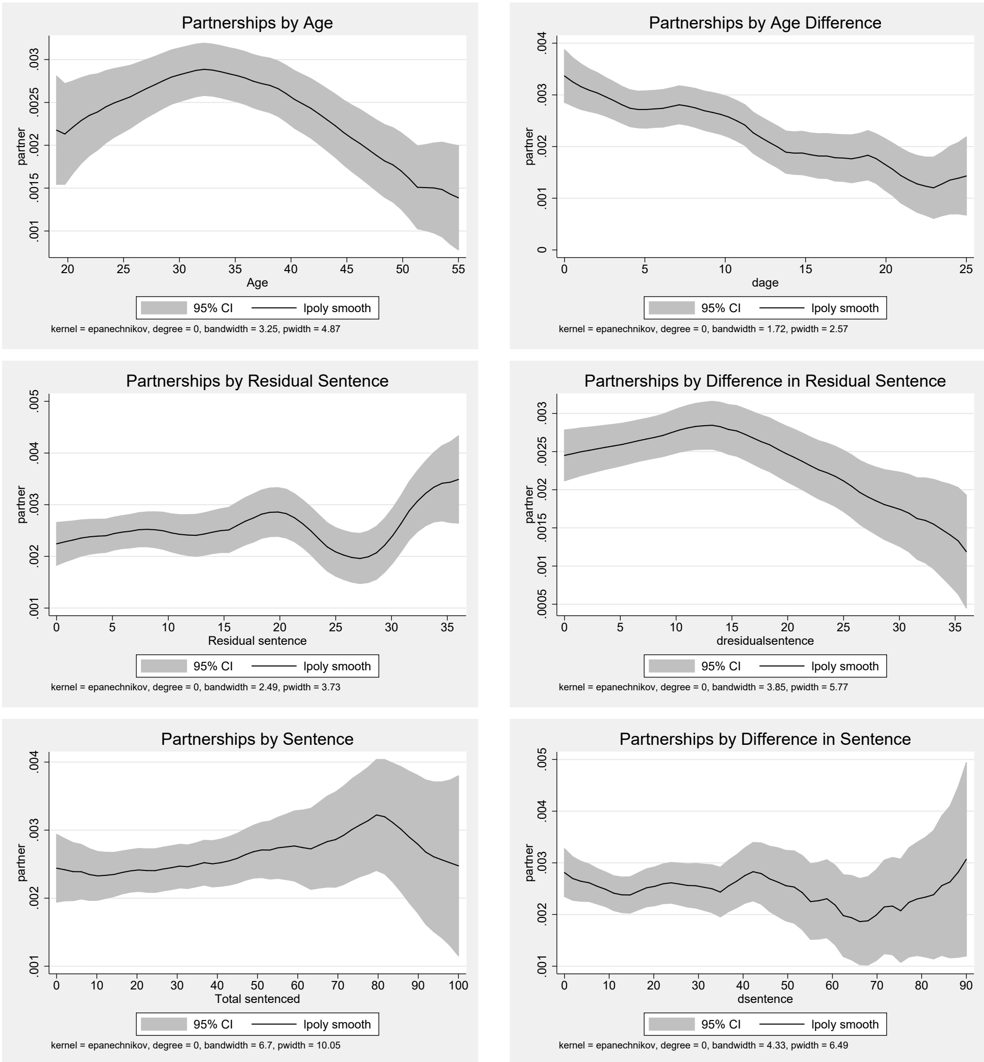

Figure 1.2: Probability of Partnerships against Continuous Variable

Notes: Local linear regressions and corresponding 95 percent confidence interval using the rule-of-thumb bandwidth. Except for the residual sentence we truncate the variables at the 95th percentile.

The patterns are less clear with respect to residual sentence, however, partnerships appear to be more likely when inmates face similar residual sentences. This is consistent with the findings of Drago and Galbiati (2012), where inmates are shown to respond to the deterrence effects faced by their peers. Finally, we observe no clear relationship between partnerships and sentence length, which is consistent with the lack of homophily with respect to crime types we document later. Table 1.3 presents the estimates of Equation 1. In order to control for the fact that the number of inmates influences the likelihood of partnership, each specification contains a full set of prison fixed effects. In column (1) we control for socioeconomic characteristics, and each subsequent column adds additional controls to the specification. Column (2) has information on sentences, column (3) adds information on crime types and whether criminal pairs committed the type of crime.

Table 1.3: Partnership Regressions

	(1)	(2)	(3)	(4)
	ij-Partners of crime ($\times$100)			
Age	-0.0022*	-0.0021**	-0.0018*	-0.0018
	(0.001)	(0.001)	(0.001)	(0.001)
Difference in age	-0.0045**	-0.0045**	-0.0045***	-0.0044**
	(0.002)	(0.002)	(0.002)	(0.002)
Education unknown	-0.0484	-0.0477	-0.0481	-0.0435
	(0.043)	(0.045)	(0.046)	(0.047)
Years of education	-0.0013	-0.0012	-0.0014	-0.0021
	(0.007)	(0.007)	(0.007)	(0.007)
Difference in years of education	-0.0005	-0.0006	-0.0006	-0.0002
	(0.005)	(0.005)	(0.005)	(0.005)
Employed	0.0470	0.0461	0.0434	0.0495
	(0.052)	(0.053)	(0.053)	(0.052)
Both employed	0.2441	0.2426	0.2377	0.2500
	(0.187)	(0.188)	(0.188)	(0.190)
Same nationality	0.1485***	0.1448***	0.1213**	0.1127**
	(0.046)	(0.046)	(0.050)	(0.051)

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	(1)	(2)	(3)	(4)
		ij-Partners of crime (×100)		
Sentence (in months)		-0.0002	0.0001	0.0002
		(0.000)	(0.001)	(0.001)
Difference in sentence		0.0000	-0.0001	-0.0002
		(0.001)	(0.001)	(0.001)
Residual sentence (months)		0.0026*	0.0028**	0.0024*
		(0.001)	(0.001)	(0.001)
Difference in residual sentence		-0.0036**	-0.0035**	-0.0030*
		(0.002)	(0.002)	(0.002)
Against public admin. (tit.2)			-0.0076	-0.0131
			(0.044)	(0.043)
Against the justice system (tit.3)			-0.0031	-0.0018
			(0.042)	(0.043)
Against public order (tit.5)			-0.1418***	-0.1428***
			(0.045)	(0.046)
Against public wellbeing (tit.6)			0.0269	0.0330
			(0.098)	(0.100)
Against the public belief (tit.7)			-0.0684	-0.0657
			(0.051)	(0.052)
Against family (tit. 11)			-0.0946*	-0.0875
			(0.055)	(0.053)
Against the person (tit. 12)			0.0175	0.0232
			(0.035)	(0.035)
Against property (tit. 13)			-0.0724	-0.0618
			(0.069)	(0.070)
Mafia crime			0.2822***	0.2831***

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	(1)	(2)	(3)	(4)
	ij-Partners of crime (×100)			
			(0.088)	(0.088)
Drugs			-0.0479	-0.0445
			(0.039)	(0.039)
Traffic violation			-0.0539	-0.0471
			(0.044)	(0.044)
Illegal detention of weapons			-0.0642**	-0.0610**
			(0.030)	(0.030)
Illegal immigration			0.0016	0.0138
			(0.056)	(0.055)
Undefined crime			0.0682	0.0635
			(0.096)	(0.099)
Other crime			-0.1104***	-0.1118***
			(0.035)	(0.033)
Both Against public admin. (tit.2)			-0.0910	-0.0755
			(0.106)	(0.106)
Both Against the justice system (tit.3)			-0.0173	-0.0353
			(0.069)	(0.068)
Both Against public order (tit.5)			-0.1267*	-0.1344*
			(0.074)	(0.076)
Both Against public wellbeing (tit.6)			-0.3166*	-0.3263*
			(0.185)	(0.190)
Both Against the public belief (tit.7)			0.4802	0.4911
			(0.309)	(0.314)
Both Against family (tit. 11)			-0.1802*	-0.1795
			(0.106)	(0.109)

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	(1)	(2)	(3)	(4)
		ij-Partners of crime (×100)		
Both Against the person (tit. 12)			-0.0091	-0.0307
			(0.056)	(0.055)
Both Against property (tit. 13)			0.0788	0.0714
			(0.072)	(0.071)
Both Mafia crime			0.5416	0.5526
			(1.456)	(1.477)
Both Drugs			0.1128	0.1098
			(0.078)	(0.079)
Both Traffic violation			-0.0275	-0.0326
			(0.075)	(0.077)
Both Illegal detention of weapons			-0.0001	-0.0032
			(0.134)	(0.136)
Both Illegal immigration			0.0461	0.0516
			(0.326)	(0.345)
Both Undefined crime			-0.5044***	-0.4816***
			(0.165)	(0.169)
Both Other crime			0.1041	0.0201
			(0.145)	(0.113)
Observations	224,328	224,328	224,328	219,172
R-squared	0.002	0.002	0.002	0.002
Nationality fixed effects	√	√	√	√
Prison fixed effects	√	√	√	√

Notes: All regressions include 120 prison fixed effects and nationality fixed effects. Clustered robust standard errors (by prison facility) in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

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	(1)	(2)	(3)	(4)
	ij-Partners of crime (×100)			

The sample used in column (4) excludes all the observations where i and j entered prison in the same month, and therefore might have been partners before even entering prison. We observe that our main results are robust when adding more controls. First of all, we notice that older inmates tend to partner up less with former inmates. Moreover, all the parametric regressions suggest that in line with the evidence shown in Figure 1.2 the age difference between two inmates plays a significant role in group formation. Inmates tend to sort by age and two inmates whose age is closer are more likely to partner up. When looking at regression (4), we observe that two inmates with an age difference of 10 years are 0.044 percentage points less likely to partner up. Relative to the baseline probability of partnership this corresponds to a 17.5 per cent reduction. This finding implies that when estimating criminal peer effects, researchers should take into account age differences when defining peer groups. Another strong predictor of partnerships is when inmates share the same nationality. Inmates with the same nationality are 0.11 percentage points more likely to partner up (or 44 per cent). These results support most of the literature on peer effects in prison that considers that peer groups should be defined with respect to nationality. The direct effects of the nationality dummies are shown in Figure 1.3, together with their 95% confidence intervals.

It does not appear that some nationalities are more likely to partner up compared to the Italian one (the excluded nationality). Countries for which we find a significant difference in terms of partnership decisions compared to Italians are countries whose citizens constitute a marginal proportion of our sample. So, Capo Verde is the only country for which citizens are significantly less likely to partner and Lithuania and Rwanda are the only two countries whose inmates are more

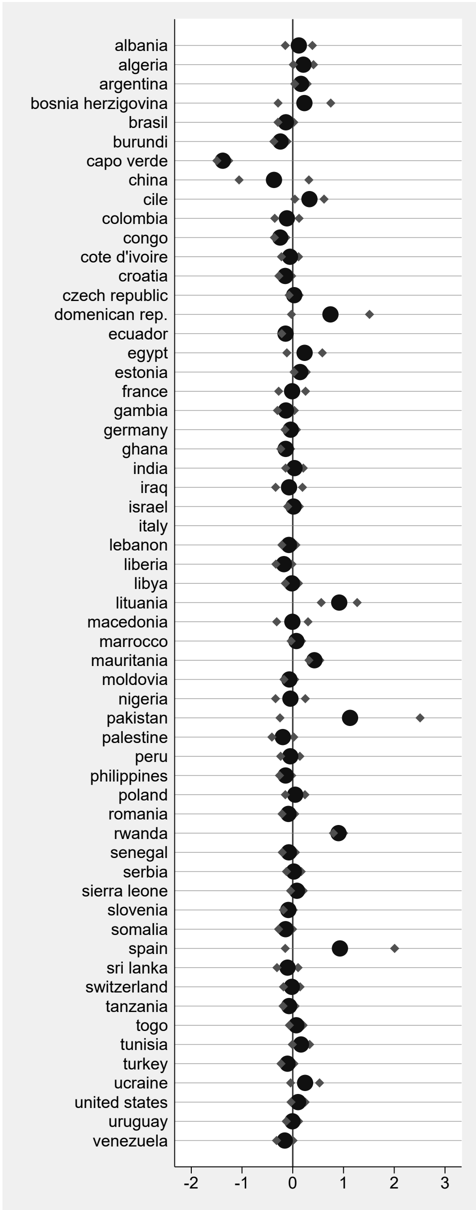


Figure 1.3: Differences Across Countries

Notes: The Figure shows the differences in the likelihood of partnering up compared to Italians, together with the 95% confidence interval. The estimates are taken from Column 4 of Table 1.3.

likely to partner up. We observe no impact of education or employment status on peer group formation. In line with the graphical evidence, we observe that inmates with higher residual sentences prefer to partner up, preferably with inmates with a similar residual sentence. The first result might be explained by the fact that a higher residual sentence increases the expected punishment. In response to the increased punishment inmates may partner up to reduce the likelihood of an arrest. The second result is in line with Drago and Galbiati (2012), who show that peers' average residual sentence influences an inmate's recidivism behaviour. Individuals with similar residual sentence will face similar incentives, which is likely to help the matching process. Inmates with a 10-month difference in terms of residual sentences are 0.3 percentage points less likely to associate (12 per cent). When it comes to crime types, we observe that mafia criminals are the most likely to partner up, while those who committed crimes against the family and against the public order are the least likely to partner up. We do not observe that inmates partner up with inmates of similar skills. If anything, there is evidence of complementarities among inmates whose crimes one does not associate with a criminal career (i.e. crimes against the family, the public order, and the public safety). As a result of spending time in prison, these inmates might look for more profitable crimes. Figure 1.4 shows the effect by crime type of interacting with a member of an organised crime group, together with the 95% confidence interval. Power is clearly an issue as only 2 per cent of the inmates have committed a mafia related crime. Yet we do find that inmates who were in prison because of crimes against the public administration (mainly corruption) are more likely to partner up when encountering a mafioso, which is in line with the many corruption scandals involving the mafia. We also find that inmates who were in prison for crimes against the public order, or traffic violations, somehow sporadic crimes, are less likely to partner up with mafiosi.

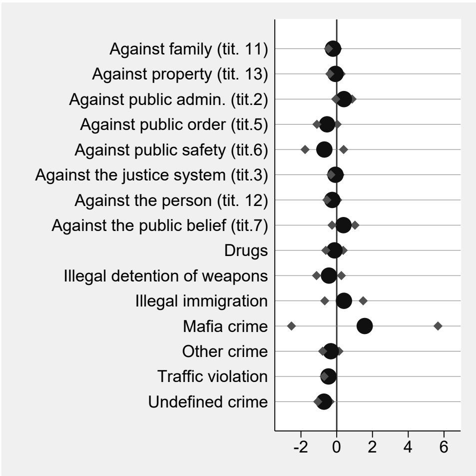


Figure 1.4: The Organised Crime Effect

Notes: The Figure shows the effect of interacting with a member of an organised crime group, together with the 95% confidence interval.

1.5 Conclusion

This paper emphasises the driving factors of partnership decisions of former inmates for re-offending purposes. We find evidence that nationality, age and residual sentence are key determinants of peer group formation. Mafia criminals are more likely to partner up, and more generally career criminals, are more likely to partner up. As for matching across crime types, there is some evidence of complementarities, and possibly learning across crime types. Mostly from what appear to be sporadic crimes by unprofessional criminals to career crimes.

This paper contributes to the growing literature on peer effects in prison in several ways. Firstly, we present a method to measure partnerships based on the date of re-incarceration of inmates who spent time in prison together. We also show how to deal with the misclassification probabilities when there is information on the new crime that has been committed. We show evidence that the likelihood that we classify two individuals as being peers whereas they are not re-offending together (α_1) is equal to 3% and that the likelihood that someone is not classified as a peer when in reality he or she is (α_0) is close to 0. We then show how to inflate regression coefficients to recover their true values.

Secondly, peer effects in prison can depend on information dissemination, skills transfer, and matching for re-offending purposes, as well as combinations of all these factors. We show that matching for re-offending purposes is an important driver of peer effects, and we show which dimensions drive the matching. Moreover, our findings pave the way to more accurate studies on peer effects in prison. As explained previously, authors rely on arbitrary definitions to define peer groups such as nationality or they use findings from other disciplines. However, defining peer groups correctly is crucial to avoid misleading conclusions. Since we model directly peer group formation in prison, our study suggests ways to improve current definitions of peer groups when studying peer effects in prison. For instance, most of the current literature defines peer groups based on nationality. As stated above, nationality is a driving factor of peer group formation, which supports the inclusion of nationality as a criterion to define peer groups in current studies. However, we also observe that age and incentives (in the form of residual sentence) are important factors to take into account. With respect to crime types we observe that career criminals are likely to influence inmates who committed crimes that are not associated with a criminal career, like crimes committed against the family (violence against children, incest, etc.), or crimes committed against the public order (disorderly conduct, public drunkenness, etc.).

Our results also have practical implications for policy purposes and can enable policymakers to design more accurate prevention policies based on the following information: the characteristics of the prisoners that were released with any of the other inmates who are still in custody. For instance, if two inmates are released on the same day from the same prison, authorities should be concerned if they have the same nationality or similar ages.

Chapter 2

Can predictive policing reduce domestic abuse? Evidence from Essex

2.1 Introduction

Domestic abuse is a major concern for police forces in the UK since many of them face over 100 related calls every hour (HMIC, 2015). In the UK, 488,000 domestic abuse offences were recorded by the police in the year ending in March 2017 while the Crime Survey estimated two million victims in England and Wales (ONS, 2017). Domestic abuse accounts for 10% of all recorded crime (Women's Aid, 2018) and associated costs are estimated to be £5.5bn a year in England including £1.6bn for physical and mental health costs and £1.2bn in criminal justice costs (Trust for London, 2017). However, researchers have estimated that the overall social cost of abuse is at least £15.7 bn per year (Walby, 2009).

In 2010, the government recommended to switch from a top-down policy response to a more localised response in order to better address this national issue.

They emphasised prevention, provision of support to victims, working in partnership with other agencies and risk reduction (Matczak et al., 2011). In March 2011, it confirmed the utmost importance of domestic abuse in the policy agenda and to support the new policy orientation it introduced a new £28 million action plan, "Call to End Violence against Women and Girls: Action Plan".

One of the main issues in designing policies to reduce domestic abuse is that, to deal with offenders, the police need victims to report offences. According to the Office of National Statistics (ONS), only 62% of the victims in the UK report abuse to anyone (ONS, 2013) and only 21% of abuse is reported to the police (ONS, 2016). In addition to enabling the police to take actions, reporting is essential as underreporting or late reporting[1] can have long-term consequences, which can be even more detrimental to victims. Underreporting also gives rise to substantial economic costs and the Early Intervention Foundation (EIF) estimated in 2016 that the cost of late intervention in England and Wales was £5.23 billion per year. So, in order ro be efficient, policies that target offenders should also be combined with measures that support victims and encourage them to report.

In Essex, the policy targets both offenders and their victims. To better identify high-risk offenders, data intelligence units work hand in hand with policymakers and use risk assessment to trigger policy implementation. Since 2014, the main assessment tool on which the Essex Police have been relying on is the Strathclyde model. This model is based on a risk score that measures how dangerous each suspect[2]is and it is updated every week. In this model, the higher the risk score, the more dangerous a suspect[3]. When the risk score is above a certain threshold, police forces inform suspects who they will be put under higher surveillance, which increases

[1]As opposed to early reporting, late reporting corresponds to a situation where victims do not report small incidents of domestic abuse and they end up reporting more serious incidents later on as their situation escalated.

[2]Suspects are all the individuals that were reported for domestic abuse at least once during the time span of the data.

[3]The risk score lies between 0 and 100. See Section 2.4 on the Strathclyde model for more information on how to construct the risk score.

their expected probability that the police will respond upon being reported by their victims. Moreover, the police contact their victims to encourage them to report and aim to increase victims' trust that the police will respond to their reporting.

One of the biggest obstacles to analyse policies to reduce domestic abuse is the lack of good data. Using a Regression Discontinuity Design (RDD) set up, I exploit unusually rich data from the Essex Police and show that suspects just past the threshold are 9% more likely to be reported again for domestic abuse within one month compared to those that stayed just below. It is challenging to understand whether an observed increase in domestic abuse incidents is the result of higher rates of reporting by victims or higher rates of the underlying crime. My model and empirical approach distinguish between these two channels, and I suggest that the deterrence of the offender caused by the increasing probability of punishment is outweighed by an increase in reporting from the victims[4].

This paper also investigates the heterogeneous impact of the policy across types of observed recidivism and shows evidence that results are driven by an increase in the number of calls related to domestic abuse for which the police could not establish criminal charges. A potential explanation for this is that the policy pushed victims to report low-type events of domestic abuse that they would not have reported otherwise. Although this effect of increasing reporting may appear positive, this paper highlights that policies encouraging the reporting of events of domestic abuse that do not give rise to a legal response can have a perverse effect and lead to escalation.

The rest of the chapter is organised as follows: section 2.2 summarises the literature, section 2.3 presents the policy context in Essex, section 2.4 explains the Strathclyde model, section 2.5 shows the data, section 2.6 describes the behavioural response to the policy, section 2.7 presents the empirical strategy, section 2.8 shows

[4]Note that, since suspects are not aware of the fact they have risk scores, they cannot modify their behaviours accordingly and manipulate the risk score.

the empirical results, section 2.9 investigates heterogeneity, section 2.10 underlines potential perverse effects of the policy, section 2.11 presents robustness checks and section 2.12 concludes.

2.2 Literature review

There is a growing economics literature that investigates predictors of domestic abuse. A wide range of papers studies how intra-household allocations can affect domestic abuse. Aizer (2010) studies the role of the gender wage gap on domestic violence in the US and emphasises that reductions in the gender wage gap explain 9 per cent of the decline in domestic abuse witnessed between 1990 and 2003. Bobonis et al. (2013) find that transfer programmes in Mexico in which funds are targeted to women decrease the incidence of spousal abuse. So, beneficiary women are 40 per cent less likely to be victims of physical abuse, but are more likely to receive violent threats with no associated abuse. Using a randomised experiment in Ecuador, Melissa et al. (2016) investigate whether cash, vouchers, and food transfers targeted to women and intended to reduce poverty and food insecurity also affected intimate partner violence. Results indicate that transfers reduce controlling behaviours and physical and/or sexual violence by 6 to 7 percentage points.

Other papers anchor on different literatures and investigate the impact of various factors on domestic abuse such as sport events or political representation. For instance, Card and Gordo (2011) look at the impact of football results in the American National Football League (NFL) on domestic abuse. They find that losses in games that the home team was predicted to win by more than 3 points lead to an 8 per cent increase in police reports of at-home male-on-female intimate partner abuse. Iyer et al. (2012) find that an increase in female representation in local governments in India induces a large and significant rise in crimes against women. They suggest that this increase is good news, driven primarily by greater reporting rather than greater incidence of such crimes. These papers underline predictors of domestic abuse but results are hard to exploit for policy purposes and they give little insights on how to design feasible interventions to reduce domestic abuse.

Research is scant on the impact of policies that are specifically designed to reduce domestic abuse or increase reporting. In one of the rare papers on the topic, Iyengar (2010) analyses the impact of mandatory arrest laws in the United States (US) on subsequent domestic abuse. She exploits the fact that mandatory arrest laws do not exist in all US states and that the timing in the implementation of the existing laws differed across states. Mandatory arrest laws require the police to arrest abusers when a domestic abuse incident is reported. Using the FBI Supplementary Homicide Reports (SHR), she finds that mandatory arrest laws actually increased intimate partner homicides. This paper intends to contribute to this literature by studying the impact of a policy to reduce domestic abuse in Essex.

As discussed, one of the issues in designing policies to reduce domestic abuse is that incidents go unreported. According to the ONS (2013), only 62% of victims report to someone. Among victims that report, almost 50% told friends, relatives or a neighbour, 12% called the police, 10% a counsellor or therapist and 8% a health professional. Another study from the ONS (2016) finds that only 20% of abuse is reported to the police (ONS, 2016). Many factors can affect the reporting of domestic abuse such as the financial situation of the victims or their dependence on their offender, having children, or having a temporary immigration status (APPG, 2015). Unfortunately, there is very little room for policymakers to impact these factors and induce victims to report in order to protect them.

Another important factor that leads to underreporting is the fact that some victims do not trust the police. According to data from the ONS, in 2011-12 one of the main reasons for not reporting abuse was that victims perceived the police would not (or could not) do anything about it. In line with this, a survey for the 2014 HMIC Inspection finds that 30% of the victims do not report because of lack of trust or confidence in the police to take actions in the wake of their reporting. By contacting victims of known suspects and informing them that the police would take actions to carefully investigate any future reporting from them, the Essex Police aim to increase trust in the police. This is a potential cost-efficient channel to increase reporting and, by quantifying the impact of this policy, this paper aims to pave the way for more domestic abuse policies that trigger these channels.

2.3 Policy context in Essex

2.3.1 Key facts in Essex

An interesting feature of Essex for policy analysis is that this county of south-east England has socio-economic characteristics that are close to the average for the country. To understand how Essex performs within the UK, I use deprivation deciles to see where Lower Layer Super Output Area (LSOA)[5] in Essex belong. By definition, the lower the decile, the more deprived the area. Table 2.1 presents the average deprivation deciles that LSOAs from Essex belong to for 6 indices of deprivation.

Table 2.1: Deprivation indices in Essex

Deprivation Category	Deprivation Index
Crime	5.85
Income	6.03
Employment	6.12
Education	4.90
Health	6.88
Housing	5.25

From Table 2.1, I observe that the average crime decile in which LSOAs in Essex belong to is close to the national average i.e. 5.85. Average Income, Employ-

[5]LSOAs are geographical areas with a population of around 1500. Figures are obtained from the English Indices of Deprivation 2015.

ment and Health deprivation indices lie into the 6th decile, which means that Essex performs slightly better than the national average in these areas. The housing deprivation index lies into the 5th decile. To better understand crime trends in Essex compared to national trends, I look at how crime rates per 1000 inhabitants within 12 months vary quarterly in the UK compared to figures in Essex[6]. Between January

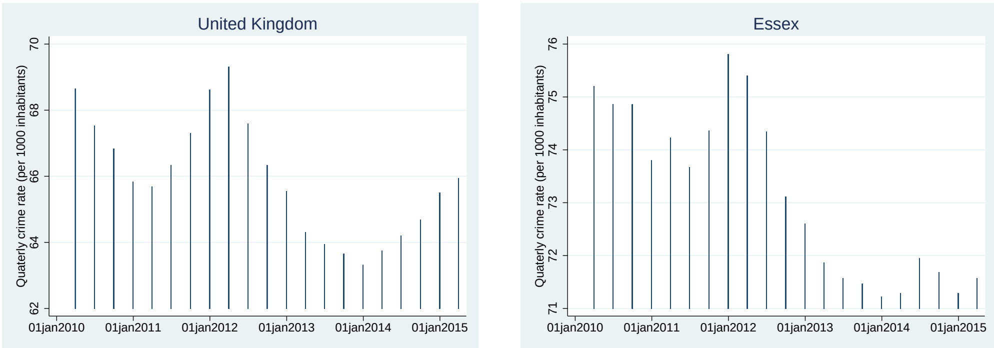


Figure 2.1: Quarterly crime rates per 1000 inhabitants (based on a rolling 12-month window).

1st 2010 and June 1st 2015, we observe that crime rates per 1000 inhabitants in the UK slightly decreased from 68 to 66 reported crimes per 1000 inhabitants. Over the same period, crime rates in Essex decreased from 75 to 71 per 1000 inhabitants but remained slightly above the national average[7].

2.3.2 Policy tools in Essex

A main obstacle that may have prevented economists from carrying out studies on the efficacy of policies to reduce domestic abuse is the quality of available data. For instance, in Essex, the main assessment method that was implemented before the Strathclyde model is the Domestic Abuse, Stalking and Harassment and Honour-

[6]Figures are computed using crime counts from the Crime Survey for England and Wales (CSEW) and population census levels in 2011.

[7]Although aggregate crime rates are informative, they may not represent trends in domestic abuse. Unfortunately, aggregate data that would enable us to consistently compare trends in domestic abuse in Essex to trends at the country level are not available.

based abuse risk identification (DASH) risk tool. This model was launched in 2009 and is still the main model that is used in the UK[8]. In this model, victims are classified in three distinct risk levels (standard, medium, or high risk) by frontline officers depending on their answers to a 27-question questionnaire. A main issue of the DASH model for policy evaluation purposes is that officers must use their professional judgement to classify victims in these three distinct risk levels (Robinson et al., 2016). Moreover, once frontline officers have classified victims, police forces decide at the local level which risk level they want to respond to and there is no data on how each police force proceeds. For instance, some police forces trigger a policy response for all victims classified as medium risk or high risk while others just focus on high-risk victims. Finally, re-assessment depends on the will of the officers and on their subjective perception of the situation of a victim. As a consequence, the DASH is not updated consistently and researchers can wrongly rely on an initial assessment that may differ over time.

Unlike the DASH risk tool, the Strathclyde model relies on an algorithm that is applied to administrative data on the domestic abuse history of each suspect[9] in the past 12 months. This model was created to help the police to better identify high-risk suspects of domestic abuse. More specifically, it enables the police to attach a dynamic risk score to each suspect that indicates how dangerous they are[10]. Since it is computed by officers from the central office in Chelmsford, it also ensures that the procedure is uniform across suspects as well as over time. Moreover, since policy responses are driven by a common threshold of the risk score across police units, it guarantees that the policy response is applied consistently for a given level of risk. Finally, unlike the DASH model, the risk score is systematically updated every

[8]In 2009, National Policing leads endorsed a risk model to support and improve the police response to cases of domestic abuse: the Domestic Abuse, Stalking and Harassment and Honour-based abuse risk identification, assessment and management model (DASH).

[9]Recall that suspects are individuals that were reported at least once during the time span of the data i.e. from 2013 to 2015. The police calculated the algorithm for each suspect only from 2014, which corresponds to the start of the policy.

[10]See Section 4 for more information on how the risk score is computed.

week, which enables the police to reassess, weekly, how dangerous offenders are and allocate resources accordingly. These are the main reasons for why the Essex Police opted for the Strathclyde model in early 2014. From a policy evaluation point of view, all these features of the Strathclyde model are essential as they enable one to examine the causal impact of the policy in Essex on observed recidivism. The following section describes in details how the risk score is calculated in practice by the police.

2.4 The Strathclyde model

As discussed, in order to identify the most dangerous suspects of domestic abuse and trigger policy response, the Essex police rely on the Strathclyde model. This model was constructed for risk assessment purposes and intends to help the police to better target high-risk individuals. The cornerstone of this model is an index i.e. a risk score designed to evaluate how dangerous a suspect of domestic abuse is. To compute it, the police apply an algorithm to administrative data on the domestic abuse history of each suspect. When the risk score is above a certain threshold, they implement policies to deter recidivism and increase reporting[11]. This index is updated every week and the police can reassess over time how dangerous suspects are.

2.4.1 The risk score

To compute the risk score on a particular date, the police use administrative data at the suspect level for the previous 12 months. The risk score relies on three main components that define how dangerous a suspect is according to the Strathclyde model: how many times an individual has been reported in the past 12 months,

[11] Suspects that pass the threshold and their victims are targeted by the policy until the suspects go back below the threshold.

when these events occurred in the past 12 months and the types of events in the past 12 months. According to the model, these components characterise respectively the frequency, the recency and the gravity of the events of domestic abuse that a suspect was involved in. The police then apply an algorithm to these components in order to attach subscores to each of them that capture their intensity[12]. The risk score is based on a weighted average of these three subscores: the Frequency score, the Recency score and the Gravity score (RFG). Each subscore and, subsequently, the risk score is computed over the past 12 months and, updated every Monday. So each week, offenders get a risk score that may change compared to the previous week. The score lies between 0 and 100. When it goes above 67.5, policy responses are implemented. The risk score is calculated as follows:

$$RiskScore_{it} = (FrequencyScore_{it} + RecencyScore_{it} + GravityScore_{it})/4 \quad (2.1)$$

with $FrequencyScore_{it}$ the frequency score of individual i at date t, $Recency\ Score_{it}$ the recency score of individual i at date t and $GravityScore_{it}$ the gravity score of individual i at date t. The Frequency score is computed over 200 while the other 2 subscores are both computed over 100. The risk score is the sum of these three scores divided by 4 and is computed over 100. Therefore, in the Strathclyde Model, the frequency score has more weight than the Recency score and the Gravity score.

2.4.2 The frequency score

To calculate the frequency score at date t, one needs to calculate first the number of incidents that were reported for each suspect in the preceding 12 months i.e. the frequency. The value is then compared to thresholds defined in a table to obtain the frequency score. Table 2.2 shows the correspondence between the number of incidents within the calendar date and 12 months before. For instance, if

[12] Subsection 2.4.2 to 2.4.4 describe in details how the police compute each subscore.

an individual was responsible for 5 incidents in the past 12 months, his/her frequency score is 150.

Table 2.2: Frequency score

Incidents (past 365 days)	Frequency Score
10+	200
9	150
8	150
7	150
6	150
5	150
4	100
3	75
2	50
1	1

2.4.3 The recency score

To calculate the recency score at date t, one needs to first to obtain the average recency i.e. the average number of days between date t and all the incidents that were reported for each individual in the preceding 12 months. For instance, let us assume that there were two incidents within 12 months before the calendar date. One incident was reported 20 days before t and the other one 60 days before. To calculate the average recency, one has to add up these two periods of time and divide by the number of incidents over the past 365 days i.e. the frequency, which is 2. The average recency score would then be (60+20)/2, which is equal to 40. Finally, one needs to look this number up in a table to get the recency score. From Table 2.3, one can observe that an average recency of 40 corresponds to a recency score of 50.

Table 2.3: Recency score

Average recency	Recency score
0 - 14 Days	100
15 -30 Days	75
31 - 60 Days	50
61 - 90 Days	30
91 - 120 Days	20
121 - 150 Days	10
151 - 180 Days	5

2.4.4 The gravity score

To calculate the gravity score at date t, one needs to get first the most serious offence that was reported for each suspect in the 12 months before t i.e. the gravity. Each type of offence corresponds to a gravity score that can be found in Table 2.4.

Table 2.4: Gravity score

Gravity 100			Gravity 75	Gravity 50
Attempted Murder	Committing an Offence with Intent to Commit a Sexual Offence	Sexual Activity with a Child Family Member	ABH Female	Breach of the Peace
GBH Female	Controlling a Child Prostitute ot a Child Involved in Pornography	Sexual Assault	ABH Male	Criminal Damage
GBH Male	Engaging in Sexual Activity in the Presence of a Child	Sexual Assault of a Child Under 13	Abduction Female	Drugs Possession
Malicious Wounding with Intent	Exposure	USI	Abduction Male	Prevent Breach of Peace
Manslaughter	Gross Indecency	Voyeurism	Affray	Civil Tresspass
Murder	Incest	Abandonment Female	Assault Constable in exec of duty	Attempted Criminal Damage
Possession of an Offensive Weapon	Inciting a Child Family Member to Engage in Sexual Activity	Abandonment Male	Assault by Beating (Battery)	Deception
Abuse of Position of Trust: Causing a Child to Watch a Sexual Act	Indecency towards child	Aggravated Burglary	Common Assault Female	Disqualified Driving
Abuse of Position of Trust: Sexual Activity with a Child	Indecent Assault Female	Breach of ASBO	Common Assault Male	Drink and Drive
Administering a Substance with Intent	Indecent Assault Male	Breach of Bail Conditions	Drunk in Charge of a minor	Drunk and Disorderly
Arranging or Facilitating the Commission of a Child Sex Offence	Indecent Exposure	Breach of Court Bail	Obstruct/Resist Constable in exec of duty	Going Equipped
Assault by Penetration	Indecent Photos of Children	Breach of Exclusion Order	Resisting Arrest	Offences Against Telecommunications Act
Assault of a Child Under 13 by Penetration	Indecent photographs of Children aged 16 or 17	Breach of Injunction	Robbery	Other Indictable/Triable Offences
Attempted Rape	Meeting a Child Following Sexual Grooming Etc.	Breach of Non Molestation Order	Arson	Sect 4 RTA
Bestiality	Paying for Sexual Services of a Child	Breach of Prison Licence	Blackmail	TWOC
Buggery Female	Pornography making /keeping	Breach of Restraining Order	Harassment	Theft
Buggery Male	Pornography on the Internet	Cruelty	Public Order Act Section 4	Theft of Motor Vehicle
Causing a Child to Watch a Sexual Act	Position of Trust Offences Sexual Activity/Intercourse	False Imprisonment	Burglary	Threats to Cause Criminal Damage
Causing a person to Engage in Sexual Activity Without Consent	Rape	Kidnapping	Malicious Communications	Unlawful removal of child from care
Causing or Inciting a Child Under 13 to Engage in Sexual Activity	Rape of a Child Under 13	Neglect	Outraging Public Decency	
Causing or Inciting a Child to Engage in Sexual Activity	Sex with an Adult Relative: Penetration	Recall to Prison	Perverting the course of Justice	
Child Sex Offences Committed by Children or Young Persons	Sexual Activity With A Child	Threats to Kill	Public Order Act Section 5	
Trafficking out of the UK for Sexual Exploitation	Violence to Secure Entry	Witness Intimidation	Threatening Violence to Secure Entry	
Threats to Cause GBH				

For instance, consider an individual who committed a murder and also harassed a victim in the past 12 months. From Table 2.4, we can observe that murder is considered as an offence of high gravity, which gives it a gravity of 100. We also observe that an harassment is considered as an offence of medium gravity with a gravity of 75. Since to compute the gravity score, what matters is only the most serious offence, the gravity score of the individual will be 100.

2.5 Data

The dataset was collected by the Essex Police and contains information on all 30,000 suspects of domestic abuse in Essex from 2013 to 2015. It includes characteristics of the suspects such as their age, gender, and where they live, as well as information on the crimes they were suspected of perpetrating. This dataset was collected for policy response purposes. It enables us to calculate a risk score for each individual based on the Strathclyde Model described in Section 4. Recall that the risk score is based on the following three subscores: the Frequency score, the Recency score, and the Gravity score (RFG).

Recidivism is the probability that a suspect was reported for domestic abuse within 1 month after the calendar date t[13]. Recidivism can be divided into two components i.e. Offence and Incident. Offence is the probability that a suspect is reported within 1 month after date t for domestic abuse events for which the police established criminal charges. Incident is the probability that a suspect is reported within 1 month after the calendar date t for domestic abuse events for which the police could not establish any criminal charges. Table 2.5 presents the descriptive statistics for data within bandwidth 20 of the threshold, which is the main bandwidth I use in my analysis[14].

[13] Although the risk score is updated every week, we look at recidivism within 1 month as it allows us to have enough inmates that reoffended over this time span. Results taking recidivism within 1 week are also significant but the data is more noisy.

[14] Table A.2 from Appendix A present the descriptive statistics for the whole data.

Table 2.5: Summary statistics (Bandwidth 20)

Variable	Mean	Std. Dev.	N
Recidivism	0.135	0.342	77608
Offence	0.057	0.232	77608
Incident	0.078	0.269	77608
Age	34.075	11.181	68310
Female	0.09	0.286	81393
White	0.774	0.418	81393
RecencyScore	22.141	29.421	81393
GravityScore	88.182	14.494	81393
FrequencyScore	125.226	43.546	81393
RiskScore	58.887	7.809	81393
RiskScoreAbove	0.156	0.362	81393

From Table 2.5, results show that 13.5% of the suspects are reported again for domestic abuse within one month. Among these individuals, 5.7% are reported for offences that led to criminal charges and 7.8% for incidents. Suspects were, on average, 34 years old, 9% female and 77% white. The average $RecencyScore$, $GravityScore$ and $FrequencyScore$ are respectively 22, 88 and 125. The average risk score is almost 59 and 15.6% of the suspects have a risk score above the threshold[15].

2.6 Behavioural response to the policy

2.6.1 General set up

Domestic abuse can be represented in a simple game theoretic model with two players i.e. an offender and a victim. To show the impact of the policy on players' decisions, I opt for a simple two-step sequential game that is repeated monthly. First, I assume that an offender decides whether or not he wants to abuse a domestic partner. Then, the victim must decide whether or not she wants to report it to the

[15] Notice that the risk score is highly discrete with a small number of mass points (72) that have a large number of observations each (on average 23,000). See Figure 2.3 from Section 7.2 for the distribution of the risk score

police. The situation can be represented as follows:

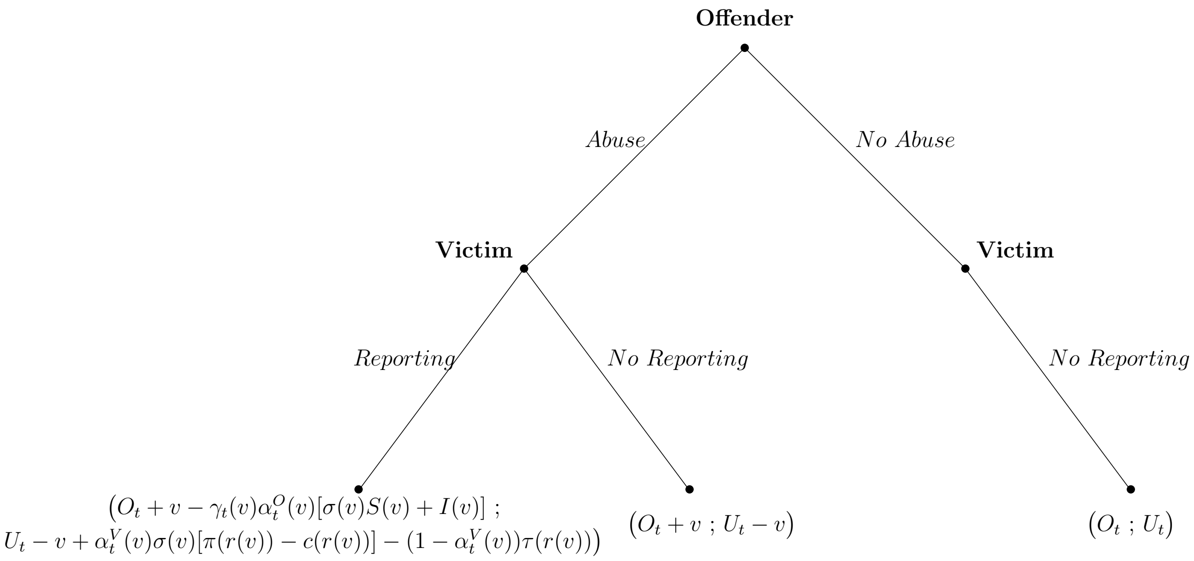


Figure 2.2: Game tree representation

Formally, this extensive game can be defined as follows: $\langle N, H; P, (\succeq_i)\rangle$ where: $N = \{Offender, Victim\}$; H consists of the 6 histories $\varnothing$, (A), (NA), (A, R), (A, NR), (NA, NR)[16]; $P(\varnothing) = Offender$ and $P(h) = Victim$ for every nonterminal history $h \neq \varnothing$. The following subsections define the payoffs of each player and how the policy can affect them and, subsequently, the outcome of the game.

2.6.2 Victims' reporting decisions

Assume that the victim gets utility from her relationship and that being exposed to domestic abuse negatively affects the quality of her relationship. Assume also that the victim faces the following tradeoff when deciding whether or not to report to the police. On the one hand, if reporting to the police triggers a legal response, sanctions may deter future domestic abuse and lead to an expected gain.

[16] A stands for *abuse*, NA for *no abuse*, R for *reporting* and NR for *no reporting*.

On the other hand, the punishment of the offender may negatively affect her relationship or financial situation, which gives rise to a potential cost[17]. Note that legal sanctions on the offender can only occur if the reporting of the victim triggers a police intervention in first place. However, reporting does not always give rise to a police response and, if a victim sees that her complaint is left unheard, she may feel even more lonely and psychological distress will give rise to an emotional cost. This cost can also be thought of as victim's perception of the potential reprisal from the offender if he learns that the victim reported him and the police did not respond. As a consequence, before deciding to report, victims may evaluate whether they believe that the police will trust them and that their complaint will trigger police actions[18]. The situation can be represented as follows:

$$U_t(Victim) = \begin{cases} U_t - v + \alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] \\ \qquad\qquad -(1-\alpha_t^V(v))\tau(r(v)) & \text{if } v > 0 \text{ and } r = 1 \\ U_t - v & \text{if } v > 0 \text{ and } r = 0 \\ U_t & \text{if } v = 0 \end{cases} \tag{2.2}$$

where $U_t(Victim)$ is the expected utility of the victim at time t. U_t is the utility of the relationship for the victim at time t. v is a real positive number that represents the cost of being exposed to domestic abuse[19]. $\alpha_t^V(v)$ represents the trust of the victim in the police to respond to her reporting of an event of domestic abuse v at time t. It is an expected probability that lies between 0 and 1. $\sigma(v)$ represents the expected probability that the reporting of an event of domestic violence v leads to legal sanctions. $r(v)$ is a binary variable equal to 1 if an individual reports an

[17]For instance, an offender may decide to leave the victim that reported him and she may lose her main source of income.

[18]Recall that according to data from the Office of National Statistics, in 2011-12 one of the main reasons for not reporting abuse was that victims perceived the police would not (or could not) do anything about it. In line with this, a survey for the 2014 HMIC Inspection finds that 30% of the victims do not report because of lack of trust or confidence in the police to take actions in the wake of their reporting.

[19]For now, I hypothesise that domestic violence is binary and the cost of domestic abuse is equal to v in case of violence and 0 otherwise.

event v and 0 otherwise[20]. $\pi(r(v))$ is a real positive number that corresponds to the expected benefit of the victim from a reporting of v that leads to legal sanctions, conditional on being abused. $c(r(v))$ is a real positive number that corresponds to the expected negative consequences of the punishment of the offender for the victim, conditional on being abused and reporting an event v. $\tau(r(v))$ represents the expected emotional/potential reprisal cost of reporting an event v of domestic abuse that does not lead to any response from the police. We assume that $\alpha_t^V(v)$, $\sigma(v)$, $c(r(v))$ and $\tau(r(v))$ are all monotonic and increasing functions in v.

Conditional on being exposed to domestic abuse, a victim will decide to report if there is a gain from it. In other words, a victim will report if and only if:

$$U_t - v + \alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha_t^V(v))\tau(r(v)) > U_t - v \tag{2.3}$$

$$\Leftrightarrow \alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha_t^V(v))\tau(r) > 0 \tag{2.4}$$

From equation (4), we can observe that an individual will only report if the expected net gain from reporting i.e. $\alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))]$, is greater than the expected emotional and/or reprisal cost from a complaint left unheard i.e. $(1 - \alpha_t^V(v))\tau(r(v))$. Let us define the probablity of reporting $\gamma_t(v)$, which is a function equal to 1 if condition (4) holds and 0 otherwise[21].

$$\gamma_t(v) = \mathbf{1}_{\{(\alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_t^V(v))\tau(r(v)) > 0)\}} \tag{2.5}$$

2.6.3 Offenders' decisions

Assume that an offender gets utility from his relationship with his victim and that domestic abuse increases his utility. For instance, domestic abuse can be thought of as an enforcement tool for the offender to induce the victim to make an

[20]For the sake of simplicity, I assume that individuals either do not report or fully report and that victims can only report if they were victimised in first place.

[21]I assume that individuals play in pure strategies i.e. they do not mix.

action to increase his utility. Let us also assume that the offender gets a disutility from the punishment he gets when his victims reports him. His expected utility can be defined as follows:

$$O(Offender)_t = \begin{cases} O_t + v - \gamma_t(v)\alpha_t^O(v)[\sigma(v)S(v) + I(v)] & \text{if } v > 0 \\ O_t & \text{if } v = 0 \end{cases} \tag{2.6}$$

Where $O(Offender)_t$ is the expected utility of the offender at time t. O_t is the utility of his relationship for the offender at time t. v represents the expected gain from exerting a level of violence v. $\gamma_t(v)$ is the expected probability that the offender is reported by his victim for a given event of domestic abuse v at date t, conditional on abusing her. $\alpha_t^O(v)$ is the expected probability of the offender at time t that the police will respond to the reporting of his victim, conditional on exerting a level v of domestic violence. $\sigma(v)$ represents the expected probability of getting legal sanctions after being reported for a level of domestic violence v. $S(v)$ corresponds to the expected legal sanctions for the offender if he exerts a level of domestic violence v, conditional on being reported. $I(v)$ represents the expected cost of the police intervention for the offender if he exerts a level of domestic abuse v and he is reported. $\alpha_t^O(v)$, $\sigma(v)$, $S(v)$ and $I(v)$ are all monotonic and increasing functions in v.

An offender will exert domestic abuse if the gain from it is greater that the expected punishment. In other words, he will perpetrate domestic abuse if and only if:

$$O_t + v - \gamma_t(v)\alpha_t^O(v)[\sigma(v)S(v) + I(v)] > O_t \tag{2.7}$$

$$\Leftrightarrow v - \gamma_t(v)\alpha_t^O(v)[\sigma(v)S(v) + I(v)] > 0 \tag{2.8}$$

In other words, an offender will only abuse if the gain v from it is greater than the expected punishment $\gamma_t(v)\alpha_t^O(v)[\sigma(v)S(v) + I(v)]$.

2.6.4 Potential outcomes of the game

Assume that the victim and the offender have complete information. In other words, both players know their potential payoff when playing as well as the potential payoff of the other player. To find the potential equilibria of this extensive game, I proceed by backward induction and investigate the conditions under which each outcome can occur. First, I look at the best response of the victim, conditional on the action of the offender. Then, I look at the best response of the offender, conditional on the strategies of the victim. I tally 3 different potential outcomes of the game: (A, R), (A, NR) and (NA, NR).

In order for (A, R) to be the outcome of the game, the victim must find it more profitable to report i.e. $\alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha_t^V(v))\tau(r(v)) > 0$, conditional on being abused. So, the best response for the victim (V) will be to report the abuse of the offender (O) i.e. $BR_V(A) = R$. In order for this outcome to be a Subgame Perfect Nash Equilibrium, the best response of the offender, knowing that he will be reported if he abuses his victim, must be to exert domestic abuse i.e. $BR_O(R) = A$. This is true if the gain v from exerting violence is greater than the expected punishment i.e. $v - \alpha_t^O(v)[\sigma(v)S(v) + I(v)] > 0$[22].

In order for (A, NR) to be the outcome of the game, the victim must find it more profitable not to report i.e. $\alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha_t^V(v))\tau(r(v)) \leqslant 0$, conditional on being abused. So, the best response for the victim (V) will be not to report the abuse (A) i.e. $BR_V(A) = NR$. Conditional on not being reported, the best response of the offender will be to abuse his victim since, by definition, abusing gives rise to a gain v such that $v > 0$. Under this condition, $BR_O(NR) = A$ and (A, NR) is a Subgame Perfect Nash Equilibrium.

In order for the outcome of the game to be (NA, NR), the victim must find it profitable to report the offender, conditional on being abused i.e. $\alpha_t^V(v)\sigma(v)$ $[\pi(r(v)) - c(r(v))]$ $-(1 - \alpha_t^V(v))\tau(r(v)) > 0$. Recall that domestic abuse gives rise

[22]Notice here that abusing is a dominant strategy for the offender

to a gain v and, if the victim does not report, the offender always will be better off offending. Knowing that he will be reported if he offends, to reach this equilibrium, the offender must be better off not offending than offending and being reported i.e. $v - \alpha_t^O(v)[\sigma(v)S(v) + I(v)] \leqslant 0$. Under these conditions, (NA, NR) is a Subgame Perfect Nash Equilibrium.

2.6.5 Impact of the policy

As discussed, the objective of the Essex Police is to increase the trust victims have that the police will respond to their reporting. I define $\alpha_{t1}^V(v)$ as the trust in the police under the policy that takes place at t_1 such that $\alpha_{t1}^V(v) \geqslant \alpha_{t0}^V(v)$. From equation (2.4), it can easily be shown that, conditional on $\pi(r(v)) - c(r(v))$ being greater than 0, an increase in trust in the police increases the likelihood of reporting[23]. In other words, if a victim has a net expected gain from reporting a given event of domestic abuse, then perceiving that the police are more likely to respond to her complaint will increase the likelihood that she reports. So, the policy is expected to increase reporting.

Since the policy informs suspects that they will put under higher surveillance, it increases their expected probability $\alpha_t^O(v)$ that the police respond to a reporting from their victims. I define the expected probability of the offender at t_1 that the police will respond to the reporting of his victim under the policy i.e $\alpha_{t1}^O(v)$ such that $\alpha_{t1}^O \geqslant \alpha_{t0}^O(v)$. From equation (2.8), it can be easily shown that an increase in $\alpha_t^O(v)$ has a deterrence effect on the offender[24]. Therefore, the policy is expected to reduce crime[25].

[23]If we take the first derivative of the expression from equation (2.4) with respect to $\alpha_t^V(v)$, we obtain the following expression:$d[\alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha_t^V(v))\tau(r(v))]/d\alpha_t^V(v) = \sigma(v)[\pi(r(v)) - c(r(v))] + \tau(r(v))$, which is always positive if $\pi(r(v)) - c(r(v)) > 0$.

[24]$d[v - \gamma_t(v)\alpha_t^O(v)[\sigma(v)S(v) + I(v)]]/d\alpha_t^O(v) = -\gamma_t(v)[\sigma(v)S(v) + I(v)]$, which is always negative, conditional on $\gamma_t(v) > 0$

[25]Recall that offenders are not aware that victims are contacted by the police and encouraged to report. They might however understand from their victims that the police contacted

Potential changes in the parameters of the utility functions of both players may affect their payoffs and, subsequently, their decisions. Let us assume that the extensive game presented in Section 6.1 is played once at t_0 and that the policy takes place right after t_0. The game is played a second time at t_1. In order to understand how the policy may affect each potential equilibrium, I hypothesise a given equilibrium at t_0 and investigate how changes in parameters as a result of the policy being introduced right after t_0 may affect the equilibrium at t_1. More specifically, I will show which equilibria at t_0 will potentially change at t_1 under the policy.

Assume that at t_0 the SPNE of the game was (A, R). As discussed, in order for this outcome to occur, the two following conditions must hold: $\alpha^V_{t0}(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t0}(v))\tau(r) > 0$ and $v - \alpha^O_{t0}(v)[\sigma(v)S(v) + I(v)] > 0$. The policy introduction at t_1 potentially increased trust in the police such that $\alpha^V_{t1}(v) \geqslant \alpha^V_{t0}(v)$. As a consequence, $\alpha^V_{t1}(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t1}(v))\tau(r(v)) \geqslant \alpha^V_{t0}(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t0}(v))\tau(r(v)) > 0$. Therefore, conditional on being abused, the victim will still report. Whether the initial equilibrium will stay in the wake of the policy will depend on the offender and, more specifically, on how he perceives the increase in the likelihood of the police to respond to a potential reporting from his victim. At t_1, the equilibrium will be $(A, R)_{t1}$ if $v - \alpha^O_{t0}(v)[\sigma(v)S(v) + I(v)] \geqslant v - \alpha^O_{t1}(v)[\sigma(v)S(v) + I(v)] > 0$. However, if $v - \alpha^O_{t1}(v)[\sigma(v)S(v) + I(v)] > 0 > v - \alpha^O_{p}(v)[\sigma(v)S(v) + I(v)]$, the offender will not find it profitable anymore to abuse his victim, conditional on the victim reporting him. As a consequence, he will stop offending and the new equilibrium will be $(NA, NR)_{t1}$.

Assume that at t_0 the SPNE of the game was (A, NR). Recall that for (A, NR) to be the outcome of the game, the victim must be better off not reporting i.e.

them but let us assume for now that they do not update their beliefs on the payoffs of the victims. As a consequence, we can assume that for the offender, $\gamma_t(v)$ is taken as constant between t_0 and t_1

$\alpha_t^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_t^V(v))\tau(r(v)) < 0$, conditional on being abused. In order for this SPNE at t_0 to stay at t_1 i.e. $(A, NR)_{t_1}$, the victim must not find it profitable to report at t_1 and $\alpha_{t1}^V(v)$ must be such that $0 > \alpha_{t1}^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_{t1}^V(v))\tau(r(v)) \geqslant \alpha_{t0}^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_{t0}^V)\tau(r(v))$. However, if $\alpha_{t1}^V(v)$ is such that $\alpha_{t1}^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_{t1}^V(v))\tau(r(v)) > 0 > \alpha_{t0}^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_{t0}^V(v))\tau(r(v))$, the victim will now be better off reporting, conditional on being abused and the outcome at t_1 will be $(A, R)_{t_1}$. The value of $\alpha_{t1}^O(v)$ does not affect the outcome since the offender still believes that the victim will not report him. As a consequence, he assumes that abusing is still a dominant strategy.

Assume now that at t_0 the SPNE of the game was (NA, NR). This suggests that the offender was better off not committing domestic abuse[26] at t_0. In other words, $\alpha_{t0}^O(v)$ was such that $v - \alpha_{t0}^O(v)[\sigma(v)S(v) + I(v)] < 0$ and $\alpha_{t0}^V(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1-\alpha_{t0}^V(v))\tau(r(v)) > 0$. Since the policy increases $\alpha_t^O(v)$ such that $\alpha_{t1}^O(v) \geqslant \alpha_{t0}^O(v)$, $v - \alpha_{t1}^O(v)[\sigma(v)S(v) + I(v)] \leqslant v - \alpha_{t0}^O(v)[\sigma(v)S(v) + I(v)] < 0$ and this equilibrium does not change in the wake of the policy.

To sum up, the two SPNE that may change under the policy are $(A, R)_{t0}$ and $(A, NR)_{t0}$, whereas $(NA, NR)_{t0}$ will not be affected by the policy. As discussed, the policy may deter recidivism and, the equilibrium $(A, R)_{t0}$ at t_0 can become $(NA, NR)_{t1}$ at t_1, which may have a negative effect on observed recidivism. The deterrence effect of the policy can also make the SPNE $(A, NR)_{t0}$ at t_0 become $(NA, NR)_{t1}$ at t_1. Although in this case the policy is effective in reducing crime, since victims were not reporting crime initially, this will not be captured by empirical results. Since the policy leads to increased in trust in the police, reporting may increase and the SPNE $(A, NR)_{t0}$ at t_0 can become $(A, R)_{t1}$ at t_1. This is expected to have a positive effect on observed recidivism. Section 2.7 will test empirically whether the reporting effect dominates the deterrence effect.

[26]Conditional on being reported. Otherwise, he is better offending.

2.6.6 Heterogenous impact of the policy on reporting

So far, the model assumed that domestic abuse was a binary outcome, which does not take into account the intensity of the events of domestic abuse. However, the impact of the policy may be heterogeneous across events of domestic abuse of different intensity. To understand the heterogeneous impact of the policy across different intensity levels, imagine that domestic violence has either high (v_h) or low intensity (v_l), with $v_h > v_l$. For instance, v_h may correspond to a rape and v_l to an attempt of light coercive control.

Victims may have varying levels of trust in the police to respond to their reporting depending on the type of crime they report. For instance, a victim that wants to report a very serious event such as a rape may be more certain that her complaint will trigger a police response. On the contrary, if she is subject to a relatively less serious event of domestic abuse such as light coercive control, she may expect that the police are very unlikely to respond to her complaint. Let $\alpha_t^V(v_h)$ be the trust of the victim in the police to respond to the reporting of a high-intensity event and $\alpha_t^V(v_l)$ be the the trust of the victim in the police to respond to the reporting of a low-intensity event, with $\alpha_t^V(v_h) > \alpha_t^V(v_l)$.

Offenders may expect initially that the probability that the police respond to the reporting of their victims for events of high intensity $\alpha_t^O(v_h)$ is higher compared to low-intensity events $\alpha_t^O(v_l)$ i.e $\alpha_t^O(v_h) > \alpha_t^O(v_l)$. For instance, conditional on being reported, an offender may expect that the police will put high efforts to investigate a potential rape and low efforts or no effort to investigate light coercive control.

As discussed, the potential decrease in observed recidivism in the wake of the policy will be driven by the switch from the SPNE $(A, R)_{t0}$ at t_0 to $(NA, NR)_{t1}$ at t_1 under the following condition: conditional on being reported, the offender must find it initially more profitable to offend and, then, be deterred by the policy i.e. $v - \alpha_{t0}^O(v)[\sigma(v)S(v) + I(v)] > 0 > v - \alpha_{t1}^O(v)[\sigma(v)S(v) + I(v)]$

In order for the policy to deter high-type observed recidivism, we must have the following condition:

$$v_h - \alpha_{t0}^O(v_h)[\sigma(v_h)S(v_h) + I(v_h)] > 0 > v_h - \alpha_{t1}^O(v_h)[\sigma(v)S(v_h) + I(v_h)] \tag{2.9}$$

When being reported for a rape, offenders may expect that the police will investigate with certainty i.e. $\alpha_t^O(v_h) = 1$ and equation (9) becomes:

$$v_h - [\sigma(v_h)S(v_h) + I(v_h)] > v_h - \alpha_{t1}^O(v_h)[\sigma(v_h)S(v_h) + I(v_h)] \tag{2.10}$$

$$\Leftrightarrow \alpha_{t1}^O(v_h) > 1 \tag{2.11}$$

However, by definition, $\alpha_{t1}^O(v_h) \leqslant 1$ and condition (2.11) cannot hold. As a consequence, the policy will not deter recidivism for high-type events for which offenders already perceive that the police with respond with certainty. In order for the policy to deter low-type observed recidivism, we must have the following condition:

$$v_l - \alpha_{t0}^O(v_l)[\sigma(v_l)S(v_l) + I(v_l)] > 0 > v_l - \alpha_{t1}^O(v_l)[\sigma(v_l)S(v_l) + I(v_l)] \tag{2.12}$$

For instances of coercive control, offenders may have a feeling of impunity and they may expect $\alpha_t^O(v_l) \simeq 0$. Therefore equation (2.12) becomes:

$$v_l > 0 > v_l - \alpha_{t1}^O(v_l)[\sigma(v_l)S(v_l) + I(v_l)] \tag{2.13}$$

So, even a small increase in the probability that the police response may be enough to deter low-type recidivism and ensure that condition (2.13) holds.
To sum up, through analysing these two extreme cases, we can infer that the policy will be a stronger deterrent for low-type events compared to high-type events for which offenders may already expect high police efforts, conditional on being reported.

As discussed in Section 6.5, the potential increase in observed recidivism will be driven by the switch from the SPNE $(A, NR)_{t0}$ at t_0 to $(A, R)_{t1}$ at t_1 in the wake of the policy under the following conditions: $\alpha^V_{t1}(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t1}(v))\tau(r(v)) > 0 > \alpha^V_{t0}(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t0}(v))\tau(r(v))$ and $v - \gamma(v)\alpha^O_{t1}(v)[\sigma(v_l)S(v) + I(v)] > 0$. In order for the policy to increase the reporting of high-type events, we must have the following condition:

$$\begin{aligned} \alpha^V_{t1}(v_h)\sigma(v_h)[\pi(r(v_h)) - c(r(v_h))] - (1 - \alpha^V_{t1}(v_h))\tau(r) > 0 > \alpha^V_{t0}(v_h)\sigma(v_h) \\ [\pi(r(v_h)) - c(r(v_h))] - (1 - \alpha^V_{t0}(v_h))\tau(r(v_h)) \end{aligned} \tag{2.14}$$

When reporting a high-type event such as a rape, victims may expect that the police will investigate their complaint with certainty i.e. $\alpha^V_t(v_h) = 1$. So, equation (2.14) becomes:

$$\alpha^V_{t1}(v_h)\sigma(v_h)[\pi(r(v_h))-c(r(v_h))]-(1-\alpha^V_{t1}(v_h))\tau(r) > 0 > \sigma(v_h)[\pi(r(v_h))-c(r(v_h))] \tag{2.15}$$

However, by definition, $\alpha^V_{t1}(v_h) \leqslant 1$. As a consequence, equation (2.15) cannot hold and the policy is expected to have no impact of the reporting of high-type events for which victims already believe with certainty that the police would put high efforts to investigate their complaint. In order for the policy to increase the reporting of low-type events, we must have the following condition:

$$\begin{aligned} \alpha^V_{t1}(v_l)\sigma(v_l)[\pi(r(v_l)) - c(r(v_l))] - (1 - \alpha^V_{t1}(v_l))\tau(r) > 0 > \alpha^V_{t0}(v_l)\sigma(v_l) \\ [\pi(r(v_l)) - c(r(v_l))] - (1 - \alpha^V_{t0}(v_l))\tau(r(v_l)) \end{aligned} \tag{2.16}$$

When reporting a low-type event such as a low-intensity coercive control, victims may expect that the police will not allocate much resources to investigate their complaint i.e. $\alpha^V_{t1}(v_l) \simeq 0$. So, equation (2.16) becomes:

$$\alpha^V_{t1}(v_l)\sigma(v_l)[\pi(r(v_l)) - c(r(v_l))] - (1 - \alpha^V_{t1}(v_l))\tau(r) > 0 > -\tau(r(v_l)) \tag{2.17}$$

So an increase in $\alpha^V_t(v_l)$ such that $\alpha^V_{t1}(v_l) > \alpha^V_{t0}(v_l)$ may increase reporting if condition (2.17) holds. These two extreme examples show that the policy is expected to have a greater impact on the reporting of low-type events for which victims did not believe initially that the police would put efforts to investigate their complaint and less impact on the reporting of high-type events.

To sum up, the policy is expected to give rise to a greater increase in the reporting of low-type events compared to high-type events. It should also deter domestic abuse more for low-type events than for high-type events. Through investigating the net effect of the policy on observed recidivism across different types of domestic abuse, this paper will first test empirically across types of recidivism whether the reporting effect dominates the deterrence effect.

2.6.7 Longer term impact and potential perverse effect of the policy

As discussed, the policy may decrease recidivism and, if the SPNE at t_0 was $(A, R)_{t0}$, it may become $(NA, NR)_{t1}$ at t_1 under some conditions. This change in outcome occurs if, conditional on being reported, the increase in the expected probability that the police respond to a reporting from his victim at t_1 i.e $\alpha^O_{t1}(v)$ deters the offender (unlike at t_0). Since the offender stops offending at t_1, he cannot learn from experience whether $\alpha^O_{t1+n}(v)$ changes after t_1 and he will assume that it is always equal to $\alpha^O_{t1}(v)$. Therefore, he will stop reoffending and at t_2, the outcome of the game will be $(NA, NR)_{t2}$. Likewise, if at t_0 the outcome of the game was $(A, NR)_{t0}$ and it becomes $(NA, NR)_{t1}$, the offender will stop offending and the outcome will stay identical at t_2 i.e $(NA, NR)_{t2}$. For both cases, the short-term effect of the policy at t_1 will be similar to the long-term effect.

On the contrary, if the SPNE at t_0 was $(A, NR)_{t0}$ and it became $(A, R)_{t1}$ at t_1, the outcome of the game in the following period will depend on the best strategy of the offender, knowing that he will now be reported[27]. Recall that in order for this switch from t_0 to t_1 to happen, the victim must not report at t_0

[27]Recall that, conditional on not being reported, abusing is always a dominant strategy

and start reporting in the wake of the policy at t_1. In other words, the following condition must hold: $\alpha^V_{t1}(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t1}(v))\tau(r(v)) > 0 > \alpha^V_{t0}(v)\sigma(v)[\pi(r(v)) - c(r(v))] - (1 - \alpha^V_{t0}(v))\tau(r(v))$. Recall that the offender is not aware that the police contacted his victim at t_1 to encourage her to report. So, he believes that for his victim $\alpha^V_{t1}(v) = \alpha^V_{t0}(v)$, hence he thinks that the victim will not report him and he bases his reasoning at t_1 on $\gamma_{t0}(v)$, which is equal to 0, and not on $\gamma_{t1}(v)$. However, $\gamma_{t1}(v)$ is equal to 1 and his victim will report him.

When he gets reported at t_1, whether or not he will reoffend at t_2 depends on how he updates his expected payoffs at t_2. The offender will abuse at t_2 only if he gets a positive net expected payoff, conditional on being reported. In other words, he will reoffend at t_2 if $v - \alpha^O_{t2}(v)[\sigma(v)S(v) + I(v)] > 0$ and the SPNE at t_2 will be $(A, R)_2$. However, if $v - \alpha^O_{t2}(v)[\sigma(v)S(v) + I(v)] < 0$, he will stop offending at t_2 and the SPNE at t_2 will be $(NA, NR)_2$. Empirical regressions will test how individuals that were reported in the wake of the policy at t_1 adjust their behaviour at t_2.

Whether the offender stops reoffending may also depend on the punishment he gets after being reported and on whether he updates his beliefs on the likelihood of getting legal sanctions. Let us now assume that the probability of getting legal sanctions can change over time i.e $\sigma(v)$ now depends on t as follows: $\sigma_t(v)$. Recall that $\sigma_t(v)S(v) + I(v)$ is his expected punishment and the actual punishment may make him update his beliefs on future punishments. If the police establish criminal charges at t_1 i.e $\sigma_{t1}(v) = 1$, the offender may believe that $\sigma_{t2}(v)$ will be higher. For instance, he may learn that a given type of abuse may trigger legal sanctions, which makes his expected payoff from abusing lower. Therefore, it is more likely that he gets an expected negative payoff and he stops abusing in the future.

Inducing victims to report their offenders is seen as essential to deter future recidivism. However, in some cases, reporting might give rise to an increase in the intensity of crime, which may jeopardise the efficacy of the policy. To understand this potential issue, let us now get back to the two heterogeneous cases developed in subsection 2.6.6 and let us assume that an individual can choose between exerting high-type abuse v_h or low-type abuse (v_l). Since, by definition, $v_h > v_l$, an individual will choose low-type domestic abuse only if he believes that his victim would report him for both types of violence or only for high-type abuse. If an individual believes he will be reported only if he exerts high-type domestic abuse, he will decide to exert low-type domestic abuse at t_0 if:

$$v_l > v_h - \alpha^O_{t0}(v_h)[\sigma(v_h)S(v_h) + I(v_h)] \tag{2.18}$$

Assume that the policy pushed a victim to report a low-type domestic abuse at t_1. If the reporting led to a police response at t_1, and the offender knows that from now on he will be reported, he might now be better off exerting high-type domestic abuse at t_2 if[28]:

$$v_l - \alpha^O_{t2}(v_l)[\sigma(v_l)S(v_l) + I(v_l)] < v_h - \alpha^O_{t2}(v_h)[\sigma(v_h)S(v_h) + I(v_h)] \tag{2.19}$$

Notice that, even in a case where the reporting would lead to legal sanctions only for high-type domestic abuse, the cost of the police intervention $I(v_l)$ might be enough to cause escalation to high-type abuse.

2.7 Empirical strategy

2.7.1 Econometric model

As discussed, the objective of this paper is to understand the effectiveness of the policy based on the Strathclyde model to reduce domestic abuse in Essex. The main challenge researchers face while analysing the impact of policies to reduce domestic abuse is that the data only contain events that were reported i.e. observed

[28]Conditional on $v_h - \alpha^O_{t2}(v_h)[\sigma(v_h)S(v_h) + I(v_h)] > 0$

recidivism. Recall that the effect of the policy on observed recidivism is a combination of the deterrence effect on the offenders and the reporting effect on the victims. Moreover, the effects of the policy on offenders and victims may lead to the observed recidivism going in opposite directions and the main challenge researchers face is to disentangle them. To do so, I look at observed recidivism from offenders across different types of recidivism and subgroups. More specifically, since the police define a threshold of the risk score above which they trigger policy responses, I compare recidivism for individuals just above the threshold with those that stayed just below. So, the risk score of the Strathclyde model provides a natural set up for a Regression Discontinuity Design (RDD). The main econometric model is as follows:

$$\begin{aligned} Y_{imt} = RiskScoreAbove_{it}\gamma + f(RiskScoreAdj_{it}) + \\ f(RiskScoreAdj_{it})RiskScoreAbove_{it}\sigma + W_i\beta + \epsilon_{it} \end{aligned} \tag{2.20}$$

with Y_{im} a measure of recidivism for individual i within m months after the calendar date t. $RiskScoreAbove_{it}$ is a dummy equal to 1 if the risk score was above the 67.5 threshold at date t and 0 otherwise. $RiskScoreAdj_{it}$ is the risk score of individual i minus the 67.5 threshold at date t; f(.) is a polynomial function of degree n; W_i is a vector of characteristics of individual i. The parameter of interest is γ, which measures the local causal effect of the police response on recidivism, around the cutoff.

2.7.2 Validity of the econometric design

In order for the Regression Discontinuity Design (RDD) to be valid, individuals must not be able to precisely manipulate the assignment variable i.e. the risk score (Lee, 2008). As discussed, offenders are not aware they are given a risk score, and therefore, there is no reason to believe that they could manipulate it. Figure 2.3 shows the distribution of the risk score. The vertical line on the histogram corresponds to the 67.5 threshold. As expected, since offenders are not aware that they are assigned a risk score, we do not observe any discontinuity in the density of the risk score around the threshold.

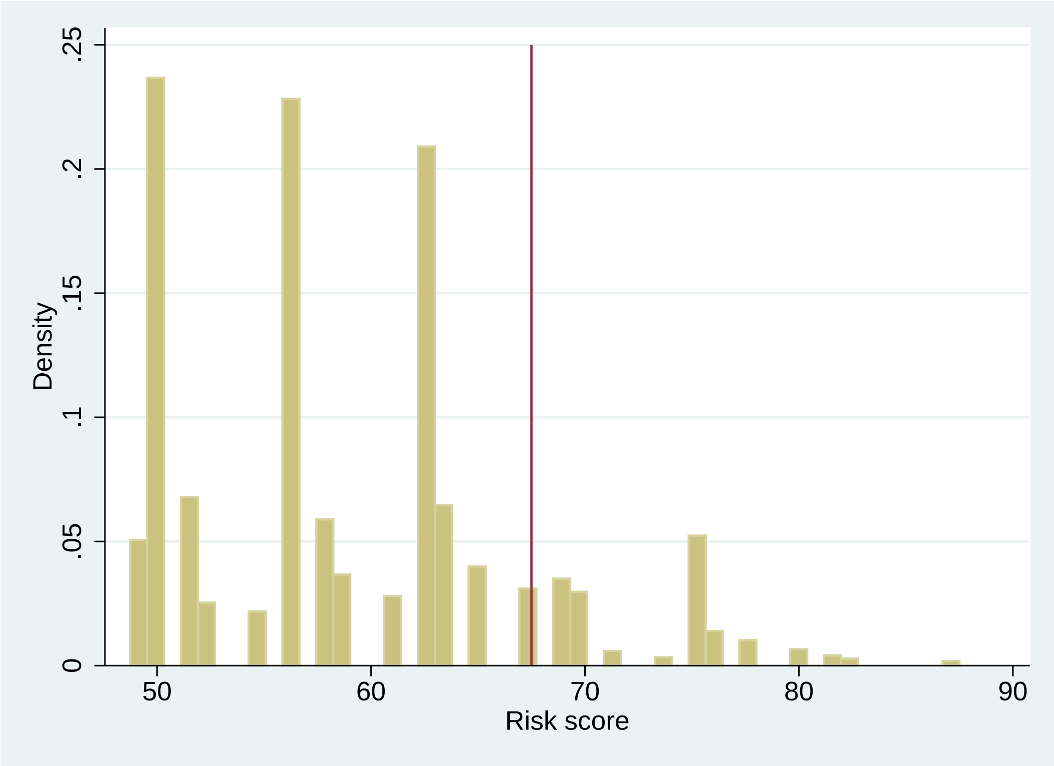


Figure 2.3: Distribution of the risk score (20 bandwidth)

If individuals cannot manipulate the running variable around the threshold, it can then be assumed that the variation in the treatment near the threshold is randomised as though from a randomised experiment. The key implication of the local randomisation result implies that, if the variation in the treatment near the threshold is approximately randomised, then it follows that all baseline characteristics - all those variables determined prior to the realisation of the assignment variable - should have the same distribution just above and just below the cutoff.

Table 2.6 presents the descriptive statistics of my three control variables on both sides of the threshold using a bandwidth of 20[29]. We observe that on both

[29]Main regressions are based on a bandwidth of 20 since it allows me to have enough

sides of the threshold, descriptive statistics of the control variables are of similar magnitude. So, the average age is slightly greater than 34 years old below the threshold compared to almost 34 years old above the threshold. 9% are female below the threshold versus 7% for individuals above the threshold. Finally, below the threshold 77% are white versus 82% on above the threshold.

Table 2.6: Descriptive statistics (By side of the threshold, bandwidth 20)

	Below the threshold				Above the threshold			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Age	34.12	11.24	14	88	33.77	10.81	16	67
Female	0.09	0.29	0	1	0.07	0.25	0	1
White	0.77	0.42	0	1	0.82	0.39	0	1
	N=58976				N=10665			

In order to show evidence that there is no discontinuity at the threshold in the baseline characteristics, I show nonparametric regressions between the risk score and each control variable. Figure 2.4 depicts the nonparametric relationship between the risk score and the baseline control variables. The graph in the top left corner corresponds to the link between the offender's gender and the risk score. I observe no clear pattern between the risk score and gender. Moreover, there is no clear discontinuity at the threshold. The graph in the top right corner represents the nonparametric relationship between the risk score and the ethnicity. Likewise, I do not observe a clear pattern, or a sharp discontinuity at the threshold. The graph in the bottom left corner shows the link between the risk score and the age and does not show any clear pattern, or a discontinuity at the threshold.

mass points while comparing suspects on both sides of the threshold that are as similar as possible. I show later that regressions are very robust to different bandwidths.

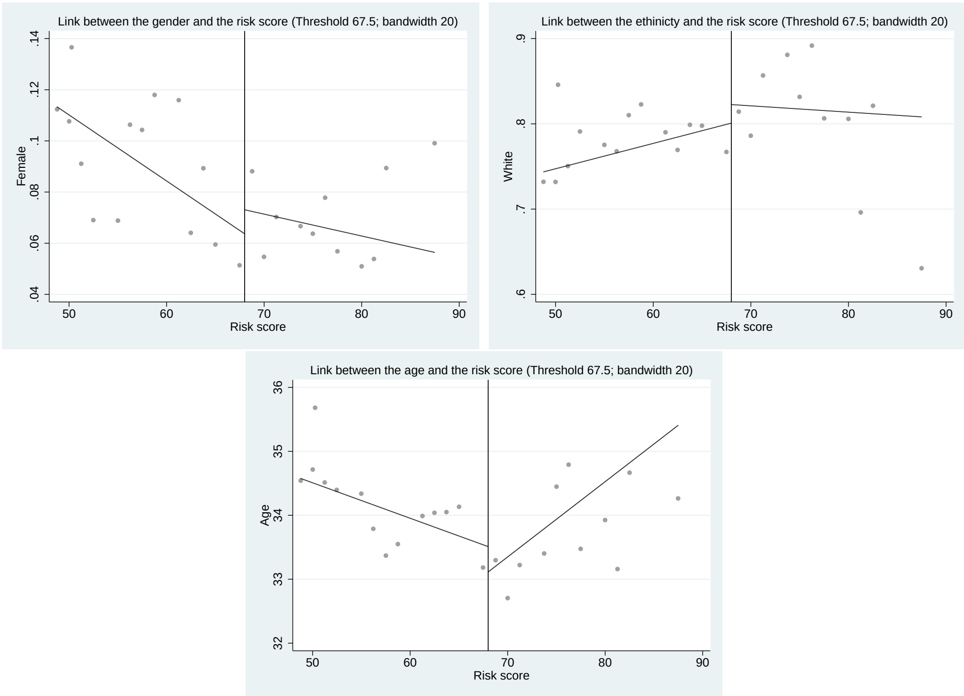


Figure 2.4: Link between the risk score and the baseline control variables (Bandwidth 20, polynomial of degree 1)

Although nonparametric graphs do not suggest clear discontinuities at the threshold between the risk score and control variables, I run parametric regressions to test the significance of these relationships. Table 2.7 shows Ordinary Least Square (OLS) regressions of the treatment on each control variable. Column (1), column (2) and column (3) depict the impact of the policy on the gender, the ethnicity and age respectively. None of the regressions are significant, which is consistent with the fact that graphs do not exhibit any clear discontinuity at the threshold on the graphs.

Table 2.7: Link between the risk score and the baseline control variables (OLS regressions, polynomial of degree 1)

	(1) Female	(2) White	(3) Age
Treatment	0.0127	0.0100	-0.541
	(0.0174)	(0.0131)	(0.362)
Bandwidth	[-20;20]	[-20;20]	[-20;20]
Observations	67,172	67,172	67,172
R-squared	0.002	0.002	0.002

Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

2.8 Empirical results

2.8.1 Impact of the policy on observed recidivism

Figure 2.5 represents a nonparametric graph of the relationship between the risk score of the suspects and their reported recidivism the following month.

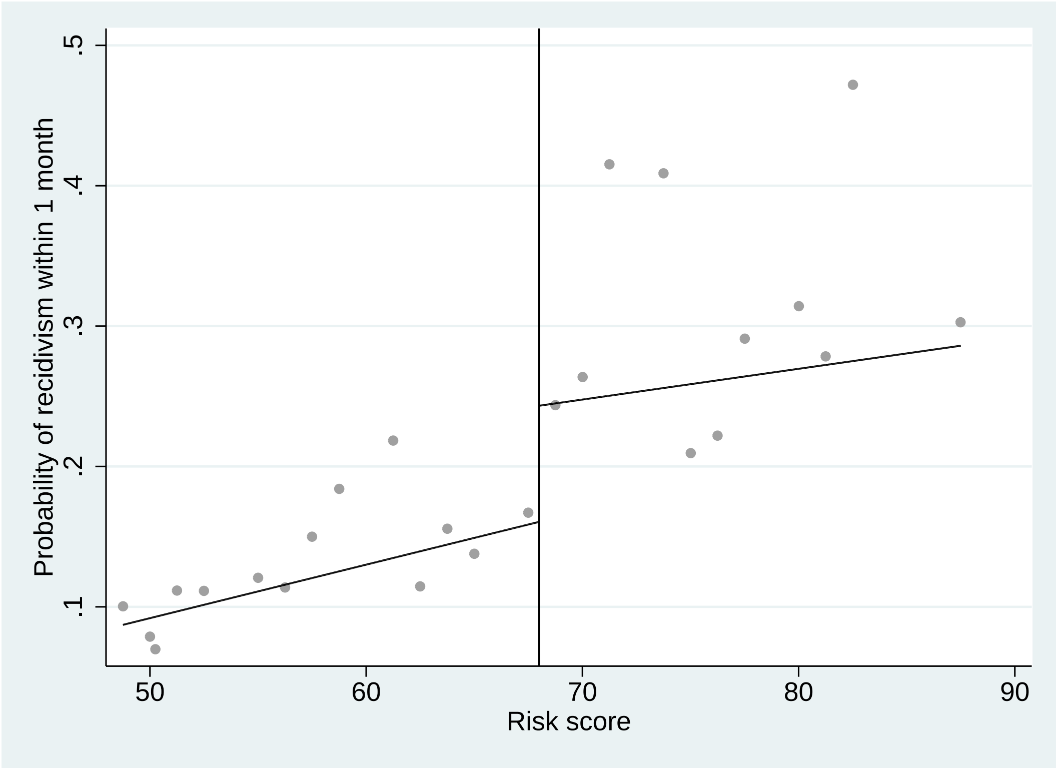


Figure 2.5: Impact of the risk score on the probability of recidivism within 1 month (Threshold 67.5, bandwidth 20)

Figure 2.5 depicts an increasing relationship between the dangerousness of the offenders i.e. their risk score and their likelihood to be reported again for domestic abuse within 1 month. Moreover, we observe that there is a clear positive discontinuity in recidivism at the 67.5 threshold.

The model predicts that the policy should deter recidivism from the offenders[30] and increase the likelihood that victims report. This positive discontinuity in observed recidivism at the threshold suggests that the reporting effect outweighs the expected decrease in recidivism.

Recall that recidivism includes both reported events that led to a criminal offence (or charges) and those that did not. Looking at the impact of the policy separately on both types of events can help to disentangle the different effects of the

[30]Recall that, since individuals were not aware that they were allocated risk scores, there is no reason to believe that they modified their behaviours accordingly.

policy. Recall that the variable Offence corresponds to the probability that an event of domestic abuse for which the police established criminal charges occurred within 1 month after date t[31]. Figure 2.6 depicts the nonparametric relationship between the probability of Offence and the risk score.

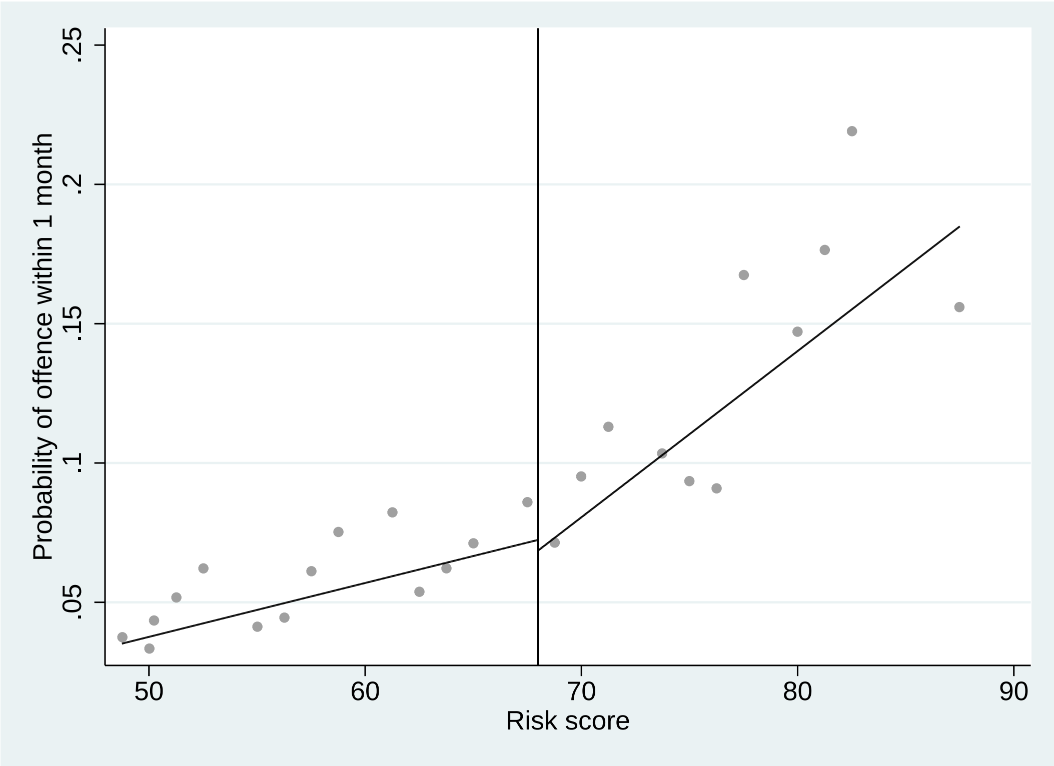


Figure 2.6: Impact of the risk score on the probability of offence within 1 month (Threshold 67.5, bandwidth 20)

Unlike in Figure 2.5, there is no discontinuity at the threshold and so, there is no increase in the probability of being charged in response to the policy. As discussed, the impact of the policy on reported crime is the net effect of the deterrence effect on the offenders and the increase in victims' reporting. As explained in Section 6, this may suggest either that the decrease in recidivism caused by the policy is compensated by an increase in reported recidivism. Another interpretation is that

[31]Notice that the justice system may take time to respond to incidents highlighted by the police and Offence corresponds to events that gave rise to charges but it does not imply that offenders have been judged or sentenced yet.

the policy has no effect on deterrence, and on reporting.

Figure 2.7 graphs the relationship between the risk score and the probability of an incident within 1 month.

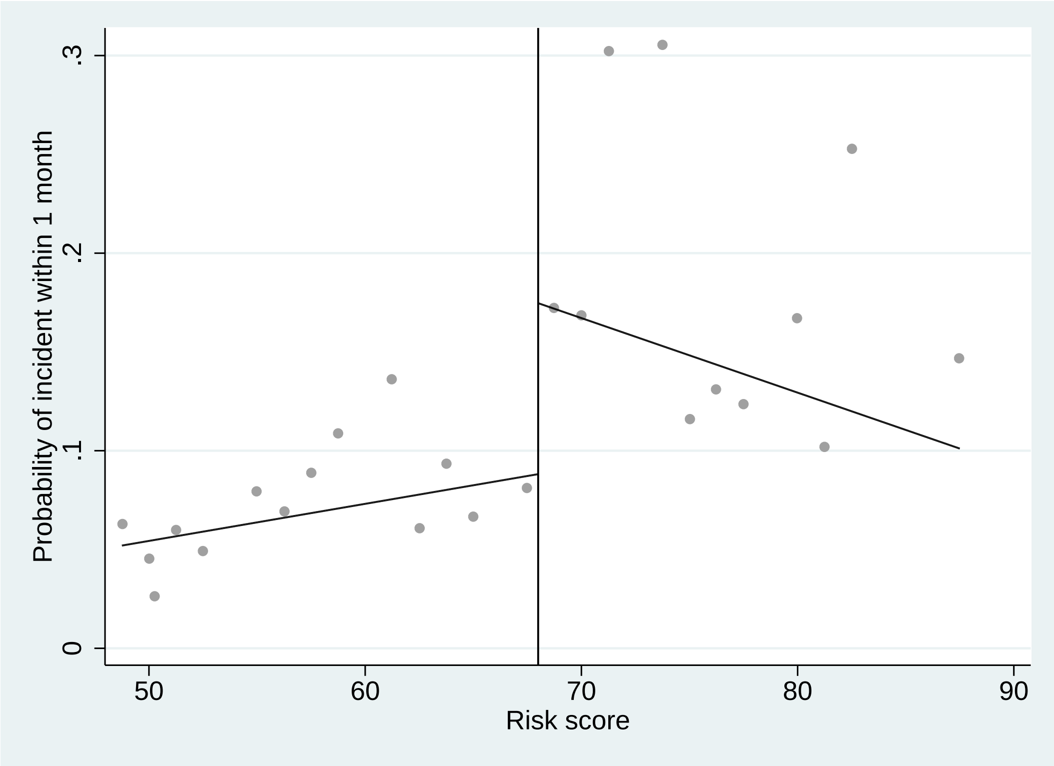


Figure 2.7: Impact of the risk score on the probability of incident within 1 month (Threshold 67.5, bandwidth 20)

Recall that Incident corresponds to the probability that a suspect was reported within 1 month after date t for domestic abuse events for which the police could not establish criminal charges. There is a clear jump at the threshold. Since, according to the model, the policy should deter crime as well as increase reporting, this suggests that the potential decrease in incidents as a result of the increasing probability of punishment is outweighed by the increase in the reporting of incidents. To assess the magnitudes and statistical significances of the discontinuities visible in these figures, I turn to formal RDD estimates.

Table 2.8 shows Ordinary Least Squares (OLS) regressions of the impact of the policy response on subsequent domestic abuse within one month. All these regressions include a polynomial of degree 1 and are based a bandwidth of 20[32]. In addition to a treatment dummy, the risk score and the interaction between both, all regressions include the following controls: Age, White and Female. Standard errors are clustered at the Risk score level [33].

Table 2.8: Impact of the risk score on the probability of recidivism within 1 month

	(1) Recidivism	(2) Offence	(3) Incident
Treatment	0.0891***	-0.00895	0.0981***
	(0.0276)	(0.0111)	(0.0198)
Bandwidth	[-20;20]	[-20;20]	[-20;20]
Observations	64,256	64,256	64,256
R-squared	0.023	0.009	0.014

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

Results are in line with graphical evidence and exhibit a positive impact of the policy on reported recidivism. Column (1), show that offenders with scores past the threshold i.e. who were affected by the policy are almost 9% more likely to be reported again within 1 month. As expected, I do not observe any impact of the policy on the likelihood of the offenders being reported for events for which the police could establish criminal charges i.e. offences. The probability that offenders were reported for incident of domestic abuse for which the police could not establish

[32] In Section 11, I show that results are entirely robust to a selection of different bandwidths and a polynomial of degree 2. As explained in Cattaneo et al. (2017), higher-order polynomials tend to produce over-fitting of the data and unreliable results near the cufoff points. This issue is particularly important when using discrete data with a few mass points. Therefore, I present results only with polynomials of degree 1 and degree 2.

[33] Following Lee and Card (2008), we interpret the difference between the true conditional expectation and the estimated regression function as a random specification error that introduces a group structure in the standard errors and we cluster the standard errors at the risk score level to correct for this.

criminal charges is almost 10% higher for offenders under the policy. Considering that I observe the net effect of the impact of the policy on crime and reporting, this underlines that the effect of the policy on reporting may be even higher. So the 10% increase in reported incidents can be thought of as a lower bound of the increase in reporting in response to the policy.

To sum up, results emphasize that the policy shed light on events of domestic abuse that would have gone underreported. More specifically, they show that suspects under the policy are more likely to be reported for incidents that do not lead to criminal charges but there is no evidence of an increase in reported crime. One interpretation of these results is that the policy makes victims more likely to start reporting small events of domestic abuse for which they were not expecting initially a response from the police. Moreover, for events that lead to criminal charges, victims and/or witnesses may be more likely to report them in general, which that may explain why the policy has no clear effect on the reporting of such events. Although, this increase in reporting is positive as it enables to identify events of domestic abuse, whether it is beneficial to the victims in the long term will depend on how their reporting will affect the actual payoffs of their offenders. As discussed, the increase in reported events comes from changes initial SPNE (A,NR) that became (NA,NR) in the wake of the policy at t_1. Section 2.10 will examine whether these changes at t_1 will deter domestic abuse and it will investigate the long-term impact of the policy.

2.9 Heterogeneity of the policy on subgroups

Many exogenous factors may affect how individuals respond to the policy. For instance, female offenders might be more deterred than male offenders. Moreover, the ethnicity of the offenders may affect their belief that the police will take actions against them. Likewise, offenders' age may also influence their response to the policy. To understand whether the impact of the treatment is heterogeneous across different types of offenders, this section investigates the impact of the treatment across gender, ethnicity and age groups.

Figure 2.8 describes the relationship between the risk score and the probability of recidivism by gender. The first graph looks at the impact of the treatment on women and the second one on men.

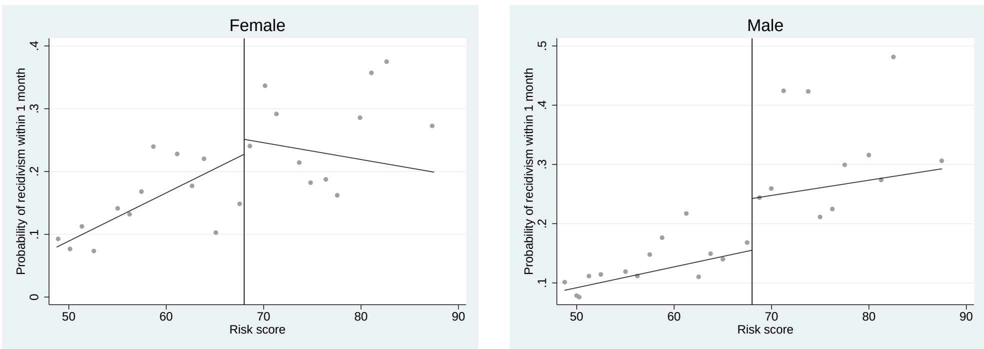


Figure 2.8: Impact of the risk score on the probability of recidivism within 1 month (By gender, bandwidth 20)

The policy seems to have no effect on female offenders whereas I observe a positive effect of the treatment for male offenders. Table 2.9 shows OLS regressions of the impact of the policy on subsequent domestic abuse within one month. All these regressions include a polynomial of degree 1 and are based on a bandwidth of 20. Column (1) and column (2) show respectively regression results for women and men. In line with graphical evidence, they emphasize that the impact of the policy is positive but not significant for women while it increases observed recidivism by almost 9.5% for male offenders.

One explanation for this is that women are more deterred by the policy than men. So, female offenders may reduce crime more than male offenders, which attenuates the effect on observed recidivism of the potential increase in reporting from their victims. Another explanation for this results is that female offenders are treated differently by the police than male offenders due to the context of their offences. According to the police, female offenders are usually part of troubled relationships where they usually perpetrate offences in response to domestic abuse from their partners[34]. As a consequence, the police may be less keen to encourage their victims to report against them.

Figure 2.9 describes the relationship between the risk score and the probability of recidivism across ethnicities. The first figure corresponds to individuals that are white and the second to those that are non-white.

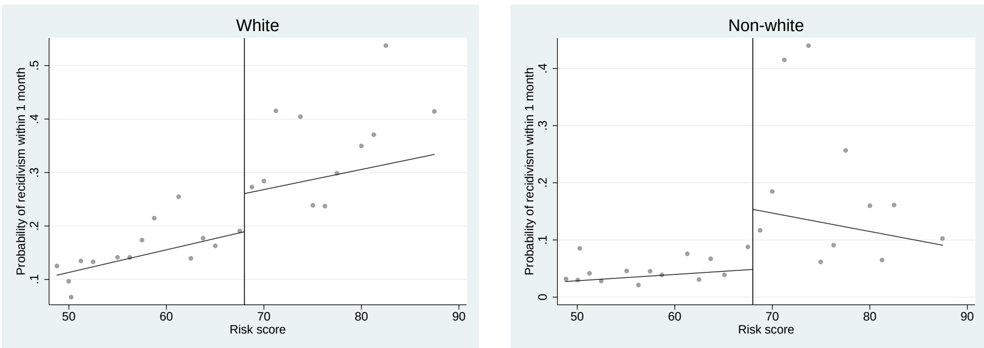


Figure 2.9: Impact of the risk score on the probability of recidivism within 1 month (By ethnicity, bandwidth 20)

The graphs exhibit a sharper discontinuity for non-white offenders. Columns (3) and (4) from Table 2.9 show that white offenders are 7% more likely to re-offend while non-white offenders exhibit an average increase of almost 35%. This might reflect the fact that offenders from minorities ethnic groups are less deterred by the

[34]For my sample, female offenders are more likely to be both victims and offenders (50% versus 18%).

policy. Finally, if minority offenders are more likely to have minority victims, the results could be driven by the fact that usually minorities trust the police less and so the efforts of the police to encourage victims to report might have more impact than in populations that usually report more.

Figure 2.10 describes the relationship between the risk score and the probability of recidivism across age groups. The graph in the top left corner corresponds to offenders whose age is in the lowest 25th percentile i.e. less than 25 years old. The graph in the top right corner corresponds to suspects whose age lies between the median age and the 25th percentile i.e. between 25 and 34 years old. The graph in the bottom left corner represents individuals whose age is above the median and below the 75th percentile i.e. between 34 and 44 years old. Finally, the graph in the bottom right corners corresponds to individuals whose age is in the top 75 percentile i.e. above 44 years old.

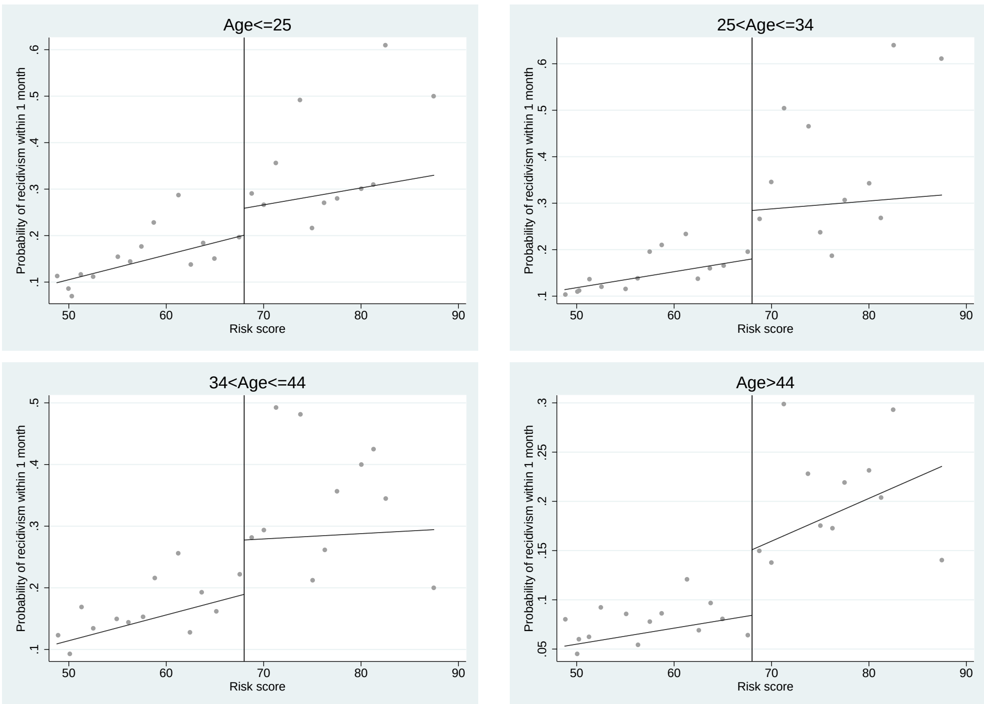


Figure 2.10: Impact of the risk score on the probability of recidivism within 1 month (By age groups, bandwidth 20)

The graphs from Figure 2.10 do not emphasise any clear pattern in the impact of the treatment across age groups. Implying that deterrence may be similar across age groups.

Regressions results do not show any clear difference across age groups either. Column (5) from Table 2.9 emphasises that young offenders are 6.27% more likely to re-offend when exposed to the treatment. Point estimates go up to 10.5% for individuals aged 25 to 34 (see column (6)) and fall to approximately 9% for offenders between 34 and 44 years old. Offenders above 44 are about 11% more likely to exhibit recidivism.

Table 2.9: Impact of the risk score on recidivism by individual characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: *Recidivism*								
Treatment	0.0255	0.0949***	0.0715**	0.349***	0.0627*	0.105**	0.0898***	0.108***
	(0.0388)	(0.0283)	(0.0261)	(0.0596)	(0.0312)	(0.0410)	(0.0310)	(0.0238)
Sample	Female	Male	White	Non-white	Age $\leq$ 25	25¡Age$\leq$ 34	34¡Age$\leq$ 44	Age¿44
Bandwidth	[-20;20]	[-20;20]	[-20;20]	[-20;20]	[-20;20]	[-20;20]	[-20;20]	[-20;20]
Observations	64,256	64,256	59,255	5,001	17,063	19,656	15,631	11,882
R-squared	0.009	0.014	0.021	0.063	0.024	0.022	0.021	0.027

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

2.10 Longer term impact and potential perverse effects of the policy

Empirical results confirm that the policy increased reporting within 1 month after it took place. The policy pushed victims to report events of domestic abuse at t_1 (or within one month) that they would not have reported otherwise. Whether the unexpected reporting changes the future behaviour of the offender at t_2 (or in the second month after the policy takes place) depends on his payoff after being reported. If the punishment of the offender at t_1 is lower than his gain from violence, he will keep on abusing at t_2 and we have the following situation: $(A, NR)_{t0}$ at t_{t0} becomes $(A, R)_{t1}$ at t_1 and stays at t_2 such that the SPNE is $(A, R)_{t2}$. However, if the punishment of the offender at t_1 is greater than his gain from violence, he will stop abusing at t_2 and we have the following situation: $(A, NR)_{t0}$ at t_0 will become $(A, R)_{t1}$ at t_1 and $(NA, NR)_{t2}$ at t_2, which will deter crime at t_2.

To test whether the increase in reporting at t_1 deters recidivism at t_2, we investigate the impact of the policy in the second month after it took place (at t_2), conditional on being reported within one month (at t_1). Table 2.10 shows Ordinary Least Squares (OLS) regressions of the impact of the policy response in the second month conditional being reported within one month. Recidivism2 corresponds to the probability that a suspect was reported for domestic abuse in the second month after the calendar date t. Recidivism2 can be divided into two components i.e. Offence2 and Incident2. Offence2 is the probability that a suspect is reported in the second month after date t for domestic abuse events for which the police established criminal charges. Incident2 is the probability that a suspect is reported in the second month after the calendar date t for domestic abuse events for which the police could not establish any criminal charges. All these regressions include a polynomial of degree 1 and a bandwidth of 20. In addition to a treatment dummy, the risk score and the interaction between both, all regressions include the following controls: Age, White and Female. Standard errors are clustered at the Risk score level.

Regression (1) from Table 2.10 shows the impact of the policy on recidivism in the second month after the policy took place, conditional on the offender being reported in the first month[35]. Under the policy, we observe almost a 8% increase in the probability of being reported in the second month, conditional on being reported in the first month. This suggests two things. First, on average, the policy does not deter offenders in a longer run. Moreover, the gain from reporting is important enough for the victims in the first month to make them keep on reporting in the future. So, although their offender does not get legal sanctions, revealed preferences may suggest that victims get an indirect benefit from reporting. For instance, the police may put them in touch with charities that support them.

Table 2.10: Impact of the risk score on the probability of recidivism within the second month after passing the threshold

	(1) Recidivism2	(2) Incident2	(3) Incident2	(4) Offence2	(5) Offence2
Treatment	0.0785***	0.137***	0.0341	0.0992***	-0.109**
	(0.0214)	(0.0192)	(0.0396)	(0.0345)	(0.0402)
Incident	Yes	Yes	No	Yes	No
Offence	Yes	No	Yes	No	Yes
Observations	7,979	4,544	3,435	4,544	3,435
R-squared	0.019	0.022	0.002	0.028	0.043

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions also include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

As explained by the model, the higher the increase in the probability that the police respond, the more likely the deterrence effect and the efficacy of the policy in the long run. Moreover, for a given event of domestic abuse, legal sanctions at t_1 makes it more likely that the payoff of the offenders is negative, which deters

[35] Regression results investigating recidivism in the third month after suspects come under the policy are qualitatively similar to those I present in Table 2.10 and are available upon request. However, due to the smaller sample size, I do not present these results.

future recidivism if it increases the expected future probability of the offender to get legal sanctions for such events. Furthermore, under some conditions, the reporting of low-intensity events may lead to escalation, which may jeopardise the effect of the policy in the long term. To further understand the longer term effect of the policy, I consider how the reporting of events of different intensity within one month after the policy took place affects the observed recidivism of different intensity in the following month.

Column (2) shows the impact of the policy on the probability of events of domestic abuse in the second month for which the police could not establish criminal charges (Incident2 equal to 1), conditional on being events of domestic abuse for which the police could not establish criminal charges were reported in the first month after the policy (Incident equal to 1). Regression results show that suspects are about 14% more likely to be reported for an incident in the second month if they were already reported for an incident in the first month (if Incident is equal to 1). One explanation for this is that they were reported in the wake of the policy within 1 month but the punishment they got did not deter them. In this situation, although being reported can already be seen as a punishment, the fact that the reporting did not lead to criminal charges did not deter them on average. From the victim's side, there is no evidence that they lose trust in the police after reporting events in the wake of the policy for which the police could not establish criminal charges. They may keep on reporting because their expected gain from it is now higher than before the policy. As suggested, they may get indirect benefits from reporting such as getting support from charities.

Column (3) shows the impact of the policy on the probability of being reported for events of domestic abuse in the second month for which the police could not establish criminal charges if an offence was reported in the first month after the policy i.e. if Offence was equal to 1. If suspects were reported in the first month for incidents of domestic abuse for which the police established criminal charges

(Offence is equal to 1), they are not more likely to be reported again for an incident that does not lead to criminal charges in the second month. This suggests that the potential punishment of the suspects deters future domestic abuse and the potential increase in victims' reporting compensates the decrease in crime.

Column (4) underlines the impact of the policy on offence in the second month after the policy took place, conditional on suspects being reported for an incident that did not lead to criminal charges in the first month (Incident equal to 1). Results show almost a 10% increase in the probability of being reported for an offence in the second month if suspects were reported in the first month for an incident that did not lead to criminal charges. As explained by the model, one explanation for this is that offenders that were reported in the first month after the policy took place were exerting low-intensity violence because their expected payoff was higher than when exerting high-intensity abuse that would be reported. So when reported for low-intensity abuse, the punishment given rise to the efforts of the police made offenders become better off committing high-intensity crime, knowing that in both cases they would be reported. Recall that the effect of the policy is driven by an increase in the reporting of events of domestic abuse of low intensity. So, these results cast doubt on the long-term efficacy of the policy.

Column (5) presents regression results of the impact of the policy on offence in the second month after the policy took place, conditional on suspects being reported for an incident that led to criminal charges in the first month in the wake of the policy (Offence equal to 1). I notice almost a 11% decrease in crime in the second month. This suggests that a high punishment can deter future crime.

2.11 Robustness checks

2.11.1 Main robustness checks

As described in Lee (2008), researchers face a trade-off in using RDD . On the one hand, using a larger bandwidth yields more precise estimates as more observations are available to estimate the regression. On the other hand, larger bandwidths increase the potential bias of the estimates. In the main regressions, I use a bandwidth of 20 in order to get enough distinct data points[36] while reducing potential biases that may arise from using a larger bandwidth.

To test the robustness of my results, I show main regressions both with a larger bandwidth (30) and with a smaller bandwidth (10). Table 2.11 shows OLS regressions of the impact of the policy response on subsequent domestic abuse within one month. All these regressions include a polynomial of degree 1. Columns (1), (2) and (3) from Table 2.11 show OLS regressions of the impact of the policy respectively on recidivism, offence and incident using a bandwidth 30. Columns (4), (5) and (6) from Table 2.11 show OLS regressions of the impact of the policy respectively on recidivism, offence and incident using a bandwidth 10. In line with main regressions results, in Table 2.8 the results are driven by incidents and I find no significant effect of the treatment on recidivism that led to criminal charges.

Due to the discreteness of the data, as a robustness check, I follow Cattaneo et al. (2017) and run regressions with a polynomial of degree 2[37]. Figure 2.11 shows the nonparametric relationship between the risk score and Recidivism (column (1)), Offence (column (2)) and Incident (column (3)) respectively fitting a polynomial of degree 2. Results are in line with the graphs with polynomials of degree 1 and the positive effect of the treatment on recidivism (graph in the top left corner) is driven

[36] Recall that the data is highly discrete with a small number of mass points (72) that have a large number of observations each (on average 23,000).

[37] For continuous data, the literature recommends to run regressions with polynomials up to degree 4 or degree 5 as a robustness check.

Table 2.11: Impact of the risk score on recidivism within 1 month (different bandwidth)

	(1) Recidivism	(2) Offence	(3) Incident	(4) Recidivism	(5) Offence	(6) Incident
Treatment	0.0919***	-0.00584	0.0978***	0.152***	0.00551	0.147***
	(0.0237)	(0.0101)	(0.0170)	(0.0298)	(0.0143)	(0.0197)
Bandwidth	[-30;30]	[-30;30]	[-30;30]	[-10;10]	[-10;10]	[-10;10]
Observations	159,932	159,932	159,932	32,159	32,159	32,159
R-squared	0.031	0.013	0.018	0.016	0.004	0.015

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

by the increase in the probability of incident (graph in the bottom). I observe no impact of the treatment on recidivism that led to criminal charges (graph in the top right corner).

Table 2.12 present OLS regression regressions with a polynomial of degree 2. Columns (1), (2) and (3) show the impact of the treatment on Recidivism, Offence and Incident respectively with a 20 bandwidth. Regression results are similar qualitatively and line up with the graphs from Figure 2.11. Once again, the increase in observed recidivism (column (1)) is driven by the increase in incidents (column (3)) and not by the probability of offence (column (2)).

As a robustness check, I also present OLS regressions with a polynomial of degree 2 with different bandwidths. Columns (1), (2) and (3) from Table 2.13 show OLS regressions of the impact of the treatment on Recidivism, Offence and Incident respectively with a bandwidth 10. Columns (4), (5) and (6) show the impact of the treatment on Recidivism, Offence and Incident respectively with a bandwidth of 30.

In line with all previous results, the impact of the policy on observed recidivism is driven by Incident i.e. events of domestic abuse that did not lead to criminal charges.

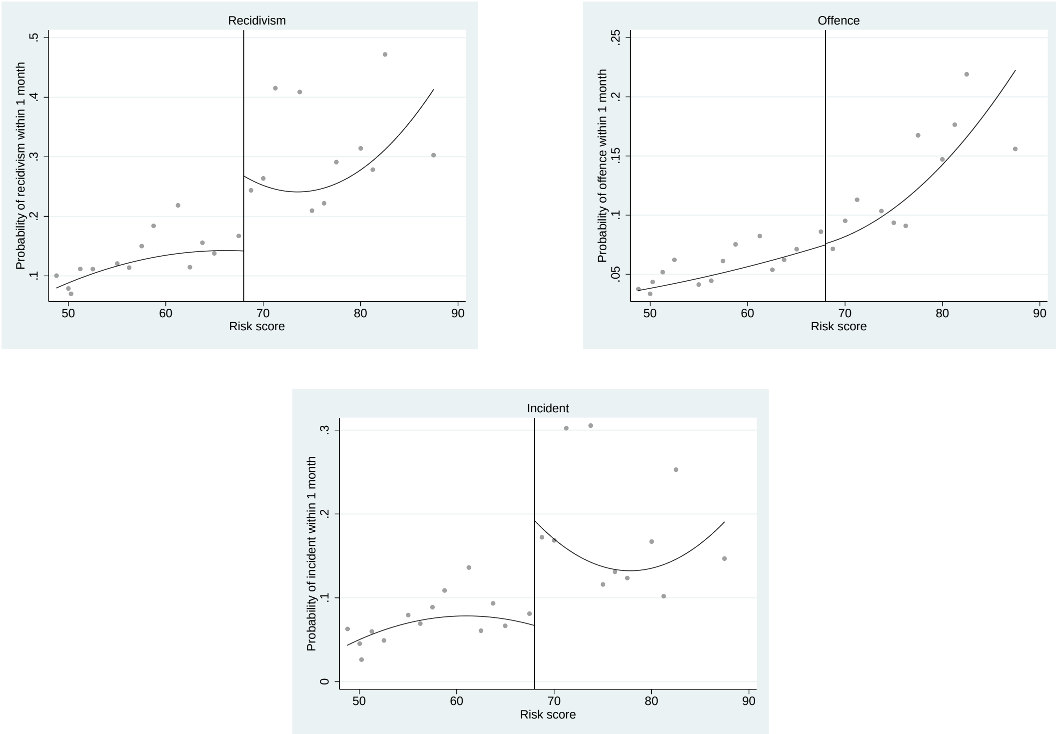


Figure 2.11: Impact of the risk score on recidivism within 1 month (Bandwidth 20, polynomial of degree 2)

Table 2.12: Impact of the risk score on recidivism within one month (Polynomial of degree 2)

	(1) Recidivism	(2) Offence	(3) Incident
Treatment	0.161***	0.00664	0.155***
	(0.0337)	(0.0157)	(0.0214)
Bandwidth	[-20;20]	[-20;20]	[-20;20]
Observations	64,256	64,256	64,256
R-squared	0.024	0.009	0.016

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, female and white.

Table 2.13: Impact of the risk score on recidivism (Polynomial of degree 2, different bandwidth)

	(1) Recidivism	(2) Offence	(3) Incident	(4) Recidivism	(5) Offence	(6) Incident
Treatment	0.161***	0.00664	0.155***	0.111**	0.00379	0.108***
	(0.0337)	(0.0157)	(0.0214)	(0.0494)	(0.0237)	(0.0320)
Bandwidth	[-30;30]	[-30;30]	[-30;30]	[-10;10]	[-10;10]	[-10;10]
Observations	159,932	159,932	159,932	32,159	32,159	32,159
R-squared	0.031	0.013	0.018	0.017	0.004	0.015

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

2.11.2 Difference in difference approach

As a further robustness check, I perform a Difference in Difference (DiD) analysis. More specifically, I look at whether having a risk score above the threshold when the policy is implemented increases the probability of being reported again for domestic abuse. As discussed previously, the data spans over two years. To calculate the risk score, one needs information on offenders within 12 months before each date at which the risk score is calculated and the policy takes place in the second year of the data. Therefore, I can only get an unbiased measure of the risk score for the second year of the data which is the same period of time during which the policy is implemented. Moreover, earlier in time that risk scores are calculated in the first year of the data, the less information there is and the potential bias. To better understand potential biases when computing risk scores over the first year of the data i.e. before the policy is implemented, it is worth looking at each component of the risk score.

Recall that the gravity score is based on the most severe event that occurred within 12 months before risk scores are calculated at t. When computing the gravity score with less data than the 12 months it requires, the challenge is that I might miss events that are more severe than the ones I capture within the available time span of the data[38]. As a consequence, the gravity score I obtain may be biased downward and can be considered as a lower bound.

To calculate the frequency score, I need to get the total number of events that occurred in the 12 months preceding calculation at t. The more the number of events, the higher the frequency score. So, calculating a risk score with less data than the 12 months it requires may cause me to miss events. As a consequence, the frequency score may be biased downward and can be considered as a lower bound.

To obtain the recency score, recall that I need to calculate the average time between calculation at date t and all the events that occurred in the previous 12 months. Using less data to compute the risk score potentially removes events that occurred further back in time. As a consequence, I potentially deflate the average time between a given calendar date and events in the past. The lower the average recency, the higher the recency score. As a consequence, by using less data, I potentially inflate the recency score.

So, when I compute a risk score with less data than the 12 months it requires, I obtain a biased risk score. The gravity score and frequency score are potentially biased downward and the recency score is potentially biased upward. As a benchmark, I first perform a DiD analysis with this biased risk score. The identification relies on a common trends assumption. Figure 11 shows the average probability of recidivism within 1 month over time for both the control group (individuals below the threshold) and the treatment group(individuals above the threshold). The policy takes place in week 62[39].

From Figure 2.12, I observe that average recidivism in the control group i.e. individuals below the threshold is around 5% and does not vary much over time. On

[38]Notice that, since the gravity score is based on the most severe offence within 12 months, events for which I do not have data and that are less severe will not affect the computation of the gravity score.

[39]Since the risk score may be more biased using earlier data, I show figures using data from 3 months before and after the implementation of the policy. Graphs with other time spans are available upon request.

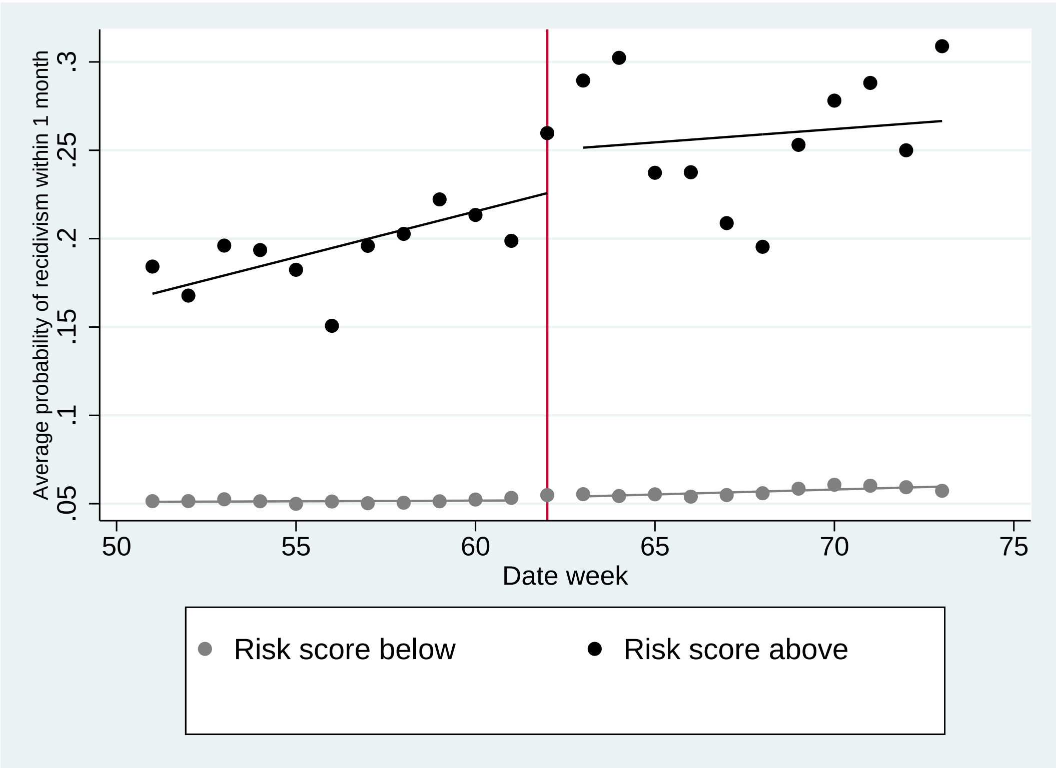


Figure 2.12: Average probability of recidivism within 1 month

Notes: This figure enables me to compare the average recidivism of individuals on both side of the threshold before and after the policy took place i.e. the common trends assumption. The vertical line represents the time when the policy was implemented.

the contrary, the average probability of recidivism in the treatment group increases over time and lies between 15% and 22% before the policy and between 20% and 31% under the policy. So, it seems that there is an increase in observed recidivism for individuals above the threshold while under the policy. To test the significance of this relationship, I run OLS regressions to investigate whether being above the threshold when the policy takes place i.e. being treated affects observed recidivism. Table 2.14 presents the results.

Table 2.14: Impact of the risk score on recidivism (Difference in difference (DinD))

	(1) Recidivism	(2) Incident	(3) Offence
Treatment	0.0447**	0.0415**	0.00317
	(0.0186)	(0.0161)	(0.0144)
Observations	3,490,892	3,490,892	3,490,892
R-squared	0.005	0.002	0.004

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, risk score above dummy, treatment time dummy, interaction between the risk score and the time dummy, age, female and white.

Column (1) from Table 2.14 shows the impact of the treatment (i.e. being above the threshold when the policy is implemented) on observed recidivism. Regression results underline that individuals who are above the threshold under the policy are 4.47% more likely to be reported again for domestic abuse. Column (2) and column (3) highlight the impact of the treatment on Incident (i.e. the probability of being reported within a month for domestic abuse that did not lead to criminal charges) and Offence (i.e. the probability of being reported within a month for domestic abuse that led to criminal charges) respectively. Like in the RDD framework, results are driven by Incident, the probability of which increases by 4.5% whereas I do not observe any impact on Offence. Note that the magnitude of the coefficient is two times smaller compared to the RDD set up, which could be explained by the fact I look at the average effect over the whole data and not at the effect around the threshold.

As discussed, risk score computed over the whole data are likely biased. Moreover, this bias could go both ways since the gravity score and the frequency scores are potentially biased downward while the recency score is potentially biased upward. In order to obtain lower bound estimates, I create a lower bound of the risk score by computing risk scores with a recency score equal to 0 (using the frequency score and the gravity score obtained over the whole data). Figure 2.13 presents the average probability of recidivism within 1 month for both the control group (individuals below the threshold) and the treatment group(individuals above the threshold)[40] over time.

Table 2.15 show OLS regression results when using these lower bounds of the risk score. Column (1), column (2) and column (3) show the impact of the treatment on Recidivism, Incident and Offence respectively.

Table 2.15: Impact of the risk score on recidivism (Difference in difference (DinD), lower bound estimates)

	(1) Recidivism	(2) Incident	(3) Offence
Treatment	0.0514***	0.0383***	0.0130
	(0.0166)	(0.00886)	(0.00935)
Observations	3,490,892	3,490,892	3,490,892
R-squared	0.004	0.002	0.003

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, risk score above dummy, treatment time dummy, interaction between the risk score and the time dummy, age, female and white.

In line with previous results, I observe that the policy has a positive impact on

[40]Recall that the policy takes place in week 62.

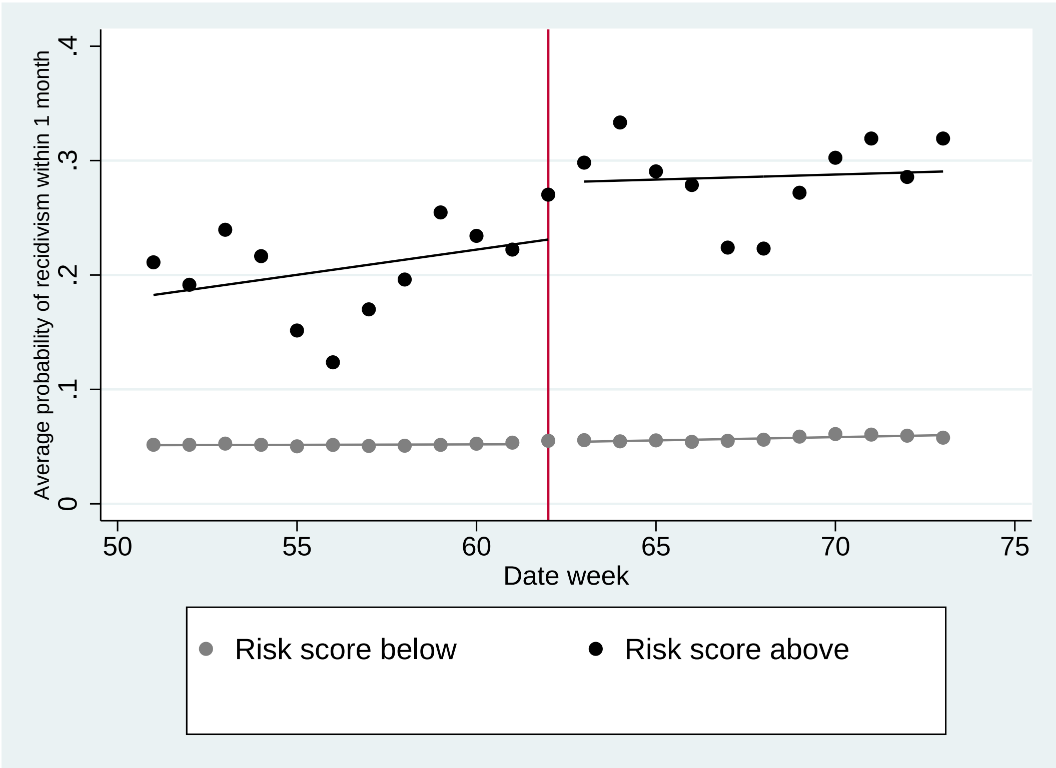


Figure 2.13: Average probability of recidivism within 1 month

Notes: This figure enables me to compare the average recidivism of individuals on both side of the threshold before and after the policy took place i.e. the common trends assumption. The vertical line represents the time when the policy was implemented.

observed recidivism within one month. Moreover, the increase in observed recidivism is driven by Incident and there is no impact on Offence. To test the robustness of these results, I perform a DiD using lower bounds estimates of the risk score with different time windows around the implementation of the policy. Table 2.16 presents OLS regressions of the impact of the treatment on Recidivism using all the data (column (1)), a one month window before and after the policy (column (2) and a one week window (column (3)) respectively.

Table 2.16: Impact of the risk score on recidivism (Difference in difference (DinD), lower bound estimates, different time windows.

	(1) Recidivism	(2) Recidivism	(3) Recidivism
Treatment	0.0514***	0.0814***	0.0617**
	(0.0166)	(0.0102)	(0.0219)
Time window	All data	3 months	1 week
Observations	3,490,892	788,121	94,575
R-squared	0.004	0.004	0.004

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, risk score above dummy, treatment time dummy, interaction between the risk score and the time dummy, age, female and white.

I observe that the impact of the policy on observed recidivism is robust across all window and always significant and positive. One may worry that the increase in incidents is due to the fact that victims minimised high-type events. In other word, in the wake of the policy victims may report being victims of low-type domestic abuse incidents while facing serious crimes in reality. As a robustness check, I aggregate the number of events of domestic abuse at the weekly level in order to compare trends before and after the policy (that takes place in week 62). Figure 2.14 shows two graphs of respectively the total number of incidents and offences over time.

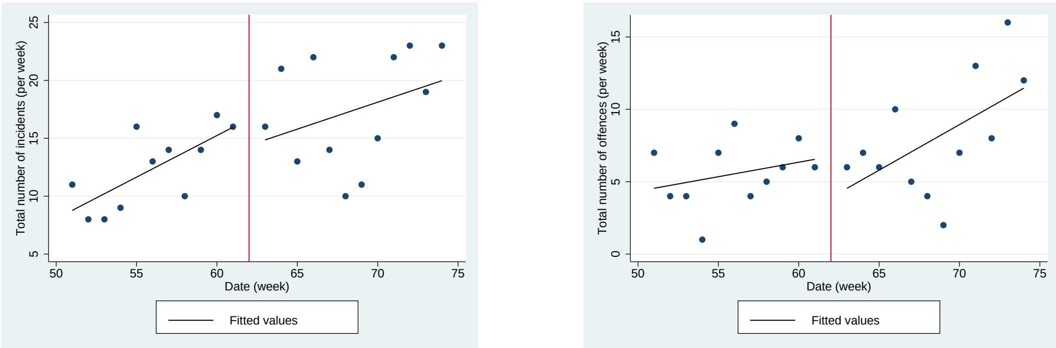


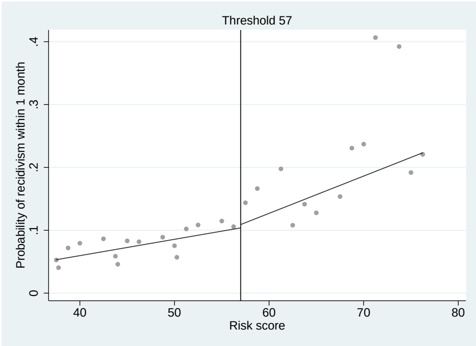
Figure 2.14: Total number of events of domestic abuse (per week, individuals above the threshold)

From the first graph in Figure 2.14, I observe that, before the policy, the total number of incidents for individuals that are above the threshold lies between 6 and 17. The number of incidents reported increases under the policy to between 10 and 25. When I look at trends in offences in the second graph from Figure 2.14, offences lie between 2 and 10 before the policy takes place and between 3 and 15 under the policy. So, graphs do not suggest a transfer from high-type events to low-type events.

2.11.3 Placebo

I construct Placebo tests using different risk score thresholds. Recall that the risk score threshold with regards to which the policy is based is 67.5. I look at the impact of artificial thresholds i.e. 57.5 and 77.5 on my main variable of interest i.e. Recidivism. The graph in the top left corner of Figure 2.14 shows the impact of the risk score on the probability of recidivism using a 57.5 threshold. I see no discontinuity at the threshold. When I look at the impact of the risk score on the 77.5 threshold, I see a small jump for recidivism (graph in the top right corner).

Table 2.17, shows OLS regressions of the impact of the risk score on recidivism using different Placebo thresholds and bandwidth. Columns (1), column (2) and column(3) show respectively the impact of the policy on recidivism using the



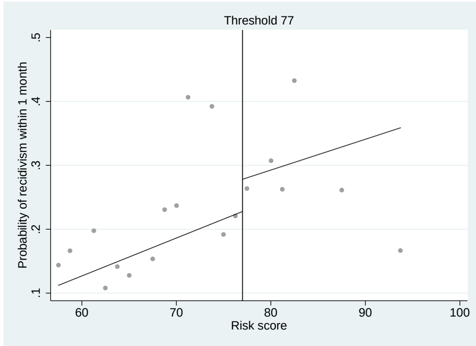


Figure 2.15: Impact of the risk score on recidivism within 1 month (Placebo thresholds, polynomial of degree 1)

57.5 placebo threshold with a bandwidth 30, a bandwidth 20 and a bandwidth 10. Column (4), column (5) and column (6) are analogous to columns (1), column (2) and column (3) except that I use the 77.5 threshold. None of the regressions show any impact of the treatment on recidivism.

Table 2.17: Impact of the risk score on recidivism (Placebo, different thresholds)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Recidivism*						
Treatment	0.0260	0.0189	0.0207	0.0473	0.0466	0.0864
	(0.0198)	(0.0205)	(0.0232)	(0.0504)	(0.0495)	(0.0543)
Cutoff	57.5	57.5	57.5	77.5	77.5	77.5
Degree Polynomial	1	1	1	1	1	1
Bandwidth	[-30;30]	[-20;20]	[-10;10]	[-30;30]	[-20;20]	[-10;10]
Observations	237,215	32,937	10,388	64,258	32,937	10,388
R-squared	0.032	0.015	0.008	0.021	0.015	0.008

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

2.12 Conclusion

This paper studies the impact of a policy based on the Strathclyde model to reduce domestic abuse in Essex. It shows evidence that suspects of domestic abuse that are targeted by the policy are 9% more likely to be reported again for domestic abuse events within 1 month. Recall that the policy aims to deter offenders through informing them they will be under higher surveillance and also to encourage victims to report by increasing domestic abuse recognition and trust in the police response. So, the effect of the policy on observed recidivism corresponds to the net effect of the policy on both victims and offenders. As a consequence, the increase in reporting from the victims may be even greater than 9%. So, this result can be thought of as a lower bond of the effect of the policy on reporting.

Interestingly, the effect of the policy on reporting is driven by events of domestic abuse for which the police could not establish any criminal charges. More precisely, I observe a 10% increase in the probability that suspects targeted by the policy are reported again for events of domestic abuse that did not lead to criminal charges. However, there is no evidence that the policy has an effect on the reporting of crime. This may be because the policy has no impact on both the deterrence of the suspects neither on the reporting of crime or that these effects cancel each other out.

One interpretation of the heterogeneous effect of the policy on different types of domestic abuse is that, when committing domestic abuse that could lead to criminal charges, offenders may already expect that the police would put efforts to investigate their case. So, the policy may not change greatly their expected beliefs about being punished and, subsequently, their behaviour. Moreover, when reporting more severe events of domestic abuse, victims may also already expect a police response. On the contrary, for low-type events, victims may not have reported in the absence of the policy but they may now understand that it is legitimate for them to report these low-type events and they may now believe that the police will respond to their complaints.

Although the fact that the policy increased reporting in the short term is positive, the long-term impact of the policy will depend on the actual response of the police in the wake of the reporting. Results show that, conditional on being reported within one month after the policy took place, offenders are also more likely to be reported again in the second month. This result does not show evidence that, on average, the policy deterred crime in the long run. However, this suggests that victims keep on reporting and, subsequently, that reporting has a gain for them. Interestingly enough, when we look at the impact of being reported in the wake of the policy for different levels of violence, we observe that reporting may only deter crime in the medium run if it leads to criminal charges. On the contrary, the reporting of events that do not lead to criminal charges appears to increase observed recidivism. Considering that the short-term impact of the policy seems to be driven by events of domestic abuse that did not lead to criminal charges, this casts doubts on the efficacy of the policy in reducing crime.

Social workers, as well as the police or the justice system, have developed several approaches to prevent domestic abuse. For instance, social workers engage in teaching skills that promote respectful, nonviolent relationships through individual, relationship, community, and societal level change are key strategies. Moreover, NGOs create protective environments, shelters and/or try to strengthen economic support for families. All these methods to prevent domestic abuse are more or less expensive and some preventive tools may be more cost-effective than others. For instance, annual funding to provide core support for refugees and other accommodation-based services, rape support centres and national helplines exceed £80 million (Allen and Strickland (2017)).

Another way to tackle domestic abuse and its long-term consequences is to increase reporting and, in particular, early reporting. Recall that improving reporting is also essential under limited resources as, underreporting and especially, late reporting gives rise to substantial economic costs and the Early Intervention Foundation (EIF) estimated in 2016 that the cost of late intervention in England and Wales is £5,230 million per year. One way to promote reporting is to increase trust in the police among victims, which is relatively cheap to implement. By demonstrating empirically and quantifying how an increase in the trust in the police can increase early reporting, this paper aims to contribute to the elaboration of cost-effective prevention policies.

Chapter 3

Natural disasters and migrants' responses: Evidence from the United Kingdom

3.1 Introduction

Natural disasters have huge impacts both on human beings and on the economy. According to Yang (2008), from 1970 to 2002, 2.74 million people were killed by natural disasters and 2.70 million people were injured. This corresponds to 85,425 deaths and 84,325 people injured per year. Natural disasters also caused substantial economic damages worldwide. Over the same period, they amounted to US$987 billion. These phenomena are not restricted to specific regions since 39% of world population lives in countries that experienced damages of at least 3% of GDP (Yang, 2008). Moreover, many people live in environments at risk since about 19 per cent of the Earths land area and 3.4 billion people are relatively highly exposed to natural disasters (World Bank, 2005). Natural disasters may remain an issue in the future as trends are increasing and figures should go beyond the current average of 600 disasters per year worldwide (World Bank, 2005). In line with this, the number of individuals affected by disasters is expected to increase since populations are growing in disaster-prone areas (Kellenberg et al., 2011). Natural disasters also have a negative effect on poverty and, due to their high prevalence and impact in developing countries, they may jeopardise the 2030 policy agenda of the United Nations (UN) to eliminate extreme poverty (ODI, 2013).

Remittances are at the top of the research agenda of international organisations due to their important impact on economic development. They represent substantial amounts of money and, in 2015, migrants remitted US$441 billion through formal channels. They are often the largest flows of money sent to developing countries and, in 2015, they amounted to 10 per cent of GDP for 25 developing countries and represented 3 times the volume of aid flows (World Bank, 2016). In the aftermath of natural disasters, remittances play an important role in mitigating the effects of natural disasters. They can contribute to the replacement of the capital destroyed by shocks, and help people compensate for income losses or to relocate. International organisations believe that remittances could increase even further and that more understanding of the behaviours of the senders is needed to unleash the full potential of remittances (House of Commons, 2007; World Bank, 2013). The main contribution of this paper is to shed light on how migrants living in the UK modify their economic behaviours to fund remittances when facing natural disasters in their home countries.

First, this study provides evidence that migrants living in the UK modify their remitting behaviours in the wake of natural disasters. Furthermore, it examines how migrants manage to fund remittances when facing unexpected shocks like natural disasters. More specifically, it investigates several potential channels through which migrants may increase remittances: labour supply, savings and leisure consumption. For this analysis, I compile a dataset from 3 different sources: the Understanding Society household panel survey (USS), the World Bank Indicators (WBI) and the

Emergency Events Database (EM-DAT). Regressing directly measures of past natural disasters on current outcome variables, this paper emphasises two main results. It underlines that male migrants living in the UK and originating from developing countries[1] are more likely to remit in response to natural disasters. Moreover, it shows evidence that male migrants increase labour supply, and decrease monthly savings and leisure in response to natural disasters. Modifications in migrants' behaviours emphasise that migrants have the capacity to make economic adjustments in the wake of natural disasters. The fact that migrants can increase labour supply shows that UK labour markets are flexible enough to enable migrants to adjust.

The rest of the paper is organised as follows: section 2 summarises the literature, section 3 contains the identification strategy, section 4 describes the data, section 5 presents the results, section 6 shows robustness checks and section 7 concludes.

3.2 Literature review

Migrants send remittances for various reasons and researchers distinguish between two main types of remittances. The first main category of remittances described in the literature encompasses all monetary transfers sent for personal motives. For instance, in their seminal paper on Botswana, Lucas and Stark (1985) emphasise that altruism is an important personal motive behind remitting behaviours. Another kind of remittances that falls into this category corresponds to remittances sent to buy a wide range of services (Rapoport and Docquier, 2005). For instance, a migrant who has a house in his home country may need to pay for upkeep. Remittances sent for personal motives also include monetary transfers sent to relatives in order to gain their support in case of migration failure. This occurs, for instance, if

[1]The data contain information from 2009 to 2015 on migrants originating from the following 12 developing countries with the higher stocks of migrants in the UK: Bangladesh, China, Ghana, India, Jamaica, Kenya, Nigeria, Pakistan, South Africa, Sri Lanka, Turkey and Uganda.

a migrant does not find a job in his host country and needs to move back. This is what Amuedo-Dorantes (2008) describes as a self-insurance motive. Finally, it also corresponds to regular flows of money to pay back loans contracted by migrants in order to finance their migration (Rapoport and Docquier, 2005). The second main type of remittances corresponds to remittances sent due to family arrangements. It includes remittances sent for risk-diversification and consumption smoothing purposes. Migration decisions are often taken at the family level (Azam and Gubert, 2006) and it is common that migrants are sent to serve as an insurance against income shocks. Then, in case of shocks such as natural disasters, migrants are expected to compensate income losses by sending remittances.

There is an extensive literature, both at the macro and the micro levels showing evidence that remittances play an important role in smoothing the effects of natural disasters. At the country level, Yang (2008) shows that an increase in hurricane exposure is associated with greater remittance flows. Likewise, Ebeke et al. (2013) underline that countries with an increase in natural disasters receive more remittances. In line with these findings, Mohapatra et al. (2012) find the same phenomenon but for countries with larger emigrant stocks as a share of the home country population. There is also extensive evidence at the individual level that migrants increase remittances as a result of disasters in their home countries. Lucas and Stark (1985) provide evidence that migrants from Botswana increase remittances in response to droughts. Miller and Paulson (2007) emphasise the same phenomenon looking at income shocks caused by rainfall in Thailand. Clarke and Wallsten (2003) find similar findings after the hurricane Gilbert in Jamaica. Unlike these papers that all focus on one receiving country and one type of natural disaster, this study will focus on several types of disasters in all the source countries of the migrants of my sample. Moreover, to my knowledge, no paper has investigated how migrants living in the UK and originating from developing countries respond to natural disasters in their home countries. Thus, a main contribution of this paper is to provide evidence that migrants from developing countries are more likely to remit in the wake of natural disasters in their home countries[2]. Results suggest that migrants can increase remittances to compensate for losses caused by natural disasters in their home countries.

There is very little understanding of how migrants make decisions over remittances, other economic variables and behavioural factors. Using German data, Dustmann and Mestres (2010) show that changes in migrants' return plans are related to large changes in remittance flows. In line with this, Bauer and Sinning (2009) and Sinning (2011), underline that migrants who intend to stay in Germany only temporarily have a higher propensity to save, save larger amounts and remit more than permanent migrants. Interestingly, they observe that economic characteristics and the composition of households in home and host countries do not seem to affect these behaviours. Sinning (2011) also shows that the correlation between household size and migrants' remittances is significantly negative and that remittances are higher if close relatives live in the sending country. In these papers, researchers analyse choices that are made ex-ante and are endogenous. Unlike the current literature, this paper will exploit natural disasters, which are exogenous shocks, in order to better understand what kind of economic adjustments migrants make to fund an increase in remittances. The assumption behind this is that natural disasters at a given time are orthogonal to migrants' characteristics. This paper underlines that migrants increase labour supply and decrease savings and leisure as a result of natural disasters.

Recent work commissioned by international organisations has attempted to shed light on the impediments and hurdles that could prevent migrants from increasing remittances. In line with this, the World Bank commissioned a project in 3 European cities including London (Greenback Project, 2013) to understand which

[2]This paper focuses on migrants from developing countries since the latter have weaker institutions, which makes remittances crucial to cope with natural disasters.

policy reforms could boost the increase in remittance outflows from Europe (Geenback Project, 2013). They emphasise that the speed of the transactions and the costs are the two major factors affecting the amounts of remittances. A key question that has not been addressed is whether migrants have the capacity to increase remittances and send more than they usually do. Looking at unexpected shocks and how migrants respond to them is a way to investigate this question. Modifications in migrants' behaviours such as the fact that men increase labour supply and decrease savings support the hypothesis that there is room to increase remittance outflows in the UK. Moreover, by showing evidence that labour markets are crucial in enabling migrants to increase remittances, this paper suggests that easing migrants' access to labour markets may be a valuable policy tool to boost remittances.

An inherent drawback in most European remittance datasets is that they only include migrants' remitting status. Those that do contain remittance amounts tend to have smaller samples and lack panel dimension. Most UK data suffer from these features and no analysis with individual panel data has been performed. Clark and Drinkwater (2007), using a cross-section survey data, carried out the only empirical study on remittances at the household level in the UK. They show that richer households and those with more immigrants tend to remit more. However, some unobservables such as behavioural characteristics of the migrants may be correlated with explanatory variables and jeopardise the results. In order to remove of all potential unobservables that are fixed over time, this paper uses a micro-panel data and regressions include individual fixed effects. When looking at the impact of disasters in migrants' home countries on remittances, only analysing remitting statuses can be misleading. For instance, let us assume that in year t there were natural disasters and a migrant sent money both in response to natural disasters and to repay a loan. Let us suppose now that one year later (in year t+1) there were no natural disasters and this migrant only sent money to repay his loan. In both years, his remitting status, i.e. the extensive margin of remittances was the same and one may wrongly assume he did not respond to natural disasters in his country of origin. Also, as previously exposed, migrants may anticipate natural disasters in their country of origin and send regular amounts of money, which will not be captured either. By looking at how migrants adjust their behaviours in response to natural disasters in their home countries, this paper proposes a method to overcome data limitations and to obtain information on the intensity of remittances. The underlying assumption is that migrants' changes in response to natural disasters underline changes in remittances.

3.3 Identification strategy

As stated above, the main objective of this paper is to examine whether migrants in the UK modify their behaviours in the wake of natural disasters in their country of origin. First, I test the impact of a contemporaneous natural disaster shock on the extensive margin of remittance behaviour, that is, the probability of remitting. However, given the problem of only looking at the extensive margin, as discussed above, I also look at the intensive margin. To do so, I then turn to the margins along which remitters adjust in the labour market so as to fund the change in remittances. Specifically, I test the probability of switching from unemployment to employment, the number of hours worked conditional on being in the labour force, and the probability of having a second job. I also investigate the role of other potential margins of adjustments that migrants may use to fund remittances such as monthly savings and leisure consumption like the probability of doing sport. Finally, I investigate how these changes affect migrants through looking at their subjective financial situation.

In order to show how migrants respond when their countries are experiencing more natural disasters, I use the following flexible specification:

$$Y_{ist} = \alpha + topXC_{its}\beta + N_{it}\gamma + W_{st}\sigma + \eta_t\delta + \eta_i\tau + \epsilon_{ist} \tag{3.1}$$

where Y_{ist} is an outcome of interest for migrant i from source country s and interviewed at date t; $topXC_{its}$ is a natural disasters dummy variable that I further describe below; N_{it} is a set of individual characteristics of migrant i at time t; W_{st} is a set of characteristics of the source country at time t; η_i is a migrant fixed effect and η_t is a set of year dummies.

For this analysis, I assume that migrants decide how much to remit annually based on ex-ante expected levels of disasters in their source countries. These expectations are based on past natural disasters, i.e. the baseline incidence of natural disasters over a reference period (the reference period is discussed below). To fund these remittances, they make ex-ante choices with regards to economic variables such as labour supply or savings. If their expectations differ from contemporaneous shocks, migrants may readjust how much they remit ex-post and modify other economic variables accordingly to fund remittances. To identify this phenomenon, I compare contemporaneous disasters to past disasters using disasters dummies. $topXC_{its}$ is a dummy equal to 1 if contemporaneous disasters experienced by a migrant i from source country s between his interview at date t and 12 months before are above the X% of a baseline distribution of past disasters of this variable over a reference period. The intuition behind this method is that the higher the value of X, the more likely that migrants formed expectations that are below contemporaneous natural disasters. As a consequence, the higher the value of X, the more important the adjustments ex-post.

Besides enabling me to capture the impact of current natural disasters relative to past disasters through dummies, this flexible specification also has three main advantages. First, it allows me not to impose any assumptions on the structural form of the relationship between natural disasters and remittances. Second, it enables me to clearly determine whether disasters of all magnitudes affect remittances and, if not, above which threshold natural disasters affect migrants' behaviours. For instance, one can think that small disasters may be easier to handle for local populations without the need for extra help from abroad. Third, the dummies enable me to compare the thresholds from which natural disasters affect remittances and transmission mechanisms are similar.

Y_{ist} corresponds either to the remitting status, labour outcomes, monthly savings, the probability not to do sport or migrants' subjective financial situation. N_{it} includes migrants' age, education, number of children, marital status and net income. It is important to condition on age since older migrants may have experienced more natural disasters and may find it easier to anticipate shocks. Likewise, education may affect anticipations. Family factors like the number of children and marital status can also play a role in how migrants respond. So, controlling for the number of children captures the fact that migrants' share of income available for remittances purposes may depend on the composition of the family. Moreover, when a couple is married, individuals may pool money and only one spouse may send money. Finally, richer migrants may be more likely to remit so I condition on net income[3].

W_{st} contains the log of the GDP per capita in the source country and the log of the exchange rate. Countries with higher levels of GDP tend to be more diversified and recover better from disasters. Moreover, variations in productivity among countries are driving factors of remittances. Since disasters can affect the GDP, destroy capital and affect productivity, I control for the log of the GDP per capita. Conditioning on the exchange rate enables me to take into account the fact that migrants tend to think of amounts in local money when they remit and natural disasters can affect it.

Some unobservables both at the country and the individual levels may be cor-

[3]All regressions include net income except labour regressions

related to past disasters and affect migrants' responses. For instance, countries with higher natural disasters and higher variations may also be those whose migrants have the lowest level of education. This may affect the way they anticipate disasters and, subsequently, how they react to change in past disasters. Finally, some behavioural characteristics or migrants' taste may affect their expectations on disasters. For instance, people may read more general news and subsequently be exposed to more information on future disasters. So, their anticipations may be based on a richer set of information. As a consequence, they may react less to current disasters since they formed better expectations. To remove unobservables that are fixed over time, all regressions include migrant fixed effects.

3.4 Data description

To examine how migrants respond to natural disasters in their country of origin, I compile a dataset from 3 different sources: the Understanding Society Survey (USS), the World Bank indicators (WBI) and the Emergency Events Database (EM-DAT).

3.4.1 Survey data and economic indicators

The USS tracks about 2,400 migrants from developing countries for 6 waves over the period 2009-2015. Migrants from developing countries included in the survey are those with the highest stocks of migrants in the United Kingdom: Bangladesh, China, Ghana, India, Jamaica, Kenya, Nigeria, Pakistan, South Africa, Sri Lanka, Turkey and Uganda. The dataset contains information on individual characteristics including labour market, leisure and savings decisions as well as remitting behaviour. The WBI dataset provides information on country macroeconomic indicators such as GDP, GDP per capita and exchange rate. Table 3.1 shows descriptive statistics of the variables of the USS and the WBI [4].

Table 3.1: Descriptive statistics (all genders, all waves)

Variable	Mean	Std. Dev.	Min	Max
Remit (=1)[a]	0.28	n/a	0	1
Weekly Hours of work	28.72	15.30	0	97
Employed (=1[b])	0.88	n/a	0	1
2ndJob (=1)	0.035	n/a	0	1
Hourly Wage	5.29	10.43	0	60
Monthly Savings	85.44	441.35	0	10000
Financial (=1)[c]	0.51	n/a	0	1
Nosports (=1)[d]	0.54	n/a	0	1
Children	0.48	0.95	0	8
Married (=1)	0.63	n/a	0	1
Monthly net income	1277.61	1251	0	15000
Age	44.51	15.95	16	101
Degree (=1)	0.43	n/a	0	1
ln (GDP pc)	8.02	1.34	6.32	11.12
ln (Exchange rate)	3.21	2.01	-0.36	7.86
N=3754				

[a] *Remit* is a dummy variable equal to 1 if a migrant remitted in the last 12 months.
[b] *Employed* refers to all migrants in the labour force
[c] *Financial* is a dummy equal to 1 if a migrant declares being just about getting by financially or having difficulties. It is based on a variable containing integers from 1 to 5 corresponding to migrants' subjective financial situation. Migrants responding 1 declare *Living comfortably*, 2 *Doing alright*, 3 *Just about getting by*, 4 *Finding it quite difficult* and 5 *Finding it very difficult.*
[d] *Nosports* is a dummy variable equal to 1 if migrants reported doing no sport in the past 12 months.

From Table 3.1, we observe that almost 28% of the migrants remitted in the past 12 months[5]. Migrants work on average almost 29 hours per week, 88% are employed and 3.5% have a second job. The average hourly wage is about £5.29 and the net monthly income is £1277. Individuals report saving, on average, more than £85 monthly. 54% of the migrants did not do sport in the last 12 months, they

[4]See Table A1 and Table A2 from Appendix A for descriptive statistics by gender.
[5]Using the Fourth National Survey of Ethnic Minorities in the UK, Clark and Drinkwater (2007) find that on average 23.65% remit. Their cross-section survey data was collected between 1993 and 1994 and as discussed in the introduction trends in remittances have been increasing over the past decades.

have on average 0.48 child, 63% are married, the average age is 44 years old and less than half of them have a degree. 51% of the migrants report just about getting by financially or facing financial difficulties. Finally, the log of the GDP per capita of the home country is 8.02, the log of the GDP in the home country is 26.65 and the log of the exchange rate 3.21.

3.4.2 Natural disasters

Information on natural disasters is provided by the EM-DAT, which is hosted by the Centre for Research on the Epidemiology of Disasters (CRED) of the Universit Catholique de Louvain (UCL) in Brussels. It encompasses information on natural disasters, including the location and the exact date at which they occurred. It includes reliable daily information on the human impact of disasters such as the number of people affected by disasters between 2000 and 2015 worldwide. This dataset has the advantage of providing a clear measure of the number of people affected by natural disasters in source countries. For a disaster to be recorded in the data, it must satisfy one of following criteria: 10 or more people dead; 100 or more people affected; declaration of a state of emergency; call for international assistance. The types of disasters recorded over the period of this study include: drought, earthquake, epidemic, extreme temperature, flood, landslide and storm. Table 3.2 shows the distribution of the types of natural disasters for my sample.

Table 3.2: Descriptive statistics on disaster types

Disaster type	Freq.	Percent
Drought	17	3.26
Earthquake	55	10.56
Epidemic	36	6.91
Extreme temperature	21	4.03
Flood	205	39.35
Landslide	43	8.25
Storm	144	27.64
Total	521	100.00

From Table 3.2, we observe that there were 521 natural disasters in the 12 countries included in the survey data between 2009 and 2015. This corresponds to a yearly average number of disasters of 7.2 in each country. Floods are the main type of disaster and they amount for almost 40%. Storms are also a major type of disaster and correspond to 27% of the sample. Then, by order of frequency, we observe: earthquake(10.5%), landslide (8.25%), epidemic (6.91%), extreme temperature (4.03%) and drought (3.20%).

To capture the intensity of natural disasters, I create a variable that measures the proportion of the population affected by natural disasters in the country of origin of each migrant within 12 months before his interview. To do so, I first sum the number of people affected by natural disasters for the 12 months before the date of the interview of a migrant. For instance, if during this period of time there were 2 natural disasters, with respectively 20,000 and 5,000 people affected, the sum of the number of people affected over the period of interest would be 25,000. Then, I divide this value by the population in their source country in 2010[6]. Finally, I multiply this variable by 100 to get the percentage of people affected by natural disasters in the source country of a migrant 12 months before his interview, i.e. $POPAFFEC_{survey}$.

Table 3.3 shows summary statistics of $POPAFFEC_{survey}$ by country. We observe that, on average, 2.21% of the population in migrants' home countries were affected by natural disasters within 12 months before the interview of migrants, with a standard deviation of 3.37. Figures show important disparities both within and between countries. For instance, in China disasters are high and 7.56% of people are affected on average by disasters before migrants' interviews. There is a

[6] I obtain the information on population stocks from the World Bank indicators dataset. Since disasters variables are constructed at the daily level but population is at the yearly level, to avoid the fact that variations in the population from one year to another jeopardise the ratio, I need to use a fixed value of population. For instance, one could wrongly overestimate the ratio if there were deaths or migrations due to disasters in the previous year (at t-1) and the ratio is based on the current year t. One may also argue that the population may vary across years and that this will also make estimates less precise when fixing the value of the population. Results using yearly values for population do not change and are available upon request.

high variance in natural disasters and the standard deviation is greater than 5%. As discussed in the identification strategy section, I assume that migrants form expectations about future disasters. If disasters are higher than expected, they may adjust their behaviour. A high variance in disasters makes it harder for migrants to anticipate disasters correctly and, subsequently, they may be more likely to adjust their behaviour. To see whether migrants adjust ex-post, it is then crucial to have high within-country variation.

Table 3.3: Percentage of the population affected by natural disasters within 12 months before the interview of each migrant*

Country	Mean	Std. Dev.	Min	Max
All	2.21	3.37	0	19.87
Bangladesh	1.57	1.36	0	4.71
China	7.56	5.08	0	19.87
Ghana	0.24	0.24	0	0.66
India	0.75	0.39	0	1.55
Jamaica	1.28	2.90	0	8.02
Kenya	3.74	4.26	0	11.19
Nigeria	0.95	1.57	0	4.44
Pakistan	3.86	4.37	0	12.04
Sri Lanka	6.05	3.04	0	14.55
South Africa	0.13	0.16	0	0.41
Turkey	0.02	0.02	0	0.06
Uganda	0.74	0.99	0	3.41

*$POPAFFECTED_{survey}$

There are several advantages in using the accumulation of disasters over 12 months. First, this time span provides enough variation with regards to disasters both within and between countries. Second, it ensures that the results are not driven by seasonality. Third, some transmission mechanisms such as labour market responses may not be immediate and migrants need time to adjust.

As discussed previously, this paper assumes that migrants form their expectations of future disasters based on past disasters. Since the distribution of natural disasters may vary over time, a migrant may form different expectations of future disasters at different points in time i.e. interviews. As a consequence, for each migrant, I construct a distribution of past disasters before each of his interviews. To construct these migrant-specific distributions, I need information at the daily level on all natural disasters that occurred before their interview and over a period of time from which they may form their expectations. Recall that $POPAFFEC_{survey}$ corresponds to the percentage of the population affected by natural disasters within 12 months before the interview at date t of a migrant m from source country s. So, I need to compare natural disasters that took place within 12 months before the interview of a migrant to a distribution of natural disasters based on what happened within 12 months before each data point over a period of reference.

To do so, for each country, I create the percentage of the population affected by disasters in the past 12 months before each calendar date i.e. day of the disasters data. I name this variable $POPAFFEC_{all}$. It is analogous to $POPAFFEC_{survey}$, except that it is created for each day of the time span of the disasters data and not just before the interviews of the migrants. I then create time-and- migrant-specific distributions of this variable over a period of reference starting from 12 months before the interview and going back in time. It enables me to compare natural disasters that occurred within 12 months before the interview of a migrant to what migrants experienced during this time period of reference.

Figure 3.1 shows the distribution of $POPAFFEC_{all}$ by country. We observe

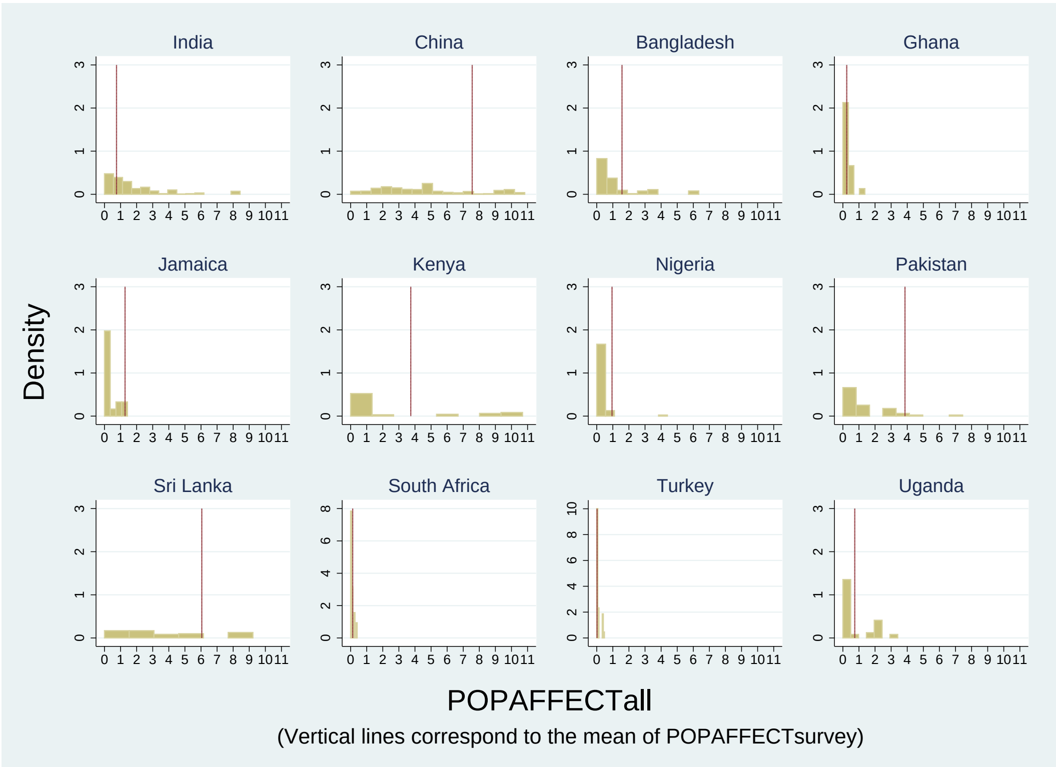


Figure 3.1: Distribution of the population affected by natural disasters 12 months before each data point of the administrative data by country ($POPAFFEC_{all}$)

that some countries have important variations in natural disasters and $POPAFFEC_{all}$ takes a wide range of values like in China or in India. On the contrary, other countries do not face many natural disasters and values of $POPAFFEC_{all}$ are always low like in South Africa. Interestingly enough, for some countries, disasters values are very different when we compare figures based on the administrative data and those based on the survey sample. So, although a migrant can experience high exposure to natural disasters before his interview compared to other migrants from the sample and originating from the same country, this exposure can be relatively low compared to what the population from this country usually experiences. For instance, in India, the average of $POPAFFEC_{survey}$ is low (0.75%) with a low standard deviation (0.39) and a maximum value of 1.55 (see Table 3.3). So, at first glance, a migrant with a value of $POPAFFEC_{survey}$ of 1.55% seems to face big natural disasters. However, when we look at Figure 3.1, $POPAFFEC_{all}$ can go beyond 8%. So, if one was just looking at the distribution of disasters before the interviews of the migrants from the sample i.e. $POPAFFEC_{survey}$, he could wrongly assume that 1.55% of the population affected by natural disasters is an extremely high value whereas, in reality, it is not as extraordinary for India as we can see from Figure 3.1. This confirms that it is essential to compare the values of $POPAFFEC_{survey}$ based on migrants' dates of interviews with an exogenous distribution based on the administrative data. To see further graphical evidence of how the distribution of $POPAFFEC_{survey}$ and $POPAFFEC_{all}$ may vary, vertical lines from Figure 1 on the histograms depict the average of $POPAFFEC_{survey}$ for each country based on my sample.

To obtain migrant-and-time-specific distributions, I construct a distribution of $POPAFFEC_{all}$ based on the disasters data between 12 months and the past 9 years before the interview of each migrant. In other words, I attach to each migrant a distribution of $POPAFFEC_{all}$ that is specific to his source country using a rolling window of 8 years starting from one year before his interview. Since $POPAFFEC_{survey}$

is a measure of disasters over 12 months before the interview, it is essential to start building migrant-specific distributions before this period of time in order for the distribution not to be affected by disasters captured by $POPAFFEC_{survey}$. Also, I use this time span because it is the longest period of time that the data allow me to compute[7]. The main advantage of using the longest time span is that it gives me as many as possible data points to create the richest distribution.

Finally, I create a set of dummies based on these distributions. The identification strategy relies on these dummies. For each migrant, at each interview, I create the following set of disasters dummies: $topX_{its}$. More precisely, $topX_{its}$ is a dummy variable equal to 1 if migrants' value of $POPAFFEC_{survey}$ is greater than X% of the disasters measure in his reference distributions[8] (i stands for migrants, t for time of the interview and s for source country). For instance, $top70_{its}$ is equal to 1 if a migrant i experienced disasters in the past 12 months in his source country s whose magnitude is greater than 70% of those that occurred within 12 months before each date of the disasters data in his country between 1 year and 9 years before his interview at time t.

As one may expect, the distribution of $POPAFFEC_{survey}$ based on migrants' interview in the survey sample and the distribution of $POPAFFEC_{all}$ based on daily observations from the whole disasters data are slightly different. So, the distribution from migrant-specific dummies based on the survey sample slightly differs from migrant-and-time-specific distributions computed with the whole disasters data. Table 4 shows summary statistics of migrant-specific dummies.

[7]Regressions results based on a 7 year-rolling window are included in Section 3.9.1. Regressions with other shorter distributions are available upon request but results do not change qualitatively when shortening distributions.

[8]Recall that distributions are based on $POPAFFEC_{all}$

Table 3.4: Distribution of dummies based on migrant specific distributions

Variable	Mean	Std. Dev.	Min	Max
top10C (=1)	0.88	0.33	0	1
top20C (=1)	0.80	0.40	0	1
top30C (=1)	0.67	0.47	0	1
top40C (=1)	0.60	0.49	0	1
top50C (=1)	0.48	0.50	0	1
top60C (=1)	0.37	0.48	0	1
top70C (=1)	0.28	0.45	0	1
top80C (=1)	0.16	0.37	0	1
top90C (=1)	0.10	0.30	0	1

For instance, we observe that 28% of the individuals experienced shocks greater than 70% of the natural disasters in their individual and time specific distributions computed with the whole disasters data. 16% of the individuals faced shocks greater than 80% and 10% had shocks greater than 90%. For more information on disasters distributions, check Tables B.4, B.5 and B.6 from the Appendix B to see how dummies' cutoffs vary over time for the three countries of the sample with most migrants (Bangladesh, India and Pakistan).

3.5 Results

Notice that disasters dummies are non-exclusive so all regressions include one dummy. As a consequence, in all regression tables, each combination between a line and a column corresponds to a regression. Recall that all regressions include controls for migrants' age, their education, the number of children they have, their marital status, the log of the GDP per capita in the source country, the log of the GDP in the source country and the log of the exchange rate. All regressions except labour regressions also contain monthly net income. Moreover, all regressions include year fixed effects and individual fixed effects. In the USS data, remittances are only included in wave 1 and wave 4. As a consequence, in all regressions, I use wave 1 and wave 4 of the survey in order to have a sample that contains most outcome variables I use. The only outcome variable that is not included in wave 1 is savings. As a consequence, savings regressions are based on waves 2, 4 and 6.

3.5.1 Remittances

Table 3.5 shows regression results of the impact of natural disasters on the probability to remit. All regressions are estimated through Ordinary Least Squares (OLS) and include year and individual fixed effects. Results show evidence that migrants are more likely to remit when natural disasters hit their home countries. Column (1) shows regression results for all migrants, column (2) for all men and column (3) for all women. Columns (4), (5) and (6) show regression results for migrants in the labour force. Column (4) corresponds to all migrants in the labour force, column (5) only for men in the labour force and column (6) for women in the labour force.

Not surprisingly, we notice that disasters need to be of a certain magnitude to modify migrants' remitting behaviour. So, if we look at disasters lying above the bottom 10%, 20%, 30% and 40% of the distribution, we do not observe any effect of natural disasters on the probability of remitting. The latter is true no matter which subsample, i.e. column we are analysing (Column (1) to column (6)). Two phenomena may explain these results. First, since natural disasters on average are not very high, natural disasters in expectation may be low and, as a consequence, migrants do not modify much their ex-ante behaviours. So, they may be more likely to adjust ex-post behaviours when disasters are of high magnitude. Another possibility is that migrants who anticipated higher levels of disasters than actual shocks just increase consumption. Due to the limitations of the data with regards to consumption, it is hard to test this hypothesis. When natural disasters are falling in the top 50% of migrants' distributions, the latter become more likely to remit. If we take into account the whole sample, migrants become 5% more likely to start remitting in response to a shock in the top 50% of the distribution (see column

Table 3.5: Impact of disasters on the probability to remit

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Remit* = 1						
top10C	0.0323	0.0763	0.0791	.06344	0.106	0.0208
	(0.0353)	(0.0681)	(0.0604)	(0.0702)	(0.0822)	(0.0220)
top20C	0.0448	0.0327	0.0604	0.0430	0.0148	0.0557
	(0.0285)	(0.0423)	(0.0400)	(0.0404)	(0.0544)	(0.0692)
top30C	-0.0108	0.00539	-0.0262	-0.0190	0.0337	-0.0466
	(0.0271)	(0.0402)	(0.0379)	(0.0411)	(0.0562)	(0.0731)
top40C	-0.000594	0.0198	-0.0182	-0.00958	0.0394	-0.0932
	(0.0282)	(0.0411)	(0.0397)	(0.0422)	(0.0551)	(0.0698)
top50C	0.0508*	0.0869**	0.0150	0.0486	0.105**	-0.0530
	(0.0284)	(0.0409)	(0.0395)	(0.0413)	(0.0519)	(0.0644)
top60C	0.0720**	0.0931*	0.0493	0.0672	0.130**	-0.0600
	(0.0343)	(0.0497)	(0.0478)	(0.0484)	(0.0627)	(0.0768)
top70C	0.0379	0.122**	-0.0307	0.0411	0.176**	-.0460
	(0.0393)	(0.0568)	(0.0541)	(0.0543)	(0.0728)	(0.0621)
top80C	0.0376	0.0920	-0.0126	0.0690	0.201**	-0.0433
	(0.0473)	(0.0659)	(0.0672)	(0.0664)	(0.0884)	(0.110)
top90C	-0.0194	0.0775	-0.108	0.0193	0.158	0.366
	(0.0579)	(0.0775)	(0.0786)	(0.0867)	(0.107)	(0.530)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	2,306	1,108	1,198	1,194	734	458
Migrants	1,153	554	599	597	367	229

Linear probability model with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

(1)). Likewise, disasters above the bottom 60% of the distribution are affecting positively migrants' remitting behaviour but the magnitude is slightly greater and goes up to 7.2%. These are important variations considering that 28% of migrants from my sample remit on average. When we look at disasters above the bottom 70%, the effect of natural disasters on remittances is positive but not significant anymore. Moreover, it is twice smaller in magnitude. Likewise, when we look at the impact of natural disasters when the latter fall above the bottom 80%, the coefficient on top80C has similar magnitude as the one on top70C and is positive but not significant. Finally, the coefficient becomes negative although not significant when disasters are above the bottom 90% of migrants' distributions.

Two main phenomena are responsible for this. First, disasters of high magnitude are less frequent. As a consequence, standard errors tend to be higher and lead to less precise estimates. Second, there is some heterogeneity in the response to natural disasters across gender. More specifically, if we look at column (2) and column (3) that represent respectively men and women, we observe that only men respond to natural disasters. So, men who experienced disasters above the bottom 50% are 8.68% more likely to remit. When disasters fall above the bottom 60% of the distribution of natural disasters, men are 9.31% more likely to remit. The coefficient is even bigger when disasters are in the above the bottom 70% of the distribution and reaches 12.2%. Coefficients are still positive but not significant when looking at the top 20% and 10% of the distribution. However, for the latter, standard errors go up and estimates are less precise. Unlike for men, the coefficients are never significant for women and become negative when disasters are above the bottom 70% of the distribution. As a consequence, although the coefficient on men is positive for instance on top70C (column (2)), the aggregate effect is much smaller and not significant (column (1)).

The fact that women do not adjust their behaviour may be subject to two main interpretations: either women anticipate shocks perfectly and smooth remittances or they just do not send more money in response to disasters. Another possibility is that women already send money for other purposes and the fact they might increase the intensive margin is not captured in regressions. Another interpretation is that households may be pooling resources and men may be in charge of sending remittances. So women could adjust to increase remittances but since they send through men, we do not observe any effect. The next section on transmission mechanisms shows scant evidence that women respond to natural disasters. One of the explanations for this to happen is that women are more constrained by children. Due to the small sample size, it is hard to test this hypothesis and analyse the behaviour of women with no children. Finally, one may think that, since women have lower net incomes on average, they have less margins on which to respond.

Other characteristics may make migrants more likely to remit in response to disasters. For instance, migrants in the labour force may be able to adjust one more variable i.e. labour supply in order to fund remittances compared to pensioners for instance. As a consequence, it may then be easier for them to start remitting when in the aftermath of natural disasters. The next section on transmission mechanisms will investigate whether migrants in the labour force adjust labour supply. Before doing so, it is worth looking at whether migrants in the labour supply are more likely to start remitting in response to natural disasters. Columns (4), (5) and (6) from Table 3.5 underline the impact of natural disasters on migrants in the labour force. Once again we see that only men are likely to change remitting behaviour in the wake of natural disasters when the latter are in the top 50% of disasters or above. There is a positive effect at the aggregate level but not significant and a negative effect for women, which is not significant either. Once again, the positive and significant effect on men is offset by the one on women, which makes coefficients shrink at the aggregate level and become insignificant despite smaller standard errors. When we look at men in column (5), we observe that when natural disasters in men's home countries are above the 50% of the disasters they experienced in the past, they are

10% more likely to remit. The impact is bigger for disasters lying above the bottom 60%, 70%, 80% and 90% of the distribution. It is respectively 13%, 17.6%, 20.1% and 15.8%. All these coefficients are significant except the one on top90C. However, the sample size for individuals experiencing natural disasters in the top 10% is much smaller and standard errors are almost twice bigger than for disasters larger than the 50th percentile of the reference period (10.7 versus 5.19).

When we compare the magnitude of the coefficients between all men and only men in the labour force (column (2) and column (5)), we observe that the magnitude is higher for men in the labour force. So, they seem more responsive to disasters. When we look at coefficients from top50C to top90C, the coefficient for men is systematically lower than for men in the labour force. From top50C to top90C we get respectively 8.69% versus 10.5%; 9.31% versus 13%; 12.2% versus 17.6%, 9.2% versus 20.1% and 7.75% versus 15.8%. As discussed, this may be because men in the labour force are more able to adjust their income stream.

Notice that migrants may already be remitting for other reasons. So, although they may increase the amount they send, this is not captured in regressions. So, these results can be considered as a lower bound of the impact of disasters on the probability to remit for this purpose. The rest of the result section will give some insights on the intensity of remittances. It will also shed light on the types of economic adjustments that migrants may make in the wake of natural disasters to fund remittances. The empirical challenge to tackle is to show evidence of whether modifications in migrants' behaviours reflect changes in the intensive margin of remittances. In order to do so, it will first show that migrants increase labour supply and decrease savings. Then, to support the fact that these changes are not caused by an increase in consumption, I will show that migrants consume less leisure and report worse financial situations.

3.5.2 Labour supply

As described in the previous section, migrants in the labour force appear to be more reactive to natural disasters in their countries of origin. Labour supply is then a potential transmission mechanism that would explain why these migrants are more reactive to disasters. Figure 3.2 shows nonparametric regressions of the variation in migrants' hours of work and the variation in the percentage of people affected by disasters in their home countries 12 months before their interview, i.e. $POPAFFEC_{survey}$. The first graph that includes all migrants shows a slight positive correlation between migrants' variation in weekly hours of work and the variation in the percentage of people affected by natural disasters in their home countries. When we look at men, we observe a clear positive correlation between these two variables. Unlike for men, there is no clear relationship for women.

Table 3.6, shows OLS regressions of the impact of natural disasters on labour supply. Columns (1), (2) and (3) show the impact of natural disasters on the extensive margin. In line with previous findings, it seems that only men react to natural disasters. The fact that only men are both more likely to remit and to be more employed in the wake of natural disasters suggests that they use labour income to fund remittances. Moreover, like in the previous section, only disasters of high magnitude give rise to a response from male migrants. From column (2), we observe that when male migrants face disasters that are in the bottom 70% of their distribution, they do not adjust the extensive margin of labour supply. However, when they face disasters above the top 70% of their distribution, they are 7.71% more likely to work. The figure increases when disasters lie in the top 80% and it makes migrants 9.8% more likely to work. These results underline that male migrants that were unemployed are more likely to be employed in the wake of natural disasters. An interpretation of this phenomenon is that they increase search efforts or are more likely to accept job offers they would not accept in the absence of natural disasters of high magnitudes. The coefficient on top90C is also positive and equal to the one on

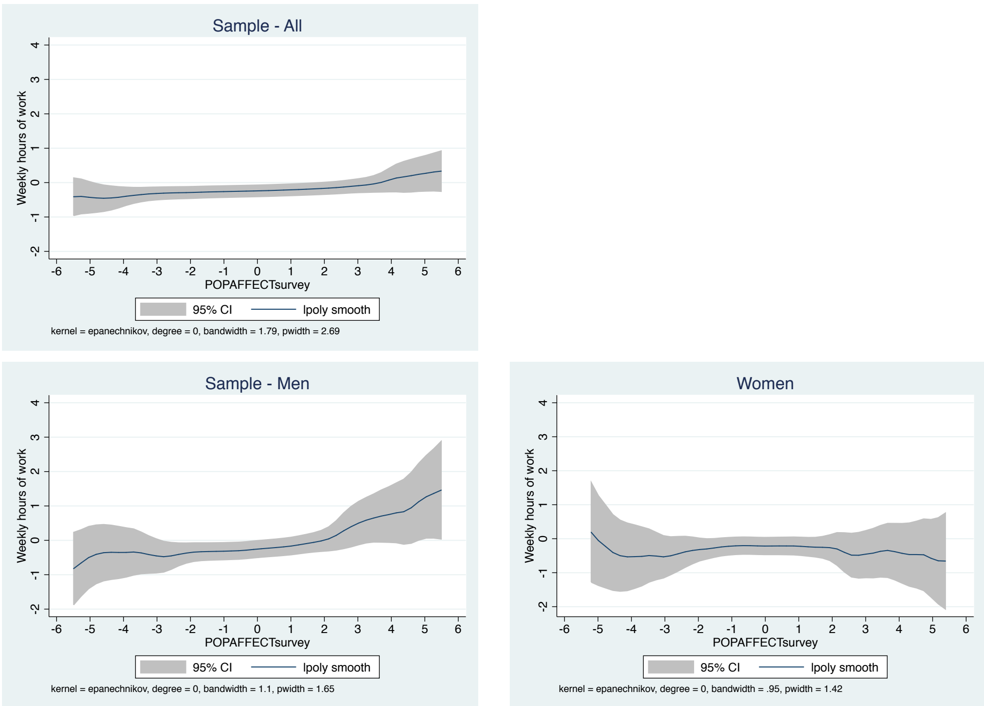


Figure 3.2: Impact of the variation in natural disasters ($POPAFFEC_{survey}$) on the variation in weekly hours of work

Note: Local linear regressions and corresponding 95 per cent confidence interval using the rule-of-thumb bandwidth.

top70C but not significant. Once again, this may be due to the fact that there may be a few observations in the top 10% and we observe that standard errors inflate. In line with previous findings on remittances, women do not adjust their extensive margin in the wake of natural disasters.

When we look at the intensive margin, the pattern is similar but male migrants adjust labour supply from the top 50% of disasters in the reference distribution, which is similar to what we observed for remittances. So, men experiencing disasters in the top 50% of their reference distribution work on average 1.9 hours more weekly. Figures go up to more than 4 hours for men whose countries were hit by disasters in the top 80% or 90% of their reference distributions. Considering that the average hourly wage is around £5.29, this implies an increase in income by approximately £40 for disasters in the top 50% and by more than £80 monthly for disasters lying in the top 10% of their reference distribution.

One may wonder how migrants manage to increase hours of work. Indeed, although some jobs may offer the possibility to do extra hours, not all jobs offer this flexibility. Moreover, some jobs may not pay for extra hours. Although, it would be of interest to understand whether migrants do work extra hours at their primary jobs and get more money, the limitations of the data do not enable me to test this hypothesis. Another way for migrants to increase hours of work is to get a second job. The first 3 columns of Table 3.7 highlight the impact of natural disasters on the probability to have a second job. Columns (1), (2) and (3) represent respectively the impact of natural disasters on the probability of having a second job for the whole sample, men and women. All regressions are estimated through OLS. When we look at the whole sample (column (1)), we observe no impact of natural disasters on the probability to get a second job. However, when we only look at men (column (2)), we observe that when disasters are of important magnitude, migrants are more likely to have a second job. So, when disasters are above the bottom 60% of disasters in their reference distributions, male migrants are 4.76% more likely to get a second job.

Table 3.6: Impact of disasters on labour supply

	Employment			Weekly Hours of work		
	(1)	(2)	(3)	(4)	(5)	(6)
top10C	0.0176	0.0225	0.0124	1.232	1.285	2.019
	(0.0267)	(0.0364)	(0.0372)	(1.115)	(1.592)	(1.445)
top20C	0.00775	-0.00410	0.0341	1.108	-0.0205	1.3302
	(0.0216)	(0.0296)	(0.0306)	(0.838)	(1.333)	(0.809)
top30C	0.0127	0.0214	0.0207	1.164	-0.196	1.794
	(0.0226)	(0.0296)	(0.0360)	(0.829)	(1.211)	(1.298)
top40C	0.0195	0.0249	0.0307	1.024	0.798	1.496
	(0.0220)	(0.0292)	(0.0326)	(0.769)	(0.965)	(1.199)
top50C	0.0268	0.0460	0.00388	1.190	1.908*	0.403
	(0.0223)	(0.0292)	(0.0334)	(0 .791)	(1.100)	(1.225)
top60C	0.0143	0.0346	-0.0256	0.420	1.559	-1.052
	(0.0239)	(0.0319)	(0.0348)	(0.903)	(1.267)	(1.259)
top70C	0.0264	0.0771**	-0.0729	0.534	3.198**	2.354
	(0.0329)	(0.0391)	(0.0484)	(1.151)	(1.614)	(1.552)
top80C	0.0481	0.0986**	-0.0495	1.226	4.602***	-2.207
	(0.0378)	(0.0482)	(0.0567)	(1.325)	(1.767)	(1.900)
top90C	0.0619	0.0771	0.00152	1.333	4.062**	-2.203
	(0.0387)	(0.0560)	(0.0468)	(1.323)	(1.948)	(1.533)
Sample	All	Men	Women	All	Men	Women
Observations	1.796	1,084	712	1.796	1,084	712
Migrants	898	542	356	898	542	356

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country, and the log of the exchange rate.

Figures are higher for migrants who experienced disasters above the top 70%, 80% and 90% with an increase in the probability to get a job by respectively 7.11%, 16% and 7.80%. Women do not seem to be more likely to get a second job in response to natural disasters. This is in line with previous findings that showed that only men were adjusting labour supply.

To sum up, there is evidence that male migrants increase labour supply in the wake of natural disasters. First, results show that unemployed male migrants are more likely to start working. This can be interpreted as an increase in their search efforts or a willingness to accept job offers they would reject in the absence of natural disasters. Moreover, male migrants already in employment increase hours of work through getting a second job. Both of these phenomena explain the increase in male working hours. Men might also increase through extra hours but the limitations of the data do not enable me to test for this hypothesis. An important potential phenomenon that could jeopardise these results is the fact that disasters could give rise to an increase in migrant inflows. This could affect wages negatively and migrants could then increase labour supply just to keep constant levels of consumption.

Figure 3.3 shows nonparametric regressions of the impact of disasters on wages. They express whether variations in the population affected by disasters implies changes in wages. It shows no clear pattern of a relationship between natural disasters and wages.

Columns (4), (5) and (6) of Table 3.7 show Poisson regressions[9] of the impact of natural disasters on hourly wages. Please notice that due to missing values, the sample size substantially goes down so results should be interpreted cautiously. Regression results do not show any impact of natural disasters on wages.

[9] I use Poisson regressions due to the Poisson shape of the data that contains many 0s. Results using an Inverse Hyperbolic Sine (IRS) function are similar qualitatively and presented in Table B.20 from Appendix B.

Table 3.7: Impact of disasters on labour supply

	2ndJob			Hourly Wage		
	(1)	(2)	(3)	(4)	(5)	(6)
top10C	0.0336	0.0413	-0.0176	-0.105	0.0121	-0.0634
	(0.0261)	(0.0297)	(0.0286)	(0.0745)	(0.143)	(0.0661)
top20C	0.00965	0.0404	-0.0163	-0.110	-0.0769	-0.0663
	(0.0161)	(0.0258)	(0.0175)	(0.0703)	(0.113)	(0.0716)
top30C	0.0151	0.0242	0.00249	-0.0882	-0.0107	-0.0382
	(0.0175)	(0.0232)	(0.0250)	(0.0840)	(0.131)	(0.149)
top40C	0.00333	0.0206	-0.00790	-0.0937	0.000452	-0.201
	(0.0168)	(0.0207)	(0.0287)	(0.0819)	(0.109)	(0.211)
top50C	0.00865	0.0104	0.0192	0.00629	-0.0452	0.00412
	(0.0175)	(0.0237)	(0.0264)	(0.0439)	(0.0722)	(0.0775)
top60C	0.0260	0.0476*	0.0220	-0.00867	-0.137	-0.386
	(0.0208)	(0.0274)	(0.0344)	(0.0368)	(0.113)	(0.288)
top70C	0.0342	0.0711*	0.0260	-0.232	-0.146	-0.426
	(0.0266)	(0.0370)	(0.0441)	(0.167)	(0.166)	(0.345)
top80C	0.0524	0.160***	-0.0125	-0.101	-0.0379	-0.298
	(0.0350)	(0.0528)	(0.0488)	(0.201)	(0.185)	(0.344)
top90C	0.0219	0.0780**	0.00714	-0.153	0.0639	-0.265
	(0.0262)	(0.0394)	(0.0496)	(0.167)	(0.106)	(0.382)
Sample	All	Men	Women	All	Men	Women
Observations	1516	832	684	264	144	120
Migrants	758	416	342	132	72	60

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

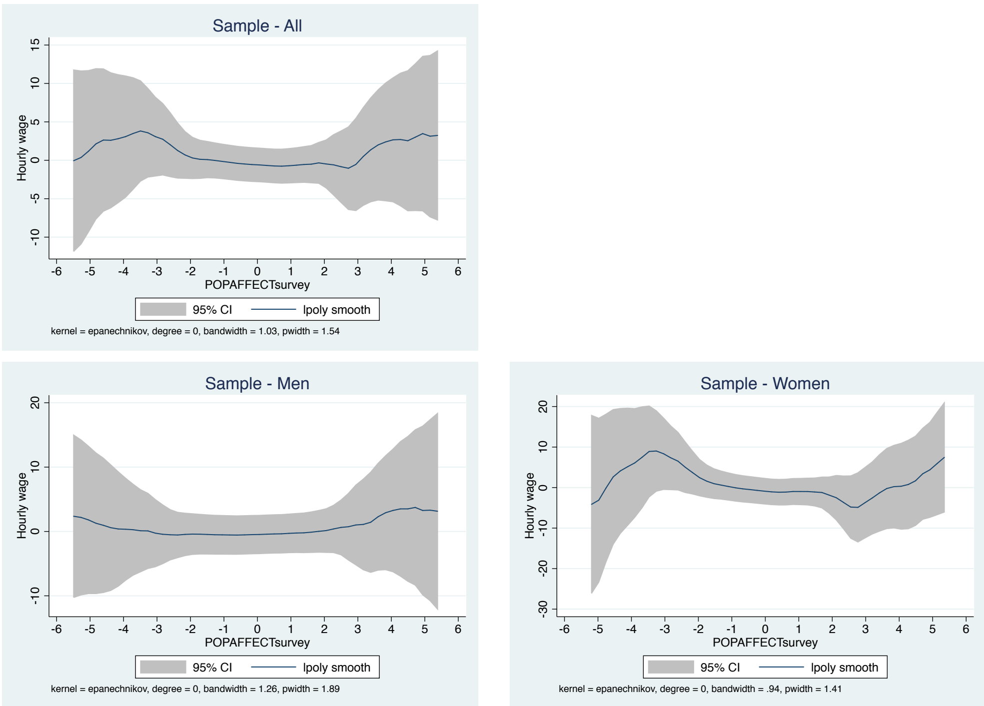


Figure 3.3: Impact of the variation in natural disasters ($POPAFFEC_{survey}$) on the variation in hourly wages

Note: Local linear regressions and corresponding 95 per cent confidence interval using the rule-of-thumb bandwidth.

3.5.3 Savings

Not all migrants may be able to increase labour supply in order to fund remittances. Moreover, some migrants may manage to do so but not as much as they would need to fully fund remittances. For instance, a migrant may be able to do extra hours but not as much as he needs to cover his increase in remittances. As a consequence, migrants may need to exploit other resources to fund remittances in the wake of natural disasters. Adjusting savings may be another way for them to do so. Figure 3.4 emphasises that an increase in the population affected by disasters leads to a decrease in savings. This is true for both men and women. When we look at the whole sample, there is a clear decrease around 3%. Interestingly enough, natural disasters start affecting savings when the latter are affecting more than 3% of the population. This is in line with previous findings with regards to hours of work.

Table 3.8 shows Poisson regressions with individual fixed effects on how natural disasters affect savings [10]. Since the survey data I use does not contain information on savings in the same waves as for remittances and labour, I use waves 2, 4 and 6, which are the waves available for savings. Column (1), (2) and (3) correspond to regression results based on the whole sample. Once again, it seems that disasters only affect savings when the former are above a certain threshold. We observe that migrants start decreasing monthly savings when they experienced shocks lying in the top 50% of their reference distributions or above. Below this threshold, natural disasters do not significantly affect savings.

When we look at the whole sample (column (1)), we observe that migrants save 41% less when the later experienced shocks in the top 50% of their reference distributions. Considering that the average monthly savings are £85, this corresponds to a decrease in savings by almost £35. The coefficients on dummies top60C

[10] Once again, I use Poisson regressions due to the Poisson shape of the data that contains many 0s. Results using an Inverse Hyperbolic Sine (IRS) function are similar qualitatively and presented in Table B.21 from Appendix B.

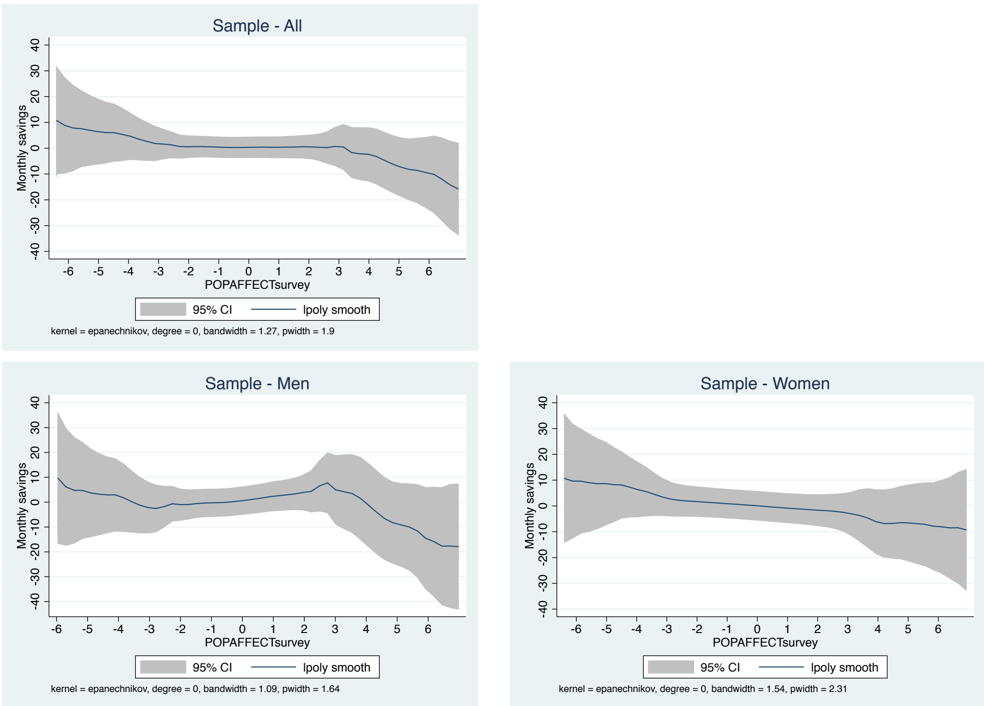


Figure 3.4: Impact of the variation in natural disasters ($POPAFFEC_{survey}$) on the variation in monthly savings

Note: Local linear regressions and corresponding 95 per cent confidence interval using the rule-of-thumb bandwidth.

to top90C are all negative and significant except the one on top70C, which is not significant. If we focus on men (column (2)), we observe the same pattern and male migrants only respond to natural disasters when they are above the bottom 50% of their reference distributions or above. All the coefficients on dummies top50C to top90C are negative and coefficients increase as the magnitude of disasters increases. For instance, male migrants who experienced disasters lying in the top 50% of their reference distributions save 30.4% less monthly, which corresponds to almost a £32 decrease in monthly savings. Figures go up to 71.1% for disasters in the top 10%, which corresponds to a decrease in savings by slightly less than £75. These monetary decreases in savings are of the same magnitude as the increases in labour income for men (£40 for disasters in the top 50% and £80 for disasters in the top 90%). Like for men, women start savings less when disasters are above the bottom 50%. Coefficients on disasters dummy variables are all negative from top50C to top90C. However, only the ones on top60C and top90C are significant. So, although previous results underlined no evidence that women were adjusting behaviour in the wake of natural disasters, there is weak evidence that women respond to natural disasters. Thus, unlike for remittances and labour supply results, there is no clear evidence that men are driving the results.

Column (4), (5) and (6) show regression results for migrants in the labour force. When we look at both male and female together (column (4)), we observe that disasters start affecting migrants when they are larger than the 50th percentile of the reference period. The pattern is however less clear than when looking at the whole sample. So, only coefficients on top50C, top60C and top90C are significant. For instance, migrants who experienced shocks in the top 50% of their reference distributions save 42% less monthly. Like for remittances and labour outcome variables exposed previously, results show that men are driving the results. Indeed, men in the labour force do decrease savings unlike women. The former start remitting more when disasters are above the bottom 40% of their reference distributions and all dummies above (from top50C to top90C) are negative and significant. Interestingly, the magnitude of the coefficients is similar to those when looking at men with all types of occupations. So, results do not show evidence that male migrants in the labour force increase labour supply in order to keep constant levels of monthly savings.

Table 3.8: Impact of disasters on monthly savings

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Savings*						
top10C	0.136	-0.174	0.403	0.212	0.0724	-0.0108
	(0.191)	(0.142)	(0.252)	(0.199)	(0.184)	(0.166)
top20C	-0.0683	-0.0168	-0.0157	-0.0257	0.0763	0.0368
	(0.153)	(0.173)	(0.211)	(0.161)	(0.266)	(0.121)
top30C	-0.0156	-0.208	0.344	-0.0126	-0.196	-0.0261
	(0.163)	(0.166)	(0.275)	(0.164)	(0.177)	(0.107)
top40C	-0.0631	-0.479	0.463	-0.0390	-0.465*	-0.000113
	(0.201)	(0.234)	(0.358)	(0.222)	(0.245)	(0.101)
top50C	-0.409**	-0.304**	-0.499	-0.418*	-0.451***	-0.0272
	(0.201)	(0.148)	(0.260)	(0.215)	(0.173)	(0.0965)
top60C	-0.417**	-0.301*	-0.508**	-0.409*	-0.488***	-0.109
	(0.206)	(0.165)	(0.241)	(0.221)	(0.180)	(0.104)
top70C	-0.178	-0.331	-0.0673	-0.0154	-0.333*	-0.113
	(0.164)	(0.205)	(0.243)	(0.152)	(0.195)	(0.110)
top80C	-0.414**	-0.439**	-0.369	-0.257	-0.434**	-0.134
	(0.187)	(0.205)	(0.300)	(0.192)	(0.196)	(0.126)
top90C	-0.796***	-0.711**	-0.899***	-0.609**	-0.536*	-0.286
	(0.234)	(0.319)	(0.343)	(0.239)	(0.314)	(0.176)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	1,746	846	896	1,181	673	506
Migrants	747	366	379	510	295	214

Poisson regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

To sum up, results show that in the wake of natural disasters, migrants are more likely to increase labour supply and save less. As exposed previously, men that face disasters in the top 50% of their reference distributions increase monthly labour earnings by almost £40. Moreover, they decrease monthly savings by £32. This can be interpreted as a potential increase in remittances by £72 monthly. If we look at men who experienced disasters in the top 10% of their reference distributions, we observe that they increase monthly labour earnings by £80 and decrease savings by almost the same amount. This corresponds to a potential increase in remittances by £155. To put these amounts into perspective, it is worth looking at average amounts of remittances that migrants living in the UK send. In 2015, the Greenback project commissioned by the World Bank carried out a survey of 602 Bangladeshi, Ghanaian and Romanian remitters in London. They observe that Romanians sent the largest amounts of money back home among men in the survey with more than £290 monthly on average. Then they find Bangladeshis remit on average almost £210 per month and Somali and Ghanaians approximately £125 monthly.

3.5.4 Leisure consumption

To bring more evidence that these changes reflect an increase in the intensive margin of remittances, it is important to show that disasters also affect migrants' consumption. This paper looks at migrants' leisure consumption.

Table 3.9 shows evidence that migrants decrease leisure. More specifically, it underlines that migrants are more likely to report no sport when they face important disasters in their country of origin. A decrease in leisure can be either interpreted as a decrease in money available for leisure due to an increase in remittances or a decrease in time available due to increasing labour supply. Column (1), (2) and (3) correspond to regression results based on migrants with all types of occupations. Column (2) shows that men are more likely to report doing no sport in the past 12 months when facing natural disasters lying in the top 70% of their reference distributions or above. So, men are 5.29% more likely not to do sport when natural disasters are in the top 70% of their distribution. Figures go up to almost 12% when disasters are in the top 90%. Interestingly enough, once again women seem not to adjust.

Column (4), (5) and (6) show regression results for migrants in the labour force. Only men who experienced disasters in the top 90% are less likely to do sport. These results suggest that male migrants in the labour force are more likely to do sport in the wake of natural disasters compared to male migrants from the whole sample. Since male migrants in the labour force increase labour income in the aftermath of natural disasters and are those that are the less likely to stop doing sport, this underlines that the decrease in leisure consumption is more due to financial reasons than time allocation issues. However, this hypothesis has to be taken cautiously for two reasons. First, coefficients on top70C, top80C and top90C for male migrants in the labour force are of the same magnitude as those for male migrants from the whole sample. More specifically, coefficients on top70C, top80C and top90C are respectively 5.29, 7.54 and 11.9 for all male migrants and 3.72, 8.86 and 14.4 for male migrants in the labour force. Moreover, the sample size for male migrants in the labour force is smaller than the one for all male migrants and standard errors are bigger, which may explain the lack of significance of the coefficients on top70C and top80C for male migrants in the labour force. So, it is then impossible to draw any tangible conclusions when we compare results obtained with all men or just men in the labour force.

Table 3.9: Impact of disasters on the probability not to do any sport in the past 12 months

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Nosports* = 1						
top10C	-0.00549	-0.00950	0.00189	0.00192	0.0125	-0.00672
	(0.0243)	(0.0381)	(0.0317)	(0.0352)	(0.0480)	(0.0519)
top20C	-0.0169	-0.00384	-0.0239	-0.0245	-0.00839	-0.0433
	(0.0192)	(0.0295)	(0.0252)	(0.0283)	(0.0343)	(0.0410)
top30C	-0.00616	0.00394	-0.00835	-0.00679	-0.00727	0.00426
	(0.0181)	(0.0277)	(0.0238)	(0.0263)	(0.0365)	(0.0405)
top40C	-0.00266	0.0175	-0.00949	-0.0231	0.0267	-0.0422
	(0.0199)	(0.0302)	(0.0264)	(0.0284)	(0.0716)	(0.0457)
top50C	-0.00628	0.00996	-0.0108	-0.0193	-0.000202	-0.0387
	(0.0173)	(0.0249)	(0.0238)	(0.0249)	(0.0305)	(0.0428)
top60C	-0.000780	0.0174	-0.00788	-0.0133	0.00640	-0.0477
	(0.0190)	(0.0266)	(0.0267)	(0.0280)	(0.0332)	(0.0507)
top70C	0.00614	0.0529*	-0.0219	-0.00648	0.0372	-0.0863
	(0.0228)	(0.0315)	(0.0327)	(0.0324)	(0.0372)	(0.0587)
top80C	0.0252	0.0754*	-0.00130	0.0697	0.0886	0.0424
	(0.0320)	(0.0454)	(0.0453)	(0.0457)	(0.0543)	(0.0797)
top90C	0.0380	0.119**	-0.0170	0.0874*	0.144**	-0.00438
	(0.0338)	(0.0512)	(0.0448)	(0.0472)	(0.0607)	(0.0766)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	3,754	1722	2,022	1,922	1,170	750
Migrants	1,877	861	1,011	961	585	375

Linear probability model with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

3.5.5 Financial situation

Previous results emphasise that migrants adjust behaviour in the aftermath of natural disasters. More specifically, results show that migrants increase labour supply, save less and consume less leisure such as sports. The underlying assumption that may explain these modifications in migrants' behaviour is that they increase remittances to compensate losses caused by natural disasters in their home countries. One way to show further evidence that migrants respond to natural disasters and are affected by them is to investigate their financial situation. Natural disasters can affect migrants' financial situations in two main ways: either because they increase remittances or because they have assets in their home country that were damaged.

Although it is not easy to disentangle the two phenomena, by looking at the subjective reported financial situation across different groups of migrants, this paper will show that migrants who respond to natural disasters are also those reporting the worst financial situations. Answering this question will also shed light on whether modifications in migrants' behaviours enable them to fully fund remittances and maintain a similar subjective financial situation. For instance, one might think that migrants increase labour supply and maintain the same levels of consumption. So, although natural disasters might affect their behaviour, increasing labour supply enables them to fund remittances and to maintain similar financial situations. Table 3.10 shows the impact of natural disasters on migrants' subjective financial difficulties. Recall that Financial is a dummy equal to 1 if migrants report to be just about getting by financially or facing financial difficulties and 0 otherwise.

Column (1) corresponds to regression results based on the whole sample. Migrants report worse financial situations when they face natural disasters from the top 30% of their reference distributions. Moreover, coefficients are all positive and statistically significant from top60C and migrants are more than 5% more likely to report difficult financial situations when facing natural disasters in the top 60% of their reference distribution. Interestingly enough, when we compare results for men (column (2)) and women (column (3)), we observe that these results are driven by men and we notice that natural disasters do not seem to affect women's subjective financial situations. Recall that male migrants are the only ones that are more likely to remit, to increase labour supply and decrease leisure consumption in the wake of natural disasters. Moreover, they also decrease savings to a similar extent to female migrants. Conditional on the fact that male and female migrants have the same probability to have assets that were affected by natural disasters, the fact that male migrants report worse financial situation compared to women suggests that increasing remittances negatively affects their financial situations.

By comparing regressions results between males from the whole sample (column (2)) and males from the labour force only (column (5)), one can observe how adjusting labour supply can help migrants increase remittances without being too affected by it financially. Results show that coefficients on top40C and top50C are only significant for the sample with all men. This may be due to the fact that increasing labour income helps male migrants in the labour force to fully fund remittances when damages are not too important. However, when disasters are of higher magnitude, increasing labour supply may not be sufficient to fully compensate the increase in remittances since the potential earnings from labour supply may be smaller compared to losses. However, male migrants from the labour force seem to be more likely to report worse financial situations when disasters are very high, i.e. that lie in the top 80% or 90% of their distributions. It might be due to the fact that male migrants in the labour force are those that are the most expected to help.

Table 3.10: Impact of disasters on migrants' subjective financial difficulties

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Financial =1*						
top10C	0.0197	0.00736	0.0337	0.00831	0.0139	0.00965
	(0.0304)	(0.0444)	(0.0417)	(0.0412)	(0.0549)	(0.0626)
top20C	0.00517	0.0188	-0.00413	-0.00946	-0.00468	-0.00862
	(0.0250)	(0.0352)	(0.0354)	(0.0330)	(0.0444)	(0.0501)
top30C	0.0333*	0.0433	0.0242	0.0100	0.0171	-0.0111
	(0.0193)	(0.0272)	(0.0272)	(0.0271)	(0.0352)	(0.0434)
top40C	0.0190	0.0517*	-0.00896	0.00452	0.0330	-0.0429
	(0.0189)	(0.0268)	(0.0266)	(0.0264)	(0.0343)	(0.0419)
top50C	0.0281	0.0476*	0.0118	0.0159	0.0451	-0.0309
	(0.0193)	(0.0276)	(0.0269)	(0.0273)	(0.0345)	(0.0465)
top60C	0.0516**	0.0827***	0.0288	0.0316	0.0868**	-0.0429
	(0.0206)	(0.0307)	(0.0281)	(0.0292)	(0.0380)	(0.0460)
top70C	0.0612**	0.101***	0.0345	0.0407	0.122***	-0.0760
	(0.0246)	(0.0382)	(0.0323)	(0.0358)	(0.0461)	(0.0572)
top80C	0.0653**	0.0746*	0.0585	0.0430	0.114**	-0.0543
	(0.0269)	(0.0397)	(0.0367)	(0.0373)	(0.0480)	(0.0603)
top90C	0.0696**	0.0552	0.0769*	0.0903**	0.116**	0.0371
	(0.0320)	(0.0492)	(0.0421)	(0.0433)	(0.0552)	(0.0714)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	4,428	2,050	2,376	2,272	1,394	876
Migrants	2,214	1,025	1,188	1,136	697	438

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

3.6 Robustness checks

3.6.1 Fixed distributions

Regressions in the body of the paper rely on individual-specific disasters dummies. As a robustness check, I create dummies that are still country-specific but not migrant-specific. So, it enables me to compare current disasters, i.e. $POPAFFEC_{survey}$

to a fixed country-level distribution. To create these country-specific distributions, the method is slightly different than for individual-specific distribution. I use the distribution of $POPAFFEC_{all}$ based on administrative data between 12 months and the past 9 years before the start of the survey data, which is the longest time period I have in my data.

Dummies based on fixed country distributions, enable me to compare individuals from the same country with regards to the same benchmark. Notice that fixing the distribution implies that we assume that migrants are not updating their beliefs. Another drawback of this method though is that fixing the distribution implies that for different individuals, we compare current events with respect to a distribution whose start can be between 1 year and 5 years before current disasters.

Table B.7 from Appendix B shows the descriptive statistics of these dummies. Regression results using a fixed distribution with these dummies are shown in Appendix B.2 and are qualitatively similar but with coefficients that tend to be more significant.

3.6.2 Further robustness checks

In the main regression, I use dummies based on a distribution starting one year before the interview. I provide another robustness check and construct dummies on distributions starting two years before migrants' interviews. This idea is that migrants may be affected by cumulative disasters. For instance, if disasters were very important between 12 months and 24 months before the date of their interview, they may have debts and the way they react to disasters within 12 months before their interview may be different than if they were no disasters in the past. Results do not show evidence of such a phenomenon. Due to data constraints, this implies that the window is over 7 years instead of 8. Results are in line with previous results and do not change qualitatively when using this window. Tables are shown in Appendix B.3.

3.7 Conclusion

This paper shows evidence that male migrants respond to natural disasters in their country of origin. They appear to be more likely to remit when disasters hit their home countries. To understand how migrants may manage to fund remittances in the wake of natural disasters, this study investigates 3 main channels: labour supply, savings and leisure consumption. Interestingly enough, in most transmissions mechanisms that this paper explores, only men adjust behaviour. This is in line with the fact that only male migrants are likely to change remitting status. Another interesting feature of the results is that migrants only respond to natural disasters when the latter are of high magnitude, i.e. in the top 50% of migrants' distributions. One of the reasons to explain this is that migrants may anticipate disasters and make ex-ante adjustments in order to smooth remittances. Since they are less likely to anticipate extreme disasters they may not have enough money to fund remittances, hence the need to adjust ex-post. Moreover, since natural disasters on average are not very high, natural disasters in expectation may be low and, as a consequence, migrants do not modify much their ex-ante behaviours. So, they may be more likely to adjust ex-post behaviours when disasters are of high magnitude. Another possibility is that migrants who anticipated higher levels of disasters than actual shocks just increase consumption. Due to the limitations of the data with regards to consumption, it is hard to test this hypothesis.

First, this paper looks at how migrants may adjust labour supply in order to fund remittances. Results show that male migrants increase both the extensive and intensive margins of labour supply. For instance, when men face disasters in the top 50% of their reference distributions, they work on average 1.9 more hours weekly, which corresponds to an average £40 monthly increase in labour earnings. If we look at men who experienced disasters in the top 10% of their reference distributions, we observe that they work on average 4 hours more weekly for an increase in monthly labour earnings by £80. Male migrants in the labour force may increase hours of work through 3 main channels. First, some male migrants in the labour force may switch from unemployment to employment. Second, male migrants that already have a job may get a second job. Third, male migrants may do extra hours at their job. This paper shows that male migrants are more likely to be employed when natural disasters are above the bottom 70% of migrants' reference distributions. Moreover, this study underlines that male migrants are more likely to get a second job when natural disasters are above the bottom 60% of their reference distributions. These results suggest that UK labour markets are flexible enough to enable them to adapt. The limitations of the data do not allow me to test whether migrants increase extra hours.

Not all migrants may be able to adjust labour supply in order to fund remittances. Another channel through which migrants may manage to increase remittances is savings. Regression results underline that migrants decrease monthly savings in the aftermath of natural disasters. So, when men face disasters in the top 50% of their reference distribution, they decrease monthly savings by £32. If we look at men who experienced disasters in the top 10% of their reference distribution, we observe that they decrease savings by £75. If we sum up the decrease in savings and the increase in labour supply earnings, this corresponds to a potential increase in remittances by £72 for disasters in the top 50% and £155 for disasters in the top 10%. To put these amounts into perspective, based on my own calculations using the same disasters database, people affected by natural disasters in the countries I focus on over the same time period faced an average economic loss of £300 monthly. Money potentially available due to changes in migrants' behaviours may be even bigger since migrants report having less leisure like sport activities. However, the data do not have information on how much migrants spend on sport activities so I cannot quantify by how much they can potentially increase remittances by stopping doing sport. Not surprisingly, they also declare worse financial situations.

The fact that migrants are able to react to specific shocks means that there is potential for increasing remittances and that policymakers in the United Kingdom should keep on facilitating remittance transactions. Recent discussions in the UK parliament to reform the UK remittance market to make it more competitive are in line with this. One could also consider different financial incentives such as tax rebates for migrants sending remittances when their countries are hit by natural disasters. Also, the fact that migrants rely on labour markets and increase labour supply to fund remittances in the wake of natural disasters underlines that labour market reforms to migrants' access to the labour market could boost remittances.

Conclusion

Using microeconometrics methods, this thesis analyses three vulnerable groups of people that have recently become a policy priority in Europe and that are now at the top of the policy agenda. More specifically, it sheds light on the behaviour of prisoners, victims of domestic abuse and migrants. Results emphasise important factors that policymakers should consider while designing policies to support these three vulnerable groups.

The first chapter of this thesis highlights the driving factors of partnership decisions of former inmates for re-offending purposes. Exploiting the 2006 prison pardon in Italy that released important groups of inmates, it shows that nationality, age and residual sentence are key determinants of peer group formation. Mafia criminals are more likely to partner up, and more generally career criminals, are more likely to partner up. As for matching across crime types, there is some evidence of complementarities, and possibly learning across crime types. With respect to crime types we observe that career criminals are likely to influence inmates who committed crimes that are not associated with a criminal career, like crimes committed against the family (violence against children, incest, etc.), or crimes committed against the order (disorderly conduct, public drunkenness, etc.).

These results have practical implications for policy purposes and can enable policymakers to design more accurate prevention policies based on the characteristics of the prisoners that were released with any of the other inmates who are still in custody. For instance, if two inmates are released on the same day from the same prison, authorities should be concerned if they have the same nationality or similar ages. Also, since career criminals are likely to influence inmates who committed crimes that are not associated with a criminal career, the allocation of prisoners into cells could be optimised based on these results.

The second chapter of this thesis studies the impact of a policy to reduce domestic abuse in Essex. This policy informs high-risk suspects that they will be put under higher surveillance and contact their victims and encourage them to report. Using an RDD setup, results show evidence that suspects of domestic abuse that are targeted by the policy are 9% more likely to be reported again for domestic abuse events within 1 month. The effect of the policy on reporting is driven by events of domestic abuse for which the police could not establish any criminal charges. More precisely, results show a 10% increase in the probability that suspects targeted by the policy are reported again for events of domestic abuse that did not lead to criminal charges. However, there is no evidence that the policy influences the reporting of crime. This may be due to the fact that either the policy has no impact on both the deterrence of the suspects and on the reporting of crime or that these effects compensate each other.

Although the fact that the policy increased reporting in the short term is positive, results emphasise that the long-term impact of the policy depends on the actual response of the police in the wake of the reporting. Results show that offenders are also more likely to be reported again in the second month, conditional on being reported within one month after the policy was put in place. These results imply that, on average, the policy does not deter crime in the long run. However, results also show that victims keep on reporting and, subsequently, that reporting has a gain for them. When looking at the impact of being reported in the wake of the policy for different levels of violence, one can observe that reporting may only deter crime in the long run if it leads to criminal charges. On the contrary, the reporting of events that does not lead to criminal charges appears to increase observed recidivism.

These results aim to contribute to the elaboration of cost-effective prevention policies. Currently, there are several approaches to prevent domestic abuse that have been developed by policy, the justice system, social workers and NGOs. For instance, social workers engage in teaching to promote respectful and nonviolent relationships through individual, community, and social levels changes. NGOs create protective environments, shelters and try to strengthen economic support for families. Many of these methods are expensive. For example, in the UK, annual funding to provide core support for victims and other accommodation-based services, rape support centres and national helplines exceed £80 million (Strickland and Allen, 2017). Increasing reporting through building trust with the police is a relatively cheap measure to implement that can increase early reporting. By demonstrating empirically and quantifying how an increase in the trust in the police can increase early reporting, this paper contributes to the elaboration of cost-effective prevention policies.

The third chapter of this thesis shows evidence that male migrants living in the UK and originating from developing countries respond to natural disasters in their country of origin. By combining a household panel survey of migrants in the UK and natural disasters data, I find that male migrants are more likely to remit when disasters hit their home countries. To fund remittances in the wake of natural disasters, I investigates three main channels: labour supply, savings and leisure consumption. Results show that male migrants increase both the extensive and intensive margins of labour supply. For instance, when men face disasters in the top 50% of their reference distributions , on average they work 1.9 hours more per week, which corresponds to a £40 increase in labour earnings per month. Results also underline that migrants decrease monthly savings in the aftermath of natural disasters. So, when men face disasters in the top 50% of their reference distribution, they decrease monthly savings by £32.

To put these amounts into perspective, based on my own calculations using the same disasters database, people affected by natural disasters in the countries I focus on over the same time period faced an average economic loss of £300 monthly. Money potentially available due to changes in migrants' behaviours may be even bigger since migrants report having less leisure like sport activities. Not surprisingly, they also declare worse financial situations. The fact that migrants are able to react to specific shocks means that there is potential for increasing remittances and that policymakers in the UK should keep on facilitating remittance transactions. Recent discussions in the UK parliament to reform the UK remittance market to make it more competitive are in line with this. One could also think of different financial incentives such as tax rebates for migrants sending remittances when their countries are hit by natural disasters. Also, the fact that migrants increase labour supply to fund remittances in the wake of natural disasters underlines that labour market reforms to ease migrants' access to the labour market could boost remittances.

Appendix A

Chapter 2

Table A.1: Link between the risk score and the baseline control variables (OLS regressions, different bandwidth)

	(1) Female	(2) White	(3) Age	(4) Female	(5) White	(6) Age
Treatment	0.0346	0.0215	-1.487**	0.0109	0.00842	-0.553
	(0.0195)	(0.0161)	(0.526)	(0.0186)	(0.0128)	(0.395)
Bandwidth	[-10;10]	[-10;10]	[-10;10]	[-30;30]	[-30;30]	[-30;30]
Observations	33,102	33,102	33,102	167,809	167,809	167,809
R-squared	0.001	0.004	0.002	0.001	0.004	0.002

OLS regressions. Robust standard errors clustered at the risk score level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: risk score, interaction between the risk score and the treatment dummy, age, female and white.

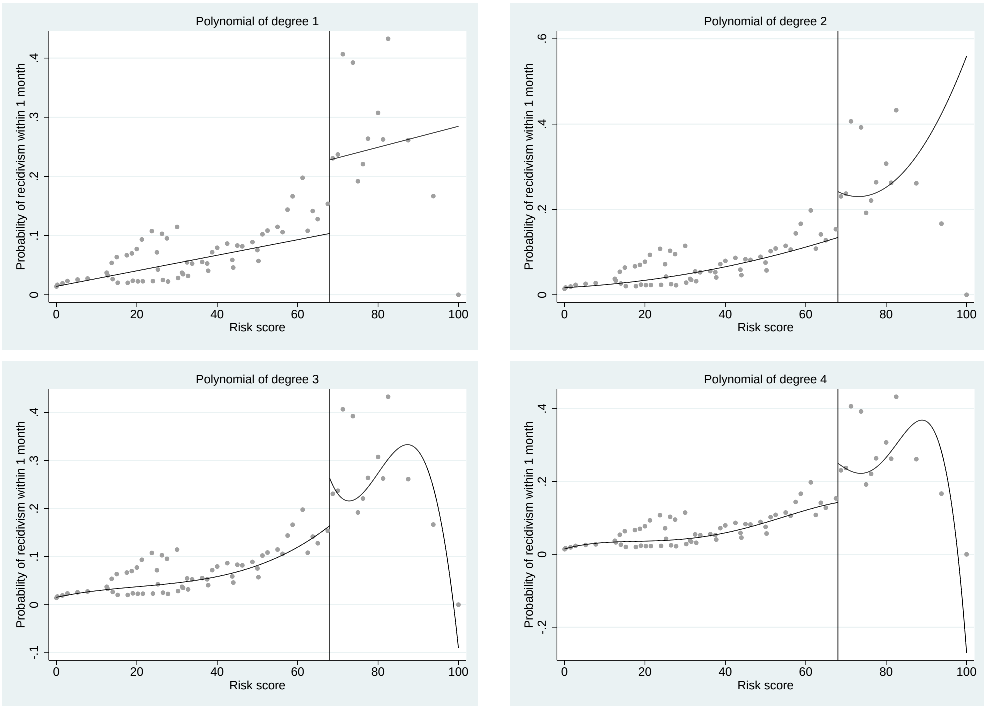


Figure A.1: Impact of the risk score on recidivism within 1 month (All data)

Table A.2: Summary statistics (All data)

Variable	**Mean**	**Std. Dev.**	**N**
Recidivism	0.056	n/a	1636905
Offence	0.020	n/a	1636905
Incident	0.036	n/a	1636905
Age	35.444	12.678	1636905
Female	0.258	n/a	1783966
White	0.825	n/a	1783966
RecencyScore	10.089	21.362	1464987
GravityScore	21.117	35.289	1464987
FrequencyScore	16.043	34.797	1464987
RiskScore	11.812	16.643	1464987
RiskScoreAbove	0.007	0.085	1464987

Appendix B

Chapter 3

B.1 Further descriptive statistics

Table B.1: Descriptive statistics: Men

Variable	Mean	Std. Dev.	Min	Max
Remit (=1)[1]	0.31	n/a	0	1
Weekly Hours of work	30.67	15.66	0	97
Employed (=1)[2]	0.89	n/a	0	1
2ndJob (=1)	0.37	n/a	0	1
Hourly Wage	5.04	4.67	0	45
Monthly Savings	105.62	399.87	0	8000
Financial (=1)[3]	0.51	n/a	0	1
Nosports (=1)[4].	0.49	n/a	0	1
Children	0.02	0.17	0	3
Married	0.69	n/a	0	1
Net income	1519.76	1445.55	0	15000
Age	45.05	16.07	16	92
Degree (=1)	0.45	n/a	0	1
ln (GDP pc)	7.95	1.30	6.32	11.12
ln (GDP)	26.63	1.69	23.21	30.49
ln (Exchange rate)	3.30	1.93	-0.36	7.86

N=1722

[]

Table B.2: Descriptive statistics: women

Variable	Mean	Std. Dev.	Min	Max
Remit (=1)[5]	0.24	n/a	0	1
Weekly Hours of work	26.34	14.51	0	97
Employed (=1)[6]	0.88	n/a	0	1
2ndJob (=1)	0.34	n/a	0	1
Hourly wage	5.60	14.62	0	60
Monthly Savings	68.14	473.38	0	10000
Financial (=1)[7]	0.51	n/a	0	1
Nosports (=1)[8].	0.59	n/a	0	1
Children	0.88	1.15	0	8
Married (=1)	0.63	n/a	0	1
Net income	1056.07	991.25	0	15000
Age	44.02	15.82	16	101
Degree (=1)	0.41	n/a	0	1
ln (GDP pc)	8.10	1.38	6.32	11.12
ln (GDP)	26.66	1.78	23.21	30.49
ln (Exchange rate)	3.13	2.07	-0.36	7.86

N=2022

Table B.3: Disasters frequency by country

Country	Drought	Earthquake	Epidemic	Extreme Temperature	Flood	Landslide	Storm	Total
Bangladesh	1	0	0	4	10	3	16	34
China	7	38	0	3	67	22	79	216
Ghana	0	0	7	0	7	0	0	14
India	1	5	1	12	47	4	32	102
Jamaica	1	0	0	0	0	0	2	3
Kenya	3	0	6	0	14	1	0	24
Sri Lanka	2	0	2	0	15	3	3	25
Nigeria	0	0	9	0	11	0	2	22
Pakistan	0	6	1	2	20	4	2	35
Turkey	0	5	1	0	6	4	0	16
Uganda	1	0	9	0	3	2	1	16
South Africa	1	1	0	0	5	0	7	14
Total	17	55	36	21	205	43	144	521

Table B.4: Country level distribution of POPAFFECTED: Bangladesh

Variable	Mean	Std. Dev.	Min	Max
D10	0.22	n/a	0.15	0.22
D20	0.37	n/a	0.23	0.40
D30	0.43	n/a	0.36	0.48
D40	0.64	n/a	0.48	0.99
D50	0.89	n/a	0.65	1.09
D60	1.27	n/a	0.90	1.43
D70	2.59	n/a	1.24	2.97
D80	5.41	n/a	1.88	6.16
D90	19.33	n/a	6.12	23.76

Table B.5: Country level distribution of POPAFFECTED: India

Variable	Mean	Std. Dev.	Min	Max
D10	0.48	n/a	0.35	0.60
D20	0.59	n/a	0.37	0.66
D30	0.93	n/a	0.56	1.68
D40	1.26	n/a	0.79	2.33
D50	1.55	n/a	0.97	2.69
D60	1.87	n/a	1.04	2.93
D70	2.40	n/a	1.14	4.06
D80	3.05	n/a	1.36	5.87
D90	13.95	n/a	3.00	27.78

Table B.6: Country level distribution of POPAFFECTED: Pakistan

Variable	Mean	Std. Dev.	Min	Max
D10	0.01	n/a	0.00	0.04
D20	0.04	n/a	0.00	0.09
D30	0.15	n/a	0.01	0.22
D40	0.40	n/a	0.10	0.97
D50	0.96	n/a	0.22	2.97
D60	1.55	n/a	0.75	3.18
D70	2.01	n/a	0.84	3.18
D80	3.56	n/a	3.02	4.43
D90	8.02	n/a	4.12	11.98

Table B.7: Dummies based on country distributions

Variable	Mean	Std. Dev.	Min	Max
top10C (=1)	0.83	n/a	0	1
top20C (=1)	0.80	n/a	0	1
top30C (=1)	0.63	n/a	0	1
top40C (=1)	0.53	n/a	0	1
top50C (=1)	0.48	n/a	0	1
top60C (=1)	0.39	n/a	0	1
top70C (=1)	0.36	n/a	0	1
top80C (=1)	0.19	n/a	0	1
top90C (=1)	0.11	n/a	0	1

B.2 Robustness checks using a fixed distribution

This section includes regression results using country specific distributions. Results are in line with main results.

Table B.8: Impact of disasters on the probability to remit

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Remit* = 1						
top10C	0.0584	0.0509	0.0713	0.0634	0.0169	0.111
	(0.0373)	(0.0567)	(0.0506)	(0.0549)	(0.0732)	(0.0834)
top20C	0.0375	0.0206	0.0688	0.0337	-0.00733	0.0969
	(0.0321)	(0.0489)	(0.0428)	(0.0479)	(0.0613)	(0.0735)
top30C	0.00792	0.0138	0.0107	0.0269	0.0815	-0.0593
	(0.0330)	(0.0494)	(0.0449)	(0.0495)	(0.0653)	(0.0707)
top40C	-0.0103	-0.00236	-0.0277	-0.0158	0.0193	-0.104
	(0.0360)	(0.0567)	(0.0479)	(0.0518)	(0.0686)	(0.0818)
top50C	0.0273	0.0634	0.00721	0.0210	0.0718	-0.0938
	(0.0309)	(0.0446)	(0.0402)	(0.0457)	(0.0546)	(0.0724)
top60C	0.0611	0.0972*	0.0394	0.0570	0.134**	-0.0912
	(0.0398)	(0.0507)	(0.0523)	(0.0574)	(0.0615)	(0.0833)
top70C	0.0756*	0.127**	0.0212	0.0753	0.148**	-0.0960
	(0.0410)	(0.0539)	(0.0555)	(0.0559)	(0.0659)	(0.0836)
top80C	0.0357	0.0763	-0.0290	0.0667	0.178*	-0.154
	(0.0501)	(0.0719)	(0.0713)	(0.0708)	(0.0977)	(0.103)
top90C	0.0147	0.0662	-0.112	0.0662	0.143	-0.172
	(0.0601)	(0.0852)	(0.0851)	(0.0929)	(0.122)	(0.142)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	2.306	1,108	1,198	1,194	734	458
Migrants	1.153	554	599	597	367	229

Linear probability model with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.9: Impact of disasters on labour supply

	Employment			Weekly Hours of work		
	(1)	(2)	(3)	(4)	(5)	(6)
top10C	0.000767	-0.0307	0.0373	0.490	-1.017	2.149
	(0.0239)	(0.0327)	(0.0347)	(0.991)	(1.400)	(1.313)
top20C	0.00953	-0.00602	0.0336	0.791	-0.0640	1.935
	(0.0224)	(0.0305)	(0.0312)	(0.888)	(1.206)	(1.259)
top30C	0.0187	0.0206	0.0298	1.392	1.922	1.409
	(0.0306)	(0.0395)	(0.0497)	(1.076)	(1.364)	(1.724)
top40C	0.0268	0.0614	-0.0125	0.265	1.933	-1.392
	(0.0310)	(0.0424)	(0.0448)	(1.052)	(1.392)	(1.611)
top50C	0.0358	0.0631*	-0.0172	1.145	2.945**	-1.370
	(0.0255)	(0.0328)	(0.0397)	(0.916)	(1.269)	(1.421)
top60C	0.0421	0.0854**	-0.0395	1.238	3.821***	-2.296
	(0.0293)	(0.0373)	(0.0434)	(1.037)	(1.192)	(1.624)
top70C	0.0230	0.0627*	-0.0533	0.289	2.361*	-2.444
	(0.0316)	(0.0379)	(0.0491)	(1.223)	(1.413)	(1.824)
top80C	0.0550	0.0958**	-0.0404	1.147	3.862**	-1.836
	(0.0366)	(0.0484)	(0.0539)	(1.418)	(1.876)	(2.092)
top90C	0.0611*	0.0623	0.0181	1.158	3.807**	-2.118
	(0.0361)	(0.0536)	(0.0383)	(1.337)	(1.695)	(1.650)
Sample	All	Men	Women	All	Men	Women
Observations	1.796	1,084	712	1.796	1,084	712
Migrants	898	542	356	898	542	356

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.10: Impact of disasters on labour supply

	2ndJob			Hourly Wage		
	(1)	(2)	(3)	(4)	(5)	(6)
	(FE)	(FE)	(FE)	(FE)	(FE)	(FE)
VARIABLES	2ndJob	2ndJob	2ndJob	Wage	Wage	Wage
top10C	-0.00506	0.0244	0.0465	-0.0648	-0.0384	-0.0643
	(0.0355)	(0.0325)	(0.0619)	(0.0797)	(0.132)	(0.0594)
top20C	-0.0129	-0.0191	0.0624	-0.0563	0.0288	-0.171*
	(0.0333)	(0.0308)	(0.0634)	(0.0705)	(0.117)	(0.104)
top30C	0.0263	0.0142	0.0698	-0.0954	-0.0290	-0.107
	(0.0397)	(0.0524)	(0.0619)	(0.105)	(0.175)	(0.118)
top40C	0.101**	0.112*	0.0653	-0.113	0.0882	-0.0778
	(0.0494)	(0.0642)	(0.0735)	(0.132)	(0.144)	(0.116)
top50C	0.0666	0.0619	0.124*	-0.0449	0.0503	-0.113
	(0.0420)	(0.0504)	(0.0721)	(0.108)	(0.108)	(0.113)
top60C	0.115*	0.131	0.125*	-0.0446	-0.0701	-0.269
	(0.0596)	(0.0932)	(0.0673)	(0.122)	(0.132)	(0.177)
top70C	0.145**	0.172	0.118	-0.0363	-0.0326	-0.566
	(0.0672)	(0.107)	(0.0744)	(0.146)	(0.150)	(0.349)
top80C	-0.00673	0.0818	-0.0325	-0.0398	0.0131	-0.487
	(0.0593)	(0.0648)	(0.0819)	(0.164)	(0.158)	(0.299)
top90C	-0.0420	0.0738	-0.102	0.0699	0.145	-0.531
	(0.0626)	(0.0467)	(0.0876)	(0.118)	(0.0971)	(0.382)
Sample	All	Men	Women	All	Men	Women
Observations	1516	832	684	264	144	120
Migrants	758	416	342	132	72	60

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.11: Impact of disasters on monthly savings

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Savings*						
top10C	-0.247	-0.233	-0.0348	-0.286	-0.0701	-0.515
	(0.200)	(0.211)	(0.310)	(0.210)	(0.217)	(0.323)
top20C	-0.135	-0.232	0.225	-0.116	-0.104	-0.000648
	(0.174)	(0.181)	(0.286)	(0.184)	(0.187)	(0.316)
top30C	-0.432	-0.0594	-0.591	-0.478	-0.131	-0.560
	(0.306)	(0.237)	(0.358)	(0.320)	(0.246)	(0.368)
top40C	-0.140	-0.394*	0.283	-0.102	-0.416*	0.212
	(0.185)	(0.227)	(0.311)	(0.194)	(0.238)	(0.288)
top50C	-0.138	-0.368*	0.164	-0.0719	-0.355*	0.416
	(0.176)	(0.212)	(0.269)	(0.182)	(0.215)	(0.273)
top60C	-0.262*	-0.318*	-0.235	-0.187	-0.296*	-0.0430
	(0.137)	(0.163)	(0.238)	(0.142)	(0.161)	(0.256)
top70C	-0.315**	-0.339*	-0.236	-0.176	-0.326*	-0.0322
	(0.158)	(0.180)	(0.249)	(0.154)	(0.182)	(0.269)
top80C	-0.457***	-0.421**	-0.548*	-0.325*	-0.352*	-0.383
	(0.170)	(0.185)	(0.292)	(0.170)	(0.182)	(0.316)
top90C	-0.744***	-0.646*	-0.902**	-0.523**	-0.434	-0.703*
	(0.249)	(0.337)	(0.357)	(0.254)	(0.326)	(0.370)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	1,746	846	896	1,181	673	506
Migrants	747	366	379	510	295	214

Poisson regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.12: Impact of disasters on the probability not to do sport in the past 12 months

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Nosports* = 1						
top10C	0.0113	0.00595	0.0201	-0.0182	0.0159	-0.0104
	(0.0217)	(0.0310)	(0.0300)	(0.0381)	(0.0414)	(0.0498)
top20C	-0.00363	-0.00310	-0.0239	-0.0219	0.00247	-0.0441
	(0.0210)	(0.0286)	(0.0252)	(0.0310)	(0.0395)	(0.0485)
top30C	-0.0218	0.0159	-0.00835	-0.0395	-0.00325	-0.0843*
	(0.0221)	(0.0335)	(0.0238)	(0.0325)	(0.0419)	(0.0507)
top40C	-0.0336	0.0133	-0.0625*	-0.0570	-0.0235	-0.0865
	(0.0269)	(0.0408)	(0.0355)	(0.0378)	(0.0474)	(0.0625)
top50C	-0.0277	-0.00881	-0.0336	-0.0444	-0.0395	-0.0263
	(0.0222)	(0.0329)	(0.0298)	(0.0318)	(0.0394)	(0.0545)
top60C	-0.0156	0.00485	-0.0182	-0.0521	-0.0427	-0.0494
	(0.0278)	(0.0403)	(0.0381)	(0.0387)	(0.0455)	(0.0704)
top70C	0.00969	0.0498	-0.00761	0.00557	0.00222	0.0234
	(0.0269)	(0.0377)	(0.0385)	(0.0377)	(0.0437)	(0.0695)
top80C	0.00844	0.0483	-0.0108	0.0203	0.0346	0.00733
	(0.0296)	(0.0414)	(0.0426)	(0.0413)	(0.0494)	(0.0721)
top90C	0.0460	0.111**	0.00412	0.100**	0.141**	0.0374
	(0.0365)	(0.0559)	(0.0487)	(0.0510)	(0.0664)	(0.0830)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	3,754	1722	2,022	1,922	1,170	750
Migrants	1,877	861	1,011	961	585	375

Linear probability model with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.13: Impact of disasters on migrants' subjective financial difficulties

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Financial = 1*						
top10C	-0.000542	-0.0217	0.0218	-0.00569	-0.0265	0.0359
	(0.0296)	(0.0427)	(0.0410)	(0.0396)	(0.0529)	(0.0601)
top20C	0.00885	0.0113	0.00647	0.0115	0.00272	0.0311
	(0.0269)	(0.0388)	(0.0369)	(0.0362)	(0.0482)	(0.0546)
top30C	0.0520**	0.0547*	0.0487	0.0265	0.0276	0.0146
	(0.0224)	(0.0319)	(0.0312)	(0.0327)	(0.0409)	(0.0541)
top40C	0.0209	0.0661**	-0.0192	0.00721	0.0346	-0.0531
	(0.0214)	(0.0309)	(0.0298)	(0.0305)	(0.0385)	(0.0499)
top50C	0.0247	0.0700**	-0.0153	0.0247	0.0547	-0.0422
	(0.0217)	(0.0324)	(0.0293)	(0.0321)	(0.0406)	(0.0548)
top60C	0.0334	0.0820**	-0.00626	0.0148	0.0728	-0.0804
	(0.0251)	(0.0369)	(0.0341)	(0.0366)	(0.0476)	(0.0564)
top70C	0.0542**	0.0865**	0.0262	0.0187	0.0722	-0.0738
	(0.0252)	(0.0376)	(0.0339)	(0.0369)	(0.0470)	(0.0596)
top80C	0.0628**	0.0859**	0.0467	0.0471	0.115**	-0.0519
	(0.0260)	(0.0395)	(0.0346)	(0.0373)	(0.0473)	(0.0616)
top90C	0.0619*	0.0545	0.0632	0.0702	0.104*	0.00431
	(0.0330)	(0.0498)	(0.0439)	(0.0448)	(0.0570)	(0.0733)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	4,428	2,050	2,376	2,272	1,394	876
Migrants	2,214	1,025	1,188	1,136	697	438

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

B.3 Robustness checks using a shorter distribution

This section includes regressions with a different rolling window: over 7 years starting 2 years before the interview. Results don't change qualitatively.

Table B.14: Impact of disasters on the probability to remit

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Remit* = 1						
top10C	0.0201	0.0182	0.0177	0.0321	-0.00698	0.0866
	(0.0306)	(0.0460)	(0.0419)	(0.0406)	(0.0566)	(0.0600)
top20C	0.0288	0.0298	0.0291	0.0319	0.0167	0.0496
	(0.0285)	(0.0417)	(0.0405)	(0.0405)	(0.0547)	(0.0603)
top30C	-0.00721	0.000459	-0.0165	-0.0119	0.0366	-0.0888
	(0.0275)	(0.0412)	(0.0375)	(0.0422)	(0.0582)	(0.0631)
top40C	-0.000572	0.0104	-0.00911	-0.00957	0.0245	-0.0733
	(0.0285)	(0.0429)	(0.0390)	(0.0413)	(0.0545)	(0.0660)
top50C	0.0265	0.0682*	-0.0123	0.0279	0.0680	-0.0534
	(0.0260)	(0.0372)	(0.0366)	(0.0393)	(0.0503)	(0.0640)
top60C	0.0686**	0.108**	0.0321	0.0693	0.159***	-0.0764
	(0.0337)	(0.0465)	(0.0484)	(0.0478)	(0.0584)	(0.0757)
top70C	0.0605*	0.120**	0.00954	0.0455	0.156**	-0.0446
	(0.0365)	(0.0512)	(0.0517)	(0.0502)	(0.0655)	(0.0851)
top80C	0.0119	0.0444	-0.0185	0.0242	0.127*	-0.0798
	(0.0367)	(0.0545)	(0.0492)	(0.0552)	(0.0733)	(0.0870)
top90C	-0.0319	0.0690	-0.120	0.00598	0.158	-0.107
	(0.0593)	(0.0840)	(0.0780)	(0.0877)	(0.112)	(0.120)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	2.306	1,108	1,198	1,194	734	458
Migrants	1.153	554	599	597	367	229

Linear probability model with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home countryand the log of the exchange rate.

Table B.15: Impact of disasters on labour supply

	Employment			Weekly Hours of work		
	(1)	(2)	(3)	(4)	(5)	(6)
top10C	-0.000320	-0.00666	0.0174	0.186	-0.279	-0.297
	(0.0205)	(0.0288)	(0.0277)	(0.901)	(1.276)	(0.650)
top20C	0.00243	0.00249	0.0179	0.764	0.496	-0.176
	(0.0212)	(0.0294)	(0.0319)	(0.841)	(1.151)	(0.670)
top30C	0.0215	0.0298	0.0306	1.619*	1.426	0.919
	(0.0229)	(0.0311)	(0.0353)	(0.851)	(1.112)	(0.785)
top40C	0.0211	0.0308	0.0226	0.378	0.944	0.686
	(0.0225)	(0.0301)	(0.0327)	(0.771)	(1.070)	(0.687)
top50C	0.0318	0.0593**	0.0104	0.995	2.316**	0.536
	(0.0213)	(0.0286)	(0.0297)	(0.800)	(1.120)	(1.183)
top60C	-0.000331	0.0536*	-0.0420	0.117	2.213*	-1.304
	(0.0236)	(0.0317)	(0.0322)	(0.903)	(1.261)	(1.149)
top70C	-0.00233	0.0574	-0.0485	-0.850	2.107	-2.404
	(0.0283)	(0.0375)	(0.0427)	(1.023)	(1.387)	(1.604)
top80C	0.0132	0.0620*	-0.0427	-0.665	2.418*	-0.0740
	(0.0281)	(0.0365)	(0.0451)	(1.014)	(1.353)	(0.680)
top90C	0.0583	0.0958*	-0.0128	0.0634	4.457**	-2.199
	(0.0392)	(0.0550)	(0.0441)	(1.212)	(1.753)	(1.502)
Observations	1.796	1,084	712	1.796	1,084	712
Migrants	898	542	356	898	542	356

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.16: Impact of disasters on labour supply

	2ndJob			Hourly Wage		
	(1)	(2)	(3)	(4)	(5)	(6)
top10C	0.0305	0.0392	-0.00576	-0.0202	0.0571	-0.0643
	(0.0188)	(0.0326)	(0.0231)	(0.0671)	(0.116)	(0.0594)
top20C	0.00823	0.0383	-0.0210	-0.0910	0.0140	-0.171*
	(0.0159)	(0.0252)	(0.0186)	(0.0717)	(0.114)	(0.104)
top30C	0.0158	0.0286	0.00155	-0.0528	0.0457	-0.107
	(0.0183)	(0.0258)	(0.0247)	(0.0880)	(0.153)	(0.118)
top40C	0.00830	0.0306	-0.00200	-0.0395	0.0382	-0.0778
	(0.0176)	(0.0228)	(0.0283)	(0.0827)	(0.114)	(0.116)
top50C	-0.00443	0.00478	0.00460	-0.0642	-0.00942	-0.113
	(0.0163)	(0.0217)	(0.0263)	(0.0751)	(0.0872)	(0.113)
top60C	0.00975	0.0386	-0.00476	-0.155	-0.129	-0.269
	(0.0205)	(0.0256)	(0.0354)	(0.103)	(0.125)	(0.177)
top70C	0.0247	0.0619**	0.0221	-0.102	-0.0326	-0.566
	(0.0256)	(0.0309)	(0.0410)	(0.153)	(0.150)	(0.349)
top80C	0.0298	0.0959**	-0.0120	-0.114	0.0983	-0.487
	(0.0307)	(0.0420)	(0.0459)	(0.126)	(0.119)	(0.299)
top90C	0.0200	0.0828**	-0.00199	-0.0603	0.145	-0.531
	(0.0276)	(0.0405)	(0.0485)	(0.165)	(0.0971)	(0.382)
Sample	All	Men	Women	All	Men	Women
Observations	1516	832	684	264	144	120
Migrants	758	416	342	132	72	60

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.17: Impact of disasters on monthly savings

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Savings*						
top10C	0.0922	0.105	0.106	0.203	0.236	0.106
	(0.160)	(0.183)	(0.205)	(0.175)	(0.197)	(0.205)
top20C	-0.129	-0.268	0.194	-0.0704	-0.110	0.194
	(0.166)	(0.174)	(0.256)	(0.172)	(0.179)	(0.256)
top30C	0.124	-0.171	0.344	0.155	-0.141	-0.0261
	(0.172)	(0.185)	(0.275)	(0.169)	(0.199)	(0.107)
top40C	-0.393*	-0.291	-0.361	-0.416*	-0.318	-0.296
	(0.224)	(0.204)	(0.280)	(0.240)	(0.212)	(0.288)
top50C	-0.387*	-0.358**	-0.388	-0.413*	-0.428**	-0.421
	(0.208)	(0.157)	(0.275)	(0.226)	(0.170)	(0.297)
top60C	-0.321	-0.139	-0.445*	-0.374	-0.341	-0.456*
	(0.227)	(0.196)	(0.251)	(0.237)	(0.212)	(0.265)
top70C	-0.182	-0.379*	-0.0225	-0.0662	-0.344*	-0.113
	(0.167)	(0.207)	(0.262)	(0.162)	(0.198)	(0.112)
top80C	-0.434**	-0.525**	-0.557*	-0.300	-0.338*	-0.400
	(0.188)	(0.213)	(0.290)	(0.193)	(0.203)	(0.313)
top90C	-0.744***	-0.646*	-0.902**	-0.523**	-0.722**	-0.703*
	(0.249)	(0.337)	(0.357)	(0.254)	(0.326)	(0.370)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	1,746	846	896	1,181	673	506
Migrants	747	366	379	510	295	214

Poisson regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.18: Impact of disasters on the probability not to do sport in the past 12 months

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Nosports* = 1						
top10C	-0.00580	-0.0101	-6.35e-05	-0.00281	0.00397	-0.00745
	(0.0216)	(0.0327)	(0.0292)	(0.0315)	(0.0431)	(0.0462)
top20C	-0.00446	-0.00816	-0.000210	-0.0118	-0.00656	-0.0159
	(0.0187)	(0.0287)	(0.0246)	(0.0278)	(0.0375)	(0.0405)
top30C	-0.0136	0.00654	-0.0234	-0.0213	-0.0101	-0.0253
	(0.0193)	(0.0292)	(0.0255)	(0.0287)	(0.0364)	(0.0459)
top40C	-0.0299	0.00350	-0.0434	-0.0416	-0.00769	-0.0742
	(0.0204)	(0.0273)	(0.0273)	(0.0292)	(0.0372)	(0.0479)
top50C	-0.0142	0.00284	-0.0186	-0.0249	-0.0143	-0.0271
	(0.0182)	(0.0265)	(0.0247)	(0.0258)	(0.0318)	(0.0434)
top60C	-0.00403	0.0296	-0.0203	-0.0235	0.00419	-0.0690
	(0.0188)	(0.0268)	(0.0258)	(0.0280)	(0.0335)	(0.0496)
top70C	0.00885	0.0431	-0.0108	-0.0115	0.0114	-0.0539
	(0.0262)	(0.0381)	(0.0363)	(0.0367)	(0.0437)	(0.0665)
top80C	0.000369	0.0311	-0.0129	0.00809	0.0208	-0.00823
	(0.0295)	(0.0425)	(0.0418)	(0.0430)	(0.0525)	(0.0734)
top90C	0.0398	0.110**	-0.00545	0.0866*	0.141**	0.00655
	(0.0361)	(0.0551)	(0.0481)	(0.0501)	(0.0651)	(0.0807)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	3,754	1722	2,022	1,922	1,170	750
Migrants	1,877	861	1,011	961	585	375

Linear probability model with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.19: Impact of disasters on migrants' subjective financial difficulties

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *Financial =1*						
top10C	0.0188	0.000747	0.0360	-0.00579	-0.0106	0.00565
	(0.0250)	(0.0359)	(0.0346)	(0.0324)	(0.0427)	(0.0498)
top20C	0.0192	0.0230	0.0180	-0.00172	-0.00372	0.00805
	(0.0249)	(0.0355)	(0.0347)	(0.0333)	(0.0448)	(0.0500)
top30C	0.0322	0.0442	0.0213	0.0125	0.0243	-0.0151
	(0.0197)	(0.0283)	(0.0273)	(0.0278)	(0.0365)	(0.0439)
top40C	0.0237	0.0427*	0.00782	0.0108	0.0281	-0.0231
	(0.0184)	(0.0259)	(0.0262)	(0.0256)	(0.0325)	(0.0419)
top50C	0.0342*	0.0614**	0.0103	0.0263	0.0515	-0.0165
	(0.0180)	(0.0254)	(0.0255)	(0.0257)	(0.0323)	(0.0434)
top60C	0.0445**	0.0546*	0.0355	0.0259	0.0606	0.0166
	(0.0200)	(0.0306)	(0.0265)	(0.0291)	(0.0377)	(0.0987)
top70C	0.0559**	0.0905**	0.0295	0.0251	0.0897**	-0.0744
	(0.0232)	(0.0356)	(0.0307)	(0.0339)	(0.0439)	(0.0539)
top80C	0.0432**	0.0743**	0.0195	0.0323	0.0913**	-0.0673
	(0.0218)	(0.0328)	(0.0294)	(0.0305)	(0.0377)	(0.0529)
top90C	0.0545*	0.0533	0.0530	0.0480	0.115**	-0.0529
	(0.0308)	(0.0464)	(0.0411)	(0.0430)	(0.0531)	(0.0696)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	4,428	2,050	2,376	2,272	1,394	876
Migrants	2,214	1,025	1,188	1,136	697	438

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

B.4 Further robustness checks

Table B.20: Impact of disasters on the hourly wage (Inverse Hyperbolic Sine (IHS) transformation of the dependent variable)

	(1)	(2)	(3)
Dependent variable: *Wage* = 1			
top10C	-0.0404	0.0309	-0.120
	(0.0581)	(0.0836)	(0.0783)
top20C	0.0115	0.0117	0.0158
	(0.0456)	(0.0605)	(0.0702)
top30C	0.00500	0.0203	-0.00981
	(0.0458)	(0.0587)	(0.0718)
top40C	0.00797	0.00458	0.0125
	(0.0431)	(0.0558)	(0.0684)
top50C	-0.0126	-0.0289	0.0256
	(0.0399)	(0.0517)	(0.0644)
top60C	0.000512	-0.00362	0.00243
	(0.0437)	(0.0573)	(0.0681)
top70C	-0.00341	0.0338	-0.0821
	(0.0478)	(0.0591)	(0.0798)
top80C	-0.0255	0.0842	-0.188*
	(0.0701)	(0.0959)	(0.101)
top90C	-0.0949	0.0283	-0.286**
	(0.0868)	(0.123)	(0.113)
Sample	All	Men	Women
Observations	264	144	120
Migrants	132	72	60

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

Table B.21: Impact of disasters on monthly savings (Inverse Hyperbolic Sine (IHS) transformation of the dependent variable)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *IHS(Savings)*						
top10C	-0.144	-0.343	0.0395	-0.121	-0.0947	-0.225
	(0.134)	(0.394)	(0.184)	(0.186)	(0.237)	(0.300)
top20C	0.0207	0.0184	0.0239	0.114	0.295	-0.128
	(0.124)	(0.171)	(0.179)	(0.178)	(0.225)	(0.288)
top30C	0.000451	-0.155	0.140	-0.114	-0.196	-0.0414
	(0.135)	(0.190)	(0.189)	(0.186)	(0.235)	(0.300)
top40C	-0.0340	-0.161	0.0632	-0.0606	-0.138	0.0596
	(0.112)	(0.165)	(0.150)	(0.158)	(0.201)	(0.248)
top50C	-0.152	-0.206	-0.105	-0.252*	-0.199	-0.301
	(0.0974)	(0.146)	(0.129)	(0.146)	(0.179)	(0.253)
top60C	-0.164	-0.123	-0.205	-0.174	-0.206	-0.152
	(0.101)	(0.149)	(0.136)	(0.149)	(0.184)	(0.256)
top70C	-0.116	-0.0613	-0.163	-0.101	-0.126	-0.0744
	(0.108)	(0.159)	(0.145)	(0.155)	(0.191)	(0.269)
top80C	-0.585***	-0.454*	-0.413**	-0.299	-0.308	-0.284
	(0.213)	(0.254)	(0.206)	(0.227)	(0.330)	(0.299)
top90C	-0.796***	-0.466	-0.641**	-0.495	-0.263	-0.867**
	(0.234)	(0.328)	(0.275)	(0.303)	(0.416)	(0.435)
Occupation	All	All	All	LF	LF	LF
Gender	All	Men	Women	All	Men	Women
Observations	1,746	846	896	1,181	673	506
Migrants	747	366	379	510	295	214

OLS regressions with year and individual fixed effects. Robust standard errors clustered at the individual level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions include the following controls: age, degree, number of children, marital status, monthly net income, log of the GDP per capita in the home country, the log of the GDP in the home country and the log of the exchange rate.

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