Immigration and Crime:
A Microeconometric Study

By
Georgios Papadopoulos

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Abstract

Although the relationship between immigration and crime has been a very controversial subject in the UK, the empirical evidence is limited. This thesis intends to narrow this gap by providing a comprehensive investigation for England and Wales of immigrants' both active and passive involvement in criminal activities.

Before exploring the aforementioned relationship, Chapter 1 discusses and provides solutions to an identification issue that afflicts leading models for under-reported count data. It also provides some tips for practitioners that intend to use these models in applied research. These findings are important for this thesis, since estimators that deal with under-reporting are considered in Chapter 2.

Chapter 2 studies the individual-level relationship between immigration and crime using self-reported crime data. Although this work focuses on property crime, violent crime is also considered. Both binary and count data models that account for under-reporting are used, since under-reporting is a concern in crime self-reports. Our findings suggest that, if anything, immigrants under-report by less than natives. Most importantly, these models predict that after controlling for under-reporting and basic demographics, immigrants are less involved in criminal activities, but the estimated difference is statistically insignificant. Nevertheless, an extensive sensitivity analysis indicates that this estimate is very robust, suggesting that this relationship exists, but data limitations and complexities of the considered models reduce the precision of the estimated coefficient.

Finally, Chapter 3 comprehensively examines whether victimization experiences are different between immigrants and natives. Very interestingly, although observed demographic differences can explain the positive property crime victimization-immigration differentials, unobserved factors give rise to a negative association between immigration and violent victimization. All results suggest that this is due to immigrants' lifestyle choices associated with lower victimization risks. As will be explained throughout Chapter 3, this finding is consistent with the findings of Chapter 2.

Contents

List of Figures

List of Tables

Introduction

The phenomenon of international migration has been a subject of controversy for politicians, policy makers, and the general public in all countries that sustained large inflows of immigration.[1] Consequently, academic communities have devoted a considerably large amount of research to understand the actual impact of migration on many different aspects of both the host countries and countries of origin. These include the effects of immigration on different dimensions of the labour market (Borjas, 1994, 1999a, 2003) and the welfare state of the host countries (Borjas and Trejo, 1991, and Borjas, 1999b), the impact of brain drain in the countries of origin (Mountford 1997, and Beine, Docquier and Rapoport, 2001, 2008) and the relationship between ethnic diversity and economic performance and growth (Alesina and La Ferrara, 2005), to mention only a few.

Following this debate, scholars have also devoted a lot of research to understand the reasons behind the heterogeneity in individual beliefs towards migration movements by studying the economic and noneconomic determinants of people being anti or pro-immigration (see, for example, Mayda, 2006, O'Rourke and Sinnott, 2006, and Facchini and Mayda, 2009). A particular aspect of immigration where we expect to observe a high proportion of anti-immigration protesters is the relationship between immigration and crime. Indeed, at least for the UK where this thesis focuses on, using data from two important social surveys (look at the introduction of Chapter 1) we can observe that a large fraction of population believes that immigration and crime are linked positively.

However, very interestingly, a high proportion of the researchers' community in social sciences does not share the same view, particularly when it comes to the empirical evidence.

[1] Before getting into the main stage of this introduction, it is important to note that each chapter of this thesis is independent and therefore provides its own comprehensive introduction and conclusion.

Actually most research about the immigration situation in the US indicates that there is a negative association, but results from Europe suggest that there is a positive or no association between immigration and crime (see, Section 2.3). Nevertheless, we need to stress that compared to other aspects of immigration, such as the ones described in the previous paragraphs, the available evidence is rather inconclusive. It is also important to note that the crime-immigration association has been generally overlooked by economists as the majority of the empirical evidence comes from criminological or sociological research.

Most importantly, although this subject is of major interest in political and public debates in the UK, there is lack of empirical evidence. Therefore, this thesis intends to narrow this gap by providing a micro-investigation of immigrants' involvement in criminal activities both as offenders and victims in the UK. It will be made clear to the reader that looking at both the offending behaviour and the victimization experiences of immigrants will provide a more satisfactory picture of the immigration-crime relationship and the rationale behind it.

As this thesis focuses on the micro-immigration-crime link, microeconomic and microeconometric tools will be used throughout. However, in this work we particularly focus on the econometric techniques and specifications that are used as an attempt to overcome data or other methodological limitations. Actually, it could be said that this work is in a sense two-dimensional, as there are two major themes that stand out independently. The first one is the empirical research questions itself, while the second one is the microeconometric methodology, specification, and theory developed to answer the research questions.

Actually, this thesis starts in an unusual way as the first chapter does not investigate the main research question, but it is a small theoretical econometric investigation of some estimators for count data that are used in the second chapter. However, it would be more proper to briefly explain the subject of the first chapter once we understand the limitations of the data and estimators used in the second chapter, which investigates the relationship between immigration status and criminal behaviour in England and Wales.

In this chapter most attention is paid to property crime, although violent crime is also considered. The chapter starts by developing a simple theoretical model of property crime through which the individual link between immigration and property crime is discussed. The model depicts several channels that may lead to higher (because of labour outcomes) or lower (because of deterrent or risk factors) involvement of immigrants in illegitimate activities, in comparison to natives. Therefore, as opposed to the public sentiment in the UK that immigrants are more criminal than natives, the theoretical analysis is not able to predict the direction of the relationship in object. As a consequence, an empirical analysis is required to establish this link. Nevertheless, a major limitation is that true crime is unobserved and therefore, data on criminal activity are really difficult to obtain. Although police records on criminal activity do exist, they do not give any information about immigration status of offenders. Even if this information was available, this type of data would not be appropriate for my study as there is evidence that two thirds of the crime remains unrecorded, but most importantly (for reasons explained in Section 2.4) immigrant population is over-represented in official criminal records. Moreover, prison data would be even more misleading, as people in prison can be considered as a highly selected sample that does not represent the general population.

Thus, the most appropriate strategy would be to use data of survey self-reported crime.[1] For this purpose the Offending, Crime, and Justice Survey (OCJS) of 2003 is used, a representative national survey of (computer-based) self-reported crime. Although the survey design is developed so as to obtain the most reliable responses possible (see, Section 2.4), under-reporting is still a major concern. Therefore, conventional econometric models that ignore this type of measurement error provide inconsistent estimates of the determinants of the actual crime, especially if respondents' reporting behaviour depends on respondents' characteristics. As the dependent crime variable is observed in count form, both count data models, the Poisson-Logit and Negative Binomial-Logit (NB-Logit), and binary choice models, the Misclassification Probit model (MisProbit), that are developed to take into account under-reporting are considered. These are two-index parametric models which, using only observed crime self reports, give information for the determinants of both the true criminal activity and the reporting behaviour. However, identification of the count data models considered in this chapter is afflicted by a subtle problem and further assumptions are required in order to identify the parameters of interest.

[1] Another idea would be to conduct an appropriate controlled experiment for criminal actions, where participants would form a representative part of the general population. This is left for future research.

Therefore, in Chapter 1, which is an extended version of the paper by Papadopoulos and Santos Silva (2008), we provide a thorough investigation of the conditions under which the Poisson-Logit and two popular generalizations of this model (the NB2-Logit and the NB1-Logit) are identified. Although these models are described in the well known monographs for count data by Cameron and Trivedi (1998) and Winkelmann (2008), this identification problem has not been recognized by any work prior to the present thesis. As mentioned in the previous paragraph, the Poisson-Logit is a double index model where by only observing the reported counts we want to draw inference for both the determinants of total counts (modeled as if they follow the Poisson distribution) and the probability of a count to be successful (modeled as a Logit). In the self-reported crime context, total counts correspond to the number of actual but unobserved crimes and the probability of a successful count corresponds to the probability of reporting a committed crime.

In this chapter we show that without appropriate restrictions, taking either the form of at least one sign restriction on the Logit part, or at least one exclusion restriction on the Poisson part, it is not possible to identify the parameters of the Poisson-Logit model, as two identical "global" maxima exist. However, these restrictions must be "strong". In the sign restriction context this means that the sign of at least one coefficient must be determined by well established theoretical results and that the estimated coefficient which we want to impose the restriction on must be statistically significant. In the exclusion restriction context this means that the excluded variables from the Poisson part must have a significant effect on the Logit part and no effect on the Poisson part. Also note that exclusion restrictions on the Logit part can be helpful towards identifying only those coefficients of the Poisson part which are set to zero in the Logit part.

This study also reveals that the same identification problem is present in NB2-Logit. However, even without the aforementioned type of restrictions, a "weak" form of identification is achieved when the NB1-Logit specification is adopted. Here we use the term "weak" as identification of the conditional mean is achieved solely because of an extra parametric assumption on the form of the conditional variance of the dependent variable. Therefore, identification of the mean is highly dependent on the specification of the variance, which has negative consequences in terms of robustness of the estimator. Finally, this identification failure does not extend to models where a different conditional distribution function for binary data is used for the reporting process, such as the Probit.

Although identification of the Poisson-Logit and the NB2-Logit is achieved when we impose exclusion restrictions on the count process, it is high likely that another local maximum exists. Actually the likelihood value of the local maximum will be very close to the likelihood value of the global one if the exclusion restrictions are not "strong", and therefore, identification will be more difficult. As a result, practitioners that intend to use the above models must perform a thorough search for alternative maxima before accepting the first achieved maximum as the global one. Thus, some tips are also provided that can be used to help the practitioner find the global maximum. All theoretical results are supported by an empirical illustration with data on labour mobility.

Back to Chapter 2, due to the small number of positives, a fact that affects the robustness of the count data models, it has been considered as more appropriate to base the main results on the binary choice models and use count data models for sensitivity analyses only. The results of the MisProbit model indicate that respondents considerably under-report their criminal activity. They also suggest that under-reporting is not constant, but it rather depends on respondents' characteristics. However, if anything, immigrants tend to under-report by less than natives. These results are strengthened by the count data models which also indicate that, the probability to report a committed crime depends on respondents' characteristics and also, being an immigrant increases the probability to report a committed crime. Nevertheless, although the interpretation of the coefficients in the Logit part of the count data models is clear, it is important to note that we must be cautious with the interpretation of the coefficients in the under-reporting equation of the MisProbit model, as exactly the same model can be obtained under a zero-inflation framework. According to the zero-inflation specification a fraction of people consists of genuine noncriminals who regardless of their observed characteristics never commit and consequently never report any crimes. Therefore, the MisProbit model is not able to distinguish between zero-inflation and under-reporting. Moreover, this means that only a part of the population participates in the binary model to either commit crimes or not and the estimates of the crime equation must be interpreted as if we exclude genuine noncriminals.

The MisProbit model reveals that, after controlling for under-reporting (zero-inflation) and for basic demographic characteristics, the probability of committing a property crime is lower for immigrants, but the difference is statistically insignificant. This finding is supported by estimation of count data models as well, as being an immigrant (insignificantly) decreases the mean number of actual crimes. Furthermore, violent crime results are in line with the findings of property crime, as the immigrant-crime association is also negative but not statistically significant. A further series of robustness checks (for example, different exclusion restrictions, weighted versus unweighted estimation, and some types of restrictions in the sample) indicates that, although statistically insignificant, the immigration-crime estimated differential is very robust. This suggests that this relationship exists, but data limitations and complexities of the models considered in this chapter reduce the precision of the estimated coefficient.

In the next step, recognising that immigrants' choice of location is not random, we decompose immigrants by region of residence. This exercise interestingly reveals that different regions attract immigrants of different criminal behaviour, or that immigrants adapt differently across regions. According to these results, London is the place with the least criminal immigrants, but South of England is the place with the most crime-prone immigrants. We further allow for the fact that immigrant population is highly heterogeneous by decomposing immigrants by ethnic background. The results of this exercise indicate that immigrants of different ethnic groups exhibit different criminal behaviour. Particularly, black immigrants are less involved in criminal activities than their native counterparts, even though this is also the group that faces the most unfavorable labour outcomes. However, this analysis is restricted by the limited variation between the (small) number of individuals in each particular group and the small number of positives in the dependent variable. For this reason the analysis is kept very descriptive in the sense that we do not investigate the forces behind the underlined estimated relationships. Further research considering a much larger sample could be useful. However, at the moment the OCJS is the only available survey for England and Wales.

Finally, it is also important to stress that this chapter does not use "validation" data, that is data that correspond to respondents' true criminal behaviour. Although the results are relatively robust across several specification and models, both binary and count data models that take into account under-reporting are based on similar assumptions and therefore, to some extent it is expected that they would provide similar results. Hence, we cannot say with certainty that these models "work" and whether they provide results that reflect the true criminal activity. Thus, it would be important for future research to find relatively similar situations where both the under-reported number and the actual number of incidents is observed. As a potential example we could consider the investigation of individual determinant's of students absenteeism, where we observe both survey data on self-reported absenteeism and official school records of absences. Thus, if the models used in this chapter "work", we would expect that the estimates from the models that use the under-reported data but control for under-reporting to be similar to the estimates of the model that uses the actual number of absences. Of course, availability of such data is questionable.[1]

In Chapter 3, we study the "other side of the coin"; that is, the relationship between immigration status and victimization in England and Wales. Although investigation of this relationship is very important to understand the whole immigration-crime picture, it has been totally neglected by the researchers' community. For this purpose, we use data from the 2007/08 sweep of the British Crime Survey, a representative victimization survey where respondents were asked in face-to-face interviews about their victimization experiences in household and personal crime. As will be made clear from the results of this chapter, the investigation of this relationship provides many interesting insights for both the criminal behaviour and the reporting behaviour of immigrants.

In the empirical analysis we look at both instrumental and violent crime, but we particularly focus on the latter.[2] This is because, compared to instrumental victimization, violent victimization is a much more complex process since it is highly dependent on interactions and interrelations between both potential offenders and potential victims prior to the incident. Therefore, as opposed to instrumental crime, (unobserved from the author) potential

[1]Another potential application could consider victimization data from the British Crime Survey. People tend to report for some reasons only one third of the suffered crimes to the police. However, for each victimization incident we know whether the victim reported it to the police. Thus, we know the (under-reported) number of incidents reported to the police and the actual number of incidents.

[2]Instrumental crime can be defined as any criminal action where the offender targets victim's property, whereas in violent crime the offender's target is to hurt the victim itself. This is actually very important for the understanding of the forces behind the empirical results.

victim's personal behaviour is a very strong determinant of violent crime.

Regarding the empirical results, we first find that the probability of being a victim of a burglary or a personal theft is higher for immigrants, which as expected however, can be well explained by the fact that immigrants exhibit some demographic characteristics associated with higher victimization relative to natives. Contrary to the above, we interestingly find that conditional on basic demographic characteristics, immigrants face a lower risk of violent victimization compared to natives.[1] Thus, because of some unobserved characteristics such as unobserved behavioural factors, immigrants encounter a lower risk of violent victimization although they face the same risk of instrumental crime. For instance, a possible story, which is examined throughout the chapter, is that immigrants follow different lifestyle choices associated with lower victimization risks.

However, violent crime is composed of three very different crime types with respect to the relationship between offenders and victims. We actually have information about whether a violent crime was committed by a stranger, or an acquaintance, or a family member. Very interestingly, breaking down violence into these three groups, we find that the immigration-violence estimated differential is driven by the lower crime immigrants suffer by acquaintances and by family members relative to natives, as there is no association for crime by strangers. This is not consistent with the previous hypothesis though, because if immigrants followed the aforementioned lifestyle choices we would expect a lower probability of victimization by strangers as well. The next sections are devoted to examining this pattern.

Firstly, we examine whether immigrants are less willing to report crime committed by familiar people relative to natives due to some cultural factors, but they do not mind reporting crime by strangers. Using data on self-reported domestic victimization (which data are proved to be less under-reported) and the information on whether there was someone else present during the face-to-face interview (which might have affected the reporting behaviour of respondents), we show that immigrants do not under-report domestic crime by more than natives. Therefore, we do not expect that they under-report crime by acquaintances either.

In the next step we investigate whether the unexpected pattern could be explained by

[1] Controlling for many other observed characteristics associated with the risk of victimization does not alter the result. Actually, the effect of immigration on violent victimization is remarkably robust.

the fact that immigrant are more likely to suffer racially motivated crime (RMC) relative to natives, a crime that does not depend (or at least in depends much less compared to other types of violence) on interactions and interrelations between the victim and the offender. Using the information about whether the victim perceived a violent action as being racially motivated, we show that if immigrants did not face RMC, they would also face a significantly lower risk of victimization by strangers.

Finally, we examine whether immigrants' lower risk of victimization by acquaintances or family members could be because more recent immigrants have a smaller number of acquaintances (network effect) or smaller households. First of all, we find that even the most recent immigrants have larger households than natives of the same age. Moreover, as information about the number of acquaintances is not available, we examine the "network effect" hypothesis by assuming that immigrants start with small networks when they enter the country which are being broadened over time. Therefore, we attempted to capture this effect by exploiting the information about duration of time that immigrants have spent in the host country. However, based on the results, we argue that although assimilation patterns exist (for all violent crime types), the contribution of the "network effect" should be relatively weak.

Therefore, all evidence of this study suggests that indeed, immigrants face a lower risk of violent victimization because they follow lifestyles associated with a lower exposure to criminal activities. This result is consistent with the findings of Chapter 2. For instance, individuals that exhibit a relatively lower involvement in violent crime activities are directly less exposed on violent victimization. Another plausible story is again related to the results of the second chapter but from the offenders' point of view. That is, if we assume that people primarily socialize with people of the same background and if we also accept that immigrants are slightly less likely to commit violent crimes, holding everything else constant, we would expect a negative relationship between being an immigrant and the probability to suffer a violent crime by acquaintances or family members, but a much lower difference for crime by strangers (see, subsection 3.2.1 for a numerical example).

In the rest of the chapter, based on criminological theories of repeated victimization, the total number of victimization incidents is used to investigate whether the effect of being an immigrant on the *probability* of victimization is different from the effect of being an immigrant on the *number* of victimization incidents. To investigate this issue we use several count data models that allow for the effect of the independent variables to be different at different parts of the outcome variable's distribution, such as hurdle models for counts (Mullahy, 1986) and the quantile estimator for counts (Machado and Santos Silva, 2005). However, the results show that patterns of repeated victimization are generally the same between immigrants and natives and that if differential repeated victimization between immigrants and natives exists, this is only for individuals that suffer large numbers of victimization incidents.

The count data models used in this section seem very promising towards analysing the determinants of victimization incidents. Although some interesting relationships are revealed, our analysis was restricted by the fact that the number of positives is relatively too small. Therefore, it would be interesting to re-investigate the issue once we pool several sweeps of the BCS.

In a nutshell, this thesis provides interesting contributions to both the empirical relationship between immigration and crime, and to some econometric issues surrounding models for counts and models developed to take into account under-reporting. Moreover, this study opens several fields for future research, which are going to further enrich the understanding of the empirical questions and the econometric issues analysed in this thesis.

Chapter 1

The Poisson-Logit Model: Identification Issues and Extensions

1.1 Introduction

In applied work, researchers are in many occasions forced to use variables which are measured with error, sometimes due to the data collection methods or because of the special nature of some variables. Under-reporting or, under-recording can be listed as a particular type of measurement error, where the observed size of the variable of interest is only a subset of its actual size. For example, this problem is present in surveys, where people are reluctant to reveal the true size of a particular activity, or, in cases where the recording mechanism is unable to record the total amount of actual events.[1]

Specifically, this paper is concerned with the problem of under-reporting/under-recording in count data models.[2] This is a well known problem in the statistics and econometrics literature[3] and well described in the two monographs of Cameron and Trivedi (1998) and Winkel-

[1]See, for example, Feinstein (1991), who discusses the tax evasion problem, Alessie, Gradus and Melenberg (1990), who explore the consequences of not observing small expenditures in consumer expenditure surveys and propose solutions (using a "count amount" model - see, also Van Praag and Vermeulen, 1993), and MacDonald (2002), who discusses the so-called "dark figure" of recorded crime.

[2]This first Chapter is essentially a much more extensive version of the paper "Identification Issues in Models for Underreported Counts", (2008) co-written with Professor João Santos Silva. Therefore, this Chapter shares many common features with the aforementioned paper.

[3]Studies on this problem can be traced back to early works by statisticians such as Leslie and Davis (1939) and Moran (1951) who discuss the problem of estimating the number of total animals in a given area having information only on the trapped animals, by using the assumption that the underlined population of animals decreases as more of them are trapped (and given that there is no reproduction). More recent works include, Olkin, Petkau and Zidek (1981), who develop some estimators to estimate the true number

mann (2008).[1] Although some theory has been developed to deal with this problem, empirical research on this topic is still limited. It needs to be stressed that under-reporting/under-recording is a concept beyond its literal meaning. It includes every situation where the amount of observed, reported events is only a subset of the total number of unobserved, actual events. There are many examples in empirical applications where the above idea can be put into effect. For instance, in a crime survey, respondents may not be willing to reveal the actual number of the crimes they have committed (see, Papadopoulos, 2011b). Moreover, in an application of workers' absenteeism, it may be the case that some absences in a workplace will not be recorded if the monitoring mechanism is weak (see, Mukhopadhyay and Trivedi, 1995). In a different context, a researcher interested in labour mobility may wish to model (unobserved) job offers during a fixed period of time, having only data on (observed) job changes. Since the number of job changes is only a subset of job offers, this situation can be included in the broad concept of under-reported/under-recorded counts (see, Winkelmann and Zimmermann, 1993).

We need to note that the concept of "under-reporting" is conceptually different from "under-recording". In under-reporting, the individual who is responsible for an action is the one that determines the decision whether to report this particular action. For instance, in a crime survey, whether someone reports a crime that he/she has committed, depends on his/her own characteristics. Contrary to that, what determines whether a crime committed by an individual is recorded or not, may be totally irrelevant to the offender's characteristics. In this particular example, this will depend for example on police effectiveness or on laws severity. As will be made clear later in this paper, this is important for the identification of the model presented in the following section that intends to correct for under-reporting/under-recording. Nevertheless, from now on we will be referring to this measurement error problem as under-reporting for ease of exposition.

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of trials (and discuss their stability), given that successful trials are independent random binomial variables, Feinstein (1989, 1990) who explores the problem of compliance and detection, Solow (1993), who discusses the problem of incomplete records of counts of some historic events and estimates the "inclusion" probability under the assumption that this probability monotonically increases over time, and Yannaros (1993) who discusses under-reporting in the context of reported/recorded to police crime and estimates a lower bound for the probability of reporting and consequently an upper bound for the number of the true number of crimes, to mention only a few.

[1] Furthermore, a model accounting for under-reporting is implemented in a popular software for econometrics (see, Econometric Software, Inc., 2007).

The special nature of count data (non-negative integers) has concerned econometricians throughout the last decades.[1] The benchmark model for count data is the Poisson regression model, an important property of which is that its density falls within the Linear Exponential Family (LEF).[2] As Gourieroux, Monford and Trognon (1984a) show, if a density belongs to the LEF, consistency of the Maximum Likelihood Estimator (MLE) only requires correct specification of the conditional mean. Thus, the Poisson MLE is a very robust estimator since it is consistent even when the true density (true data generating process) is not Poisson, given correct specification of the mean.[3] The Poisson MLE that permits misspecification of higher moments is known as Poisson Pseudo-MLE (see, also, Cameron and Trivedi, 1998, and Winkelmann, 2008).

A limitation of the Poisson model is the assumption of equi-dispersion, which (in a regression framework) means that the conditional mean equals the conditional variance. If the data in hand are over-dispersed (under-dispersed), meaning that the variance is higher (lower) than the mean, conventional Poisson MLE standard errors, obtained from estimating the variance matrix $n^{-1}I^{-1}$ (where I is the information matrix), will be under-estimated (over-estimated) resulting in inflated (deflated) asymptotic t-statistics and thus, in incorrect inference (see, Cameron and Trivedi, 1986). This is not very restrictive though, since as Gourieroux, Monford and Trognon (1984a) showed, we can still obtain valid inference by estimating the variance matrix as $n^{-1}I^{-1}JI^{-1}$ (where J is the variance of the score vector).[4] This results in the Pseudo-ML standard errors, which are simply known as robust standard errors. If the variance is higher than the mean, an alternative is to use a different distribution that allows for over-dispersion, such as the Negative Binomial family of distributions (NB). The NB2 and NB1 are the most popular models in the literature.[5] Although the NB models

[1] Milestone works dealing with methods appropriate for count data are, among others, Jorgenson (1961), Gourieroux, Monford and Trognon (1984a,b), Hausman, Hall and Griliches (1984), and Cameron and Trivedi (1986).

[2] A density function with mean λ belongs to the LEF if it can be written as $f(y, \lambda) = e^{A(\lambda)+B(y)+C(\lambda)y}$. Thus, the Poisson model belongs to the LEF with $A(\lambda) = -\lambda$, $B(y) = -\ln(y!)$ and $C(\lambda) = \ln(\lambda)$.

[3] Very briefly, this important property comes from the fact that the first order condition (Score Funcion) of any LEF can be written as $(\partial C(\lambda)/\partial\lambda)[y - \lambda]$ (see, for example, Winkelmann, 2008) and therefore, if $E(y) = \lambda$ the expected score converges to the observed score, since the MLE sets the observed score to zero.

[4] This simple modification of the variance-covariance matrix is similar to the modification required in continuous data under heteroskedasticity (see, White, 1982).

[5] These models allow for over-dispersion, as we will describe in detail later, by introducing an unobserved gamma distributed parameter with mean equal to one and variance equal to a parameter α_i. The NB2 model with mean λ_i is obtained if α_i is the same for every individual (homoskedastic), while the NB1 follows if α_i

can lead to efficiency gains in the presence of over-dispersed data compared to Poisson MLE (since they exploit information of the second moment), they do not belong to the LEF and hence, they are less robust in the sense that both the conditional mean and variance must be correctly specified (see, Cameron and Trivedi, 1998).[1]

However, although quite robust, the conventional Poisson model becomes inappropriate under some other types of misspecification, under-reporting of the count outcome being one of them.[2] Consequently, when under-reporting is evident, a conventional regression model for count data will be misspecified and the estimation procedure will generally yield inconsistent estimates. On this direction, models that take into account this source of misspecification have been developed. In the next section, such a regression model will be presented. This model is named Poisson-Logit (Poisson-Logistic Regression Model) since it is a double index model based on the mixture of a Poisson with a Logit. True counts are generated by a Poisson process and a different process, modeled as a Logit, determines whether an actual event is reported. This is the simplest and the most popular among all other models developed for this purpose. As will be made clear later, its simplicity can be attributed to the assumption of independence between the count and the reporting process. Two natural extensions of the Poison-Logit model are also presented, the Negative Binomial 2-Logit (NB2-Logit) and the Negative Binomial 1-Logit (NB1-Logit). As in the simple NB regression model, NB-Logit is used to take into account gamma distributed unobserved individual heterogeneity.[3]

is a function of the regressors such that $\alpha_i = \delta/\lambda_i$ (heteroskedastic).

[1] However, note that the NB2 belongs to the LEF only if α is known (fixed), which is not true in practice.

[2] For a detailed analysis on sources of misspecification, see Winkelmann (2008), page 102.

[3] In the literature of econometrics and statistics there are a few studies that deal with under-reporting. These are the following: a NB2-Probit model is developed by Feinstein (1989) under the name of "Detection Controlled Random Poisson". In this study he discusses the problem that inspectors of nuclear plants sometimes fail to detect a number of violations, so that the detected violations are only a number of the true violations. This concept can be naturally applied in any situation that involves compliance and inspection (see, also, Feinstein, 1990). A Poisson-Probit model is applied in transportation research by Kumara and Chin (2005) who want to identify the determinants of actual road accident given the recorded ones. Winkelmann (1998) presents a model where the strong assumption of independence between the two underlined processes is relaxed. This is succeeded by allowing unobservables from both processes to be correlated. For convenience, the reporting process is developed as a Probit. Pararai, Famoye and Lee (2006), use the Generalized Poisson distribution instead of the Poisson, a model that is appropriate in the presence of both over and under-dispersion. Li, Trivedi and Guo (2003) on the other hand, develop a structural Generalized Negative Binomial mixture of Poisson regression, suitable for both under and over-reporting. Winkelamann (1996) adopts a Bayesian approach, where he can estimate the parameters of the model by simulating their joint posterior distribution using the Marcov chain-Monte Carlo simulation method, although each parameter's marginal posterior distribution is analytically intractable. On the other hand, Fader and Hardy (2000) are able to derive analytic expressions for the marginal posteriors of interest, by using a Beta Binomial-Negative Binomial Distribution model, however, not in a regression but in a univariate framework. Van Praag and

Although very useful as an idea, identification of the Poisson-Logit model is problematic, in the sense that even under the strong parametric assumptions required for setting up the model, two identical "global" maxima exist and therefore, identification of the parameters of interest is not possible. However, as will be made clear later, identification of the parameters of the Poisson-Logit model can be achieved under further assumptions, which take the form of either sign restrictions or exclusion restrictions. Moreover, it will be shown that exactly the same identification issues arise in the NB2-Logit. Contrary to this, another parameterization of the Negative Binomial distribution gives rise to the NB1-Logit model, whose structure makes identification easier. Finally, all the analysis implies that the identification problems affecting the Poisson-Logit cannot be extended to a model where the Probit specification is used instead of the Logit.

The rest of this paper is organized as follows: in Section 1.2 the Poisson-Logit model is presented. Section 1.3 generalizes this model to allow for gamma specific unobserved individual heterogeneity, giving rise to the Negative Binomial family of models. Section 1.4 discusses the identification issues of the presented frameworks. In Section 1.5 some possible solutions to these identification issues are discussed. A general discussion on the aforementioned analysis follows in Section 1.6. Section 1.7 briefly discusses other models that are used in different contexts but either their conditional mean is specified as the Poisson-Logit's one or they face similar identification problems. Section 1.8 uses an empirical application to labour mobility, adopted by Winkelmann and Zimmermann (1993), to illustrate the theoretical results of this study. Finally, Section 1.9 consists of concluding remarks.

1.2 The Poisson Logistic Regression Model

In this section we present the Poisson-Logit model, introduced in Winkelmann and Zimmermann (1993), but also discussed in Mukhopadhyay and Trivedi (1995). To begin with, consider the data generating process (DGP) where true events are generated by a Pois-

Vermeulen (1993) develop a "count-amount" model based on a different approach, utilizing the extra information that an event is recorded only if it exceeds a threshold value. Finally, Cohen (1960) discusses a situation of Poisson distributed counts where a proportion of ones are recorded as zeroes. For example, when an inspector examines an item he/she may conclude that it is perfect even if there is one small defect, while he/she records correctly items with two or more defects. However, he recognizes that this situation is unrealistic.

son process, but whether each event is reported is determined by a Bernoulli process, a mechanism known in the literature as binomial thinning.[1] According to this procedure, for individual i, the total amount of events is considered as the sum of a sequence of Bernoulli trials, where for every particular event there is a constant probability of "success" equal to p. If an event turns to be successful, it is consequently reported, whereas if unsuccessful, with probability $1 - p$, it remains unreported. As a consequence, the observed counts are only a subset of the true counts. In a regression framework, the estimates of a conventional Poisson model which aims to identify the parameters of the true events will most probably be inconsistent, since these estimates are based on the reported events rather than the true events (see, Winkelmann, 2008). However, a more appropriate *compound* Poisson distribution, combining the poisson process with the reporting process can be developed.[2]

It should be mentioned that throughout the analysis a regression framework is assumed, in which the object of interest is the distribution of the true (unobserved) events for individual i, y_i^*, conditional on a set of regressors $x_i = (x_{1i}, x_{2i})$. Vector x_{1i} is assumed to affect the Poisson process while vector x_{2i} affects the reporting process. These two sets of regressors may be identical, disjoint or overlapping.[3]

To start with, assume that y_i^*, conditional on the set of covariates x_{1i}, follows the Poisson distribution. Therefore, the conditional probability of the random variable to be equal to a realization y_i^* is given by,

$$\begin{aligned} &\Pr(Y_i^* = y_i^* | x_{1i}) = e^{-\lambda_i} \lambda_i^{y_i^*} / y_i^*!, \\ &\lambda_i = E[y_i^* | x_{1i}] = e^{x'_{1i}\beta}, \end{aligned} \tag{1.1}$$

where λ_i is both the Poisson conditional mean and variance, a result known as "equi-dispersion". As it is very common in econometrics literature, λ_i is assumed to depend

[1] The "binomial thinning" process is introduced in count data regression models for time series. See, for example, Steutel and Van Harn (1979), and McKenzie (1985). In this paper, the binomial thinning operator will be modeled as a Logit.

[2] For a detailed discussion of compound and mixture distributions refer to Johnson, Kemp, Kotz (2005).

[3] According to note 5, in cases that we deal with under-reporting, x_{1i} and x_{2i} will generally be identical, unless there are good reasons to advocate an exclusion restriction either from the reporting or the count process. However, if our research project deals with under-recording, x_{1i} and x_{2i} will generally be disjoint or overlapping, but not identical. As will be seen later, this is important for the identification of the models presented in this paper.

exponentially on x_{1i}, which ensures non-negativity of the Poisson conditional expectation.[1]

More importantly, assume that y_i, which denotes the number of events reported in a given period (observed events) by individual i, is given by the sum of independent and identically distributed (i.i.d) random Bernoulli variables B_{ij}, so that,

$$y_i = B_{i1} + B_{i2} + \ldots\ldots + B_{iy_i^*} = \sum_{j=1}^{y_i^*} B_{ij}, \tag{1.2}$$

where y_i^* is the total number events and therefore, $y_i \leq y_i^*$.[2] Moreover, the probability of an event to be reported (successful), is assumed to depend on the set of regressors x_{2i}. In the present specification this is modeled as a Logit, thus given by,

$$\Pr(B_{ij} = 1|x_i) = \Lambda(x'_{2i}\gamma) = \Lambda_i = \frac{e^{x'_{2i}\gamma}}{1 + e^{x'_{2i}\gamma}}. \tag{1.3}$$

If it is assumed that y_i^* is conditionally independent from B_{ij}, (1.2) implies that y_i has a *compound*, or differently, a *stopped-sum* distribution , i.e. a binomial distribution stopped by Poisson.[3] The distribution of y_i can be derived using probability generating functions (PGF) (see, for example, Feller, 1968). The PGF of the Poisson and the Bernoulli distributions, for any real k and z, are given by

$$\begin{aligned} G^{y^*}(k) &= e^{\lambda(k-1)}, \\ G^{B}(z) &= 1 - p + pz, \end{aligned} \tag{1.4}$$

where λ is the Poisson parameter and p is the probability that an event is reported. Then,

[1]Following the results of the Pseudo-MLE, if the true mean is misspecified, for example if it is not log-linear in the population, then MLE is inconsistent. There have been suggestions to use a more general function for the mean, such as $E(y|x) = [1 + \omega(x'\beta)]^{1/\omega}$ which is known as the Box-Cox transformation (see, Wooldridge, 1992). This transformation nests both the linear case for $\omega = 1$ and the exponential case for $\omega = 0$.

[2]Here, it is implicitly assumed that it would never be the case that someone reports an event that did not happen, so that there is no over-reporting. For a model allowing for both under-reporting and over-reporting, see Li, Trivedi and Guo (2003).

[3]The name *stopped-sum* comes from the fact that the summation of Bernoulli variables is "stopped" by the value of the Poisson distributed latent variable y_i^*. It needs to be noted that the assumption of independency is quite strong. It is highly likely that the reporting probability depends on the number of true events and vice versa. For example, an individual would be less likely to report a crime if the number of crimes he/she has committed is quite high. Also, an absence from work is more probable if the probability of recording this particular absence is quite low. Of course, this assumption can be relaxed, something that will lead to more complicated results which are beyond the scope of this paper (see, for example, Winkelmann, 1998)

it can be shown that the *compound* PGF of y, under independence of y_i^* from B_{ij}, is given by,

$$G^y(k(z)) = G^{y^*}(G^B(z)) = e^{\lambda(G^B(z)-1)} = e^{\lambda(-p+pz)} = e^{\lambda p(z-1)} = e^{\mu(z-1)}. \tag{1.5}$$

Thus, the distribution of y is also Poisson with mean and variance equal to $\mu = \lambda p$.[1] In the regression framework, $\lambda_i = e^{x'_{1i}\beta}$ and $p_i = \Lambda_i$. The resulting conditional probability of y_i, and the conditional mean are given by,

$$\begin{aligned} &\Pr(Y_i = y_i|x_i) = e^{-\mu_i}\mu_i^{y_i}/y_i!, \\ &\mu_i = E[y_i|x_i] = \lambda_i\Lambda_i, \end{aligned} \tag{1.6}$$

respectively. This model is named Poisson-Logit for obvious reasons. Parameters β and γ (henceforth denoted as $\theta = (\beta, \gamma)$) can be estimated by the method of Maximum Likelihood, as we can easily specify the likelihood function from (1.6). The resulting log-likelihood function is given by,

$$\ell(\theta) = \ln \mathscr{L}(\theta) = \sum_{i=1}^{n} \Big(-\mu_i + y_i \log \mu_i - \ln{(y_i!)}\Big). \tag{1.7}$$

Estimation of θ follows by maximization of (1.7) using numerical algorithms, such as the Newton-Raphson, as the first order conditions (FOCs) for optimality are non-linear.[2] Hence, according to this framework, by only observing the reported events, we are able to estimate the impact of x_{1i} and x_{2i} on the true events and on the probability for each event to be

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[1]This result can be traced even further back than Feller (1968) to the early works in statistics by Neyman (1939) and Catcheside (1948). Neyman explains that in a given area, if the number of eggs laid by a fly per plant follow the Poisson distribution with λ, and if these masses of eggs hatch independently with probability of survival p, then the survived flies also follow the Poisson distribution with $\mu = \lambda p$. Similarly, Catcheside, in an example adjusted to genetics, says that if a given dosage of radiation causes breakages to chromosomes (where the number of total unobserved breakages per cell follow the Poisson distribution), and if there is a constant probability, $1 - p$, for a break chromosome to heal , then the observed breakages follow the Poisson distribution with parameter $\mu = \lambda p$.

[2]The Score and Hessian of the Poisson-Logit model are given by, $s(\theta) = \frac{\partial \ell(\theta)}{\partial \theta} = \sum_{i=1}^{n} (y_i - \mu_i) \begin{bmatrix} x'_{i1} \\ x'_{i2}(1-\Lambda_i) \end{bmatrix}$ and $H(\theta) = \frac{\partial^2 \ell(\theta)}{\partial\theta\partial\theta'} = \sum_{i=1}^{n} -\mu_i \begin{bmatrix} x_{i1}x'_{i1} & x_{i1}x'_{i2}(1-\Lambda_i) \\ x_{i1}x'_{i2}(1-\Lambda_i) & x_{i2}x'_{i2}\left[(1-\Lambda_i)^2 + \frac{y_i-\mu_i}{\mu_i}\Lambda_i(1-\Lambda_i)\right] \end{bmatrix}$, respectively. The lower right block of the second matrix is negative if $y_i < \lambda_i(2\Lambda_i - 1)$ and therefore, $H(\theta)$ is not always negative definite. Consequently, $\ell(\theta)$ is not globally concave which may lead to multimodality. This feature and its consequences will be discussed later.

reported, respectively.

We notice that the Poisson-Logit probability distribution is the same as the one of the traditional Poisson model, with a modified conditional mean. Therefore, as the density of the traditional Poisson model belongs to the LEF (see, Gourieroux, Monfort, and Trognon, 1984), so does the density of the Poisson-Logit model with μ in place of λ. Therefore, using the result of the Pseudo-MLE, consistent estimation of θ only requires correct specification of the conditional mean. That is, the true DGP need not be Poisson-Logit but the true mean must be given as $\mu_i = \lambda_i \Lambda_i$. However, in cases of misspecified distributional assumptions, estimates of higher moments will be inconsistent. Therefore, valid inference still requires that the conditional variance is correctly specified (equal to μ_i in the case of Poisson-Logit). Particularly, as it is the case for the conventional Poisson model, it can be shown that if $Var(y_i|x_i) > E(y_i|x_i)$, the Poisson-Logit MLE standard errors will be underestimated yielding false inference for the parameters of interest. In these cases, statistical inference must be based on Pseudo-ML standard errors which consistently estimate the variance of θ as explained in the introduction. Therefore, as long as we are confident about the specification of μ_i, the Pseudo-ML is a consistent estimator for both θ, and the variance of θ.

It is clear that the above *compound* model is applicable not only in cases of under-reporting, but whenever the observed number of events is a subset of the actual number. Thus, as mentioned in the introduction, "under-reporting" can be considered as a broader concept. For example, Winkelmann and Zimmermann (1993) analyze job offers, which can be considered as the true DGP in labour mobility models, by merely observing the number of job changes (see also, Section 1.7). In this sense, using the Poisson-Logit, that imposes more structure to the model, they are able to estimate the impact of employee's characteristics on both the number of outside job offers they receive and the probability of accepting an outside job offer. As another example consider firms' innovative activity. As Wang, Cockburn, and Putermam (1998) discuss, economists usually use the number of patents as an indicator of a firm's inventive activity since inventive activity is not directly observed. However, having only data on the count of patents, the Poisson-Logit model enables the researcher to draw inference for the probability of an invention to be patented and the determinants of the true inventive activity.

1.3 Extensions - The Negative Binomial Logit Case

A basic property of the Poisson-Logit model is that μ_i is both the conditional mean and variance of the dependent variable. This result often makes the Poisson distribution less appropriate in fitting "real" data, since many empirical applications reveal that over-dispersion exists. In the presence of over-dispersion, although the Poisson MLE complemented by robust standard errors is totally appropriate, given that the conditional mean is correctly specified, researchers tend to use models more suitable for over-dispersed data, the most popular being the Negative Binomial family of models (NB). One way to obtain the NB model is by combining a Poisson distribution with an independent gamma distributed error term (see, for example, Cameron and Trivedi, 1986). This error term can be regarded as an unobserved individual heterogeneity, for instance, because of omitted regressors from the mean specification.

To this end, suppose that there is an unobservable individual effect, $\upsilon_i = e^{\varepsilon_i}$, which is gamma distributed with $E(\upsilon_i) = 1$ and $Var(\upsilon_i) = \alpha_i$. Moreover, assume that y_i^* conditional on x_{1i} and υ_i is Poisson distributed with mean $\lambda_i e^{\varepsilon_i}$. Thus, conditional on x_{1i} only, the distribution of y_i^* is Poisson-Gamma with conditional probability,

$$\Pr(Y_i^* = y_i^*|x_{1i}) = \int \frac{e^{-\lambda_i \upsilon_i}(\lambda_i \upsilon_i)^{y_i^*}}{y_i^*!} g\left(\upsilon_i, 1/\alpha_i\right) d\upsilon_i \equiv E_\upsilon\Big[\Pr(Y_i^* = y_i^*|x_{1i}, \upsilon_i)\Big], \tag{1.8}$$

where $g(.)$ is the gamma density function with parameter $1/\alpha_i$. Averaging out (1.8) leads to the NB distribution[1] with mean, λ_i and variance,

$$\omega_i = \lambda_i + \alpha_i \lambda_i^2. \tag{1.9}$$

Therefore, this formulation allows for over-dispersion since $\alpha_i > 0$, implies that, $\omega_i > \lambda_i$.[2]

Now, similarly to the previous section, assume that y_i has a *stopped-sum* distribution as given in (1.2), however, in this case this sum is stopped by the value of y_i^*, which follows

[1]For a proof, see, Cameron and Trivedi (1998), p101.

[2]The mean of the NB can be obtained by using the Law of Iterated Expectations as $E(y|x) = E_\upsilon\left[E(y|x,\upsilon)\right] = E_\upsilon[\lambda\upsilon] = \lambda$. The variance is obtained by using the Law of Total Variance, as $Var(y|x) = E_\upsilon\left[Var(y|x,\upsilon)\right] + Var_\upsilon\left[E(y|x,\upsilon)\right] = E_\upsilon[\lambda\upsilon] + Var_\upsilon[\lambda\upsilon] = \lambda + \alpha\lambda^2$. For further details about the Negative Binomial models refer to Hausman, Hall and Griliches (1984), Cameron and Trivedi (1986, 1998), and Winkelmann (2008).

the NB rather than the Poisson distribution. Again, the probability of an actual event to be reported, conditional also on this error term, is given by Λ_i. The distribution of y_i can be similarly derived using a *compound* PGF, resulting from compounding a NB PGF together with a Bernoulli PGF. Following Anscombe (1950), the NB PGF for y^*, with mean equal to λ and variance equal to $\lambda(1+\alpha\lambda)$, is given by,

$$G^{y^*}(k) = (1+\alpha\lambda-\alpha\lambda k)^{-\alpha^{-1}}. \tag{1.10}$$

Therefore, the *compound* PGF for y_i is the following:

$$\begin{aligned} G^{y}(k(z)) &= G^{y^*}(G^B(z)) = (1+\alpha\lambda-\alpha\lambda G^B(z))^{-\alpha^{-1}} = (1+\alpha\lambda-\alpha\lambda(1-p+pz))^{-\alpha^{-1}} \\ &= (1+\alpha\lambda p-\alpha\lambda pz)^{-\alpha^{-1}} = (1+\alpha\mu-\alpha\mu z)^{-\alpha^{-1}}. \end{aligned} \tag{1.11}$$

Thus, the distribution of y is NB with mean μ and variance $\mu(1+\alpha\mu)$. According to the regression framework, similarly to the procedure followed for the simple NB model, the distribution of y_i conditional on x_i only, is NB with mean equal to μ_i and variance given by,

$$\omega_i = \mu_i + \alpha_i\mu_i^2. \tag{1.12}$$

As it is clear from (1.12), the NB-Logit converges to the Poisson-Logit model as α_i approaches zero, since consequently, ω_i converges to μ_i.[1]

The NB2 is the most used and cited model among the NB family. It is obtained if it is assumed that the variance of the error term, α_i, is constant (homoscedastic), so that the variance of y_i is $\omega_i = \mu_i + \alpha\mu_i^2$, which is quadratic on μ_i. The conditional probability of the NB2-Logit follows the conditional probability of the simple NB2 model, with mean

[1]A formal proof that shows how the NB probability function converges to Poisson as $\alpha \to 0$, can be found in Wineklmann (2008), p23.

parameter $\mu_i = \lambda_i \Lambda_i$ in place of λ_i, such that,

$$\Pr(Y_i = y_i|x_i, \alpha) = \frac{\Gamma(y_i + \alpha^{-1})}{\Gamma(y_i + 1)\Gamma(\alpha^{-1})} \left(\frac{\alpha^{-1}}{\alpha^{-1} + \mu_i}\right)^{\alpha^{-1}} \left(\frac{\mu_i}{\alpha^{-1} + \mu_i}\right)^{y_i}$$
$$= \frac{\Gamma(y_i + \alpha^{-1})}{\Gamma(y_i + 1)\Gamma(\alpha^{-1})}(1 + \alpha\mu_i)^{-(\alpha^{-1}+y_i)}(\alpha\mu_i)^{y_i}. \tag{1.13}$$

We can estimate θ and the additional "overdispersion" parameter α by Maximum Likelihood, specifying first the log likelihoods from (1.13), which is,

$$\ln \mathscr{L}(\alpha, \beta, \gamma) = \sum_{i=1}^{n} \left(\ln \left(\frac{\Gamma(y_i + \alpha^{-1})}{\Gamma(y_i + 1)\Gamma(\alpha^{-1})} \right) - (\alpha^{-1} + y_i) \ln(1 + \alpha\mu_i) + y_i(\ln \mu_i + \ln \alpha). \right) \tag{1.14}$$

Another model of this family that is extensively used in empirical works is the NB1. The NB1 is obtained if we consider that α_i is not constant but instead, it is a function of the regressors (heteroscedastic) according to the following relationship, $\alpha_i = \delta/\lambda_i$. Substituting this relationship into (1.12) we obtain the variance of the NB1-Logit,

$$\omega_i = \mu_i + \delta\lambda_i\Lambda_i^2. \tag{1.15}$$

Thus, this different parameterization leads to a variance form which is no more quadratic on μ_i, but rather, linear with respect to λ_i and quadratic with respect to Λ_i. Given (1.15) the probability distribution of the NB1-Logit model is given by,

$$\Pr(Y_i = y_i|x_i, \delta) = \frac{\Gamma(y_i + \delta^{-1}\lambda_i)}{\Gamma(y_i + 1)\Gamma(\delta^{-1}\lambda_i)} \left(\frac{\delta^{-1}\lambda_i}{\delta^{-1}\lambda_i + \mu_i}\right)^{\delta^{-1}\lambda_i} \left(\frac{\mu_i}{\delta^{-1}\lambda_i + \mu_i}\right)^{y_i}$$
$$= \frac{\Gamma(y_i + \delta^{-1}\lambda_i)}{\Gamma(y_i + 1)\Gamma(\delta^{-1}\lambda_i)}(1 + \delta\Lambda_i)^{-(\delta^{-1}\lambda_i+y_i)}(\delta\Lambda_i)^{y_i}. \tag{1.16}$$

The log likelihood of NB1-Logit is therefore given by,

$$\ln \mathscr{L}(\alpha, \beta, \gamma) = \sum_{i=1}^{n} \left(\ln \left(\frac{\Gamma(y_i + \delta^{-1}\lambda_i)}{\Gamma(y_i + 1)\Gamma(\delta^{-1}\lambda_i)} \right) - (\delta^{-1}\lambda_i + y_i) \ln (1 + \delta\Lambda_i) + y_i(\ln \delta + \ln \Lambda_i) \right). \tag{1.17}$$

From (1.17) it is clear that the parameters of the NB1-Logit model do not always appear together, as opposed to both Poisson-Logit and NB2-Logit where the regressors of both the count and reporting processes affect the log likelihood function only through μ_i. The interesting implication of this feature will be discussed in Section 1.5. Similar to the NB2-Logit, maximum likelihood can be performed to estimate the values of β, γ and δ that maximize the likelihood of having obtained the observed data.[1]

We need to stress, however, that although these two models may lead in efficiency gains in the presence of over-dispersion, they do not belong to the LEF.[2] Thus, the robustness properties of the Pseudo-MLE do not hold in this case. For consistency of the NB-Logit models it is required that the true DGP is NB-Logit, so that the individual unobserved heterogeneity is gamma distributed with variance equal to α and δ/λ_i for NB2-Logit and NB1-Logit, respectively. Therefore, by using the NB-Logit models instead of the Poisson-Logit we trade-off robustness for a possible gain in efficiency.

1.4 Identification Issues in the Poisson-Logit model

Even though the Poisson regression model is very robust, the Poisson-Logit is a double-index model where identification of θ is problematic. As will be made clear in this section, apart from all the aforementioned assumptions in the set up of the model, further parametric assumptions are needed in order for the parameters β and γ to be identifiable. This identification problem stems from the fact that the Poisson-Logit mean is given as the product of the Poisson mean (exponential) and the Logit function. Consider the following simple manipulation of μ_i:

$$\mu_i \equiv e^{x'_{1i}\beta}\frac{e^{x'_{2i}\gamma}}{1+e^{x'_{2i}\gamma}} = e^{x'_{1i}\beta+x'_{2i}\gamma}\frac{1}{1+e^{x'_{2i}\gamma}} = e^{x'_{1i}\beta+x'_{2i}\gamma}\frac{e^{-x'_{2i}\gamma}}{1+e^{-x'_{2i}\gamma}} \equiv \mu_i^*, \tag{1.18}$$

[1]Studies that explore the NB1-Logit and NB2-Logit models are limited. However, a detailed description of the NB2-Logit model together with its FOCs and its Information Matrix can be found in Mukhopadhyay and Trivedi (1995), and Cameron and Trivedi (1998). In addition, a conditional probability function similar to the NB1-Logit is obtained by Van Praag and Vermeulen (1993).

[2]The NB2-Logit belongs to the LEF only for given α. Nevertheless, in practice α is subject to estimation as there is no *a priori* information about its value.

where we notice that the conditional mean can be written in two different ways as $\mu_i = \lambda_i \Lambda_i$ and $\mu_i^* = \lambda_i e^{x_{2i}'\gamma}(1 - \Lambda_i)$, where $\mu_i \equiv \mu_i^*$. This simple, yet important, result has critical consequences for the identification of the Poisson-Logit model.

First, recall that the Poisson-Logit MLE aims to determine the values of θ that maximize the value of the likelihood function. From (1.7) we can see that these parameters enter the log likelihood function only through μ_i, which appears in the log likelihood only as a whole (the Poisson mean is not separated from the Logit at any point). Therefore, identification of β and γ will depend only on μ_i. Now assume that there is μ_i that maximizes the likelihood function. However, since $\mu_i \equiv \mu_i^*$, μ_i^* also maximizes the same likelihood function. This means that, μ_i is observationally equivalent to μ_i^*, unless appropriate restrictions are imposed on θ.[1] Particularly, as will be made clearer in the next paragraphs, unless at least one exclusion restriction is imposed on β, (1.18) implies that there are two Poisson-Logit regression models with conditional means μ_i and μ_i^*, that lead to the same likelihood value.

To fix ideas, consider first the case where the same regressors appear in both processes, so that $x_i = x_{1i} = x_{2i}$. According to this assumption, (1.18) gives,

$$\mu_i \equiv e^{x_i'\beta}\frac{e^{x_i'\gamma}}{1+e^{x_i'\gamma}} = e^{x_i'(\beta+\gamma)}\frac{e^{-x_i'\gamma}}{1+e^{-x_i'\gamma}} = e^{x_i'\beta^*}\frac{e^{x_i'\gamma^*}}{1+e^{x_i'\gamma^*}} \equiv \mu_i^* \tag{1.19}$$

Therefore, there are always two observationally equivalent models and identification fails. The parameters θ and θ^* of the two models are related to each other in the following manner: if the parameters of the first model are given by β for the Poisson and γ for the Logit part, then the parameters of the second model are given by $\beta^* = \beta + \gamma$ and $\gamma^* = -\gamma$, respectively. In Section 1.7, we illustrate this result using a real data set.

1.5 Possible Solutions to the Identification Problem

In this section, we firstly present the conditions under which the Poisson-Logit model is identified. One possible identification strategy is to use sign restrictions on the Logit process, henceforth called "reporting process", which is helpful whenever prior information of the sign

[1] By "observational equivalence" we mean that there are two different linearly dependent sets of parameters, for example, $\theta = (\beta, \gamma)$ and $\theta^* = (\beta^*, \gamma^*)$, that maximize the value of the likelihood function.

of at least one parameter of the reporting process exists. Moreover, exclusion restrictions on the reporting or the count process can be used. This requires that we have initial information for at least one parameter of β or γ to be zero, meaning that a regressor that belongs to the reporting process has no effect on the reporting process, or the opposite.[1] Following another direction, we consider small departures from the Poisson-Logit model by assuming a different distribution for the count process, or a different model for the reporting process, or any combination of the two. We will also show that it is possible to identify θ by imposing a different structure on the density function. In this section only the theoretical results are presented. Discussion of these findings and empirical illustrations follow in subsections 1.6 and 1.8, respectively.

1.5.1 Sign Restrictions on the Reporting Process

A first way to identify θ is by imposing at least one sign restrictions on the reporting process. It must be stressed that this option is valid only if established theoretical results clearly suggest the direction of the impact of an independent variable on the reporting process. For instance, consider the example of labour mobility adopted by Winkelmann and Zimmermann (1993), where job offers follow the Poisson distribution and the probability to accept an offer is given as a Logit. Now suppose that a hypothetical "well" established theory for labour mobility suggests that more "firm specific" human capital accumulation (FS-HCA) by employees increases wage in the current job but not the wage offered by outside firms. Therefore, more FS-HCA increases the wages differential between the current job and potential outside job offers. Consequently, following this theory, an increase in FS-HCA will have a negative effect in the probability of a worker to accept a job offer, therefore, resulting in a negative coefficient in the Logit part.[2]

Since without exclusion restrictions two observationally equivalent models always exist with $\theta = (\beta, \gamma)$ and, $\theta^* = (\beta + \gamma, -\gamma)$, the effect of this variable will be positive in the one

[1] If the model was not afflicted by this identification problem, it would be natural to assume that the individual characteristics affecting the count process are the same with the individual characteristics determining the reporting process. For example, assume that the decision to commit a crime depends on the gender, age and race. However, the probability of reporting this crime would be naturally affected by the same features. Therefore, it is a quite strong assumption to *a priori* restrict a coefficient to zero. There must always be rational reasons behind our choices.

[2] Winkelmann (2008) in section 9.7 presents a brief review on theories developed for labour mobility.

model but negative in the second. Hence, identification is achieved since we reject the model in which the coefficient appears with the wrong sign. Finally, notice that sign restrictions on the count process are not appropriate as β and $\beta^* = \beta + \gamma$ can be of the same sign, so that the effect of a variable can possibly be of the same direction in both models.

1.5.2 Exclusion Restrictions on the Reporting Process

As mentioned at the beginning of this section, imposing exclusion restrictions can help identifying θ. However, it is easy to show that if exclusion restrictions are placed only on the Logit part, by restricting some elements of γ to zero, only the elements of β corresponding to the zeros in γ are identified.

Consider the case where we *a priori* know that at least one regressor belongs only to the count process. Differently, this can be considered as restricting the corresponding elements of γ to zero. When exclusion restrictions are placed on γ, vector x_{2i} can be considered as a subset of x_{1i}, so that in vector x_{1i} there is at least one variable that does not appear in x_{2i}. Assume that this set of regressors is denoted by w_i. Thus, since vector x_{1i} consists of vector x_{2i} plus vector w_i, the exclusion restrictions on the Logit part could be thought of as having added another set of regressors w_i in the Poisson part, changing the Poisson-Logit mean into $\mu_i = e^{x'_{2i}\beta + w'_i\eta}\Lambda_i$, where η consists of the parameters corresponding to the zeros in γ. Now, following the same reasoning as in (1.18) and (1.19) we have that,

$$\mu_i \equiv e^{x'_{2i}\beta + w'_i\eta}\frac{e^{x'_{2i}\gamma}}{1+e^{x'_{2i}\gamma}} = e^{x'_{2i}(\beta+\gamma)+w'_i\eta}\frac{e^{-x'_{2i}\gamma}}{1+e^{-x'_{2i}\gamma}} = e^{x'_{2i}\beta^* + w'_i\eta^*}\frac{e^{x'_{2i}\gamma^*}}{1+e^{x'_{2i}\gamma^*}} \equiv \mu_i^*. \tag{1.20}$$

Therefore, even in this case two observationally equivalent models exist where $\beta^* = \beta + \gamma$ and, $\gamma^* = -\gamma$, but $\eta^* = \eta$. It is clear that β and γ remain unidentified, since two different set of these parameters will lead to exactly the same likelihood value. In spite of this, all the elements included in η are identified, as η is identical in both μ_i and μ_i^*. Hence, unless we are interested only in η, this kind of restrictions seems inappropriate.

1.5.3 Exclusion Restrictions on the Count Process

Now assume that exclusion restrictions are placed in the Poisson part, by setting some parameters of β to zero. Consequently, x_{2i} consists of x_{1i} plus a set of regressors that corresponds to the excluded variables of the Poisson part. Let us denote this vector by q_i. Therefore, the probability of reporting an event is now given by $\Lambda(e^{x'_{1i}\gamma+q'_i\varphi})$, where φ contains the parameters in the reporting process corresponding to the restricted to zero parameters of β. Accordingly, we have:

$$\mu_i \equiv e^{x'_{1i}\beta}\frac{e^{x'_{1i}\gamma+q'_i\varphi}}{1+e^{x'_{1i}\gamma+q'_i\varphi}} = e^{x'_{1i}(\beta+\gamma)+q'_i\varphi}\frac{e^{-x'_{1i}\gamma-q'_i\varphi}}{1+e^{-x'_{1i}\gamma-q'_i\varphi}} \neq e^{x'_{1i}\beta^*}\frac{e^{x'_{1i}\gamma^*+q'_i\varphi^*}}{1+e^{x'_{1i}\gamma^*+q'_i\varphi^*}} \equiv \mu_i^*, \tag{1.21}$$

where $\beta^* = \beta+\gamma$, $\gamma^* = -\gamma$, and $\varphi^* = -\varphi$. As we notice from (1.21), the two models μ_i and μ_i^* are not observationally equivalent in this case, since the vector q_i appears in the Poisson mean of μ_i but not in μ_i^*, and identification for the whole model is achieved.

1.5.4 Specifying the Count Generating Process, as Negative Binomial 1

As mentioned before, models for count data that use the Negative Binomial distribution have been very popular as they allow for over-dispersion through the extra parameter α (or δ, in NB1 case). As presented in Section 1.3, allowing for gamma distributed unobserved heterogeneity in the Poisson-Logit model gives rise to the NB-Logit family of models. There, the two basic generalizations of the Poisson-Logit model were presented, the NB2-Logit and the NB1-Logit.

Concerning the NB2-Logit model, it is clear from (1.14) that its log likelihood depends on the regressors only through μ_i, as it is the case in the Poisson-Logit model. This is because of the homoscedastic form of the variance of the gamma distributed error term α. As a consequence, identification of the NB2-Logit model requires exactly the same conditions established for the Poisson-Logit model.

On the other hand, according to the NB1-Logit model, the variance of the error term is heteroscedastic of the form δ/λ_i. This is incorporated into the log likelihood function

(1.17), where it now depends on x_{1i} through λ_i, and on x_{2i} through Λ_i, separately. Thus, in a sense, the likelihood function of the NB1-Logit model can distinguish the count process from the reporting process, and consequently, β from γ. As a result, adopting the NB1 distribution, identification becomes possible even when both parts of the model contain the same regressors.

Nevertheless, it is very important to stress that NB1-Logit model is not a LEF and therefore, it is not robust in misspecifications of moments higher than the conditional mean. Therefore, since NB1-Logit MLE achieves identification of the mean by assuming a particular form of hetersoskedasticity of the error term, and consequently, by imposing a different structure on ω_i, the estimates of θ will be inconsistent if the variance form is misspecified.

1.5.5 Specifying the Reporting Probability as a Probit or CLogLog

Another very popular model that deals with binary choice problems is the Probit model, which exhibits nearly the same properties as the Logit model (see, Maddala, 1983). Nevertheless, assume that the correct specification for reporting a particular event is given by a Probit model instead of a Logit. According to the Probit model, $\Pr(B_{ij} = 1|x_i) = \Phi(x'_{2i}\gamma)$, where $\Phi(.)$ is the standard normal cumulative distribution function (CDF).

Given this assumption, the Poisson-Logit changes into the Poisson-Probit model with mean equal to $\mu_i = \lambda_i \Phi(x'_{2i}\gamma)$. As opposed to the Logit, the functional form of the Probit model cannot give rise to the identification problem described in Section 1.4, even when the regressors are the same in both parts of the model. This is obvious, since now $\mu_i = e^{x'_i\beta}\Phi(x'_i\gamma) \neq e^{x'_i(\beta+\gamma)}\Phi(-x'_i\gamma)$. Therefore, when the probability of reporting an event is given by a Probit, identification of the whole model is achieved.

Although less popular, the complementary log-log model (CLogLog) has also been used in the literature. Contrary to the Probit or Logit, this model assumes a non-symmetric CDF that is derived from the extreme value distribution. Therefore, according to this model $\Pr(B_{ij} = 1|x_i) = 1 - \exp(-e^{x'_i\gamma})$. As CLogLog relaxes the assumption of symmetry, it becomes more appropriate in cases where the observed average probability of the outcome is close to one or close to zero. Therefore, if there are good reasons to believe that the probability of reporting a true event is very close to one or very close to zero, a researcher could advocate that a Poisson-CLogLog model is more appropriate and use the CLogLog CDF instead of the symmetric Logit. As it is the case for the Poisson-Probit, $\mu_i = e^{x_i'\beta}(1 - \exp(-e^{x_i'\gamma})) \neq e^{x_i'(\beta+\gamma)}(1 - \exp(-e^{-x_i'\gamma}))$ and this model is identified.

1.6 Discussion

The theoretical results developed in this paper suggest that identification of the Poisson-Logit, and the NB2-Logit models is problematic, in the sense that without further parametric assumptions two identical "global" maxima exist. However, identification is achieved if true events follow the NB1 distribution, or if the reporting process is specified as a Probit model. In this section, further implications of the above results will be discussed. Moreover, some tips for researchers who intend to use the above models will be described.

As explained in subsection 1.5.1, a first way to achieve identification is by sign restrictions on the reporting process. We need to stress that this type of restriction becomes more appropriate the more certain we are about the theoretical result that determines the sign of the "restricted" coefficient. For instance, in the example of subsection 1.5.1, if information of the FS-HCA was publicly available, it could increase the wage of outside offers as well, making the change in wages differential uncertain. Moreover in practice, given correct specification of the conditional mean,[1] the effect of the "restricted" variable should be statistically significant. In fact, the more significant the effect, the more certain we are about the appeared sign in the two models. Finally, in many cases the "restricted variable" is not directly observed and therefore, the researcher is forced to use proxy variables. In the previous example, FS-HCA is not observed in practice but it can be approximated by "job experience". However, it is ambiguous whether general "job experience" captures the true effect of FS-HCA.

More interestingly, the results of subsection 1.5.3 showed that when exclusion restrictions are imposed on the count process, identification of the whole model is achieved, since there cannot be two linearly dependent sets of parameters that lead to the same likelihood value. However, even in this case, it is clear from (1.21) that the identification problem is exactly

[1] By correct specification we mean that not only should the true mean be given by $\lambda_i \Lambda_i$ but also that both processes include all the required information. That is, we do not include irrelevant variables, and we do not omit variables that must be included.

restored when $\varphi = 0$, or when the regressors excluded from the Poisson part, q_i, are perfectly collinear with the remaining elements of this vector, x_{1i}. Hence, the closer we move towards the one of these two conditions, the smaller the effect of the exclusion restriction, and the more difficult the identification becomes. Practically, we find that if the exclusion restriction is very "weak", meaning that the excluded from the count process variable has a very small effect on the reporting process, another local maximum probably exists with likelihood value very close to the global one and estimated parameters very close to $\theta^* = (\beta + \gamma, -\gamma)$. If a second maximum does exist, the estimation process, using zeros or conventional Poisson estimates for starting values in the count process, will always converge either towards the global or the local maximum. If a researcher performing the Poisson-Logit or the NB2-Logit MLE in real data is unaware of these problems, he/she may be puzzled estimating parameters with unexpected signs or implausible values. Section 1.7 will present a very comprehensive example of this situation.

Thus, although an appropriate restriction guarantees identification of θ, it is not guaranteed that the global maximum has been found. Therefore, estimation of the above models must be always accompanied by a thorough search for alternative maxima. A very useful way of searching for other candidate maxima is the following: firstly, a regression is performed using randomly chosen values for the coefficients of the reporting process and conventional Poisson or NB2 estimates for the coefficients of the count process. This helps the estimation to be smoother, avoiding possible numerical errors in the optimization procedure. Unless more problems exist, the model will converge on log likelihood value $\ln \hat{\mathscr{L}}$, corresponding to estimates $\hat{\theta} = (\hat{\beta}, \hat{\gamma})$. According to the theoretical results, the other maximum will be close to $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$. Hence, the estimated values of $\hat{\theta}^*$ can be used as starting values for a second regression. If the second maximum exists, it will be found by this second regression, with log likelihood value $\ln \tilde{\mathscr{L}}^*$ and $\tilde{\theta}^* \approx \hat{\theta}^*$. Consequently, if we find both maxima, we will accept the set of parameters that maximize the likelihood of obtaining the observed data. It would also be useful to note that sometimes, different numerical algorithms work better in different models or different data, in the sense that they perform with lower number of numerical errors and achieve convergence more easily.[1] Therefore, in case a numerical algo-

[1]Some popular algorithms available in econometric packages are: the Newton-Raphson which uses an-

rithm does not perform well, before coming to the decision that there is something wrong with our model, it would be very practical to run the same model with alternative numerical optimizers.

In footnote 2 of page 14, we mentioned that the likelihood function of the Poisson-Logit model is not always globally concave which might lead to multimodality. Therefore, not always can we be certain that only two maxima exist. In practice, there might be cases where more than two maxima exist. The method described above could not succeed in reaching a, supposedly, third local maximum. One way to reach a potential third maximum would be to use random starting values for all the parameters and to experiment with different numerical algorithms. If this is repeated many times, it is highly likely that the regression procedure will converge in every candidate maximum.

Of course a researcher, using over-dispersed data could assume that the observed data are generated by a NB1-Logit model and avoid using sign or exclusion restrictions. However, the NB1-Logit MLE is less robust than the Poisson-Logit MLE, since it does not fall within the LEF. Therefore, consistency of the estimated parameters requires not only correct specification of μ_i, but also that the data are truly generated by a NB1-Logit process. Most importantly, as mentioned in Section 1.5.3, identification is achieved by assuming a specific form of heteroskedasticity for α_i. Therefore, the estimates will be inconsistent if the variance form is misspecified.

As explained in subsection 1.5.5, the Poisson-Probit model is identified even when $x_{1i} = x_{2i}$. In spite of this, since the shape of the standard normal pdf is very similar to the logistic probability function, it is quite possible that still multiple maxima exist, whose likelihood values are very close to each other. The procedures described above can assist the researcher to check for alternative maxima as $e^{x_i'\beta}\Phi(x_i'\gamma) \simeq e^{x_i'\beta}\Lambda(x_i'\gamma/s) \simeq e^{x_i'(\beta+\gamma/s)}\Phi(-x_i'\gamma)$, where s is a scaling parameter ($\approx 3^{1/2}/\pi$, see, Maddala, 1983) used in order for the Probit and Logit parameters to be approximately the same. Finally, the Poisson-CLogLog model is also identified even when $x_{1i} = x_{2i}$. However, the empirical results (which are not presented in

alytic second derivatives and performs very well if the likelihood function is globally concave. The Bernt-Hall-Hall-Hausman (BHHH) which uses only first derivatives (outer product of the score), which results in lower computational intensity, and the Broyden-Fletcher-Goldfarb-Shanno (BFGS) which is a refinement of Davidon-Fletcher-Powell (DFP) and also uses first order derivatives. For details, see Chapter 10, in Cameron and Trivedi (2005).

Section 1.8 but are available on request) show that again a second maximum exists with likelihood value very close to the global one and estimates very close to $\hat{\theta}^*$. This might be, because conditional on vector x_i the distribution of the Logit model is similar to the distribution of the CLogLog model.

1.7 Other Related Models

Now that the conditions under which identification of the Poisson-Logit model (and small departures from it) are understood, it would be important to briefly discuss other models that are used in different contexts but either their conditional mean is specified as the Poisson-Logit's one or they face similar identification problems.

To begin with, it would be interesting to briefly discuss the connection between the Poisson-Logit model and the popular Zero-Inflation Poisson model (ZI-Poisson) as introduced by Mullahy (1986) and Lambert (1992). Although the interpretation of the Poisson-Logit for under-reporting can be very different from the interpretation of ZI models, the conditional mean in both models is specified as $\mu_i = \lambda_i \Lambda_i$, where in Poisson-Logit, Λ_i is the conditional probability of reporting an actual event, while in the ZI-Poisson model it denotes the conditional probability of having **no** Zero-Inflation.[1] However, the ZI-Poisson model is not afflicted by the identification problem discussed above because it imposes more structure in the log likelihood function (see, Lambert, 1992). Actually, the extra structure imposed on the likelihood function has as a result that it depends separately on λ_i and Λ_i and thus, we can still identify the parameters β and γ without extra restrictions. Nonetheless, this type of ZI models does not belong to the LEF and therefore, they are very sensitive to distributional misspecfications.[2]

Instead, Staub and Winkelmann (2010) propose the less parametric Poisson Quasi-

[1]According to the ZI models there are two different sources of zeroes. As a comprehensive example, consider the question "how many times you go fishing per month". A proportion of people will reply "zero times" regardless of their characteristics x, because they actually never go fishing (perhaps because they do not like it at all, or because there is not a lake, river, or sea around the area they reside so that they can use it for fishing - these are called "structural zeros"). The rest of them, who fish sometimes, will say either "zero" (incidental zeroes) or "n times" depending on whether they actually went fishing during the given period. However, it is important to note that the ZI-Poisson model can be also interpreted as a model of total under-reporting. That is, everybody that under-reports, reports exactly zero counts.

[2]Particularly, even if the mean is correctly specified, the ZI-Poisson MLE is not consistent if the DGP is given by a distribution different from the ZI-Poisson.

Likelihood (PQL) estimator that estimates the same conditional mean without making any assumptions on the exact distribution of the counts, resulting in a model that is formally identical to the Poisson-Logit. Thus, they use the results of the Pseudo-ML to advocate that consistency of this estimator only requires correct specification of the mean. As it is clear, identification of this model requires exactly the same assumptions needed for identification of the Poisson-Logit, which will be discussed in the next section.

It is also important to stress that this identification problem is not limited to count data models. Actually, it arises in any model where the conditional mean is specified as the product of an exponential function and a Logit, and there is not extra structure imposed by the researcher on the estimation procedure. Mullahy (1998), for example, motivated by the earlier work of Duan at al (1983), introduces a two-part model for non negative data (calling it Modified two-part model (M2PM)) that is applicable in both count and continuous data. According to this model, conditional on x, the probability to observe a positive value, $\Pr(y > 0|x)$, is given by a Logit, and once a non-zero outcome is observed, the expected value of the observed amount, $E(y|x, y > 0)$, is given as an exponential function that also depends on x. The conditional expectation is thus given by $\mu_i = E(y|x) = \Pr(y > 0|x)E(y|x, y > 0) = \Lambda_i\lambda_i$, which is the same as the conditional mean of the Poisson-Logit model. This model can be put into effect in applications that include a quite frequent zero "corner solution", such as the amount money spent on medical care, or expenditures in unhealthy products such as alcohol.

According to this model, the effect of x on the probability of observing a positive outcome is allowed to be different from the effect of x on the total amount, conditional on having observed a positive outcome (see, also Pohlmeier and Ulrich, 1995). Mullahy proposes two estimation procedures: 1) the two-steps estimation (M2PM-2), where in the first step a Logit is used to model $\Pr(y > 0|x)$ and in the second step $E(y|x, y > 0) = e^{x_i'\beta}$ is estimated by nonlinear least squares (NLLS), and 2) the one-step estimation (M2PM-1), where $\mu_i = E(y|x) = \Lambda_i\lambda_i$ is directly estimated using NLLS, minimizing the objective function $\sum_{i=1}^{n}(y_i - \mu_i)^2$. It is clear that in the M2PM-1, the sum of square residuals depends on θ only through μ_i and thus, the same identification problem arises. However, in M2PM-2, by estimating β and γ separately, more structure is imposed on the model and therefore, θ is identified without further restrictions.

Finally, a similar identification issue arises in a model for binary choice data that allows for missclassification probabilities developed by Hausman, Abrevaya, and Scott-Morton (1998). According to this model, $\Pr(y_i^* = 1|x_i) = \Phi(x_i'\beta)$, where y^* refers to the true but unobserved outcome, and $\Phi(.)$ denotes the Probit CDF. The true outcome, however, is subject to missclassification, where the missclassification probabilities are given by $a_0 = \Pr(y_i = 0|y_i^* = 1)$ and $a_1 = \Pr(y_i = 1|y_i^* = 0)$, where y refers to the observed outcome. In the context of misreporting, the misclassification of one as zero takes the interpretation of under-reporting, while the misclassification of zero as one takes the interpretation of over-reporting. It can be easily shown that the probability to observe an outcome is given by $\Pr(y_i = 1|x_i) = a_1 + (1 - a_0 - a_1)\Phi(x_i'\beta)$. Estimation is straightforward by ML using numerical optimizers. Hausman, Abrevaya, and Scott-Morton (1998) show that the model is not globally identified since, $a_1 + (1 - a_0 - a_1)\Phi(x_i'\beta) = \widetilde{a_1} + (1 - \widetilde{a_0} - \widetilde{a_1})\Phi(-x_i'\beta)$ where $\widetilde{a_0} = 1 - a_1$ and $\widetilde{a_1} = 1 - a_0$. Thus, there are two observationally equivalent models with parameters (a_0, a_1, β) and $(\widetilde{a_0}, \widetilde{a_1}, -\beta)$. Identification is achieved by imposing the "monotonicity" condition, which states that $a_0 + a_1 < 1$. According to this, we are able to rule out the "wrong" maximum, since $a_0 + a_1 < 1$ implies that $\widetilde{a_0} + \widetilde{a_1} > 1$. If this condition fails, the misclassification probabilities are too large, and therefore, the data are most probably too noisy to provide reasonable results.

1.8 An Illustration using Data on Labour Mobility

This section provides some examples that illustrate the theoretical results of this study. This illustration, should by no means be considered as an empirical application aiming to identify the determinants of labour mobility. It should be regarded instead as an example showing readers a practical application of the theoretical results discussed above.

This illustration uses the same data used by Winkelmann (2008) in an empirical application to labour mobility, in Chapter 9 of his monograph.[1] The original data set comes from the German Socio-Economic Panel (GSOEP) which is provided by the Deutsches Institut

[1] The data used throughout the empirical illustration have been kindly provided by Rainer Winkelman and are from the public use version of the German Socio-Economic Panel Study.

fur Wirtschaftsforschung.[1] This subsample considers 1,962 males between 25 and 50 years old in 1974. The dependent variable is the number of *Direct Job Changes*, thus, it is a pure count variable. The set of independent variables includes, *Education*$*10^{-1}$, *Experience*$*10^{-1}$, *Experience*$^2 * 10^{-2}$, *Union Membership*, *German*, *Qualified White Collar*, *Ordinary White Collar*, *Qualified Blue Collar* (excluded group being ordinary blue collar) and *Single*. These are the variables that Winkelmann also uses in his aforementioned work. For descriptive statistics and details about the data and the variables used in this empirical application refer to Winkelmann (2008).

Moreover, the model used in this illustration is also adopted by Winkelmann and Zimmermann (1993). As briefly described in Section 1.2, this model intends to identify the determinants of job offers and the probability to accept an offer by merely observing accepted job offers. That is, workers receive (unobserved) job offers, assumed to be distributed as Poisson, NB2 or NB1 variables. For every job offer a decision is taken whether to accept or reject it. If a job is accepted (successful event) it is consequently reported, where decision of acceptance is modeled as a Logit or a Probit. Therefore, given that the process of receiving (a number of) job offers is independent from the process of accepting or rejecting them, then (observed) job changes follow a stopped sum distribution given by (1.2). Consequently y_i follows the Poisson-Logit, or any other of the generalizations considered in this paper, depending on the assumptions we make on the distribution of true events and the probability of acceptance.

All the theoretical results obtained in the previous sections are being tested in the remainder of this section.

1.8.1 Same Regressors in both Processes

This subsection examines the case where both the count process (offers) and the reporting process (probability to accept an offer) are assumed to be affected by the same vector of regressors x_i. In this example, we can see in practice the identification failure described earlier. The results are given in Table 1.1. The reporting probability process, modeled as a Logit is reported in the upper part of this table, whereas the count process is reported at

[1]See, Wagner, Burkhauser and Behringer (1993) for more information.

the lower part. Robust standard errors are presented in parentheses. Results of using the Probit specification to model the reporting process are presented in subsection 1.7.4.

The identification failure is obvious. For the Poisson-Logit and NB2-Logit, two identical "global" maxima exist (same log-likelihood value). Therefore, there are two observationally equivalent models with very different parameterizations of the conditional mean. Very clearly, it can be checked that if the estimated coefficients of the one model are given by $\hat{\theta} = (\hat{\beta}, \hat{\gamma})$, the estimates of the other model are given by $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$. Thus, as far as the Logit part is concerned, we see that the coefficients of the two equivalent models have the same values but opposite signs. Regarding the count process, any $\hat{\beta}_{j(2)}$, which is the estimated coefficient of regressor j in model (2), is given by $\hat{\beta}_{j(1)}$, the estimated coefficient of the same regressor in the count process of model (1), plus $\hat{\gamma}_{j(1)}$, the estimated coefficient of the same regressor in the Logit part of model (1), and vice versa.[1]

The NB2-Logit results are presented in columns (3) and (4). We notice that exactly the same situation occurs for this model, confirming the theoretical results of subection 1.5.4. Moreover, it is also clear that NB2-Logit fits the data better than the Poisson-Logit model, given by the better log likelihood value of the NB2-Logit. This may be the result of including the extra parameter α to account for gamma distributed unobserved heterogeneity.

The most interesting results appear in model (5), where the NB1-Logit estimated coefficients are reported. This column establishes the theoretical argument of subection 1.5.3, that if a NB1 distribution is used instead of the Poisson or the NB2 one, the identification problem vanishes. Here, only one maximum seems to exist (at least one could be found after many repeated regressions and different methods), with coefficients approaching the coefficients of the first and third column (apart from the $Experience^2 * 10^{-2}$ case). Also, it is worth noting that the NB1 model exhibits the highest log likelihood value, which situation holds in all results presented in the following subsections. Nevertheless, these differences are quite small in magnitude.

Moreover, it was argued in Section 1.5 that an appropriate sign restriction solves the identification problem. For example, in subsection 1.5.1 we explained that a hypothetical theory

[1]Therefore, for example, the education coefficient at the count process of column 1 (2), which is 1.962 (-0.976), is given by the education coefficient of the count process of column 2 (1), -0.976 (1.962), plus the education coefficient of the Logit process of column 2 (1), which is 2.938 (-2.938).

of labour mobility suggests that as experience increases, the probability to accept an external job offer should decrease, so that the coefficient in the Logit process is expected to be negative. However, as can be seen from model (2), both $Experience{*}10^{-1}$ and $Experience^2 * 10^{-2}$ are positive which means that the probability to accept a job offer increases at an increasing rate. Hence, according to the "established" theoretical results we should reject model (2) and accept model (1). However, notice that these coefficients are not statistically different from zero and therefore, this sign restriction becomes less appropriate in this particular example. Of course, this is just an illustration where μ_i is probably misspecified. In real empirical applications, this kind of decisions must be based on well established theoretical results and well specified models.

1.8.2 Exclusion Restrictions on Logit

It has been established in subsection 1.5.2, that the model can be only partially identified by restricting at least one coefficient of the Logit part to zero, since only the elements of β corresponding to the zeros in γ can be identified. This situation is depicted in Table 1.2, where the constant, along with the coefficients of other five dummies in the Logit process are restricted to zero.[1]

Table 1.2 supports all theoretical results given in subsection 1.5.2. Once more, it is very interesting that two observationally equivalent models exist with $\hat{\theta} = \big((\hat{\beta}, \hat{\eta}), \hat{\gamma}\big)$ and $\hat{\theta}^* = \big((\hat{\beta} + \hat{\gamma}, \hat{\eta}), -\hat{\gamma}\big)$. Thus, the parameters in the count process that correspond to the excluded variables in the Logit part, $\hat{\eta}$, are identified as they are the same in both models. Concerning the remaining coefficients in which no exclusion restrictions have been imposed, we notice that two different but linearly dependent sets of estimates maximize the log likelihood function. Therefore, β and γ remain unidentified.

Finally, according to the results of NB1-Logit, the model is identified since only one maximum seems to exist. However, in this case, it cannot be said as before that the coefficients of NB1-Logit are in accordance with the first or the second model.

[1] It should be noted that in this example we follow the specification followed in Winkelmann and Zimmermann (1993). The results of specification (2) and (4) are the results presented in Table 1.2 (overlapping case) in Winkelmann and Zimmermann (1993). However as opposed to their study, here we use robust (Pseudo-ML) standard errors.

1.8.3 Exclusion Restrictions on Count Process

We have seen in subsection 1.5.3 that one way to identify all elements of θ is to restrict at least one coefficient of the count process to zero. However, it is still possible that local maxima exist. In the few next pages two different cases are presented. In the first one, the variable excluded from the count process has a very small effect on probability of accepting a job offer, so that it is a "weak" exclusion. On the other hand, the second case shows the situation where a "strong" exclusion restriction is imposed, in the sense that the excluded variables' impact on the probability of acceptance is large.

1.8.3.1 Excluding a very Insignificant Variable from the Count Process

It has been argued in subsection 1.5.2, that by excluding a variable from the count process identification is achieved, yielding estimates $\hat{\theta} = (\hat{\beta}, \hat{\gamma})$. However, if all elements of φ approach zero, a second maximum will possibly exist with estimates $\tilde{\theta}^* = (\tilde{\beta}^*, \tilde{\gamma}^*)$ very close to $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$. Suppose now, that the correct specification of the mean is given by including the *Ordinary White Collar* variable to the already existed regressors of subsection 1.8.1. Furthermore, assume that this variable belongs only to the reporting process so as it can be excluded from the count process. Moreover, note that *Ordinary White Collar* has a very small effect on the acceptance decision.

The results, presented in Table 1.3, are a good illustration of the situation explained before. When *Ordinary White Collar* is excluded from the count process, two maxima still exist with log likelihood values very close to each other. Moreover, it is remarkable how close the estimates of the second maximum are to $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$. However, as the estimated parameters that maximize the likelihood of having obtained the observed data are obtained by model (1), the second model should be rejected. The results for NB2-Logit reinforce these findings. Finally, it is interesting that the maximum of the NB1-Logit model gives estimates with values closer to the ones of the accepted model.

1.8.3.2 Excluding Dummies from the Count Process with Large Effect on Logit Process

Contrary to the previous case, the example in this subsection shows how the identification problem is suppressed when the excluded variables have large coefficients in the Logit process. Here we examine the effect of excluding the dummies *Unionist* and *German*. Although there are reasons to believe that these regressors should have been included in the count process, in this illustration we assume that they belong only to the Logit part. The results are presented in Table 1.4.

According to the findings of the Poisson-Logit, being a *Unionist*, or *German* significantly decreases the probability of accepting a job offer. In contrast, although larger in magnitude, these dummies are very imprecisely estimated in the NB2-Logit. In both cases, however, we can see that although a second maximum exists, its log likelihood value is much smaller than the one of the global maximum. Thus, according to these results we accept the maxima of models (1) and (3). Recall that this is the model that we accept when we exclude *Ordinary White Collar* from the count process. Furthermore, for both Poisson-Logit and NB2-Logit, the estimates of the second maximum, $\tilde{\theta}^* = (\tilde{\beta}^*, \tilde{\gamma}^*)$, are far away from $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$. Finally, the estimates of the NB1-Logit model are more similar to the estimates of the accepted models. Thus, it is important for the researcher to perform a thorough search of alternative maxima, as local maxima may still exist.

1.8.4 Specifying the Reporting Process as a Probit

In this section we illustrate the results of assuming the probability of accepting a job offer to be given by a Probit. As noted in subsection 1.5.5, we are able to identify all elements of θ in both the Poisson-Probit and NB2-Probit models, even when $x_{1i} = x_{2i}$. However, it is still possible that at least a second maximum exists with likelihood value close to the ones from the Poisson-Logit model. This situation is depicted in Table 1.5, where the same regressors as in subsection 1.8.1 are considered.

First of all, from Table 1.5, we can see that still at least two maxima exist. Basically, for this particular specification of the mean, several maxima exist in the neighborhood of the maximum of models (2) and (4). Nevertheless identification is achieved, since the maxima in models (1) and (3) have the largest log likelihood values. It can also be seen that the difference between the log likelihood values of the two models is very small. Furthermore, a quite interesting result from Table 1.5 is that the estimated coefficients of the local maximum cannot be associated to $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$.[1] Finally, NB1-Probit still fits the data better than the NB2-Probit.

The findings in this table suggest that we should accept models (1) and (3) which are similar to the models (2) and (4) from Table 1.1. Nonetheless, it would be more appropriate to compare the Poisson-Probit and NB-Probit models with their corresponding Logit models when we use a specification that guarantees identification of the Poisson-Logit or NB2-Logit models. We consider, for example, the estimates when we exclude the dummies *Unionist* and *German* from the count process when the reporting process is modeled as a Probit, and compare them with the results from Table 1.4, where the same exclusion restrictions are considered in Poisson-Logit and NB-Logit models. Table 1.6 shows that only one maximum for each model seems to exist. In this case, we can see that the estimates of the Poisson-Probit and NB2-Probit MLE are similar to the ones of the accepted models (1) and (3) of their Logit counterparts from Table 1.4. Moreover, the NB1-Probit results are quite close to the results of the NB1-Logit. However, this by no means should be considered as an indicator that the aforementioned models provide similar results under similar conditions. Further research must be done to shed light on the properties of the count data models that specify the reporting process as a Probit.

1.9 Conclusion

This paper investigates the conditions under which the Poisson-Logit and other simple modified models for under-reported count data are identified. The theoretical results reveal that it is impossible to identify the parameters of the Poisson-Logit model, unless further para-

[1] Results of considering different specifications (in terms of regressors used) for the mean of the Poisson-Probit and NB2-Probit models (however, with the same data), showed that in all cases that local maxima exist, their coefficients have no relationship with $\hat{\theta}^* = (\hat{\beta} + \hat{\gamma}, -\hat{\gamma})$ even after taking into account that the coefficients of the Probit model can be approximated by rescaled Logit coefficients (look at last paragraph of Section 1.6).

metric assumptions are imposed. A first way to identify this model is to assume that at least one regressor does not affect either the reporting or the count process. Although partial identification is achieved when exclusion restrictions are imposed only on the reporting process, the whole model is identified whenever we exclude at least one variable from the count process. However, these variables must not have a zero coefficient in the Logit part and neither can they be perfectly collinear with the remaining regressors of this vector. If such a regressor is not available, sign restrictions on at least one parameter of the reporting process can be used. This restriction must be based on rational choice, for example, by considering established economic theories.

Two basic extensions of the Poisson-Logit model that take into account possible overdispersion have also been presented. As this study shows, in order to identify the NB2-Logit model we require exactly the same conditions established for the Poisson-Logit. On the other hand, identification of the NB1-Logit model seems easier, since the different specification of the variance disentangles the effect of the regressors on the mean of the count process and on the probability of reporting. However, in this case identification of μ_i is achieved by assuming a different parametric specification for ω_i. This has obvious consequences for the robustness of the NB1-Logit MLE. Finally, it has been noted that the identification problems of the Poisson-Logit and NB2-Logit do not extend to models where $\Pr(B_{ij} = 1|x_i)$ is not of the Logit form, like in a Poisson-Probit model.

Nevertheless, although under further parametric assumptions identification is achieved, it is still possible that multiple maxima exist. Therefore, an estimation of the above models must be accompanied by a thorough search for alternative maxima. Otherwise, we are not able to know whether a global or a local maximum has been found. For this reason, in Section 1.6 few methods assisting a practitioner that uses the above models to find alternative maxima have been proposed. Finally, an empirical application to labour mobility has illustrated all the theoretical results of this paper.

Table 1.1. Modelling Reporting Probability as a Logit

Same Regressors in both Processes

Y= Number of Direct Job Changes	Poisson - Logit (1)	(2)	NegBin2 – Logit (3)	(4)	NegBin1–Logit (5)
Logit Process					
Constant	3.439***	-3.439***	3.327***	-3.327***	2.696
	(1.075)	*(1.075)*	*(1.059)*	*(1.059)*	*(3.515)*
Education*10^{-1}	-2.938	2.938	-2.798	2.798	-1.709**
	(2.263)	*(2.263)*	*(2.462)*	*(2.462)*	*(0.806)*
Experience*10^{-1}	-0.791	0.791	-0.661	0.661	-0.686
	(1.254)	*(1.254)*	*(1.319)*	*(1.319)*	*(1.196)*
Experience^2*10^{-2}	-0.362	0.362	-0.449	0.449	0.126
	(0.538)	*(0.538)*	*(0.593)*	*(0.593)*	*(0.256)*
Count Process					
Constant	-1.451	1.988	-1.317	2.010	-0.078
	(2.005)	*(1.455)*	*(2.120)*	*(1.571)*	*(0.677)*
Education*10^{-1}	1.962	-0.976**	1.842	-0.957**	0.538
	(2.610)	*(0.462)*	*(2.803)*	*(0.445)*	*(0.453)*
Experience*10^{-1}	-0.464	-1.255*	-0.630	-1.292*	-0.489
	(1.013)	*(0.655)*	*(0.976)*	*(0.743)*	*(0.337)*
Experience^2*10^{-2}	0.549	0.187	0.643	0.196	0.056
	(0.455)	*(0.123)*	*(0.495)*	*(0.134)*	*(0.099)*
α^{-1} (Negbin2-Logit)	-	-	0.705	0.705	0.610
δ^{-1} (Negbin1-Logit)			*(0.084)*	*(0.084)*	*(0.502)*
Log Likelihood	-2,058.99	-2,058.99	-1,888.99	-1,888.99	-1,882.34
N	1,962	1,962	1,962	1,962	1,962

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 1.2. Modelling Reporting Probability as a Logit

Exclusion Restrictions in Logit Process

Y= Number of Direct Job Changes	**Poisson – Logit** (1)	(2)	**NegBin2 – Logit** (3)	(4)	**NegBin1 – Logit** (5)
Logit Process					
Education*10^{-1}	-3.619	3.619	-3.784	3.784	-0.701***
	(2.261)	*(2.261)*	*(2.986)*	*(2.986)*	*(0.169)*
Experience*10^{-1}	5.715	-5.715	5.981	-5.981	-0.173
	(4.458)	*(4.458)*	*(5.490)*	*(5.490)*	*(0.280)*
Experience^2*10^{-2}	-3.102	3.102	-3.240	3.240	0.031
	(2.483)	*(2.483)*	*(3.036)*	*(3.036)*	*(0.083)*
Count Process					
Constant	0.823	0.823***	0.929***	0.929***	0.825***
	(0.265)	*(0.265)*	*(0.313)*	*(0.313)*	*(0.243)*
Education*10^{-1}	3.291	-0.328*	3.429	-0.354	0.984***
	(2.317)	*(0.198)*	*(3.049)*	*(0.233)*	*(0.416)*
Experience*10^{-1}	-6.380	-0.665***	-6.704	-0.723	-0.527
	(4.472)	*(0.197)*	*(5.503)*	*(0.170)*	*(0.392)*
Experience^2*10^{-2}	3.172	0.070	3.323	0.082	0.069
	(2.501)	*(0.067)*	*(3.054)*	*(0.056)*	*(0.118)*
Unionist	-0.292	-0.292***	-0.308***	-0.308***	-0.272***
	(0.094)	*(0.094)*	*(0.082)*	*(0.082)*	*(0.079)*
German	-0.397***	-0.397***	-0.422***	-0.422***	-0.342***
	(0.130)	*(0.130)*	*(0.097)*	*(0.097)*	*(0.104)*
Qualified White Collar	0.069	0.069	0.036	0.036	-0.020
	(0.196)	*(0.196)*	*(0.174)*	*(0.174)*	*(0.165)*
Ordinary White Collar	0.179	0.179	0.184	0.184	0.188
	(0.193)	*(0.193)*	*(0.229)*	*(0.229)*	*(0.174)*
Qualified Blue Collar	0.133	0.133	0.114	0.114	0.066
	(0.117)	*(0.117)*	*(0.112)*	*(0.112)*	*(0.106)*
α^{-1} (Negbin2-Logit) δ^{-1} (Negbin1-Logit)	-	-	0.735	0.735	0.200
			(0.066)	*(0.066)*	*(0.082)*
Log Likelihood	-2,039.40	-2,039.40	-1,875.95	-1,875.95	-1,869.62
N	1,962	1,962	1,962	1,962	1,962

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 1.3. Modelling Reporting Probability as a Logit

Excluding an Insignificant Dummy ("Ordinary White Collar") from Count Process

Y= Number of Direct Job Changes	**Poisson – Logit** (1)	(2)	**NegBin2 – Logit** (3)	(4)	**NegBin1–Logit** (5)
Logit Process					
Constant	3.420***	-3.415***	3.313***	-3.271***	2.638
	(1.048)	*(1.088)*	*(1.024)*	*(1.031)*	*(3.641)*
Education*10-1	-2.877	2.951	-2.751	2.729	-1.695***
	(2.316)	*(2.357)*	*(2.423)*	*(2.485)*	*(0.830)*
Experience*10-1	-0.813	0.722	-0.680	0.602	-0.688
	(1.257)	*(1.411)*	*(1.313)*	*(1.443)*	*(1.229)*
Experience2*10-2	-0.352	0.388	-0.442	0.466	0.128
	(0.547)	*(0.610)*	*(0.592)*	*(0.880)*	*(0.262)*
Ordinary White Collar	0.082	0.231	0.094	0.287	0.104
	(0.204)	*(1.073)*	*(0.194)*	*(0.880)*	*(0.312)*
Count Process					
Constant	-1.395	1.966	-1.273	2.030	-0.067
	(2.047)	*(1.496)*	*(2.090)*	*(1.658)*	*(0.724)*
Education*10-1	1.887	-0.973**	1.783	-0.963**	0.539
	(2.683)	*(0.470)*	*(2.770)*	*(0.456)*	*(0.455)*
Experience*10-1	-0.460	-1.236*	-0.628	-1.293	-0.484
	(0.983)	*(0.686)*	*(0.953)*	*(0.796)*	*(0.345)*
Experience2*10-2	0.543	0.183	0.639	0.192	0.054
	(0.456)	*(0.130)*	*(0.490)*	*(0.143)*	*(0.101)*
α^{-1} (Negbin2-Logit)	-	-	0.706	0.705	0.601
δ^{-1} (Negbin1-Logit)			*(0.084)*	*(0.084)*	*(0.522)*
Log Likelihood	-2,058.87	-2,058.90	-1,888.89	-1,888.91	-1,882.29
N	1,962	1,962	1,962	1,962	1,962

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 1.4. Modelling Reporting Probability as a Logit

Excluding "Unionist" and "German" from the Count Process

Y= Number of Direct Job Changes	**Poisson – Logit** (1)	(2)	**NegBin2 – Logit** (3)	(4)	**NegBin1-Logit** (5)
Logit Process					
Constant	3.897***	-6.547***	4.007**	-4.269***	6.317
	(0.756)	*(2.196)*	*(1.671)*	*(0.798)*	*(4.850)*
Education*10^{-1}	-2.234	2.681***	-1.193	-0.879*	-1.562**
	(1.788)	*(0.933)*	*(3.577)*	*(0.490)*	*(0.603)*
Experience*10^{-1}	-0.298	1.323	-1.005	-0.209	-1.251
	(1.337)	*(1.137)*	*(2.929)*	*(0.480)*	*(1.133)*
Experience^2*10^{-2}	-0.421	-0.150	-0.221	0.038	0.138
	(0.594)	*(0.257)*	*(0.677)*	*(0.134)*	*(0.252)*
Unionist	-0.461**	-0.315***	-0.557	-0.300***	-0.820**
	(0.202)	*(0.107)*	*(0.350)*	*(0.092)*	*(0.361)*
German	-0.764**	-0.351***	-0.955	-0.333***	-2.312
	(0.375)	*(0.133)*	*(0.762)*	*(0.117)*	*(3.429)*
Count Process					
Constant	-1.015	6.652***	-0.324	4.901***	-0.328
	(1.413)	*(2.255)*	*(2.296)*	*(0.701)*	*(0.352)*
Education*10^{-1}	1.403	-2.404***	0.542	0.722**	0.448
	(1.839)	*(0.776)*	*(2.686)*	*(0.433)*	*(0.447)*
Experience*10^{-1}	-0.978	-1.994*	-0.870*	-0.579	-0.537***
	(0.800)	*(1.103)*	*(0.480)*	*(0.422)*	*(0.197)*
Experience^2*10^{-2}	0.567	0.269	0.475	0.077	0.107*
	(0.517)	*(0.229)*	*(0.338)*	*(0.122)*	*(0.064)*
α^{-1} (Negbin2-Logit)	-	-	0.754	0.004	0.888
δ^{-1} (Negbin1-Logit)			*(0.090)*	*(0.000)*	*(0.263)*
Log Likelihood	-2,027.18	-2,045.04	-1,868.97	-1,870.95	-1,864.02
N	1,962	1,962	1,962	1,962	1,962

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 1.5. Modelling Reporting Probability as a Probit

Same Regressors in both Processes

Y= Number of Direct Job Changes	Poisson - Probit (1)	(2)	NegBin2 – Probit (3)	(4)	NegBin1-Probit (5)
Probit Process					
Constant	-2.134***	-2.403	-2.059***	-1.353	0.544
	(0.660)	*(3.717)*	*(0.617)*	*(1.610)*	*(9.165)*
Education*10^{-1}	1.820	-0.590	1.707	-0.398	-0.733
	(1.540)	*(0.469)*	*(1.574)*	*(0.380)*	*(1.877)*
Experience*10^{-1}	0.513	-0.978	0.440	-1.354	-0.229
	(0.823)	*(0.748)*	*(0.873)*	*(1.208)*	*(1.333)*
Experience^2*10^{-2}	0.226	0.020	0.276	0.018	0.046
	(0.337)	*(0.053)*	*(0.356)*	*(0.035)*	*(0.237)*
Count Process					
Constant	1.992**	4.378	2.039	2.200	0.496
	(0.660)	*(9.693)*	*(1.681)*	*(2.722)*	*(6.266)*
Education*10^{-1}	-0.977*	2.228	-0.962**	1.053	0.537
	(1.540)	*(3.996)*	*(0.443)*	*(1.643)*	*(0.733)*
Experience*10^{-1}	-1.279	2.684	-1.328	2.355	-0.536
	(0.823)	*(1.863)*	*(0.863)*	*(0.592)*	*(0.550)*
Experience^2*10^{-2}	0.194	0.394	0.202	0.811	0.059
	(0.337)	*(0.832)*	*(0.157)*	*(1.527)*	*(0.116)*
α^{-1} (Negbin2-Probit)	-	-	0.705	0.703	0.360
δ^{-1} (Negbin1-Probit)			*(0.084)*	*(0.084)*	*(2.379)*
Log Likelihood	-2,058.88	-2,060.21	-1,888.91	-1,889.61	-1,882.69
N	1,962	1,962	1,962	1,962	1,962

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 1.6. Modelling Reporting Probability as a Probit

Excluding "Unionist" and "German" from the Count Process

Y= Number of Direct Job Changes	**Poisson–Probit** (1)	**NegBin2–Probit** (2)	**NegBin1–Probit** (3)
Probit Process			
Constant	2.840*	3.246	4.034
	(1.590)	*(2.157)*	*(2.852)*
Education*10^{-1}	-0.773	-0.637	-0.852**
	(0.584)	*(0.449)*	*(0.354)*
Experience*10^{-1}	-0.778	-0.994	-0.833
	(0.642)	*(0.686)*	*(0.694)*
Experience^2*10^{-2}	0.027	0.082	0.095
	(0.146)	*(0.182)*	*(0.153)*
Unionist	-0.415*	-0.465**	-0.525***
	(0.225)	*(0.235)*	*(0.197)*
German	-0.742	-0.944	-1.563
	(0.669)	*(1.041)*	*(2.109)*
Count Process			
Constant	-0.326	-0.207	-0.268
	(0.627)	*(0.443)*	*(0.302)*
Education*10^{-1}	0.447	0.279	0.369
	(0.761)	*(0.478)*	*(0.354)*
Experience*10^{-1}	-0.547**	-0.533**	-0.545***
	(0.267)	*(0.250)*	*(0.185)*
Experience^2*10^{-2}	0.202	0.173	0.109*
	(0.150)	*(0.165)*	*(0.061)*
α^{-1} (Negbin2-Probit)	-	0.750	0.923
δ^{-1} (Negbin1-Probit)		*(0.089)*	*(0.215)*
Log Likelihood	-2,029.79	-1,869.80	-1,864.23
N	1,962	1,962	1,962

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Chapter 2

The Relationship between Immigration Status and Criminal Behaviour

2.1 Introduction

Academic debates on the relationship between immigration and crime date back at late 1800s and early 1990s (see, for example, Hart, 1986, Hourwich, 1912, and Taft, 1933), following large inflows of European and Canadian citizens into the United States. It seems that the native born population of countries that sustained heavy migration inflows always developed hostile feelings against foreigners. This can be attributed to the fact that natives always feared that immigrants could take away their jobs and deteriorate several problems of the host countries, including crime. Although the bulk of the media in the host countries supported and even strengthened this hostile feelings against immigrants, researchers' community often concluded the opposite. Many found evidence that immigrants seem to commit fewer crimes than natives, even though they usually encounter unfavorable circumstances, such as blocked opportunities, or acculturation problems (Tonry, 1997, Hagan and Palloni, 1998, and Mears, 2001).

The present study attempts to investigate the relationship between immigration and crime in England and Wales.[1] In spite of the fact that the immigration-crime link is such a controversial subject, it is generally overlooked by the research community compared to other aspects of crime. To my knowledge, apart from the very recent study by Bell, Machin and Fasani (2010), there is not any other study that investigates whether such a relationship exists in the UK.[2]

As figure 2.1 demonstrates, England and Wales have recently experienced a steady increase in immigration stock relative to the total population. It is notable that, as explained by Hatton and Tani (2005), immigration net flows were responsible for about one half of the population growth during the 90's. Moreover, Hatton (2005) explains that after reaching the minimum between 1993 and 1995, immigrant net flows increased until 2000, reaching the figure of 100,000 individuals per year. After 2000, the proportion of immigrants increased even more, because of the large inflow of around 560,000 Eastern European workers between 2004 and 2006.[3]

Following this increase in foreign population, immigration in the UK became a very controversial subject, and one of the "hottest" topics in political agenda. During the last two decades natives have developed negative beliefs against their immigrant counterparts, with regard to labour market outcomes, cultural issues, and crime. This hostile tendency is quite clear if we look at the UK sample of two very important attitudes surveys, the European Social Survey (ESS) of 2002, and the International Social Survey Programmes (ISSP) of 1995 and 2003, where questions related to immigration and crime were included.

According to the ESS (see, Table 2.1) there is a clear tendency towards the perception that immigrants have worsened UK's crime rates. Even more interesting findings come from the ISSPs (Table 2.2). Although in ISSP of 1995 only 26% of the respondents believe that immigrants increase crime rates (agree and strongly agree), this figure increases to around 40% in 2003. These findings coincide with the increase in population of immigrants. It is

[1]Scotland and Northern Ireland are excluded from the survey used to investigate this research question because of their separate criminal and justice system, which generates incomparable crime statistics.

[2]However, there is a crucial difference between the present study and the study of Bell, Machin and Fasani (2010); that is, the present study looks at this relationship from a micro perspective, examining whether immigrants are more prone to criminal activities than natives, whereas their study focuses on the effect of two waves of large inflows of immigrants on crime rates.

[3]This evidence comes from the Worker Registration Scheme and National Insurance Number applications (see, Gilpin at al, 2006, Blanchflower at al, 2007, and Lemos and Portes, 2008).

quite interesting that natives developed negative beliefs about immigrants despite the fact that crime rates (at least for total crime) started falling after 1995 (see, figure 2.1 and 2.2. Also see, Smith, 2006, and Kershaw and al, 2008, p.2). To reinforce these findings, the results of an ordered Probit regression model are presented in Table 2.3, where a simple dummy for year 2003 aims to capture the evolution in natives' attitudes, once we have pooled the data from ISSP 1995 and ISSP 2003. This is done because we recognize that the sample in 1995 might differ in many aspects from the sample in 2003. It is clear that even after controlling for some basic characteristics correlated with respondents' attitudes, such as education, party affiliation, income, gender, and age, moving from 1995 to 2003 strongly increased the sentiment that immigrants increase crime rates.[1]

But which are the theoretical reasons that link immigration with crime? There are two distinct effects of immigration on crime. The first one, that I call the "aggregate" effect or macro effect, states that immigration inflows are related to economy's crime rates as they: 1) can affect aggregate outcomes of the domestic economy, such as wages and unemployment, and 2) may impose cultural conflicts and social disorganization according to criminological theories (see, for example, Martinez and Lee, 2000). The second one, which I call the "individual" or micro effect, is the direct effect. This states that immigrants are more or less crime-prone than natives for reasons that are described in detail in the next section.

This paper attempts to shed light on the direct relationship by investigating the question: *Are immigrants more or less involved in criminal activities than natives and why?* It should be also stressed that this work focuses on property crime, which can be better explained by economic theory, as opposed to violent crime, which is better explained by psychological factors rather than material needs. Therefore, psychological theories developed basically by criminologists, rather than economic theory, would stand better to explain violent crime.[2] In the next section a simple economic model of crime is presented to investigate the direct

[1] The base group for the variable "party" is "left-wing". For "education" the base group is "high education". The marginal effects on the "year 2003" dummy, which are not presented here but are available from the author upon request, show that the probability of responding with "agree that immigrants increased crime rates" increased by 7.3 percentage points from 1995 to 2003, and the probability of responding with "strongly agree..." by 5.9 percentage points. These differences, calculated using the 'nlcom' command in Stata®, are statistically significant at 1% significance level. Note that the standard errors are calculated using the delta method.

[2] However, as the explanatory variables used to the empirical analysis may also determine the decision to commit violent crimes, the results of a violent crime model are presented in subsections 2.8.2-2.8.4

immigration-crime relationship.

For the purposes of the empirical analysis individual level crime data are needed. A first choice would be to use recorded by police crime and compare the crime records of immigrants to those of natives. However, data on recorded crime in England and Wales are very poor in terms of information provided, and therefore, are inappropriate for an individual empirical investigation. Most importantly, these data provide no information on the immigration status of criminals. In Section 2.4, other shortcomings of this kind of data are explained. In another direction, self-reports on crime can be used, a practice that is very common among criminologists and sociologists (see, Junger-Tas and Marchall, 1999). Therefore, in the present study the Offending, Crime and Justice Survey (OCJS) of 2003 is used, a nationally representative survey that asks people in England and Wales about their experiences and attitudes towards criminal activities (Hamlyn at al, 2003).[1] Not only does this kind of surveys reveal to some extent unreported/unrecorded to the police crime, but it also provides a rich set of respondents' attributes which enables the investigation of the determinants of criminal behaviour.

Of course, to identify these relationships it is required that respondents truthfully reveal their criminal activity. Nevertheless, reliability of self-reports on crime is a major concern as many individuals may be reluctant to provide sincere answers to questions related to such sensitive activities. Therefore, under-reporting is a major concern, although nowadays many techniques are used to improve the reliability and validity of these data (Thornberry and Krohn, 2000). From the econometric point of view, estimators that ignore under-reporting are inconsistent (see, for example, Hausman, Abrevaya and Scott-Morton, 1998, for binary outcome models, and Winkelmann and Zimmermann, 1993, for count data models). Therefore, it is highly possible that they provide misleading estimates for the coefficients of interests. This problem becomes even more salient if differences in respondents' characteristics are associated with different reporting behaviour (so that under-reporting is not random), and more importantly, if immigrants' reporting behaviour differs from natives' one. Nevertheless, more appropriate econometric models incorporating this problem can be developed

[1] The OCJS data used in this Chapter are sponsored by the Home Office and provided by the UK Data Archive.

and applied. Given that these models are correctly specified, they consistently estimate the determinants of true crime by using only data of self-reported crime. Section 2.5 presents in detail the issue of econometric modeling.

Initially, we treat immigrants as a homogeneous group of people. However, this may not be proper for plenty of reasons. For example, immigrants of different ethnic backgrounds might be very different from each other. Furthermore, location of immigrants is not randomly assigned, but it is a rather complicated process that depends on many factors. For example, if immigrants try to match their abilities with the opportunities that each area provides, more crime-prone immigrants would decide to reside in areas that offer more criminal opportunities. Or, as the location of immigrants also depends on central decisions, it might be that different kind of immigrants are located in areas characterized by different socioeconomic features. For example, following the 1999 Immigration and Asylum Act, asylum seekers were located by the National Asylum Support Service in specific areas, London being excluded (see, Bell, Machin and Fasani, 2010). Given the facts above, the estimated effect of immigrant status on criminal behaviour might be misleading and therefore, the effect of immigration on crime is also investigated once we decompose immigrants by ethic group and location.

The remainder of this paper is organized as follows. Section 2.2 puts the individual decision to commit property crimes in a simple economic framework of individual supply of crime. Utilizing this simple economic model, it also investigates the individual relationship between immigration and property crime. In Section 2.3 a very brief review of studies on this topic is presented. In Section 2.4 some methodological issues of self-reports are discussed. Section 2.5 offers a presentation of the econometric models that are more appropriate in the presence of under-reporting. Section 2.6 discusses the data and the variables and offers some basic descriptive statistics. The main results follow in Section 2.7. In Section 2.8 robustness of these results is checked. Section 2.9 investigates whether the immigrant-native property crime differentials depend on ethnic status or the regions they reside. Finally, discussion of the empirical results follows in Section 2.10, and Section 2.11 concludes.

2.2 An Economic Model of Property Crime

As discussed in the introduction, this study investigates whether immigrants are different from natives with regard to their behaviour towards criminal activities, and particularly towards property crime. Therefore, in this section the individual relationship between immigration and property crime is examined. We start with a general examination of a simple model of property crime. In the next subsection we examine how immigration status is associated with this model, and consequently what this model predicts about criminal activity of immigrants compared to natives.

Generally, the economic theory of crime is based on the idea of the rational individual who chooses how to allocate his/her time between legitimate and illegitimate activities so as to maximize his/her personal expected utility. As there is a probability of apprehension, the final outcome of the criminal act is uncertain.

Becker (1968) has offered the first prominent paper to incorporate economic theory on the analysis of criminal behaviour. However, in this early work illegal and legal activities were considered as mutually exclusive. A few years later, Ehrlich (1973), in a cornerstone work, relaxed this assumption so as individuals are utility maximizers who allocate their time between crime and work. In another vital work, Block and Heineke (1975) criticize the two previous works on the grounds that they treat crime and punishment outcomes as if they can be always represented by their pecuniary equivalents. Using a more general, multiattributed utility function, they show that the results of Becker's and Ehrlich's works hold only under very special conditions, and that determining the supply of property crime is a harder task that needs further assumptions.[1] From then on, many other economic theoretical models have been developed[2] and tested using micro or macro data.[3] It needs to be stressed that in general, property crime fits better in the economic models of crime, since violent crimes can be considered as non market activities that are primarily motivated by hate or passion

[1] The utility function is given as consisting of three attributes, $U(L,T,W)$, (where L and T are, time spent on legal and illegal activities respectively, and W represents wealth), rather than a wealth only function.

[2] Cameron (1988) and Eide (1999) are good surveys on this topic. Freeman (1999) is also an excellent survey that discusses many aspects surrounding the economic theory of crime from both a theoretical and an empirical perspective.

[3] See, for example, Sjoquist (1973), Woplin (1978), Witte (1980), Myers (1983), Reilly and Witt (1996), Cornwell and Trumbull (1994), and Kelly (2000), to mention only a few.

(Ehrlich, 1973).[1]

Economic models of crime have been extended beyond the classical theory of crime deterrence, particularizing in examining relationships such as investment in human capital and crime (Lochner, 2004, Lochner and Moretti, 2004), inequality and crime (Chiu, Madden, 1998), the effect of economic incentives on crime (Machin and Meghir, 2004), crime and unemployment (Burdett, Lagos and Wright, 2003), crime and social interactions (Glaeser, Sacerdote and Scheinkman, 1996), etc. Nevertheless, there is no theoretical framework that investigates the relationship between immigration and crime. For this reason, the following subsection presents a simple model that incorporates immigration with the purpose of demonstrating why immigrants might exhibit different criminal behaviour than natives.

To make it as simple as possible, the following model is a one period model under uncertainty that borrows features from Ehrlich (1973), and Lochner and Moretti, (2001). This model is by no means a complete investigation of criminal behaviour, but it illustrates quite well why someone would expect differences in participation rates of illegitimate activities between immigrants and natives.

Consider a rational individual who, after receiving the initial endowment z, optimally decides how to allocate his/her total time available, τ, between criminal activity, τ_i, and work, τ_ℓ. We assume that leisure time, where the individual consumes all his/her outcomes, is constant and therefore does not affect the results of the model.[2] Although in general z can represent other individual characteristics such as, age, gender, parental features, respondent's location features, etc., in this model z is an indicator variable that determines immigration status. In turn, z is assumed to affect most of the parameters of this model.

Uncertainty is incorporated in the model because of two reasons. First, there is a probability of apprehension, $\pi(\tau_i, z)$, in case the individual is involved in criminal activities. Second, legal outcomes are also not certain because there is a probability of unemployment, $\mu(z)$, which is assumed to be given exogenously at the beginning of the period.

If the individual is employed in the legal sector, he/she receives wage $w(\tau_\ell, z)$. This legal

[1]However, as will be clear later, some factors determining property crimes, such as probability of apprehension, severity of punishment and risk aversion, are directly associated with violent crime as well.

[2]Without loss of generality, τ can be considered as the time available for allocation between the legal and illegal activities after extracting leisure time from total available time.

wage depends positively on τ_ℓ, such that $dw(\tau_\ell, z)/d\tau_\ell > 0$, and $d^2w(\tau_\ell, z)/d\tau_\ell^2 < 0$. The latter can be assumed to be negative as productivity and efficiency may decrease as more time is spent on work. On the other hand, if unemployed he/she receives an unemployment benefit $D(\tau_\ell)$, which is the same for immigrants and natives, but depends linearly on τ_ℓ, so that $dD(\tau_\ell)/d\tau_\ell > 0$ and $dD^2(\tau_\ell)/d\tau_\ell^2 = 0$. Thus, this benefit is acquired only as long as the individual spends time on legal sector, such as time on looking for a new job (which time must be then reported) and increases with τ_ℓ. Also it is assumed that $\underline{w}(\tau_\ell, z) > D(\tau_\ell)$ and $d\underline{w}(\tau_\ell, z)/d\tau_\ell > dD(\tau_\ell)/d\tau_\ell$, where $\underline{w}$ is the minimum wage rate.[1]

Apart from legal opportunities, the individual also faces illegal opportunities, given by $k(\tau_i, z)$, which consists of financial and psychological (mental) outcomes measured in their pecuniary equivalent.[2] Apart from financial and psychological gains, $k(\tau_i, z)$ also includes some costs (measured in their pecuniary value) associated with a crime, such as bad reputation, compunction, regrets, uneasiness, etc. The costs of trial, conviction and punishment, are not included in these costs but, as will be shown shortly, they will be introduced as distinct components of the utility function. Similarly to the legal wage, "criminal wage" also depends continuously on τ_i, with $dk(\tau_i, z)/d\tau_i > 0$, and $d^2k(\tau_i, z)/d\tau_i^2 < 0$. Thus, when the individual enters the market he/she considers a continuous set of illegal opportunities. We assume that illegal opportunities that pay high pecuniary returns require considerable time in the illegal sector and that they also involve higher psychological costs.[3]

There are two states of nature, the good State A, where someone is employed with probability $1 - \mu(z)$, and the bad State B, where someone is unemployed with probability $\mu(z)$. The corresponding returns from legal and illegal actions in the good and the bad states

[1] Otherwise, there could exist cases where it would be optimal for the individual to remain unemployed, so that the probability of being unemployed would be endogenous, and the model would have been more complicated.

[2] By pecuniary equivalent we mean the amount of money that someone is willing to pay in order to get this gain or to avoid a cost.

[3] Therefore, regardless of psychological costs, someone who pursuits high returns to illegal actions can either commit many crimes, or one high value crime which requires much time spent in the illegal sector though. This can be the case, as this type of property crimes requires much time for organization, preparation, etc.

respectively are the following,

$$y_a = w(\tau_\ell, z) + k(\tau_i, z),$$
$$y_b = D(\tau_\ell) + k(\tau_i, z), \tag{2.1}$$

where y_a is associated with State A, and y_b with State B. Therefore, the expected utility once consuming y_a, y_b, (without considering potential punishment) is given by,

$$(1 - \mu(z))\, u(y_a) + \mu(z)u(y_b) \tag{2.2}$$

with $u'(y_\jmath) = \partial u(y_\jmath)/\partial y_\jmath > 0$, and $u''(y_\jmath) = \partial^2 u(y_\jmath)/\partial y_\jmath^2 < 0$, where $\jmath = (a, b)$.

Moreover, crime is a risky action. Thus, if someone is involved in criminal activities he/she faces a probability of arrest, $\pi(\tau_i, z)$, as described before. We assume that this probability increases with time spent on illegal sector, so that $d\pi(\tau_i, z)/d\tau_i > 0$, but the sing of the second derivative is uncertain.[1] Here we assume that if arrested, conviction, and thus, punishment is certain. Nothing is lost from this simplification since it can be shown that it does not affect the implications of the model. Conviction occurs at the end of the period, where the individual receives a punishment $P(\tau_i, z)$, pecuniary or not pecuniary such as imprisonment, with $dP(\tau_i, z)/d\tau_i > 0$ and $dP(\tau_i, z)/d\tau_i > 0$.[2] According to the above, the present value of the expected future punishment is given by,

$$\Pi(\tau_i, z) = \rho(z)\pi(\tau_i, z)P(\tau_i, z) \tag{2.3}$$

where $\rho(z)$ discounts punishment since it occurs at the end of the period. For simplicity, expected punishment is measured in utility terms as in Lochner and Moretti, (2001).[3]

[1] It could be negative, since self-protection improves as people spend more time in criminal activities. On the other hand, it could be positive as well, as more time in the illegal sector allows the law enforcement to acquire more evidence against the criminal, which increases the probability of apprehension

[2] Any kind of punishment, as in the cases on Becker (1968) and Ehrlich (1973), is measured in its monetary equivalent. Here it is assumed that the more serious the crime the stricter the punishment becomes.

[3] That this future potential punishment is measured in utility terms has as implication that punishment is separable from (2.2). Otherwise, this future punishment should have been incorporated in the utility function in the same manner as in (2.2). In that case, there should have been four mutually exclusive states, for employed and not arrested, unemployed and not arrested, employed and arrested, and unemployed and arrested, as described in Ehrlich (1973). This would result in four mutually exclusive utility outcomes, each associated with the probability of the state of nature to be observed and the total expected utility would have been, $U(\tau_i) = (1 - \pi(\tau_i))(1 - \mu)u(w(\tau_\ell) + k(\tau_i)) + (1 - \pi(\tau_i))\,\mu \cdot u(k(\tau_i)) + \pi(\tau_i)(1 -$

Henceforth, z is omitted from the equations for brevity.

Given all the above, the total expected utility received by both legal and illegal activity from both states is the following,

$$U(\tau_i, \tau_\ell) = (1-\mu)\, u\,(y_a) + \mu\, u\,(y_b) - \rho\, \pi(\tau_i) P(\tau_i). \tag{2.4}$$

Thus, the problem of the individual is to allocate his/her available time between legal and illegal activities in order to maximize (2.4) subject to the time constraints,

$$\tau = \tau_i + \tau_\ell, \qquad \text{and,} \qquad \tau_i \geq 0, \tau_\ell \geq 0. \tag{2.5}$$

Substituting $\tau_\ell = \tau - \tau_i$, and (2.1) into (2.4), the problem simplifies into an optimization problem with one variable. The Kunh-Tucker first order condition for τ_i is given by,

$$\frac{dU\,(\tau_i)}{d\tau_i}\tau_i = 0, \qquad \frac{dU\,(\tau_i)}{d\tau_i} \leq 0, \qquad \tau_i \geq 0. \tag{2.6}$$

The interior solution of spending some time in illegal activities is $dU(\tau_i)/d\tau_i = 0$,

$$\begin{aligned}\Rightarrow \Big((1-\mu)u'(y_a) + \mu\, u'(y_b)\Big)\frac{dk}{d\tau_i} - \Big((1-\mu)u'(y_a)\frac{dw}{d\tau_\ell} + \mu u'(y_b)\frac{dD}{d\tau_\ell}\Big) \\ = \rho\left(\frac{d\pi}{d\tau_i}P(\tau_i) + \frac{dP(\tau_i)}{d\tau_i}\pi(\tau_i)\right),\end{aligned} \tag{2.7}$$

so that the marginal utility from criminal activities minus the marginal utility from legal activities must be equal to the marginal punishment.[1] The sufficient condition for a strict

$\mu)u\,(w(\tau_\ell) + k(\tau_i) - \rho P(\tau_i)) + \pi(\tau_i)\mu \cdot u\,(k(\tau_i) - \rho P(\tau_i))$ rather than the simpler function (2.4).

[1]The LFS of the FOC could also be written as, $(1-\mu)u'(y_a)\left(\frac{dk}{d\tau_i} - \frac{dw}{d\tau_\ell}\right) + \mu\, u'(y_b)\left(\frac{dk}{d\tau_i} - \frac{dD}{d\tau_\ell}\right)$, so that the marginal utility from the good and the bad state must be equal to the marginal punishment. Here, $dw(\tau_\ell)/d\tau_\ell$ and $dD(\tau_\ell)/d\tau_\ell$ can be considered as the opportunity costs of crime of not spending the extra time $d\tau_i$ on the legal sector.

global maximum is given by,

$$\Delta = (1-\mu)\left[u''(y_a)\left(\frac{dk}{d\tau_i} - \frac{dw}{d\tau_\ell}\right)^2 + u'(y_a)\left(\frac{d^2k}{d\tau_i^2} + \frac{d^2w}{d\tau_\ell^2}\right)\right]$$
$$+ \mu\left[u''(y_b)\left(\frac{dk}{d\tau_i} - \frac{dD}{d\tau_\ell}\right)^2 + u'(y_b)\frac{d^2k}{d\tau_i^2}\right] - \rho\left(\frac{d^2\pi}{d\tau_i^2}P + 2\frac{d\pi}{d\tau_i}\frac{dP}{d\tau_i} + \pi\frac{d^2P}{d\tau_i^2}\right) < 0. \quad (2.8)$$

Since the term on the right hand side of (2.7) is weakly positive, and given that (2.8) holds, it is required that the marginal utility from criminal activities is at least as high as the marginal utility from the legal sector. This is because crime is a risky action that involves losses in the case of a potential future punishment. Thus, the term on the right hand side can be considered as the extra marginal compensation required for crimes to be committed. Note also that if $\frac{dk}{d\tau_i} < \frac{dw}{d\tau_\ell}$, so that the marginal return from crime is lower than the marginal legal return, it is highly unlikely that (2.7) holds. However, it could still hold in cases where the unemployment rate is very high, the unemployment benefits are very small, and the marginal legal return is only a bit larger than the marginal illegal return.

As the criminal wage rate is in general quite small in comparison to the legal wage rate for most property crimes, and if we consider that for most people the criminal wage further decreases by the psychological costs associated with a crime, the corner solution where someone allocates all his/her time in legal actions is highly possible.[1] Moreover, property crimes that pay a quite high financial return are also very rare, as according to our assumptions, crimes that pay high returns require plenty of time which in turn increases the risk of apprehension and the severity of punishment.[2] Also, crimes that pay a high return involve much higher psychological costs than psychological gains for most people, so that $k(\tau_i)$ is not large enough. Finally, we must also consider that many individuals do not exhibit strong criminal ability which might decrease $k(\tau_i)$ (if less able criminals target in criminal activities that pay low returns) or increase $\pi(\tau_i)$. All the above are possible reasons to explain why crime is such a rare event.[3] On the other hand, the individual will specialise

[1] For most people, property crimes would include more psychological costs because of regret, bad reputation, etc, rather than psychological gains because of possible satisfaction.

[2] Think for example bank robberies, or car thefts. Although the crimes itself may not need so much time, we can assume that they need a lot of preparation, which is also measured in τ^i.

[3] In my sample, the proportion of people who have have admitted committing at least one property crime during last year is just 5%.

in the illegal sector ($\tau_\ell = 0$), if and only if, the marginal legal utility plus the marginal cost of punishment is smaller than the marginal utility from illegitimate activities, which is highly unlikely for most people.

A main point of (2.7) is that, starting from an equilibrium where the individual spends some time on the illegal sector, the better the opportunities in legal sector, expressed as higher $dw(\tau_\ell)/d\tau_\ell$, the higher the opportunity cost of crime is. Therefore, holding everything else constant, the participation in illegal activities will decrease. The same is true if there is an exogenous increase in $D(\tau_\ell)$. This change will increase the marginal legal utility, and therefore it is less likely that (2.7) is satisfied. The opposite holds for the marginal return to crime. As it becomes higher compared to the marginal return to legal activities, the individual is better off if he/she allocates more time to criminal activities than before.

Another important result is that, starting from an interior solution for crime, the effect of an increase in unemployment rate is positive, as somebody would expect. The comparative static analysis shows that $d\tau_i^*/d\mu = -\left(u'(y_b)(\frac{dk}{d\tau_i} - \frac{dD}{d\tau_\ell}) - u'(y_a)(\frac{dk}{d\tau_i} - \frac{dw}{d\tau_\ell})\right)/\Delta$, which is positive *iff*,

$$u'(y_b)\left(\frac{dk}{d\tau_i} - \frac{dD}{d\tau_\ell}\right) - u'(y_a)\left(\frac{dk}{d\tau_i} - \frac{dw}{d\tau_\ell}\right) > 0. \tag{2.9}$$

Now, as $\frac{dD}{d\tau_\ell} < \frac{dw}{d\tau_\ell}$, we know that $(\frac{dk}{d\tau_i} - \frac{dD}{d\tau_\ell}) > (\frac{dk}{d\tau_i} - \frac{dw}{d\tau_\ell})$. But, moreover, as $w(\tau_\ell) > D(\tau_\ell)$ we have that $y_a > y_b$, and since the individual is risk averse ($u''(y_\jmath) < 0$), which implies that $u(.)$ is strictly concave, we know that $u'(y_b) > u'(y_a)$. Thus, the first term of the LHS of (2.9) is always higher than the second term and (2.9) always holds.

The effect of all the components of the potential punishment is also the expected one. For example, following an exogenous increase in the probability of punishment, the marginal return to criminal activities must also go up to compensate this increase in potential punishment. Otherwise the individual would decrease τ_i. The same will be the effect if there is an exogenous increase in the severity of punishment.[1]

[1]Note that in my model, the deterrent effect of a 1 percent increase in π is equal to the deterrent effect of a 1 percent increase in P. This is because of the simplification that punishment is measured in utility terms, which means that all individuals are risk neutral with respect to punishment, although they are risk averse with respect to legal or illegal wages. If the punishment was incorporated in the utility functions of a risk averse individual, as described in note 3 of page 52, it can be shown, as in Ehrlich (1973), that an 1 percent increase in P has a larger effect than an 1 percent increase in π.

Finally, risk attitudes, which can be expressed through the discount factor or the curvature of the utility curves, are quite important. As someone becomes less risk averse or more impatient, he/she discounts future potential punishment more heavily (lower ρ). In this way, the marginal return from crime must be higher for a more risk averse or a more patient individual, since he/she puts much weight on the consequences of a possible future apprehension. Moreover, as y goes up, $u'(.)$ decreases by more for a more risk averse individual (as his/her utility function is more "curvy"), which consequently decreases the left hand side of (2.7), resulting in a higher extra compensation required for the more risk averse person to participate in the illegal sector.

2.2.1 Immigration and Crime

What could this simple model tell us about immigrants' behaviour towards crime? Since z determines whether someone is an immigrant or a native, immigration status affects the first order condition (2.7) through many channels. Although immigrants do not form a homogeneous group of people, as individuals of very different ethnic backgrounds are included in this group, they exhibit some common features that distinguish them from natives. As is explained below, some of these features are positively related to crime and some negatively, so that link between being an immigrant and criminal behaviour is not obvious.

First of all, since immigrants generally face lower legal opportunities, meaning that they have on average lower $dw(\tau_\ell)/d\tau_\ell$, or higher μ (see, for example, Algan at al, 2010), we would expect a positive link. For instance, they hold lower quality jobs and a lower chance of getting accepted in higher status jobs. This might be for instance because of discrimination, limitations in human capital or in language proficiency, etc. According to this, they may find opportunities in illegal sectors more attractive. Regarding criminal opportunities, it is not clear whether immigrants face a higher or a lower $dk(\tau_i)/d\tau_i$. It is therefore not appropriate to associate immigration with crime using the return to criminal activities.

On the other hand, there are a few reasons which would indicate a negative association between being an immigrant and criminal behaviour. Some evidence by criminologist shows that the criminal justice system and law enforcement are biased in various stages against ethnic minorities (see, for example, Smith, 1997, Feilzer and Hood, 2004). This implies that immigrants may face more severe punishments compared to natives. Moreover, highly deprived areas are generally associated with both higher concentration of immigrant population and higher concentration of police force. This increases the risk of apprehension. Finally, immigrants also face deportation which is a punishment specific to them. This could be considered as a large disincentive to commit crimes (Butcher and Piehl, 2007). Thus, according to the above, we would expect that the average immigrant faces both higher $\pi(\tau_i)$ and $P(\tau_i)$.

In another direction, discount factors and risk attitudes may also be different for immigrants. It could be said that immigrants are willing to take more risks, since migration is in general a risky action with quite uncertain outcomes (see, for example, Jaeger at al, 2010). On the other hand, there is empirical evidence that immigrants are more risk averse than the native population (see, for example, Bonin at al, 2009). A considerable number of immigrants leave their families back at their countries of origin. Even though they have taken the risk to migrate away from their countries, they target on a better life for them and their families. In addition, they would like to feel socially equal to natives by presenting a highly responsible and credible behaviour. They may not be willing to take highly risky actions which can cost them their presence in the host country. In addition, a large proportion of immigrants come from poor countries. Since they have already faced quite harsh conditions, it could be assumed that they are more resilient not only in financial difficulties but also in psychological and physical severities.

Furthermore, discount factors and risk behaviour are strongly associated with cultural factors. Therefore, coming from different cultures, risk attitudes and discount factors may have been shaped quite differently. Cultural differences are also important for the perceptions towards the moral dimension of crime. Thus, psychological costs, also incorporated in $dk(\tau_i)/d\tau_i$, may be very different between immigrants and natives.

Finally note that, the model does not explicitly include variables for demographic factors such as age, gender, or location features, that are found to be associated with crime. Therefore, there could be also some indirect effects of immigration on crime if immigrants are different from natives with respect to these demographic features.[1] Thus, taking all the

[1] All the discussion above concerns the individual supply of property crime that economic theory predicts.

above discussion into consideration, the individual relationship between immigration and property crime cannot be determined by this theory, and can only be established by an empirical analysis, using a well specified model and appropriate data.

2.3 Immigration and Crime. A Review of Research

Although other indicators of crime, such as education, inequality, labour opportunities, etc., have been well studied by economists, the empirical research of the immigration-crime nexus is limited. However, the literature by criminology and sociology scholars is much more extended, both theoretically and empirically. Traditionally, these studies are developed in countries which have experienced large inflows of migrants. For instance, the US with the inflow of Latino and Afro-Caribbean population, Germany with immigrants from Turkey and the former Yugoslavia, Netherlands with Turkish and Moroccans, etc.

The results of various researchers are often contradictory. This is natural, mainly because the empirical results by each researcher are subject to the composition of immigrant population in each destination country, the circumstances that immigrants encounter in different countries, the differences in the data sets they use, and last but not least, the different statistical tools and strategies each researcher follows. Thus, we cannot identify globally what is the effect of immigration on crime by looking only at one country, or one approach, but we need to look at the broader picture.

The literature review is presented in two subsections. In the first one the results found by economists are presented, whereas the second one briefly presents the results found by sociologists and criminologists. A basic difference between studies by criminologists and sociologists and studies by economists is the theoretical hypothesis. The first group bases its theory on disorganization and culture conflicts whereas economists associate immigrants

In another direction, long before economists, criminologists developed some ideas on the immigration-crime nexus. Starting with eccentric ideas that immigrants commit more crimes just because they are a group of inferior individuals (see, Armstrong, 1935, and, Sellin, 1938), they switched to more rational theories based on psychological patterns. One of the earliest theories is based on the so-called "strain" theory, presented by Merton (1938), which states that immigrants present adverse behaviour due to accumulative pressure, as for example, because of discrimination, racism and unequal social and financial opportunities. Other theories suggest that there might be deviant behaviour by both immigrants and natives because of cultural conflicts. Thus, contrary to economic theory, criminologist's theories stand better for violent crime. For an excellent survey on these theories the reader may refer to Martinez and Lee (2000).

with crime through the economic models of crime. The second main difference stems from the fact that economists traditionally use more analytical statistical and mathematical tools than the other group. Thus, in general the studies by economists use more sophisticated and in many cases, more appropriate statistical models.[1]

Before presenting details of the empirical findings, in a nutshell, the available literature seemingly agrees on the following: both macro and micro-analysis in the US indicates that there is a negative association between immigration and crime. Regarding Europe, criminologists find that immigrants are over-represented in official records, but less involved in criminal activities according to crime self-reports. Finally, the empirical work by economists in Europe suggests either a positive link (mostly for property crime) or no link.

2.3.1 Empirical Evidence by Economists

To begin with, as mentioned in the introduction, to my knowledge there is only one study concerning the UK. Bell, Machin and Fasani (2010) examine how two separate large waves of immigrants affected crime rates. These waves are, the late 1990s wave of asylum seekers and the large inflow from the "A8" Eastern European countries since May 2004. What they find is that the first wave is associated with higher property crime, even after controlling for endogenous location using fixed effects and instrumental variables.[2] However, they find that the A8 wave did not affect property crime.[3] Moreover, their results indicate that there is no effect for violent crime. They argue that this finding is consistent with a simple economic model of crime, as asylum seekers face much lower legal opportunities realtive to A8 immigrants and natives, and therefore, illegal activities seem more attractive to them.

As far as I know, the first attempt by economists to investigate the immigration-crime relationship is that of Bucher and Piehl (1998a). Using data from the Uniform Crime Reports and Current Population Surveys, they first look at the aggregate effects of immigration on crime in the US, during the 80s. Although they find that there is a positive relationship

[1] This review does not intend to criticize or judge the methods and specifications of different researchers, but instead, it is purely descriptive.

[2] As asylum seekers where located by the National Asylum Support Service, they were mostly located in unpopular areas with a large amount of vacant houses. Thus, they instrument for endogenous location decisions by the number of dispersal accommodation in each local area.

[3] In this case they control for endogenous location by using the availability of flights to A8 countries as an exogenous variation for immigrants choices of location.

between crime rates and the fraction of recent (within one year) immigrants, this association fades out once they include controls both correlated with the location choice of immigrants and crime rates. Actually, the effect of immigration becomes negative but statistically insignificant. Using fixed effect analysis, they find that there is no association (negative but insignificant) between flows of immigrants and crime rates, or flows of immigrants and growth in crime rates (one year changes). Their results are strengthened by the use of self reports from the National Longitudinal Survey of Youth of 1980. What they find is that in all cases immigrants report considerably less crime (statistically significant at 5% in most cases), a relationship that is more clear once they control for other individual characteristics associated with crime. Immigrants are also less involved in arrests and convictions. Nevertheless, they do not use any strategy to control for any possible under-reporting, a major concern in self-reports.

Bianchi, Buonanno and Pinotti (2007), study the same research question but for Italy, by using police administrative records for Italian provinces. Using a panel data set from 1990 to 2003, they find a positive relationship between the size of immigrant population and most categories of crime rates, even after controlling not only for other determinants correlated with the factors that determine both crime and the location choice of immigrants, but also for province and year dummies. However, they recognize that even after controlling for these factors, there can still be some time varying unobserved factors correlated with both immigration and crime (for example, a economic crisis in a specific area would reduce the cost of living, which would in turn attract immigration population, but at the same time, also increase crime rates). Moreover, there is the concern of reverse causality, since crime rates in an area could affect the location choice of immigrants. Therefore, they employ a two-stage least square approach, using changes over time of immigrant population in the rest of Europe as an instrument for changes of immigrant population in Italy. Arguing that this is not a "weak" instrument, they find that there is no relationship between immigration and most categories of crime. However, there still exists a positive association for murders, robberies and thefts.

Finally, in another direction Bucher and Piehl (1998b, 2007), use institutionalization rates as a proxy for incarceration rates. Using the 5% Public Use Microsamples of the US census in 1980, 1990, and 2000, they find that the probability of an immigrant being incarcerated is much lower, even after controlling for educational attainment and ethnic status. They also find that this difference has increased over the last three decades, as more recent immigrants have the lowest incarceration rates. They attribute this to two reasons. First, they argue that this improvement is due to the stricter legislation for immigrants, since recent laws have broadened the crimes for which an immigrant is deported. They find that although deportation itself does not drive the results (meaning that the share of immigrants is not lower in prisons just because they get deported), it acts as a deterrent effect specific to immigration population. Second, they show evidence that the recent migration process to the US selects individuals who are either less crime-prone, or more responsive to deterrent effects.

2.3.2 Empirical Evidence by other Scholars

As opposed to economists, criminologists and sociologists have paid more attention to this relationship. Here, I will try to briefly describe the results of the most important works in each country.

The majority of immigration-crime studies come from the US and focuses on violent crime.[1] Most of evidence from the US shows that although the public opinion keeps associating immigrants with crime, immigration is not associated with higher crime, and in many cases it is associated with lower crime. Hagan and Palloni (1998), perform an empirical analysis using a sample of 34 Standard Metropolitan Statistical Areas of the US. By regressing logged arrests on the proportion of immigrants in the population, they find no association between immigration and both property and violent crime. Reid at al. (2005), combining 2000 US Census data and 2000 Uniform Crime Report (UCR), explore how the immigrant population affects crime rates across a sample of metropolitan areas. They find that, after controlling for demographic and economic conditions, immigration does not affect violent and nonviolent crime. Lee, Martinez and Rosefend (2007), examine whether

[1] A more detailed review can be found in Hagan and Paloni (1998), and Martinez and Lee (2004). For most recent evidence refer to Stowell (2007). Also, each individual study described in the subsection provides some related literature. For example see, Ousey and Kubrin (2009), Stowell at al (2009), and Wadsworth (2010). Stawell at al (2009), also provide a table that presents important information for the main studies in the US (Table 1, p.895).

immigration increased homicide in the three border cities of Miami, El Paso, and San Diego, using 3,345 homicide occurrences happened between 1985 and 1995. Poisson regression results indicate that there is no relationship, or, even a negative relationship between the percentage of new immigrants and homicide levels. Stowell (2007), in the same direction examines the crime-immigration nexus for three US cities, Alexandria, Houston and Miami. Using neighborhood-level data from 2000, he also finds no direct evidence between the proportion of recent (less than ten years) immigrants in the population and violent crime levels. However, he finds a negative association for Miami.

Even more recently, there was a quite large amount of publications on this subject for the US.[1] Most of those studies also show that if there is an association, this is negative. Very briefly, Ousey and Kubrin (2009), using fixed effects for 159 US cities (with more than 100,000 residents) for the three time periods 1980, 1990 and 2000, find that the proportion of recent immigrants decreases violent crime. Wadsworth (2010), in a similar manner, uses a fixed effect model for 459 cities (with more than 50,000 residents), between 1990 and 2000. Both robbery and homicide rates are examined. The results of this study suggest that the proportion of foreign born population and the proportion of new immigrants decreased crime rates within this time period. Stowell at al (2009) use a panel over the period 1994-2004 for 103 metropolitan areas (with more than 500,000 residents). Their results indicate that there is a negative association between changes in immigration concentration and changes in crime rates. This effect is particularly stronger for robberies.

The "Homicide Studies" journal published an issue on 2009 that solely focuses on aspects between immigration and homicides and other crime types. Graif and Sampson (2009), in a neighborhood study of Chicago, using a "weighting" estimator that assigns different weights for points of different proximity to each data point, find that higher concentration of foreign born population is either negatively or not associated with homicide. Feldmeyer and Steffebsmeier (2009), using homicide arrest data from California, find that the proportion of recent (entered USA between 1990 and 2000) immigrants does not affect the mean number of overall homicides, but it does affect negatively the mean of homicides against white and black people. Vélez (2010), by allowing the effect of immigration to be different between ad-

[1] For all studies described in this paragraph, crime data are collected from the Uniform Crime Reports.

vantaged and disadvantaged neighborhoods of Chicago (by using an interaction term), finds that an increase of recent immigrants decreases the number of homicides in disadvantaged areas but has no effect in the more advantaged ones. In another study for Chicago, Chavez and Griffiths (2009), argue that growing immigrant population was unrelated to homicide patterns. Polczynski at al (2009), look at arrest rates for different types of crime in Orange County, Florida, and show that arrest rates are generally lower for foreign born individuals. Moreover, their results suggest that concentration of immigration is not associated with the number of arrests. Finally, the studies of Akins, Rumbaut, and Stansfield (2009), and Stowell and Martinez (2009), focus on the area of Miami. The former suggests that for the area of Austin, Miami, where the immigration population increased by around 580 percent, there is no association between migration and homicides. In the latter, they find that neighborhoods with higher number of Latino immigrants exhibit lower levels of homicide.

In Germany the studies of immigration-crime link are based on official criminal statistics, since in official records from police or courts there is a categorization of people as foreigners or not. Using police and court data Albrecht (1987) finds that foreign population's involvement in criminal activities is higher than that of Germans. However, this relationship disappears once controls for socio-economic conditions and demographic differences are accounted for. The same positive relationship is also found in Albrecht (1997). However, in this second study the higher involvement of immigrants in crime persists, even after controlling for the above factors. Finally, Chapin (1997), using basic statistic analysis finds that changes in foreign population increase the growth of crime levels.

Evidence from official statistics of Switzerland also suggests that immigrants are less law-abiding. For example, Killias (1997) using police and conviction statistics finds that, although in the 70s immigrants displayed similar crime rates with natives, after the 80s immigrants were over-represented in crime statistical tables. However, he expressed many concerns about the reliability of official crime statistics. Contrary to that, using self-reported data from more than 3,000 adolescents and employing basic statistics, Vazsonyi and Killias (2001), find that first generation adolescent immigrants display slightly lower crime rates than native Swiss adolescents. This result coincides with other works in Switzerland that have used self-reported crime data. They also find that second-generation immigrants are more crime-prone than natives, a result that it is common in literature.

In Netherlands, Junger-Tas (1997), finds that Moroccans and Antilleans are overrepresented in official criminal statistics. Contrary to that, he also presents a review of self-report studies in Netherlands which suggest that the above groups, and other groups of ethnic minorities, are less involved in crime. Concerning France, Tournier (1997), finds that although immigrants are over-represented in prison statistics and statistics of criminal suspects, a large fraction of their crimes concerns violations of immigration law regulation. Thus, when he controls for this fact the difference between foreigners' and natives' involvement in crime is considerably lower. In a study for Sweden, Martens (1997), finds that the fraction of immigrants who have been suspected for crimes is clearly higher than the fraction of native population. A difference still exists even after immigrant-native differences are accounted for, although it becomes noticeably smaller. He also finds that first-generation immigrants display higher crime rates than second-generation immigrants, a result that contradicts with findings in other countries. Finally, Yeager (1997) presents a cross-country review of immigration and criminality. According to this review, immigrants are not as highly involved in crime as natives in Canada and Australia. Criminal records form France, Sweden, Netherlands and Germany indicate that immigrants are over-represented in various criminal aspects. He also briefly describes the results of crime self-reports for Switzerland, Netherlands and Germany, which suggest that immigrants are less involved in crime than natives citizens.

2.4 The OCJS. Some Methodological Issues

The basic target of the present study is to identify whether immigrants are more or less crime-prone than natives, even after controlling for the fact that immigrants might exhibit some differences in basic demographic characteristics associated with higher or lower crime. For this purpose the Offending Crime and Justice Survey (OCJS) of 2003 is used, a nationally representative survey which asks people in England and Wales about their experiences as offenders and their attitudes towards criminal activities.[1] Although a few earlier offending

[1] Although three subsequent OCJSs exist (2004, 2005, 2006), they are particularized in adolescent delinquency (people from 10 to 25 years old). Thus, they are not appropriate for the purposes of my research question.

surveys in the UK exist, this is the largest one and the most sophisticated in terms of design and construction.[1] This section discusses some features, advantages and limitations of the OCJS, and self-report studies in general.

A basic target of the OJCS is to provide an accurate measure of the prevalence of offending in the general population, as opposed to studies of already convicted criminals, and to investigate the factors related to committing crimes (Hamlyn, and Hales 2003).[2] However, this is a hard task if we consider a few limitations of the OCJS.

To begin with, validity and reliability of the responses is a concern, since questions try to elicit information in a very sensitive part of personal activities such as crime. Particularly, a response error is expected that most probably takes the form of under-reporting if respondents conceal some aspects of true crime activities. However, it is important to stress that computer-based interviews are used as opposed to face-to-face interviews, a method that is found to increase the reliability of responses (see, for example, Turner at al, 1998, and Newman at al, 2002).[3] As will be explained in Section 2.5, a conventional regression model that does not take into acount under-reporting would result in inconsistent estimated coefficients for the true crime, as it is developed to estimate the coefficients of the observed, reported crime. More importantly, if for any reasons immigrants under-report by more than natives, the estimated effect of being an immigrant on crime will be downward biased.

Another concern stems from the fact that some individuals selected by the survey's conductors denied participation in the survey. In spite of the fact that response rates of the OCJS are very close to response rates of other population surveys,[4] such as the Labour Force Survey or the British Crime Survey (BCS) (see, Sharp and Budd, 2005), estimates of prevalence of crime will be downward biased in case non-respondents commit more crimes (see, Farrington at al., 1990).[5] In addition, if the effect of being an immigrant on criminal

[1]For a review of other self-report studies the reader can refer to Farrington (2003), Thornberry and Krohn (2000), and Junget-Tas and Marsall (1999).

[2]For surveys of convicted criminals see, Budd at al. (2005).

[3]Actually, the OCJS aimed to get the highest level of reliability possible by using well trained interviewers, appropriately designed questionnaires and feasibility studies.

[4]The response rate for the core sample and youth-boost sample is around 74%. For the nonwhite-boost sample the response rate is around 50% which is common in surveys that include nonwhite boosts. Depending on the assumptions used to provide the response rate estimates, Hamlyn at al (2003) provide un upper and lower limit on the response rate estimates. For the core and youth-boost sample these are 78.5% and 73% and for nonwhite-boost, 60% and 45% respectively, a figure that matches closely the figures of 2001 census.

[5]Although the weights used in empirical analysis take into account non-respondents (see, Hamlyn and

behaviour is different between respondents and non-respondents, the coefficient measuring the difference between immigrants and natives propensity to crime will be biased. Moreover, this survey does not capture individuals currently in institutions who would most probably commit more crimes than the general population if they were free. The consequences are similar to the previous point.

Despite these limitations, relying on self-reports constitutes the most suitable method to identify predictors of criminal behaviour. As Thornberry and Krohn (2000) point out, the best way to identify factors of criminal behaviour would be to observe the actual behaviour of potential criminals, self reports being the nearest proxy to actual criminal behaviour. The OCJS also provides important information on a wide range of characteristics of potential criminals as opposed to victimization surveys and official statistics. Actually, self-reports is the most commonly used technique in criminology research to discuss causes of crime (see, Hagan, 1993, and Junger-Tas and Marshall, 1999). Moreover, it would be misleading to attempt to identify the determinants of criminal behaviour by comparing convicted and non-convicted individuals, since there is quite a large number of individuals that have committed crimes but are not convicted. Similar logic applies to comparisons between arrested immigrants and natives.

As we saw in the previous section, some criminologists prefer to use data of recorded crime. There are two main pitfalls in using official recorded crime by the police. First of all, these statistics are very poor in terms of information provided. Concerning official statistics in England and Wales, they offer no information concerning immigration status. Therefore, they are inappropriate for native-immigrant crime comparisons. Furthermore, even if the same information was available, it is widely accepted that many crimes are not reported to the police, and many reported crimes are not even recorded because of inside-police operational reasons (for example, some reported crimes are not considered serious enough to be recorded). This is the so-called "dark-figure" of crime, for which a vast literature exists (see, for example, MacDonald, 2001, 2002).[1] Moreover, there is some evidence by

Maxwell, 2003, and Budd, Sharp and Mayhew, 2005), they do not control for possible higher crime of non-respondents.

[1]For example, using data from the BCS of 2007/08 we find that around 60% of crimes were not reported to the police.

criminologist that the criminal justice system and law enforcement are biased in various stages against ethnic minorities, mostly against black individuals (see, for example, Smith, 1997, Feilzer and Hood, 2004). Perhaps, immigrants are also more "visible" to the police because of over-policing in target areas where ethnic minorities are concentrated, increasing the likelihood of immigrants to be arrested (Sharp and Budd, 2005). Therefore, official statistics would overestimate immigrants' crime if immigrants face a higher probability of arrest for the same crimes, or, if police officers disproportionately record crimes that are supposedly committed by immigrants.

On the other hand, although victimization surveys, such as the BCS for England and Wales, provide the most precise estimates of the actual crime, they do not provide any information about criminals' characteristics. However, if we accept that the BCS reveals the figure of crime that is the closest to the true one, we could compare official and OCJS' crime figures to BCS' one in order to evaluate their precision in measuring crime. Kershaw and Walker (2008), suggest that recorded crime is only 42% of the total crime in England and Wales. Concerning the OCJS, Budd, Sharp and Mayhew (2005), suggest that the figure of violent crime is quite close to that of BCS, but the count of property crime is quite lower than in BCS. This would suggest that there is under-reporting in property crime. However, these figures must be treated with caution since there are fundamental design differences between these two sources, and therefore, they do not provide comparable crime figures (see, Budd, Sharp and Mayhew, 2005).

2.5 Econometric Models

As will be described in the next section, the dependent variable is observed in count form (number of crimes committed during the twelve months prior to the interview). Therefore, count data models (number of crimes) or binary choice models (crime or not) are more appropriate than simple linear models. Nevertheless, as will be better explained in the next section, the very large number of zeros in the property crime variable, resulting in very low variation in the dependent variable, will make estimation of count data models quite harsh, mainly when estimators to allow for under-reporting are used. Alternatively, a safer choice would be to use the binary information, whether or not someone has committed a crime last year although these models do not use all available information. Therefore, a compromise would be to base my empirical results on binary choice models, while count data estimation models can be used for robustness check analysis.

As explained in detail before, under-reporting is the main concern in this data set.[1] Traditional nonlinear estimators for binary and count data are inconsistent if under-reporting, or more generally, response error in the outcome variable is present (Hausman, Abrevaya and Scott-Morton, 1998, Cameron and Trivedi, 1998 (p.313), and Winkelmann, 2008).[2] The problem is even more salient if under-reporting depends on individual characteristics which is most probably the case with self-reports in crime. On this direction, parametric models that take into account misreporting (both under and over-reporting for binary choice models, but only under-reporting in count data models) will be used. Another concern is the large number of zeros (95%) in the dependent variables. This has obvious consequences for the precision of the estimates in both binary and count data models. Adding the fact that estimation of models that account for under-reporting is quite demanding, to achieve precise estimates quite rich samples are required.

Moreover, as explained in the previous section, there is also a potential sample selection problem, since it is likely that people who refused participation in the survey are more prone to crime. However, models that correct for sample selection problems, such as the Heckit procedure (see, Heckman 1976, 1979), would require information of non respondents, which is not available. Therefore, this problem is ignored in the analysis, hoping that non respondents exhibit the same criminal behaviour, or at least that the crime differentials between immigrant and natives respondents follow the same pattern with crime differentials between immigrant and native respondents. In the following subsections models that control for misclassification, or, under-reporting are described.

[1]Although we could assume that someone would never report a crime if he/she has not committed one, we could not rule out that over-reporting may be present as well. This could be attributed to the fact that the OJCS is a retrospective study and measurement error in both directions could be possible. Although over-reporting is most possible unintentional, so that it is random, under-reporting is most probably intentional.

[2]Note that, as it is well known, random response error in linear models does not affect consistency. The only consequence of the presence of this error is the increase of the error variance which leads to less precise estimates. However, in our case, estimation of linear models is inappropriate as we deal with count data. Moreover, the error component because of under-reporting is most probably not random but it depends on respondents' attributes, which leads to endogeneity.

2.5.1 Binary Choice Models

In this section the model of Hausman, Abrevaya and Scott-Morton (1996, 1998) is presented, a parametric model that takes into account both probabilities of under-reporting (misclassification of a true one as zero) and over-reporting (misclassification of a true zero as one). Throughout the present study I refer to this model as MisProbit (Misclassification-Probit).

The model comes naturally from a latent variable specification. To simplify things, assume that in a given period of time, an individual would commit a crime (or a number of crimes) if the total utility from committing this crime is higher than the utility obtained from not committing it. So, let u_i^* be the (unobserved) utility obtained if committing the crime(s) minus the utility if not committing it (them). We specify u_i^* to depend linearly on the vector of characteristics x_i such that,

$$u_i^* = x_i'\beta + \epsilon_i, \tag{2.10}$$

where ϵ_i is a random i.i.d error. Thus, the individual commits at least one crime according to the following,

$$y_i^* = \begin{cases} 1 & \text{if } u_i^* > 0 \\ 0 & \text{if } u_i^* \leq 0 \end{cases} \tag{2.11}$$

where y^* is a dummy for the true but unobserved crime. Given (2.10) and (2.11), conditional on x_i, the probability of committing a crime is given by,

$$\Pr(y_i^* = 1|x_i) = \Pr(u_i^* > 0|x_i) = \Pr(\epsilon_i > -x_i'\beta|x_i) = F(x_i'\beta) \tag{2.12}$$

where $F(x_i'\beta)$ is assumed to have a known functional form such as the standard normal, if $\epsilon_i \sim \mathscr{N}(0, 1)$.

Let us now define y_i to be a dummy for the reported and therefore, observed crime. Suppose that the reported crime does not coincide with the actual crime since there is misreporting (in this context defined as misclassification). The probabilities of misclassification are defined as follows,

$$
\begin{aligned}
a_1 &= \Pr(y_i = 1 | y_i^* = 0), \\
a_0 &= \Pr(y_i = 0 | y_i^* = 1),
\end{aligned}
\tag{2.13}
$$

where a_1 is the probability of reporting a crime, conditional on committing no crime (over-reporting) and a_0 is the probability of reporting no crime, conditional on committing crimes (under-reporting). Notice that according to this specification the misclassification probabilities do not depend on x_i but only on y_i^*.

It is easy to derive the conditional probabilities of the observed crime, incorporating the probabilities of misclassification. The response tree in figure 2.3 is a very clear way to do that. It is clear that these probabilities are given by,

$$
\begin{aligned}
\Pr(y_i = 1 | x_i) &= (1 - F(x_i'\beta))\, a_1 + F(x_i'\beta)(1 - a_0) \\
&= a_1 + (1 - a_0 - a_1) F(x_i'\beta),
\end{aligned}
\tag{2.14}
$$

$$
\begin{aligned}
\Pr(y_i = 0 | x_i) &= (1 - F(x_i'\beta))\, (1 - a_1) + F(x_i'\beta) a_0 \\
&= 1 - a_1 - (1 - a_0 - a_1) F(x_i'\beta),
\end{aligned}
\tag{2.15}
$$

and therefore the expected value $E(y_i|x_i)$ is also given by (2.14). We can estimate, a_0, a_1, and, β, using the method of Maximum Likelihood (MLE) once we have specified the log-likelihood function as,

$$
\ln \mathscr{L}(\beta, a_0, a_1) = \sum_{i=1}^{n} \Big(y_i \ln\left[\Pr(y_i = 1|x_i)\right] + (1 - y_i) \ln\left[\Pr(y_i = 0|x_i)\right] \Big). \tag{2.16}
$$

Given correct specification of the model, meaning that the specified model of constant misclassification is the correct model under the true data generating process (DGP), maximization of (2.16) using numerical optimizers, such as the Newton-Raphson, yields consistent estimates for the coefficients of true crime, the probability of under-reporting, and the probability of over-reporting.[1]

We notice that a_0 is only designed to capture total under-reporting as opposed to partial under-reporting. That is, the probability of under-reporting will ignore the cases where individuals report just a portion of the total number of crimes they have committed. However, as will be explained later in this section, models that use the count form of the dependent variable are able to estimate the probability of any committed crime to be reported, given that this probability is constant for each individual.

Hausman, Abrevaya and Scott-Morton (1998) show that the model is not globally identified since for symmetric $F(.)$, $a_1 + (1 - a_0 - a_1)F(x_i'\beta) = \widetilde{a_1} + (1 - \widetilde{a_0} - \widetilde{a_1})F(-x_i'\beta)$ where $\widetilde{a_0} = 1 - a_1$ and $\widetilde{a_1} = 1 - a_0$. Thus, there are two observationally equivalent models with parameters (a_0, a_1, β) and $(\widetilde{a_0}, \widetilde{a_1}, -\beta)$. Identification is achieved by imposing the "monotonicity" condition, which states that $a_0 + a_1 < 1$. According to this, we are able to rule out the "wrong" maximum, since $a_0 + a_1 < 1$ implies that $\widetilde{a_0} + \widetilde{a_1} > 1$. If this condition fails, the misclassification probabilities are too large, and therefore, the data are most probably too noisy to obtain reasonable results.

As the assumption of constant misclassification is not realistic in many applications, including the present study, it can be easily relaxed if we model a_0 and a_1 to be functions of covariates, so that, $a_0 = F(z_i'\gamma)$, and $a_1 = F(w_i'\delta)$, where F can be the cumulative distribution of a binary model, such as a Probit or a Logit. Vectors x, z, w can be the same, disjoint or overlapping. No further assumptions are required to identify this model. There are some papers in the literature that have utilized this estimator (see, for example, Leece, 2000, Artis at al, 2002, Caudil and Mixon, 2005, and Falaris, 2007). Nevertheless, none of them allows the probabilities of misclassification to depend on covariates.[2]

[1]Hausman, Abrevaya and Scott-Morton (1996) explain why estimating a simple nonlinear binary model, under the presence of misclassification, leads to inconsistency even when misclassification is constant. Intuitively, the MLE of a simple binary model that ignores misclassification will set the score (FOC) to zero, although if misclassification exists the expected score of the ML is not zero, which leads to inconsistency. So, if constant misclassification is a problem, the likelihood function that must be maximized is (2.16). Notice however, that identification of the model comes from the nonlinear specification of the log likelihood function. This has negative consequences on the robustness of this model. For this reason, Hausman, Abrevaya and Scott-Morton (1996, 1998) also propose a semi-parametric estimation.

[2]It is interesting that this estimator is very similar to the Detection Controlled Estimator, presented in Feinstein (1989). In that paper Feinstein explains that sometimes inspectors fail to detect a violation. However, they never detect a violation if there is not one. Therefore, in the simplest form, when the probability of detection and the probability of violations are independent, he derives the same log-likelihood function as (2.14), if we set a_1 to zero. However, in his estimator, a_0 depends on regressors. This concept can

Although this is quite an easy model to implement in econometric software, in practice estimation is quite difficult, particularly when the misclassification probabilities are allowed to depend on variables, and when the data are quite noisy with very low variation particularly in the dependent variable. Always, exclusion restrictions could help the estimation procedure. Moreover, as Hausman, Abrevaya and Scott-Morton at al (1996) stress, it is quite difficult to get precise estimates and imprecision will increase the higher the misclassification becomes. They note that this is partly because the corrected for misclassification Information Matrix is not block diagonal. Simple binary models ignore this fact and consequently, underestimate the true standard errors. Thus, it could be said that the MisProbit MLE corresponds to the true precision, given correct specification of the model. According to this, in cases where misclassification is very high, as it may be the case for crime self-reports, quite rich samples are needed in order to obtain precise estimates.

So far, we have regarded zeros coming from two different sources; misclassification of 1 as 0, and zeros from the traditionally binary choice model. Nonetheless, zero-inflation can be incorporated into this model if we think of zeroes coming from a third source. That is, there are some individuals who, regardless of the conditioning set x_i, never commit any crimes (and consequently they do not report any). We will call these individuals "genuinely non criminals". If we do not incorporate this zero-inflation probability separately, the estimated probability a_0 cannot distinguish under-reporting from zero-inflation.[1] This is clear if we examine figure 2.4, where a_0 and a_1 are expressed as probabilities of zero-inflation and one-inflation respectively. Following this tree we notice that the conditional probabilities of observing 1 and 0 are exactly as in (2.14) and (2.15). However, in this case the interpretation of a_0 and a_1 is very different. Although, probability of one-inflation is unrealistic, zero-inflation is quite possible.[2] Nevertheless, it is possible to separate under-reporting from

be naturally applied in any situation that involves compliance and inspection, as for example, tax evasion (Feinstein, 1991).

[1]This idea of adding in a probability of inflation can be traced back to Gaudry and Dagenais (1979) where they developed an estimator for multinomial choice models, calling this the "dogit" model. Gaudry (1980) and Swait and Ben-Akiva (1987), apply this model for individual choices between a set of different transportation modes. They explain that given a set of choices, an individual is either captive to one choice regardless of his/her characteristics (inflation probability) or free to choose from the full set of choices (traditional multinomial choice model). In this models there is inflation probability for each category, whereas in the model presented here there is only zero-inflation probability, making this model more similar to zero-inflation models for count data as described in Mullahy (1986) and Lambert (1992).

[2]One-inflation could be interpreted as follows: there are some individuals who, regardless of x_i, always

zero-inflation by incorporating zero-inflation separately in the likelihood function. This model is presented in Appendix A.[1]

2.5.2 Count Data Models

The models presented here are based on the Poisson Logistic Regression model of Winkelmann and Zimmermann (1993), also presented in Mukhopadhyay and Trivedi (1995).[2] This model is a particular case of a Poisson *stopped-sum* distribution where the i.i.d random variables to be summed follow the Bernoulli distribution with constant probability of success p. According to this specification, the observed number of counts is given as,

$$y = b_1 + b_2 + \ldots\ldots + b_{y^*}, \tag{2.17}$$

where y^* is a latent variable of the true counts that follows the Poisson distribution with mean and variance equal to λ.[3] A basic assumption of any *stopped-sum* distribution, is that y^* and b_i are conditionally independent.[4] Under this independency condition it is easy to show (for example, using probability generating functions as in, Feller, 1968) that the observed counts, y, also follow the Poisson distribution with parameter $\mu = p\lambda$ (see, for example, Papadopoulos, 2011a). It is clear that $y \leq y^*$, and it is said that the observed counts are "under-reported". We must underline that this specification assumes no over-reporting.[5]

The above concept can be extended in a multiple regression framework, where the probability of success and the true Poisson process are allowed to depend on covariates. In the Poisson-Logit model, the true counts follow the Poisson distribution with,

$$\begin{aligned}
&\Pr(Y_i^* = y_i^* | x_i) = e^{-\lambda_i} \lambda_i^{y_i^*} / y_i^*!, \\
&\lambda_i = E[y_i^* | x_i] = e^{x_i'\beta},
\end{aligned} \tag{2.18}$$

commit crimes in the specified time period and they are always willing to report that they have committed them.

[1]However, note that this model is identified only because its nonlinear functional form.

[2]Here, only a brief discussion of these models is presented. For a more detailed analysis the reader may refer to Winkelmann, 2008, Cameron and Trivedi, 1998, and Papadopoulos, 2011a.

[3]The name *stopped-sum* comes from the fact that the summation of Bernoulli variables is *stopped* by the value of the Poisson distributed latent variable y^*.

[4]This assumption is relaxed in Winkelmann (1998).

[5]At least we need that over-reporting is not correlated with the regressors, so that it is totally random.

and the probability of success is given as a Logit, so that,

$$\Pr(b_{ij} = 1|z_i) = \Lambda(z_i'\gamma) = \Lambda_i = \frac{e^{z_i'\gamma}}{1 + e^{z_i'\gamma}}. \tag{2.19}$$

Consequently, it can be shown that the observed counts also follow the Poisson distribution (see, Papadopoulos, 2011a) with conditional probability distribution and expectation,

$$\begin{aligned} \Pr(Y_i = y_i|x_i, z_i) &= e^{-\mu_i}\mu_i^{y_i}/y_i!, \\ \mu_i = E[y_i|x_i, z_i] &= \lambda_i\Lambda_i, \end{aligned} \tag{2.20}$$

respectively. The log-likelihood function is given by,

$$\ln \mathscr{L}(\beta, \gamma) = \sum_{i=1}^{n} \Big(-\mu_i + y_i \ln \mu_i - \ln (y_i!) \Big). \tag{2.21}$$

Papadopoulos and Santos Silva (2008), show that identification of all the parameters of this model requires at least one exclusion restriction in the Poisson process, or at least one sign restriction on a parameter of the Logit part (see also, subsection 2.8.1). This means that we know with certainty that either at least one of the elements of β is zero, or the sign of at least one element of γ (see, Papadopoulos, 2011a, for a detailed discussion).

Despite the fact that consistency of the Poisson-Logit estimator only requires that $\mu_i = E[y_i|x_{1i}]$, as this model belongs to the Linear Exponential Family (see, Papadopoulos, 2011a), we can extend this model to account for possible over-dispersion, relaxing the strong assumption that both the conditional mean and variance of y_i are equal to μ_i. This approach is quite popular as in many empirical applications it is high likely that $E(y_i|x_i) < Var(y_i|x_i)$. The standard way to do this is by adding an unobservable individual effect $\upsilon_i = e^{\epsilon_i}$ which will account for extra unobserved heterogeneity. Now, conditionally on x_i and υ_i, y_i has a Poisson distribution with parameter $\mu_i\upsilon_i$. Under the usual assumption that υ_i has a gamma distribution with unit mean and variance α_i, the distribution of y_i conditional on x_i only, after integrating out the unobservable individual effect, is negative-binomial with mean μ_i and variance $\omega_i = \mu_i + \alpha_i\mu_i^2$. If the variance of υ_i is constant (homoscedastic), we obtain the Negative Binomial 2-Logit (NB2-Logit) with variance, $\omega_i = \mu_i + \alpha\mu_i^2$.

The log-likelihood of the NB2-Logit is the following,

$$\ln \mathscr{L}(\alpha,\beta,\gamma) = \sum_{i=1}^{n} \left(\ln \left(\frac{\Gamma(y_i+\alpha^{-1})}{\Gamma(y_i+1)\Gamma(\alpha^{-1})} \right) - (\alpha^{-1}+y_i)\ln(1+\alpha\mu_i) + y_i(\ln\mu_i + \ln\alpha). \right) \quad (2.22)$$

Maximization of (2.22) will yield consistent estimates for (α, β, γ), given correct specification of the model, that is, the true DGP is NB2-Logit.[1] As Papadopoulos and Santos Silva (2008) show, the conditions required for identification of this model are exactly the same as the one's required for the Poisson-Logit model (for more details see, Section 2.8.1, or Papadopoulos 2011a). To provide evidence against equi-dispersion a Lagrange-multiplier (Score) test can be performed since Poisson-Logit and NB2-Logit are nested, as can be shown that NB2-Logit reduces to the Poisson-Logit when α goes to zero (see, Cameron and Trivedi, 1998, or Winkelmann 2008 for a formal proof).[2] Other possible NB-Logit models considering a different functional form for the variance are presented in Appendix B. Finally, we need to stress that similarly to the models for binary choice, models for count data with under-reporting incorporated can also be generalized to take into account zero-inflation. These models are also presented in Appendix B.

2.6 Data Set and Discussion of Variables

Before describing the details of the data set, it must be stressed that some effort has been made to keep the sample size as large as possible. The reason behind this is twofold. First, the econometric methods used in the empirical analysis are very demanding, as explained in the previous section. Secondly, the variation of the dependent variable is very low, as 95% of respondents reported no property crime. Therefore, larger samples will assist on estimating the coefficients of interest more precisely.

To achieve the highest sample size possible, I exploit the sophisticated design of the

[1]Correct specification of the mean is not enough for consistency as the NB2-Logit is an LEF only for fixed value of α. However, since α is subject to estimation, practically NB2-Logit is not an LFE and therefore, misspecification of higher than the mean moments will lead to inconsistency. Therefore, we actually need to assume the much stronger assumption that the data are generated by a NB2-Logit distribution.

[2]Note that both the likelihood-ratio and the Wald tests are not valid for testing the null that $\alpha = 0$. This is because these statistics have a limiting χ^2 distribution only in the interior of all possible values of α. However, under the null, α is on the boundary since it can only be positive. However, the Score test is valid even if we want to test α on the boundary. For details, see, Wooldridge (2010).

OCJS that makes use of "boost" samples. There are three independent samples in the OJCS; the core sample (10-65 year olds, 58%), the youth-boost sample (10-25 year olds, 27%), and the nonwhite-boost sample (non-white individuals, 10-65 years old, 15%).[1] Each sample is accompanied by its (sampling) weighting variable, which must be used to restore the representative of each sample. I increase the sample size by around 5,000 individuals by adding these three samples together. However, to re-establish representativeness, a weighting variable that combines the three separate weights is used.[2] A tabulation by sample type follows in Table 2.4. Thus, the resulting data set I use in my empirical investigation consists of 11,658 individuals, 5,604 males and 6,054 females, between 10 and 65 years old.[3]

Concerning the dependent variable, as explained before, this paper focuses on property crime which consists of: thefts and attempted vehicle thefts, thefts and attempted thefts of parts from inside or outside vehicles, domestic and commercial robbery, domestic and commercial burglary, thefts from person, thefts from work, thefts from school, thefts from shops, other thefts, and criminal damage of cars or other objects. The information on property crime can be either used to construct a dichotomous variable which takes the value one if someone has committed a property crime during the year prior to the interview, or a count variable measuring the number of property crimes during the same specified period. The latter will be used for robustness analysis only, as the large number of zeroes complicates the estimation of count data models.[4] The observed distribution of the property

[1] Criminal behaviour of people between 10 and 25 years old is the primary interest of the OJCS. The subsequent OJCSs of 2004, 2005 and 2006 include only 10-25 years old individuals. Some of them are included in all OJCSs constituting a panel. The percentages in the parentheses of Table 2.4 denote the fraction of people of each sub-sample to the total sample size. This is the total sample size I use in my analysis and not the initial total sample size of the unrefined OJCS.

[2] This weighting variable was kindly provided by the Home Office. A detailed analysis of the construction of the combined weighting variable is given in the Appendix F of Hamlyn and Hales (2003).

[3] We need to note that by using individuals from 10 to 65 years old, we include students, retirees and people not in the labour force such as house keepers. Therefore, there is a departure from the economic model of crime which is designed for people in the labour force. However, these groups could somehow fit in the model as we can think that students are looking at their future legal opportunities, housekeepers are considering the household income, and retirees receive a legal stream of pensions. Anyhow, in the empirical analysis we try to formulate and estimate the behaviour towards property crime for the whole population. This was inevitable, as limiting the sample only to individuals in the labour force reduces the sample size at a point where obtaining sensible results seems impossible (at least for the models with under-reporting and misclassification).

[4] Alternatively, one could use the variable "ever committed a property crime" which increases the percentage of positives to around 30%. However, this is not an appropriate variable to use because of two reasons. Firstly, the possibility of response error would be much higher if we considered the whole lifespan of an individual, since this is a retrospective survey. Secondly, some crimes that immigrants report are committed long before the decision of an immigrant to immigrate in the host country.

crime variable is given in Table 2.5. We observe that 94% is concentrated in 0 crimes, 5.5% is concentrated between 1 and 14 crimes, and the rest 0.5% is spread from 15 up to 225 crimes. Using the sampling weights, we find that the unconditional mean is 0.34 crimes per person, whereas the unconditional variance is 34.32. Therefore, the raw data suggest that there is over-dispersion, which indicates that Negative Binomial models may provide a better fit to the data. Moreover, notice that the sample size differs between the count and the binary form of the variable. This is because some respondents who reported a crime were reluctant to report the number of crimes they committed. Consequently, these observations are recorded as missing cases, leading in further reduction in the variation of the dependent variable.

Information about violent crime is also available.[1] Although violent crime is not the main subject of this paper, a few empirical results of violent crime are presented in subsection 2.8.2. Moreover, it would be possible to separate crime in more types (such as burglary, robbery, criminal damage, vehicle thefts, other thefts, etc). The large number of zeros, however, does not allow to use these crime types separately.

Although many respondents' characteristics are available, only controls for basic demographic characteristics, such as age, gender, regions and ethnic background are considered in the empirical investigation. This followed strategy is attributed mainly to two reasons. Firstly, the main target of this study is to identify whether immigrants' criminal behaviour (mainly for property crime) would be different from natives' one if immigrants and natives shared the same basic demographic characteristics. Of course, it would be interesting to explore the behaviour of the impact of immigration on criminal behaviour once other controls such as education, working status, parental characteristics, marital status, risk factors, etc, are included. However, most of these variables are derived from questions that involve only people older than 17 years old, which results in reducing the sample by around 2,500 individuals. Moreover, some other variables, such as risk factors, contain many missing cases which would reduce the sample size even more.[2] Thus, the reduction in the sample size is

[1] Violent crime consists of: assaults with and without injuries, and commercial and personal robberies. Notice that both property and violent crime include robberies. However, also note that only 9 individuals reported a robbery.

[2] Some of the independent variables include many missing values, ranging from 180 to 424 cases. Dropping all these missing cases would result in losing around 1,300 extra cases. Instead, dummy variables, which take

the second reason why these controls are not used. Actually, the empirical investigation showed that when the estimators that control for misreporting are used, the variation of the reduced sample does not allow identification of the parameters of interest.[1] Instead, an "open" discussion will try to identify the factors that result in potential estimated crime differentials between immigrants and natives.

Proceeding now to the independent variables, it would be proper to first discuss the main regressor, a dummy that indicates whether someone is an immigrant or a native. While it is common in empirical studies to define an immigrant as the person who is not born in the host country, a question for country of birth is not available in the questionnaire. The question used to construct immigration status is the following: "Can I just check how long have you lived in the United Kingdom?" Respondents that replied with "All my life" are considered as natives. Otherwise, they are classified as immigrants.[2] A limitation of this construction is that there can be some natives who had left UK but returned after a certain period of time. These people may have categorized themselves as living in the UK less than their whole life, therefore as "immigrants", although they should be considered as natives, particularly if the period of staying outside the UK was very small. People born in the UK but lived most of their life in another country would exhibit very common characteristics with immigrants. Thus, it would not be very unreasonable to place them in the same group with actual immigrants. Nevertheless, I would not expect this number to be large enough, as according to the core sample, the weighted percentage of people who did not live in the UK their whole life is 9.2%, which is quite close to the percentage of immigrants in the UK from other sources in 2003.[3] Although in the initial core sample only 729 immigrants appear, I have increased their number to around 2,000, by exploiting the youth-boost and most importantly the

the value one if the associated variable has a missing value could be constructed and used together with the "parent" variable. However, the use of these variables seemed impossible, since in most cases the variation between the regressand and these "missing" variables is very low.

[1]Actually, to achieve convergence we had to impose many exclusion restrictions from both processes, which led to bad misspecification of the models, since some of the excluded variables actually belong to the two processes. Thus, using these models would not be enough to shed light on the question of interest, and possibly it would result in misleading conclusions.

[2]Respondents had to choose among the following alternatives: 1) Less than 12 months, 2) More than 12 months but less than 2 years, 3) More than 2 years but less than 5 years, 4) more than 5 years but less than 10 years, 5) 10 years or more but not the whole life, and 6) All of his/her life.

[3]For instance, according to the OECD estimates, the proportion of foreign born population in the total population in the UK was 8.8% in 2003 and 9.3% in 2004.

nonwhite-boost. This has been done mostly to increase precision of "immigrant" estimates. To restore representativeness, as described shortly before, a combined weighting variable of the weights of the three distinct data sets is used.

In the remainder of this section a description of the other covariates that are used in the regression analysis is presented. These variables involve very basic characteristics of the population, such as age, gender, and region of residence. Descriptive statistics of the independent and the dependent variables can be found in Table 2.6.

It is well known in criminologists' research that age is closely linked to criminal behaviour (see, for example, Farrington, 1986). Most evidence suggests that crime peaks in the teenage years and then falls steadily. Therefore, higher powers for the "age" variable should be also used. Gender is another very significant determinant of crime as men's crime rates are universally much higher than women's ones (see, for example, Steffensmeier and Allan, 1996). Thus, a dummy distinguishing men from women is used. Finally, controlling for the region where the respondent lives seems to be quite important to capture regional unobserved characteristics associated with crime (see, for example, Glaeser and Sacerdote, 1999), such as high poverty rates, or high unemployment rates. Moreover, different areas may be associated with higher returns to illegal acts, or, lower probabilities of arrest. Using the standard regional variable, ten regional dummies have been constructed. Nonetheless, the very demanding econometric models require the grouping of regions into four groups. These are North (North, York, North West), Midlands (East, West, and Wales), and South (East Anglia, South East and South West). London will be the baseline group.

In a second specification also ethnic background is added. Criminologists have devoted much research on how different ethnic groups relate to different criminal behaviour (see, for example, Torny, 1997). Most of them find that individuals from ethnic minorities are disproportionately represented in official crime records.[1] However, using self-reports the opposite is found (see, for example, Sharp and Budd, 2005). The reasoning behind any possible link between different ethnic groups and crime shares many arguments similar to the link between immigration and crime. Since a higher fraction of immigrants come from ethnic minorities compared to the native population, we would expect that inserting dummies

[1]It must be stressed that ethnic minorities are most of the times defined as non-white individuals.

for ethnic groups would have a strong effect on the impact of immigration status on crime.[1] Five dummies for ethnic groups are constructed. White or not, Black or not, Asian or not, Mixed or not, and Other Ethnicity or not. As the proportion of people of other ethnicity is very low, Other Ethnicity is grouped together with Asians. This does not seem inappropriate as around 50% of people from other ethnic groups are Chinese.

Finally, I would like to devote a few lines to two "special" variables which will be also used in the next sections. As described in Section 2.5 identification of count data models requires at least one exclusion restriction on crime process, or a sign restriction on the reporting process. Although, as we will see at the next sections, there is no available *a priori* information on any sign of the parameters in the reporting process, we can use some information in the data set to impose an exclusion restriction on the crime process by constructing a variable assumed to belong to the reporting process only. Note that, as was also described in Section 2.5, the binary choice models are identified even without any exclusion restriction. However, this extra information will be also used in binary choice models in order for the analysis to be consistent across all models, and because it can also facilitate the estimation procedure. However, as will be made clear in the subsection 2.8.4 the exclusion restriction does not drive the binary models results. In the next two paragraphs two available options are described.

Firstly, respondents have been asked whether they replied to the questions concerning crime truthfully. Thus, a dummy variable of truthfulness can be generated. As will be explained also later, it is reasonable to assume that this variable belongs only to the reporting process, because can be considered as a characteristic that shapes the reporting behaviour. Therefore, since this variable appears only in the reporting process, we technically have an exclusion restriction on the crime process. This will help to distinguish the probability to report a crime from the probability to commit a crime, even though reliability of responses on this question is also doubtful.[2]

Secondly, although interviewers tried to provide a private environment while conducting the interviews, in 32% of the cases (3,768 cases) there was someone else present during

[1] In the core sample, the fraction of immigrants to the total population of ethnic minorities is about 61%.

[2] It is noteworthy that 93% of the respondents replied that they truthfully answered all questions concerning crime.

the interview, mostly in the cases of young individuals. Even though crime questions in the OCJS are self-completed in a computer (as opposed to face-to-face interviews), and although it was stressed by the interviewers that nobody should disturb the interviewee during the self-completion part, it is still possible that presence of someone else could affect the reporting behaviour of the respondents. Therefore, a dummy is constructed that takes the value 1 if someone else was present. Since there are 409 missing cases, I have also constructed a dummy that takes the value one for these missing cases. Moreover, in the cases where someone else was actually present during the self-completion part, there is the extra information whether this other person actually looked at the screen (15% of the 3,768 cases). Thus, a dummy variable is generated to capture the fact that the reporting behaviour might have been more affected in the cases where someone else looked at the answers. More discussion of these variables will follow in subsections 2.7.2 and 2.8.4.

2.7 Main Results

As discussed in Section 2.5, the main results follow from the binary choice variable, whether or not someone has committed a crime last year, whereas the extra information from counts is used for robustness checks.[1] In this section, first the results of conventional models for binary and count data are presented. The main findings of the binary models that allow for misclassification probabilities follow in subsection 2.7.2. It must be stressed that throughout the empirical analysis the appropriate sampling weights to restore representativeness of the sample are used. This is mainly because there are different sampling probabilities for young people (youth-boost) and ethnic minorities (ethnic-boost).[2]

[1]All the empirical analysis of this study is implemented in Stata® and TSP® econometrics software.

[2]It must be stressed that in a stratified sample, which is the case in the current sample because of different sampling probabilities for youth boost and ethnic boost groups relative to the core sample, if the conditional expectation is correctly specifies, the unweighted MLE is still consistent and more efficient than the weighted MLE (see, for example, Wooldridge, 2010). However, since weighted and unweighted regressions yield different estimates across different models, it is more appropriate to use the sampling weights. However, as will be discussed in subsection 2.8.3, the unweighted results of the covariate-dependent Probit with misclassification are very similar to the weighted ones.

2.7.1 Preliminary Results

To acquire a first idea of the impact of being an immigrant on crime, conventional Probit results of regressing the crime variables on immigration status only are presented in Table 2.7.[1] Even though this work focuses on property crime, in this table also results for all the different categories of crime are presented in order for the reader to obtain a more general idea. Assuming that there is no misreporting, this table clearly shows that being an immigrant does decrease the probability of committing crimes for all categories. Table 2.8 portrays the results for the count form of these variables, using a Negative Binomial 2 model (NB2). Not only are immigrants less likely to commit crimes but they also commit fewer crimes. This is evident in these results, since taking into account the number of reported crimes, immigration coefficient becomes even more statistically significant in all but drugs related offences. As mentioned in the previous section, there are 54 extra missing cases in the count form of property crime variable. However, this is not much of a concern since the Probit results hardly change even when these missing cases are dropped.

As these models are nonlinear, interpretation requires the calculation of marginal effects. Focusing on property crime, we find that being an immigrant reduces the probability of committing a property offence in last year by 1.81 percentage points (from 5.68% to 3.87%), a relative change of 46.78%. Regarding the NB2 model, this preliminary estimation says that, without taking into account differences in demographic factors, an immigrant yearly commits 0.160 crimes on average whereas a native commits 0.366 crimes, a difference of 0.206 crimes (a percentage increase of 128.75%).

Nonetheless, it would be more interesting to see whether immigrants exhibit different criminal behaviour than otherwise comparable, in terms of basic demographic characteristics, native-born individuals. Thus, in Tables 2.9 and 2.10 we look at this difference, once we have controlled for fundamental demographical features such as gender, age and region of residence in specification 1, and also for ethnicity in specifications 2 and 3. Although the sign maintains, the statistical significance fades away in both binary and count models.[2]

[1] Although, Complementary Log-Log (CLogLog) models might be more appropriate when there are so many zeroes, as it assumes an asymmetric cumulative function, the empirical results showed that nothing was gained by using this model, in terms of better fit or any differences in the estimates. Moreover, specification tests were not of support of using an asymmetric binary model such as the CLogLog.

[2] Although insignificant, it would be interesting to evaluate the magnitude of these coefficients once we

Specifications 2 and 3 show that the coefficient on immigration dummy becomes even smaller and less significant once we control for ethnicity. This is expected, since immigrants are relatively more nonwhite than natives (see, Table 2.6) and, as the results show (without controlling for under-reporting), white individuals are more crime-prone. However, in the NB2 model the immigration status coefficient does not lose so much of its magnitude. Finally, from specification 3 in both Tables 2.9 and 2.10 we can see that Asians & Others, and in a lesser degree Blacks, are less prone to crime than otherwise comparable Whites.[1]

The above results are just a first indicator of the crime picture as we can hardly draw inference for the actual crime relying on the reporting crime, unless there is no under-reporting. According to these results we are only confident to say that for some reasons immigrants report less crime than natives, but this difference fades away once we control for some basic characteristic. However, someone would argue that this difference exists because immigrants may under-report criminal activities by more than natives. If this is the case, immigrants may still commit more crimes than natives but at the same time under-report by more, resulting in this negative coefficient. This can also be argued for the case of white individuals if the groups of ethnic minorities are reluctant to report truthfully. Nevertheless, more appropriate (yet, parametric) models that control for this possibility, resulting in estimated coefficients corresponding to the actual crime, are presented in the following subsection.

2.7.2 Probit Model that allows for Misclassification

2.7.2.1 Constant Misclassification

In this subsection the results of the MisProbit model are presented. As discussed in Section 2.5.1, this is a parametric model that takes into account both under-reporting (misclassifica-

have constructed a "representative" individual. Thus, what would be the difference in the probability of committing a property crime between a native and an immigrant, who are both 25 years old, males, and live in London? According to this model this is -1.95 percentage points but statistically insignificant, a relative effect of 26.37%. The NB2 model says that being an immigrant, holding all other characteristics constant, reduces the expected number of crime by 0.12, which is statistically insignificant as well.

[1] Note that the variable 'age' and its quadratic do not provide a very good fit. To obtain the pattern of the impact of age on property crime we need to include up to the fourth power of age. Following this, immigration dummy's effect becomes slightly more significant. Here, we present only the quadratic term in order to be in line with the specifications we use in the models that control for misreporting.

tion of one as zero) and over-reporting (misclassification of zero as one). This model captures the actual probability of committing a crime, but it will be misspecified if probabilities of misclassification do depend on covariates. Although it may be sensible for over-reporting to be considered as constant, since we can assume that people may over-report randomly, it cannot be the case for under-reporting. It is highly possible that the same characteristics affect both the probability of committing a crime and the probability of reporting it. The assumption of constant misclassification will be relaxed in the next subsection.

Table 2.11 presents the results of MisProbit in three specifications, as in the previous subsection. There are a few important findings that deserve some discussion. To start with, we notice that in the 1^{st} specification, being an immigrant still decreases the probability of committing a crime. However, this coefficient is even less significant than the conventional Probit. We can also see that this coefficient turns positive once we control for ethnicity but it is always very statistically insignificant.

It is also very important to stress the estimated value of misclassification of one as zero, which is around 81% and statistically significant at any level of significance. This seems very large, as this estimate indicates that 81% of people have committed crimes but have reported none of them. However, as emphasized in the previous section, we must be cautious with the interpretation of this probability as this model cannot distinguish the probability of under-reporting from the probability of never committing a crime and therefore never reporting one (zero-inflation). Nevertheless, nothing can be said about the importance of each, unless we model it somehow into the likelihood function.[1] In any way, the interpretation of the coefficients does not change, which still capture true crime given that misclassification is constant. Concerning the probability of misclassification of zero as one, it has a clear-cut interpretation as probability of over-reporting, since interpretation as one-inflation seems unreasonable.[2] Moreover, the estimated value of this probability of 0.012 is also expected,

[1]Such a model together with results is presented in Appendix A. Although it seems that identification of this model requires the same conditions as the simple MisProbit, it does not behave very well in estimation terms and consequently its results are questionable. This may be a consequence of the very noisy (crime self-reported) data used in this paper. Future research using less noisy data and larger/richer samples could reveal more interesting things about the behaviour of this model. Also, further theoretical investigation of this model could reveal interesting outcomes. Also, the parametric assumptions of this model may be too strong to give reasonable estimates.

[2]In this context, one-inflation would mean that: given the set of covariates x_i some people always commit and always report that they have committed crimes independent of these x_i.

as we would not expect that people would report crimes that they did not commit. However, we cannot ignore it since it is very statistically significant.

Moreover, as was also discussed in Section 2.5.1, although the sample size is fairly large, the estimates are very imprecise perhaps because of the noisy nature of crime self-reports.[1] The only coefficients that preserve some of their significance are the coefficients on gender and ethnicity. We also notice that although less imprecise, the coefficients are quite larger in size. Furthermore, the average conditional probability of committing a crime calculated as $\widehat{\Pr(y_i = 1)} = \sum_{i=1}^{n} \Phi(x_i'\hat{\beta})/n$, is now around 29%. This is much higher than the predicted average probability of the simple Probit model, which is calculated to be 6%. More discussion on this finding will follow in the next subsection.

It can be also noticed that the maximum of the MisProbit model corresponds to a log likelihood value that is only slightly larger than the log likelihood of the conventional Probit, even though two extra statistically significant parameters are added into the model. This can be attributed to the fact that the estimated coefficients of the MisProbit model are less precise in comparison to Probit.

2.7.2.2 Allowing Misclassification of 1 as 0 to depend on Regressors

As opposed to the previous subsection, this subsection presents the results of a MisProbit model in which misclassification of true ones as zeroes (under-reporting) is allowed to depend on regressors, whereas misclassification of true zeroes as ones (over-reporting) is assumed to be constant.[2] Since the same individual is responsible for both actions of committing and reporting a crime, logically both processes are functions of the same variables.[3] However,

[1]Note also, that if misreporting exists, the standard Probit model overestimates the asymptotic t-statistics. As Hausman, Abrevaya and Scott-Morton (1998) point out, the higher the misclassification probabilities, the more difficult it is to obtain a good fit. However, the estimated variance will correspond to the true precision, given of course that misclassification is constant.

[2]Treating over-reporting as constant helps identifying all the parameters of interest. Although constant over-reporting is a sensible assumption to make, there might still be cases where this probability depends on the regressors. Nevertheless, the estimation analysis showed that identifying all coefficients of a fully specified model (a model that includes the same regressors in both probabilities of misclassification) seemed impossible. The estimation analysis also showed that it is feasible to identify all parameters of a model where there are extra exclusion restrictions from the reporting process (these results are available upon request). However, this practice would result in a mispecified model, since many variables that actually belong to the reporting process are excluded.

[3]For instance, age affects both the probability to commit a crime, as younger people commit more crimes in general, and the probability to report a crime, as younger individuals would be less willing to reveal their true criminal behaviour. Similar arguments hold for the other independent variables.

as mentioned in Section 2.5, exclusion restrictions would assist the estimation procedure, even though this model is identified even without any exclusions (see, Hausman, Abrevaya and Scott-Morton, 1998). To be consistent with the NB2-Logit model which is used as a robustness check (see, subsection 2.8.1), an exclusion restriction on the crime process is used, as this is crucial for the identification of the NB2-Logit.[1] The exclusion must be "strong", in the sense that the variable which is excluded from the crime process must have a significant effect on the reporting process. Otherwise, inserting variables that have no effect on the reporting part, and at the same time are correlated with the rest of the variables in this part, could result in undesirable outcomes. As described in the previous section, some information in the data set can be used to construct two variables that are assumed to affect only the reporting process.

A first choice would be to use the information whether someone else was present during the self-completion part. There is evidence, at least for face-to-face interviews (see, for example, Aquilino, 1993) that someone else's presence during responding to sensitive questions affects the reporting behaviour. However, since the questions about crime were (computer-based) self-completed, which is a much more private environment, the effect of this dummy could be much smaller than in face-to-face interviews. Indeed, as the results show (see, subsection 2.8.4), this turns to be a "weak" restriction.

In another direction, the variable "truthfulness" can be exploited, a dummy that takes the value one if people said that responded in all questions concerning crime truthfully. This variable is used only in the reporting process, as it makes sense to assume that whether or not someone has truthfully reported his/her actual criminal activity at the time of the survey could not affect the action of committing a crime before the survey took place. If any empirical relationship exists, this would be because "truthfulness" is correlated with unobserved characteristics correlated with crime, or because there is a reverse causality of committed crimes on "truthfulness".[2] The problem is that it is also not correct to assume that "truthful-

[1]Note that exclusion restrictions on the reporting process does not solve the identification problem (see, Papadopoulos and Santos Silva, 2008)

[2]For example, the probability to answer "I was truthful" would be higher for people who commit more crimes but report fewer, if this was a way to hide misreporting. Or, it might be that, it is less possible for people who commit no crimes to say that they are not truthful, as there is no reason for them to lie. In both cases we would expect a negative relationship between reported crime and "truthfulness". In fact, a weighted Probit regression of "trufulness" on number of reported property crimes, showed that this is actually the

ness" actually affects the reporting behaviour, unless the reported "truthfulness" coincides with the actual behavioural characteristic of how truthful someone is. However, what we assume here is that being "truthful" while answering questions about crime is a feature that "shapes" some behavioural attributes, which in turn affects the reporting behaviour. In any way, as will be discussed also later, the results show that "truthfulness" actually has a very significant effect on the reporting process but no effect on the crime process, once "truthfulness" is included in both processes.[1]

According to all the above, the main results of this section are based on the specification where "truthfulness" affects only the reporting process. In the robustness check section it will be shown that this exclusion restriction does not drive the results. Some results of using "other's presence" as an exclusion restriction are also presented in the robustness check section. There, we will also see that the inclusion of this dummy has some undesirable effects on the estimation of the model. As we discussed in the previous section a dummy "someone looked at the screen during the self completion part" can be also constructed. The results also show that this information is unrelated to the probability of not reporting committed crimes.[2]

The results of this model are depicted in Table 2.12. The estimated coefficients of the crime process are reported in the upper part of this table, whereas the coefficients of the "reporting" process are presented in the lower part. Before discussing the effect of the immigration dummy, I would like to mention some main features of the findings of this model. First of all, the log likelihood corresponding to the global maximum is considerably improved comparing to the previous model. Since the covariate-dependent MisProbit model nests the MisProbit with constant misclassification (when all coefficients but the constant of the "reporting process" are zero), we can construct a likelihood ratio test to test whether misclassifying one as zero is constant. Since the likelihood ratio statistics is around 49 for all three specifications, there is strong evidence against the null hypothesis that misclassification

case.

[1]This is true for both binary choice, and count data models. If "truthfulness" is included only in the crime process, it has a small but statistically significant effect.

[2]This dummy has no effect either using it as an interaction term (so that conditional on the presence of someone else there is no effect of some of them looking at the screen), or using it alone without controlling for the cases where someone was present but did not look at the screen.

of one as zero is constant.[1]

According to this models, the predicted average probability of committing a property crime during the last period, calculated as $\sum_{i=1}^{n} \Phi(x_i'\hat{\beta})/n$, is around 29%, which is in line with the result of the previous subsection.[2] However, notice that if we accept the interpretation of misclassification of 1 as 0 as zero-inflation, this is actually the predicted probability of committing a crime only for those that participate in the binary choice model. Moreover, the average probability of misclassifying an one as zero, calculated now as $\sum_{i=1}^{n} \Phi(z_i'\hat{\gamma})/n$, is now 10 percentage points lower than the estimate of the Probit model with constant misclassification. Finally, notice that the coefficient of over-reporting is again around 1.3% and statistically significant at 1%.

Concerning the main objective of this research work, this table shows that even after controlling for any potential difference in the reporting behaviour of immigrants, the coefficient on immigration status is still negative and fairly larger than before. In the 1^{st} specification this coefficient doubles in size, comparing to the previous model, and becomes statistically significant at 10% significance level. However, after controlling for ethnicity, although still negative and fairly large, it becomes insignificant. Finally, from all three specifications we can say that being the "representative" individual and immigrant, reduces the probability of committing a property crime by around 6 percentage points, before controlling for ethnicity, and around 4 percentage points, after controlling for ethnicity.[3]

The reason why immigration status coefficient becomes larger in magnitude can be attributed to the fact that native-born individuals in fact under-report by more than immigrants, and not the opposite. However, as mentioned before, the coefficients on the reporting behaviour can also take a zero-inflation interpretation. Therefore, the negative coefficient might also mean that a smaller proportion of immigrants belong to the group of genuine

[1]A Wald test that all coefficients of the "reporting" process but the constant are zero gives similar results.

[2]Note that, although the estimates of the BCS are not directly comparable to the ones from the OCJS, we have estimated that the probability of suffering a crime in 2008 was around 0.235. Notice finally, that in BCS commercial crimes and crimes against children are not included. Also note that crime rates were slightly higher in 2003 according to both recorded to the police crime and the BCS (see, Kershaw at al, 2008).

[3]The predicted probabilities to commit a crime for the "representative" individual are 4.27%, and 9.87%, for an immigrant and a native, respectively. This corresponds to the marginal effect of 5.6 percentage points, and relative effect of 131.14%. After controlling for ethnicity the above figures become $\approx$ 6%, 10%, 4 percentage points and 66.7%, respectively.

non-criminals. These two interpretations contradict each other but we cannot say which effect is larger. If the "reporting" process was measuring only under-reporting, it would be easier to analyze what would be the direction of the change in crime process coefficients once we control for the corresponding characteristics in the "reporting" process. Thus, if immigrants under-report by less, this would result in the coefficient of crime process to become more negative. However, the effects of changing the portion of zero-inflation on the crime process coefficients are not clear. According to this interpretation, a positive coefficient for immigration status would just mean that fewer immigrants comparing to natives participate in the binary choice decision of committing crimes or not. Thus, it does not give information about how the remaining individuals, who may or may not commit crimes, behave. In any case, we must say that the total effect of controlling for under-reporting on the coefficients of the crime process is not easy to predict, since this will depend on all inter-correlations of the variables across the two processes and all variables within a process. However, notice that the coefficient of immigration status on the reporting process is statistically insignificant in all specifications.

It would be also interesting to briefly discuss the effects of the other explanatory variables. To begin with, it is notable that being a white individual still increases the probability of committing a crime. However, this effect is only significant at 10% significance level. From the 3^{rd} specification, we notice that black individuals' coefficient is negative and significant at 10%, in contrast to the previous model where it was very insignificant. On the other hand, the coefficient on "Asian & Others" dummy is still negative but now insignificant.

Concerning gender, the sign on "male" dummy is still the expected one since males commit more crimes than females. Males' coefficient is still significant at 1% significance level, even though the sign in the reporting process is negative and significant at 10%. This negative sign indicates that females are more reluctant to report their criminal activities truthfully, perhaps because of "embarrassment" effects.[1] However, this may also indicate that it is more likely for a female to belong to the genuine non criminal group of people, which is also reasonable.

The results of the regional dummies are also interesting. First of all, including these

[1]It is relatively less acceptable by the society if a woman commits a crime.

dummies in the reporting process, we control for area-specific unobserved characteristics that may affect the decision to misclassify. We see that people who do not live in London commit more crime. Comparing to the previous models, the magnitude and significance of these three coefficients increase. Nevertheless, only people living in North England seem to commit significantly more crime (but just in 10%). This change may be attributed to the significant effect that this dummy has on the reporting process.

Furthermore, we can see that both "age" and "age squared" have a significant effect in both processes. Concerning the crime process, the negative sign on age and the positive sign on age squared variables indicate that crime falls with age but in a decreasing rate. A quick calculation using the coefficients in Table 2.12 show that the minimum is reached at about 42 years of age. We must notice that these results do not coincide with the theoretical views of the effect of age on crime, where crime increases with age during the adolescence years and then steadily falls. Although for conventional Probit this shape is captured once a quartic on age is included, for covariate-depended MisProbit, including higher powers of age does not provide a better fit.[1] The coefficients of age and age^2 variables in the reporting process are also negative and positive respectively. This also predicts that people under-report by less as they are getting older at a decreasing rate (or that they are increasingly likely to switch to the category of potential criminals).

Special attention must be finally paid to the effect of the "truthfulness" dummy. As mentioned before, this dummy belongs only to the reporting process.[2] Table 2.12 shows that this dummy has the largest coefficient among all the regressors in the reporting part. It is also statistically significant at 1% significance level. This coefficient can be interpreted in two contradictory ways. Respondents who replied that they answer all crime questions truthfully are honest people who, regardless the other observed characteristics, never commit and never report crimes. On the other hand, this coefficient may also indicate that "truthful"

[1] Regressions that include up to the 5^{th} power of "age" have been performed. Including more powers, results in no convergence of the optimization procedure.

[2] The estimation results, when "truthfulness" is also included in both processes of the covariate-dependent MisProbit model, show that this dummy has no effect in the crime process. The rest of the estimates are virtually unchanged. Thus, these results also support the theoretical assumption that "truthfulness" should not be included in the crime process. However, even if there was an effect, this would be misleading since a significant effect would just capture unobserved characteristics that are both correlated with crime and how truthful someone is.

respondents are people that under-report by more than "non truthful" respondents, so that they commit more crime than what they actually report.

2.8 Robustness Checks

2.8.1 Count Data Models

In this section we examine whether the results of modified count data models that take into account under-reporting coincide with the main findings of the MisProbit. Of course these two models are not directly comparable, since the count data models use the extra information of the number of property crimes. Nevertheless, similar estimates between the count and the binary models used in the present study, mostly with respect to the reporting process, would strengthen the reliability of the results of the main analysis. As described in Section 2.6, the unconditional variance of the dependent variable is much larger than the unconditional mean. This is a first rough but strong indicator against the equi-dispersion assumption of the Poisson distribution that the mean equals the variance. Therefore the NB2 distribution, that allows for over-dispersion by using an extra parameter that accounts for extra unobserved heterogeneity, may provide a better fit to the data. Table 2.13 portrays the results of three NB2 models. The 1^{st} column reproduces the results of the simple NB2 model for the sake of comparisons. The 2^{nd} model, which is the NB2-Logit, controls for under-reporting, while the 3^{rd} one also incorporates Zero-Inflation.[1]

Regarding the NB2-Logit model, Papadopoulos and Santos Silva (2008) showed that unless exclusion restrictions are imposed on the count part, there are two linearly dependent sets of parameters that correspond to the same maximum likelihood value. Consequently, the model is unidentified since it cannot be said which set of estimated parameters is the "correct" one. Although sign restrictions in the reporting part is a possible solution, here there is not any *a priori* information to suggest the sign of any of the parameters of the reporting process. Therefore, in line with the binary choice models, to identify all the parameters of the model the "truthfulness" dummy is used in the reporting process only. In

[1] The formulation, the density, and the log likelihood function of the ZI-NB2-Logit is presented in Appendix B.

subsection 2.8.4 we will show the consequences of excluding the dummy "other present" from the crime process. However, we must be very cautious since, although the model is globally identified, there can still exist more than one maxima. Therefore, a thorough analysis must be performed to find all possible maxima.[1] Also notice that this model does not control for over-reporting, differing in this aspect from binary choice models. However, the covariate-dependent MisProbit gives very similar results even when the probability of over-reporting is assumed to be zero.[2] It is also important to stress that the structure of this model provides more information about the data generating process than the binary choice model. The binary models only provide information about reporting or not crimes, regardless of how many crimes someone has committed. This model on the other hand, provides estimates for the probability of any given committed crime to be reported.[3]

The regression analysis showed that the global maximum of the NB2-Logit is the one depicted in Table 2.13.[4] The upper part of this table presents the coefficients of the crime, or differently, count process. The lower part presents the coefficients of the reporting process, which is the probability of reporting a committed crime. First of all, the very large value of α must be noticed, which is statistical significant in any significance level.[5] Therefore, there is evidence that the data are over-dispersed even after conditioning on the set of regressors.

Concerning the immigration status coefficient in the crime process, we can see that even after controlling for under-reporting, it is negative and even larger in value than the conventional NB2 model. This is the consequence of immigrants' coefficient in the reporting process being positive. That is, being an immigrant increases the probability of reporting a given crime and therefore decreases the conditional expectation of crime by more than the

[1]Some tips on several possible ways to find the best maximum are described in Papadopoulos and Santos Silva (2008). In the present analysis, the regression analysis showed that several local maxima exist.

[2]These results are available upon request. This similarity of the coefficients across the two models was expected, since the probability of over-reporting is too small to affect the parameters of the other processes.

[3]Although this probability is allowed to differ across individuals, it is assumed to be constant for all committed crimes by individual i regardless of the number of committed crimes he/she has committed.

[4]The estimation analysis showed that another maximum exists with log likelihood value of 2,315.80. This maximum corresponds to coefficients very different from the global maximum. These results are available from the author upon request. As shown in Papadopoulos and Santos Silva (2008), there is a relationship between the coefficients of the two models. A brief description of this relationship follows in subsection 2.8.4. Although the log likelihood value of this local maximum is close to the log likelihood value of the global maximum, the difference is sufficient to permit identification of the "correct" maximum depicted in Table 2.13. This is because the excluded variable "truthfulness" has a strong significant effect on the reporting process. In subsection 2.8.4 we show what are the consequences of a "weak" exclusion restriction.

[5]The log likelihood of the corresponding global Poisson-Logit maximum is -9,132.31.

conventional NB2 model. This finding is consistent with the binary models. In addition, in line with the binary models, the coefficient of immigration dummy is insignificant in both processes. I would like to mention that in NB2-Logit, contrary to the covariate-dependent MisProbit, the immigration status coefficient does not depend on whether or not we control for ethnicity. This is the reason why only the model that controls for ethnicity is presented in this subsection. Finally, the marginal effect of our "representative" individual says that being an immigrant decreases the expected number of crimes by around 0.19 crimes (this difference was 0.11 for the simple NB2 model).[1]

As far as the rest of the coefficients in the crime process is concerned, their direction is in accordance with the coefficients of the MisProbit model. However, we must stress that the NB2-Logit models the conditional mean of crime events, whereas the MisProbit models the conditional probability of committing a crime. Therefore, it is always possible that a few differences exist across binary and count data models, even if both models are correctly specified.[2]

As far as the reporting process is concerned, this model predicts that the average conditional probability of reporting a committed crime is 43%. However, we need to distinguish this probability from the probability of under-reporting in the binary models. The MisProbit estimates the probability of reporting zero crimes given that some crimes have been committed (or the probability of never committing and consequently never reporting crimes), whereas the NB2-Logit model estimates the probability of someone reporting a committed crime, regardless of the number of crimes he/she has committed (see also footnote 78). Therefore, this model is in a sense more structural, since it also captures cases where people under-report but still report some of their crimes. On the other hand, the binary model ignores this kind of under-reporting since it can capture only reporting zero crimes. We can see that most of the coefficients of the Logit (reporting) part in the NB2-Logit have opposite signs to the ones of the under-reporting (zero-inflation) part of the MisProbit model. This

[1]The expected number of committed crimes by the "representative" individual are, 0.1771, and 0.3628, for an immigrant and a native, respectively. This corresponds to the marginal effect of 0.19 crimes, and the relative effect of 104.9%.

[2]For example, here the coefficient of "white" dummy is insignificant but it was significant at 10% significance level in the MisProbit model. This might mean that although white people are more likely to commit a property crime, they do not commit more crimes than nonwhite individuals, so that taking into account the extra information of the counts reduces the white-nonwhite crime differential.

is expected, since here we measure probability of reporting a committed crime, whereas in MisProbit we were measuring probability of not reporting crimes.[1]

In the 3^{rd} column, the results of the ZI-NB2-Logit model are presented. According to this model, some people never report crimes just because they never commit crimes, or because they do not report any of the committed crimes. Therefore, the zero-inflation probability, in line with the MisProbit, gives the proportion of people that totally under-report or the proportion of genuine non-criminals. The rest of the individuals may or may not commit crimes, but their responses are still subject to under-reporting. In this sense, we assume that not everyone is a potential criminal, so that not everyone participates in the choice whether or not to commit crimes. In other words, the Logit part of this model measures the probability of reporting a committed crime once we partial out people that always under-report with zero, or people that never commit and consequently never report crimes.

Since identification of this model requires the same conditions as in NB2-Logit, the exclusion restriction of "truthfulness" from the crime process is also followed. To model the zero-inflation process I make use of the same variables I use to model the crime process. The coefficients of the ZI-NB2-Logit model that correspond to the global maximum are presented in the last column of Table 2.13.[2] An interesting finding of this model is that the conditional predicted probability of zero-inflation is around 62%. This is very close to what the covariate-dependent MisProbit predicted. There, the same probability was calculated to be around 71%.

This model also predicts that, even after controlling for zero-inflation, the probability of reporting a committed crime is 37.6%, which is even lower than in NB2-Logit model. According to this model, once we control for zero-inflation and under-reporting, immigrants' coefficient decreases even more. However, it is still statistically insignificant as the precision of the estimate decreases. We can also notice that in ZI-NB2-Logit, males' coefficient

[1] However, I would like to repeat that in MisProbit we could not separate zero-inflation from under-reporting, and therefore exact comparison between the coefficients of the two models would not be appropriate. Anyhow, the only striking difference is that the NB2-Logit model predicts that being a male decreases the probability of reporting a given crime, while in the MisProbit being a male decrease the probability of reporting no crimes, either because of misclassification or because of never committing and consequently never reporting crimes.

[2] As was the case for NB2-Logit, the estimation analysis shows that, again, another maximum exists with log likelihood value of 2,261.28. These results are available from the author upon request.

becomes insignificant. More interestingly, the coefficient of "white" dummy turns negative. This is perhaps because of the negative and statistically significant coefficient of this variable on the zero-inflation process, which may mean that the probability of white individuals to always under-report with zero is smaller than non-white persons. Finally, we notice that people who live in South and in North commit more crimes than people who live in London (significant at 5% and 1% respectively).

Since the "zero-inflation" process of the ZI-NB2-Logit has the same interpretation as the "misclassification of one as zero" process of the covariate-dependent MisProbit, it would be interesting to compare the corresponding coefficients of the two models. We notice that apart from the coefficients on "age" variables, all other coefficients follow the same direction. Furthermore, it can be observed that there are a few differences in the statistical significance of some coefficients. Finally, the coefficients of the "reporting a committed crime" process are relatively similar across the two NB2 models.

2.8.2 Violent Crime

In this subsection we briefly investigate what is the relationship between immigration and violent crime. Although violent crime is more impulsive, some of the reasons used in Section 2.2 can be also applied here to hypothesize a link between being an immigrant and committing violent crimes. For example, according to Merton's (1938) "strain theory" immigrants may become violent due to accumulation pressure because of discrimination, or racist behaviour against them by native population. On the other hand, a credible behaviour associated with "no crime" would be a good path of integration in the host country. Furthermore, risk attitudes and deterrent effects, which are very important in explaining the decision to commit both property and violent crimes, might be different between immigrants and natives. Other reasons that act in the one or the other direction can be thought of. Therefore, as was the case with property crime, empirical investigation can offer more insights on this link.

The results, presented in Table 2.14, are obtained using the MisProbit model. The same specification as the 2^{nd} specification of Table 2.12 is used. In the 1^{st} column the results of property crime are reproduced, whereas in the 2^{nd} column the violent crime results are depicted. It is striking how close the results between the two models are, since apart from the coefficients of the regional dummies, all other coefficients are very similar.[1] We can see that the same basic demographical characteristics are good predictors of both violent crime as well as property crime. Also, we can see that the probability of committing a violent crime but not reporting it is lower than for property crime. Finally, separating ethnicity in three groups as before, I find that now Asians & Others is the least crime-prone group.

Concerning the effect of the immigration dummy, it is again negative but slightly less significant. Hence, immigrants are slightly more law-abiding than natives for both crime types. This might be because immigrants are more risk averse, or because they are more responsive to deterrent effects.

2.8.3 Weighted *versus* Unweighted Regressions of Property Crime

As mentioned at the beginning of Section 2.7, throughout the empirical research the appropriate weights to restore representativeness of the sample are used. However, if the conditional expectation is correctly specified, both weighted and unweighted estimators are consistent, but the unweighted one is also more efficient (see, Wooldridge, 2010). Thus, if the estimated parameters of the unweighted model are very close to the parameters of the model that uses weights, there is some support of correct specification of the model.

According to my results, the weighted estimates are very different from the unweighted estimates in the constant-misclassification MisProbit model.[2] It is noteworthy, however, that the coefficient values of the weighted estimates of the covariate-dependent MisProbit are very close to the unweighted ones (see, last column of Table 2.14). Moreover, it is evident that the coefficients of the unweighted regression are more precisely estimated. The only notable difference is that in the unweighted estimation the effect of immigration is higher, in terms of

[1] The tetrachoric correlation coefficient (see, Edwards and Edwards, 1984) is 0.5760, so that it is not the case that the results are too close just because the same people who committed property crimes also committed violent crimes. In addition, notice that although both crimes include robberies, this type of crime accounts only for a very small proportion of the total number of property or violent crimes (1.2% for property crime and 1.1% for violence.

[2] Although the differences in the coefficients between conventional weighted Probit and unweighted Probit are smaller than the differences between weighted constant-misclassification MisProbit and unweighted constant-misclassification MisProbit, this cannot be an argument that the conventional Probit model is a better specified model than the MisProbit. This is because Probit is designed to capture reported crime but MisProbit intends to capture actual crime. What these results might indicate, is that a Probit model is probably a correct specification for reported crime, but constant-misclassification MisProbit is a misspecified model for actual crime.

magnitude, and statistically significant at 5%. This might be the case because the unweighted estimator is more efficient, so that the immigration coefficient in the weighted estimation is less precisely estimated. Furthermore, as we have included an ethnic-boost data set, immigrant population is over-represented in my sample. Thus, using weights has as a result to down-weigh the immigration sample, which may induce differences in immigration status coefficient. Note also that among all variables used in these models the immigration dummy is the variable with the most zeroes. Hence, down-weighing (over-weighing) a variable with a low variation could result in higher differences between weighted and unweighted regressions, than down-weighing (over-weighing) a variable with higher variation. For instance, we can see that although the use of the weighting variable down-weighs young people (as we use a youth-boost), the differences of the age variable coefficients are marginal. Finally, notice that the biggest differences are observed in the coefficients of the variables that are insignificant when we use weights, such as the coefficients of the regional dummies.

2.8.4 Are the Results Driven by the Exclusion Restriction of the "Truthfulness" Dummy?

In this subsection I briefly intent to show that the main results are not driven by the use of the exclusion restriction "truthfulness". Indeed, the covariate-dependent MisProbit gives quite similar results even without any exclusion restrictions. Moreover, we briefly examine the consequences of using the dummy "other present" as an exclusion restriction, which seems to be a rather weak restriction for property crime but a "strong" restriction for violent crime. On the other hand, exclusion restrictions are necessary to identify both the NB2-Logit and the ZI-NB2-Logit models. Therefore, also the results of using "other present" as a dummy in count data models are discussed. It needs to be stressed that when the dummy "other present" is used, as was explained in Section 2.6, a dummy for its missing cases must be also included.

First of all, the results of the covariate-dependent MisProbit are discussed, which are given in Table 2.15 for property crime and Table 2.16 for violent crime. The 1^{st} specification of Table 2.15 and Table 2.16 reproduces the 1^{st} (for property crime) and 2^{nd} (for violent crime) specifications of Table 2.14 respectively, where the variable "truthfulness" is used. For both property and violent crime we can clearly see that the coefficients of the 2^{nd} specification, where there is no exclusion restriction, are fairly similar to the coefficients of the 1^{st} specification. Concerning the coefficient of main interest, we can see that for both property and violent crimes, the probability for an immigrant to commit a crime slightly increases, but it is still negative and similar in terms of significance. Thus, according to these results, the use of "truthfulness" does not affect the main results of the model.

In specification 3 of Table 2.15, we look at the consequences of excluding "other present" from the property crime process. It seems that this dummy has no effect on the reporting process of property crime, therefore, the restriction is very "weak". We can see that inclusion of "other present" actually results in much less precise estimates for most of the parameters.[1] Thus, not only has this dummy no effect on the probability to under-report, but its interaction with the other variables in the reporting process also worsens the general behaviour of the model.[2] Consequently, the effect of immigration status dummy, which is the case for most variables, decreases in both magnitude and significance. However, it still retains its sign. On the other hand, it occurs that "other present" has a significant positive effect (at 1% significance level) on the reporting behaviour of violent crime (it increases the probability of misclassification of one as zero). Contrary to property crime, it is noteworthy that all the estimates of this specification are very close to both the 1^{st} and 2^{nd} specifications of Table 2.16, in terms of both precision and magnitude. Again, the magnitude of immigrants' coefficient slightly decreases but so does the standard error, leaving significance almost unaffected. Thus, all together, there is some evidence that the results of the covariate-dependent MisProbit are robust in relation to the exclusion restriction, as long as this is a "strong" restriction.

Regarding count data models, in Table 2.17 I present results of including "other present" in the reporting process of NB2-Logit model to test whether the results of count data models are also robust in relation to the exclusion restriction.[3] The 1^{st} column reproduces the results

[1] Note that, if the variable for the missing cases of "other present" is not included, the precision of the estimates increases, although it is still worse than the other two specifications.

[2] If we include "other present" in the crime process as well, regardless of whether we include dummies for the missing cases, we obtain much more precise estimates which are fairly close to the 2^{nd} specification.

[3] Results of ZI-NB2-Logit, which are also available from the author on request, are very similar.

of the 2^{nd} column of Table 2.13 for the sake of comparisons. As can be seen from the 2^{nd} column, "other present" has again no effect on the probability of reporting a committed property crime. However, in this case we must be very careful as another maximum very close (in terms of the log likelihood value) to the global maximum exists that corresponds to very different parameter estimates. This is presented in the 3^{rd} column. As Papadopoulos and Santos Silva (2008) show, it appears that there is a close relationship between the parameters of the two maxima. Given that $\theta = (\beta, \gamma)$ is the set of true parameters of the model, where β corresponds to the vector of parameters of true crime and γ to the vector of parameters of the probability to report a committed crime, if the exclusion restriction is "weak", another maximum very close to the true one exists with parameter values $\tilde{\theta} \simeq (-\beta, \beta + \gamma)$. The stronger the exclusion, as for example the case for "truthfulness", the easier it is to distinguish the correct maximum and the higher the deviation of $\tilde{\theta}$ from $(-\beta, \beta + \gamma)$ becomes.[1] Despite the identification problem of this model, given that we accept that the correct maximum is the one in column 2, it is clear that the estimated parameters are very similar whether we use "truthfulness" or "other present" as an exclusion restriction. Once more, the coefficient on immigration status slightly reduces in magnitude but it gains in precision, resulting in a slightly higher p-value.

Note finally, that the dummy "someone else looking at the screen during the self completion part" has no effect neither in binary nor count data models for property crime, regardless whether we use it alone, or when we use two dummies, "other present but did not look" and "other present but looked". However, both dummies have an equal, statistically significant (at 5%) impact on violent crime.

Thus, according to the analysis of this subsection, the results from both binary and count data models are not driven by the exclusion restriction of "truthfulness".

To sum up, all the results of this section strengthen the relationship found in the main results of subsection 2.7.2.2. That is, immigrants' involvement in criminal activities is lower than natives' one. More robustness checks presented in Appendix C also agree with this

[1] Actually, there are cases where the model identifies as global maximum the "wrong" maximum, where the parameters of the reporting process have the opposite from the expected sign. However, from the author's experience, this happens only in cases where the exclusion is very "weak".

finding. Thus, although the main results indicate that this relationship is statistically insignificant, this section (and the results of Appendix C) suggest that it is very robust.

2.9 Decomposition of Immigrants by Ethnicity and Regions

Throughout the empirical analysis, a negative relationship between property crime and being an immigrant has been observed, other things being equal. However, we have treated immigrants as a homogeneous group of people which is not realistic. Therefore, in this subsection I decompose immigrants by ethnic groups and by region (by using interaction terms), to investigate whether different groups of immigrants are different with regard to their criminal behaviour. These results are presented in Table 2.18 where the covariate-dependent MisProbit is used in all cases. Although this table presents only the estimates of the crime process, the interaction terms are also inserted in the reporting process. Thus, as in all models so far, there is only one exclusion restriction with the form of including "truthfulness" only in the reporting part. Note that all the coefficients of the interaction terms in the reporting process are statistically insignificant. All results are available from the author upon request. Finally, I would like to stress that this subsection is used to illustrate the results of the aforementioned decomposition, letting discussion be a part of the next section.

2.9.1 Interaction between Immigration and Ethnicity

It is a fact that immigrant population in England and Wales is very heterogeneous, as far as the ethnicity is concerned. For example, there are black immigrants coming from Caribbean or African countries, Asians from both the south and the east parts of Asia, and white population from both Europe and the "old" Commonwealth of Nations, such as Australia or Canada. Naturally, immigrants from different countries of origin have grown up with different principles, in different socioeconomic conditions, so that they differ a great deal in many aspects, both between each other and with respect to the native population. Thus, their criminal behaviour may differ as well. Moreover, following the same reasoning, we might also expect that foreigners who belong to an ethnic group, for instance Asians, will exhibit different behaviour than natives of the same ethnic group, as the latter are better adapted in the British lifestyle. In this subsection I intend to investigate the above concepts by decomposing immigrants in four groups, which are, 'White immigrants', 'Black immigrants', 'Asian & Other immigrants', and 'Mixed immigrants'.[1]

First of all, comparing each group with the whole native population (regardless the ethnicity of natives), we find that the probability to commit a crime is considerably smaller for black immigrants (significant at 1%).[2] Moreover, 'Asians & Other' immigrants also commit less crime than natives, but it is significant only at 20% significance level. Finally, the coefficients of the other two groups are also negative but very insignificant. From the above, it seems that there are differences in the criminal behaviour among the immigrant groups. However, the only statistically significant difference is between black immigrants and white immigrants (at 10%).

Next, we investigate whether there are differences in criminal activity between each group of immigrants and their native counterparts. For this purpose, three interaction terms are used, and the results are presented in the 1^{st} specification of Table 2.18.a. From this table it seems that black immigrants commit less crime than black natives. Moreover, according to these results there is no difference in crime between the other three immigrant groups and their native counterparts. Although the interaction term "black & immigrant" is statistical insignificant, redefining the dummies by disaggregating the population in eight groups (see, Table 2.18.b) we find that black immigrants commit significantly (at 5% significance level) less crime than black natives. We also find that this is the least crime-prone group. They commit significantly less crime than all other groups but 'Asians & Others' and 'Mixed' immigrants. This is quite interesting since the involvement of black natives in criminal activities is not different than the involvement of all other groups. We can conclude that, due to some unobservable characteristics, black immigrants are less crime-prone than black,

[1] It would be better if there was a disaggregation of immigrant population in more groups, since, for example, black immigrants from Caribbean would be different from black immigrants from Africa. The data set actually includes a derived variable that separates immigrants in 15 groups. However, the use of this variable would be impossible, because there is not enough variation between these groups and the dependent variable to identify the parameters of interest.

[2] These results are also not presented but they are available upon request.

white, and mixed natives.

Finally, note that interaction terms between immigration status and ethnic groups for violent crime show that the effect of being an immigrant on violent crime does not differ among the different ethnicities.

2.9.2 Interaction between Immigration and Region

As mentioned in the introduction, location of immigrants is not randomly assigned. Different locations may attract different types of immigrants, or, immigrants located in different places may face different conditions, which in turn may affect their criminal activity. In this subsection interaction terms between regional dummies and immigration status are used. The results are shown in the 2^{nd} specification of Table 2.18.a.

From this table there are two things that merit some discussion. Firstly, we can notice that immigrants located in London are much less involved in criminal activities than natives located in London (since this is what the coefficient of the dummy "immigrant" now captures). This difference is significant at 1% level of significance. Also, we notice that immigrants who live in South England commit considerably more crime than immigrants of London. This relationship is much clearer in Table 2.18.c, where we redefine the population in eight groups according to the immigration status and the region of residence. It is clear that immigrants of London are the least crime-prone group. Apart from immigrants located in North and Midlands, they commit considerably less crime than all other groups. On the other hand, it is also interesting that immigrant population located in South is the most crime-prone category. However, they do not commit significantly more crime than the native population of Southern regions. Finally, we find that although immigrants from Midlands and North commit less crime than their native counterparts, these differences are statistically insignificant.

Finally, as it is the case for ethnic background, interaction terms between immigration status and regional dummies are very insignificant in the case of violent crime.

2.10 Discussion

In Sections 2.7 and 2.8 I presented and evaluated the results of different models that control for under-reporting. Particularly, the results of the covariate-dependent MisProbit showed that if immigrants exhibited the same basic demographic characteristics with natives, there would be a negative association between actual criminal behaviour and immigration status. Even though the estimated difference is statistically insignificant, all the results in the sensitivity analysis section (and in Appendix C) suggest that it is actually quite robust. Most importantly, the results of the unweighted MisProbit signified that if we were able to obtain a larger sample, the estimated negative association would be much more precise. Therefore, altogether the robustness of the association indicates that this relationship probably exists, but the nature of the models and data do not allow precise estimation.

In Section 2.2 some channels through which there can be a positive or a negative relationship between property crime and immigration were discussed. However, it was concluded that even if these channels are "active", the final outcome is uncertain as they operate in opposite directions. How can the estimated difference can be explained by the theoretical framework? A possible story is the following. It is a fact that immigrants are located in more deprived areas and confront blocked opportunities, perhaps because of human capital limitations (for instance, in terms of language efficiency), because employers tend to prefer natives, or due to other reasons (see, Algan at al, 2010). There are also, to some extent, cultural conflicts, and difficulties of adjustment. However, at the same time immigrants may be more risk averse and discount future less heavily. They might also be more responsive to the deterrent effects of potential punishment (Bucher and Piehl, 2007). In addition, not only do immigrants face a higher probability of apprehension, but they are also confronted with the threat of deportation. Finally, coming from poorer countries, they are satisfied even with much lower economic outcomes than natives. Therefore, if we accept that some of the factors associated with more crime actually exist, we must also accept that the factors associated with lower crime work in the opposite direction over-balancing the situation. Therefore, if immigrants did not encounter the problems associated with more crime, they would be even less prone to crime compared to natives.

The use of interaction terms have provided some interesting insights. Although as a whole immigrants are insignificantly less involved in criminal activities, immigrants located in London are considerably less likely to participate in illegal activities than natives of London (but also natives of all other regions). It might be that immigrants integrate in London more easily than in other locations. Furthermore, concentration of immigrants in specific areas might create strong social controls that discourage criminal activities. In addition, as immigrants are more responsive to deterrent factors, strict policing in London would discourage criminal activities of immigrants by more than natives. The sure thing is that there are many unobserved cultural differences between immigrants and natives with respect to criminal behaviour. Finally, it could be that immigrants with different criminal propensities are located in areas other than London by central agencies, such as the National Asylum Support Service. For example, asylum seekers, which is the group that according to their economic outcomes would find illegal sectors the most attractive, were located in unpopular areas outside London (see, Bell, Machin and Fasani, 2010).

But why are immigrants located in South more crime-prone than most of the other groups? This may indicate that immigrants in these areas encounter problems of adaptation in the English society, or that the socioeconomic conditions they face are less favorable than those of other regions. They may also present adverse behaviour due to accumulative pressure, for example, because of discrimination, racism by natives, or cultural conflicts. Additionally, it might be the case that South England pulls the most crime-prone groups of immigrants, perhaps because there are criminal opportunities that suit them better than other groups of immigrants. It must be stressed though, that in spite of the fact that immigrants in South are more crime-prone than immigrants in London, their involvement in crime is not statistically different from the involvement of natives in South.

Finally, we have found evidence that the group of black immigrants is significantly less involved in criminal activities than both black natives and white natives. This is very interesting if we consider that black immigrants, particular those emigrating from Africa, exhibit the most unfavorable socioeconomic conditions (see, for example, Algan at al, 2010, and Dustmann and Theodoropoulos, 2010). Therefore, unobserved cultural and deterrent factors may have a stronger effect for this group.

2.11 Conclusion

This study investigated the individual relationship between immigration and property crime in England and Wales. Although there is a public sentiment that immigrants are more involved in criminal activities, both the theoretical and the empirical results of this paper lead to different conclusions.

A simple economic model of crime that incorporates immigration has been developed in Section 2.2. Both this model and other theories developed by sociologists and criminologists illustrated that, even though there are reasons to believe that immigration can be associated with crime, the sign of this association is not clear. Therefore, in order to investigate the empirical relationship between immigration and property crime, the Offending, Crime, and Justice Survey of 2003 was employed, a representative national survey of self-reported crime.

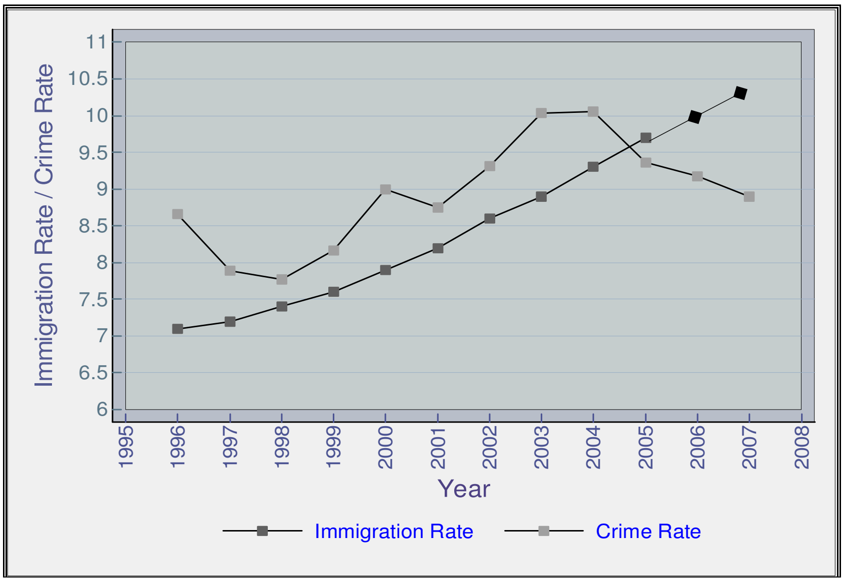
Models that account for under-reporting were developed and used, as this is the major concern in crime self-reports. First of all, the empirical analysis showed that under-reporting exists. Moreover, we showed that under-reporting is not constant, but it rather depends on respondents' characteristics. However, if anything, immigrants tend to under-report by less than natives. Nevertheless, it was explained that the coefficients of the reporting process of the covariate-dependent MisProbit model must be treated with caution, since the reporting process can be also interpreted in a zero-inflation framework. The models indicated that reporting zero crimes because of total under-reporting or because of zero-inflation, conditional on the set of covariates, is around 70%.

Concerning the crime process, the results of the covariate-dependent MisProbit model suggested that, taking into account under-reporting or zero-inflation, the predicted probability to commit a property crime is about 29%, much higher than what the conventional Probit model predicts (about 6%). Most importantly, according to the findings of the covariate-dependent MisProbit, there is a negative but not statistically significant association between immigration and crime, which is, however, significant at 10% significance level if we do not control for ethnicity.

It is important to also stress that all robustness checks reinforce the above relationship and suggest that although insignificant, this relationship exists. For example, the estimated difference is much more precise (significant at 5%) if we do not use sampling weights that downweigh the immigrant population. Moreover, exploiting the extra information of the count form of the property crime variable, and using a NB2-Logit framework, we get to the same conclusion. Natives commit more crimes than immigrants, but this difference is statistically insignificant. The effects of the other covariates on crime are also robust across the binary and the count data models. Moreover, our findings also suggest that immigrants are slightly less involved in violent criminal activities as well. Finally, it is quite important that the results of the models used in empirical analysis are not driven by the assumption that "truthfulness" affects only the reporting process.

Finally, the use of interaction terms offered some interesting insights. Immigrants located in London are considerably less involved in property crime activities than natives. Contrary to that, immigrants in South are more crime-prone than immigrants in London, but not more crime-prone than natives in South (although South immigrants' effect on the probability to commit a crime is higher than all other groups but statistically insignificant). Thus, it might be that either different socio-economic conditions that immigrants encounter in different locations and their interactions with the native population may affect their criminal behaviour, or that different areas attract different types of immigrants. Finally, the decomposition of immigrants by ethnic group showed that black immigrants display a considerably lower probabilities of committing a property crime than black natives and white natives, despite the fact that they are the least favored group with regard to their economic outcomes. However, notice that the analysis of interaction terms is limited by the small sample size of each separate group and the low variation in the dependent variables. Further investigation is required to establish whether the effect of being an immigrant on criminal behaviour differs with respect to immigrants' demographic characteristics.

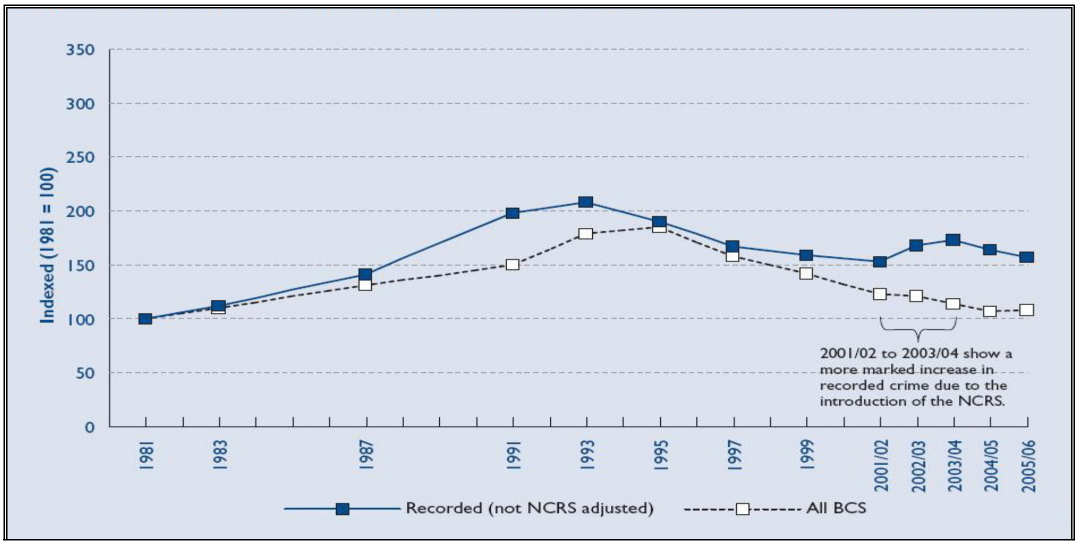
Figure 2.1. Immigration Rates and Crime Rates through time[∇]



The Immigration rate statistics are provided by the OECD Stat. Extracts

The Recorded Crime rate statistics are constructed using data from the Home Office – Research Development Statistics.

Figure 2.2. Crime Indexes through time: Recorded crime *Vs* BCS[◊]



[∇] This graph is constructed without adjustments for the change in recording of crime method happened in April 1998 and the introduction of the National Crime Recording Standard (NCRS) across England and Wales in April 2002. Both changes had the effect of increasing the number of crimes recorded by the police and thus, numbers of recorded crimes are not comparable with previous years. Therefore, the positive tendency for recorded crime between 1998 and 2003 can be considered as a result of these changes, and not as a true increase in crime rates. In figure 2, where the crime data are adjusted for the change in 1998 but not for the introduction of NCRS, it is clear that there is a negative trend up to 2002. The British Crime Survey also coincides with this negative growth rate for crime. It needs to be stressed that criminologists consider the BCS as more reliable than recorded by police crime, since many crimes are not reported to the police, and some reported crimes are not recorded.

[◊] This figure is taken by the independent review of Smith (2006), carried out for the Home Office, page 2.

Figure 2.3. MisProbit: Interpretation as Misclassification

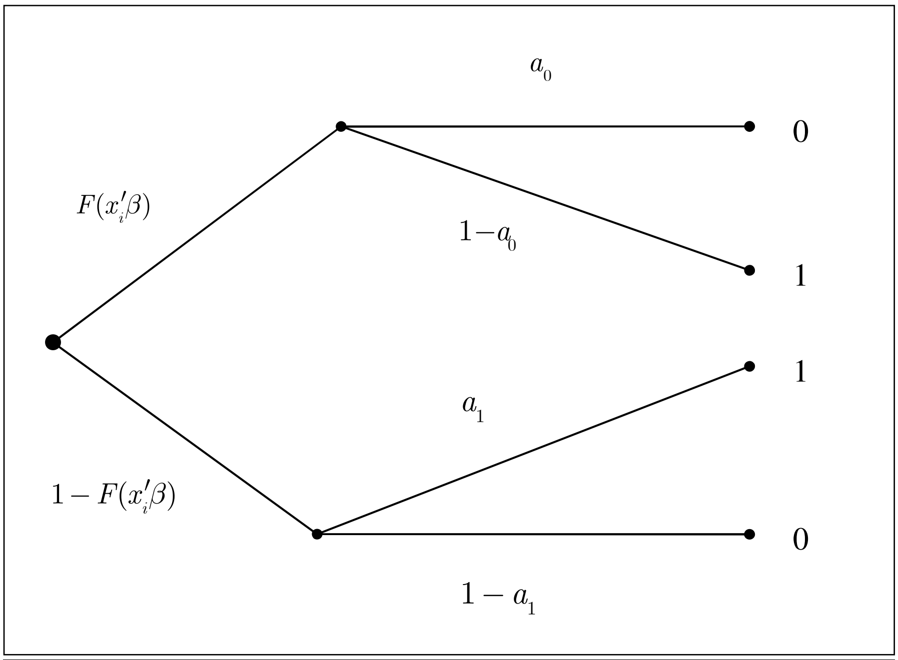


Figure 2.4. MisProbit: Interpretation as Zero-One-Inflation

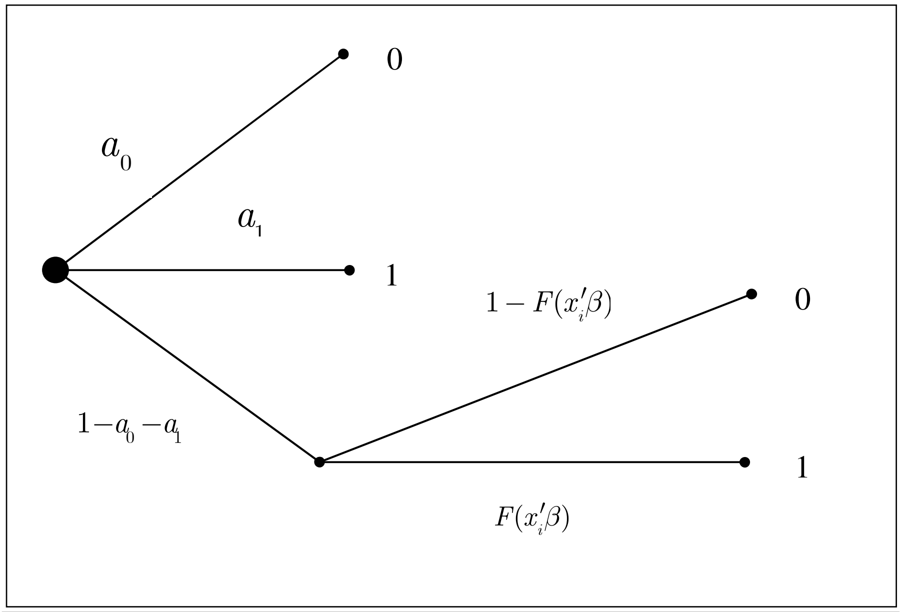


Table 2.1 ESS 2002

Immigrants make country's crime problems worse or better	%
Crime problems made worse	8.3
1	5.8
2	13.5
3	16.5
4	16.9
5	30.8
6	4.0
7	2.2
8	1.3
9	0.3
Crime problems made better	0.3
Total	100

Table 2.2. ISSP 1995(n=996) / 2003(n=834)

Immigrants increase crime rates	1995	2003
Agree strongly	7.8	13.6
Agree	18.2	26.3
Neither agree nor disagree	34.9	32.6
Disagree	31.7	24.5
Disagree strongly	7.3	3.1
Total	100	100

Source: ESS(2002), ISSP(2003)

Table 2.3. Ordered Probit. Determinants of Natives' Attitudes

Immigrants Increase Crime Rates	Coefficients	Robust Standard Errors
ISS 2003	0.372***	*(0.054)*
Male	0.267***	*(0.055)*
Age	0.008***	*(0.002)*
Income	-0.013**	*(0.006)*
Center	0.049	*(0.076)*
Right	0.368***	*(0.067)*
No party	0.225***	*(0.079)*
Low Education	0.599***	*(0.073)*
Middle Education	0.390***	*(0.066)*
N	1,635	
Log-Likelihood	-2,247.69	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

Table 2.4. Tabulation of OCJS Respondents by Sample Type

Sample Type	Total		Immigrants		Natives	
	N	%	N	%	N	%
Core 10-65	6,771	58.08	729	36.25	6,042	62.63
Boost 10-25	3,098	26.57	186	9.25	2,912	30.19
Ethnic Boost	1,789	15.35	1096	54.50	693	7.18
Total	11,658	100.00	2,011	100.00	9.647	100.00

Table 2.5. Tabulation of Number of Property Crimes

	Frequency	Percent	Cum.
0	10,927	94.17	94.17
1	251	2.16	96.33
2	123	1.06	97.39
3	67	0.58	97.97
4	40	0.34	98.31
5	48	0.41	98.72
6	29	0.25	98.97
7	6	0.05	99.03
8	11	0.09	99.12
9	5	0.04	99.16
10	12	0.1	99.27
11	14	0.12	99.39
12	9	0.08	99.47
13	4	0.03	99.5
14	1	0.01	99.51
15	3	0.03	99.53
16	1	0.01	99.54
17	1	0.01	99.55
18	1	0.01	99.56
19	4	0.03	99.59
20	8	0.07	99.66
21	1	0.01	99.67
22	3	0.03	99.7
23	3	0.03	99.72
24	1	0.01	99.73
25	2	0.02	99.75
27	1	0.01	99.76
28	1	0.01	99.77
30	4	0.03	99.8
33	1	0.01	99.81
34	1	0.01	99.82
35	4	0.03	99.85
36	1	0.01	99.86
40	2	0.02	99.88
41	1	0.01	99.89
50	1	0.01	99.9
54	1	0.01	99.91
55	1	0.01	99.91
56	1	0.01	99.92
57	1	0.01	99.93
60	1	0.01	99.94
73	1	0.01	99.95
100	2	0.02	99.97
113	1	0.01	99.97
168	1	0.01	99.98
194	1	0.01	99.99
225	1	0.01	100
Total	11,604	100	

Table 2.6. Descriptive Statistics

Variables	N			Mean	Weighted Mean			Min	Max
	All	Nat.	Imm.		All	Nat.	Imm.		
Crime Variables									
Any Property Crime last year	11,658	9,647	2,011	0.063	0.055	0.057	0.039	0	1
Any Violent Crime last year	11,667	9,641	2,026	0.072	0.054	0.056	0.036	0	1
Number of Property Crime last year	11,604	9,598	2,006	0.371	0.342 *(5.858)*[◊]	0.366 *(6.011)*	0.160 *(2.218)*	0	225[∅]
Independent Variables									
Immigrant	2,069			0.174	0.119[∇]			0	1
Native	9,853			0.826	0.881				
Age	11,922	9,853	2,069	32.549	36.738 *(17.473)*	36.554 *(17.146)*	38.098 *(19.017)*	10	66
Male	5,755	4,748	1,007	0.483	0.497	0.496	0.505	0	1
Female	6,167	5,105	1,062	0.517	0.503	0.504	0.495		
White	9,284	8,702	582	0.779	0.909	0.956	0.553	0	1
Black	743	291	452	0.062	0.023	0.010	0.120	0	1
Asian	1,116	496	620	0.094	0.045	0.022	0.214	0	1
Other	350	91	259	0.029	0.012	0.003	0.073	0	1
Mixed	429	273	156	0.036	0.012	0.008	0.039	0	1
North	3,249	2,898	351	0.273	0.274	0.288	0.175.	0	1
Midlands	2,822	2,480	342	0.237	0.235	0.246	0.150	0	1
South	3,856	3,352	504	0.323	0.351	0.358	0.298	0	1
London	1,992	1,122	870	0.167	0.139	0.107	0.376	0	1
Truthfulness	11,118	9,271	1,847	0.933	0.942	0.946	0.915	0	1
Other Present	3,768	3,171	597	0.327	0.285	0.288	0.263	0	1

[◊] Weighted standard deviations in paretheses.

[∅] The max for immigrants is 60 property crimes, while the max for natives is 225 property crimes.

[∇] Notice that the weighted mean for the core sample only is 0.091 which is very close to the percentage of immigrants in the UK from other sources. The weighted mean presented here is calculated from the combining sample (core & youth boost & ethnic minorities boost). Although the weights are used to restore representativeness of the sample, these weights are designed to restore representativeness with respect to age and race composition (and also with respect to non respondents). Therefore, it is not surprising to notice a 2.8 percentage points difference.

Table 2.7. Probit Estimates for all Crime Categories

Any … in last year	Coefficient	Robust St.Error	Log - Likelihood	N
Property Offence	-0.184**	*(0.089)*	-2,467.26	11,658
Violent Offence	-0.209***	*(0.082)*	-2,440.57	11,667
Drugs related Offence	-0.091	*(0.144)*	-680.19	11,866
Vehicle Theft	-0.137	*(0.255)*	-358.43	11,873
Criminal Damage	-0.468***	*(0.134)*	-693.62	11,858
Burglary	-0.485**	*(0.210)*	-131.93	11,870
Robbery	-0.231	*(0.277)*	-31.87	11,897
Other Theft	-0.149	*(0.091)*	-2188.28	11,713
Assault	-0.210***	*(0.082)*	-2438.79	11,676

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

Table 2.8. Negative Binomial Estimates for all Crime Categories

Number of ….. in last year	Coefficient	Robust St.Error	Alpha	Log - Likelihood	N
Property Offences	-0.825**	*(0.351)*	57.46***	-2,400.98	11,604
Violent Offences	-1.062***	*(0.236)*	50.02***	-2,397.71	11,640
Drugs Offences	-0.144	*(0.645)*	406.34***	-675.27	11,862
Vehicle Thefts	-1.792**	*(0.722)*	542.04***	-289.88	11,869
Criminal Damages	-2.451***	*(0.494)*	182.86***	-571.78	11,856
Burglaries	-3.035***	*(0.932)*	2,123.43***	-115.21	11,869
Robberies	-2.373**	*(1.105)*	7,695.16***	-25.65	11,897
Other Thefts	-0.664*	*(0.366)*	64.41***	-2,157.15	11,695
Assaults	-1.060***	*(0.237)*	50.05***	-2,393.85	11,649

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.9. Probit Estimates

Probit	(1)		(2)		(3)	
	Coefficient	Robust S.E	Coefficient	Robust S.E	Coefficient	Robust S.E
Probability of Committing a Property Offence in Last Year						
Constant	-1.204***	*(0.127)*	-1.454***	*(0.143)*	-1.134***	*(0.129)*
Immigrant	-0.127	*(0.102)*	-0.025	*(0.111)*	-0.014	*(0.109)*
Age	-.0178**	*(0.007)*	-0.018***	*(0.007)*	-0.018**	*(0.007)*
Age^2	0.0001	*(0.0001)*	0.0001	*(0.0001)*	0.0001	*(0.0001)*
Male	0.362***	*(0.049)*	0.362***	*(0.049)*	0.364***	*(0.049)*
White			0.331***	*(0.077)*		
Black					-0.201*	*(0.119)*
Asian & Other					-0.501***	*(0.101)*
Mixed					-0.016	*(0.115)*
Region South	0.089	*(0.082)*	0.028	*(0.082)*	0.034	*(0.083)*
Region Midlands	0.061	*(0.084)*	0.007	*(0.085)*	0.012	*(0.086)*
Region North	0.066	*(0.086)*	0.009	*(0.085)*	0.015	*(0.086)*
Sample Size	11,658		11,658		11,658	
Log Likelihood	-1,452.88		-1,447.94		-1,445.82	
Predicted.Prob.Crime	0.064		0.061		0.062	

Table 2.10. Negative Binomial 2 Estimates

NegBin2	(1)		(2)		(3)	
	Coefficient	R.S.E	Coefficient	R.S.E	Coefficient	R.S.E
Expected number of Property Offences in Last Year						
Constant	-1.299***	*(0.505)*	-1.897***	*(0.529)*	-1.065***	*(0.484)*
Immigrant	-0.441	*(0.356)*	-0.309	*(0.352)*	-0.232	*(0.353)*
Age	0.001	*(0.031)*	0.002	*(0.030)*	0.003	*(0.030)*
Age^2	-0.001*	*(0.0004)*	-0.001*	*(0.0004)*	-0.001*	*(0.0004)*
Male	0.669**	*(0.278)*	0.672**	*(0.275)*	0.663**	*(0.276)*
White			0.788***	*(0.268)*		
Black					-0.736**	*(0.328)*
Asian & Other					-1.337***	*(0.366)*
Mixed					0.112	*(0.435)*
Region South	0.504*	*(0.284)*	0.305	*(0.279)*	0.244	*(0.283)*
Region Midlands	0.593**	*(0.272)*	0.441	*(0.269)*	0.370	*(0.274)*
Region North	1.256**	*(0.501)*	1.065**	*(0.507)*	1.008**	*(0.503)*
Sample Size	11,604		11,604		11,604	
Log Likelihood	-2,346.791		-2,344.4531		-2,342.7878	
Alpha	47.06***		46.66***		46.33***	
Predicted.Num.Crimes	0.407		0.388		0.388	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.11. MisProbit Estimates for Property Crime: *Constant Misclassification*

Mis.Probit	(1)		(2)		(3)	
	Coefficient	Robust S.E	Coefficient	Robust S.E	Coefficient	Robust S.E
Probability of Committing a Property Offence in Last Year						
Constant	0.395	*(0.954)*	-0.226	*(0.701)*	0.678	*(1.016)*
Immigrant	-0.217	*(0.278)*	0.055	*(0.334)*	0.091	*(0.379)*
Age	-0.056	*(0.045)*	-0.060	*(0.046)*	-0.064	*(0.051)*
Age^2	0.000	*(0.000)*	0.000	*(0.000)*	0.000	*(0.000)*
Male	0.914^{**}	*(0.379)*	0.926^{***}	*(0.379)*	0.997^{**}	*(0.443)*
White			0.826^{**}	*(0.405)*		
Black					-0.511	*(0.362)*
Asian and Other					-1.341^{*}	*(0.714)*
Mixed					-0.064	*(0.344)*
Region South	0.291	*(0.283)*	0.147	*(0.250)*	0.199	*(0.297)*
Region Midlands	0.156	*(0.241)*	0.024	*(0.225)*	0.063	*(0.257)*
Region North	0.230	*(0.264)*	0.109	*(0.250)*	0.164	*(0.297)*
Prob of Misclassification of One as Zero (Under-reporting or Zero-Inflation)						
Constant	0.813^{***}	*(0.070)*	0.811^{***}	*(0.066)*	0.819^{***}	*(0.060)*
Prob of Misclassification of Zero as One (Over-reporting)						
Constant	0.013^{***}	*(0.005)*	0.012^{***}	*(0.005)*	0.013^{***}	*(0.005)*
Sample Size	11,658		11,658		11,658	
Log Likelihood	-1,452.05		-1,446.92		-1,444.77	
Predicted.Prob.Crime	0.295		0.280		0.295	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.12. MisProbit Property Crime: *Covariate-Dependent Misclassification of 1 as 0*

Mis.Probit	(1)		(2)		(3)	
	Coefficient	R.S.E	Coefficient	R.S.E	Coefficient	R.S.E
Probability of Committing a Property Offence in Last Year						
Constant	2.597***	*(0.895)*	2.335***	*(0.896)*	2.833***	*(0.926)*
Immigrant	-0.431*	*(0.251)*	-0.254	*(0.273)*	-0.277	*(0.275)*
Age	-0.248***	*(0.045)*	-0.259***	*(0.046)*	-0.255***	*(0.047)*
Age^2	0.003***	*(0.001)*	0.003***	*(0.001)*	0.003***	*(0.001)*
Male	0.439***	*(0.156)*	0.440***	*(0.156)*	0.429***	*(0.156)*
White			0.542*	*(0.293)*		
Black					-0.559*	*(0.323)*
Asian and Other					-0.638	*(0.430)*
Mixed					0.004	*(0.391)*
Region South	0.300	*(0.219)*	0.273	*(0.231)*	0.272	*(0.227)*
Region Midlands	0.151	*(0.209)*	0.124	*(0.228)*	0.124	*(0.223)*
Region North	0.501*	*(0.282)*	0.496	*(0.318)*	0.498	*(0.317)*
Prob of Misclassification of One as Zero (Under-reporting or Zero-Inflation)						
Constant	2.196***	*(0.522)*	2.267***	*(0.507)*	2.072***	*(0.516)*
Immigrant	-0.391	*(0.371)*	-0.410	*(0.346)*	-0.437	*(0.359)*
Age	-0.205**	*(0.081)*	-0.195***	*(0.074)*	-0.193***	*(0.074)*
Age^2	0.003**	*(0.001)*	0.003***	*(0.001)*	0.003***	*(0.001)*
Male	-0.260*	*(0.144)*	-0.277*	*(0.142)*	-0.285***	*(0.142)*
White			-0.198	*(0.224)*		
Black					-0.264	*(0.447)*
Asian and Other					0.467	*(0.327)*
Mixed					0.104	*(0.281)*
Region South	0.201	*(0.228)*	0.262	*(0.224)*	0.252	*(0.221)*
Region Midlands	0.151	*(0.215)*	0.224	*(0.215)*	0.211	*(0.212)*
Region North	0.432**	*(0.211)*	0.490**	*(0.212)*	0.480**	*(0.207)*
Truthfulness	0.876***	*(0.319)*	0.868***	*(0.256)*	0.854***	*(0.255)*
Prob of Misclassification of Zero as One (Over-reporting) +						
Constant	-2.244***	*(0.292)*	-2.234***	*(0.204)*	-2.256***	*(0.210)*
Sample Size	11,658		11,658		11,658	
Log Likelihood	-1,427.97		-1,422.05		-1,419.99	
Predicted.Prob.Crime	0.285		0.292		0.295	
Predicted.Prob.Under	0.681		0.709		0.709	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

+ Which corresponds to probability of misclassification of zero as one of $\Phi(-2.234)=0.0127$

Table 2.13. Negative Binomial 2 Models

	NB2		NB2-Logit		ZI-NB2-Logit	
	Coefficient	R.S.E	Coefficient	R.S.E	Coefficient	R.S.E
Expected number of Property Offences in Last Year						
Constant	-1.897***	*(0.529)*	8.633**	*(3.671)*	10.950**	*(5.288)*
Immigrant	-0.309	*(0.352)*	-0.617	*(0.636)*	-0.757	*(0.721)*
Age	0.002	*(0.030)*	-0.677***	*(0.224)*	- 0.711**	*(0.308)*
Age^2	-0.001*	*(0.000)*	0.009***	*(0.003)*	0.010**	*(0.005)*
Male	0.672**	*(0.275)*	1.457***	*(0.443)*	0.657	*(0.498)*
White	0.788***	*(0.268)*	0.196	*(0.611)*	-0.939	*(0.706)*
Region South	0.305	*(0.279)*	1.024	*(0.696)*	1.504**	*(0.694)*
Region Midlands	0.441	*(0.269)*	0.247	*(0.561)*	0.501	*(0.682)*
Region North	1.065 **	*(0.507)*	1.676**	*(0.662)*	2.733***	*(0.949)*
Probability of Reporting a Committed Crime						
Constant			-12.253***	*(3.510)*	-13.285***	*(4.520)*
Immigrant			0.451	*(1.043)*	0.130	*(0.869)*
Age			1.008***	*(0.206)*	1.058***	*(0.230)*
Age^2			-0.015***	*(0.003)*	-0.015***	*(0.004)*
Male			-1.333*	*(0.687)*	-0.406	*(0.625)*
White			0.638	*(1.062)*	1.053	*(0.994)*
Region South			-1.508	*(0.937)*	-1.890**	*(0.771)*
Region Midlands			0.255	*(0.954)*	-0.480	*(0.878)*
Region North			-1.628	*(1.027)*	-2.800***	*(1.010)*
Truthfulness			-1.237***	*(0.475)*	-1.963***	*(0.468)*
Probability of Zero-Inflation						
Constant					-1.212	*(1.133)*
Immigrant					-0.533	*(0.549)*
Age					0.138**	*(0.060)*
Age^2					-0.001	*(0.001)*
Male					-0.960***	*(0.259)*
White					-1.634***	*(0.387)*
Region South					0.490	*(0.480)*
Region Midlands					0.136	*(0.561)*
Region North					0.820*	*(0.479)*
Sample Size	11,604		11,604		11,604	
Log Likelihood	-2,344.45		-2,313.99		-2,258.49	
alpha	46.66***		41.64***		15.474***	
Pred.Pr. of Reporting			0.430		0.376	
Pred.Pr. of ZI					0.617	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.14. Weighted Property Crime *versus* Weighted Violent Crime *versus* Unweighted Property Crime

Mis.Probit	Weighted Property Crime		Weighted Violent Crime		Unweighted Property Crime	
	Coefficient	Robust S.E	Coefficient	Robust S.E	Coefficient	Robust S.E
Probability of Committing an Offence in Last Year						
Constant	2.335***	*(0.896)*	2.778**	*(0.925)*	2.576***	*(0.569)*
Immigrant	-0.254	*(0.273)*	-0.241	*(0.341)*	-0.493**	*(0.210)*
Age	-0.259***	*(0.046)*	-0.315***	*(0.056)*	-0.270***	*(0.035)*
Age^2	0.003***	*(0.001)*	0.004***	*(0.001)*	0.004***	*(0.001)*
Male	0.440***	*(0.156)*	0.428***	*(0.102)*	0.464***	*(0.125)*
White	0.542*	*(0.293)*	0.447***	*(0.172)*	0.487**	*(0.189)*
Region South	0.273	*(0.231)*	-0.063	*(0.241)*	0.050	*(0.162)*
Region Midlands	0.124	*(0.228)*	-0.085	*(0.242)*	-0.032	*(0.177)*
Region North	0.496	*(0.318)*	-0.140	*(0.246)*	0.103	*(0.201)*
Prob of Misclassification of One as Zero (Under-reporting or Zero-Inflation)						
Constant	2.267***	*(0.507)*	6.174***	*(1.250)*	2.509***	*(0.526)*
Immigrant	-0.410	*(0.346)*	-0.553	*(0.416)*	-0.209	*(0.262)*
Age	-0.195***	*(0.074)*	-0.646***	*(0.154)*	-0.261***	*(0.066)*
Age^2	0.003***	*(0.001)*	0.012***	*(0.003)*	0.004***	*(0.001)*
Male	-0.277*	*(0.142)*	-0.229	*(0.149)*	-0.179	*(0.130)*
White	-0.198	*(0.224)*	-0.057	*(0.264)*	-0.074	*(0.211)*
Region South	0.262	*(0.224)*	-0.237	*(0.310)*	0.151	*(0.178)*
Region Midlands	0.224	*(0.215)*	-0.063	*(0.309)*	0.222	*(0.184)*
Region North	0.490**	*(0.212)*	-0.256	*(0.316)*	0.299	*(0.192)*
Truthfulness	0.868***	*(0.256)*	0.876***	*(0.253)*	1.181***	*(0.197)*
Prob of Misclassification of Zero as One (Over-reporting)						
Constant	-2.234***	*(0.204)*	-2.081***	*(0.088)*	-2.175***	*(0.121)*
Sample Size	11,658		11,667		11,658	
Log Likelihood	-1,422.05		-1,303.74		-2,441.32	
Predicted.Prob.Crime	0.292		0.223		0.260	
Predicted.Prob.Under	0.709		0.514		0.650	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.15. Truthfulness *versus* No Exclusion *versus* Other Present, for Property Crime

Mis.Probit	(1) Property Crime (Truthfulness)		(2) Property Crime (No Exclusion)		(3) Property Crime (Other Present)	
	Coefficient	Rob. S.E	Coefficient	Rob. S.E	Coefficient	Rob. S.E
Probability of Committing an Offence in Last Year						
Constant	2.335***	*(0.896)*	2.298	*(1.558)*	0.985	*(5.056)*
Immigrant	-0.254	*(0.273)*	-0.232	*(0.276)*	-0.143	*(0.528)*
Age	-0.259***	*(0.046)*	-0.242***	*(0.083)*	-0.170	*(0.285)*
Age^2	0.003***	*(0.001)*	0.003***	*(0.001)*	0.002	*(0.004)*
Male	0.440***	*(0.156)*	0.401***	*(0.149)*	0.382***	*(0.122)*
White	0.542*	*(0.293)*	0.369	*(0.297)*	0.369*	*(0.219)*
Region South	0.273	*(0.231)*	0.175	*(0.247)*	0.085	*(0.456)*
Region Midlands	0.124	*(0.228)*	0.066	*(0.203)*	0.022	*(0.212)*
Region North	0.496	*(0.318)*	0.379	*(0.297)*	0.369	*(0.219)*
Prob of Misclassification of One as Zero (Under-reporting or Zero-Inflation)						
Constant	2.267***	*(0.507)*	3.070***	*(0.715)*	2.906**	*(1.179)*
Immigrant	-0.410	*(0.346)*	-0.343	*(0.305)*	-0.387	*(0.399)*
Age	-0.195***	*(0.074)*	-0.192*	*(0.107)*	-0.244	*(0.367)*
Age^2	0.003***	*(0.001)*	0.003**	*(0.001)*	0.004	*(0.005)*
Male	-0.277*	*(0.142)*	-0.239	*(0.169)*	-0.168	*(0.394)*
White	-0.198	*(0.224)*	-0.239	*(0.240)*	-0.103	*(0.760)*
Region South	0.262	*(0.224)*	0.200	*(0.194)*	0.188	*(0.558)*
Region Midlands	0.224	*(0.215)*	0.126	*(0.208)*	0.086	*(0.355)*
Region North	0.490**	*(0.212)*	0.438**	*(0.181)*	0.475*	*(0.266)*
Truthfulness	0.868***	*(0.256)*				
Other Present					0.294	*(0.505)*
Prob of Misclassification of Zero as One (Over-reporting)						
Constant	-2.234***	*(0.204)*	-2.552***	*(0.709)*	-2.941	*(2.019)*
Log Likelihood	-1,422.05		-1,432.94		-1,430.67	
Predicted.Prob.Crime	0.292		0.291		0.170	
Predicted.Prob.Under	0.709		0.684		0.475	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.16. Truthfulness *versus* No Exclusion *versus* Other Present, for Violent Crime

Mis.Probit	(1) Violent Crime (Truthfulness)		(2) Violent Crime (No Exclusion)		(3) Violent Crime (Other Present)	
	Coefficient	Rob. S.E	Coefficient	Rob. S.E	Coefficient	Rob. S.E
Probability of Committing an Offence in Last Year						
Constant	2.778**	*(0.925)*	2.470***	*(0.677)*	2.354***	*(0.645)*
Immigrant	-0.241	*(0.341)*	-0.177	*(0.271)*	-0.169	*(0.259)*
Age	-0.315***	*(0.056)*	-0.296***	*(0.044)*	-0.291***	*(0.040)*
Age^2	0.004***	*(0.001)*	0.004***	*(0.001)*	0.004***	*(0.001)*
Male	0.428***	*(0.102)*	0.410***	*(0.093)*	0.419***	*(0.092)*
White	0.447***	*(0.172)*	0.429***	*(0.158)*	0.447***	*(0.172)*
Region South	-0.063	*(0.241)*	-0.050	*(0.213)*	-0.042	*(0.212)*
Region Midlands	-0.085	*(0.242)*	-0.085	*(0.216)*	-0.074	*(0.210)*
Region North	-0.140	*(0.246)*	-0.128	*(0.158)*	-0.117	*(0.211)*
Prob of Misclassification of One as Zero (Under-reporting or Zero-Inflation)						
Constant	6.174***	*(1.250)*	7.666***	*(0.927)*	7.509***	*(0.995)*
Immigrant	-0.553	*(0.416)*	-0.502	*(0.423)*	-0.503	*(0.440)*
Age	-0.646***	*(0.154)*	-0.723***	*(0.110)*	-0.742***	*(0.135)*
Age^2	0.012***	*(0.003)*	0.014***	*(0.002)*	0.014***	*(0.004)*
Male	-0.229	*(0.149)*	-0.283*	*(0.150)*	-0.276*	*(0.157)*
White	-0.057	*(0.264)*	-0.060	*(0.266)*	-0.071	*(0.269)*
Region South	-0.237	*(0.310)*	-0.186	*(0.319)*	-0.165	*(0.337)*
Region Midlands	-0.063	*(0.309)*	-0.050	*(0.321)*	-0.020	*(0.343)*
Region North	-0.256	*(0.316)*	-0.259	*(0.327)*	-0.252	*(0.330)*
Truthfulness	0.876***	*(0.253)*				
Other Present					0.350***	*(0.160)*
Prob of Misclassification of Zero as One (Over-reporting)						
Constant	-2.081***	*(0.088)*	-2.108***	*(0.085)*	-2.095***	*(0.085)*
Log Likelihood	-1,303.74		-1,307.16		-1,305.09	
Predicted.Prob.Crime	0.223		0.207		0.207	
Predicted.Prob.Under	0.514		0.470		0.483	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.17. NegBin2-Logit – Truthfulness *versus* Other Present

NegBin2-Logit	(1) Truthfulness		(2) Other Present – A –		(3) Other Present – B –	
	Coefficient	R.S.E	Coefficient	R.S.E	Coefficient	R.S.E
Expected number of Property Offences in Last Year						
Constant	8.633**	*(3.671)*	7.572**	*(3.009)*	-5.849***	*(1.538)*
Immigrant	-0.617	*(0.636)*	-0.475	*(0.595)*	-0.306	*(0.640)*
Age	-0.677***	*(0.224)*	-0.619***	*(0.180)*	0.422***	*(0.145)*
Age^2	0.009***	*(0.003)*	0.009***	*(0.003)*	-0.007***	*(0.002)*
Male	1.457***	*(0.443)*	1.462***	*(0.394)*	0.125	*(0.477)*
White	0.196	*(0.611)*	0.175	*(0.662)*	0.869	*(0.611)*
Region South	1.024	*(0.696)*	0.860	*(0.601)*	-0.270**	*(0.507)*
Region Midlands	0.247	*(0.561)*	0.161	*(0.558)*	0.694	*(0.627)*
Region North	1.676**	*(0.662)*	1.588**	*(0.711)*	-0.201	*(0.575)*
Probability of Reporting a Committed Crime						
Constant	-12.253***	*(3.510)*	-11.470***	*(3.664)*	11.870***	*(2.799)*
Immigrant	0.451	*(1.043)*	0.189	*(0.972)*	-0.094	*(0.931)*
Age	1.008***	*(0.206)*	0.910***	*(0.208)*	-0.939***	*(0.182)*
Age^2	-0.015***	*(0.003)*	-0.014***	*(0.003)*	0.014***	*(0.003)*
Male	-1.333*	*(0.687)*	-1.325**	*(0.659)*	1.139	*(0.708)*
White	0.638	*(1.062)*	0.734	*(1.191)*	-0.525	*(0.974)*
Region South	-1.508	*(0.937)*	-1.024	*(0.902)*	0.992	*(0.771)*
Region Midlands	0.255	*(0.954)*	0.519	*(0.995)*	-0.416	*(0.878)*
Region North	-1.628	*(1.027)*	-1.373	*(1.164)*	1.903	*(0.845)*
Truthfulness	-1.237***	*(0.475)*				
Other Present			-0.677	*(0.533)*	-0.600*	*(0.349)*
Sample Size	11,604		11,604		11,604	
Log Likelihood	-2,313.99		-2,314.29		-2,314.61	
alpha	41.64***		41.94***		41.99***	
Pred.Pr. of Reporting	0.430		0.448		0.464	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.18.a Interaction Terms

Covariate Dependent MisProbit	(1) Ethnicity		(2) Regions	
	Coef	R.S.E	Coef	R.S.E
Constant	2.777***	*(0.898)*	1.997**	*(0.838)*
Immigrant	-0.196	*(0.287)*	-0.917***	*(0.352)*
Age	-0.252***	*(0.046)*	-0.248***	*(0.036)*
Age^2	0.003***	*(0.001)*	0.003***	*(0.001)*
Male	0.428***	*(0.158)*	0.521***	*(0.140)*
White			0.571	*(0.372)*
Black	0.013	*(0.367)*		
Asian and Other	-0.617	*(0.457)*		
Mixed	0.372	*(0.541)*		
Region South	0.275	*(0.237)*	0.009	*(0.217)*
Region Midlands	0.139	*(0.228)*	-0.051	*(0.226)*
Region North	0.489	*(0.316)*	0.301	*(0.289)*
Immigrant*Black	-0.934	*(0.583)*		
Immigr*Asian&Other	0.054	*(0.730)*		
Immigrant*Mixed	-0.615	*(0.919)*		
Immigrant*South			1.483***	*(0.570)*
Immigrant*Midlands			0.690	*(0.556)*
Immigrant*North			0.240	*(0.539)*
Sample Size	11,658		11,658	
Log Likelihood	-1,418.40		-1,413.96	
Predicted.Prob.Crime	0.297		0.243	

Table 2.18.b. Interaction Terms (Specification (1) Cont) [⊕]

Covariate Dependent MisProbit	(1) Ethnicity	
Immigrant*Black		
Immigrant*Asian&Other	0.359	*(0.709)*
Immigrant*Mixed	0.678	*(0.780)*
Immigrant*White	0.921*	*(0.475)*
Native*Black	1.131**	*(0.522)*
Native*Asian&Other	0.501	*(0.585)*
Native*Mixed	1.490**	*(0.669)*
Native*White	1.117***	*(0.404)*

Table 2.18.c. Interaction Terms (Specification (2) Cont)

Covariate Dependent MisProbit	(2) Regions	
Immigrant*London		
Immigrant*South	1.492***	*(0.484)*
Immigrant*Midlands	0.639	*(0.504)*
Immigrant*North	0.541	*(0.459)*
Native*London	0.917***	*(0.352)*
Native*South	0.927***	*(0.308)*
Native*Midlands	0.867***	*(0.320)*
Native*North	0.541***	*(0.459)*

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

[⊕] This model is exactly the same with the one presented in Table 7.7, apart from the way we define the variables associated with the interaction terms. Thus, all the other coefficients are exactly the same with specification (1) of Table 7.7 and therefore, not presented here. The same holds for the second specification

Appendix A. Zero-Inflated MisProbit

As mentioned in Section 2.5, it would be a good idea to incorporate a zero inflation probability in the MisProbit model in an attempt to separate under-reporting from zero-inflation. In other words this model will attempt to separate potential criminals from genuine non criminals. In this Appendix, this model together with some empirical results are presented. To this end, assume that there is a fraction of people, ξ, that never commit and consequently never report a crime. The remaining fraction of individuals, $1-\xi$, follow the binary choice model with misclassification. The corresponding response tree is presented in figure 2.5. The conditional probabilities for the reported crime now become,

$$\begin{aligned}\Pr(y_i = 1|x_i) &= (1-\xi)\left[(1-F(x_i'\beta))\,a_1 + F(x_i'\beta)(1-a_0)\right] \\ \Pr(y_i = 0|x_i) &= \xi + (1-\xi)\left[(1-F(x_i'\beta))\,(1-a_1) + F(x_i'\beta)a_0\right]\end{aligned} \tag{A.1}$$

Then, we specify the log-likelihood function as in (2.14) and we find the values of ξ, a_0, a_1, and β that maximize it. In case the probability of zero inflation is given by a Logit model, such as $\xi_i = e^{q_i'u}/(1+e^{q_i'u})$, the log-likelihood takes the following form,

$$\begin{aligned}\ln L(\beta, u, a_0, a_1) = \sum_{i=1}^{n} &-\ln(1+e^{q_i'u}) + y_i \ln\left[(1-F(x_i'\beta))a_1 + F(x_i'\beta)(1-a_0)\right] \\ &+ (1-y_i)\ln[e^{q_i'u} + (1-F(x_i'\beta))(1-a_1) + F(x_i'\beta)a_0].\end{aligned} \tag{A.2}$$

Estimation of this model seems difficult if probabilities of misclassification and zero-inflation are all covariate-dependent, since with one data set we try to estimate four distinct processes. Instead, given quite large samples, estimation could be feasible if for example, zero-inflation probability is allowed to depend on regressors but one of the misclassification probabilities is considered as constant. In any way, estimation of these models is a hard task, particularly when noisy data such as crime data are used.

The estimation analysis has shown that, although identifiable theoretically, misclassification and zero-inflation probabilities cannot be estimated if they are all considered as constants.The zero-inflation parameter remains unidentified even when under-reporting depends on covariates. However, if zero-inflation depends on regressors, both the ZI-MisProbit model with constant misclassification and the covariate dependent ZI-MisProbit model behave better. First, the case of constant misclassification is presented, followed by the covariate dependent ZI-MisProbit.

Constant Misclassification

Although under-reporting seems to be covariate dependent, here the results of a model of constant under-reporting is presented. Naturally, zero-inflation should depend on the same vector of regressors. Here, truthfulness, which is assumed to affect the zero-inflation probability, is used to facilitate the optimization procedure. However, an extra exclusion was required (here in the form of not including age squared in the reporting process), even though there was no specific reason for this. Otherwise, misclassification probabilities were forced to be negative, and therefore, not helping the separation of zero-inflation from under-reporting. This is also the reason why this model was not presented in the main results analysis, as its behaviour was not trustworthy, perhaps because of the combination of noisy data and complicated models. Nevertheless, a few results which will be presented in this Appendix indicate that this model "works", as it can potentially separate zero-inflation from under-reporting.

These results are presented in Table 2.19. In the first column results of the MisProbit model with constant misclassification are presented, whereas the ZI-MisProbit results are presented in column 2. It is very interesting that, after controlling for zero-inflation, the probability of under-reporting is predicted to be 0.48, almost 33 percentage points lower than the model in column 1. The predicted probability of zero-inflation is 39.3%. Thus, this model says that almost 40% of people (about 3,680 individuals) never commit crimes and consequently they do not report any, and from the rest of them, 48% report no crimes even though they have committed at least one.[1] Furthermore, we can notice that all coefficient of the zero-inflation process are statistically insignificant. We finally see that both the predicted probability of committing a crime and the probability of over-reporting is almost the same across the two models.

[1] Apart from the case where age squared was included in both processes, all other specifications show that given the set of controls, zero-inflation probability is around 40% and probability of under-reporting is around 35%.

Covariate-Dependent Under-reporting

As we have seen from the main results, the mix of under-reporting and zero-inflation is covariate-dependent. In the previous part only zero-inflation was allowed to depend on covariates. In this part we will have a look at the model where both processes are covariate-dependent. Misclassifying a zero as one will still be considered as constant. Before proceeding to the results, we must stress that with one data set we try to identify the parameters of three different processes. In addition to that, we must be cautious not to misspecify the model by excluding variables that must be included. For example, in the previous model I did not include age squared in the zero-inflation process without a special reason. Therefore, this model is too demanding to produce reliable estimates with such noisy data. However, some results are presented in Table 2.20, which show that this model also "works".

Once more, the first column replicates the second specification of Table 2.12. First of all, we notice that all parameters of this model are identified. It can be said that, this is a "better specified" model, since there is only one exclusion restriction from both the zero-inflation and crime processes. This role, as before, is played by the dummy "truthfulness". On the other hand, as mentioned before, this is a too complicated model relative to the quality of data and trustworthiness is a question here.

Unfortunately, the results of this model do not coincide with ZI-MisProbit with constant misclassification. In this model, we see that the predicted probability of crime is around 51%, which says that from potential criminals (since we have separated the genuine non criminals) almost half of them commit at least one property crime. Moreover, we can also notice that the predicted probability of zero-inflation is around 8%, which is much smaller than the predictions of the previous model. Nonetheless, there is one common finding across these two. In both models, apart from truthfulness which seems to be significant in the current model, the independent variables do not seem to affect the probability of being genuine non-criminal. However, the values of their corresponding coefficients differ considerably. For some of these variables even the direction of the effect is the opposite one.

Comparing the first column with the second, there are a few things that merit some discussion. Although we would expect the predicted probability to be lower, the ZI-MisProbit gives almost the same probability of under-reporting and an extra zero-inflation probability of 8%. Thus, this model, at least for the data of this study, does not seem to separate zero-inflation from under-reporting. Regarding the immigrant coefficient, in contrast with column 1, the ZI-MisProbit model says that being an immigrant increases the probability of crime, but the coefficient is statistically insignificant. Although the rest of the coefficients in the crime process follow the same direction as the coefficients of the first column, they are very different in terms of magnitude. The rest of the coefficients of the two processes are not discussed further since this model is just presented to show that if there are good reasons to believe that the generating data process follows a ZI-MisProbit model, given a richer data set, there might be gains from using it.

Figure 2.5. Zero Inflation MisProbit

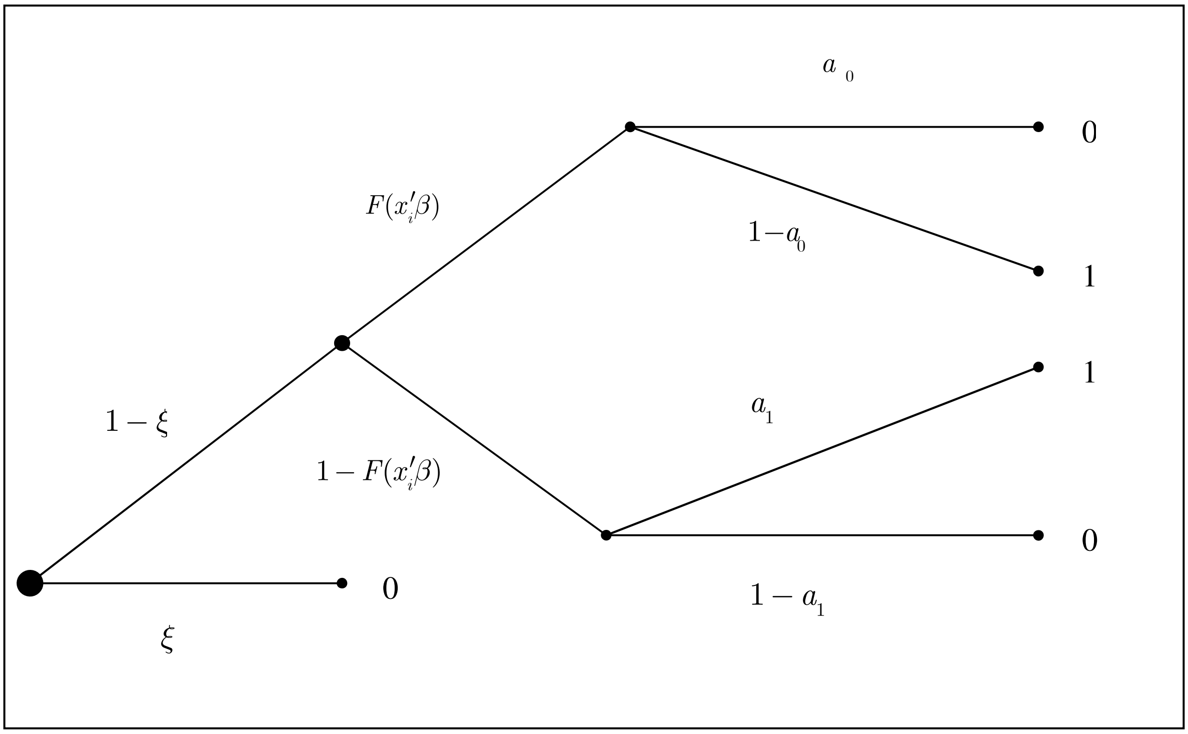


Figure 2.6. Zero-Inflation – Poisson-Logit

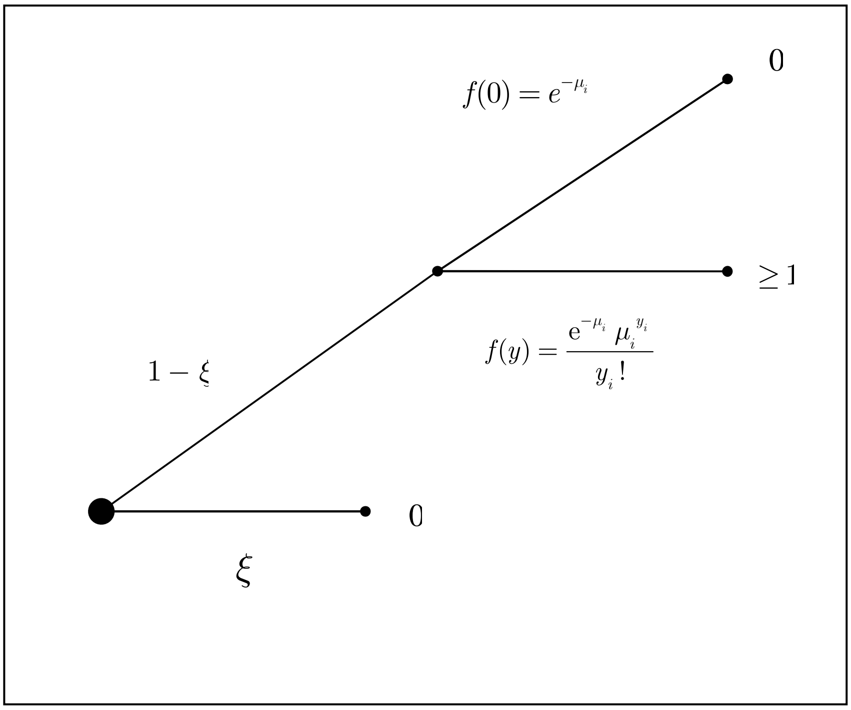


Table 2.19. MisProbit *Vs* ZI-MisProbit. *Constant Misclassification*

	MisProbit		ZI-MisProbit	
	Coefficient	Robust S.E	Coefficient	Robust S.E
Constant	-0.226	*(0.701)*	0.671	*(0.977)*
Immigrant	0.055	*(0.334)*	0.095	*(0.439)*
Age	-0.060	*(0.046)*	-0.126**	*(0.057)*
Age^2	0.000	*(0.000)*	0.001*	*(0.000)*
Male	0.926***	*(0.379)*	0.546***	*(0.212)*
White	0.826**	*(0.405)*	0.719*	*(0.388)*
Region South	0.147	*(0.250)*	0.484	*(0.493)*
Region Midlands	0.024	*(0.225)*	0.489	*(0.453)*
Region North	0.109	*(0.250)*	0.627	*(0.505)*
Prob of Misclassification of One as Zero (Under-reporting)				
Constant	0.811***	*(0.066)*	0.480*	*(0.288)*
Prob of Misclassification of Zero as One (Over-reporting)				
Constant	0.012***	*(0.005)*	0.014**	*(0.006)*
Prob of Zero-Inflation				
Constant			2.509	*(6.468)*
Immigrant			0.136	*(0.977)*
Age			-0.075	*(0.053)*
Male			-0.448	*(0.383)*
White			-0.073	*(0.668)*
Region South			1.013	*(1.364)*
Region Midlands			1.283	*(1.179)*
Region North			1.359	*(1.216)*
Truthfulness			3.668	*(4.989)*
Sample Size	11,658		11,658	
Log Likelihood	-1,446.92		-1,429.16	
Predicted.Prob.Crime	0.280		0.269	
Predicted Prob. of ZI			0.393	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Table 2.20. MisProbit Vs ZI-MisProbit. *Covariate-Dependent Misclassification*

	(1)		(2)	
	Coefficient	R.S.E	Coefficient	R.S.E
Prob of Property Offence in Last Year				
Constant	2.335***	*(0.896)*	5.959	*(3.660)*
Immigrant	-0.254	*(0.273)*	0.756	*(0.748)*
Age	-0.259***	*(0.046)*	-0.386*	*(0.203)*
Age^2	0.003***	*(0.0006)*	0.003*	*(0.002)*
Male	0.440***	*(0.156)*	1.225	*(0.828)*
White	0.542*	*(0.293)*	1.380*	*(0.787)*
Region South	0.273	*(0.231)*	1.650	*(1.585)*
Region Midlands	0.124	*(0.228)*	1.973	*(1.902)*
Region North	0.496	*(0.318)*	1.951	*(1.642)*
Prob of Misclassification of One as Zero (Under-reporting)				
Constant	2.267***	*(0.507)*	-1.068**	*(0.505)*
Immigrant	-0.410	*(0.346)*	0.061	*(0.185)*
Age	-0.195***	*(0.074)*	0.165***	*(0.039)*
Age^2	0.003***	*(0.001)*	-0.003***	*(0.001)*
Male	-0.277*	*(0.142)*	-0.306***	*(0.085)*
White	-0.198	*(0.224)*	-0.394***	*(0.140)*
Region South	0.262	*(0.224)*	0.132	*(0.144)*
Region Midlands	0.224	*(0.215)*	0.305**	*(0.144)*
Region North	0.490**	*(0.212)*	0.208	*(0.155)*
Truthfulness	0.868***	*(0.256)*	0.623***	*(0.138)*
Prob of Zero-Inflation				
Constant			-44.141	*(68.415)*
Immigrant			0.271	*(0.847)*
Age			8.640	*(12.355)*
Age^2			-0.406	*(0.533)*
Male			-0.557	*(0.498)*
White			0.354	*(0.718)*
Region South			-0.394	*(0.632)*
Region Midlands			-0.754	*(0.849)*
Region North			-0.503	*(0.948)*
Prob of Misclassification of Zero as One (Over-reporting)				
Constant	-2.234***	*(0.204)*	-2.140***	*(0.083)*
Log Likelihood	-1,422.05		-1,415.86	
Predicted.Prob.Crime	0.292		0.509	
Predicted.Prob.Under	0.709		0.719	
Predicted.Prob.Inflation			0.076	

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Appendix B. NB1-Logit, Generalized-NB-Logit, ZI-Poisson-Logit, and ZI-NB2-Logit

In subsection 2.5.2 we discussed that identification of the NB2-Logit model requires exactly the same conditions established for the Poisson-Logit model (exclusion restriction on count process or sign restrictions on reporting process). A model that is identified even without the restrictions described above is the Negative Binomial 1-Logit (NB1-Logit), which is obtained if we assume that α_i depends on regressors in the following manner, $\alpha_i = \theta/\lambda_i$ (see, Papadopoulos, 2011a, and Papadopoulos and Santos Silva, 2008). According to this form of variance of ϵ_i, the variance of y_i changes to $\omega_i = \mu_i + \theta\lambda_i\Lambda_i^2$. It should be noted that identification of the conditional mean is easier only because we impose more structure on the variance. Hence, if the variance form of α_i is misspecified, the estimates of θ will be in general inconsistent.

Instead of assuming the form of the variance, we can specify a generalization of it as $\omega_i = \mu_i + \theta\lambda_i^{2-c}\Lambda_i^2$, where c is an extra parameter to be estimated. In case $c = 0$, a NB2-Logit is obtained, whereas in case $c = 1$, a NB1-Logit is obtained. Therefore, identification becomes "weaker" as c gets closer to 0. According to this general parameterization of the variance the following log-likelihood arises,

$$\ln L(\theta, c, \beta, \gamma) = \sum_{i=1}^{n} \ln\left(\Gamma(y_i + \theta^{-1}\lambda_i^c)/\Gamma(y_i + 1)\Gamma(\theta^{-1}\lambda_i^c)\right) - \\ (\theta^{-1}\lambda_i^c + y_i)\ln(1 + \theta\lambda_i^{1-c}\Lambda_i) + y_i(\ln\lambda_i^{1-c} + \ln\Lambda_i + \ln\theta) \quad \text{(B.1)}$$

Similarly to the models for binary choice, models for count data with under-reporting can also be generalized to take into account zero-inflation. First, the Zero-Inflation-Poisson-Logit (ZIP-Logit) specification is presented. A construction of a response tree similar to (5.2) is helpful to derive the conditional probabilities of interest. As before, there is a fraction of people, ξ, that never commit and consequently never report crimes. The remaining fraction of individuals, $(1 - \xi)$, can either commit or not commit crimes, but their responses are subject to under-reporting, meaning that they follow the Poisson-Logit model. Therefore, zeroes come from zero-inflation, or, from the Poisson-Logit mixture distribution. That is, zeroes because of under-reporting, or zeroes because of the choice not to commit crimes. The response tree is presented in figure 2.6 (page 122). In this case, ξ cannot distinguish between zeroes because of never committing crimes (zero inflation) and always reporting zeroes (total under-reporting), which was the case in the binary choice model.

In this tree, $e^{-\mu_i}$, is the probability of zero from the Poisson-Logit model, and $\mu_i = \lambda_i \Lambda_i$. According to this model, the conditional probabilities of zero and positives are the following,

$$\begin{aligned} \Pr(y_i = 0|x_i) &= \xi + (1-\xi)e^{-\mu_i}, \\ \Pr(y_i > 0|x_i) &= (1-\xi)\frac{e^{-\mu_i}\mu_i^{y_i}}{y_i!}. \end{aligned} \tag{B.2}$$

The mean of this model is given by $\nu_i = (1-\xi)\mu_i$ and variance $\omega_i = (1-\xi)(1+\xi\mu_i)\mu_i$, so that there is overdispersion. If ξ is a constant, the log-likelihood is given by,

$$\ln L(u,\beta,\gamma) = \sum_{y=0} \ln\left(\xi + (1-\xi)e^{-\mu_i}\right) + \sum_{y>0} \ln\left((1-\xi)\frac{e^{-\mu_i}\mu_i^{y_i}}{y_i!}\right). \tag{B.3}$$

However, ξ can also depend on covariates, so that we try to model what are the characteristics that lead people never committing crimes or always reporting no crimes. If ξ is a Logit, with $\xi_i = e^{q_i' u}/(1+e^{q_i' u})$, then we can write the log-likelihood function of the ZIP-Logit as,

$$\begin{aligned} \ln L(u,\beta,\gamma) = -\sum_{i=1}^{n} \ln\left(1+e^{q_i' u}\right) &+ \sum_{y=0}\left(e^{q_i' u} + e^{-\mu_i}\right) \\ &+ \sum_{y>0}\left(-\mu_i + y_i \ln\mu_i - \ln(y_i!)\right). \end{aligned} \tag{B.4}$$

Identification of this model requires the same assumptions established for the Poisson-Logit, so that exclusion restrictions in the Poisson part, or sign restrictions on the Logit part are required.

Reformulation as a Zero-Inflation-NB2-Logit (ZI-NB2-Logit) model is straightforward. The probability of an observed zero outcome from the NB2-Logit is now given by $(1+\alpha\mu_i)^{\alpha^{-1}}$, and if ξ is a Logit, the resulting log-likelihood is given as,

$$\begin{aligned}\ln L(\theta, u, \beta, \gamma) = -\sum_{i=1}^{n} \ln\left(1 + e^{q_i' u}\right) + \sum_{y=0} \ln\left(e^{q_i' u} + (1 + \alpha\mu_i)^{\alpha^{-1}}\right) + \\ \sum_{y>0} \ln\left(\Gamma(y_i + \alpha^{-1})/\Gamma(y_i + 1)\Gamma(\alpha^{-1})\right) - \\ (\alpha^{-1} + y_i)\ln(1 + \alpha\mu_i) + y_i(\ln\mu_i + \ln\alpha).\end{aligned} \tag{B.5}$$

The mean of the ZI-NB2-Logit is given by $\nu_i = (1 - \xi)\mu_i$ as before, and the variance is given by $\omega_i = (1 - \xi)(1 + \xi\mu_i + \theta\mu_i)\mu_i$. Again, identification of this model requires the same conditions established for the Poisson-Logit and the NB2-Logit models.

Appendix C. More Robustness Checks

In this Appendix we consider three extra robustness checks. All results of the these three exercises are presented in Table 2.21. The first specification of this table gives the results of the second specification of Table 2.12 for the sake of comparisons. Note that the covariate-dependent MisProbit is used to obtain all the estimates presented in this Appendix. However, Table 2.21 presents only the results of the crime part. The estimates of the reporting part are available from the author on request.

Dropping Very Recent Immigrants

As a first exercise we drop from the sample immigrants who have reported that have been in the country for less than 12 months. This serves two purposes. Firstly, the OCJS does not record crimes that happened outside the UK. Since the crime questions concern individuals' criminal behaviour during the 12 months prior to the day of the interview, there might be some cases of very recent immigrants who have committed crimes outside the UK which are not recorded. Thus, the immigration coefficient would be downward biased if we overlooked those cases. However, at the same time, some of the most recent immigrants may have committed crimes in their source countries and mistakenly recorded them as happened in the UK. Nevertheless, we would not like to include these reported crimes in our sample either as we are only interested in the criminal behaviour of immigrants in the host country, since their countries of origin may exhibit very different characteristics associated with property crime, such as different economic opportunities and deterrent factors. Therefore, by dropping immigrants that have been in the UK for less than a year we avoid these two ambiguous scenarios. From specification 2 of Table 2.12 we notice that when we drop these 117 cases, the coefficient on migration status slightly increases in magnitude but it is still statistically insignificant.

Dropping very Young Individuals

The analysis so far has included people from 10 to 66 years old. In this exercise we are looking at the consequence of dropping very young individuals, as responses of children might be less reliable. However, notice that dropping even very young individuals, since we include a youth-boost (3,185 individuals) we lose many observations which are essential for the behavior of the model. The results are presented in four specifications. Specification 3 excludes respondents younger than 11 years old, specification 4 also excludes 12 year olds, specification 5 also excludes 13 year olds and specification 6 also excludes 14 year olds. First notice that the immigration coefficient becomes more negative if we drop ten year olds and keeps increasing in magnitude as we drop individuals of 11, 12, and 13 years of age and it actually becomes significant at 10% in specification 5. However, notice that although the remaining individuals might provide more reliable information, the precision of all estimates substantially decreases in specifications 4 to 6. Although not presented in the table, the precision of the coefficients of the under-reporting (zero-inflation) part also decreases considerably. This is because, as explained throughout this study, given the low variation of the dependent variable, the noisy nature of self-reports and the complexities of the MisProbit MLE, the sample size is very important. Thus, dropping 1,443 individuals in specification 6 results in very harmful consequences for the behavior of the model. Also notice that if we drop 14 years old individuals, which reduces the sample size by 1,785 observations, results in no convergence of the estimation procedure.

Without Criminal Damage

Even though criminal damage is also a crime against the property, it entails only psychological gains to the offenders and therefore, it is not very clear whether it is proper to include it in the property crime variable. This is because as it is the case for violent crime, criminal damage cannot be well explained by the economic model of crime. First, excluding criminal damage (1.51% positives) reduces the probability of observing a property crime from 5.47% to 4.91%. The results in specification 5 and 6, where in specification 6 we have no exclusion restriction, show that the immigration-property crime differential slightly reduces in magnitude but it still retains its sign. Notice however, that increasing the number of zeroes also results in less precise estimates for all estimates, as in the previous exercise.

Table 2.21. More Robustness Checks

Mis.Probit Property Crime	(1) Property Crime	(2) > 1 year Immigrants	(3) Age>10	(4) Age > 11	(5) Age>12	(6) Age>13	(7) No Criminal Damage	(8) No Cr.Damage No Truthfulness
Constant	2.335***	2.220**	1.872**	1.816*	1.462	1.362	1.912*	2.363
	(0.896)	*(0.881)*	*(0.812)*	*(1.049)*	*(1.217)*	*(1.118)*	*(1.005)*	*(1.666)*
Immigrant	-0.254	-0.301	-0.325	-0.524	-0.668*	-0.713	-0.162	-0.201
	(0.273)	*(0.287)*	*(0.286)*	*(0.403)*	*(0.374)*	*(0.466)*	*(0.278)*	*(0.306)*
Age	-0.259***	-0.249***	-0.241***	-0.225***	-0.190**	-0.187**	-0.254***	-0.254***
	(0.046)	*(0.044)*	*(0.045)*	*(0.065)*	*(0.091)*	*(0.084)*	*(0.052)*	*(0.076)*
Age^2	0.003***	0.003***	0.003***	0.003***	0.002*	0.002*	0.003***	0.003***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*
Male	0.440***	0.463***	0.434***	0.339	0.209	0.176	0.531***	0.458***
	(0.156)	*(0.160)*	*(0.169)*	*(0.281)*	*(0.338)*	*(0.309)*	*(0.160)*	*(0.170)*
White	0.542*	0.439*	0.566**	0.553	0.449	0.545	0.609*	0.375*
	(0.293)	*(0.259)*	*(0.283)*	*(0.409)*	*(0.493)*	*(0.645)*	*(0.329)*	*(0.381)*
Region	0.273	0.228	0.382	0.552	0.798	0.892	0.315	0.245
South	*(0.231)*	*(0.218)*	*(0.257)*	*(0.466)*	*(0.788)*	*(0.844)*	*(0.257)*	*(0.284)*
Region	0.124	0.096	0.208	0.292	0.337	0.348	0.211	0.160
Midlands	*(0.228)*	*(0.218)*	*(0.242)*	*(0.344)*	*(0.529)*	*(0.588)*	*(0.247)*	*(0.235)*
Region	0.496	0.416	0.564	1.008	1.393	1.345*	0.526	0.462
North	*(0.318)*	*(0.289)*	*(0.350)*	*(0.710)*	*(0.958)*	*(0.869)*	*(0.351)*	*(0.432)*
Sample Size	11,658	11,541	11,365	10,997	10,620	10,215	11,658	11,658
Log Likelihood	-1,422.05	-1,413.88	-1,392.28	-1,358.77	-1,322.63	-1,265.83	-1,321.94	-1,329.40
Predicted. Prob.Crime	0.292	0.272	0.249	0.313	0.355	0.357	0.250	0.302
Predicted. Prob.Under	0.709	0.678	0.657	0.753	0.791	0.798	0.664	0.715

Robust standard errors are presented in parentheses.

(***), denotes statistical significance at 1% significance level

(**), denotes statistical significance at 5% significance level

(*), denotes statistical significance at 10% significance level

Chapter 3

The Relationship between Immigration Status and Victimization

3.1 Introduction

The link between immigration and crime is well discussed among scholar and non-scholar communities. Nevertheless, most of the discussions by non-scholars concern immigrants' involvement into criminal activities as offenders. This one-dimensional treatment has led to the sentiment that immigrants are more involved in illegal actions. This is in most cases in contradiction with scholars' findings (mostly by criminologists and sociologists) which suggest the opposite.[1] For instance, Papadopoulos' (2010b) findings, in a study for England and Wales, suggest that immigrants' participation in criminal activities as offenders (both in property and violent crimes) is slightly lower than natives' one as opposed to the public sentiment.[2]

Since scholars have also focused on the immigration-crime link from the offending point of view, they have overlooked the other important side of the coin which concerns the engagement of immigrants in crime as victims. To my knowledge there are no comprehensive studies that concentrate on this relationship,[3] as most studies focus on the determinants

[1] For a review of the literature for the involvement of immigrants as offenders refer to Papadopoulos (2010b).

[2] These results are obtained using the Offending, Crime and Justice Survey of 2003 and appropriate estimators to correct for possible under-reporting of self-reported crime.

[3] To the author's knowledge, the only finding on this link for the UK comes from Machin, Bell and Fasani (2010) study, who find that immigrants are less likely to be victimized using British Crime Survey data from

of victimization in general (see, for example, Miethe, Stafford and Long, 1987, Smith and Jarjoura, 1989, Kennedy and Forde, 1990, Mustaine and Tewksbury, 1998, Wiles, Simmons and Pease, 2003, Tseloni, Wittebrood, Farrell and Pease, 2004, and Tseloni, 2006). More relevant studies look at the experiences of ethnic minority groups without distinguishing between native and immigrant populations (see, for instance, Clancy at al, 2001, Jansson, 2006) and some of them are criminological studies that focus on a very specific aspect of victimization experiences by ethnic minorities, namely, racially motivated crime, or differently, "hate" crime (see, Gabbidon and Greene 2009, Spalek, 2008, and Kalunta-Crumpton, 2010).

This study, therefore, intends to fill this gap by investigating the victimization differences between immigrants and natives in England and Wales.[1] Looking into victimization would complete the crime picture and possibly provide many interesting insights for immigrants' behaviour towards criminal activities. Moreover, this study would shed some light on the social integration of immigrants into the society. As a result, the findings of this work could be a useful tool for policy makers.

The first aim of the present study is to comprehensively examine whether immigrants are more or less at risk of becoming victims of crime and whether differences would still exist if immigrants shared the same demographical characteristics with the native population. In a second step we try to identify the reasons that lead to higher or lower victimization of immigrants. For the purposes of the above analysis we use the 2007-08 sweep of the British Crime Survey (BCS), a representative victimization survey where respondents were asked in face-to-face interviews about their victimization experiences in household and personal crime.[2] We need to note that the nature of the victimization incident is very different across different crime categories, such as property crime (burglaries, vehicle thefts, other thefts, criminal damage) and personal crime (personal thefts, violence). Therefore, the immigration-victimization link will be examined separately for the different crime categories, but more attention will be paid to violent crime.

2004 to 2008. However, the victimization part concerns only a very small part of their paper, so that they give only a very narrow picture of the immigration-victimization link.

[1]This study examines the victimization experiences of immigrants and natives only in England and Wales, as Northern Ireland and Scotland are excluded from the British Crime Survey because of their distinct criminal justice system.

[2]The BCS data used in this study are sponsored by the Home Office and provided by the UK Data Archive.

Although the present study is mainly empirical, and to some extent methodological, some theory developed by criminologists and sociologists will still be presented in the next section, that formalizes the theoretical expected link between immigration and victimization. This theory is based on potential victims' *lifestyle-exposure* (Hindelang, Gottfredson, and Garofalo, 1978) and *routine activities* (Cohen and Felson, 1979) which shape their so-called criminal opportunity structure. In the present study these theories are adjusted to incorporate simple notions from the seminal economic models of crime by Becker (1968) and Ehrlich (1973).

According to the above theories there are many channels, at least for instrumental crime[1] both against the household and against the person, through which immigration and victimization are linked either positively or negatively. For instance, immigrants would be more victimized as they are disproportionately located in deprived areas where crime rates are much higher. On the other hand, immigrants are less attractive as targets since they usually possess fewer properties, or relatively less valuable objects. Therefore, the theory cannot provide a clear-cut relationship between immigration and pecuniary crime; this is rather an empirical question. As our data provide a lot of information on attributes that are associated with instrumental crime, we are able to acquire a better understanding of the reasons why we (do not) observe differential risks of victimization between immigrants and natives.[2]

The case of violent crimes is less obvious, as violence refers to expressive actions where the offender intends to hurt the person and not to acquire his/her property. In violent crime, contrary to property crimes, inter-relations and interactions between potential offenders and potential victims are important. Thus, personal behaviour is a much stronger predictor of violent victimization compared to instrumental victimization. Although the above theories are still valid (given some conceptual modifications), it is difficult to identify the theoretical channels through which immigrants become more or less likely to be victimized, as most determinants of the violent victimization incident are unobserved factors determined by the

[1] By instrumental I mean pecuniary, or differently, a crime that the intention of the offender is to acquire victim's property and not to hurt the person itself.

[2] For example, even if it is the case in the raw data that immigrants face the same risk of becoming victims of burglaries, we know that immigrants would actually face a lower risk of burglary victimization if they were located in natives' residents, as immigrants are located in relatively more deprived areas where the crime rates are higher.

potential victim and his/her relationship and interactions with potential offenders. For instance, some people would be less likely to suffer a violent crime if they followed particular lifestyles associated with lower crime. However, since most aspects of this lifestyle are unobserved, only speculations can be done to explain the factors that have generated this differential risk of victimization among different groups of individuals.

Nevertheless, there is one channel that is very clear. Holding all other factors associated with victimization constant, immigrants would still be at higher risk of violent victimization than natives because of racially motivated crime. To what extent can racially motivated crime explain differences in the victimization patterns between immigrants and natives? This is also an interesting issue that will be examined. Moreover, violent crime consists of three distinct types of very different nature, namely crime suffered by strangers, crime suffered by acquaintances and crime suffered by family members (or ex-family members, such as ex-partners). As will be seen later, modeling these three crime types separately will provide some very interesting insights on the immigration-victimization nexus.

Another important point is that, since the questionnaire of the BCS involves some questions that try to elicit very sensitive information, misreporting is a concern. For instance, there is evidence that respondents tend to under-report domestic violence perhaps because of fear of reprisal, or because they want to protect the offender (Walby and Allen, 2004, and Felson at al, 2006). If immigrants' reporting behaviour differs from natives' one then, the coefficient representing the difference in domestic victimization between immigrants and natives will be biased. However, in Section 3.6, by utilizing two different strategies we show that immigrants do not under-report by more than natives.

Once the above relationships are established using a thorough examination of sensitivity tests, some equally interesting topics will be examined. For example, exploiting the number of victimization incidents we will be able to develop a better understanding of the victimization experiences of immigrants. Is the use of count data models going to change the picture obtained by the binary choice models? If yes, count data models have something to say about differential repeated victimization experiences between immigrants and natives. As will be clear later, conventional count data models, such as the Poisson or the Negative Binomial regression models, are inadequate to explain the underlying relationship between immigration and victimization due to limitations of the data set, such as the presence of a few extreme cases where respondents reported a very high number of victimization incidents, or, the very large number of zeroes. Therefore, models that take into account these limitations are used.

Other interesting topics that will be investigated involve whether the ethnic composition or the location of immigrants matters, whether there are assimilation patterns in the immigrants' victimization experiences and whether immigrant victims perceive their victimization experiences as more serious than otherwise comparable natives.

We need to note that there will be no separate section for the econometric models used throughout this study. Instead, if the econometric models used in each section deserve a formal presentation or at least some clarifications or discussions, they will be given at the beginning of each corresponding section.

The rest of this study is organized as follows. In the next section a brief exposition of a victimization theory together with a short discussion of the link between immigration and crime is presented. Section 3.3 is devoted to explaining some technical parts of the BCS and the construction of the dependent variables. Additionally, a description of the data used in the empirical analysis and some descriptive statistics are presented. In Section 3.4 a basic analysis for household crime follows, where we investigate whether immigrants are less or more likely to be victims of household crimes, focusing on inside and outside burglaries. Section 3.5 examines the experiences of personal crime. Although some results on personal theft are also presented, this section puts more weight on violent victimization. Section 3.6 provides a thorough sensitivity analysis with regard to the results of the previous section. Section 3.7 delivers a few results of interaction terms and perceived seriousness of victimization incidents. A comprehensive analysis of count data models follows in Section 3.8. Finally, Section 3.9 consists of concluding remarks.

3.2 Theoretical Perspectives on Victimization

Before discussing the theoretical concepts of victimization it is worth noting that although this study also presents results on inside and outside burglaries and on personal thefts, most of the attention is paid to violent victimization. The results for the other crime types, such as vehicle crime, criminal damage and household thefts will be briefly discussed but not presented in detail. Moreover, as these crime types are of a very different nature, we need to emphasize that to some extent the theoretical concepts apply differently to the different crime groups.

When it comes to the offender-victim relationship it is natural to argue that in many cases full responsibility falls onto the offender (although victims could still be unintentionally responsible). For instance, think of a young girl whose purse gets stolen in a station of London's Underground. This is not always the case though. Even early theories (see, for example, Von Hentig, 1940 and Wolfgang, 1958) admit that there are cases in which offenders do not bare full responsibility, but the crime is a function of the underlying offender-victim relationship evolving prior to the victimization incidence. Crimes are considered as interactive acts that depend upon the actions of both parts. Thus, these theories rule out the factor of "randomness" in victimization incidents.[1] For instance, precipitation theory, first discussed by Wolfgang (1958), argues that to some extent it is the victims' provocative behaviour that initiates subsequent crimes against them (see, Schultz, 1968, and Curtis, 1974). Clearly, the above theories seem more appropriate to describe violent crimes where for instance, the victim using gestures or offensive language initiates an assault. Or, we could think of a case of domestic crime where the interaction of family members is very important.

However, the theories that have attracted most both theoretical and empirical research are based on the concepts of *lifestyle-exposure* (Hindelang, Gottfredson, and Garofalo, 1978) and *routine activities* (Cohen, and Felson, 1979). Earlier concepts, such as the importance of offender-victim relationship are integrated into these more recent ones. We need to note that although each of these theories was initially developed for different purposes, they are closely related and the present study treats them as a single comprehensive theoretical framework (see, Meier and Miethe, 1993, for an elaborate exposition of these theories). According to them, *routine activities* and particular *lifestyles* of potential victims shape a criminal opportunity structure which consists of four distinct risk factors that are associ-

[1] By "random" victimization incidents I mean situations where, there is no prior relationship between the offender and the victim and the victimization incident does not depend on the interaction between offenders and victims.

ated with victimization. These factors are: *proximity*, *exposure*, *attractiveness* and *capable guardianship*. *Proximity* and *exposure* create the criminal opportunity structure, whereas *attractiveness* and ability of effective *guardianship* determine the criminals' choice of victims (Miethe and Meier, 1990).

Proximity is defined as the physical distance between locations that potential targets tend to spend most of their time in and locations where potential offenders mostly act. For instance, living in highly deprived areas, where the crime rates are high, increases the probability to be victimized, as it increases the probability of contacting potential offenders. This concept becomes less relevant as the mobility of the target increases, since the task of identifying the distance between offenders and victims becomes more difficult. Therefore, although the concept of *proximity* is very clear for household crimes, it loses some transparency once we deal with personal crime. However, it is still important as most victims tend to socialize in areas close to their residences.[1]

Exposure refers to the physical visibility or availability of potential victims. The meaning of this concept changes substantially between different types of crime. For personal violence, *exposure* can be conceptualized as the general *routine activities* or *lifestyles* of potential victims, associated with higher or lower likelihood of victimization. For instance, people that mostly stay at home and do not socialize in bars or pubs tend to be less likely to suffer a violent crime. Here, general lifestyle also includes relationships and interactions of potential offenders with potential targets. Thus, this concept also incorporates the earlier theories of precipitation. For household crime, this risk factor takes a very different meaning. For instance, for inside or outside burglaries *exposure* may refer to the location of the house (such as main road or cul-de-sac), or the amount of properties someone possesses. For vehicle crime just a high number of cars owned by an individual can be considered as an indicator of high *exposure*.

Target attractiveness is defined as the material (for acquisitive crimes) or symbolic (for violent crimes) desirability (value) of targets to potential offenders. The notion of *attractive-*

[1]According to the victim forms of the 2007/08 BCS around 20% of all personal victimization incidents happened inside or immediately outside victims' residence. From the rest of them, 6% occurred in workplace, 18% at pub/bar/club, 35% in other public or commercial location and 22% elsewhere. Moreover, it is very interesting that for the incidents that did not happen inside or outside residence, 40% of them took place within 15 minutes from victim's residence.

ness is again very different across acquisitive and violent crimes. For instance, in household crime of acquisitive nature, the appearance of the house, or the information of offenders for valuable objects inside the house increases *attractiveness*. For personal thefts, the general appearance can indicate a level of *attractiveness*. On the other hand, violence is an expressive crime, as offenders target to hurting the victim itself without being interested in victim's valuable possessions.[1] Just the ethnicity of a potential victim can be considered as highly attractive attribute for an extremist. In other cases *attractiveness* develops through interactions and interrelations among people. For example, a member of a gang finds as an attractive target a member of another gang (with regard to the symbolic utility that the offender gains if he/she commits the crime). Or, two persons with a history of previous arguments find one another more attractive to a potential offence.

Finally, *physical* or *social guardianship* is the effectiveness of objects (*physical guardianship*) or people (*social guardianship*) in preventing crime from occurring. For personal crimes, *guardianship* is the ability of the person, or the ability of people around him/her, to protect him/her. Having a weapon in apparent place, or guards, is a type of *physical guardianship*. Also demographic features as height, weight, age, appearance, could indicate an ability of protection. *Physical guardianship* for dwellings and vehicles could be for example security measures, neighbourhood watching program, etc. On the other hand social measures could be number of hours house left unoccupied, number of household members (more members indicates that the house is left unoccupied less hours per day, which decreases the likelihood to be burglarized), knowledge on what to do in case someone breaks into the house, etc.

The basic economic theory of crime is closely related to the above sociological views. A two-stage model which borrows simple notions from the early economic models of crime by Becker (1968) and Ehrlich (1973) could be formulated to describe the victim-offender relationship. According to these early models of crime, individuals use a rational cost-benefit analysis where they weigh the expected costs and benefits in utility terms and subsequently decide how much time to allocate in legal and criminal activities in order to maximize their net expected utility. Since crime involves uncertainty, because of potential apprehension

[1]We need to note that for robberies there is a violent act together with the theft. However, as the primary target of the offender is instrumental I consider robbery as personal theft.

and consequent future punishment, the notion of risk aversion is very important. At the same time, uncertainty and risk aversion are also very important from the potential victims' point of view, as the actions of potential victims could not perfectly determine the criminal activity against them.[1]

Although in reality the situation is much more complicated, a simple model could be formulated as follows: in the first period the (rational) potential victims, given the level of risk aversion and the initial values of *attractiveness*, *exposure*, *proximity* and *capable guardianship* (as these are determined by their exogenous socio-economic and demographic attributes), consider a set of different strategies and the possible consequent actions of potential offenders for each different strategy. Consequently, they re-evaluate their position by determining to some extent the optimal levels of *attractiveness*, *exposure*, *proximity* and *capable guardianship* in order to maximize the net benefits. For instance, people that are highly afraid of potential offenses (such as older people), which could translate into very high risk aversion, would decide to exhibit very low *exposure*, for example, by staying mostly at home and avoiding going out at night, or to increase *guardianship* by taking higher physical measures of protection. On the other hand, people that value enjoyment by much more than safety (such as younger people), which could be related to lower risk aversion, would disregard many potential dangers and exhibit high *exposure* and *attractiveness* for the sake of amusement.

In the second stage, once the opportunity criminal structure is set by the determination of *proximity*, *exposure*, *attractiveness* and *guardianship*, potential criminals come into play. Each of the four risk factors can be translated into costs and benefits for the offender. For instance, a highly attractive person or household would result in higher utility for the offender, a well protected house increases the uncertainty of success of the criminal action and therefore increases costs, a household of high *exposure* decreases uncertainty and therefore, decreases costs, and so on. Consequently, potential criminals, comparing their legal and illegal opportunities and taking into account their criminal ability and risks they are willing to take, decide whether to commit crimes and consequently which targets to hit in order to maximize their expected utility. Of course, the whole procedure is more complicated since potential victims cannot perfectly observe the actual risks of victimization for each

[1] For instance, a burglary cannot be avoided with certainty even if the potential victim is very cautious.

strategy they follow, and in a similar manner in the second period the four risk factors are not perfectly observed by the potential criminal. Moreover, this model also ignores the possibility that potential victims can at the same time be potential offenders. Nevertheless, this simple form together with the socio-criminological views could give some predictions on the immigrant-victimization relationship.

We need to emphasize that all ascribed or acquired attributes, such as age, gender and race, or education, income, family and employment conditions, respectively, are associated with victimization likelihood through their effects on the described risk factors. For example, males generally prefer to socialize more frequently in dangerous places and they exhibit a more aggressive behaviour relatively to females. Therefore, they would decide to be more exposed to criminal activities, which makes them more likely to become victims of violence. However, the situation is very different for domestic crime. Males within a family are victimized to a lesser degree because they exhibit higher *guardianship*. Moreover, the effect of some other attributes is ambiguous as they affect victimization risk through two or more risk factors. For instance, more affluent households are associated with both higher or lower risk of a burglary, since high household income may indicate a better protected house (more *capable guardianship*) or a very attractive target (since there are many valuable objects both in the house and outside the house).

3.2.1 The Immigration-Victimization Link

Immigration status (at least for the purposes of the empirical analysis) can be considered as an attribute ascribed to an individual.[1] Although immigrant population is rather heterogeneous, immigrants share some common characteristics. In Table 3.3 some descriptive statistics from the BCS 2007-08, by immigration status, can be found. From this table it is clear that immigrants are relatively younger, more from ethnic minorities and relatively more married. It is also clear that they are more unemployed and there is evidence that they are on average poorer and face lower legal opportunities relative to natives (see, for example, Algan at al, 2010). Given all the above characteristics, and assuming that labour outcomes enter the problem exogenously, immigrants evaluate their initial levels of *attrac-*

[1] Thus, we consider immigrants' behaviour after the decision to migrate.

tiveness, *exposure*, *proximity* and *guardianship*, as all these exogenous attributes are to some extent associated with these four risk factors.[1] Consequently, they reevaluate their position by following strategies that minimize the victimization risks for each crime group given all the aforementioned constraints.

For instance, location, and consequently *proximity*, is constrained by the labour outcomes of immigrants. As we can see from the descriptive statistics immigrants are disproportionately located in deprived inner city areas, mostly of London. This could be the consequence of the following reason. As immigrants face unfavorable labour outcomes they can only afford to reside in areas where the rents are relatively low. It happens that these areas are relatively more deprived with high crime rates and therefore, of higher *proximity*. Nevertheless, given the above constraint, immigrants reduce the risk of both personal and household victimization by choosing to reside (within these areas of high *proximity*) in locations with high concentration of the same ethnic group. This develops a type of natural protection, or provides a higher insurance against risk of victimization by increasing *social guardianship*. At the same time, household *physical guardianship* is also constrained by their labour outcomes, as they could not afford means of high protection. Residing in the aforementioned areas performs as a natural *social guardianship* that intends to balance the lower *physical guardianship*.

Moreover, as immigrants disproportionately belong to ethnic minority groups they are in higher danger of racially motivated violence, since they are relatively more attractive to extremist groups. Therefore, they might choose to balance this unfavorable position by choosing *routine activities* and lifestyle *exposure* associated with lower victimization (and therefore, by reducing *exposure*). In addition, a proportion of immigrants might feel alienated and react in this perceived hostile environment by following strategies that reduces the risk of victimization. Finally, immigrants could naturally exhibit different *exposure*, because of cultural differences that are associated with different lifestyles.

As mentioned in the introduction, violence consists of three crime types of very different nature, namely *crime by strangers*, *crime by acquaintances*, and *domestic crime*. Theoretical

[1] For example, younger people prefer to have a social life associated with higher *exposure*. Married people on the contrary follow lifestyles associated with lower *exposure*.

predictions on the association between immigration status and domestic crime or crime by acquaintances can be given by immigrants' relative participation in the illegal sector as offenders. For instance, according to the "homogamy" principle immigrants tend to socialize with other immigrants of the same ethnic group and therefore, a large proportion of immigrants' acquaintances or family members are immigrants as well. If we accept that immigrants, according to Papadopoulos (2010b), are slightly less likely to commit violent crimes, holding everything else constant, we would expect a negative relationship between being an immigrant and violent crime suffered by acquaintances or family members.[1]

However, a negative relationship could be also observed because of "network effects", a concept closely related to *exposure*. For instance, crime suffered by acquaintances could be lower for more recent immigrants due to the fact that more recent immigrants know fewer people. Therefore, the "pool" of acquaintances is larger for natives or earlier immigrants. According to this, we could expect that as time spent in the host country increases, immigrants enlarge their group of acquaintances, and therefore, to some extent they assimilate to natives' risk of victimization by acquaintances.

As it is clear from the discussion of this section, the unobserved interactions and interrelations among people are relevant for violent crime, but not for household burglaries and personal thefts. Household burglaries more or less depend on observed household characteristics. The fact that the household reference person is an immigrant should not affect the risk of victimization, given that we are able to control for all household characteristics associated with burglary victimization.[2] The only unobserved (by the author) characteristic that might be important to describe instrumental victimization risks is the size of potential victims' possessions (apart from the number of vehicles which is observed).[3] Fortunately, the BCS provides a rich set of household characteristics directly associated with *lifestyle-exposure*

[1] As an example, consider the following simple calculation. Assume that the probability to commit a crime is 6% and 10% for an immigrant and a native respectively. Also, assume that 5% of natives' acquaintances are immigrants, but 60% of immigrants' acquaintances are immigrants. According to these assumptions, holding everything else constant, the probability for an immigrant to suffer a crime by an acquaintance is $6\% \times 0.60 + 10\% \times 0.40 = 7.6\%$, but this figure is 9.8% for natives, so that the difference is 2.2 percentage points.

[2] Unless criminals seek places that are inhabited by immigrants or criminals have information about the immigration status of residents and tend to prefer targeting these places. However, for a household crime it is the instrument that is much more important than the person who owns it or resides in it.

[3] This can be considered as more important for properties outside the dwelling as they are directly observed by potential criminals, as opposed to interior properties.

and *routine activities*, such as hours home left unoccupied, being in a neighbourhood watching program, house condition, type, location, etc (see, next section). he situation of personal thefts is a bit more complicated due to the fact that it entails personal contact and thus, the potential criminal can directly observe the potential victim. However, as for burglaries, personal theft is in a sense more "random" in the sense that personal behavior is not an important predictor of the action.

Nevertheless, for violent crime, the risk of victimization highly depends on the unobserved strategies associated with particular *lifestyle-exposure* and *routine activities* that immigrants set in order to reduce the victimization costs. As described above, *lifestyle-exposure* and *routine activities* might be very different between immigrant and native groups and therefore, the theory cannot provide a clear-cut relationship. This should instead be established by the empirical analysis. Hopefully, the empirical analysis would also provide many insights on the reasons behind the observed immigrants-natives violent victimization differentials.

In addition, we need to recognize that immigrant population is highly heterogeneous and the different groups of immigrants (for example, according with their ethnic background, or the time spent in the UK) might be associated with different unobserved victimization-prone factors. This subject will be examined in Section 3.7. Finally, for some reasons explained in Section 3.8, repeated victimization may be different between immigrants and natives. Count data models will provide insights on this relationship.

3.3 BCS, Dependent Variables and Descriptive Statistics

In the first subsection of Section 3.3 a brief description of the British Crime Survey together with some important issues concerning the construction of the dependent variables is presented. A description of the data together with some descriptive statistics follow in the second subsection.

3.3.1 The British Crime Survey and Dependent Variables

The British Crime Survey 2007-08 (BCS), carried out by the Home Office, is a representative (primarily) victimization survey where respondents in England and Wales were asked in face-to-face interviews about their victimization experiences in both household and personal crime. As will be described later, the BCS also includes computer-based self-completed interviews for the more sensitive crimes. such as domestic violence and sexually motivated offences. Moreover, it does not interview people from Scotland and Northern Ireland as they now conduct separate surveys. The reference period for all interviews refers to the victimization incidents during the last 12 months prior to the date of the interview. It is one of the largest social surveys in England and Wales as it interviews approximately 47,000 respondents per year.

This survey is ideal to identify determinants of victimization since, together with information on victimization experiences, a large set of demographic characteristics together with information on household and personal characteristics associated with victimization are available. Note that, since the BCS interviews only private households, it does not cover commercial victimization, frauds and victimless crime, crime against children, crime against people currently in institutions, and murders (for details of the BCS refer to Bolling, Grant, and Donovan, 2008, I).

For the purposes of this study, information from three separate files of the BCS 2007-08 was combined using the unique identifier variable from the three data sets. These files are: 1) the Main BCS data set, where information for all respondents and their households regardless of their victimization experiences is included, 2) the Victimization Form data set, in which details of each crime reported by victims are given, and 3) the Self Completion data set of domestic violence, where all people between 16-59 years old, by participating in computer-based self-reported interviews, provided information on their experiences of domestic violence.[1]

These three data sets were constructed by using a complicated procedure whose main steps are briefly described as follows: interviewees, after giving some information on de-

[1] There is evidence that respondents under-report by less in computer-based self-reports (see, for example, Turner at al, 1998). Therefore, as mentioned in the introduction, the purpose of using this information is to check whether immigrants under-report violent crime by more or less than natives.

mographic and other individual and household characteristics, were asked a list of screener questions about whether they suffered any type of victimization incidents during the last 12 months (against them or against their household). In case the respondent reported a suffered crime, a victim form was given for each crime suffered. The victim forms assigned to each individual were limited to six. Each victimization form contained detailed information on the crime incident. This information was next used by trained coders to assign either a valid or an invalid victimization code.[1] The cases in which the conductor was uncertain about the offence code to be assigned were sent to Home Office to be crosschecked by Home Office experts. There, a finalized code was assigned. If a particular crime in a given victim form was described as a "series" crime, where a series crime is defined as "the same thing, done under the same circumstances and probably by the same people", the number of the incidents was recorded. The classification of crime codes is depicted in Table 3.1.

It is important to note that some incidents included a sequence of crime events which might have been of different nature. For instance, we could imagine a case where a stranger broke into a house to steal valuables but during the act of burglary the victim tried to prevent the incident resulting in suffering an assault with serious wounding. Eventually, the offender also burned the house. This incident (which is of course extreme and not very likely to have happened) involves three separate crimes but it will be recorded as arson because arson takes priority over burglaries and serious wounding. In similar cases the final coding depends on the seriousness of the incident. For details on the coding and which crimes take priority over other ones refer to Bolling, Grant and Donovan (2008, II).

The dependent variables used in this study were created from the offence code variable given in the Victim Forms data set (see, Table 3.1). As the question of interest in this study is to identify whether immigrants are more or less likely to be victims of criminal activity (and in extension whether immigrants are more frequently victimized than natives) a grouping of the individual codes was required. Otherwise there was not enough variation in the dependent variable to give precise and robust estimates for the coefficients of the

[1] The incidents are given invalid codes if the offence was a duplicate, if the offender was described as mentally ill, if the offender was a police member on duty, and if incidents that initially were given a victim form decided to be coded as no crimes after a scrutinized examination. Note that incidents outside England and Wales were given a valid code.

models. Seven main groups were constructed according to the nature of each crime code (as judged by the author being of the same nature), five of them for household crime and two for personal crime. For household crime these are: *Inside Burglaries* (codes 51-53), *Outside Burglaries* (codes 50, 57, 58), *Vehicle Thefts* (codes 60-64, 71, 72), *Household Thefts* (codes 55, 56, 65-67, 73)[1] and *Vandalism* (codes 80-86).[2] Regarding personal crime, these are: *Personal Theft* (codes 41-45) and *Personal Violence* (11-13, 21).[3]

For all constructed variables I use both the binary information, which is used in the first part of the empirical analysis (Sections 3.4-3.7), and the count form (number of crimes suffered), which is used in the second part (Section 3.8). For each crime group, the binary dependent variable is just a dummy that takes the value one if the individual reported a victimization of that crime group in at least one victim form and zero otherwise. The count variables are created by using the "series" information from the victim forms. For example, if an assault with wounding was considered as a "series" crime, the number of assaults forms the count variable. Moreover, if the same individual suffered another assault, for instance without injury, the number of assaults from this victim form, as indicated by the "series" information, are added to the previous count.[4] Finally, note that it is possible two victimization forms to be assigned by the conductor for two very similar crimes, which even belong to the same code, if some characteristics of the first (series of) incident(s) are considered by the coders to be different from the second (series of) incident(s).[5]

As mentioned in the introduction, the personal violence variable is the mix of three crimes

[1]Separation between inside, outside and other thefts was also considered.

[2]A separation between home vandalism and vehicle vandalism was considered to be interesting.

[3]For details on the crimes that each individual code included refer to the Offence Coding Coders Manual in Bolling, Grant, and Donovan (2008, II).

[4]We need to note that the main data provides derived crime variables which are used by the Home Office to calculate prevalence and incidence rates. However, for each crime code in these variables a cap of five crimes is imposed. Therefore, the total count for a crime group, say violent crime, will be the sum of crimes from each victimization form that fall within this crime group, where the number of crimes in each victim form is censored in five crimes. Thus, the resulting count variable will be the sum of up to six censored at five crimes. According to this, it is not proper to use a simple right censored at 30 crimes count data model but a model that allows for censoring at 5 crimes for each victimization form someone gets. This of course will result in a very complicated situation. Moreover, these derived variables do not include cases where the coder was uncertain what code to assign.

[5]For instance, consider a case where a victim suffered 15 assaults without injury (1^{st} victimization form) and 5 assaults again without injury (2^{nd} victimization form). The difference between these two series of crimes is that, for instance, the first series of assaults were committed by an acquaintance whereas the second series by a partner. Therefore, although these two crimes at the end take the same code (number 13), two different victimization forms are assigned. To construct the count of assault without injury for this individual we need to sum the count from the 1^{st} victimization form and the count from the 2^{nd} victimization form.

of very different nature. Crime suffered by strangers, crime suffered by acquaintances and domestic crime. Since these three crimes are different in many dimensions it is more proper to treat them as three separate crime categories. Fortunately, this information is also given in the Victim Forms data set and three separate dummies or count variables can be created.[1] This will be well discussed in Section 3.5.

However, another question is raised. Is it appropriate to treat these three types as being independent from each other (and therefore, model them as three independent equations)? For instance, when we consider crime by acquaintances, is it appropriate to consider an individual who suffered a domestic crime but not a crime by acquaintances as being the same with an individual who did not suffer crimes at all? It might be more proper to take into account the fact that people who suffered a violent crime of one type may share common unobserved characteristics with people that suffered a violent crime of another type. Allowing for these unobserved factors to be correlated might result in efficiency gains. This will be further discussed in Section 3.6.

Finally, for each (series of) crime event(s) the information whether it is (they are) perceived as a racially motivated crime, together with the reason why it is (they are) perceived as such, is available. Therefore, (perceived) racially motivated crime can be controlled for.

3.3.2 Description of the Data

To begin with, although in the empirical analysis I focus on burglaries, personal thefts and violent crime, the distribution of the count form of all dependent variables is presented in Table 3.2. However, the full distribution of the violent crime variables is presented separately in Table 3.22. There are two main issues that deserve a brief discussion. Firstly, the number of zeroes is very large for most of the variables. Thus, for some variables it is hard to obtain precise estimates because of the low variation in the dependent variable, particularly for count data models which are not very robust when the presence of zeroes is very high. Secondly, there are few cases of victims that reported extreme number of crimes. For instance, in variable *Personal Theft* there is only one person above ten crimes, who actually reported

[1] The 'do' files (Stata® format) for the creation of dependent variables from the Victim Forms data set are available from the author upon request.

97 crimes, or, for *Inside Burglary* there are eight people that reported between 70 and 100 crimes. In this table for ease of exposition we cap the crime count at ten plus more. Count data models are very sensitive to these cases, particularly when the positive counts are too few to identify the parameters assumed to affect the conditional mean, and when the extreme cases are very dispersed from the less extreme cases. Someone would think of dropping these cases because they could be considered as highly unreliable. However, this practice would result in sample selection issues. Therefore, as will be discussed in Section 3.8, we also use several modified count data models that are both (in a sense) more robust under these cases and more appropriate to explain the observed distribution of victimization incidents. Finally, it is also clear that the dispersion of most variables is very high. Therefore, the Negative Binomial distribution that allows for over-dispersion may be more appropriate to fit the observed data.

Moreover, descriptive statistics of the dependent and independent variables are presented in Table 3.3. The mean for native and immigrant groups for all variables is also given in order to have a first indication on the victimization differences between immigrants and natives. In addition, we will be able to observe the aspects in which immigrants differ from natives with regard to their observed characteristics. It must be noted that, the immigration status variable is created as a dummy that takes the value 1 if the respondent or the house reference person is not born in the UK. Moreover, the information of how many years the respondent lives in the UK can be exploited to examine assimilation patterns of the immigration-native victimization differentials. This will be examined in Section 3.6.

A first look at the raw data shows that there are victimization differences between immigrant and native groups, although they are very small in most cases. Regarding acquisitive crime, both household and personal, we can see that the probability and the mean victimization are higher for immigrants, apart from *Outside Burglary* (and *Outside Thefts* or *Other Thefts*).[1] Moreover, *Home Criminal Damage* is slightly lower but *Vehicle Criminal Damage* is slightly higher for immigrants. Concerning *Violent Crime*, which is the crime group most discussed in this study, we can see that immigrants are less victimized. However, the picture

[1]Here I do not discuss statistical significance of the differences as these descriptive statistics are used just as a first indication.

is different if we break violence into the categories discussed before, as immigrants are much less victimized by acquaintances and family members, but slightly more by strangers.

In addition, in Table 3.3 the independent variables which will be used in the main analysis are also presented. Again, the mean for both immigrants and natives is given. Note that the means for the respondent's and the household reference person's characteristic are separately given. This is because the appropriate variables in personal crime are the personal characteristics, but in household crime it is the household characteristics. The main observed differences between immigrants and natives is that immigrants are younger (which can be considered mainly as a measure of *exposure*) and that they are relatively more concentrated in London, urban and inner city areas, but most importantly that they reside in relatively more deprived areas (which can be thought as *proximity* measures).[1] Thus, a first question in the main analysis would be: what would be the immigrant-native differences in the likelihood to suffer a crime if immigrants displayed the same basic demographic characteristics?

Moreover, immigrants are more married, more of nonwhite ethnic groups, more renters and they reside relatively more in flats (mainly *exposure* measures). They also live fewer years at their current home or area (which is a measure of *social guardianship*) and finally, they possess fewer cars (measure of *exposure*). There are no strong differences in income and education. Hence, another question would be: if there still are differences, can they be explained by the remaining observed individual and household characteristics?

Finally, notice that for some of the independent variables there are many missing cases. Dropping all these cases would result in losing too much information. Therefore, a dummy is created for each variable that contains a considerable number of missing cases that takes the value one if the particular variable displays a missing value and zero otherwise. Thus,

[1]The *Deprivation Index* is the "Multiple Deprivation Index of England and Wales" for 2007, constructed as a weighted mixture of the individual deprivation indices (Income deprivation, Employment deprivation, Health deprivation and disability, Education, skills and training deprivation, Barriers to housing and services, Living environment deprivation, and Crime deprivation index) provided by the Department of Communities and Local Governments for England and Welsh Assembly Government for Welsh. Very briefly, this index, that takes integer values from 1 to 10, provides a measure of multiple deprivation at the Lower Super Output Areas (LSOAs) level by considering some indicators of deprivation. These values indicate the decile of deprivation in which someone scores. For example, if someone scores at the 7th decile, only 30% of the population resides in more deprived areas. Each respondent, depending on the small level area that he/she resides, is matched by the Home Office with the corresponding decile of this variable. For more information on these indices refer to Noble at al (2008). In the empirical analysis I include this variable as an 1 - 10 integer index that measures the effect of scoring at a one decile higher on the probability of victimization.

these dummies intend to absorb the effects of the missing cases of each characteristic on the dependent variables.

In a summary of this subsection, we saw that immigrants suffer in general slightly more property crime and personal theft (apart from outside thefts and home criminal damage) but less violent crime than natives, although they live in more deprived inner city neighbourhoods where violent crime is much higher. However this picture changes if we distinguish crime by strangers from crime by acquaintances and family members. More on these relationships will be discussed in the next two sections.

3.4 Risk of Household Crime

In this section simple Probit results for household crime are presented.[1] As discussed in the previous section household crime was separated in five mutually exclusive groups. However, here mainly the results of *Inside Burglaries plus Attempts* and *Outside Burglaries plus Attempts* are presented. The results of the other variables are briefly discussed in the second subsection. Full results are available from the author on request. The regressors believed to affect the conditional expectation of the dependent variables are assumed to be the same for both crime groups.[2]

In the results that follow four specifications of the conditional mean are presented. In specification 1 the effect of the household reference person (hrp) being an immigrant on the likelihood of victimization is considered without taking into account that immigrants differ from natives in many dimensions. In specification 2 some important *proximity* measures are controlled for. In specification 3 some important characteristics of the hrp are also included, which are thought in literature to be associated mostly with the risk factor of *exposure*. Finally, in specification 4 some extra important household characteristics that are theoretically associated with *exposure*, *attractiveness* and *guardianship* are used.

[1] All the empirical results in this study are obtained using Stata® and TSP® econometrics software.

[2] Thus, we assume that the factors that affect the criminal opportunity structure through their effects on the four risk factors are generally the same for the two crime variables.

3.4.1 Inside Burglary

Before discussing the results we need to note that 81% of the *Inside Burglaries plus Attempts* incidents the victim did not know the offender, whereas only 10% of the cases happened because of preexisted personal relationship/history between the victim and the offender. Therefore, although there are a few cases where interrelations and interaction between victim-offender matter, inside burglary can be considered in a high degree as "random" where criminals solely target the property without interest in the household composition and without intentions to victimize household members. Thus, we can assume that offenders target specific dwellings not because of the composition of the residents, but because these specific dwellings exhibit characteristics associated with higher risk of inside burglary victimization. Moreover, notice that most of the times, criminals' information about interior properties is limited, so that the value of the interior properties would not be a large factor for the risk of victimization. Instead, *attractiveness* is approximated by the external household characteristics.

According to the above, we would expect that if a relationship between immigration and inside burglaries exists, it is not because criminals prefer targeting immigrants' properties, but because immigrants' household characteristics are associated with more or less victimization, as discussed in Section 3.2. These characteristics refer to both direct household characteristics such as location and external condition, and indirect characteristics associated with the four risk factors, such as household reference person's age, marital status, or how many hours the house is left unoccupied. Therefore, we would expect that this association would fade out if we were able to control for the characteristics that make immigrants' properties subject to higher or lower victimization.

The Probit results are presented in Table 3.4. First of all, we can see that the likelihood of victimization increases if the hrp is an immigrant. The marginal effect is 0.74 percentage points (which is statistically significant at 1% significance level) which is fairly large in magnitude if we bear in mind that the probability to suffer an inside burglary is 2.99% for immigrants and 2.25% for natives, a relative effect of 33%. Note that the result is almost identical if we control for respondent's immigration status rather than hrp's immigration status. This was expected because according to the "homogamy" principle it is highly possible that if the respondent is an immigrant the hrp is also an immigrant.[1]

Hence, dwellings in which the hrp is an immigrant are disproportionately victimized. However, a major part of this difference can be explained by the fact that immigrant disproportionately reside in urban areas where the deprivation index is much higher, two factors that are highly associated with the risk of inside burglary.[2] Moreover, from specification 3 it is clear that the rest of the difference is explained by hrp's basic characteristics indirectly associated with *exposure*, *attractiveness*, and *capable guardianship*. The association even becomes negative if we include the extra controls of the fourth specification. It is important to note that, as the research question mainly concerns the immigration-native victimization differentials, discussion of the effects of the other variables will not be given in the main text but as a note for each different crime group.[3]

3.4.2 Outside Burglary

Outside Burglaries plus Attempts (burglaries of non-connected domestic garage/outhouse) are considered separately due to the following two reasons. Firstly, as immigrants disproportionately reside in flats or maisonettes, they probably possess fewer outside properties, such as non-connected to the main house garages, outhouses, storehouses and conservatories.[4] Therefore, controlling for other characteristics, the risk of outside burglary is expected to still be lower for immigrants. Unfortunately, information of outside properties is not given in the BCS. Secondly, outside properties can be considered as "safer" targets because of lower

[1] The tetrachoric correlation coefficient (see, Edwards and Edwards, 1984) is 0.9841.

[2] The marginal effect decreases to 0.29 percentage points and it is statistically insignificant.

[3] We can see that if the hrp is older, married, employed and owner, the victimization risk falls. However, the gender of the hrp does not affect risk of victimization. For the rest of the coefficients in specification 4 we have the following relationships: as the perceived condition of the house increases, risk of victimization also increases. Also, condition of the dwelling relative to the other dwellings in the neighbourhood is important as both better and worse condition houses are of higher risk of victimization. Moreover, detached houses, and properties located on main or the side of the road are associated with more crime. Number of adults in the house and hours that the house is left unoccupied have no effect. On the other hand, if the respondent is a lone parent the risk of victimization increases. The longer the respondent resides in the same house the lower the likelihood of an inside burglary. In addition, if the property is in a neighbourhood Watching Program the risk of victimization decreases (significant at 10%). The joint effect of income dummies, having less than 10,000 pounds of annual income as the reference group, is significant at 1% with 50+ group being the only group associated with more crime than the base group (significant at 10%). Finally, education dummies are jointly significant at 10%, with more crime for higher educated people.

[4] We need to stress that theft of outside properties and car thefts are not included in outside burglaries but they are treated separately.

physical and *social guardianship*. Using the Victim Forms data set we can see that in 96% of the cases the criminal was a stranger[1] and that in 99% of the cases the incident could not be attributed to previous personal history or relationship. Hence, the same argument in favor of "randomness" used for *Inside Burglaries plus Attempts* holds here as well. Finally, notice that for this crime category we observe very few positives (99% of zeroes).

The results are depicted in Table 3.5. In spite of the fact that the variation of the dependent variable is very low, Table 3.5 shows that the immigration coefficient is very robust across all specifications. We see that the likelihood of victimization is lower for immigrant households and statistically significant at 5% regardless of the control variables. To evaluate the magnitude of this difference marginal effects are calculated. For example, for specification 2, evaluated for a household that is located in an average deprived area, in the inner city of an urban area in London, the probability of an *Outside Burglary plus Attempt* is around 0.3 percentage points lower for households in which the hrp is an immigrant, with a relative effect of around 60%.

Thus, even though immigrants live in relatively more deprived areas, they face a much lower probability of victimization. This may be attributed, as mentioned before, to the fact that immigrants possess fewer domestic outside properties. Unfortunately, there is no information on non-connected domestic outside properties and therefore, we are not able to test the above argument. However, a zero-inflation (ZI) count data model could be relevant in this case (see, Mullahy, 1986, and Lambert, 1992). According to the ZI model some households will never experience an outside burglary just because they do not own any outside properties. It is interesting that, in accordance with this previous argument, ZI models for counts show that the immigration status coefficient is positive in the inflation equation and significant at least at 10% significance level in most specifications. A zero-inflated Probit model was also employed, whose log-likelihood function resembles the log-likelihood of the MisProbit model presented in Papadopoulos (2010b) if the one inflation probability is constraint to be equal to 0. Although the behaviour of this model in terms of estimation was not trustworthy, its results also indicate that the proportion of immigrants

[1] Of course, this might be because in most of outside burglaries it is highly likely that the victim had no contact with the offender, and therefore, could not be able to evaluate whether he/she knew the offender.

in the zero inflation category is more than the proportion of natives and significant at 5% level of significance. All results are available from the author on request.

Moreover, we could think that earlier immigrants are better settled and therefore, their outside properties would be more similar to natives' ones. Thus, we expect to observe a lower risk of outside burglaries for earlier immigrant with an assimilation pattern as the number of years in the country increases. Unfortunately, *Number of Years in the Country* is not provided for the hrp, but we could approximate it with respondent's *Number of Years in the Country* since, as in the previous section, using the variable *Immigrant* instead of *Hrp Immigrant* the results were identical. The results, which are presented in the first two rows of the second part of Table 3.5, are quite supportive of the above argument.[1] We can see that when we include the linear trend for the number of years of an immigrant in the host country, more recent immigrants are associated with a much lower probability of victimization (even lower than before) and that this probability converges to natives' one as years in the country increase (although the marginal effects show that it takes more than 40 years for immigrants to assimilate to natives' probability of outside burglary victimization). Note that the "assimilation" coefficient is insignificant in specifications 1 and 2 because we do not control for age, as immigrants that are more years in the country are relatively older, and older people are associated with lower victimization risks. Once we control for age, the coefficient of immigration dummy increases in magnitude and the "assimilation" coefficient becomes significant at 5% significance level. Finally, we need to note that most regressors have an insignificant effect on the probability to suffer an outside burglary.[2]

3.4.3 Remaining Household Crime Groups

In this subsection the main results of the association between immigration and the risk of victimization for *Vehicle Thefts*, *Household Thefts* and *Criminal Damage* are briefly discussed.

[1] Here, only the coefficients of interest are presented. Full results are available upon request.

[2] For the coefficients in specification 4 we have the following relationships: only relative condition affects victimization, as the better the condition relatively to other houses, the higher the risk of victimization. There is no effect for worse condition. The dummies for the type of the house have no joint effect. Being located in a main road increases the risk of victimization but being in a side road does not affect it. *Number of Adults* has no effect as well. On the contrary, as for inside burglary, lone parents' households experience higher risk. Moreover, there is no effect for, *Hours Unoccupied*, *Years at Home* and *Years in Area*, *neighbourhood Watching Program* and income dummies. Finally, education is jointly significant at 1%, with more crime for more educated people (more than a-levels).

The results are not presented but are available from the author on request.

To begin with, contrary to burglaries, *Vehicle Thefts* are much more often, as the probability of victimization in the raw data is 6.53%. Therefore, the estimates obtained for this crime group are much more precise. Once more, we expect that holding everything else constant, immigrants would experience a lower risk of vehicle thefts just because they own fewer vehicles. However, as opposed to outside burglary, in this case we have information on both the number of cars a household owns and on ownership of motorbikes and bicycles. The results show that immigrants face a higher risk of vehicle thefts (statistically significant at 1%) even though they own fewer vehicles, if we do not control for demographic disadvantages of immigrants. Thus, the coefficient of the effect of immigration status on vehicle crime increases once we control for this fact by including the natural logarithm of vehicles as a regressor and considering only the population that possesses vehicles.[1] However, as expected, if basic demographic differences between immigrants and natives are controlled for, the difference in the likelihood of victimization fades out.

Household Thefts consists of *Inside Thefts* (0.25% positives), *Outside Thefts* (2.62% positives), *Other Household Thefts* that do not fall within these two categories (1.76% positives) and *Attempted Thefts* (0.16% positives).[2] The results indicate that immigrants do not experience a higher risk of being victims of *Household Thefts* even thought they have some demographic disadvantages (the coefficient is 0.002 and very insignificant). Therefore, as we

[1]The reason why we include the number of vehicles in the natural logarithm form is the following: firstly, it is important to note that any binary choice model could be thought of as a censored at 1 crime count data model. For example, in the Poisson case, the probability of the zero outcome is $e^{-\lambda}$ and the probability of a positive is $1 - e^{-\lambda}$ where λ is the Poisson conditional mean. Thus, the structure of the conditional mean of the binary model should be consistent with the structure of the conditional mean of a count data model. As it is very common in count data models, in order to ensure nonnegativity we consider the mean to be given by $\lambda_i = e^{x_i'\beta}$. Moreover, it is natural to assume that the risk of suffering a vehicle crime is proportional to the number of vehicles someone possesses (in the same way we model cases where different individuals are exposed on the outcome y for a different time interval), since the number of vehicles can be considered as a direct measure of *exposure*. Thus, if N is the number of vehicles someone possesses, the mean in this particular case is given by $\frac{\lambda_i}{N} = e^{x_i'\beta} \Rightarrow \lambda_i = N \cdot e^{x_i'\beta}$. Therefore, the number of vehicles should be included in the regression framework as the ln of N, so that $\lambda_i = e^{x_i'\beta + \ln N}$. From the last expression it is clear that we cannot include the households with zero vehicles. Intuitively, considering only the population that possesses vehicles, we directly control for the zero-inflation probability which is the probability of not suffering vehicle crimes just because of no possession of any vehicles (No *exposure*).

[2]The differences between a burglary and a household theft are explained in detail in Bolling, Grant, and Donovan (2008). Very briefly, inside thefts consist of the cases where there was a theft by a person who was in the house with the consent of households members. Outside thefts consists of thefts of properties outside the house without any sign of outside burglary. Other household thefts include all other categories of household thefts excluding personal thefts.

control for demographic differences the coefficient becomes negative and significant at 5%. It has to be stressed that these results are driven by *Outside Thefts*, as it is the variable with the most positives. If we break household thefts in the three categories we observe the following: for *Inside Thefts* immigrant coefficient is always positive but insignificant in all specifications. For *Outside Thefts* it is negative but insignificant in specifications similar to 1 and 2 of Tables 3.4 and 3.5, but negative and significant at 10% if we include further controls. Finally, for *Other Thefts* it is positive and insignificant in specification 1, but negative and insignificant in specifications 2, 3 and 4. Thus, immigrants face a lower probability of *Household Thefts* probably because they do not own many outside properties, or because they are more capable of protecting and monitoring their outside properties.

Finally, the nature of *Criminal Damage* is very different, since vandalism is an expressive crime, as opposed to the other crimes discussed above which can be considered as acquisitive crimes. *Criminal Damage* includes *Home Criminal Damage* (2.48% positives), *Vehicle Criminal Damage* (5.37% positives), *Other Criminal Damage* (0.11% positives) and *Arson* (0.001% positives). The empirical analysis shows that, as for *Household Thefts*, although immigrants stay in disadvantageous areas, they experience the same risk of vandalism. Therefore, the coefficient of immigration status becomes negative and significant at 5% in specifications 3 and 4 (but not significant in specification 2). Further analysis shows that the previous effect is driven by the effect of immigration on *Home Criminal Damage*, as there is no relationship for *Vehicle Criminal Damage* (as it was the case for *Vehicle Theft*).

3.5 Risk of Personal Victimization

In this section the results of personal victimization are presented. First of all, personal victimization differs from household victimization in one essential element; it entails personal contact with the victim. Therefore, personal characteristics of the victim might directly affect the criminal action. The implications of this crucial difference on the immigration-victimization relationship can be quite important. This is mostly because, as potential offenders directly observe potential victims, they are able to approximately determine the ethnic background of the potential victim. Thus, the fact that someone is an immigrant might have an effect on the victimization probability even after controlling for a large set of observed individual characteristics, if there are still immigrants' characteristics associated with personal victimization that are observed by potential offenders but unobservered in the data. For instance, immigrants may appear as more vulnerable and therefore, they could be considered as an easier and safer target.

In addition, there is also a crucial difference between the two main personal crime types, *Personal Theft* and *Personal Violence*, which indicates that they should be treated separately. Personal theft is an instrumental type of crime whereas violent crime is an expressive type. Therefore, contrary to personal theft, as discussed in Section 3.2, a violent action in most cases requires personal interaction between the potential victim and the potential offender. This should not be translated as prior history in the victim-offender relationship, as there can still be interactions that generate a violent act even for individuals that were unknown to each other prior to the incident, such as brawls or arguments in pubs and bars. According to this, there might even be cases where the victim is at the same time an offender, which is unlikely for personal theft. On the other hand, personal theft is mainly "random".[1] The potential offender observes the potential victim and once a set of information is obtained, an evaluation of the expected utility follows. If the expected gains are higher that the expected costs the individual commits the crime.[2] In the first subsection the risk of personal theft is examined, whereas the analysis for violent crime follows in the second subsection.

3.5.1 Risk of Personal Theft

First of all, it is important to note that in the present study robberies are considered as personal thefts although they entail violence. I examine robbery in this category rather than in violent crimes because primary target of the offender is to acquire victim's valuables and not just to hurt the victim. *Personal Thefts* (1.59% positives) consists of *Robberies plus Attempts* (0.42% positives), *Snatch Thefts from the Person* (0.15% positives), *Other Thefts*

[1] Although the victim might sometimes consider himself/herself as responsible for the action (in the sample 6% of victims of personal theft considered themselves as responsible for the action), the responsibility is unintentional.

[2] For a formal model on the decision to commit property crimes see, Papadopoulos (2010b).

from the Person (0.73% positives), and *Other Attempted Personal Thefts* (1.39% positives). As described before, although personal theft can be considered mainly as "random", personal observed by the offender characteristics are still important for the final outcome as personal theft entails personal contact. An indicator for the "randomness" of personal theft is that, 94% of personal thefts were committed by strangers, 98.8% thefts did not happen because of prior history/relationship between the offender and the victim and from the cases where the victim consider himself/herself as responsible for the action (6% of the incidents) there is no incident where the victim provoked the offender.

Table 3.6 presents the results in four specifications. In the first specification the effect of being an immigrant on the risk of a personal theft is examined, without taking into account that immigrants differ from natives in some important characteristics. Predicted probabilities and marginal effects are also presented. Specification 1 shows that the probability of victimization is much higher for immigrants (61.2% higher). As shown in specification 2, this difference cannot be totally explained by immigrant-native differences in some important demographic characteristics (the relative effect of 34.9% is still very high).[1] Thus, even after controlling for the fact that immigrants are relatively younger and less white and that they disproportionately reside in deprived urban areas could not explain the difference in the risk of victimization.[2] However, the third specification reveals that immigrants are more likely to become victims of personal theft because they disproportionately reside in London, which is, according to the estimates, the place with the highest risk of personal theft. However, the coefficient still preserves its sign. If we consider that the variation of the dependent variable is very low (although the sample is quite large) we cannot ignore this relationship. From specification 4 we can see that even after controlling for other important characteristics associated with the risk of victimization, the coefficient still preserves its magnitude.

Hence, there are still some unobservables, specific to immigrants, that increase their risk of victimization. For instance, they might be considered by potential offenders as more vulnerable targets of lower risk.[3] Or, immigrants might follow some lifestyle activities as-

[1] The marginal effects are calculated for a white male, between the age of 36 and 45, who stays in an urban area where the deprivation index takes the average value.

[2] It seems that 'ethic group' matters for personal theft. Black individuals experience a higher risk, while Asians, Chinese and Others experience a lower risk.

[3] As an example, offenders might think that immigrants are not familiar with the criminal justice system,

sociated with higher crime, such as staying out relatively more than natives at the streets of crowed disadvantageous neighbourhoods where the risk of a theft is higher. However, we should finally stress that if no controls for ethnic group are included in specifications 3 and 4, the immigration coefficient goes very close to zero (0.003 with a p-value of 0.959). This is because immigrants are disproportionately from the *Asian, Chinese & Other* ethnic group, which faces much lower risk of victimization in the 3rd and 4th specifications. However, in the second specification, not adding ethnic dummies even increases the significance of the estimated immigration status coefficient (the value of the coefficient is 0.110 with a p-value of 0.024).[1]

3.5.2 Risk of Violence

Violent Crime (2.54% positives) includes *Assaults with Serious Wounding* (0.21% positives), *Assaults with Other Wounding* (0.55% positives), *Common Assaults* (1.6% positives), and *Attempted Assaults* (0.32% positives).[2] We need to stress that violent crimes with sexual motive and robberies are not included in this group. As discussed before, violence is an expressive type of crime where interrelations and interactions between potential victims and potential offenders are vital. As an indicator of this, the Victim Forms data set shows that in 23.03% of the victimization incidents the victim knew the offender casually, and in 34.44% he/she knew the offender very well. Moreover, in 27.34% of the cases the incident happened because of previous history/relationship between the victim and the offender. Finally, in 6.31% of the cases (81 incidents) the respondent considers himself/herself as being responsible for the action, while in the 65.43% of these 81 incidents there was provocation by the victim,

and consequently, that to some extent they do not know how to proceed after a personal theft against them takes place. This directly decreases the risk of apprehension for the offenders and thus, uncertainty.

[1]For the last specification, the effects of the variables whose estimates are not presented in the table are the following: education dummies are jointly significant at 1% (having no qualification as the baseline group), with more than a-levels people being the most victimized group. Income dummies are jointly significant as well, but the relationship is not very clear. People of the lowest income category (10,000 or less) face higher risk than the 10,000-20,000 income category. The group from 20,000-40,000 face lower risk but the effect is insignificant, while the group 40,000-50,000 experience more risk but the effect is again insignificant. Finally, the group 50,000 more experience higher risk but still insignificant. For the dummies of employment status (where employed people is the baseline dummy) and marital status (with married people being the baseline dummy), employed and married people face the lowest victimization risk. Finally, owners experience lower risk relative to renters.

[2]You can notice that adding up the 4 violent crime groups together you obtain a probability of victimization equal to 0.0268 which is higher than the probability to suffer a violent crime (0.0254). This is because it is possible that a person suffers more than one type of crimes.

which means that probably the victim initiated the action. As explained in Section 3.2 and in the introduction of this section, unobserved (in the data) characteristics associated with *routine activities* and *lifestyle-exposure* could be important on explaining remaining differentials in the immigration-victimization relationship.

The results for this crime category are presented in Table 3.7. Specification 1 shows that, without controls, immigrants face a lower risk of victimization but the difference is statistically insignificant. However, the marginal effect is significant at 10%.[1] As it is clear from specification 2, victimization decreases considerably with age, and since immigrants are relatively younger, controlling for age (using dummies) results in increasing the magnitude of the immigration status coefficient. Therefore, if immigrants faced the native age distribution they would experience a much lower risk of violence. The marginal effect shows that being an immigrant decreases the probability of a violent incident from 3.61% to 2.48%, a difference of 1.13 percentage points. According to the estimates of specification 3, the risk of victimization remains relatively the same, with marginal difference to be 1 percentage point, or around 43% lower for immigrants. Note that, this effect increases in magnitude if we do not include regional dummies. This is very interesting, because London is the place the residents of which go through the lowest risk of violent victimization, as opposed to personal theft, where London was the place with the highest risk of victimization. Finally, it is quite important that the effect of immigration preserves its magnitude even when we use some other observed characteristics associated with risks of violence.[2]

Furthermore, in Table 3.8 we present the results of the same specifications once we include dummies for ethnic background. As expected, inclusion of ethnic dummies affects the immigration status coefficient (which becomes more significant in specification 1, but

[1] The standard errors of the marginal effects are calculated using the delta method (command 'nlcom' in Stata®).

[2] The effect for the rest of the controls in specification 4 is the following: the education dummies (where baseline group is no qualification) are not jointly significant. However, it seems that the risk of victimization increases with higher education. Being married lowers the risk while being single has the highest risk. Unemployed individuals have higher risk than employed ones, while inactive individuals endure the same risk. Regarding income dummies effects (where the base is less than 10,000 pounds), all groups suffer lower violence than the poorest group, however, the statistical significance decreases as income increases. Finally the risk increases for lone parents and bigger households. Also note that the marginal effects are evaluated for the following representative individual: a male, between 35-44 years old, residing in an average deprived urban area in the East of England, who has a-levels qualifications, and also he is married, employed, owns the place he lives and finally belongs to a family with 2 household members.

less significant in specifications 2-4), since immigrants are disproportionately from ethnic minority groups. This can be also seen by the marginal effects.[1] Concerning the effect of the ethnic dummies, although it seems that Asians and to a smaller extent Blacks experience a lower risk of victimization relative to Whites, their joint effect is insignificant. Even in the last specification where both the effect of Asians and Blacks relative to Whites is significant at 5%, the Wald test fails to reject the null (the p-value from the Wald test is 0.123).[2]

Therefore, it seems that immigrants experience a lower risk of violent victimization because of some unobserved characteristics specific with this group. A general explanation for this could be that immigrants set strategies that correspond to unobserved differences in *routine activities* or *lifestyle-exposure* associated with lower criminal activity. For instance, immigrants may avoid socializing in places where there is a high risk of violence, such as pubs or clubs.[3] Or, as (according to Papadopoulos, 2010b) immigrants are less violent, they directly demonstrate a lower exposure in violent crime, since violence is strongly associated with the criminal behaviour of both potential victims and potential offenders. As evidence of this, we can see that according to the BCS Victim Forms immigrant victims exhibit a less provocative behaviour than native victims.[4] Moreover, a part of the estimated difference could be explained by the following hypothesis, also consistent with the results of Papadopoulos (2010b) and closely related to the one above. If we accept that immigrants socialize mostly with other immigrants, and if we also assume that immigrants socialize with

[1]The marginal effects are calculated for the same individual as before, plus the extra characteristic that he is white.

[2]In addition, we need to mention that there are two variables derived from the questions, "how often have you visited a pub in the last month" and "how often have you visited a club in the last month", which are asked by the conductors to be used as a proxy for *exposure*. However, this information can be considered as a poor measure of *exposure* if we are not able to control for day-life activities and other activities associated with more or less *exposure*. Thus, this regressor is measured with error for representing a *lifestyle-exposure*, which attenuates the immigration coefficient since there is a strong and statistically significant association between being and immigrants and going to pubs and clubs (being an immigrant decreases the probability of going to clubs or bars by around 18 percentage points, a relative effect of around 53%). Nevertheless, the coefficient of immigration status is still significant at 5% in specifications 2, 3 and 4. The only case that immigration coefficient turns insignificant is when both controls for going to the pub/club and ethnicity are used.

[3]According to the BCS data 35% of all immigrants, but 53% of all natives, have been to a pub or a bar during the month prior to the interview.

[4]From the 980 victimization incidents where the victim finds himself/herself as responsible for the incident, we observe that only 6.32% of immigrant victims provoked the offender, but 8.93% of native victims provoked the offender. Note that here I include all types of crime. If we consider violent crime only, these figures change to 50% for immigrants and 65.79% for natives, but note that there are only 4 violent crime incidents where an immigrant considered himself/herself as responsible for the incident.

the same number of people as natives do, the probability of violent victimization would be lower for immigrants just because immigrants are less violent.

However, as discussed in the introduction and in Section 3.3 this result may be misleading as violent crime is composed of three different types: *Domestic Crime*, *Crime by Acquaintances*, and *Crime by Strangers*. In the next subsections we investigate the immigration-victimization relationship once violence is decomposed into the three distinct crime types.

3.5.2.1 Domestic Crime

In the present study, *Domestic Crime* refers to inter-family antisocial behaviour. This also involves violence from ex family members such as ex partners. Note that the variation of this variable is very low, as only 0.51% of the respondents reported that they had experienced domestic violence.

The Probit results are presented in Table 3.9 in four specifications.[1] The coefficient of the marital status dummies are also presented as they seem very important in explaining variations in domestic crime. We can see that the likelihood of an immigrant being a victim of domestic violence is much lower in all specifications. Being an immigrant almost halves the probability of domestic violence.[2] Someone would argue that this is driven by the fact that some immigrants, particularly younger or more recent ones, leave their families back as they intend to work a few years and return back. However, from the distribution of the number of household members across families we can see that (even more recent) immigrants have actually more members in their families, even if we control for differences in age distribution.[3] Hence, it seems that families that consist of immigrants, perhaps because of cultural differences, exhibit family values associated with lower domestic crime.

[1]Ethnic dummies are not used for domestic crime, as they do not affect the probability of domestic crime even when we do not control for immigration.

[2]The marginal effects are evaluated for a female, between 36-45 years old, with all other characteristics the same as in the previous subsection.

[3]Actually, a Poisson regression of the number of household members on immigration dummy and a linear trend for the number of years in the country, and controlling for differences in immigrant-native age distribution (including a cubic on age), shows that being a very recent immigrant (who just entered the country) increases the mean number of family members from 2.28 to 2.39, a difference that is statistically significant at 1%. Moreover, being an extra year in the country adds 0.002 members in a family, which is also significant, but only at 5%. As expected, if we do not control for 'age', being an immigrant increases the size family by almost one person, but being an extra year in the country decreases the family size by 0.028 members. Both differences are very statistically significant.

However, it might also be the case that due to cultural differences immigrants might be less willing than natives to report inter-family violence.[1] This issue will be examined in the next section.

From Table 3.9 we can also see that men are less victimized than women as expected. In addition, it is noteworthy that divorced and separated individuals face the highest risk of victimization. Thus, women get victimized by ex partners during the 12 months prior to the interview, or victimized individuals tend to move forward incidents that happened long time ago, or married people for some reasons tend to under-report disproportionately. Finally, it is worth mentioning that the deprivation index is not associated with higher crime once we control for marital status.[2]

3.5.2.2 Crime by Acquaintances

Crime by Acquaintances refers to crime suffered by people who are familiar to the victim, but not family members. Only one percent of respondents suffered a crime by familiar people. As for domestic crime, prior history is also important for this type of crime. As an indication, in around 30% of *Crime by Acquaintances* prior history was responsible for the incidence and in 55% out of the 36 cases where victims consider themselves as responsible for the incident,[3] the victim provoked the offender.

The results, depicted in Table 3.10, are striking. From specification 2 we can see that natives are more than 100 percent more likely to suffer a crime by acquaintances, once we control for some basic demographics.[4] The immigration status coefficient preserves its significance and magnitude even under a rich set of controls for observed characteristics. In specification 4, where we also include controls for ethnic status (as now ethnicity dummies have a joint significant at 5% effect), immigration coefficient loses some of its significance

[1] If immigrant families are in a sense more "traditional" or more patriarchal, fear of reprisal could be higher for them, resulting in higher under-reporting.

[2] With regard to the effects of the other variables we have the following relationships: education dummies have no joint effect. Income dummies are jointly significant with poorest people being the group associated with the highest risk of victimization. Lone parent has a positive and significant effect even after controlling for marital status and number of household members. However, bigger households are not associated with higher or lower victimizations. The effect of regional dummies is significant at 5%, London being the region with the lowest risk of domestic victimization.

[3] The victim believed that he/she is responsible for the incident in 36 out of 507 cases (7.1%).

[4] The marginal effects are calculated for a person between 36-45 years old, and rest of characteristics the same as the individual in *Violent Crime* results.

and magnitude (as now being an immigrant decreases the probability of victimization by around 60%) as anticipated, but it is still significant at 10%, which is still important given the very few zeroes in the dependent variable (even though the data set is quite large).

This result is consistent with the findings of Papadopoulos (2010b). According to the "homogamy" principle, acquaintances of one ethnic group consist in a high proportion of people from the same ethnic group. Therefore, we expect that in a high proportion, immigrants' (natives') acquaintances are immigrants (natives) as well. Since immigrants are less prone to violent crime as offenders, we expect that immigrants would be less likely to suffer crimes by acquaintances relatively to natives. Moreover, if immigrants are less anti-social, following a less "criminal" behaviour, they would not initiate arguments and fights, but at the same time they would avoid socializing with "dangerous" people, or avoid being in places where they know that there is a person with whom a prior history existed. On the other hand, it could also be that immigrants are less likely to suffer a crime by acquaintances just because they have smaller networks of acquaintances ("network effects"), a feature directly associated with *exposure*. If this is true, we expect that assimilation patterns would exist, assuming that immigrants increase their networks of acquaintances as they stay longer in the country. This hypothesis will be examined in the next section. Finally, as for domestic crime, we cannot exclude the possibility that immigrants might be less willing than natives to report crimes that suffered by friends and other familiar individuals.[1]

3.5.2.3 Crime by Strangers

Crime by Strangers involves brawls in pubs and bars (31% of the cases), arguments and fights on the streets or in public transportation means, and so forth. In the data, 1.09% of respondents went through a victimization incident by a stranger. Although this crime can be considered as more "random" than crime by acquaintances and domestic crime, interactions between offenders and victims are still important. For instance, it is not very likely that someone will be attacked in the street without any reason, unless the primary

[1] The effects of the variables whose coefficients are not presented in Table 3.10 are as follows: regional dummies affect victimization significantly, London being the place with lowest victimization. Risk also increases for bigger households. The effect of income is significant as well, and the risk of victimization becomes smaller as income increases. On the other hand, education is jointly insignificant. Finally married people and owners face a lower risk of victimization by acquaintances.

target is to acquire victim's property which is, however, recorded as a robbery (or attempted robbery if offender's effort fails). According to our data, only in 17 out of 529 incidents the victim considered himself/herself as responsible for the action (2.26%), 9 of which the victim provoked the offender (52.94%).[1]

The results for this crime category are presented in Table 3.11. Contrary to the other two types of crime, immigrants are equally likely to suffer a crime by a stranger, even after controlling for disadvantageous characteristics of immigrants. Thus, the results of *Total Violence* were driven by *Domestic Crime* and *Crime by Acquaintances*. This is in contrast with the "anti-criminal" social behaviour of immigrants discussed in the previous subsection. We would expect to observe a similar pattern between being an immigrant and *Crime by Strangers*, and being an immigrant and *Crime by Acquaintances*, if immigrants do avoid criminal actions in general. The difference could be lower, since it is more likely that strangers are not immigrants themselves, and natives exhibit slightly more violent behaviour (according to Papadopoulos, 2010b).[2] Moreover, it would be lower because a few cases could be totally "random" (not depending on social interactions), so that the unobserved immigrants' behaviour associated with lower victimization would make no difference in these "random" cases. But still, we should have observed a negative, even insignificant, relationship.

Thus, this finding raises some important questions. Why do we observe a significant negative immigrant-victimization association for *Domestic Crime* and *Crime by Acquaintances*, but no association for *Crime by Strangers*? How can this difference be explained? Is it because immigrants under-report domestic crime and crime by acquaintances? Or, is the "randomness" of *Crime by Strangers* enough to close the gap in the probability of victimization between immigrants and natives? Nevertheless, there is another possible reason which

[1]Note also, that 222 of the crimes by strangers (41.9%) happened because the offender was under the influence of alcohol or drugs and 98% because of an attack by young people, teenagers or mindless vandalism.

[2]Following the simple calculation in subsection 3.2.1, assume again that the probability of committing a crime is 6% for an immigrant and 10% for a native. However, now assume that there is 10% probability for a native to interact with a stranger immigrant (which is about the proportion of immigrants in the UK) but there is 25% probability for an immigrant to interact with a stranger immigrant (since it is still more likely that an immigrant will interact with strangers from the same ethnic background due to concentration of immigrants in specific areas.) Thus, according to this simple example, holding everything else constant, the probability for an immigrant to be recipient of a crime by stranger is $6\% \times 0.25 + 10\% \times 0.75 = 9\%$, but $6\% \times 0.10 + 10\% \times 0.90 = 9.6\%$ for immigrants, a difference of 0.6 percentage points. However, this difference for crime by acquaintances was 2.2 percentage points.

is not considered yet. Holding everything else constant, immigrants are more likely to be victims of racially motivated crime compared to natives. This is because racially motivated crime is highly associated with ethnic minorities, and immigrants are disproportionately from ethnic minorities. Finally, could the "network effect" hypothesis explain some of this difference? These issues are examined in the next section.

3.6 Sensitivity Analysis

In this section a series of robustness checks in relation to the results found in the previous section for violent crime are presented. Initially, we compare the results of violent crime, found on the previous section, with a Trivariate Probit model that controls for the possibility of correlated unobserved factors across the three crime variables, *Domestic Crime*, *Crime by Acquaintances*, and *Crime by Strangers*. Next, we attempt to shed light on the reasons why we observed a significant difference on the immigrant-native victimization association for crime by acquaintances and domestic crime, but no difference for crime by strangers. Following two different approaches we will claim that this is not due to under-reporting of victimization incidents by immigrants. Moreover, we intend to show whether racially motivated crime can explain some of the observed differences between crime by strangers and crime by familiar people. Finally, we examine whether some of this difference can be explained by "network effects", by looking at assimilation patterns.

We need to stress that henceforth, we will be controlling only for the following basic demographic characteristics: *Gender, Age, Deprivation Index, Regions, Urban* and *Inner City*. Thus, all the following results look at the differences in the likelihood of victimization between natives and immigrants, if these two groups exhibited the same basic demographic characteristics.

3.6.1 Controlling for Correlated Errors

As mentioned in the end of subsection 3.3.1 it might not be appropriate to treat the three violent categories as independent from each other. It would be more proper to take into account the fact that people who suffered a violent crime of one group may share common unobserved characteristics with people that suffered a violent crime of another group. Thus, there might be individual unobservable factors common to the three crime groups that make some individuals more inclined to victimization than others. Accordingly, we could use a model that allows for correlated errors across the three crime groups, similar to the Seemingly Unrelated Regression framework (see, Parks, 1967). This can be done by using a Trivariate Probit model which might result in efficiency gains as it exploits the information that some sets of unobserved characteristics appear in all equations (see, Greene, 2008, for a formal representation of Bivariate and Multivariate Probit models).

A complexity here is that, although there are algorithms to evaluate univariate and bivariate normal integrals, these algorithms cannot evaluate M-variate normal integrals (see, Greene, 2008). On this direction, a simulation-based integration has been developed (see, Cappellari and Jenkins, 2003). Therefore, for the purposes of this analysis a simulated maximum likelihood three-equation Probit estimator that uses the Geweke-Hajivassiliou-Keane smooth recursive simulator is used (see, Terracol, 2002).[1] Obtaining estimates by using this estimator is time demanding and therefore, the number of draws selected is quite important. According to Cappellari and Jenkins (2003) this estimator is consistent when the number of draws and the sample size go to infinite. However, they find that a number of draws close to the square root of the sample size is a reasonable number to use. In my case, it is found that the estimated coefficients change very little if the number of draws is larger than 200.[2] The results in Table 3.12 are obtained using 300 draws. The results of this model are presented in Table 3.12.

We can see that the estimates of this model, both for the immigration coefficient and the coefficients of the other regressors, are very similar to the estimates when we treated the three crime group as independent. The only change is that the estimated coefficient of immigration status in the domestic equation loses a little of its magnitude. However, since this coefficient is more precisely estimated, its statistical significance remains the same.

It is also very interesting that we estimate a significant at 1% significance level positive

[1] To obtain these estimates the 'tribrobit' command in econometrics software Stata®, written by Antoine Terracol (2002) was used. A similar Stata® command that is generalized to account for a larger number of equations is written by Cappellari and Jenkins (2003).

[2] Only changes after the second decimal points of the estimates were observed. However, the estimated correlations between the error terms seem more sensitive to the number of draws selected.

correlation of the errors between *Domestic Crime* and *Crime by Acquaintances*, and between *Crime by Acquaintances* and *Crime by Strangers*, but no correlation between *Domestic Crime* and *Crime by Strangers*. This implies that there are common unobserved characteristics between victims of domestic and by acquaintances crime and different common unobserved factors between people that suffered crime by acquaintances and people that suffered crime by strangers. Moreover, we can see that the likelihood ratio test rejects the hypothesis that the three equations are independent.

However, as the estimated coefficients are very similar between the single equation Probits and multivariate Probits, and since this is a highly time consuming estimator, we keep presenting the results of the conventional Probit models. Alternatively, the estimated correlations of the errors suggest that (the much simpler in terms of time and numerical intensity) bivariate Probits between the two crime pairs could be used. However, even these results are very close to the ones obtained by conventional Probit models.[1]

3.6.2 Examining Differences in Reporting Behaviour

As discussed above, a reason why we observe a different pattern in the immigrant-native victimization differentials between crime suffered by strangers and crime suffered by familiar people might be that immigrants under-report by more than natives crime experiences by familiar people. Thus, the question is: is it that immigrants do not hesitate to report crimes suffered by strangers (and thus, observing no differences in the risk of victimization between the two groups) but hesitate to report crimes by acquaintances and (ex) family members? To explain the differences in the victimization patterns between crime groups we must be able to exclude the possibility of differential under-reporting between immigrants and natives. In the next two subsections, following two different strategies, we show that immigrants do not under-report, at least by more than natives. Firstly, we use self-reports on domestic violence and secondly, we exploit the available information on whether the partner was present during the face-to-face interviews. Both of them will provide important insights on differences on immigrants-natives reporting differentials.[2]

[1] These results are available from the author upon request.

[2] A third approach that uses two parametric models which are more appropriate under the presence of under-reporting was also followed for both binary and count data models. The binary model, which is based

3.6.2.1 Use of the Self-Completions on Domestic Violence

As mentioned in the introduction there is evidence that respondents under-report domestic crime (see, for example, Walby and Allen, 2004). Self-completions, as opposed to face-to-face interviews, are used as a technique to elicit more reliable responses to sensitive questions (see, Turner at al, 1998). For this purpose, people from 16 to 59 years of age were asked to self-complete a computer-based questionnaire for domestic crime. Therefore, a dummy *Self-Completed Domestic Crime* was constructed which takes the value one if the individual revealed (in the computer-based questionnaire) that he/she suffered a crime by any family member and zero otherwise. This variable consists of assaults and serious threats. Note that sexual abuse is not used. Regarding under-reporting the results are striking. Only 0.51% of the respondents reported a domestic crime in face-to-face, but 3.64% in self-completion interviews.[1]

Thus, given that under-reporting is much lower in self-completions for both immigrants and natives, if in face-to-face interviews immigrants under-report by more than natives, we would expect to observe a quite smaller immigrant-native victimization differential for *Self-Completed Domestic Crime* than for *Face-to-Face Domestic Crime*, as immigrants would now report more freely.

There is a small complication that does not allow us to directly use conventional Probit models though. This is because some individuals did not participate in the self-completion procedure, probably because they refused participation. Is this because the most victimized individuals are more reluctant to participate, or just because some people did not want to participate for unrelated to crime reasons, such as being older, language difficulties, and so forth? In addition, there is an extra complication. Some people who accepted participation,

on Hausman, Abrevaya and Scott-Morton (1998), is the model presented in Papadopoulos (2010b) under the name of MisProbit apart from the difference that the probability of over-reporting in the present study is set to zero. Note that this binary model shares the same likelihood function with the Detection Control Estimator of Feinstein (1990). References for the count data models include Papadopoulos (2010a), Papadopoulos and Santos Silva (2008), Winkelmann and Zimmermann (1993), Mukhopadhyay and Trivedi (1995), Cameron and Trivedi (1998) and Winkelmann (2008). The results of these models show that if anything, immigrants under-report by less than natives. However, these results were very unreliable, probably because of both the very low variation in the dependent variable and the noisy nature of victimization data. Thus, they are not presented in this study but they are available upon request.

[1] However, we must be cautious with this difference between self-reports and face-to-face interviews, as the questionnaires between these two different types of interviews and the whole procedure followed to construct the two data sets are very different (for details refer to, Bolling, Grant, and Donovan, 2008)

for some reasons asked for the help of the interviewer to complete this questionnaire. These people did not answer the crime questions of the self-completed questionnaire.

First of all, comparing immigrants and natives' participation rates we find that the probability of an immigrant to participate in the self-completion procedure is much lower than natives' one. 5.58% of natives between 16-59 years old did not participate compared to 13.98% of immigrants. Moreover, from people that accepted participation 13.06% of natives did not complete the relevant crime questions while the 21.46% of immigrants did not complete them. Thus, altogether, 32.44% of immigrants did not complete the self-questionnaire compared to 17.91% of natives. Therefore, if people that did not participate are the ones that are victimized the most, and given that respondents report more truthfully in self-reports, then the coefficient measuring the immigrant-native *Self-Completed Domestic Crime* differential would be downward biased.

Therefore, Sample Selection Probit models would be more appropriate if sample selection problem exists (see, Heckman, 1979). In this subsection I use the estimator proposed by Van de Ven and Van Praag (1981), which is a maximum likelihood modified Probit model that provides consistent and asymptotically efficient estimates if sample selection exists. Two different specifications for the Sample Selection Probit are considered. In the first one, we treat people that accepted participation, but did not answer the crime questions (because they asked from the interviewer to complete the supposedly self-completed questionnaire), as being the same with the ones that did not participate at all, and we use a sample selection model including in crime equation only people who self-completed crime questions. In the second one, we exclude people that initially rejected participation and we keep only the sample of people that accepted participation. In this model the selection process includes only individuals who accepted participation, whereas in the first case it includes all individuals between 16-59 years old. Using these models we can actually test whether sample selection is a problem. If there is no evidence of sample selection, we can use simple Probit models for the sample of completers only.

The results are depicted in Table 3.13.[1] As can be seen from this table four separate

[1] According to the results of the previous section, marital status dummies are very important factors of domestic violence. However, the results of these models, which are not presented here but are available on request, are very similar even when we include these dummies.

specifications are used. In the first specification we present the simple Probit estimates of face-to-face interviews for all respondents between 16-59 years old for the sake of comparisons. In the second specification a model that does not correct for sample selection for the sample of the individuals that contributed to the self-completions only is given. Finally, in specifications 3 and 4 we present the results of the two Sample Selection Probit models discussed above. First of all, we note that the censoring of the sample to individuals between 16 and 59 years of age alone does not bias our results. This can be seen by specification 1, which will be the reference model for comparisons. Note also that the sample in specification 4 is different from the sample in specification 1 even though both models include all respondents between 16 and 59 years old. This is because there are some people whose answers on self-reported crime questions were recorded, for unspecified reasons, as missing cases.

It is well known that the sample selection models are better behaved if at least one exclusion restriction is imposed on the crime equation. Otherwise there is severe multicollinearity and identification is obtained only due to nonlinearity of the functional form. For this reasons in model 4 we use *No Qualification* and *Other Present* as two dummies that belong to the selection equation only. *Other Present* is a dummy variable that takes the value one if someone else was present during the face-to-face interview. Here we assume that others' presence and low education might have affected the selection process but not the crime process. The presence of someone else during the interview might have affected the selection process as the respondent might feel some kind of pressure from the other members. For instance, in an extreme case, the husband could have prohibited respondent from completing the self-report questionnaire. In another direction, the presence of others might indicate that respondents needed help during the interviews and therefore, they did not answer the relevant crime questions. *No Qualification* could be a proxy for not participation, because of difficulties in using the computer.[1] In specification 3 a more appropriate variable is used. Once they accepted self-completion, respondents replied to the question whether they have language difficulties, which is a major factor for asking help to complete the questions but not a factor for reporting domestic crimes. However, this variable cannot be used in model

[1] Note that the *No Qualification* dummy has no effect on the crime equation once we include it in the selection process. Actually, none of the variables used as “instruments” have a significant effect in the “victimization” process once they are included in the “selection” process.

4 as answers on this question are conditional on accepting participation.

Table 3.13 gives some very interesting findings. First of all, we can see that the probability of an immigrant to take part and subsequently, answer the crime questions is much lower. Moreover, it is also clear that the variables used only in the selection equation have strong negative effects in the likelihood of selection. We also notice that the immigration status coefficient is still negative and very significant.[1] Nevertheless, most importantly, there is no support of sample selection, as suggested by the estimated correlation coefficients which are not statistically significant different from zero. Moreover, notice that the estimated coefficients are very similar between the sample selection models and the simple Probit model of specification 2. Therefore, in accordance with the above, the results of specification 2 can be used.

From the results in specification 2 we can see that the coefficient of immigration status is slightly smaller than in face-to-face interviews. Using the representative individual used in the previous section for domestic violence we find that, the probability of an immigrant to suffer a domestic crime is 2.36%, while the same probability for their native counterparts is 3.92%. Thus, the estimated difference is 1.55 percentage points or a decrease of around 66%, which is lower than the relative effect in face-to-face interviews. However, the difference in the estimated victimization-immigration gap between face-to-face and self-completed interviews is too small to be interpreted as more under-reporting by immigrants. We might observe this difference just because of the different nature of the self-completion questions or, because of differences in the sample size.[2]

3.6.2.2 The Presence of Others during the Face-to-Face Interview

Presence of other family members during the (mainly face-to-face) interview process may affect the reporting behaviour of the respondents (see, for example, Conti and Pudney, 2008),

[1] Note that more positives help to obtain more precise estimates, but lower sample reduces precision.

[2] Note that when we run a Probit model in face-to-face interviews holding only the sample from specification 2, the immigration coefficient becomes insignificant. However, it is high likely that this is because of the very low variation of the domestic crime variable combined with the relatively smaller sample. Moreover, regarding the effects of the other variables on the crime equations from models 2 to 4 we have: risk decreases with age and London is the least risky place. Concerning the selection equation in specifications 3 and 4 we observe that the probability of selection decreases as age increases. Full results are available from the author on request.

and it may actually result in under-reporting if the questions refer to very sensitive information (see, Acquilino, 1993). Particularly, we would expect that the presence of respondent's partner, mainly if the respondent is a female, would reduce the reporting of domestic crime. This might be because of respondent's fear of reprisal if the partner is also the offender, or because the respondent does not want to reveal to partner a crime that suffered by other family member, such as parents. As a first indicator, the data show that the probability to report a domestic crime is only 0.19% if the partner is present but 0.57% if the partner is not present, an increase of 200%.

Thus, using this information we could say something about the reporting behaviour of immigrants relative to the reporting behaviour of natives. On this direction, we could examine whether the effect of being an immigrant on the risk of victimization in the cases where the partner is present is different from this effect in the cases where the partner is not present. According to this, if immigrants under-report by more than natives, the estimated gap will be larger in the cases where partner is present (more negative). This could be formulated using the Probit model below,

$$E(y_i|x_i) = \Phi(\beta_0 + \beta_1 Immigrant + \beta_2 Par.Present + \beta_3 Immgrant \times Par.Present), \quad (3.1)$$

where y_i, as before, is the binary variable that takes the value one if a person is victimized by a family member and zero otherwise. The coefficient of the interaction term β_3 is the one of interest. Holding everything else constant, if immigrants under-report by more than natives, we expect this coefficient to be negative. Of course, here we also assume that immigrants' reporting behaviour does not differ from natives' one under no presence of the partner.

Most importantly, this strategy requires that *Partner's Presence* is assigned randomly, so that people whose partner was present are not different from people whose partner was not present. However, this is not the case. Probit results show that people whose partner was present are relatively more males, less educated, more married, less employed, and stay relatively more in more deprived and urban areas. Also, age has an inverse U-shaped effect on the probability of the partner being present. Therefore, a more appropriate strategy would be to examine the differences in reporting behaviour between immigrants and natives once we control for these characteristics. If β_3 is still negative, there is evidence of more under-reporting by immigrants relative to natives.

The main results are presented in Table 3.14.A in three specifications (without controls, after controlling for age, gender, and area dummies, and after controlling for the previous set of variables plus dummy variables for marital, education, and employment status). Table 3.14.B shows the predictions of these models for the representative individual used in the previous section for domestic crime. The results are very interesting. We can see that in specification 1, $\hat{\beta}_3$ (the estimate of β_3) is actually positive and statistically significant, which indicates that if one group under-reports it is the one of natives. Although this estimated coefficient becomes insignificant once we use the previously discussed regressors, it is still positive and preserves some of its magnitude. Particularly, the predictions show that this difference exists due to the following: a) immigrants whose partners are present actually report (insignificantly) more that immigrants whose partners are not present (but the same in specifications 1 and 2), but b) natives whose partners are present report (insignificantly) less than natives whose partners are not present (but significantly less in specifications 1 and 2).[1] Thus, this might indicate that immigrants' reporting behaviour does not alter in the presence of their partners but natives' one does.

For the above result to take the interpretation of under-reporting by natives we conduct two further exercises. Firstly, we examine the reporting behaviour in self-completions, where the presence of the partner should have a much smaller effect both because respondents under-report relatively less in self-completions and because there were clear instructions by the interview conductor that the partner was not allowed to disrupt the interviewee by any means (for instance, not allowed to look at the computer's screen). The results which are again shown in Table 3.14.A and Table 3.14.B are quite interesting. From specifications 1 and 2 we can see that both immigrants and natives report significantly less crime when partner is present than in the cases when partner is not present. However, this is due to differences in observed characteristics between people whose partner is present and people whose partner is not present, as in specification 3 it is clear that the reporting behaviour of

[1] Differently, immigrants without the presence of their partner report significantly less than natives without the presence of their partner, but immigrants with the presence of their partner report insignificantly more than natives with the presence of their partner.

both immigrants and natives does not change under the presence of their partner.

Secondly, although the presence of the partner may affect the reporting behaviour in domestic crime, it should not affect the reporting behaviour for crimes suffered by acquaintances (or, it should affect it by much less). Indeed, the results in the lower parts of Tables 3.14.A and 3.14.B are very similar to the results of self-completions. Specification 3 shows that the probability of both immigrants and natives to report a crime by an acquaintance is exactly the same with and without the presence of their partner. In other words, being an an immigrant decreases the probability to suffer a crime by acquaintances by 0.4 percentage points regardless of the presence of the partner.

Overall, from both strategies used we can conclude that, there is no evidence that immigrants under-report by more than natives and perhaps, immigrants report more accurately than natives. Thus, there is also no reason to believe that immigrants would under-report by more than natives crime suffered by acquaintances either. Therefore, the different pattern in the immigrant-native differences in the probability of victimization between crime by strangers and crime by familiar people cannot be attributed to under-reporting. In the contrary, if we observed the true victimization incidents the differences in the probability to suffer a crime could be even larger. Thus, there should be other reasons to explain the above pattern. This is examined in the following two subsections.

3.6.3 Controlling for Racially Motivated Crime

Racially motivated crime (RMC) has been the subject of many monographs, such as Gabbidon and Greene (2009), Spalek (2008) and Kalunta-Crumpton (2010) to mention only a few. Traditionally associated with ethnic minorities, RMC refers to "hate crime" against individuals of different ethnic group. As opposed to violent crime in general, RMC does not require interactions or interrelations between the potential victims and potential offenders. Offenders, most probably extremists of one race, violently abuse people of a different race, colour, or religion, without any pre-existent argument and in most cases without any provoking action by the victim. A 43% of immigrant population in the BCS data consists of nonwhite individuals as opposed to only 2.5% for natives.

Of course, a basic complexity in empirical studies of RMC is that it is very difficult to find appropriate data. Moreover, as RMC is traditionally associated with ethnic minorities, occasionally, researchers ignore that white people can also be victims of RMC.

In the present study I deal with RMC as follows. For each victimization incident a question is asked about whether the victims think that the incident was racially motivated. As the question is asked to every victim, we control for RMC against white people as well. However, the problem is that I observe *perceived* RMC rather than *actual* RMC. Therefore, we need to take into account that, as RMC is traditionally associated with minority groups, ethnic minorities could be more likely to consider a violent crime as being of race motive compared to whites, even if the crime is of the same nature. Nevertheless, in this study I assume that victims' *perceived* RMC coincides with *actual* RMC. In the data only 37 victims of violent crime out of 1,190 victims perceived an incident suffered as RMC (3.11%). From these 37 victims 17 were immigrants (around 17% of immigrant victims) and 20 where natives (which is only the 1.83% of native victims).

Thus, I can identify all cases of racially motivated incidents and control for them by replacing their values with zeroes.[1] First of all, from Table 3.15, we can see that apart from one case, RMCs were committed by strangers, which is consistent with the argument that pre-existed history and interrelations are not needed for this crime to take place. In Table 3.16, as a first indicator, a simple mean comparison is presented before and after controlling for RMC. We can see that the immigrant-native difference in the probability to suffer a violent *Crime by a Stranger* alters sign. However, it is still far from the differences in *Domestic Crime* and *Crime by Acquaintances*.

In the next stage we look at the relationship between *Immigrant* and *Crime by Strangers* once we control for RMC and for basic demographic characteristics. This is examined in Table 3.17. In the first specification the results for *Crime by Strangers* without controlling for RMC are given. The second specification presents the estimates for *Crime by Strangers* once we replace the cases of RMC with zeroes. Finally, specifications 3 and 4 present the results of *Crime by Acquaintances* and *Domestic Crime* for the sake of comparisons. From specification 2 we can see that if RMC did not exist and if immigrants faced the same area,

[1] I have also tried dropping the RMC from the sample. The results are almost identical.

gender and age distribution, they would be less victimized by strangers, a difference that is statistically significant at 10%. The marginal effects, using the representative individual of subsection 3.5.2.2, show that after controlling for RMC, being an immigrant does reduce the probability of *Crime by Strangers* by 0.49 percentage points (which is significant at 5%), a relative decrease of around 37%, whereas there is no change in the estimated probability of crime if we do not control for RMC.

Thus, we can see that controlling for these very few cases of RMC is enough for changing the picture for crime suffered by strangers. However, comparisons with specifications 3 and 4 show that controlling for RMC is not enough to explain the estimated differences between the three crime types. As we can see from specification 2 and 3 the relative effects are much larger for *Crime by Acquaintances* and *Domestic Crime*. Thus, although RMC is able to explain some of the unexpected difference in the estimated immigrant-native victimization differentials by relationship status, some unobserved reasons remain.

3.6.4 Network Effects and Assimilation Patterns for Violent Crime

As discussed in subsection 3.5.2.2 a reason why immigrants are less likely than natives to suffer a *Crime by Acquaintances* could just be that immigrants are also more likely, particularly the most recent ones, to have a smaller network of acquaintances. However, this cannot be the case for domestic crime because, as we saw in subsection 3.5.2.1, immigrants' households consists of more members (even for the most recent immigrants). Unfortunately, the BCS does not provide any information on the number of acquaintances the respondent has. Nevertheless, in this subsection, I examine the "network effect" hypothesis by assuming that immigrants expand their networks of acquaintances as they stay longer in the country. Therefore, a linear trend that measures the number of years of an immigrant in the country is used, once I control for immigration status and basic demographic characteristics. If the aforementioned hypothesis holds, we expect the linear trend to have a positive significant effect.

At a first glance, the results which are presented in Table 3.18 provide some support on the above hypothesis. We can see from specification 1 that the linear trend has a positive and significant at 10% effect. Thus, more recent immigrants are much less victimized than natives, but immigrants' victimization probability converges to natives' one as time spent in the host country increases, perhaps because of network effects. However, we can also see that this assimilation is very slow as it takes around 70 years for immigrants to reach natives' victimization probability. Moreover, the results in specification 3, where I use four assimilation dummies, also show that more recent immigrants are less victimized by acquaintances. However, they also indicate that time spent in the country does not affect the victimization probability linearly but a quadratic trend would be more appropriate. This is evident in specification 2 as well, where a quadratic term is also included. It is clear that starting with a very large difference, the victimization differential between immigrants and natives closes but it never becomes zero. The gap reaches its minimum at around 30 years in the country and then starts increasing.

If the effect of the trend was purely due to networks effects, we would expect it to have a linear effect. Therefore, we must be cautious with the interpretation of these results as there might be some other unobserved factors involved that give rise to the observed relationship. From specification 4 we can see that there is a linear assimilation trend for *Domestic Crime* as well, even though, as we saw before, immigrants' families are larger.[1] Moreover, specification 5 indicates that a weak quadratic assimilation pattern exists for *Crime by Strangers* too, even though networks should make no difference in crime by strangers.

Given the evidence from all crime types, it seems that if networks effects exist in *Crime by Acquaintances*, they are quite weak. According to these results the following story could be more appropriate. More recent immigrants, perhaps because they consider themselves more vulnerable, set strategies associated with lower victimization. As time spent in the country increases, immigrants assimilate in natives' lifestyle, or increase their networks of familiar people, resulting in a smaller victimization difference for all crime types. However, for earlier immigrants, the picture looks different for *Crime by Acquaintances* and *Crime by Strangers*. Earlier immigrants seem to suffer fewer violent incidents even though we control for differences in the age distribution.[2] Hence, earlier immigrants, due to some unobserved

[1] The quadratic trend does not fit well in domestic crime.

[2] It is essential to control for *Age* because earlier immigrants are older and therefore, they have a lower victimization probability. Moreover, it is important to stress that controlling for *Age* by including a quadratic or a cubic term instead of dummies makes no difference in the assimilation patterns found for domestic and crime by acquaintances (but slightly weakens the assimilation relationship for crime by strangers). Therefore,

factors (perhaps cultural), follow social lifestyles associated with lower victimization than natives with the same basic demographic characteristics.

Summing up, immigrants face a lower probability of violent victimization and we have argued that this might be because immigrants follow social lifestyles associated with lower victimization. However, further analysis showed that this difference exists only for crime by familiar people, as immigrants face the same victimization probability for crime by strangers if we do not control for racially motivated crime. This should be considered as unexpected if immigrants follow a lifestyle that draws them away from crime activities, given that violent crime depends a lot on interactions between potential victims and potential offenders. However, we provided evidence that this difference is not because of more under-reporting by immigrants. Moreover, some of this difference can be explained by racially motivated crime, and perhaps in a small degree by "network effects". In addition, we should not ignore that crime by strangers is more "random". Finally, the fact that the proportion of immigrants in the "strangers group" is probably smaller than the proportion of immigrants in the "family and acquaintances group", as natives account for the 90.5% of the population (at least in the BCS data of 2007-08), could be another reason to explain the aforementioned difference, given that as found by Papadopoulos (2010b), immigrants are slightly less violent as offender than natives.

An interesting question emerging from our analysis is the following: if immigrants set the aforementioned lifestyle strategies, why do we still observe the positive (although insignificant) association for personal thefts? First of all, as has been stressed throughout this paper, personal behaviour is a highly more important determinant for violent crime than for personal thefts. This is closely related to the "randomness" that I have discussed throughout this study. Therefore, the aforementioned lifestyles of immigrants would have a much stronger effect on violent crime than on personal thefts, which has as a result to overbalance the positive victimization-immigration association because of higher *proximity* for violent crime, but not for personal thefts. In a cost-benefit setting, the above can be explained by the fact that it is much more costly (in the sense that it needs much higher effort) to reduce

we can argue that it is not the case that we observe the negative relationship for earlier immigrants because we were not able to capture the age distribution properly.

the uncertainty of suffering a personal theft than to reduce the uncertainty of suffering a violent crime.

3.7 Further Topics

3.7.1 Decomposition of Immigrants by Ethnicity and Location

So far, we have ignored the fact that there is a great deal of ethnic heterogeneity in immigrant population. It might be that, due to cultural differences, immigrants of different ethnic background may follow different social lifestyles associated with different risks of violent victimization. Moreover, location of immigrants is not randomly assigned. Different locations may attract different types of immigrants, or immigrants located in different places may face different conditions, which in turn may affect the strategies they set with regard to their *social lifestyle-exposure* and *routine activities*.

We first look at the former by including interaction terms between immigration status and ethnic background. The results are presented in Table 3.19 for all violent crime categories. Note that, although only the coefficients of interest are included, we use the specification where we control for gender, age dummies, and location characteristics (as in the third specification of Tables 3.7 and 3.8). Regarding *Total Violence*, we find that the results shown in Table 3.8 (where immigration status has a negative significant at 5% effect on violent victimization) is primarily driven by the differences in victimization experiences between White immigrant-native counterparts, and Chinese & Other immigrant-native counterparts, as there are no differences between the other three ethnic group of immigrant-native counterparts.[1] Moreover, only Asian natives suffer lower violence than White natives.

The picture is different if we decompose violent crime by relationship status. It is important to stress that for *Domestic Crime* and *Crime by Strangers* we only use interactions between *White* and *Immigrant* because, otherwise, the variation was not enough to esti-

[1]Comparing a white immigrant with a white native, who are both males, between 26 and 35 years of age and live in average deprived urban areas of East England, we find that being an immigrant decreases the probability of violent victimization by 1.09 percentage points (from 0.0534 to 0.0425, a significant difference at 10% significance level). Moreover, regarding the 'Chinese & Other' ethnic group we find the for the same representative individual, Chinese and Other immigrants' victimization probability is -0.067 percentage points lower than Chinese and Other natives (from 0.0238 to 0.0908, a significant difference at 5%).

mate all coefficients of interest. As far as *Crime by Acquaintances* is concerned, it is clear that White immigrants still face a lower probability of victimization than White natives (and significant at 5%), but this gap closes for Asians and Chinese & Other ethnic groups. Conversely, the difference even increases in magnitude for Black individuals.[1] Note that, although negative, the difference in the probability of victimization by acquaintances between non-White immigrants and non-White natives is statistically insignificant.[2] However, the picture is quite different for *Domestic Crime*. In contrast with *Crime by Acquaintances*, here the main difference is observed to be between non-White natives and non-White immigrants. Non-White immigrants suffer much less domestic crime than non-White natives but this gap closes for White people.[3] Finally, note that, there are no statistically significant immigrant-native differences across ethnic groups for *Crime by Strangers*. In this study I do not go into further investigation on the rationale of the aforementioned observed relationships but I keep the analysis totally descriptive. Thorough investigation would require a much larger data set as the variation between crime by relationship status (which is a very rare event) and immigration status by ethnic groups is quite low to estimate robust relationships. This analysis is left for future research.

Next, I consider decomposition of immigrants by regions. First of all, in order to be able to identify all coefficients of interest I group regions in four categories, keeping London as the baseline area.[4] Again, I present only the coefficients of main importance but I also control for gender, age, and other location characteristics. The results are presented in Table 3.20. Concerning *Total Violence*, the results suggest that there are not many differences across regions. Both immigrants in London and immigrants not in London are less likely to be victimized than their native counterparts,[5] but this difference is higher for immigrants of London. However, if we consider the four regional groups separately we find that although

[1] A Wald test that compares Black-immigrants against Black-natives shows that this difference is significant at 5% (p-value of 0.0362).

[2] The coefficient of the difference is -0.194 with the robust standard error of 0.163.

[3] However, there was not enough variation to further examine this relationship.

[4] These groups are, *North* (North East, North West and Yorkshire & Humberside, 12,863), *Midlands* (East Midlands, West Midlands and East of England, 15,973 obs), *South* (South East and South West, 10,142 obs) and *Wales* (4,243 obs).

[5] Running a regression of *Total Assault* on the dummy *London*, its interaction with immigration status, and the rest of the characteristics, shows that immigrant not from London also face lower crime than immigrants not from London.

the sign on the immigration-victimization relationship is still negative it turns insignificant. The only area for which the difference is still significant is *Midlands*.[1] Almost the same relationships hold for *Crime by Acquaintances*, with the only difference that now, the difference between immigrants and natives of London is higher in magnitude and that the difference is also significant for the regional group *South*. For *Domestic Crime*, the results are very insignificant probably because of the low variation between the dependent variables and the interaction terms. However, we still find that immigrants not in London are less likely to be victims of domestic crime than natives not in London.[2] Moreover, it is found that immigrants of *Midlands* suffer less domestic crime than natives of *Midlands*.[3] Finally, no statistical relationships are found for *Crime by Strangers*, even if we control for racially motivated crime.[4]

3.7.2 Seriousness of Crime

So far, we have found that if immigrants exhibited the same basic demographic characteristics with natives, they would face a lower probability of violent victimization but a similar probability of property victimization. However, we have not made any reference on the seriousness of crimes they have suffered. In this subsection I exploit information from the Victim Forms, where all victims were able to rank the "seriousness" of the crimes they suffered in a scale from 1 (not serious) to 20 (very serious). Since each victim could take up to six victim forms, I averaged the "seriousness" score for each victim and then I created

[1]This is the case perhaps because it is the region with the highest number of observations. A Wald test of the difference gives a p-value of 0.0348. For South, the Wald test gives a p-value of 0.115.

[2]A regression of *Domestic Crime* on the dummy *London*, its interaction with immigration status, and the rest of the characteristics, shows that immigrant not from London face lower crime than natives not from London with a coefficient of -0.240 which is statistically significant at 5%.

[3]The Wald test gives a p-value of 0.03.

[4]Finally, note that further exercises using interactions (which results are not presented here but are available on request) show that the highest differences between immigrants and natives exist for, residents of the most deprived areas (although the effect of the interaction term is statistically insignificant), people who rent, people who are less educated, and single individuals (but only for domestic crime). Moreover, there are no interaction effects between gender and immigration status, apart from crime by strangers (once we control for racially motivated crime) for which we find that immigrant males suffer less crime than native male (significant at 5%), but this difference closes for females. In general, it seems that the highest differences exist for the most vulnerable groups of immigrants. Perhaps immigrants who believe that they are in weak positions (lower *guardianship* or higher *proximity*) are more in fear of a potential crime against them and therefore, decide to balance their position by exhibiting lower *exposure*. As the findings indicate, the result is to suffer lower crime than their native counterparts, perhaps because the effect of lower *exposure* overbalances the effect of higher *proximity* and lower *guardianship*. This subject is left for future research.

an ordinal variable that takes value '1' if victims believed that the "seriousness" of the crimes experienced is between 1 and 5 (*Not Serious*), '2' if it is between 6 and 10 (*Relatively Serious*), '3' if it is between 11 and 15 (*Serious*) and '4' if is between 16 and 20 (*Very Serious*).[1]

It is clear that since this is a measure of perceived "seriousness", the coefficient of immigration status would be upward biased if for some reasons immigrants tend to score incidents of the same actual seriousness as being more serious. The results for total crime are presented on Table 3.21. Specification 1 uses no controls, while in specification 2, in line with previous regressions, we control for basic demographics and in specification 3 we include further controls that might be associated with perceived seriousness. What we find is that, regardless the controls we use (look at specifications 1 to 3), immigrants strongly believe that crimes they suffer are much more serious than what natives believe. As we mentioned before, this might not indicate that immigrants are recipients of more serious crimes if they, for some unobserved reasons, tend to overvalue the seriousness relatively to natives. Using the cutoff estimates and the estimated coefficients from specification 2, this model predicts that being an immigrant victim:[2] 1) decreases the probability for an experienced crime to be considered as *Not Serious* (1-5) by 8.3 percentage points, a relative effect of 11%, but 2) increases the probability for a crime to be considered as *Relatively Serious* (6-10) by 5.3 percentage points, a relative increase of 26%, 3) increases the probability for a crime to be considered as *Serious* (11-15) by 2.3 percentage points, but with a relative effect of 56% and finally, 4) increases the probability for a crime to be considered as *Very Serious* (16-20) by 0.7 percentage points, which account for an even higher relative effect of 89%. Note that all these differences are statistically significant at 1%.

Moreover, specification 4 shows that the immigration status dummy has no effect if we control for ethnic background (but it is still significant at 5% if we include the ethnicity dummies on specification 3). However, specification 5, where we interact immigration status

[1] Note that from the 11,208 victims, 66% believed that the victimization incidents they experienced are of seriousness from 1 to 5, 25% from 6 to 10, 7% from 11 to 15 and only 2% from 16 to 20. Moreover, note that creating an ordinal variable with 8 categories gives very similar results.

[2] For these predictions we use the representative individual who is a male between 25 and 35 years old, and live in an average deprived urban area in the East of England. The estimated probabilities, differences and relative effect, are calculated with the "nlcom" command in Stata®.

with ethnicity dummies, provides further interesting insights. Although White immigrants do not perceive crimes they suffer as more serious than their native counterparts, non-White immigrants do. Actually, a regression where we only interact immigration status with ethnic group *White* shows that being a non-White immigrant significantly increases the probability to perceive a crime as more serious relative to a non-White native.[1] In more detail, specification 5 shows that apart from White and Mixed ethnic groups, Black, Asian and Chinese & Other immigrants value their crime experiences as more serious than their native counterparts, although the estimated difference is statistically significant (at 5% significance level) only for the group of Asians. Finally, further analysis where we look at household crime, personal theft and violence separately shows that the above negative relationship holds for each crime category, but it is a bit less significant for personal crime.[2]

3.8 Count Data Models

All previous analysis concerned the conditional probability of victimization and provided some very robust results regarding the difference in the probability of victimization between immigrants and natives across the different crime types. However, the count nature of the victimization variable was totally neglected. Considering the count form of the crime variables and utilizing several count data models could provide some further insights on the determinants of victimization in general, and particularly, on the immigration-victimization relationship. For instance, even though immigrants face a lower probability of violent victimization, the implications of our analysis would be very different if, as will be discussed further below, immigrants experience a higher number of crime incidents than natives.

Count data are directly related to the problem of repeated victimization, as someone is said to be a "repeated" victim if he/she has suffered more than one incident of the same crime type within the reference period.[3] Together with the causes of a single crime incident,

[1]The coefficient is 0.292 with a robust standard error of 0.082.

[2]It is actually significant at 10% for violence, but insignificant for personal theft. However, notice that we only have 1,186 cases of violence and 745 cases of personal theft. Full results are available from the author on request.

[3]In general criminologists distinguish between the term "repeated victim" and the term "multiple victim" (see, for example, Hope at al, 2001). A person is a multiple victim if he/she suffered more than one type of crimes in the reference period, regardless of the number of crimes of the same crime type. For instance, a person experiences in the last 12 months both an inside burglary and an assault. However, other studies

the understanding of the channels through which repeated victimization occurs has also received a lot of attention by criminologists, in an attempt to find alternative effective policies for crime reduction which would in turn allow policy makers to efficiently allocate scarce resources in the areas where people or households face the greatest risks (see, Sparks, 1981, Farrell, 1992, Farrell, Phillips and Pease, 1995, and Osborn at al, 1996). This is important, as crime is found to be concentrated among a small group of people and areas (see, Spelman, 1995, Ellingworth, Farrell and Pease, 1995) and because prior victimization is found to be a very strong predictor of future victimization (Hindelang, Gottfredson and Garofalo, 1978, Ellingworth at al, 1997, and Wittebrood and Nieuwbeerta, 2000). Several researchers have attempted to understand the process of repeated victimization by using count data models (see, for example, Nelson, 1980, Tseloni and Pease, 2003, 2004).

There are a couple of reasons to expect that the process of having suffered a single incident is to some extent different from the process of repeated victimization. According to this, we implicitly allow for the effects of the characteristics associated with victimization to differ between the probability of a single incident and the number of incidents conditional on victimization. First of all, criminologists have made some effort towards understanding whether there is some kind of dependence among crimes suffered by the same individuals or, it is just that the characteristics associated with higher risks responsible for a first crime incident persist over time resulting in further actions against them. Event-dependence among sequences of crimes against the same individuals could be possible if a first crime initiates a positive or a negative "contagious" process. For example, a positive "contagion" (mostly for household crimes) could be the consequence of some kind of transmission of information amongst offenders concerning the vulnerability or *attractiveness* of some targets. Differently, a positive "contagion" for violent victimization could exist if following the victimization incident, victims choose to revenge or retaliate, which in turn would expose the victim to further violence. On the other hand, negative "contagion" would be the result of reevaluation of strategies following an incident, which would make victims, for instance, to increase their *guardianship* or reduce their *exposure*. However, we need to stress that these dynamics cannot

do not distinguish between these two terms (see, Farrell, 1992). In the present study, the victim is said to be "repeated" if he/she suffered more than one crime (depending on the crime type I consider) within the reference period of 12 months prior to the interview.

be identified in the absence of panel data, which is the case in the present study. This is because, firstly, we are not able to observe when the first action occurred, and secondly because the cross-section models used in this study assume independency of the incidents.[1]

Secondly, the effect of the regressors could be different between the two processes (victimization or not, and the number of incidents conditional on victimization), if there is unobserved heterogeneity within the same variables which is associated with differential victimization across the two processes. As an example, consider the relationship between gender and violence. As males exhibit a much higher *exposure*, the victimization probability is much higher for them. However, the picture could be different if we consider only victims, as females might be victimized more frequently, perhaps because of domestic violence. A similar story can be considered for immigrants. Given that immigrants are generally more vulnerable (lower risk for offenders) they decide to set strategies associated with lower *exposure* on crime. As a result, according to the findings of the previous sections, immigrants are on average less likely to be victims of violence. However, if we consider the population of the victims only, it might be the case that here we have either the immigrants that failed to successfully set the low *exposure* strategies, or groups of immigrants whose cultural characteristics are associated with higher *exposure* relative to the groups of less victimized immigrants. According to this, immigrant victims could be equally or even more victimized than native victims. Therefore, if the above were true, we would expect that the coefficient in the count data models to be less negative than in binary models, as the number of the incidents is also taken into account.

In this study I consider Poisson and Negative Binomial 2 models, as the latter also takes into account over-dispersion (by allowing for an unobserved gamma distributed error) which, as described before, is evident in my victimization data.[2] However, the nature of victimization data gives rise to two issues that require special attention. Firstly, as crime

[1]We need to note that although the Negative Binomial distribution is consistent with a count generation process with positive "contagion", in the absence of panel data we cannot distinguish between "contagion" and "heterogeneity" among different groups. For interesting details on the genesis and other aspects of the Negative Binomial distribution the reader may refer to Johnson, Kemp and Kotz (2005), Cameron and Trivedi (1998), and Winkelmann (2008).

[2]It is well known that the Poisson distribution assumes equi-dispersion meaning that the first two moments are equal to each other. For more details on count data models refer to Winkelmann (2008) and Cameron and Trivedi (1998).

is a rare event (at least if we want to consider the different crime types separately), the number of zeroes is very large. Moreover, the (very few) positives are quite dispersed for most of the crime categories. This has harmful consequences on the robustness of the count data estimators. Secondly, there are few cases of victims that reported extreme number of crimes.[1] Count data models are very sensitive to these cases, particularly when the positive counts are too few to identify the parameters of the variables assumed to affect the mean.

I deal with the second issue using two different approaches. Firstly, I censor the crime variable at different points of the violent crime distribution and then I use a Poisson model with a modified likelihood function that takes into account the censoring in the dependent variable (for details in censored count data, see, Terza, 1985, and Brannas, 1992). Not only does this strategy tests for the robustness of the estimates of the conventional count data models, but it also adds some robustness to the estimator.[2]

Furthermore, I use the "Quantile Estimator for counts" developed by Machado and Santos Silva (2005) and successfully used by Winkelmann (2006), Booth and Kee (2006), and Miranda (2008). Usual quantile estimators are developed for continuous data (see, Koenker and Bassett, 1978) and are not available for counts, or other discrete choice variables. However, very briefly, Machado and Santos Silva (2005) suggest a method that overcome this problem by adding a uniformly distributed noise to the count outcome (a method called "jittering"), which artificially generates the required smoothness of a continuous variable. Then, quantile estimation proceeds by using standard quantile techniques. Moreover, they propose averaging out the uniformly distributed noise by considering m jittering samples which increases efficiency of the estimator.[3] Utilization of this estimator serves two purposes. Firstly, the quantiles are insensitive to the extreme cases, and secondly, we can estimate the effect of the regressors on different parts of the distribution (which might be different according to the repeated victimization theories). However, as the number of zeroes in my dependent

[1] The maximum they could report in each victim form was 97 crimes. Therefore, in the extreme, someone could report 582 crimes.

[2] On the other hand, the results of censored count count data models are at same time less "robust" because the results of the Poisson Pseudo Maximum Likelihood (see, Gourieroux, Monfort, and Trognon, 1984) do not hold. Thus, the censored model would be misspesified, if the remaining counts above the considered cut-off point do not follow the poisson distribution. In the contrary, the Poisson distribution only requires correct specification of the conditional mean, with valid inference given by the Pseudo Maximum Likelihood standard errors, or differently, Eicker-White robust standard errors.

[3] For details on this estimator refer to Machado and Santos Silva (2005).

variables is very high, it is more reasonable to look at the effects of the variables on very high quantiles.

Alternatively, a model that would be also consistent with the story of differential repeated victimization could be a hurdle (two-part) model, where the "hurdle" is set at no crimes. According to this model (see, Mullahy, 1986), zeroes or positives (without distinction on the number of incidents) are generated by a distribution appropriate for binary choice models. If the realization is positive, the hurdle is crossed, and positives are generated by a truncated at zero (see, Grogger and Carson, 1991, and Gurmu, 1991) distribution for counts, such as the truncated at zero Poisson or the truncated at zero Negative Binomial distribution.[1] Hence, this model explicitly allows to separately model the binary outcome (victimized or not) from the positives (number of incidents given victimization, or differently, repeated victimization).[2] Therefore, we can directly observe whether the independent variables have different effects below and above the hurdle (thus, at different parts of the distribution). Again, the very low number of positives and the extreme reports by some individuals will constraint my analysis. Nevertheless, as an alternative and in line with the Censored-Poisson model, I develop a two-part model for censored counts. Details on the probability and likelihood functions of this modified Hurdle-Censored Poisson model are presented in the Appendix.

In this study I only present results on violent victimization, which was the centre of attention in the main analysis. Furthermore, because of the aforementioned large number of zeroes, the estimation analysis is not so reliable if we further decompose violent crime by relationship type, particularly when I use the Hurdle-Censored Poisson estimator. Thus, I mainly present results on total violent victimization and I refer to results of the separate groups when necessary.[3] Before presenting the count data results, the complete distribution together with the three unconditional moments of the violent crime variables are presented

[1] Count hurdle models (together with some modified count hurdle models) are very successfully used in health economic literature (see, for example Pohlmeier, Ulrish, 1995, and Gurmu, 1997), or other contexts (see, for example, Gurmu and Trivedi, 1996, Arulampalam, Booth 1997, Santos Silva and Covas 2000, and Helstrom, 2006).

[2] By taking into account the different data generating process below the hurdle and above the hurdle, we explicitly take into account the exceptional nature of zeroes. Moreover, the hurdle model accounts for both over-dispersion and under-dispersion.

[3] All results that are not present here are available from the author upon request.

in Table 3.22. It is clear that *Total Violence* is a rare event as only 3.54% of respondents reported at least on violent incidence. It is also clear that incidents of violence are highly dispersed and skewed to the right, a feature driven by *Domestic Crime* and *Crime by Acquaintances*, as *Crime by Strangers* is generally concentrated on the first 10 counts.

The results of the count models are presented in Tables 3.23, 3.24 and 3.25. The second and the third specifications of Table 3.23 depict the results of the conventional Poisson and Negative Binomial 2 (NB2) regression models, whereas specification 1 gives simple Logit results for the sake of comparisons. The rest of the specifications present the Censored-Poisson model, where we censor the dependent variable at 5, 10, 15, 20 and 25 crimes. Table 3.24 displays the estimates resulting from the Quantile estimator for counts, where we look at the effect of the variables at the 25th, 50th, 70th, 80th, 90th, 95th, 99th, 99.9th, and 99.99th percentiles of the distribution. We explore unusually high quantiles because, as I mentioned earlier, it is important to explore the impact of the regressors on the very right end of the distribution. It is also important to note that my results are obtained using 100 jittered samples. Finally, Table 3.25 shows the results from a simple Hurdle-Poisson model and the results from the modified Hurdle-Censored Poisson model, where I censor at 5 and 10 crimes.[1] Specifically, the first column gives the probability of crossing the hurdle for which the Poisson distribution is also used,[2] the second specification shows the Zero-Truncated results without censoring and the rest of the specifications provide the findings of the Zero-Truncated Censored models.

To begin with, apart from the coefficient on *Urban* and the fact that regional dummies have a smaller effect (relative to London) in NB2, the results of Poisson and NB2 are fairly similar. According to the NB2 model, there is quite strong evidence in favor of over-dispersion.[3] However, it is important to stress that although the Poisson regression model assumes equi-dispersion (conditional mean equal to conditional variance), which implies that

[1]The estimation procedure was numerically unstable when censoring at higher than 10 crimes was considered. This is probably because of the small number of observations above the hurdle (1,190 observations). Therefore, the results of censoring the variable at a higher value are not presented here.

[2]These estimates are closely comparable to Logit ones. Actually, the Logit probability function can be also obtained from considering only the zero probability from the Geometric version of the Negative Binomial distribution for count data (see, Mullahy, 1986).

[3]In this table, 'alpha' is the estimated variance of the gamma distributed unobserved effect. According to the NB2 model the conditional variance of the dependent variable is given by $\omega = \lambda + \alpha\lambda^2$. As the estimated 'alpha' is around 40 and statistically significant, the variance is much higher than the mean.

the variance-covariance matrix is misspecified under the presence of over-dispersion, it is absolutely valid even in the cases of very over-dispersed data. This is because, as the results of the pseudo-maximum likelihood show (see, Gourieroux, Monfort and Trognon, 1984), the Poisson Maximum Likelihood Estimator consistently estimates the conditional mean and valid inference for the variance matrix of the estimator is obtained by using robust (Eicker-White) standard errors.[1]

Comparing the binary information with the conventional count data models several interesting points emerge that need some discussion. Firstly, we can see that although the *Immigrant* coefficient is still negative, it is now insignificant. However, this should not be interpreted as higher repeated victimization of immigrants without further investigation. Indeed, the Censored-Poisson models, regardless of the cut-off point of censoring, show that the effect of immigration is still very significant and not much different in magnitude than when we use the binary information only. This suggests that the long right tail of the observed distribution affects the precision of the effect of *Immigrant* on *Total Violence*. The results of the Quantile estimator and the Hurdle-Censored Poisson model are relatively in line with the aforementioned analysis. Regarding the Quantile Estimator results, although the immigration dummy has no effect on the first quartile and the median, as expected due to the small number of positives, its effect is negative and significant along the right part of the distribution. It is also clear that the effect starts diminishing when considering very high quantiles. Finally, from Table 3.25, the zero-truncated but uncensored Poisson assigns a positive but very imprecisely estimated coefficient to the immigration dummy which, however, turns negative in Zero-Truncated Censored models if we censor at 10 crimes. Overall, these results indicate that the very few observations at the end of the observed distribution reduce the influence of the immigration dummy. This suggests that if a differential repeated victimization between immigrants and natives exist relatively to the risk of victimization from the binary choice model, this is only for individuals that suffer a large number of incidents. Unfortunately, the sample size does not permit further investigation and safer conclusions.

The most striking result is that although being a male increases the probability of suffer-

[1] However, note that if the true data are truly generated by a Negative Binomial distribution, Poisson Pseudo Maximum Likelihood is less efficient.

ing a crime, it actually decreases the mean number of crimes. The Hurdle-Poisson models present this picture clearly. Conditional on being victimized, being a male significantly decreases the number of incidents. The effect is smaller for the Zero-Truncated Censored models but still very significant. Further investigation shows that this result is primarily driven by the relationship between gender and *Domestic Crime*. Nevertheless, a negative relationship holds for *Crime by Acquaintances* too, although it is less significant, but not for *Crime by Strangers*. Thus, there is some evidence that although males are more exposed on the incident of violence, females are more repeatedly victimized. This is probably because some women are captured in "unhealthy" relationships that bring them in situations of a constant high risk of victimization.

Finaly, it is also interesting that although the effect of *Urban* is positive but insignificant in the binary models, it turns negative in the count models. From the Quantile regressions we can observe that the effect of *Urban* is the highest between the 90th and the 95th percentiles and then decreases turning negative after the 99th percentile. Similar conclusions are obtained from examining the Hurdle-Censored Poisson model. Further analysis shows that this result is driven by the impact of being in an urban area on *Crime by Strangers*.[1] Even though people in urban areas face a significantly higher risk of victimization by strangers, repeated victimization by strangers is higher in rural areas if we only victimized individuals. This indicates that in rural areas there is a higher concentration of *Crime by Strangers* among the same individuals compared to urban areas. This is an interesting finding, but further research is required to identify the reasons behind this relationship.[2]

3.9 Conclusion

This study presented a comprehensive analysis of the relationship between immigration status and victimization in England and Wales using the 2007/08 sweep of the British Crime Survey.

[1]Being in urban areas significantly increases the victimization risk, where the estimated coefficient of *Urban* in the Logit model is 0.358 with a p-value of 0.005. However, in the Zero-Truncated Poisson this estimate is negative (-0.647) with a p-value of 0.014.

[2]Further investigation of these models with regard to the effects of the other variables can result in many interesting implications. However, as this paper concentrates on the victimization-immigration relationship this analysis is skipped here, but it is subject to future research.

Initially, we presented some evidence on the immigration-victimization relationship for *Inside* and *Outside Burglaries*. Immigrants' households are more at risk of *Inside Burglaries* but this is mostly explained by the fact that immigrants reside relatively more than natives in urban and more deprived areas where the incident of an *Inside Burglary* is highly more likely. On the other hand, a negative relationship was found between immigrants' households and the incident of *Outside Burglaries*. We argued that this is probably because immigrants possess a smaller amount of properties that are subject to *Outside Burglaries* such as, outhouses, garages, etc. This argument was supported with results on assimilation patterns (earlier immigrants are better settled and therefore, possess more outside properties than more recent immigrants) and zero-inflation count models (which show that a higher proportion of immigrants belong to the zero inflation category, meaning that immigrants are in lower risk just because they own fewer outside properties).

Furthermore, we showed evidence on *Personal Thefts*, a crime that is of a very different nature since, although instrumental as well, it entails personal contact. The results indicated that immigrants are in higher risk of *Personal Thefts*, but most of this positive association can be attributed to the fact that they disproportionately reside in the areas of London where the incident of a *Personal Theft* is much more probable than any other region in England and Wales.

Next we presented a series of evidence for *Violent Crime*, in which this work focuses on. *Violent Crime*, as opposed to the aforementioned categories, is an expressive type of crime where interrelations and interactions between the potential victims and potential offenders are vital. According to this, personal behaviour is a much stronger predictor for violent victimization than for *Personal Thefts* and *Household Crime*. Even after controlling for a rich set of characteristics associated with violent victimization, the empirical analysis indicated that immigrants are still at lower risk of violence. A possible explanation, which relies on the theoretical views of this paper, is that immigrants set strategies (that determine their *lifestyle-exposure* and *routine activities*) that are associated with a lower risk of violent victimization. Nevertheless, a closer examination indicated that the negative association is due to the lower risk of victimization *by Acquaintances* and lower risk of *Domestic Crime*, since the regression results showed that there is not any association between being an immigrant and crime suffered *by Strangers*. This result is, at a first glance, not in line with the hypothesis mentioned above, since if immigrants follow a particular lifestyle associated with lower *exposure* and therefore, lower crime, we expected to observe a negative association for crime by strangers as well. Thus, the next section attempted to shed light on the differences in the estimated immigration-victimization associations across the three (by relationship status) *Violent Crime* types.

Firstly, we examined the reporting behaviour of respondents towards *Domestic Crime*, as there is evidence that respondents tend to under-report domestic crime in face-to-face interviews. Thus, if immigrants tend to under-report crime suffered by (ex) family members, the observed immigration-victimization association will be downward biased. However, both strategies that we followed showed no evidence that immigrants under-report *Domestic Crime* by more than natives, and therefore, there is no reason to believe that they would under-report crime *by Acquaintances* either. Particularly, in the first strategy we used data on computer-based self-reported crime, as there is evidence that people respond much more truthfully in computer-based than in face-to-face interviews. The results from computer-based interviews are in line with the results from face-to-face interviews, that is, immigrants are significantly less likely to be victims of domestic violence. In the second strategy, we explored the information on whether respondents' partners were present during the face-to-face interviews, as people may under-report domestic crime by more in the presence of their partner. After a thorough analysis, also comparing with results in crime by acquaintances and computer-based self-reports of domestic crime, we concluded that if one group under-reports, this is the group of natives.

In the second step, the differences in immigration-victimization patterns among *Violent Crime* types were attempted to be explained by "racially motivated crime" and "network effects". Interestingly, we showed that if we control for (the only 37 cases - 20 for natives and 17 for immigrants - of perceived) racially motivated crime, which is a much more "random" crime highly associated with ethnic minorities, the risk of suffering a *Violent Crime by Strangers* becomes negative and significant at 10%, but not of the magnitude observed for crime by familiar people and (ex) family members. Next, using the "network effect" hypothesis, meaning that immigrants increase the number of acquaintances as time in the country increases, we tested whether the lower risk of victimization *by Acquaintances* that immigrants face is just because of the fact that the groups of acquaintances are relatively smaller for more recent immigrants. Therefore, connecting that to assimilation patterns, we showed that more recent immigrants are actually in lower risk of victimization than earlier ones. However, showing some further evidence, we argued that the observed assimilation link was most probably driven by other unobserved assimilation features. If "network effects" exist, they are relatively weak, and by no means could they explain the observed differences across violence crime types.

Finally, we considered further reasons that might explain the rest of the difference. Firstly, crime by strangers is in a sense more "random" than crime by familiar people, meaning that personal behaviours, and thus social lifestyles, have a smaller effect on the victimization outcome. Moreover, looking at the behaviour of immigrants as offenders can provide some interesting insights. For example, according to the "homogamy" principle, a high proportion of immigrants' (natives') acquaintances and family members are immigrants (natives) as well. But according to Papadopoulos (2010b), immigrants are (slightly) less likely to commit violent crimes, and therefore, we expect that, everything else constant, the risk of victimization *by Acquaintances* and *Domestic Crime* would be higher for natives. Finally, violent behaviour is a direct measure of *exposure*, and therefore, since immigrants exhibit a less violent behaviour, they are also of lower risk of violent victimization. However, this effect is incorporated into the aforementioned general lifestyle activities of immigrants.

Next, we briefly discussed the seriousness of the crimes that victims face. We actually found that although immigrants are less likely to be victims of violent activity, they consider the crimes they suffer as more serious than the crimes natives suffer. Of course, if for any reasons, immigrants tend to perceive crime of the same actual seriousness as more serious, all results of this section are biased upwards. Moreover, a very brief analysis of decomposition of immigrants by ethnic status and location did not reveal any important relationships. However, a much closer examination is required, perhaps using even larger data sets (by pooling several sweeps from the British Crime Survey), since in the present study the variation between the crime variables and the different immigrant groups was too small to obtain robust results.

After establishing the above relationship for the probability of victimization we considered count data models, exploiting the count nature of the *Violent Crime* variable. Count data analysis is important because it is directly connected with the concept of repeated victimization. As explained in detail in Section 3.8, some characteristics, such as gender, could have a different effect on the probability of suffering a crime and on the number of crimes suffered given victimization. Thus, the implications of our analysis would be very different if immigrants were disproportionately victims of repeated crimes. Several models were considered (Poisson, NB2, Censored Poisson, Quantile Estimator for counts, Hurdle-Censored Poisson) to explore the association between the number of violent victimization incidents and immigration. Initially, conventional Poisson and NB2 models showed that once we take the count information into account, immigrant coefficient loses much of its significance and magnitude. However, this should not be interpreted as differential repeated victimization by immigrants, as the Censored(-Hurdle) Poisson, and the "Quantile for counts" estimator showed that this result was driven by the very end of violent crime distribution. This means that if differential repeated victimization between immigrants and natives exists, it does only among highly victimized individuals. Therefore, according to these results, the effect of being an immigrant on victimization is relatively similar in both, the probability of suffering a crime and the number of crimes suffered. However, data limitations (very few and dispersed positives) did not allow us to examine the above relationships by relationship status.

Nevertheless, the use of the count information together with appropriate count data models is very promising and it can provide many interesting insights not only about the relationship between immigration and victimization but also about the determinants of victimization is general. For instance, we showed evidence that the victimization probability is higher for males because of their higher *exposure*, but once considering the victimized individuals only, females are victimized much more frequently perhaps due to repeated domestic violence. Further analysis is subject to future research, perhaps considering pooling several sweeps from the British Crime Survey in order to increase the sample size, and consequently, the robustness of the estimated relationships.

Table 3.1. BCS Crime Codes[1]

	Category	Code	Description	Valid?
0	**Miscellaneous**	01	Refer to Home Office	
		02	Duplicate victim form	
		96	Invalid Victim Form (e.g. no information/no offence)	
1	**Assault**	11	Serious wounding	✓
		12	Other wounding	✓
		13	Common assault	✓
		14	Other assault outside the survey's coverage	
2	**Attempted assault**	21	Attempted assault	✓
3	**Sexual offences**	31	Rape	✓
		32	Serious wounding with sexual motive	✓
		33	Other wounding with sexual motive	✓
		34	Attempted rape	✓
		35	Indecent assault	✓
		39	Sexual offence outside the survey's coverage	
4	**Personal theft**	41	Robbery	✓
		42	Attempted robbery	✓
		43	Snatch theft from the person	✓
		44	Other theft from the person	✓
		45	Attempted theft from the person	✓
		48	Possibly theft but could have been loss/possibly attempted theft, but not certain	
		49	Other robbery or theft from the person outside the survey's coverage	
5	**Burglary/Theft in a dwelling**	50	Attempted burglary to non-connected domestic garage/outhouse	✓
		51	Burglary in a dwelling (nothing taken)	✓
		52	Burglary in a dwelling (Something taken)	✓
		53	Attempted burglary in a dwelling	✓
		54	Possible attempted burglary (insufficient evidence to be sure)	
		55	Theft in a dwelling	✓
		56	Theft from a meter	✓
		57	Burglary from non-connected domestic garage/outhouse – nothing taken	✓
		58	Burglary from non-connected domestic garage/outhouse – something taken	✓
		59	Other burglary, attempted burglary, theft in a dwelling, falling outside the survey's coverage	

[1] This table is taken by the BCS 2008-09 User Guide pages 19 and 20.

Table 3.1. Continued

6	**Theft**	60	Theft of car/van	✓
		61	Theft from car/van	✓
		62	Theft of motorbike, motorscooter or moped	✓
		63	Theft from motorbike, motorscooter or moped	✓
		64	Theft of pedal cycle	✓
		65	Theft from outside dwelling (excluding theft of milk bottles)	✓
		66	Theft of milk bottles from outside dwelling	
		67	Other theft	✓
		68	Possible theft, possible lost property	
		69	Other theft/attempted theft falling outside survey's coverage	
7	**Attempted theft**	71	Attempted theft of/from car/van	✓
		72	Attempted theft of/from motorcycle, motorscooter or moped	✓
		73	Other attempted theft	✓
8	**Vandalism**	80	Arson	✓
		81	Criminal damage to a motor vehicle (£20 or under)	✓
		82	Criminal damage to a motor vehicle (over £20)	✓
		83	Criminal damage to the home (£20 or under)	✓
		84	Criminal damage to the home (over £20)	✓
		85	Other criminal damage (£20 or under)	✓
		86	Other criminal damage (over £20)	✓
		87	Possibly criminal/possibly accidental damage/ nuisance with no damage	
		88	Attempted criminal damage (no damage actually achieved)	
		89	Other criminal damage outside survey's coverage	
9	**Threats**	91	Threat to kill/assault made against, but not necessarily to respondent	✓
		92	Sexual threat made against, but not necessarily to respondent	✓
		93	Other threat or intimidation made against, but not necessarily to respondent	✓
		94	Threats against others, made to the respondent	✓
		97	Other threats/intimidation outside survey's coverage	

Table 3.2. Count Data Tabulations for each Crime Group

	Acquisitive Crime													
	Total		Inside Burglary		Outside Burglary		Vehicle Theft		Inside Theft		Outside Theft		Other Theft	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%
0	40,738	*86.87*	45,805	*97.68*	46,421	*98.99*	43,832	*93.47*	46,774	*99.75*	45,663	*97.38*	46,068	*98.24*
1	4,646	*9.91*	921	*1.96*	407	*0.87*	2,496	*5.32*	86	*0.18*	1,001	*2.13*	742	*1.58*
2	950	*2.03*	97	*0.21*	43	*0.09*	398	*0.85*	18	*0.04*	148	*0.32*	55	*0.12*
3	296	*0.63*	26	*0.06*	15	*0.03*	99	*0.21*	4	*0.01*	41	*0.09*	16	*0.03*
4	120	*0.26*	12	*0.03*	1	*0.00*	35	*0.07*	4	*0.01*	19	*0.04*	6	*0.01*
5	47	*0.1*	8	*0.02*	0	*0.00*	19	*0.04*	0	*0.00*	5	*0.01*	3	*0.01*
6	29	*0.06*	4	*0.01*	2	*0.00*	1	*0.00*	3	*0.01*	4	*0.01*	1	*0.00*
7	17	*0.04*	3	*0.01*	1	*0.00*	5	*0.01*	1	*0.00*	3	*0.01*	0	*0.00*
8	8	*0.02*	2	*0.00*	0	*0.00*	1	*0.00*	0	*0.00*	1	*0.00*	1	*0.00*
9	5	*0.01*	1	*0.00*	0	*0.00*	9	*0.00*	1	*0.00*	0	*0.00*	0	*0.00*
10+	37	*0.08*	14	*0.01*	3	*0.00*	6	*0.01*	2	*0.00*	8	*0.02*	1	*0.00*

	Criminal Damage						Personal Theft					
	Total *(+ Other, Arson)*		Home		Vehicle		Total		Mugging		Theft	
	N	%	N	%	N	%	N	%	N	%	N	%
0	43,331	*92.40*	45,733	*97.5*	44,421	*94.73*	46,292	*99.00*	46,630	*99.00*	46,549	*99.30*
1	2,418	*5.16*	776	*1.65*	1,776	*3.79*	554	*1.18*	228	*0.49*	333	*0.71*
2	603	*1.29*	190	*0.41*	409	*0.87*	33	*0.07*	23	*0.05*	10	*0.02*
3	264	*0.56*	79	*0.17*	161	*0.34*	4	*0.01*	2	*0.00*	1	*0.00*
4	105	*0.22*	33	*0.07*	48	*0.10*	2	*0.00*	3	*0.01*	0	*0.00*
5	46	*0.10*	12	*0.03*	32	*0.07*	3	*0.01*	4	*0.01*	0	*0.00*
6	41	*0.09*	24	*0.05*	18	*0.04*	3	*0.01*	2	*0.00*	0	*0.00*
7	9	*0.02*	2	*0.00*	3	*0.01*	1	*0.00*	0	*0.00*	0	*0.00*
8	6	*0.01*	4	*0.01*	2	*0.00*	0	*0.00*	0	*0.00*	0	*0.00*
9	8	*0.02*	1	*0.00*	1	*0.00*	0	*0.00*	0	*0.00*	0	*0.00*
10+	62	*0.01*	39	*0.08*	22	*0.05*	1	*0.00*	1	*0.00*	0	*0.00*

Table 3.3. Descriptive Statistics

Variables		Mean			Min	Max	Mis
		All	Native	Immigrant			
Personal Crime Variables							
Violence by Strangers (Binary)		0.011	0.011	0.013	0	1	
Violence by Strangers (Count)		0.015 (0.20)	0.015 (0.20)	0.018 (0.20)	0	11	
Violence by Acquaintances (Binary)		0.010	0.011	0.006	0	1	
Violence by Acquaintances (Count)		0.027 (0.93)	0.027 (0.85)	0.034 (1.47)	0	97	
Domestic Violence (Binary)		0.005	0.005	0.003	0	1	
Domestic Violence (Count)		0.026 (1.30)	0.028 (1.37)	0.007 (0.17)	0	194	
Mugging (Binary)		0.006	0.005	0.008			
Mugging (Count)		0.009 (0.46)	0.009 (0.48)	0.010 (0.12)	0	97	
Other Personal Theft (Binary)		0.007	0.007	0.011			
Other Personal Theft (Count)		0.008 (0.09)	0.007 (0.09)	0.012 (0.112)	0	3	
Household Crime Variables							
Inside Burglary (Binary)		0.023	0.023	0.030	0	1	
Inside Burglary (Count)		0.046 (1.20)	0.044 (1.17)	0.060 (1.48)	0	100	
Outside Burglary (Binary)		*0.010*	*0.010*	*0.007*	0	1	
Outside Burglary (Count)		0.014 (0.47)	0.015 (0.50)	0.010 (0.15)	0	97	
Vehicle Theft (Binary)		0.065	0.064	0.076	0	1	
Vehicle Theft (Count)		0.085 (0.40)	0.083 (0.40)	0.099 (0.41)	0	20	
Inside, Outside, & Other Thefts (Binary)		0.047	0.047	0.046	0	1	
Inside, Outside, & Other Thefts (Count)		0.073 (1.10)	0.074 (1.12)	0.068 (0.94)	0	98	
Home Criminal Damage (Binary)		0.025	0.025	0.022	0	1	
Home Criminal Damage (Count)		0.075 (1.72)	0.077 (1.74)	0.057 (1.48)	0	97	
Vehicle Criminal Damage (Binary)		0.053	0.052	0.055	0	1	
Vehicle Criminal Damage (Count)		0.092 (1.03)	0.090 (0.96)	0.106 (1.53)	0	97	
Respondent's Characteristics							
Immigrant		0.095			0	1	
Age		50.45 (18.58)	51.01 (18.64)	45.17 (17.16)	16	101	66
Gender (female)	Male	0.454	0.455	0.444	0	1	
Marital Status	Married	0.476	0.470	0.527	0	1	20
	Cohabiting	0.088	0.089	0.074	0	1	
	Single	0.204	0.204	0.209	0	1	
	Widowed	0.115	0.119	0.071	0	1	
	Divorced	0.087	0.090	0.067	0	1	
	Separated	0.030	0.027	0.054	0	1	

Table 3.3. Continued

Variables		Mean			Min	Max	Mis
		All	Native	Immigrant			
Employment Status	Employed	0.562	0.558	0.605	0	1	64
	Unemployed	0.017	0.016	0.023	0	1	
	Inactive Student	0.002	0.002	0.004	0	1	
	Inactive Retired	0.281	0.292	0.176	0	1	
	Inactive Other	0.117	0.112	0.157	0	1	
Education	None	0.283	0.287	0.250	0	1	81
	O-level / gcse	0.199	0.208	0.112	0	1	
	A-lavel /Apprent.	0.170	0.176	0.113	0	1	
	Degree /Diploma	0.304	0.289	0.449	0	1	
	Other	0.043	0.040	0.076	0	1	
Ethnic Group	White	0.933	0.976	0.528	0	1	7
	Black	0.018	0.006	0.133	0	1	
	Asian	0.031	0.010	0.233	0	1	
	Chinese / Other	0.012	0.004	0.086	0	1	
	Mixed	0.006	0.004	0.019	0	1	
Hhd Ref Person's Characteristics							
Immigrant		0.095			0	1	
Age		52.60 (17.13)	53.18 (17.10)	47.08 (16.44)	16	101	105
Gender (female)	Male	0.624	0.624	0.622	0	1	
Marital Status	Married	0.513	0.511	0.538	0	1	23
	Cohabiting	0.090	0.092	0.070	0	1	
	Single	0.150	0.147	0.181	0	1	
	Widowed	0.118	0.123	0.074	0	1	
	Divorced	0.095	0.097	0.076	0	1	
	Separated	0.033	0.030	0.061	0	1	
Employment Status	Employed	0.610	0.603	0.675	0	1	65
	Unemployed	0.011	0.011	0.016	0	1	
	Inactive Student	0.009	0.007	0.029	0	1	
	Inactive Retired	0.280	0.291	0.177	0	1	
	Inactive Other	0.089	0.087	0.102	0	1	
Hhd Caracteristics							
Tenure Type (Renters)	Owners	0.702	0.719	0.543	0	1	127
Condition (Bad)	Indifferent	0.219	0.213	0.284	0	1	2746
	Good	0.416	0.417	0.405	0	1	
	Very Good	0.332	0.339	0.264	0	1	
Relative Condition (Same)	Better	0.085	0.085	0.077	0	1	3059
	Worse	0.062	0.062	0.070	0	1	
Accommodation Type	Detached	0.265	0.273	0.179	0	1	2549
	Semi Detached	0.332	0.339	0.265	0	1	
	Terrace	0.280	0.277	0.316	0	1	
	Flat/ Maisonette	0.119	0.107	0.237	0	1	
	Other	0.005	0.005	0.003			
Location (Other)	Main Road	0.142	0.142	0.139	0	1	
	Side Road	0.536	0.535	0.548	0	1	
Number of Adults		1.898 (1.898)	1.881 (0.809)	2.061 (0.984)	1	10	
Lone Parent		0.051	0.051	0.054	0	1	107

Table 3.3. Continued

Variables		Mean			Min	Max	Mis
		All	Native	Immigrant			
Hours Away		4.587	4.577	4.682	1	6 (index)	127
Years Home		4.902	4.996	4.015	1	7 (index)	4
Years Area		5.475	5.588	4.401	1	7 (index)	1
Neighbor Watching Program		0.272	0.275	0.242	0	1	10743
Income	under £10,000	0.202	0.201	0.205	0	1	10026
	£10,000-£19,999	0.224	0.227	0.199	0	1	
	£20,000-£29,999	0.175	0.177	0.157	0	1	
	£30,000-£39,999	0.135	0.136	0.133	0	1	
	£40,000-£49,999	0.095	0.096	0.089	0	1	
	£50,000 or more	0.153	0.150	0.187	0	1	
	nothing	0.016	0.014	0.030	0	1	
Number of Cars		1.265 (0.924)	1.284 (0.925)	1.091 (0.894)	0	4(+)	
Motorcycle		0.067	0.070	0.039	0	1	
Bicycle		0.444	0.452	0.370	0	1	
Area Characteristics							
Regions	North East	0.066	0.070	0.026	0	1	
	North West	0.118	0.122	0.076	0	1	
	Yorkshire	0.091	0.095	0.060	0	1	
	East Midlands	0.111	0.114	0.088	0	1	
	West Midlands	0.100	0.101	0.091	0	1	
	East of England	0.130	0.129	0.133	0	1	
	London	0.077	0.055	0.290	0	1	
	South East	0.111	0.110	0.123	0	1	
	South West	0.106	0.109	0.076	0	1	
	Wales	0.091	0.096	0.038	0	1	
Urban		0.744	0.730	0.880	0	1	
Inner City		0.079	0.069	0.167	0	1	
Deprived		5.232 (2.824)	5.161 (2.80)	5.911 (2.93)	1	10	
10^{th} percentile		0.109	0.110	0.096			
20^{th}		0.108	0.110	0.083			
30^{th}		0.115	0.118	0.084			
40^{th}		0.110	0.113	0.080			
50^{th}		0.098	0.100	0.073			
60^{th}		0.104	0.104	0.107			
70^{th}		0.098	0.097	0.105			
80^{th}		0.090	0.088	0.115			
90^{th}		0.088	0.084	0.126			
100^{th} percentile		0.082	0.077	0.126			

Standard deviations are presented in parentheses.

Table 3.4. The Risk of Inside Burlglury plus Attempts

Inside Burgury + Attempts	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
HRP IMMIGRANT	0.123***	*0.040*	0.048	*0.043*	0.004	*0.044*	-0.022	*0.046*
Deprived			0.046***	*0.005*	0.025***	*0.005*	0.027***	*0.006*
London			0.024	*0.048*	0.010	*0.049*	-0.017	*0.052*
Urban			0.237***	*0.036*	0.203***	*0.036*	0.215***	*0.037*
Inner City			-0.010	*0.045*	-0.045	*0.046*	-0.040	*0.047*
Hrp Age					-0.012***	*0.001*	-0.008***	*0.001*
Hrp Male					-0.037	*0.029*	-0.021	*0.030*
Hrp Married					-0.111***	*0.030*	-0.101***	*0.034*
Hrp Employed					-0.146***	*0.033*	-0.106***	*0.038*
Owners					-0.136***	*0.03*	-0.103***	*0.034*
Condition, Type Location, Num Adults, Lone Parent, Hours Unoccupied Years in home/area, Watching neighborhood, Income, Education							√	
Constant	-2.004***	*0.013*	-2.448***	*0.040*	-1.474***	*0.075*	-1.510***	*0.135*
Log Likelihood	-5,165.03		-5,061,08		-4,897.71		-4,822.63	
R^2	0.0009		0.0193		0.0479		0.0615	
N	46,810		46,810		46,588		46,525	

Table 3.5. The Risk of Outside Burlglury plus Attempts

Outside Burgury + Attempts	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
HRP IMMIGRANT	-0.160**	*0.068*	-0.173**	*0.072*	-0.180**	*0.073*	-0.182**	*0.076*
Deprived			0.038***	*0.007*	0.042***	*0.007*	0.043***	*0.008*
London			-0.091	*0.075*	-0.082	*0.076*	-0.123	*0.078*
Urban			0.092**	*0.044*	0.082**	*0.044*	0.098**	*0.045*
Inner City			-0.057	*0.067*	-0.050	*0.067*	-0.045	*0.067*
Hrp Age					-0.005***	*0.001*	-0.003*	*0.002*
Hrp Male					-0.030	*0.040*	-0.006	*0.043*
Hrp Married					0.062	*0.040*	0.021	*0.048*
Hrp Employed					-0.008	*0.050*	-0.033	*0.054*
Owners					0.139***	*0.046*	0.088*	*0.051*
Condition, Type Location, Num Adults, Lone Parent, Hours Unoccupied Years in home/area, Watching neighborhood, Income, Education							√	
Constant	-2.310***	*0.017*	-2.580***	*0.048*	-2.449***	*0.106*	-2.972***	*0.190*
Log Likelihood	-2,636.27		-2,613.27		-2582.01		-2535.72	
R^2	0.0012		0.0099		0.0157		0.0298	
N	46,810		46,810		46,588		46,525	
Assimilation								
Immigrant	-0.286**	*0.116*	-0.322***	*0.121*	-0.397***	*0.127*	-0.376***	*0.135*
Immigrant's number of years in Country	0.003	*0.003*	0.005	*0.003*	0.007**	*0.003*	0.006*	*0.004*
Immigrant (plus hrpage)	-0.404***	*0.118*	-0.419***	*0.123*				
Immigrant's no. years in Country (plus hrpage)	0.007***	*0.003*	0.008**	*0.003*				

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level

Table 3.6. The Risk of Personal Theft

Personal Theft (Incl. Attempts)	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
Immigrant	0.190***	*0.047*	0.113*	*0.059*	0.036	*0.060*	0.030	*0.060*
Male			-0.082**	*0.032*	-0.082**	*0.033*	-0.076**	*0.035*
Age 26 – 35			-0.339***	*0.053*	-0.341***	*0.054*	-0.211***	*0.059*
Age 36 – 45			-0.472***	*0.054*	-0.476***	*0.055*	-0285***	*0.065*
Age 45 – 56			-0.470***	*0.057*	-0.474***	*0.058*	-0.273***	*0.069*
Age 56 – plus			-0.479***	*0.046*	-0.477***	*0.046*	-0.196***	*0.073*
Black			0.186*	*0.097*	0.064	*0.099*	0.012	*0.100*
Asian & Other			-0.117	*0.083*	-0.159*	*0.084*	-0.163*	*0.087*
Mixed			-0.142	*0.202*	-0.217	*0.202*	-0.389*	*0.228*
Deprived			0.022***	*0.007*	0.031***	*0.007*	0.027***	*0.008*
Urban			0.138***	*0.043*	0.076*	*0.045*	0.073	*0.045*
Inner City			0.191***	*0.052*	0.129**	*0.053*	0.111**	*0.054*
North East					-0.441***	*0.083*	-0.402***	*0.085*
North West					-0.373***	*0.068*	-0.319***	*0.069*
Yorkshire					-0.364***	*0.073*	-0.292***	*0.074*
East Midlands					-0.361***	*0.070*	-0.304***	*0.071*
West Midlands					-0.322***	*0.069*	-0.259***	*0.070*
East of England					-0.311***	*0.067*	-0.259***	*0.069*
South East					-0.156**	*0.065*	-0.118*	*0.066*
South West					-0.391***	*0.074*	-0.334***	*0.075*
Wales					-0.504***	*0.083*	-0.440***	*0.084*
Education, Marital, Employment, Tenure, Income							√	
Constant	-2.254***	*0.017*	-2.083***	*0.062*	-1.760***	*0.082*	-2.062***	*0.117*
Log Likelihood	-3,198.49		-3,090.00		-3577.29		-2,974.69	
R^2	0.0024		0.0362		0.0467		0.0662	
N	46,827		46,820		46,820		46,567	
Pr(Y=1|Immigrant=1)	0.0195		0.0116		0.0093		0.0040	
Pr(Y=1|Immigrant=0)	0.0121		0.0086		0.0084		0.0036	
Diff	0.0074***		0.0030*		0.0009		0.0003	
(se)	(0.0021)		(0.0018)		(0.0015)		(0.0007)	
Ratio	1.612		1.355		1.104		1.094	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.7. The Risk of Violent Victimization

Total Assault (Incl. Attempts)	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
Immigrant	-0.068	*0.044*	-0.165***	*0.046*	-0.157***	*0.049*	-0.151***	*0.051*
Male			0.179***	*0.026*	0.185***	*0.026*	0.240***	*0.029*
Age 20 – 24			-0.195***	*0.056*	-0.216***	*0.057*	-0.200***	*0.063*
Age 25 – 34			-0.426***	*0.049*	-0.432***	*0.049*	-0.354***	*0.061*
Age 35 – 44			-0.671***	*0.049*	-0.663***	*0.049*	-0.542***	*0.064*
Age 45 – 54			-0.824***	*0.053*	-0.813***	*0.053*	-0.659***	*0.070*
Age 55 – 64			-1.119***	*0.059*	-1.107***	*0.06*	-0.924***	*0.079*
Age 65 – 74			-1.361***	*0.074*	-1.353***	*0.075*	-1.139***	*0.098*
Age 75 – plus			-1.897***	*0.138*	-1.889***	*0.139*	-1.757***	*0.168*
Deprived					0.025***	*0.005*	0.010*	*0.006*
Urban					0.059***	*0.034*	0.066*	*0.035*
Inner City					0.023	*0.048*	0.010	*0.049*
Regions					√		√	
Education, Marital, Employment, Tenure, Income, Lone Parent, Hhd members							√	
Constant	-1.948***	*0.013*	-1.306***	*0.043*	-1.654***	*0.078*	-1.861***	*0.127*
Log Likelihood	-5,536.52		-4,989.29		-4,959.87		-4,777.29	
R^2	0.0002		0.1002		0.1055		0.1275	
N	46,827		46,827		46,827		46,532	
Pr(Y=1|Immigrant=1)	0.0219		0.0248		0.0228		0.0265	
Pr(Y=1|Immigrant=0)	0.0258		0.0361		0.0327		0.0372	
Diff	-0.0038* (0.0023)		-0.0113*** (0.0028)		-0.0100*** (0.0029)		-0.0107*** (0.0036)	
Ratio	0.8512		0.6872		0.6971		0.7121	

Table 3.8. The Risk of Violent Victimization – Including Ethnic Group Dummies

Total Assault (Incl. Attempts)	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
Immigrant	-0.093*	*0.054*	-0.114**	*0.053*	-0.103**	*0.055*	-0.093*	*0.057*
Black	0.005	*0.102*	-0.130	*0.105*	-0.149	*0.108*	-0.216**	*0.110*
Asian	0.053	*0.081*	-0.156**	*0.080*	-0.174**	*0.081*	-0.170**	*0.087*
Chinese or Other	0.108	*0.115*	-0.022	*0.123*	-0.033	*0.124*	-0.037	*0.126*
Mixed	0.242*	*0.139*	-0.020	*0.145*	-0.035	*0.146*	-0.116	*0.153*
Log Likelihood	-5,534.43		-4,980.17		-4,950.12		-4,769.33	
R^2	0.0006		0.1006		0.1061		0.1283	
N	46,820		46,818		46,818		46,526	
Pr(Y=1|Immigrant=1)	0.0205		0.0281		0.0262		0.0304	
Pr(Y=1|Immigrant=0)	0.0256		0.0363		0.0331		0.0373	
Diff	-0.0051* (0.0023)		-0.0082*** (0.0035)		-0.0069** (0.0034)		-0.0070* (0.0041)	
Ratio	0.8015		0.7740		0.7919		0.8132	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.9. The Risk of Domestic Violence

Domestic (Incl. Attempts)	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
Immigrant	-0.177*	*0.091*	-0.248**	*0.100*	-0.219**	*0.107*	-0.207*	*0.112*
Male			-0.438***	*0.056*	-0.419***	*0.057*	-0.337***	*0.061*
Age 26 – 35			0.123*	*0.069*	0.167**	*0.074*	0.114	*0.081*
Age 36 – 45			-0.131*	*0.073*	-0.121	*0.082*	-0.138	*0.09*
Age 45 – 56			-0.346***	*0.087*	-0.335***	*0.104*	-0.283**	*0.115*
Age 56 – plus			-0.794***	*0.090*	-0.682***	*0.115*	-0.646***	*0.14*
Deprived			0.027***	*0.010*	0.017	*0.010*	-0.003	*0.011*
Urban			-0.005	*0.060*	-0.015	*0.062*	0.003	*0.063*
Inner City			0.086	*0.085*	0.090	*0.087*	0.065	*0.089*
Regions			√		√		√	
Cohabiting					0.129	*0.097*	0.136	*0.098*
Single					0.363***	*0.073*	0.238***	*0.084*
Widowed					-0.093	*0.195*	-0.204	*0.194*
Divorced					0.621***	*0.084*	0.450***	*0.096*
Separated					0.882***	*0.092*	0.711***	*0.106*
Education, Employment, Tenure, Income, Lone Parent, Hhd members							√	
Constant	-2.556***	*0.023*	-2.668***	*0.150*	-2.954***	*0.166*	-2.673***	*0.226*
Log Likelihood	-1,492.38		-1,345.71		-1,283.47		-1,232.80	
R^2	0.0014		0.0996		0.1412		0.1684	
N	46,827		46,827		46,811		46,532	
Pr(Y=1|Immigrant=1)	0.0031		0.0041		0.0017		0.0011	
Pr(Y=1|Immigrant=0)	0.0053		0.0084		0.0034		0.0022	
Diff	-0.0022**		-0.0042**		-0.0017**		-0.0011*	
(se)	(0.001)		(0.002)		(0.001)		(0.001)	
ratio	0.593		0.494		0.505		0.511	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.10. The Risk of Victimization suffered by Acqaintances

By Acquaintances (Incl. Attempts)	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
Immigrant	-0.209***	*0.070*	-0.299***	*0.078*	-0.274***	*0.081*	-0.165*	*0.087*
Male			0.198***	*0.037*	0.219***	*0.040*	0.222***	*0.041*
Age 26 – 35			-0.384***	*0.050*	-0.225***	*0.056*	-0.224***	*0.056*
Age 36 – 45			-0.589***	*0.053*	-0.391***	*0.064*	-0.398***	*0.064*
Age 45 – 56			-0.722***	*0.061*	-0.484***	*0.077*	-0.494***	*0.077*
Age 56 – plus			-1.18***	*0.064*	-0.896***	*0.095*	-0.91***	*0.096*
Deprived			0.031***	*0.008*	0.011	*0.008*	0.013	*0.008*
Urban			-0.022	*0.047*	-0.012	*0.049*	-0.005	*0.049*
Inner City			0.009	*0.068*	-0.003	*0.069*	-0.003	*0.070*
Regions			√		√		√	
Education, Marital, Employment, Tenure, Income, Lone Parent, Hhd members					√		√	
Black							-0.193	*0.162*
Asian							-0.582***	*0.184*
Other							-0.188	*0.200*
Mixed							-0.056	*0.189*
Constant	-2.300***	*0.018*	-2.121***	*0.105*	-2.348***	*0.161*	-2.332***	*0.163*
Log Likelihood	-2,675.70		-2,392.23		-2,297.46		-2,289.76	
R^2	0.0019		0.1076		0.1301		0.1330	
N	46,827		46,827		46,532		46,526	
$Pr(Y=1 \mid Immigrant=1)$	0.0060		0.0049		0.0032		0.0046	
$Pr(Y=1 \mid Immigrant=0)$	0.0107		0.0112		0.0072		0.0074	
Diff	-0.0047*** (0.0013)		-0.0063*** (0.0016)		-0.0039*** (0.0013)		-0.0028** (0.0014)	
Ratio	0.564		0.438		0.451		0.625	

Table 3.11. The Risk of Victimization suffered by Strangers

By Strangers (plus Attempts)	1		2		3		4	
Probit	coeff	se	coeff	se	coeff	se	coeff	se
Immigrant	0.084	*0.053*	0.010	*0.056*	-0.004	*0.059*	-0.009	*0.061*
Male			0.441***	*0.038*	0.444***	*0.038*	0.411***	*0.041*
Age 26 – 35			-0.341***	*0.05*	-0.339***	*0.051*	-0.279***	*0.057*
Age 36 – 45			-0.518***	*0.052*	-0.506***	*0.052*	-0.402***	*0.062*
Age 45 – 56			-0.672***	*0.06*	-0.658***	*0.061*	-0.543***	*0.074*
Age 56 – plus			-1.029***	*0.058*	-1.012***	*0.058*	-0.795***	*0.088*
Deprived					0.011	*0.007*	0.015*	*0.008*
Urban					0.138***	*0.048*	0.136***	*0.049*
Inner City					-0.015	*0.066*	-0.007	*0.067*
Regions					√		√	
Other regressors (as for by acquaintance crime)							√	
Constant	-2.304***	*0.018*	-2.013***	*0.041*	-2.223***	*0.094*	-2.565***	*0.154*
Log Likelihood	-2,806.62		-2,534.25		-2,525.73		-2,456.38	
R^2	0.0004		0.0974		0.1005		0.1127	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.12. A Trivariate Probit Model for Violent Victimization

Trivariate Probit (300 draws)	1st Equation Domestic		2nd Equation By Acquaintances		3rd Equation By Strangers	
	coeff	se	coeff	se	coeff	se
Immigrant	-0.213**	*0.089*	-0.298***	*0.077*	-0.013	*0.059*
Male	-0.433***	*0.054*	0.195***	*0.037*	0.445***	*0.038*
Age 26 – 35	0.118*	*0.068*	-0.382***	*0.050*	-0.338***	*0.051*
Age 36 – 45	-0.142**	*0.072*	-0.586***	*0.053*	-0.504***	*0.052*
Age 45 – 56	-0.324***	*0.081*	-0.724***	*0.061*	-0.665***	*0.061*
Age 56 – plus	-0.805***	*0.091*	-1.178***	*0.063*	-1.008***	*0.058*
Deprived	0.027***	*0.010*	0.030***	*0.008*	0.011	*0.007*
Urban	0.002	*0.060*	-0.023	*0.047*	0.140***	*0.048*
Inner City	0.086	*0.084*	0.004	*0.068*	-0.010	*0.066*
North East	0.230	*0.152*	0.271***	*0.101*	0.088	*0.093*
North West	0.255*	*0.135*	0.107	*0.097*	0.019	*0.083*
Yorkshire	0.421***	*0.133*	0.177*	*0.099*	-0.023	*0.090*
East Midlands	0.455***	*0.131*	0.166**	*0.098*	0.112	*0.082*
West Midlands	0.344***	*0.134*	0.237**	*0.096*	0.021	*0.085*
East of England	0.211*	*0.136*	0.086	*0.099*	0.027	*0.083*
South East	0.306**	*0.136*	0.206**	*0.099*	0.051	*0.085*
South West	0.404***	*0.136*	0.069	*0.104*	0.046	*0.087*
Wales	0.391***	*0.139*	0.183*	*0.103*	0.033	*0.091*
Constant	-2.660***	*0.144*	-2.118***	*0.105*	-2.231***	*0.094*
Log Likelihood	-6,254.93					
N	46,827					
Rho between 1st & 2nd	0.153***	*0.058*	LR test for Rho12=Rho13=Rho23=0			
Rho between 1nd & 3rd	0.013	*0.059*	chi2(3)=17.48			
Rho between 2nd & 3rd	0.142***	*0.046*	Prob>chi2=0.0006			

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.13. Comparisons Between Face-to-Face and Self-Reports

Self-Completion Domestic	Face-to-face Simple Probit (16 – 59)		No Sample Selection Correction		Correcting for Sample Selection (given acceptance)		Correcting for Sample Selection (16 - 59)	
Crime Equation								
	Coeff	se	Coeff	se	Coeff	se	Coeff	se
Immigrant	-0.284***	*0.103*	-0.223***	*0.062*	-0.214***	*0.074*	-0.258***	*0.066*
Male	-0.434***	*0.057*	-0.191***	*0.032*	-0.190***	*0.032*	-0.192***	*0.031*
Deprived	0.028***	*0.010*	0.035***	*0.006*	0.037***	*0.008*	0.032***	*0.007*
Urban	0.011	*0.063*	0.009	*0.039*	0.008	*0.039*	0.009	*0.039*
Inner City	0.061	*0.088*	0.028	*0.057*	0.028	*0.057*	0.026	*0.057*
Age & Regional dummies	√		√		√		√	
Constant	-2.667***	*0.152*	-1.824***	*0.086*	-1.820***	*0.089*	-1.844***	0.086
Selection Equation								
Immigrant					-0.235***	*0.031*	-0.440***	*0.026*
Male					-0.048**	*0.019*	-0.054***	*0.017*
Deprived					-0.050***	*0.004*	-0.029***	*0.004*
Urban					0.013	*0.024*	0.016	*0.022*
Inner City					0.009	*0.036*	0.004	*0.032*
Age & Regional dummies					√		√	
Language Difficulties					-0.877***	*0.047*		
Other Present							-0.159***	*0.019*
No qualification							-0.632***	*0.022*
Constant					1.818***	*0.055*	1.451***	*0.048*
Rho (p-value from Wald Test)					-0.077	(0.816)	0.233	(0.215)
Log Likelihood	-1,254.06		-3,650.27		-14,448.08		-17,519.96	
N Total	30,711		24,363		28,339		30,324	
N Uncensored					24,344		24,346	
N Censored					3,995		5,978	
Pr(Y=1|Immigrant=1)	0.0040		0.0236		0.0252		0.0197	
Pr(Y=1|Immigrant=0)	0.0090		0.0392		0.0406		0.0358	
Diff	-0.0050***		-0.0155***		-0.0155***		-0.0161***	
(se)	(0.0018)		(0.0039)		(0.004)		(0.0035)	
Ratio	0.447		0.604		0.619		0.550	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.14.A. The Presence of the Partner – Probit Estimates⊕

	Coeff	se	Coeff	se	Coeff	se
Domestic Face-to-face	1		2		3	
Immigrant	-0.227**	*0.102*	-0.281**	*0.110*	-0.275**	*0.119*
Partner Present	-0.408***	*0.093*	-0.314***	*0.100*	-0.125	*0.113*
Immigrant & Partner Present	0.446**	*0.235*	0.335	*0.254*	0.377	*0.256*
Domestic Self-completion	1		2		3	
Immigrant	-0.187***	*0.062*	-0.209***	*0.066*	-0.186***	*0.070*
Partner Present	-0.157***	*0.054*	-0.118**	*0.056*	0.067	*0.061*
Immigrant & Partner Present	0.006	*0.183*	-0.067	*0.186*	-0.023	*0.190*
Acquaintance	1		2		3	
Immigrant	-0.204***	*0.075*	-0.285***	*0.083*	-0.249***	*0.084*
Partner Present	-0.237***	*0.058*	-0.131**	*0.062*	0.005	*0.068*
Immigrant & Partner Present	0.007	*0.213*	-0.049	*0.226*	-0.021	*0.226*

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.14.B. The Presence of the Partner – Predictions

PREDICTIONS	Pr(y=1)	*Difference (s.e) Ratio*	Pr(y=1)	*Difference (s.e) Ratio*	Pr(y=1)	*Difference (s.e) Ratio*
Domestic Face-to-face	1		2		3	
Immigrant & Partner Present	0.0034	*0.0004* *(0.0022)*	0.0089	*0.0005*	0.0069	*0.0036* *(0.0043)*
Immigrant & **NO** Partner Present	0.0031	*1.119*	0.0084	*1.060*	0.0033	*2.075*
Native & Partner Present	0.0017	*-0.0043**** *(0.0006)*	0.0077	*-0.0097**** *(0.0029)*	0.0052	*-0.0022* *(0.0019)*
Native & **NO** Partner Present	0.0060	*0.291*	0.0174	*0.440*	0.0074	*0.702*
Domestic Self-completion	1		2		3	
Immigrant & Partner Present						
Immigrant & **NO** Partner Present						
Native & Partner Present						
Native & **NO** Partner Present						
Acquaintance	1		2		3	
Immigrant & Partner Present	0.0034	*-0.0033* *(0.0024)*	0.0032	*-0.0022* *(0.0023)*	0.0033	*-0.0002* *(0.0022)*
Immigrant & **NO** Partner Present	0.0067	*0.513*	0.0054	*0.588*	0.0035	*0.953*
Native & Partner Present	0.0061	*-0.0055**** *(0.0011)*	0.0083	*-0.0035**** *(0.1185)*	0.0073	*0.0001* *(0.0014)*
Native & **NO** Partner Present	0.0116	*0.524*	0.0118	*0.704*	0.0072	*1.014*

⊕ Specification 2 also includes age, gender, and area dummies. Specification 3 further includes marital, education, and employment status dummies.

Table 3.15. Tabulation of Racially Motivated Crime by Relationship Type

	Racially Motivated Crime			
	No		***Yes***	
Domestic	237	*(99.58%)*	1	*(0.42%)*
By Acquaintances	481	*(100.0%)*	0	*(0.00%)*
By Strangers	472	*(92.73%)*	37	*(7.27%)*
Immigrants	42	*(71.19%)*	17	*(28.81%)*
Natives	430	*(95.56%)*	20	*(4.44%)*

Table 3.16. Mean Comparison of Racially Motivated Crime by Relationship Type

Mean Comparisons	Immigrants	Natives	Diff	Ratio
Crime by Strangers				
Without controlling for RMC	0.0132	0.0106	0.0026	1.244
After controlling for RMC	0.0094	0.0102	-0.0007	0.930
Acquaintances	0.0060	0.0107	-0.0047	0.564***
Domestic	0.0031	0.0053	-0.0022	0.593*

Table 3.17. Probit Models before and after controlling for Racially Motivated Crime

	Strangers (No Control for RMC)		Strangers (Control for RMC)		Acquaintances		Domestic	
Probit	Coeff	se	Coeff	se	Coeff	se	Coeff	se
Immigrant	-0.004	*0.059*	-0.122*	*0.067*	-0.299***	*0.078*	-0.248**	*0.100*
Male	0.444***	*0.038*	0.438***	*0.039*	0.198***	*0.037*	-0.438***	*0.056*
Age 26 – 35	-0.339***	*0.051*	-0.341***	*0.052*	-0.384***	*0.05*	0.123*	*0.069*
Age 36 – 45	-0.506***	*0.052*	-0.531***	*0.054*	-0.589***	*0.053*	-0.131*	*0.073*
Age 45 – 56	-0.658***	*0.061*	-0.641***	*0.062*	-0.722***	*0.061*	-0.346***	*0.087*
Age 56 – plus	-1.012***	*0.058*	-1.013***	*0.06*	-1.180***	*0.064*	-0.794***	*0.09*
Deprived	0.011	*0.007*	0.005	*0.007*	0.031***	*0.008*	0.027***	*0.01*
Urban	0.138***	*0.048*	0.141***	*0.049*	-0.022	*0.047*	-0.005	*0.06*
Inner City	-0.015	*0.066*	-0.013	*0.069*	0.009	*0.068*	0.086	*0.085*
Regions◊	√		√		√		√	
Constant	-2.223***	*0.094*	-2.176***	*0.095*	-2.121***	*0.105*	-2.668***	*0.150*
Log Likelihood	-2,525.73		-2,375.9717		-2,392.23		-1,345.71	
R^2	0.1005		0.0997		0.1076		0.0996	
N	46,827		46,827		46,827		46,827	
Pr(Y=1|Immigrant=1)	0.0189		0.0129		0.0049		0.0041	
Pr(Y=1|Immigrant=0)	0.0191		0.0175		0.0112		0.0084	
Diff	-0.0002		-0.0046**		-0.0063***		-0.0042**	
(se)	(0.0027)		(0.0023)		(0.0016)		(0.0020)	
Ratio	0.990		0.736		0.438		0.494	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

◊ Regions' effect is jointly insignificant for crime by strangers.

Table 3.18. Network Effects and Assimilation Patterns

	Linear Trend (Acquaintnaces) (1)		Quadratic Trend (Acquaintances) (2)		Dummies (Acquaintances) (3)		Linear Trend (Domestic) (4)		Quadratic Trend (Strangers NO RMC) (5)	
Probit	Coeff	se	Coeff	se	Coeff	se	Coeff	se	Coeff	se
Immigrant	-0.405***	*0.106*	-0.646***	*0.165*			-0.587***	*0.157*	-0.342**	*0.151*
Number of Years in Country	0.006*	*0.004*	0.038**	*0.017*			0.015***	*0.005*	0.029*	*0.015*
Number of Years in Country^2			-0.0006*	*0.0004*					-0.0006**	*0.0003*
Immigrant 1 – 10					-0.388***	*0.114*				
Immigrant 11 - 20					-0.315*	*0.169*				
Immigrant 21 - 40					-0.089	*0.136*				
Immigrant 41 more					-0.255	*0.228*				
Male	0.198***	*0.037*	0.199***	*0.037*	0.199***	*0.037*	-0.439***	*0.055*	0.439***	*0.039*
Age 26 – 35	-0.385***	*0.050*	-0.387***	*0.050*	-0.386***	*0.050*	0.124*	*0.069*	-0.343***	*0.052*
Age 36 – 45	-0.594***	*0.053*	-0.598***	*0.053*	-0.595***	*0.053*	-0.142*	*0.073*	-0.537***	*0.055*
Age 45 – 56	-0.730***	*0.061*	-0.732***	*0.061*	-0.730***	*0.061*	-0.371***	*0.087*	-0.647***	*0.062*
Age 56 – plus	-1.191***	*0.065*	-1.184***	*0.064*	-1.184***	*0.064*	-0.833***	*0.092*	-1.011***	*0.060*
Deprived	0.031***	*0.007*	0.031***	*0.007*	0.031***	*0.007*	0.027***	*0.010*	0.005	*0.007*
Urban	-0.022	*0.047*	-0.022	*0.047*	-0.021	*0.047*	-0.005	*0.060*	0.142***	*0.049*
Regions	√		√		√		√		√	
Constant	-2.116***	*0.105*	-2.121***	*0.105*	-2.119***	*0.105*	-2.649***	*0.149*	-2.179***	*0.096*
Log Likelihood	-2,391.29		-2,389.52		-2,391.16		-1,341.63		-2,373.892	
R^2	0.1079		0.1086		0.1079		0.1022		0.1004	
N	46,808		46,808		46,808		46,808		46,771	
Marginal Effects[∇]										
1)Pr(Y=1|Immigrant=0)	0.0190		0.0190		0.0189		0.0170		0.0173	
2)Pr(Y=1|Immigrant=1)	0.0066		0.0033				0.0034		0.0071	
Difference (at 0 years) 2 - 1	-0.0124***	*0.0028*	-0.0157***	*0.0030*			-0.0136***	*0.0033*	-0.0103***	*0.0033*
Difference (after 10 years)	-0.0112***	*0.0026*	-0.0109***	*0.0027*			-0.0117***	*0.0031*	-0.0040	*0.0027*
Difference (after 20 years)	-0.0098***	*0.0025*	-0.0055	*0.0038*			-0.0088***	*0.0030*	0.0008	*0.0041*
Difference (after 30 years)	-0.0082***	*0.0028*	-0.0029	*0.0045*			-0.0047	*0.0034*	0.0013	*0.0049*
Difference (after 40 years)	-0.0064*	*0.0035*	-0.0051	*0.0050*			0.0012	*0.0052*	-0.0029	*0.0044*
Difference (after 50 years)	-0.0043	*0.0049*	-0.0104	*0.0063*			0.0092	*0.0090*	-0.0092**	*0.0045*
Difference (after 60 years)	-0.0019	*0.0068*	-0.0154***	*0.0056*			0.0201	*0.0152*	-0.0142***	*0.0039*
Difference (after 70 years)	0.0009	*0.0093*	-0.0181***	*0.0037*			0.0344	*0.0240*	-0.0166***	*0.0030*
Diff. Immigrant 1 – 10					-0.0121***	*0.0029*				
Diff. Immigrant 11 - 20					-0.0105**	*0.0043*				
Diff. Immigrant 21 - 40					-0.0038	*0.0053*				
Diff. Immigrant 41 more					-0.0091	*0.0062*				

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

[∇] The marginal effects for crime by acquaintances and crime by strangers are calculated for a male, 26 to 35 years old, in an average deprived and urban area in East of England. Note that the average 'number of years in the country' for an immigrant is 26 years. For domestic crime the marginal effects are calculation for a person with characteristics as before, but female.

Table 3.19. Decomposition by Ethnic Group

Immigration & Ethnic Background	Total Assault		Domestic		Acquaintances		Strangers (No RMC)	
	Coeff	se	Coeff	se	Coeff	se	Coeff	se
Immigrant	-0.110*	*0.063*	-0.463**	*0.193*	-0.213**	*0.097*	-0.079	*0.153*
White			-0.063	*0.130*			0.205*	*0.122*
Black	-0.184	*0.173*			0.050	*0.198*		
Asian	-0.236*	*0.122*			-0.670***	*0.255*		
Chinese & Other	0.277	*0.173*			-0.344	*0.37*		
Mixed	-0.065	*0.173*			-0.048	*0.213*		
White & Immigrant			0.331	*0.232*			0.056	*0.174*
Black & Immigrant	0.064	*0.220*			-0.427	*0.321*		
Asian & Immigrant	0.107	*0.165*			0.274	*0.335*		
(Chinese & Other) & Immigrant	-0.535**	*0.244*			0.260	*0.443*		
Mixed & Immigrant	0.166	*0.323*			0.482	*0.388*		
Log-Likelihood	-4,978.06		-1,344.55		-2,382.44		-2,363.39	
R^2	0.1010		0.1003		0.1112		0.1013	

Table 3.20. Decomposition by Location

Immigration & Location	***Regions***								***Deprivation***	
	Total Assault		Domestic		Acquaintances		Strangers (No RMC)		Acquaintances	
	Coeff	se	Coeff	se	Coeff	se	Coeff	se	Coeff	se
Immigrant	-0.324***	*0.115*	-0.271	*0.269*	-0.521***	*0.192*	-0.206	*0.140*	-0.052	*0.166*
North	0.098	*0.066*	0.312**	*0.146*	0.121	*0.094*	-0.046	*0.084*		
Midlands	0.123*	*0.065*	0.372**	*0.146*	0.121	*0.094*	-0.010	*0.082*		
Wales	0.138*	*0.076*	0.378**	*0.161*	0.128	*0.108*	-0.005	*0.099*		
South	0.112	*0.068*	0.346**	*0.15*	0.108	*0.099*	-0.015	*0.086*		
Immigrant & North	0.278*	*0.149*	0.157	*0.330*	0.332	*0.240*	0.129	*0.203*		
Immigrant & Midlands	0.154	*0.140*	-0.115	*0.322*	0.225	*0.232*	0.134	*0.176*		
Immigrant & Wales	0.171	*0.252*	0.205	*0.458*	0.491	*0.351*	-0.104	*0.399*		
Immigrants & South	0.153	*0.158*	0.066	*0.343*	0.177	*0.267*	0.065	*0.207*		
Deprived									0.033***	*0.008*
Deprived*Immigrant									-0.039	*0.024*
Log-Likelihood	-4,993.37		-1,349.82		-2.395.99		-2.378.46		-2.391.09	
R^2	0.0995		0.0968		0.1062		0.0988		0.1080	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.21. The effect of being an Immigrant on perceived Seriousness

Seriousness	1		2		3		4		5	
Ordinal Probit	Coeff	se	Coeff	se	Coeff	se	Coeff	se	Coeff	se
Immigrant	0.270***	*0.037*	0.243***	*0.039*	0.281***	*0.040*	0.063	*0.043*	0.003	*0.051*
Black							0.515***	*0.085*	0.396***	*0.138*
Asian							0.481***	*0.063*	0.371***	*0.096*
Other							0.255**	*0.105*	0.109	*0.172*
Mixed							-0.123	*0.132*	-0.099	*0.145*
Immigrant & Black									0.231	*0.175*
Immigrant & Asian									0.224*	*0.128*
Immigrant & Other									0.278	*0.218*
Immigrant & Mixed									-0.075	*0.341*
Age dummies, Male, Deprived, Urban, Inner City, Regions			√		√		√		√	
Marital, Education, Employment, Income, Tenure					√					
Cutpoint 1	0.443	0.013	0.655	0.063	0.340	0.090	0.689	0.063	0.675	0.064
Cutpoint 2	1.404	0.018	1.629	0.064	1.330	0.091	1.669	0.064	1.656	0.065
Cutpoint 3	2.131	0.029	2.365	0.069	2.082	0.095	2.414	0.069	2.402	0.07
Log Likelihood	-9,774.44		-9,675.22		-9,495.70		-9,626.40		-9,623.63	
N	11,208		11,208		11,148		11,205		11,205	

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level.

Table 3.22. Distribution of Violent Crime

	Total Violence		Violence Zero Truncated	Domestic		Domestic Zero Truncated	By Acquaintance		Acquaintance Zero Truncated	By Stranger		By Stranger Zero Truncated
Observations	46827		1190	46827		238	46827		481	46827		509
Mean	0.0692		2.7218	0.0264		5.2017	0.0274		2.6632	0.0153		1.4106
Std. Deviation	1.613		9.759	1.3048		17.589	0.9262		8.7547	0.1956		1.2464
Variance	2.6018		95.239	1.7026		309.37	0.8578		76.645	0.0382		1.5535
Skewness	71.044		11.803	103.48		7.4549	85.974		9.0086	26.721		4.7561
Percentiles 75%	0		2	0		3	0		2	0		1
90%	0		4	0		6	0		3	0		2
95%	0		6	0		12	0		5	0		3
99%	1		40	0		97	1		50	1		8
	N	%	%	N	%	%	N	%	%	N	%	%
0	45,637	97.46	-	46,589	99.49	-	46,346	98.97	-	46,318	98.91	-
1	842	1.8	70.76	126	0.27	52.94	349	0.75	72.56	412	0.88	80.94
2	164	0.35	13.78	42	0.09	17.65	67	0.14	13.93	60	0.13	11.79
3	64	0.14	5.38	22	0.05	9.24	24	0.05	4.99	15	0.03	2.95
4	29	0.06	2.44	12	0.03	5.04	7	0.01	1.46	4	0.01	0.79
5	21	0.04	1.76	7	0.01	2.94	10	0.02	2.08	6	0.01	1.18
6	23	0.05	1.93	7	0.01	2.94	6	0.01	1.25	6	0.01	1.18
7	2	0.00	0.17	1	0.00	0.42	1	0.00	0.21	0	0.00	0.00
8	5	0.01	0.42	2	0.00	0.84	1	0.00	0.21	1	0.00	0.20
9	1	0.00	0.08	0	0.00	0.00	1	0.00	0.21	0	0.00	0.00
10	13	0.03	1.09	6	0.01	2.52	3	0.01	0.62	4	0.01	0.79
11	1	0.00	0.08	0	0.00	0.00	0	0.00	0.00	0	0.00	0.00
12	2	0.00	0.17	2	0.00	0.84	1	0.00	0.21	1	0.00	0.20
13	1	0.00	0.08	0	0.00	0.00	0	0.00	0.00	0	0.00	0.00
15	1	0.00	0.08	1	0.00	0.42	0	0.00	0.00	0	0.00	0.00
20	6	0.01	0.5	2	0.00	0.84	4	0.01	0.83	0	0.00	0.00
24	1	0.00	0.08	1	0.00	0.42	0	0.00	0.00	0	0.00	0.00
25	0	0.00	0.00	0	0.00	0.00	1	0.00	0.21	0	0.00	0.00
26	1	0.00	0.08	0	0.00	0.00	0	0.00	0.00	0	0.00	0.00
40	2	0.00	0.17	1	0.00	0.42	1	0.00	0.21	0	0.00	0.00
48	1	0.00	0.08	1	0.00	0.42	0	0.00	0.00	0	0.00	0.00
50	1	0.00	0.08	0	0.00	0.00	1	0.00	0.21	0	0.00	0.00
60	1	0.00	0.08	0	0.00	0.00	1	0.00	0.21	0	0.00	0.00
75	1	0.00	0.08	1	0.00	0.42	0	0.00	0.00	0	0.00	0.00
97	5	0.01	0.42	2	0.00	0.84	3	0.01	0.62	0	0.00	0.00
100	1	0.00	0.08	1	0.00	0.42	0	0.00	0.00	0	0.00	0.00
194	1	0.00	0.08	1	0.00	0.42	0	0.00	0.00	0	0.00	0.00

Table 3.23. Poisson, Negative Binomial 2, Censored Poisson

	Logit	Poisson	NegBin2	Censored Poisson				
				5	10	15	20	25
Immigrant	-0.371***	-0.240	-0.360	-0.359***	-0.365**	-0.381**	-0.388**	-0.383**
	0.113	*0.427*	*0.283*	*0.134*	*0.149*	*0.162*	*0.177*	*0.194*
Male	0.440***	-0.382**	-0.314**	0.236***	0.167	0.106	0.055	0.023
	0.060	*0.183*	*0.136*	*0.070*	*0.082*	*0.090*	*0.099*	*0.106*
Age 26 – 35	-0.638***	-0.549**	-0.631***	-0.660***	-0.648***	-0.653***	-0.669***	-0.670***
	0.078	*0.237*	*0.171*	*0.093*	*0.109*	*0.119*	*0.129*	*0.136*
Age 36 – 45	-1.185***	-0.672**	-0.795***	-1.108***	-1.096***	-1.079***	-1.062***	-1.032***
	0.085	*0.283*	*0.207*	*0.101*	*0.117*	*0.131*	*0.147*	*0.157*
Age 45 – 56	-1.605***	-1.702***	-1.773***	-1.709***	-1.772***	-1.802***	-1.829***	-1.829***
	0.103	*0.356*	*0.217*	*0.118*	*0.133*	*0.145*	*0.159*	*0.172*
Age 56 – plus	-2.810***	-3.244***	-3.285***	-2.976***	-3.024***	-3.037***	-3.060***	-3.062***
	0.112	*0.229*	*0.217*	*0.128*	*0.152*	*0.172*	*0.191*	*0.206*
Deprived	0.054***	0.042	0.048*	0.067***	0.071***	0.071***	0.070***	0.071***
	0.012	*0.037*	*0.028*	*0.015*	*0.017*	*0.019*	*0.021*	*0.023*
Urban	0.127	-0.170	-0.365*	0.009	-0.013	-0.037	-0.056	-0.052
	0.079	*0.302*	*0.217*	*0.093*	*0.110*	*0.124*	*0.139*	*0.148*
Inner City	0.034	-0.022	0.126	0.035	0.057	0.060	0.072	0.047
	0.106	*0.211*	*0.177*	*0.127*	*0.149*	*0.160*	*0.174*	*0.177*
North East	0.530***	0.456	0.306	0.333*	0.361*	0.385*	0.412*	0.412*
	0.163	*0.280*	*0.244*	*0.187*	*0.209*	*0.221*	*0.236*	*0.238*
North West	0.265*	0.270	0.205	0.264	0.267	0.260	0.257	0.255
	0.153	*0.230*	*0.212*	*0.177*	*0.189*	*0.191*	*0.192*	*0.194*
Yorkshire	0.394**	0.746***	0.468**	0.520***	0.595***	0.638***	0.673***	0.688***
	0.157	*0.272*	*0.239*	*0.182*	*0.199*	*0.209*	*0.220*	*0.224*
East Midlands	0.535***	0.858***	0.666***	0.550***	0.579***	0.607***	0.636***	0.667***
	0.151	*0.322*	*0.250*	*0.176*	*0.190*	*0.196*	*0.204*	*0.212*
West Midlands	0.447***	1.054***	0.888***	0.569***	0.696***	0.781***	0.840***	0.866***
	0.152	*0.303*	*0.273*	*0.176*	*0.193*	*0.203*	*0.212*	*0.219*
East of England	0.230	0.882**	0.664**	0.239	0.337*	0.394**	0.449**	0.465**
	0.155	*0.424*	*0.337*	*0.181*	*0.202*	*0.211*	*0.223*	*0.227*
South East	0.396**	0.730**	0.927**	0.469***	0.482***	0.509***	0.527***	0.545***
	0.156	*0.356*	*0.377*	*0.177*	*0.190*	*0.197*	*0.204*	*0.212*
South West	0.342*	0.764**	0.702**	0.351*	0.464***	0.530***	0.590**	0.644**
	0.160	*0.347*	*0.355*	*0.188*	*0.213*	*0.227*	*0.243*	*0.257*
Wales	0.456***	1.549***	1.178***	0.498***	0.605***	0.701***	0.791***	0.876***
	0.162	*0.477*	*0.357*	*0.191*	*0.220*	*0.241*	*0.264*	*0.284*
Constant	-3.342***	-2.277***	-1.980***	-2.801***	-2.714***	-2.659***	-2.605***	-2.600***
	0.165	*0.467*	*0.355*	*0.195*	*0.221*	*0.240*	*0.262*	*0.275*
N	46,827	46,827	46,827	46,827	46,827	46,827	46,827	46,827
Alpha			40.06***					
Log-Likelihood	-4,989.07	-15,242.52	-6,839.61	-7,879.36	-8,967.57	-9,554.84	-10,103.04	-10,511.68

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level

Table 3.24. Quantiles for Counts

Quantiles	0.025	0.500	0.700	0.800	0.900	0.950	0.990	0.999	0.9999
Immigrant	-0.038	-0.319	-0.422**	-0.440***	-0.498***	-0.599***	-0.393**	-0.609*	-0.321
	0.061	*0.981*	*0.199*	*0.152*	*0.141*	*0.188*	*0.206*	*0.326*	*0.251*
Male	0.054*	0.235***	0.423***	0.449***	0.532***	0.548***	0.285**	-0.206	-0.514***
	0.030	*0.052*	*0.063*	*0.063*	*0.081*	*0.106*	*0.121*	*0.209*	*0.157*
Age 26 – 35	-0.083***	-0.371***	-0.580***	-0.594***	-0.807***	-2.158***	-0.782***	-0.733*	0.121
	0.035	*0.065*	*0.079*	*0.079*	*0.130*	*0.242*	*0.225*	*0.419*	*0.155*
Age 36 – 45	-0.129***	-0.721***	-1.080***	-1.134***	-1.363***	-3.067***	-1.112***	-0.604	0.075
	0.044	*0.193*	*0.098*	*0.090*	*0.136*	*0.127*	*0.216*	*0.465*	*0.255*
Age 46 – 55	-0.187	-0.947***	-1.537***	-1.600***	-1.827***	-3.517***	-1.703***	-1.916***	-1.192***
	0.164	*0.138*	*0.152*	*0.180*	*0.155*	*0.133*	*0.242*	*0.360*	*0.227*
Age 56 – plus	-0.229***	-1.519	-2.714	-2.790***	-3.129***	-4.749***	-6.346***	-2.775***	-2.272***
	0.069	*4.427*	*5.025*	*0.294*	*0.458*	*0.152*	*0.223*	*0.285*	*0.142*
Deprived	0.002	0.029***	0.055***	0.053***	0.062***	0.088***	0.080***	0.081***	0.073***
	0.005	*0.011*	*0.013*	*0.013*	*0.015*	*0.020*	*0.026*	*0.044*	*0.025*
Urban	0.001	0.023	0.128	0.113	0.163*	0.182	-0.007	-0.208	-1.165***
	0.032	*0.073*	*0.117*	*0.086*	*0.096*	*0.116*	*0.149*	*0.203*	*0.149*
Inner City	-0.014	0.024	-0.058	-0.004	0.040	0.024	0.150	0.152	0.269
	0.063	*0.097*	*0.119*	*0.115*	*0.141*	*0.180*	*0.261*	*0.344*	*0.209*
North East	0.044	0.387*	0.466	0.493**	0.576***	0.642**	0.071	-0.299	-0.127
	0.074	*0.216*	*0.335*	*0.197*	*0.209*	*0.264*	*0.259*	*0.357*	*0.227*
North West	-0.025	0.224	0.200	0.259	0.308*	0.382	0.237	-0.129	-0.059
	0.080	*0.212*	*0.329*	*0.185*	*0.183*	*0.249*	*0.259*	*0.262*	*0.223*
Yorkshire	0.012	0.269	0.334	0.374*	0.426**	0.568**	0.327	-0.008	0.037
	0.073	*0.227*	*0.362*	*0.193*	*0.194*	*0.266*	*0.267*	*0.338*	*0.211*
East Midlands	-0.018	0.273	0.478	0.515***	0.620***	0.737***	0.612*	0.260	0.799***
	0.097	*0.229*	*0.332*	*0.184*	*0.188*	*0.251*	*0.329*	*0.309*	*0.211*
West Midlands	0.025	0.230	0.346	0.379**	0.488***	0.627**	0.578*	0.601	1.824***
	0.077	*0.223*	*0.334*	*0.190*	*0.188*	*0.251*	*0.294*	*0.393*	*0.194*
East of England	-0.011	0.048	0.177	0.229	0.258	0.373	0.167	0.253	1.582***
	0.073	*1.104*	*0.335*	*0.189*	*0.188*	*0.245*	*0.271*	*0.335*	*0.250*
South East	0.013	0.256	0.384	0.412**	0.469**	0.659**	0.397	0.610	2.225***
	0.107	*0.341*	*0.328*	*0.189*	*0.189*	*0.260*	*0.290*	*0.516*	*0.328*
South West	-0.107	0.163	0.242	0.284	0.368*	0.499*	0.273	0.209	2.475***
	7.310	*0.402*	*0.381*	*0.205*	*0.194*	*0.263*	*0.272*	*0.459*	*0.271*
Wales	0.049	0.340	0.393	0.396**	0.480**	0.725***	0.295	1.236***	2.153***
	0.066	*0.224*	*0.354*	*0.199*	*0.203*	*0.275*	*0.275*	*0.615*	*0.281*
Constant	-0.512***	-2.313***	-3.479***	-3.449***	-3.430***	-1.974***	0.225	2.624***	3.463***
	0.074	*0.216*	*0.338*	*0.195*	*0.195*	*0.327*	*0.329*	*0.410*	*0.302*
N	46,827	46,827	46,827	46,827	46,827	46,827	46,827	46,827	46,827

Standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level

Table 3.25. Hurdle Poisson for Censored Counts

	Hurdle	Zero Trunc Poisson	Zero Truncated Censored Poisson	
			5	10
Immigrant	-0.360***	0.047	0.002	-0.026
	0.110	*0.512*	*0.189*	*0.205*
Male	0.430***	-1.106***	-0.400***	-0.446***
	0.058	*0.243*	*0.098*	*0.114*
Age 26 – 35	-0.616***	0.018	-0.175	-0.128
	0.076	*0.290*	*0.130*	*0.145*
Age 36 – 45	-1.153***	0.498	-0.022	0.005
	0.083	*0.348*	*0.130*	*0.147*
Age 45 – 56	-1.566***	-0.275	-0.491***	-0.547***
	0.101	*0.551*	*0.179*	*0.219*
Age 56 – plus	-2.765***	-0.778*	-0.693***	-0.652***
	0.111	*0.410*	*0.221*	*0.282*
Deprived	0.052***	-0.032	0.033	0.033
	0.012	*0.048*	*0.021*	*0.024*
Urban	0.123	-0.362	-0.268**	-0.269**
	0.078	*0.326*	*0.126*	*0.145*
Inner City	0.032	-0.077	0.029	0.062
	0.103	*0.274*	*0.179*	*0.196*
North East	0.530***	-0.035	-0.504	-0.345
	0.159	*0.441*	*0.323*	*0.365*
North West	0.259*	-0.023	0.024	0.026
	0.149	*0.317*	*0.263*	*0.276*
Yorkshire	0.388**	0.513	0.238	0.327
	0.153	*0.352*	*0.254*	*0.275*
East Midlands	0.523***	0.673	0.080	0.127
	0.147	*0.419*	*0.258*	*0.275*
West Midlands	0.437***	0.954***	0.303	0.480*
	0.149	*0.369*	*0.250*	*0.265*
East of England	0.224	0.985*	0.012	0.207
	0.152	*0.546*	*0.278*	*0.301*
South East	0.388**	0.556	0.153	0.157
	0.156	*0.512*	*0.259*	*0.278*
South West	0.333**	0.577	0.007	0.210
	0.157	*0.450*	*0.283*	*0.308*
Wales	0.446***	1.426***	0.089	0.267
	0.158	*0.478*	*0.273*	*0.301*
Constant	-3.365***	1.018**	0.360	0.473
	0.161	*0.513*	*0.288*	*0.315*
N	46,827	1,190	1,190	1,190
Log-Likelihood	-4,988.90	-4,361.55	-1,326.98	-1,751.82

Robust standard errors are presented in *italics*.
(***) denotes statistical significance at 1% significance level,
(**) denotes statistical significance at 5% significance level,
(*) denotes statistical significance at 10% significance level

Appendix: A Hurdle-Poisson Model for Censored Counts

This model combines results from hurdle models and censored models for counts as presented by Mullahy (1986) and Terza (1985), respectively. The hurdle part of the model recognizes that the binary outcome (zeroes or positives) is generated by a probability distribution appropriate for binary models, while the counts are generated by a truncated at zero distribution appropriate for count data. However, this model is modified to take into account that once the hurdle is crossed the probability function that has support only over the positive counts is censored at C. According to this, the probability of a zero, the probability of a positive but uncensored integer, and the probability of a censored outcome are given by,

$$\begin{aligned}
&\Pr(y=0) = f_1(0),\\
&\Pr(y=k|0<y<C) = (1-f_1(0))\left(\frac{f_2(y)}{1-f_2(0)}\right),\\
&\Pr(y\geq C) = 1-f_1(0)-(1-f_1(0))\left(\frac{f_2(1)-f_2(2)-f_2(3)\ldots f_2(C-1)}{1-f_2(0)}\right),
\end{aligned}$$

where $1-f_2(0)$ is used as a normalization to account for the zero truncation. In the present study we assume that both $f_1(.)$ and $f_2(.)$ are Poisson distributed. In a regression framework, conditional on a set of characteristics x which is assumed to be common in both processes, $f_1(.)$ and $f_2(.)$ follow the Poisson distribution with $\lambda_1 = e^{x_i'\beta}$ and $\lambda_2 = e^{x_i'\gamma}$. The likelihood function is given by,

$$\begin{aligned}
L(\beta,\gamma) = \prod_{i=1}^{n} f_1(0)^{(y=0)} \times &\left[\left(\frac{1-f_1(0)}{1-f_2(0)}\right) f_2(y)\right]^{(0<y<C)}\\
\times &\left[1-f_1(0)-\left(\frac{1-f_1(0)}{1-f_2(0)}\right)(f_2(1)+f_2(2)+f_3(3)+\ldots f_2(C-1))\right]^{y\geq C}
\end{aligned}$$

$$= \prod_{i=1}^{n} (e^{-\lambda_1})^{(y=0)} \times \left[\left(\frac{1-e^{-\lambda_1}}{1-e^{-\lambda_2}} \right) \frac{e^{-\lambda_2} \lambda_2^{y_i}}{y_i!} \right]^{(0<y<C)}$$
$$\times \left[1 - e^{-\lambda_1} - \left(\frac{1-e^{-\lambda_1}}{1-e^{-\lambda_2}} \right) \left[e^{-\lambda_2} \left(\lambda_2 + \frac{\lambda_2^2}{2} + \frac{\lambda_2^3}{3!} + \ldots + \frac{\lambda_2^{C-1}}{(C-1)!} \right) \right] \right]^{y \geq C},$$

which collapses to the standard Censored Poisson model if $\lambda_1 = \lambda_2$. Now, once we multiply and divide the second term by e^{λ_1}, the log likelihood is the following:

$$\ln L = \sum_{i=1}^{n} (y=0)(-\lambda_1) + (0<y<C) \left[\ln(1-e^{-\lambda_1}) - \ln(e^{\lambda_2}-1) - \ln(y_i!) + y_i \ln \lambda_2 \right]$$
$$+ (y \geq C) \ln \left[(1-e^{-\lambda_1}) - \left(\frac{1-e^{-\lambda_1}}{e^{\lambda_2}-1} \right) \left(\lambda_2 + \frac{\lambda_2^2}{2} + \frac{\lambda_2^3}{3!} + \ldots + \frac{\lambda_2^{C-1}}{(C-1)!} \right) \right],$$

which can be further simplified as,

$$\ln L = \sum_{i=1}^{n} (y=0)(-\lambda_1) + (y>0) \ln(1-e^{-\lambda_1}) + (0<y<C) \left[-\ln(e^{\lambda_2}-1) - \ln(y_i!) + y_i \ln \lambda_2 \right]$$
$$+ (y \geq C) \ln \left[1 - \left(\frac{1}{e^{\lambda_2}-1} \right) \left(\lambda_2 + \frac{\lambda_2^2}{2} + \frac{\lambda_2^3}{3!} + \ldots + \frac{\lambda_2^{C-1}}{(C-1)!} \right) \right].$$

From the last expression it is clear that the log likelihood function is separable. This simplifies the estimation procedure as we can separately maximize the likelihood part of the binary outcome, using all observations, and the likelihood part of the zero truncated censored counts using only the positive counts. Turning the last term into a fraction with common denominator, and separating it into two logs we can finally rewrite the likelihood function as,

$$\ln L = \sum_{i=1}^{n} (y = 0)(-\lambda_1) + (y > 0) \ln (1 - e^{-\lambda_1})$$
$$- (y > 0) \ln (e^{\lambda_2} - 1) + (0 < y < C) \left[- \ln (y_i!) + y_i \ln \lambda_2 \right]$$
$$+ (y \geq C) \ln \left[e^{\lambda_2} - 1 - \lambda_2 - \frac{\lambda_2^2}{2} - \frac{\lambda_2^3}{3!} - \ldots - \frac{\lambda_2^{C-1}}{(C-1)!} \right].$$

Maximum likelihood estimation follows using numerical algorithms, such as the Newton-Raphson.

References

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