Pre-analysis Plan: Does Wealth Inhibit Criminal Behavior? Evidence from Swedish Lottery Winners

Erik Lindqvist, Robert Östling, and Christofer Schroeder*

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Abstract

This pre-analysis plan specifies the main empirical analyses in a coming paper studying the effect of winning the lottery on subsequent criminal behavior of winners and their children. We first discuss the previous theoretical and empirical literature and present descriptive statistics of our sample. We then use regressions with reshuffled vectors of lottery prizes to evaluate the performance of different types of analytical standard errors. Next, we evaluate the statistical power of different sample restrictions and outcome variable definitions. Finally, we pre-specify our heterogeneity analyses and discuss how the lottery-based estimates will be benchmarked to the income-crime gradient and the previous literature. The plan is still preliminary and will be updated over the coming weeks.

*Lindqvist: Swedish Institute for Social Research (SOFI), Stockholm University, SE-106 91 Stockholm Sweden, and Research Institute of Industrial Economics (IFN) (e-mail: erik.lindqvist@sofi.su.se). Östling: Department of Economics, Stockholm School of Economics, Box 6501, SE-113 83 Stockholm Sweden (e-mail: robert.ostling@hhs.se). Schroeder: Department of Economics, Stockholm School of Economics, Box 6501, SE-113 83 Stockholm Sweden (e-mail: christofer.schroeder@phdstudent.hhs.se.
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1 Motivation

The objective of this pre-analysis plan is to motivate and fully pre-specify the analyses we are to perform in a paper tentatively titled "Does Wealth Inhibit Criminal Behavior? Evidence from Swedish Lottery Winners." In this paper, we will combine data from three different samples of Swedish lottery players with administrative records of criminal convictions with the goal of estimating the causal effect of lottery wealth on criminal behaviour. The lottery data used in this project have been used in a string of previous papers on adult and child health and child development (Cesarini et al. 2016); subjective health and lifestyle (Östling, Cesarini & Lindqvist 2020); subjective well-being (Lindqvist, Östling & Cesarini 2020); labor supply (Cesarini et al. 2017), and financial risk-taking (Briggs et al. 2015).

Our aim with this pre-analysis plan is not just to commit to a specific set of analyses, but to commit to analyses which make sense from a theoretical perspective, and which have high statistical power. To this end, we matched the data on criminal convictions with a vector of lottery winnings where the prize amounts have been reshuffled among participants with the same chance of winning different prizes. We then use the reshuffled prize vector to evaluate the performance of different types of analytical standard errors and how different sample restrictions and specifications of the outcome variables affect statistical power. Based on our results from these exercises, we pre-specify all main analyses in the coming paper.

Though all authors pledge that the true prize vector has not yet been matched to the data on criminal convictions, credible evidenc ande cannot be presented beyond our assertion as we have access to data on both lottery winnings and criminal outcomes. At a minimum, however, the plan shows that the specifications chosen outperform a large set of alternative specifications with respect to statistical power. Moreover, our evaluation of standard errors leads us to adopt analytical standard errors which prove to be more conservative than the standard errors typically used in applied research.

The pre-analysis plan is structured as follows: Section 2 reviews the previous literature. Section 3 provides background information on the Swedish legal system, discusses our data on criminal convictions, and provides descriptive statistics of criminal activity in Sweden. Section 4 discusses our samples of lottery players and the identification strategy. Section 5 discusses estimation, including the evaluation of standard errors and statistical power. Finally, Section 6 lists the analyses we will conduct in the paper.

2 Theory and Previous Literature

In this section, we review the previous theoretical and empirical literature on the effect of wealth on criminal behavior.

2.1 Economic models of crime

Since the seminal work by Becker (1968), economists have used rational-choice theory to analyze criminal behavior. A prominent set of models presents the perpetrator's problem within a general occupational choice framework (Ehrlich 1973, Sjoquist 1973, Block & Heineke 1975). Here, agents allocate their time between legitimate and illegitimate activities, maximising expected utility in the face of potential punishment from illegitimate activities. A key prediction of these models is that low legal market wages make individuals more prone to commit crimes for economic gain. This prediction – for which there is considerable empirical support – suggests a mechanism for the negative correlation between certain type of crime and income from legal work.[1]

But what about the direct effect of wealth? Block & Heineke (1975) point out that the effect of wealth in the models following Becker (1968) depends on risk preferences. If agents exhibit decreasing absolute risk aversion, then crime is a normal activity. Related frameworks, such as that by Allingham & Sandmo (1972), where tax evasion is modelled as a risky asset, have the same result: as crime increases the variance of potential outcomes, changes in economic circumstances which induce risk-taking (such as a positive wealth shock) also induce crime.

[1]Several studies have found that property crime increases when labor market prospects, as measured by wages (Grogger 1998*a*, Gould, Weinberg & Mustard 2002, Machin & Meghir 2004), or unemployment (Witt, Clarke & Fielding 1999, Gould, Weinberg & Mustard 2002, Edmark 2005, Öster & Agell 2007), worsen.

The models mentioned above, however, abstract from at least two potentially relevant mechanisms.[2] First, total labor supply is assumed to be fixed, and wealth therefore only affects the allocation of labor between legal and illegal activities. More recent work by Grogger (1998*a*) builds a model where leisure from illegal work is a normal good, though the specific assumptions imply only career criminals (who are at the corner solution where all work is illegal) reduce their supply of illegal labor following a wealth shock. Second, the severity of punishment is assumed to be independent of wealth. While this may be a reasonable assumption for less serious crimes, Becker (1968) points out that the utility loss of imprisonment depends on the amount of foregone consumption while serving time. To the extent that the wealthy have, as such, more to lose from imprisonment, wealth would be expected to have a negative effect on the propensity to commit more serious crimes.

Subsequent work on the economics of crime makes explicit the distinction between offenses committed for economic gain – production offenses – and offenses which involve the consumption of illicit goods – consumption offenses (Stigler 1970). Insofar as consumption offenses can be treated as consumable goods, these offences can be modeled within a standard consumer choice framework as goods with different wealth elasticities of demand (Heller, Jacob & Ludwig 2011). Increases in wealth will increase the demand for consumption offences of normal illicit goods, while decreasing the supply of consumption offences of inferior illicit goods. Though it is unclear whether illicit goods are normal or inferior, previous research suggests that illicit drugs such as marijuana, cocaine and heroine are normal goods (Van Ours 1995, Chaloupka, Grossman & Tauras 1998, Liccardo Pacula et al. 2001, Petry 2000), though there is substantial variation in the magnitude of estimated income elasticities.

Legal goods could also be "inputs" in criminal behavior, however. A key example is alcohol, which is associated with a higher risk of violent crime (Murdoch & Ross 1990), and for which estimated income elasticities are typically around 0.7 (Gallet 2007, Nelson 2013).[3] Individuals who can afford fast cars and gasoline might similarly be more likely

[2]Additional work has highlighted indirect effects of wealth on crime. For instance, increases in wealth may be diverted to investments in human capital which in turn may have a dampening effect on criminal behaviour (Lochner & Moretti 2004, Lochner 2004).

[3]Using a subset of the lottery winner sample used in this paper, Östling, Cesarini & Lindqvist

to commit traffic crimes. To the extent that agents perceive fines as the price for engaging in criminal behavior, decreasing sensitivity to the (absolute) risk level and a less tight budget constraint should also increase the propensity to commit crime for which there is a positive consumption value.

In sum, though economic theory suggest a number of mechanisms for how wealth may affect criminal behavior, the net effect is ambiguous. One could, we argue, make a reasonably strong case that the income effect and higher utility loss from imprisonment should imply that wealth reduces the risk of serious offences committed for economic gain. Further, one could also argue that the propensity to commit minor consumption offences should increase in wealth, as illicit goods (e.g., marijuana); legal goods which are "inputs" in criminal acts (e.g., gasoline for speeding), and fines become more affordable. Yet it is easy to come up with examples in which wealth might reduce the propensity to commit such crimes: higher wealth may encourage individuals to substitute away from illegal towards legal goods, for instance from moonshine to legally produced alcohol, or buy goods and services that facilitate law-abiding behavior, such as taking a taxi home from the bar rather than drive under the influence.

2.2 Sociological and criminological theories

The predictions from economic models of crime stand in contrast with standard theories in sociology and criminology by which crime is the result of deprivation, and increases in wealth are predicted to unambiguously reduce criminal activity. Merton's (1938) strain theory was one of the first of such, positing that individuals lacking the means to legally achieve socially defined and accepted goals resort to illegal methods. For Merton, the aquisition of wealth and material possessions, along with the accompanying social status, was one of the main goals he percieved to be prevalent in American society at the time, and lack of economic resources the principle factor contributing to criminal behaviour. In line with strain theory, subsequent works focus on social constructs and their interaction with economic disadvantage to explain crime, including Cohen's (1955)

(2020) finds no statistically effect of lottery wins on a survey-based measure on alcohol consumption. However, the null hypothesis that the lottery-based estimate is equal to the (positive) gradient between income and alcohol consumption could not be rejected.

subculture theory and Cloward and Ohlin's (1960) opportunity theory. Agnew's (1992) general strain theory is a more recent attempt to modernise the strain theory approach, going beyond economic disadvantage and considering additional causes of social strain.

2.3 Empirical literature on adult crime

The majority of the existent literature studying the causes of crime at the individual level has focused on the relationship between crime and unemployment (Edmark 2005, Gould, Weinberg & Mustard 2002), education (Lochner & Moretti 2004, Meghir, Palme & Schnabel 2014, Hjalmarsson, Holmlund & Lindquist 2015, Bennett 2016), and incarceration (Owens 2009, Bhuller et al. 2016, Rose & Shem-Tov 2018). These studies generally find that crime is positively associated with unemployment and negatively associated with education. Papers studying the effects of incarceration on crime generally seek to separate between incapacitation and rehabilitation effects, finding that most, if any reduction in crime due to incarceration stems from criminals being incapacitated while spending time in prison.

Much of the early empirical literature studying the relationship between economic disparity and criminal behaviour has relied on cross-sectional data at varying levels of geographical detail. These studies mostly consider cross-sectional variation in economic disparity and the evidence they offer is mixed: Patterson (1991) and Lee (2000) find a positive relationship between economic disparity and violent crime, while Messner (1982), Allen (1996), and Dreze & Khera (2000) find a negative relationship. DeFronzo (1996) and Hannon & Defronzo (1998) find a negative relationship between welfare payments and property crime at the US state level; DeFronzo (1997) finds a negative relationship between welfare payments and homicide. Ellis & McDonald (2001) and Sharkey, Besbris & Friedson (2017) provide comprehensive reviews of this literature.

Remaining at the aggregate level, a number of recent studies have attempted to harness quasi-experimental variation in economic disparity. In various settings, Mehlum, Miguel & Torvik (2006), Iyer & Topalova (2014), and Papaioannou (2017) use rainfall as an instrument for economic disparity. They all conclude that economic disparity increases property crime, while the evidence for violent crime are mixed. Using Swedish data, Sariaslan et al. (2013) compare criminal behaviour across siblings in the same families that live and attend school in different neighbourhoods, and find no relationship between violent crime and neighbourhood deprivation. Foley (2011) exploits the timing of welfare payments across US cities and finds that crimes with a likely financial motivation (including burglary, larceny-theft, motor vehicle theft, and robbery) increase over the course of monthly welfare payment cycles. Raphael & Winter-Ebmer (2001) instrument for US state unemployment rates using defence contracts and oil price shocks, and find property crime to be positively related to the rate of unemployment, with weak evidence of a positive effect on violent crime. In more recent work, Lindo, Siminski & Swensen (2018) find that increased partying in the context of US colleges increases the incidence of sexual assault. To the extent that the frequency and intensity of partying may be associated with wealth, their results suggest a further potential mechanism linking wealth and crime.

The first prominent studies utilizing data at the individual level, and considering exogenous sources of variation in economic means, came from randomised experiments run in the in the 1970s on ex-offenders in the US (Rossi, Berk & Lenihan 1980). In both the Life Insurance for Ex-prisoners (LIFE) experiment in Baltimore, and the Transitional Aid Research Project (TARP) run in Georgia and Texas, prisoners due to be released were randomly chosen to recieve financial support or unemployment insurance. The conclusions from these experiments were again mixed: results from the LIFE experiment found that those receiving income support were slightly less likely to be arrested in the year following release, while there was no difference in rates of recidivism for individuals randomly assigned to receive unemployment insurance following release in TARP.

More recently, a number of studies have sought to estimate the effects of welfare and cash transfers on criminal behaviour exploiting quasi-exogenous timing of benefit payments. Watson, Guettabi & Reimer (2019) estimate the effects of cash transfers from Alaska's Permanent Fund Dividend on daily crime rates. The cash transfers, they find, are related to increases in incidents of substance abuse both in the days and weeks following payment, as well as decreases in the incidents of property crime. They find no effect on incidents of violent crime. Chioda, De Mello & Soares (2016) estimate the effect of an expansion of a Brasilian conditional cash transfer program to school-aged youths on crime rates at the neighbourhood level. The authors find a strong negative effects of transfers on robberies, along with some evidence of negative effects on drug related crimes and crimes against minors. Carr & Packham (2019) study the relationship between monthly food-purchasing assistance from the Supplemental Nutrition Assistance Program (SNAP) and crime, and find that overall incidents in crime peak in the first and fourth week following imbursement. Evans & Popova (2014) review the literature on the effect of cash transfers on the consumption of illicit goods, concluding that the majority of studies find either no impact or a significant negative impact. Riddell & Riddell (2006) and Dobkin & Puller (2007) find an increase in the likelihood of overdose amongst drug users in the day following payment.

2.4 Empirical literature on juvenile crime

Given the substantial concentration of crime amongst the young, a large literature seeks to explain the motivating factors behind youth crime in particular. Grogger (1998*b*) builds and tests a model in which property crime is driven by real wages in the formal labour market. As individuals become increasingly attached to the labour market over their lives, their propensity to commit crimes decreases. Levitt (1998) highlights the role of more lenient punishments for the underaged and Grönqvist (2011) focuses on unemployment. A number of papers consider familial influences; Heller, Jacob & Ludwig (2011) provides a review and highlights the link between familial environment and childhood development as a mechanism through which wealth could affect criminal behaviour of youths. Levitt & Lochner (2001) and Comanor & Phillips (2002) find that growing up in a broken home is one of the most significant predictors of youth participation in both violent crime and property crime. Bjerk (2007) contends that household income has a strong negative effect on serious crimes, while Sariaslan et al. (2013) and Sariaslan et al. (2014) find no effect of neighbourhood deprivation or family income on adolescent crime. Dobbie et al. (2018) study the effects of parental incarceration.

3 Institutional Background and Data on Crime

3.1 Swedish Legal System

The primary legislative source of the law in Sweden is the Swedish Code of Statutes (*Svensk författningssamling*; SFS). The SFS contains a collection of all laws passed before the Swedish legislature and any revisions made to these. Laws in the SFS are headlined by the year in which they were passed, together with a four digit number unique to the year of passing. In contrast to many continental European countries, Swedish law is not based off a comprehensive civil code, but rather the lose collection of statutes in the SFS (Bernitz 2007). As such, legal rulings often rely on legal precedent(Rossi, Berk & Lenihan 1980) when interpreting these statutes. Since 1965, the Swedish Penal Code (*Brottsbalken*) has been the primary source of criminal law. The Penal Code outlines provisions on what constitutes various types of crime in Sweden and provides ranges of standard sanctions to be imposed in the event of violations of the code. A separate section of the code expands upon the sanctions, and provides alternative sanctions that may be applied depending on the gravity of the crime and the accused's personal circumstances. Upon its drafting, the Swedish Penal Code was to a large extent influenced by Ancel's 'social defense school' which emphasized the preventative function of criminal law and advocated for non-punative orders (Brush 1968).

There are three broad types of courts in Sweden: general courts, including district courts (*tingsrätt*), courts of appeal (*hovrätt*), and the Supreme Court (*Högsta domstolen*); administrative courts, dealing with disputes between citizens and the authorities; and special courts, which settle disputes within particular fields (for example the Patent and Market Court which deals with legal matters related to intellectual property). Criminal cases are tried in one of 48 district courts. Appeals of decisons made in the district courts are heard before one of six courts of appeal. The Supreme Court is the highest court in the Swedish judiciary and the final instance for appeals. The Supreme Court typically hears high profile cases, and those which have the potential to set a precident for future judgements.

If, following a preliminary investigation, the prosecutor decides to press charges, criminal cases are heard before a judge, up to three lay judges (for more serious crimes), and any witnesses called. A particular feature of the Swedish legal system are summary penalty orders (*åtalsunderlåtelse*) in which the prosecutor abstains from pressing charges in exchange for payment of a fine. A prerequisite for summary penalty orders to be extended is admission of guilt on behalf of the accused. These proceedings are common for crimes of a less serious nature, including traffic infractions or minor shoplifting charges.

If the accused is found guilty of the charges brought before the court, there are a wide array of sanctions that can be imposed. Sentences can include, but are not limited to, fines, community service, psychiatric care, imprisonment, probation, and juvenile detention. Fines are the most common sanction imposed, in particular for crimes of a less serious nature. In addition to any formal sentences handed out, the guilty party can be required to reimburse the travel costs for witnesses summoned, or pay any damages awarded to the plaintiff.

3.2 Crime Data

The data we use to construct our outcomes come from the register of conviction decisions (*register över lagförda personer*) maintained and provided by the Swedish National Council for Crime Prevention (*Brottsförebyggande rådet,* or *Brå* for short). The unit of observation is a conviction, corresponding to either a court sentencing, a prosecutor imposed fine, or a waiver of prosecution. Prosecutor imposed fines (*strafföreläggande*) are common for minor offences and implies that the offender accepts a fine suggested by the prosecutor without going to trial. A waiver of prosecution (*åtalsunderlåtelse*) refers to a process by which the prosecutor declines pressing charges, despite there being no doubt as to the accused having committed the crime at question – often established through an admission of guilt. Prosecution waivers are common for juvenile offenders (below the age of 18) or for adult offenders who are also being charged for more serious offences, implying the crime in question is unlikely to affect the sentence. The register does not include fines for minor offences issued by police, customs and related officials (*ordningsbot*).

Our extract from the register spans the years 1975 to 2017 and contains convictions of individuals aged 15 or older at the time of infraction; the age of criminal responsibility in Sweden. Individuals are identified by unique personal identification numbers which allow a matching to the lottery data and data on individual background characteristics. In the data, each conviction can be comprised of up to 25 crimes. The Swedish judicial system defines crimes by the principle of instance such that a single crime typically corresponds to violations occuring at the same time and place. In turn, each crime can be a violation of up to three sections of the law, including crimes against the Swedish Criminal Code (*brottsbalken, BRB*) and violations of laws in the Swedish Code of Statutes (*svensk författningssamling, SFS*). For example, a single conviction in our data may contain the single crime of fraud through forgery, where fraud is a crime according to chapter 9, article 1 of the Swedish Criminal Code, while forgery is a crime according to chapter 14, article 1 of the Swedish Criminal Code.

For each section of the law, we observe the chapter, article, and paragraph for crimes against the Swedish Criminal Code, and the exact statute and applicable paragraph for crimes against the Swedish Code of Statutes. We also observe ID numbers uniquely assigned to each section of the law for which we have a key with descriptive titles. Using this information, we classify crimes into a number of different categories. For comparability with the annual crime statistics published by the Swedish National Council for Crime Prevention and much of the previous empirical literature, we classify crimes in to the following broad categories: property crimes, violent crimes, drug crimes, white collar crimes, traffic crimes, and other crimes. Property crime includes theft, robbery, fraud, embezzlement and related types of crime. To simplify the interpretation of property crimes as a type of crime motivated by economic gain, we do not classify vandalism as a property crime. Violent crimes include (but are not limited to) assualt, unlawful threats, defamation and sexual assault. We also includes possession of illegal weapons in this category. Drug-related crimes include impaired driving, possession of illegal drugs, bootlegging and smuggling. White-collar crimes include various crimes related to tax evasion, violation of company law, benefit fraud and money laundring. Traffic crimes include, for example, impaired and reckless driving and driving without a license. Notably, many minor traffic offences (such as moderate levels of speeding) do not end

Table 1: Crime Categories

Categories	Penal code chapters (BRB) and Swedish Code of Statutes paragraphs (SFS)
Property	BRB: 8 (theft/robbery); 9 (fraud); 10 (embezzlement); 11 (accounting violations).
Violent	BRB: 3 (murder/assault); 4 (threats/kidnapping); 5 (defamation); 6 (sexual assault). SFS: 1988:254; 1973:1176; 1996:67 (weapons possession).
Drug	SFS: 1951:649 (impaired driving); 1968:64 (possession of illegal drugs); 1991:1969 (doping); 1994:1738 (bootlegging); 2000:1225 (smuggling).
White collar	SFS: 1971:69; 1975:1385; 2005:551; 1977:1160; 1977:1166; 1990:1342; 2000:1086; 2000:377; 1998:204; 1993:768; 2009:62; 2007:612; 2014:307; 2016:1307; 1923:116; 1994:1565; 1978:478; 1988:327; 1953:272; 2006:227.
Traffic	SFS: 1951:649; 1998:1276; 1972:603; 1972:595; 2002:925; 1972:599; 2001:558; 1988:327; 2009:211; 1995:521; 2001:650; 2007:612; 2004:865; 1994:1297; 1986:300; 2006:227; 1998:488; 1977:722; 1962:150.
Other	All crimes not included in any of the categories above.

The table shows the exact coding of penal code chapters (BRB) and the coding of the most common codes from the Swedish Code of Statutes (SFS).

up in the registry as the police will issue a fine on the spot. Our final category – "other crimes" – is a residual category including all violations of Swedish law not included in any of the other categories.

Importantly, a given crime can belong to multiple categories. For instance, driving under the influence of narcotics is categorised as both a traffic and a drug crime. Examples of such crimes include arson, counterfeiting, purgery, rioting, incitement, and poaching. A detailed listing of the crimes we assign to each category is included in Table 1.

Each conviction can also be associated with up to three sentences. The data contain a wide variety of sentences ranging from fines, to community service, to time in prison. Fines are by far the most common form of punishment, imposed on over 60% of all convictions in our data, and are generally handed out to those convictions deemed less serious than those punishable by some form of detention. A unique feature of the Swedish criminal justice system are day fines (*dagsböter*) which are typically handed out in convictions punishable by fine that are of a more serious nature. Day fines are comprised of two components: a number of fines and an amount which is calculated based off of one's annual pre-tax income. The total fine amount – the number of fines multiplied by the amount – is then due in one installment no more than 30 days following issuance of the fine. For less serious convictions punishable by fine, simple lump-sum fines (*penningböter*) are usually imposed.

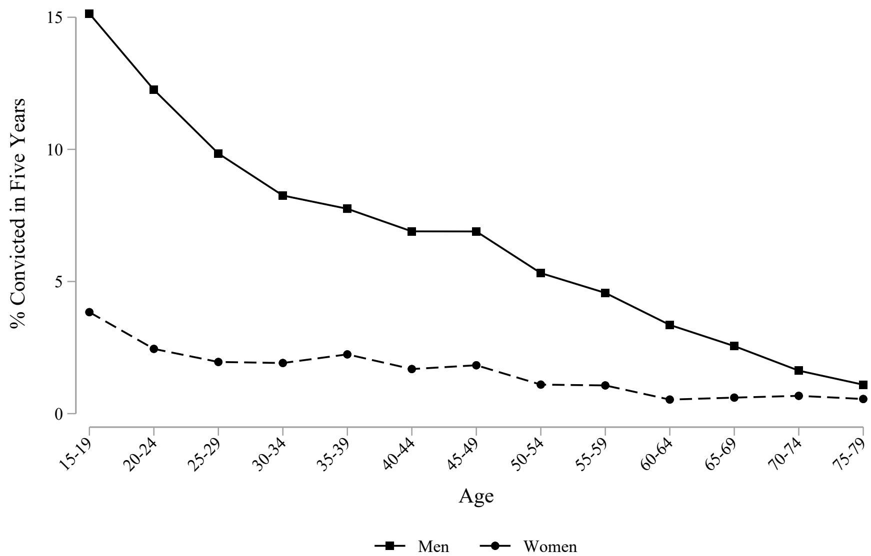
Apart from fines, the majority of other forms of punishment in the data constitute some form of restriction of freedom. These range from prison sentences for the most serious convictions, to probation and community service for less serious convictions. In many cases, underage offenders between the ages of 15-20 are sentenced to either juvenile care (*ungdomsvård*) or juvenile detention (*sluten ungdomsvård*) delivered outside of the adult correctional system.

While we focus on convictions, we also have access to data on suspects fromthe Suspects Registry (*Misstankeregistret*). This registry, which is compiled by the Swedish National Council for Crime Prevention, includes information on individuals suspected on reasonable grounds by four different government agencies: the Swedish Policy Authority (*Polismyndigheten*); the Swedish Prosecution Authority (*Åklagarmyndigheten*); the Swedish Economic Crime Authority (*Ekobrottsmyndigheten*), and Swedish Customs (*Tullverket*). The Suspects Registry data include a rough categorization of the type of crime, but for the purpose of this pre-analysis plan we only focus on the occurrence of being a suspect.

3.3 Descriptive Statistics of Crime in Sweden

This section documents basic patterns of crime in Sweden based on our data from the Swedish National Council for Crime Prevention. To this end, we use three representative samples of 50,000 Swedes each, drawn in 1990, 2000 and 2010 by Statistics Sweden. We being by showing how the fraction of the population convicted for a crime varies with age and gender. For each sample, we follow all individuals between age 15 and 74 for

Figure 1: Criminal activity by age and gender in the representative sample



The figure shows the share of men and women in different age groups from representative samples drawn in 1990, 2000 and 2010 who have been convicted for at least one crime within the next five years.

five years from the year the sample was drawn. People who die or move abroad within this five-year period are coded as missing. Figure 1 shows that men are much more likely to be convicted than women at all ages, and that the propensity to commit crimes falls sharply with age for both genders.

Panel A of Table 2 shows the share men and women convicted of different types of crime during the five years from the year the sample was drawn. About one out of 14 men (7.24%) are convicted for at least one crime compared to one in every 63 women (1.58%). The most common type of crime among men is traffic crime – one in 27 men are convicted for a traffic crime within a five-year period – while the most common type of crime among women is property crime. Unsurprisingly, the relative difference in criminal behavior between men and women is largest for violent crimes where men are more than seven times more likely to be convicted.

Panel B of Table 2 shows that fines is the most common form of punishment. Notably, the share women who receive a harsher sentence is small relative to men. While the relative risk of being sentenced to paying a fine is 4.5 times larger for men, the relative risk is more than 14 times larger for serving jail time.

Panel C shows the distribution of convicted by number of crimes. More than half of convicted men, and two-thirds of convicted women, are only convicted for one crime during the five-year period we study. A relatively small group of individuals are convicted for five crimes or more. However, this group are responsibe for 58 percent of all recorded crimes in ur data.

Income Gradients

We now describe the relationship between economic status and income, using the same representative samples as above. Because income while young or old may be poor proxies of life-time income, we restrict attention to men and women between age 30 and 54 at the time the sample was drawn (e.g., 1990, 2000, or 2010). We assign individuals into income deciles based on their average household disposable income during the five years prior to the draw relative to others of the same gender, age (five-year intervals) and from the same sampling year. To avoid simulaneity bias, we compare the share convicted during the first five years after the draw. For example, individuals from the 1990 sample are assigned to income deciles based on their income in 1985-89 while we consider their criminal behavior in 1991-95. Figure 2 shows that criminal behavior is strongly related to income. While 18.3 percent of men in the lowest income decile are convicted for a crime, the same is true for only 3.5 percent of men in the highest decile. Though the level is much lower for women, the relative decile is similar: women in the bottom decile are about seven times more likely to be convicted for a crime relative to women in the top decile. In unshown analyses (available upon request), we find the gradient is similar for men when we instead use own instead of household disposable income (roughly a factor of four between the bottom and top deciles), but considerably flatter for women.[4] The gradients get steeper when we restrict attention to more severe

[4] A likely reason for the flatter own-income gradient for women is that female labor supply is decreasing in spousal income, pushing down the incomes of highly educated women (who are likely to be married to high-income men).

Table 2: Descriptive Statistics of Convictions in a Representative Sample

A. By type of crime (% of sample)

	Men	Women
Any	7.24	1.58
Property	1.87	0.69
Violent	1.63	0.22
Drug	1.08	0.18
White collar	0.25	0.06
Traffic	3.77	0.53
Other	2.00	0.30

B. By type of sentence (% of sample)

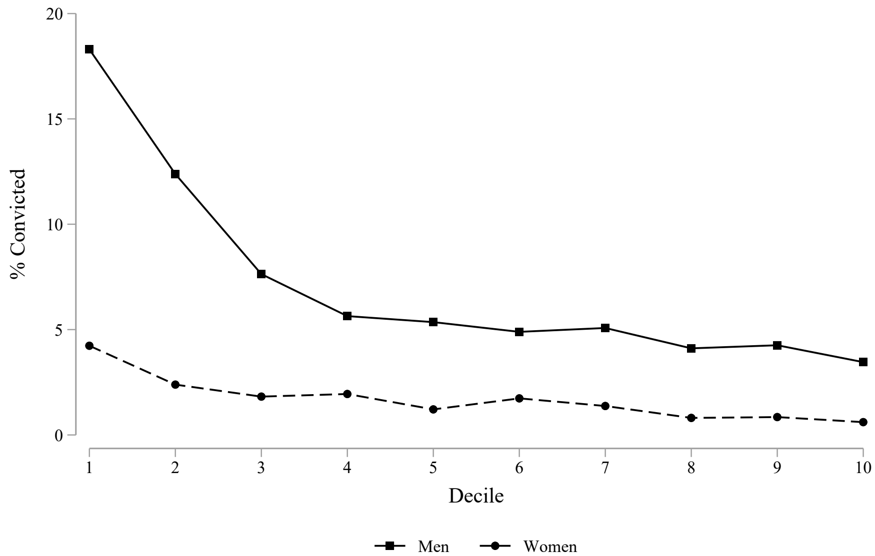
	Men	Women
Fine	5.93	1.32
Detention (including jail)	1.97	0.23
Jail	1.14	0.08

C. By perpertrator number of crimes

	Men	Women
1	56.9	66.2
2	16.6	15.0
3	6.9	6.3
4	4.4	3.4
≥ 5	15.2	9.0

The table shows descriptive statistics of convictions for three representative samples of Swedish men and women between age 15 and 79 drawn in 1990, 2000, and 2010.

Figure 2: The Crime-income Gradient



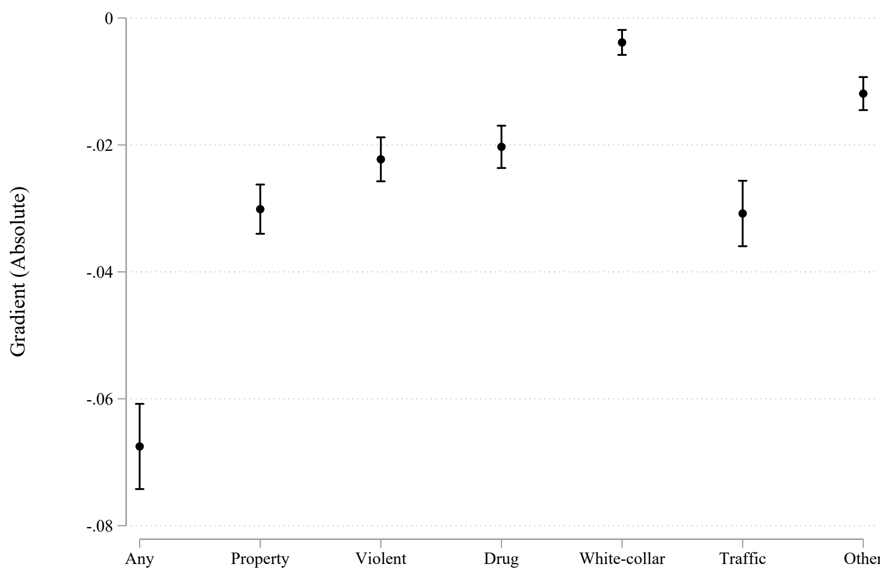
The figure shows the share of men and women age 30 to 54 from representative samples drawn in 1990, 2000 and 2010 who have been convicted for at least one crime within the next five years, split by income decile. Income deciles are assigned based on average household disposable income within the preceding five-year period by gender, age (five-year intervals), and the year the sample was drawn.

types of crimes, as proxied by the type of sentence. While men in the bottom deciles are four times more likely than men in the top to be sentenced to pay a fine, they are 16 times more likely to be sentenced to detention and 21 times more likely to go to prison.

We now turn to the question of whether the crime-income gradients vary by type of crime. To investigate this, we restrict the sample to men between 30 and 54 and regress indicator variables for having been convicted for each type of crime on the log of average household income during the five years prior to the draw and age fixed effects. We set household annual disposable income equal to a lower bound of SEK 40,000 (in 2010 prices, roughly $6,000) in case the reported income is lower.[5] Figure 3 shows an increase in log household income by 1 (corresponding to about 1.75 SDs) is associated with a 6.8

[5]The SEK/Dollar exchange rate was 6.72 on Dec 31st 2010.

Figure 3: Absolute Income Gradients by Type of Crime



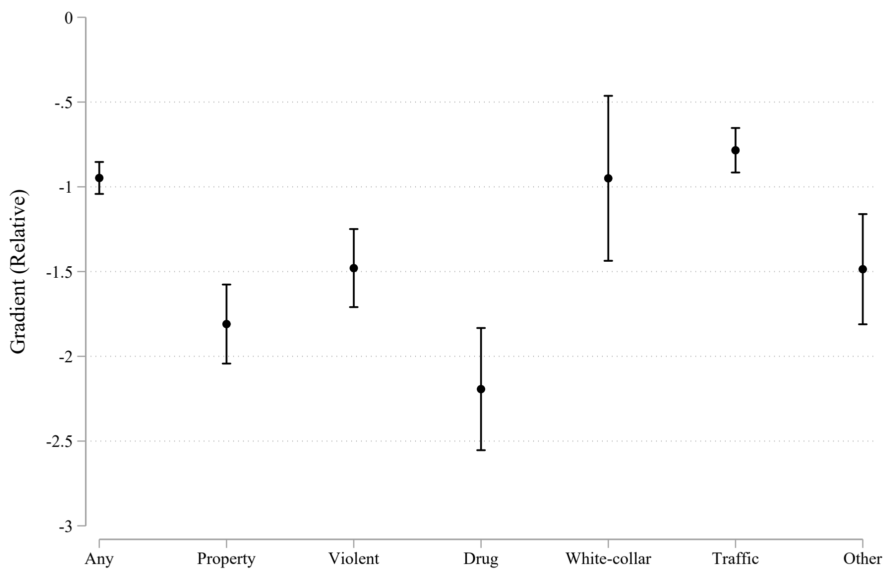
The figure shows the coefficients from regressing a dummies for being convicted for a given type of crime during a five-year period on the log of average household disposable income during the preceding five-year period. The sample consists of men between age 30 and 54 from representative samples drawn in 1990, 2000 and 2010.

percentage point lower risk of being convicted for any type of crime. The corresponding number for the different sub-categories is around 2 percentage points, except for white-collar crime where the association is weaker. Figure 4 shows the gradients divided by the average crime rate in the sample, thus giving an implied elasticity (though clearly a causal interpretation is uncalled for). This elasticity is in the ballpark of 1 for any type of crime, as well as for white-collar crimes and traffic crimes; 1.5 for violent crimes and other types of crime, and about 2 for property crimes and drug crimes.

3.4 Crime in Sweden in an International Comparison

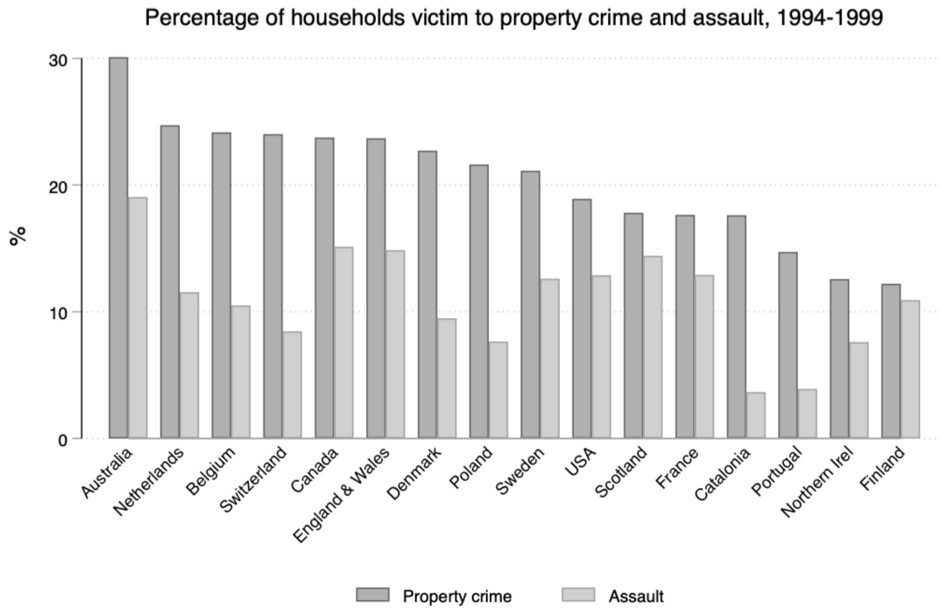
Although comparisons of criminality across boarders are difficult given differences in legal systems, enforcement, and record keeping practices, we can look to survey data

Figure 4: Relative Income Gradients by Type of Crime



The figure shows relative income gradients based on regressing a dummies for being convicted for a given type of crime during a five-year period on the log of average household disposable income during the preceding five-year period. The coefficients have been divided by the average crime rate in the sample. The sample consists of men between age 30 and 54 from representative samples drawn in 1990, 2000 and 2010.

Figure 5: Victimization



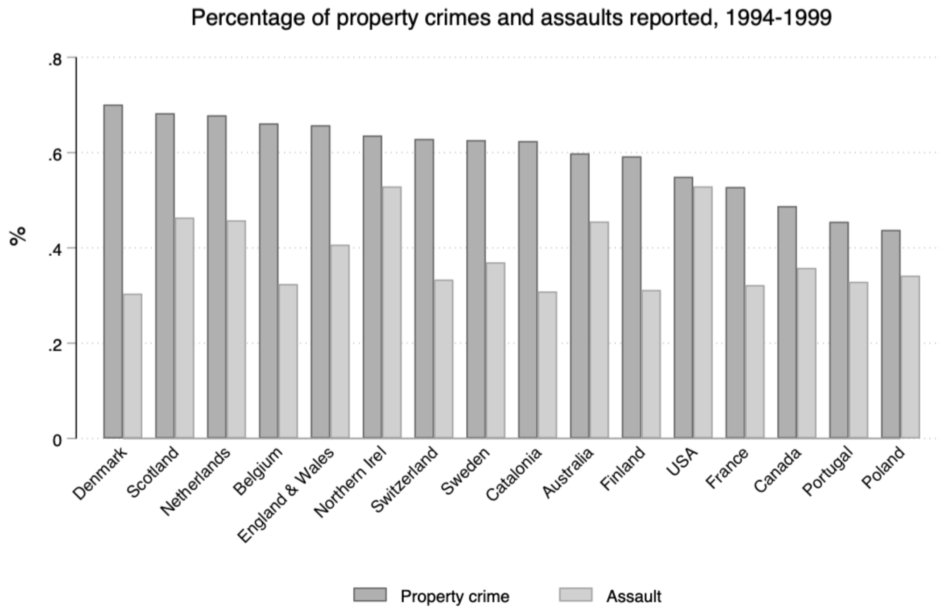
Source: International Crime Victim Survey 2000.

from the International Crime Victim Survey (ICVS) to place crime in Sweden in an international context. To provide a picture of the relative pervasiveness of crime in Sweden, Figure 5 plots the percentage of households victim to crime between 1994-1999 for the sample of countries covered by the 2000 ICVS. For both property crime and assault, Sweden falls roughly in the middle of the pack.

United Nations Office on Drugs and Crime (UNODC) log number of police reported incidents of theft and assault per capita in 2005 for a sample of OECD countries (for which data was available). For both types of crimes, Sweden lies at or near the very top of the ranking of countries in the sample.

A major factor that affects crime statistics and hinders not only international comparisons, but also longitudinal studies of crime, are differences in willingness to report

Figure 6: Share of crimes reported

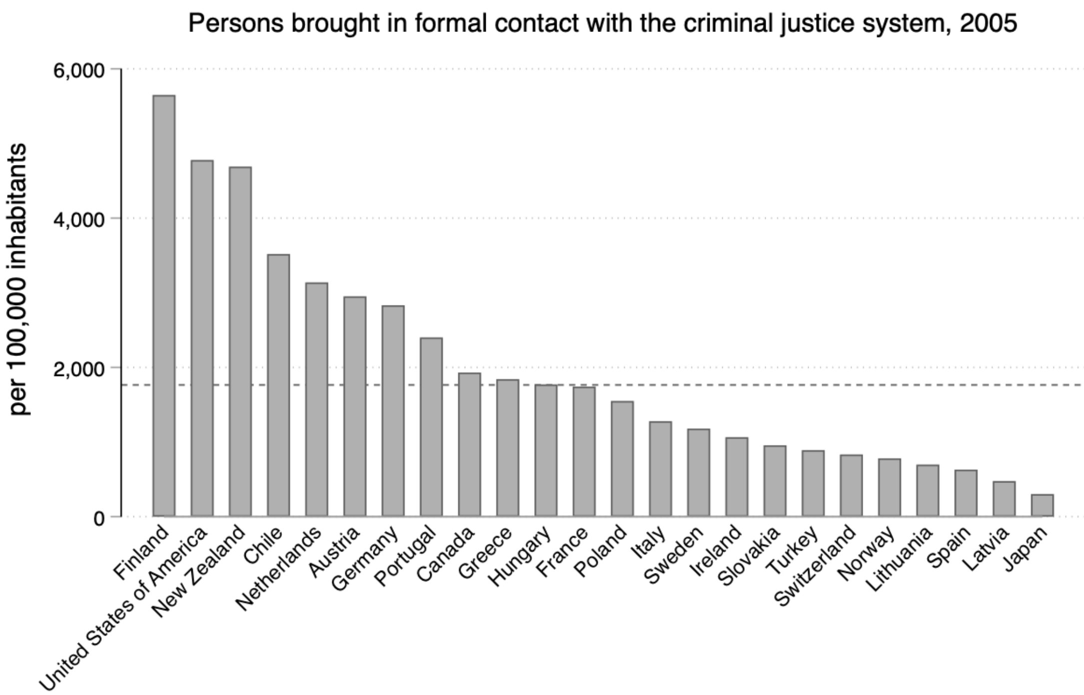


Source: International Crime Victim Survey.

crimes across jurisdictions and time. To provide a picture of the relative willingness to report crimes in Sweden, Figure 6 plots the percentage of property crimes and assaults which survey respondents reported to police between 1994-1999. For both categories, Sweden falls roughly in the middle of the ranking of countries covered in the survey.

Although comparisons of criminality across boarders are difficult given differences in legal systems, enforcement, and record keeping practices, we can look to cross-country statistics produced by the United Nations Office on Drugs and Crime (UNODC) to place crime in Sweden in an international context. Figure 7 displays the number of persons brought in formal contact with the criminal justice system in 2005 for a sample of OECD countries (for which data was available). While Sweden appears in the bottom half of the ranking, it lies only slightly below the average for European countries in the

Figure 7: Contact with the criminal justice system



Source: United Nations Office on Drugs and Crime.

sample.

4 Lottery Samples

We construct our estimation sample by matching three samples of lottery players and their spouses to the crime data described above and population-wide registers on socioeconomic outcomes from Statistics Sweden. The main threat to identification in studies of lottery winners is that the amount won might correlate with the number of lottery tickets. To overcome this problem, we use our data and knowledge about each lottery to construct define "cells" within which the amount won is random. We control for cell fixed effects in all analyses, thus ensuring all identifying variation comes from

Table 3: Cell Construction Across Lottery Samples (Adult Analyses)

	Time Period	Treatment Variable	Cell construction
PLS Fixed Prizes	1986-2003	Prize	Prize Draw
PLS Odds Prizes	1986-1994	Prize	Prize Draw ∗ Balance
Kombi Lottery	1998-2011	Prize	Prize Draw ∗ Balance ∗ Age ∗ Sex
Triss-Lumpsum	1994-2011	Prize	Year ∗ Prize Plan
Triss-Monthly	1997-2011	NPV of Prize	Year ∗ Prize Plan

players in the same cell. We describe the construction of the cells in each lottery below.

4.1 Prize-Linked Savings Accounts

Prize-linked savings accounts (PLS) accounts are bank accounts that randomly award prizes rather than paying interest (Kearney et al. 2011). Our data include two sources of information from the PLS program run by the commercial banks, Vinnarkontot ("The Winner Account"). The first source is a set of prize lists with information about all prizes won between 1986 and 2003. The prize lists contain information about prize amount, prize type and the winning account number. The second source consists microfiche images with information about the account balance of all accounts participating in the draws between December 1986 and December 1994 (the "fiche period") and the account owner's personal identification number (PIN). Matching the prize-list data with the microfiche data allow us to identify PLS winners between 1986 and 2003 who held an account during the fiche period.

Draws in the PLS lottery were held monthly throughout most of the studied time period. Account holders were given one lottery ticket per 100 SEK in account balance. There were two types of prizes in each draw: fixed prizes and odds prizes. Fixed prizes varied between 1,000 and 2 million SEK whereas odds prizes paid a multiple of 1, 10, or 100 times the account balance (odds prizes were capped at 1 million SEK during most of the sample period).

We use different approaches for each type of prize to construct the PLS cells. For fixed prizes, we exploit the fact that the total prize amount is independent of the account balance among players who won the same number of fixed prizes in a draw. We therefore assign winners to the same cell if they won an identical number of fixed prizes in a given draw, thereby excluding people who never won from the sample. Because we do not need information about the number of tickets owned to construct the fixed-prize cells, we can use fixed prizes from both the fiche period (1986-1994) and thereafter (1995-2003).

Because the amount won depends on the account balance for odds prizes, it is not enough to condition on the number of prizes win in a given draw. We therefore construct the odds-prize cells by matching individuals who won exactly one odds prize in a draw to individuals who also won exactly one prize (odds or fixed) in the same draw and who had a similar account balance. The fixed-prize winners who are this way matched to an odds-prize winner is assigned to the new odds-prize cell instead of the original fixed-prize cell. Each individual is thus assigned to no more than one cell in a given draw. However, because players can win in several draws, some players appear in multiple draws. Because account balances are unobserved after 1994; we only include odds prizes won during the fiche period (1986-1994). To keep the number of cells manageable, we consider only odds-prize cells for which the total amount won is at least 100,000 SEK.

4.2 The Kombi Lottery

The second lottery sample consists of roughly half a million individuals who participated in a subscription lottery called Kombilotteriet ("Kombi"). Kombi is run by a company owned by the Swedish Social Democratic Party. Kombi subscribers receive their desired number of tickets via mail one per month. For each subscriber, our data contain information about the number of tickets held in each draw and information about prizes exceeding 1M SEK. Two individuals who held the same number of tickets in a Kombi draw have the same chance of winning a large prize. We construct the Kombi cells by matchig each winning player to (up to) 100 non-winning players. The non-winners are randomly chosen from the set of players who had the same number of tickets in the given draw. Random assignment of prizes within cells implies that controls must be drawn with replacement from the set of potential controls. Winners may therefore be drawn as controls, and some individuals are used as controls in several draws.

4.3 Triss Lotteries

Triss is a scratch-ticket lottery owned by the Swedish government-owned gaming operator, Svenska Spel. Triss lottery tickets are widely sold in Swedish stores. Our sample consists of two categories of Triss winners which we denote Triss-Lumpsum and Triss-Monthly. Winners of either type of prize are invited to TV show broadcast every morning. At the show, winners of Triss-Lumpsum draw a new scratch-off ticket and win a prize ranging from 50,000 to 5 million SEK. Triss-Monthly winners participate in the same TV show, but instead win a monthly installment which size (10,000 to 50,000 SEK) and duration (10 to 50 years) are determined by two separate, independently drawn tickets. The exact distribution of prizes in Triss-Lumpsum and Triss-Monthly are determined by prize plans which are subject to modest revisions over the years.

We convert the Triss-Monthly prizes to their present value by using a 2 percent annual discount rate. Svenska Spel sent us data on all participants in Triss-Lumpsum and Triss-Monthly prize draws between 1994 and 2011 (the Triss-Monthly prize was introduced in 1997). We exclude about 10 percent of the Triss prizes for which the Svenska Spel data indicate the ownership of the ticket was shared between multiple people.

While the chance of winning a Triss-prize depends on the number of tickets bought, the amount won does not. We place players in the same cell if they won exactly one prize of a given type in the same year and under the same prize plan. A few cases where a player won more than one prize within the same year and prize plan are excluded from the sample.

4.4 Estimation Samples

Our base sample for the adult analyses consists of all winners and controls (as defined above) who turned at least 18 and no more than 74 in the year of the win. Merging the three lotteries gives us a sample of 356,993 lottery players within this age range. Primarily because many PLS lottery players win small prizes several times, these observations correspond to 282,642 unique individuals. To arrive at our estimation sample, we first exclude individuals who (i) lack information on basic socio-economic characteristics; (ii) shared prizes in the Triss lottery and cells without variation in the amount won. After imposing these restrictions we end up with an estimation sample of 354,247 observations (280,929 individuals).

Our base sample for the Intergenerational Analyses consists of all children of winners who were i) conceived (born no more than six months after the win) but had not yet turned 18 at the time of the lottery and ii) who were born in 2001 or earlier. We impose the latter restriction as children born in 2003 or later are not old enough to be convicted by 2017 (the last year of our data on criminal records). Children born in 2002 are excluded as the time span during which they could commit and be convicted for crimes is just a matter of months.

4.5 Prize Distribution

Table 4 shows the distribution of prizes in the pooled sample and for each lottery separately. All lottery prizes are net of taxes and expressed in units of year-2010 SEK. The total prize amount in our sample is a little over 6 billion SEK ($900 million). PLS and Triss-Monthly have the largest prize pools with over 2 billion SEK per lottery. Yet Triss-Lumpsum is the lottery which provides the most identifying variation (35%). The reason prize amount and identifying variation do not perfectly coincide is that we only use the within-cell variation.

Table 4: Distribution of Prizes Awarded

	Winners (adult analyses)					Winning parents (child analyses)				
				Triss...					Triss...	
	All	PLS	Kombi	Lumpsum	Monthly	All	PLS	Kombi	Lumpsum	Monthly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0	37,246	0	37,246	0	0					
1K to 10K	286,727	286,727	0	0	0					
10K to 100K	23,053	21,821	0	1,232	0					
100K to 500K	4,769	2,291	0	2,478	0			To be added		
500K to 1M	487	276	19	192	0					
1M to 2M	1,312	640	360	67	245					
2M to 4M	439	27	20	90	302					
>4M	214	0	5	62	147					
N.	354,247	311,782	37,650	4,121	694					
Sum (M SEK)	6,132	2,362	492	1,251	2,027					
% of variation.	100.0	27.0	11.0	35.1	26.9					

This table shows the distribution of prizes in the sample of adult winners between age 18 and 74, and among winning parents in the same age range. All prizes are after tax and measured in year-2010 SEK. In Triss-Monthly, prize amount is defined as the net present value of the monthly installments won, assuming the annual discount rate is 2%.

4.6 Testing Randomization

Key to our identification strategy is that the variation in amount won within cells is random. If the identifying assumptions underlying the lottery cell construction are correct, then characteristics determined before the lottery should not predict the amount won once we condition on cell fixed effects, because, intuitively, all identifying variation comes from within-cell comparisons. To test for violation of conditional random assignment in the adult sample, we will run the regression

$$L_{i,0} = \mathbf{Z}_{i,-1}\lambda + \mathbf{R}_{i,-1}\rho + \mathbf{X}_i\eta + \nu_i, \tag{1}$$

where $L_{i,0}$ is the prize (in \$100,000) awarded to lottery player i at $t = 0$, is$\mathbf{Z}_{i,-1}$ is a vector of pre-win characteristics measured the year prior to the lottery, including a third-order polynomial in age interacted with gender; log of household disposable income (with a lower bound set at SEK 40,000), indicator variables for whether the individual was born in a nordic country, was married and had a college degree. $\mathbf{R}_{i,-1}$ is a vector of pre-win criminal behavior, including dummy variables for being convicted for each of the six main sub-categories of crime listed above during the five-year period preceeding the lottery win and a dummy for any kind of criminal conviction since 1975. $\mathbf{X}_i$ is the vector of cell fixed effects conditional on which lottery prizes are randomly assigned.

For the child sample, we will estimate

$$L_{i,0} = \mathbf{Z}_{f,-1}\lambda_f + \mathbf{Z}_{m,-1}\lambda_m + \mathbf{R}_{f,-1}\rho_f + \mathbf{R}_{m,-1}\rho_m + \mathbf{C}_{-1}\mu + \mathbf{X}_i\eta + \nu_i, \tag{2}$$

where $\mathbf{Z}_{f,-1}$ and $\mathbf{Z}_{m,-1}$ are the same set of pre-win characteristics as in regression 3 but for the child j's biological father and mother, respectively, while $\mathbf{R}_{f,-1}$ and $\mathbf{R}_{m,-1}$ are the corresponding vectors for criminal history. $\mathbf{C}_{i,-1}$ is a vector of child-specific pre-win controls, including a third-order polynomial in age at the time of win interacted with gender.

Because we have not yet matched the true prize vector to the sample, we cannot yet estimate 1 and 2. For both samples, our main test of exogeneity is whether we can reject the null hypothesis of joint insignificance of all predetermined covariates (i.e., both socioeconomic characteristics and previous criminal record) for all lotteries combined. For completeness, we will also estimate regression 1 and 2 for each lottery separately. Yet because the possibility of rejecting the null of joint significance increases with the number of tests in independent samples, we put less emphasis on the tests for each lottery. For each test, we complement p-values based on the analytical standard error with permutation-based p-values constructed by simulating the distribution of the relevant test statistic under the null hypothesis of zero treatment effects (Young 2018).

Our previous studies based on the same sample of lottery winners have provided strong support of the notion that lottery prizes are indeed randomly assigned conditional on the cell fixed effects. However, the amount won may still correlate with previous criminal history or socioeconomic characteristics by chance. If we reject the null of joint insignificance at the 5% level, we will use a subsample of the data were we fail to reject the null as our primary estimation sample, for example by excluding one of the lotteries or groups with unbalanced covariates. Importantly, we will run the exogeneity tests decide whether to make changes to the same before we run the analyses in Section 6.

4.7 Representativeness

An important concern with lottery studies is that lottery players may not be representative of the general population. For each lottery sample, we therefore compare criminal behavior in the five years preceding the lottery event to the representative population samples drawn in 1990 (PLS lottery) and 2000 (Kombi and the two Triss lotteries). We similarly compare the lottery players' basic demographic and socio-economic characteristics (measured the year before the lottery event) to the representative samples. Because criminal behavior and socio-economic characteristics varies substantially with both age and gender, we reweight the representative samples to match the age and sex distribution of each lottery sample. We also compare the pooled lottery sample (with each lottery weighted by its share of the overall identifying variation) to a correspondingly reweighted representative sample.

Table 5 shows the share convicted in the Triss sample is similar to the representative sample, whereas PLS and Kombi lotteries are more law-abiding than the population at large. However, because the two Triss lotteries contribute such a large share of the overall identifying variation (see Table 4 above), the weighted pooled lottery sample is quite similar to the representative sample. For instance, 3.9% of the weighted pooled lottery sample were convicted for a crime in the five-year period preceding the lottery event, compared to 4.4% in the matched representative sample. Figure 8 and 9 provide a visual representation for how the weighted pooled lottery sample compare to the matched representative sample with respect to convictions for different types of crimes and sentences, respectively.

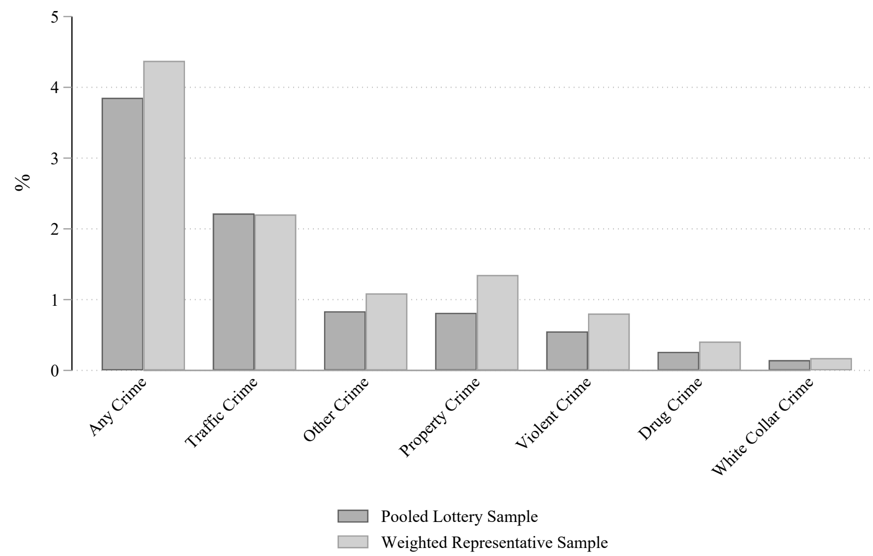
Table 5 also shows lottery players are more likely to be born in the Nordic countries and (except for the PLS lottery) have lower levels of education, but are quite similar with respect to marital status.

Table 5: Representativeness: Adult sample

	Samples							
	Pooled lottery	Matched repr.	PLS	Matched repr.	Kombi	Matched repr.	Triss lotteries	Matched repr.
Criminal record (%)								
Any crime	3.85	4.37	2.33	4.17	2.34	3.46	4.95	4.59
Property crime	0.81	1.35	0.57	1.39	0.40	0.89	1.21	1.41
Violent crime	0.55	0.80	0.19	0.65	0.29	0.52	0.91	0.90
Drug crime	0.26	0.41	0.02	0.17	0.07	0.24	0.46	0.52
White collar crime	0.14	0.17	0.07	0.14	0.09	0.18	0.21	0.17
Traffic crime	2.22	2.20	1.13	1.91	1.42	1.94	2.81	2.34
Other crime	0.84	1.09	0.59	1.16	0.39	0.62	1.10	1.13
Fine	3.19	3.62	2.07	3.51	1.95	2.89	4.11	3.78
Probation	0.79	1.02	0.17	0.79	0.43	0.77	1.10	1.31
Jail	0.45	0.62	0.12	0.57	0.24	0.48	0.67	0.65
Baseline characteristics								
Birth year	1950	1950	1940	1940	1945	1945	1954	1954
Female (%)	48.7	48.7	51.4	51.4	40.8	40.8	49.6	49.6
Nordic born (%)	95.1	91.9	96.8	94.4	98.2	91.9	93.7	90.8
College (%)	20.2	25.3	20.8	17.5	18.7	25.3	19.4	28.0
Married (%)	54.1	53.8	60.7	59.7	57.2	59.9	50.9	50.5
N								

The table shows descriptive statistics for the pooled lottery sample and each of the three subsamples that it constitutes of. We weigh each of the three subsamples by their identifying variation in amount won (the variation in prizes demeaned at the cell-level) when contructing the pooled lottery sample. The matched representative samples have the same distribution of age and gender as their respective lottery samples. We use a representative sample from 1990 to generate the matched sample for PLS and from 2000 to generate the matched samples for Kombi and Triss & Klöver. The criminal record variables give the share in each sample which has been convicted for at least one crime in a given category within the five years preceding the lottery event. The baseline characteristics are measured one year before the lottery event.

Figure 8: Representativeness: Type of Crime



The figure shows the share convicted at least once in the pooled lottery sample (age 18 to 74) during the five-year period preceding the lottery event by type of crime, as well as for the corresponding matched representative sample, weighted by the identifying variation in each lottery.

5 Estimation

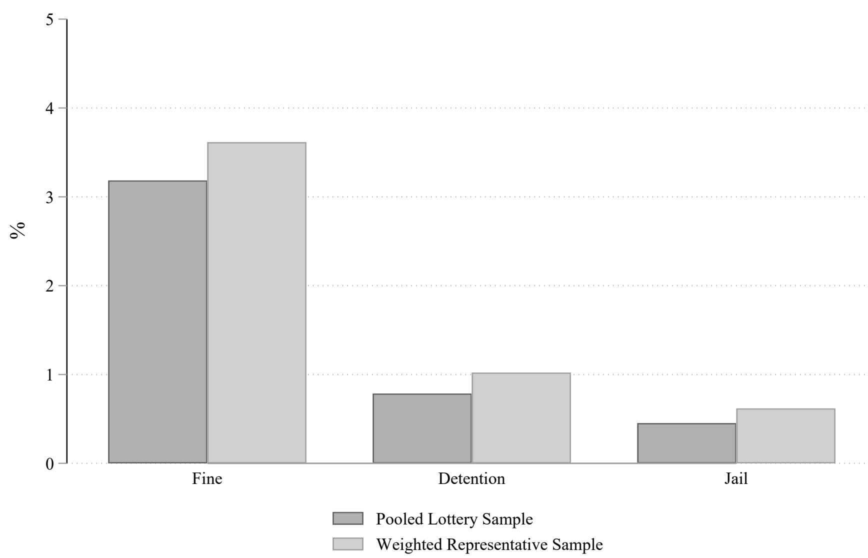
5.1 Estimating Equations

Our identification strategy exploits the fact that the lottery prizes are randomly assigned within each cell. In the adult analyses, we estimate the effect of lottery wealth on players' subsequent criminal activity by ordinary least squares, using the following main estimating equation:

$$y_{i,s} = \alpha L_{i,0} + \mathbf{Z}_{i,-1}\gamma + \mathbf{R}_{i,-1}\phi + \mathbf{X}_i\beta + \epsilon_i \tag{3}$$

where the time of the lottery event is normalized to $t = 0$. $y_{i,s}$ is a measure of criminal activity within s years after winning the lottery. We sett $y_{i,s}$ to missing for individuals who died or were registered as having migrated out of Sweden sometime before year s.

Figure 9: Representativeness: Type of Sentence



The figure shows the share convicted at least once in the pooled lottery sample (age 18 to 74) during the five-year period preceding the lottery event by type of sentence, as well as for the corresponding matched representative sample, weighted by the identifying variation in each lottery.

$L_{i,0}$ is the prize (in million SEK using price level of 2010, about \$150,000) awarded to lottery player i at $t = 0$ and $\mathbf{Z}_{i,-1}$ is a vector of pre-win characteristics, including a third-order polynomial in age interacted with gender; log of household disposable income in the year prior to the win (with a lower bound set at SEK 40,000), indicator variables for whether the individual was born in a nordic country, was married and had a college degree.$\mathbf{R}_{i,-1}$ is a vector of pre-win criminal behavior, including dummy variables for being convicted for each of the six main sub-categories of crime listed above during the five-year period preceeding the lottery win and a dummy for any kind of criminal conviction since 1975. The vectors$\mathbf{Z}_{i,-1}$ and$\mathbf{R}_{i,-1}$are included solely to improve statistical precision.$\mathbf{X}_i$ is the vector of cell fixed effects conditional on which lottery prizes are randomly assigned.

For our child sample analyses, the main estimating equation is

$$y_{ij} = \alpha L_{i,0} + \mathbf{Z}_{f,-1}\gamma_f + \mathbf{Z}_{m,-1}\gamma_m + \mathbf{R}_{f,-1}\phi_f + \mathbf{R}_{m,-1}\phi_m + \mathbf{C}_{i,-1}\theta + \mathbf{X}_i\beta + \epsilon_i \qquad (4)$$

where y_{ij} is a measure of criminal activity of child j of player i. The vectors $\mathbf{Z}_{f,-1}$ and $\mathbf{Z}_{m,-1}$ are the same set of pre-win characteristics as in regression 3 but for the child j's biological father and mother, respectively, while $\mathbf{R}_{f,-1}$ and $\mathbf{R}_{m,-1}$ are the corresponding vectors for criminal history. $\mathbf{C}_{i,-1}$ is a vector of child-specific pre-win controls, including a third-order polynomial in age at the time of win interacted with gender. The vector of cell fixed effects$\mathbf{X}_i$ is the same as in the adult analyses, except that we also condition on the winner's number of biological children prior to the win, thereby ensuring that, within a cell, the amount won per child does not depend on the number of children in the family. Combined with the smaller child sample, conditioning the cells on the number of children implies many odds prize cells become very small. We therefore drop the odds prizes from the child analyses.

In both regression 3 and 4, we let the propensity to commit a crime be a linear function of the lottery win. While most models would predict the effect size to fall with the amount won, a linear specification offers a decent approximation to the data in case outcomes depend on *lifetime* income (Lindqvist, Östling & Cesarini 2020).

We now turn to an evaluation on how well regression 3 and 4 perform with respect to the accuracy of analytical standard errors and statistical power depending on a) how we specify the dependent variable and b) the sample used.

5.2 Evaluating Analytical Standard Errors

Our previous work on the lottery data (Cesarini et al. 2016) has shown that most common types of analytical standard errors can perform rather poorly when the outcome variable is skewed. Before turning to our evaluation of statistical power, we therefore check the performance of different types of analytical standard errors.

We started with three samples: Men and women between 18 and 70 at the time of lottery event, men between 18 and 70, and children to players who were below age 18 at the time of the event. For each sample, we perturbed the (already reshuffled) prize vector 10,000 times. For each perturbation, we estimate the relevant model in 5.1 and calculate the *p*-values for four types of standard errors: unadjusted standard errors; heteroskedasticity-robust standard errors (Huber-White); standard errors adjusted for clustering at the level of the player, and the EDF-corrected robust standard errors suggested by Young (2016).[6] For each perturbation we also saved the maximum *p*-value.

We considered four different specifications for criminal behavior: a dummy for any crime, the log of the number of crimes plus 1 (to avoid log 0), the inverse hyperbolic sine transformation of the number of crimes, and the number of crimes. All outcomes were measured for the five years after the lottery event. Table 6 shows the share of *p*-values below 0.05 for each sample, type of standard error, and specification of the dependent variables.

Four facts stand out from Table 6. First, the rejection rate is too high for all four standard errors regardless of the form of dependent variable. Second, though still too

[6] We use the edfreg-command by Alwyn Young to calculate the EDF-corrected robust standard errors. This command does not allow us to both control for the cell fixed effects and to cluster the standard errors at the level of the player. The reason is that edfreg requires fixed effects included in the absorb-option to be a subset of the units used in the cluster-option. Because of the large number of cell fixed effects, incuding the fixed effects directly in the regression makes computational time prohibitively long.

high, the rejection rate is lower when the dependent variable is binary instead of some function of the number of crimes. Third, the EDF-corrected standard errors performs best in all cases. Fourth, taking the maximum standard error from each perturbation gives a rejection rate close to 0.05 in the dummy-variable case for the male sample but leads to underrejection in the full sample, suggesting the small-sample problem is less severe in the larger sample. (The opposite is true when we consider the number of crimes. A probable explanation is the low female crime rate implies the number of crimes is more skewed in the full sample, implying the small-sample issue are more severe despite the nominally larger sample).

5.3 Evaluating Statistical Power

We now turn to the next step, the goal of which is to find the sample and specification that mazimizes statistical power.

We initially restrict attention to binary dependent variables for whether an individual has commited at least one crime within a certain time period after the lottery event. Because the probability of commiting a crime in any given year is low, and varies substantially by age and gender, we focus on the semi-elasticity – the change in relative risk due to winning the lottery – when evaluating power. Specifically, we evaluate power to reject the null under the alternative hypothesis that a SEK 1 million ($150 K) reduces the risk of committing a crime by 20 percent relative to the crime rate in the matched representative sample (weighted by each lottery's share of the identifying variation) during the same time interval. For reference, we also report power to reject a null of zero effects under the alternative hypothesis that SEK 1 million reduce the propensity to commit a crime with 1 percentage point. For each sample and specification, we perturb the prize vector 200 times and calculate the maximum of the four different analytical standard errors described above. We then use the average of these 200 maximum standard errors when calculating statistical power. Because of the accuracy of the analytical standard errors may vary across specifications, as a complement we also calculate the share of perturbations for which the estimated coefficient implies an estimated effect less than 10 percent of the crime rate in the matched representative

Table 6: Evaluating Standard Errors

		Type of standard errors				
Sample	Dependent variable	Conventional	Robust	Clustered	EDF	Max
Men 18-70	Dummy for any crime	0.0965	0.0853	0.0854	0.0770	0.0496
	Log number of crimes	0.1357	0.0963	0.0963	0.0874	0.0539
	IHS number of crimes	0.1345	0.0962	0.0962	0.0879	0.0529
	Number of crimes	0.1661	0.1282	0.1282	0.1159	0.0678
All 18-70	Dummy for any crime	0.0909	0.0713	0.0710	0.0662	0.0420
	Log number of crimes	0.2133	0.0790	0.0791	0.0753	0.0736
	IHS number of crimes	0.1444	0.0972	0.0971	0.0926	0.0633
	Number of crimes	0.2253	0.1206	0.1207	0.1130	0.1029
Children	Dummy for any crime					
	Log number of crimes			To be added		
	IHS number of crimes (2)					
	Number of crimes (3)					

This table reports the share of p-values for the amount won in regression 3 (two upper panels) and 4 (lower panel) which are below 0.05 for four different types of standard errors (conventional, heteroskedasticity-robust, clustered at the level of the player, EDF-corrected robust) and the largest of these four based on 10,000 perturbations of the lottery prize vector. The sample is restricted to men between age 18 and 70 at the time of winning and the dependent variables are measured 5 years after winning. The set of control variables include age and dummies for gender, college degree and any any conviction prior to winning.

sample (the semi-elasticity) and an absolute effect smaller than 0.5 percentage points.

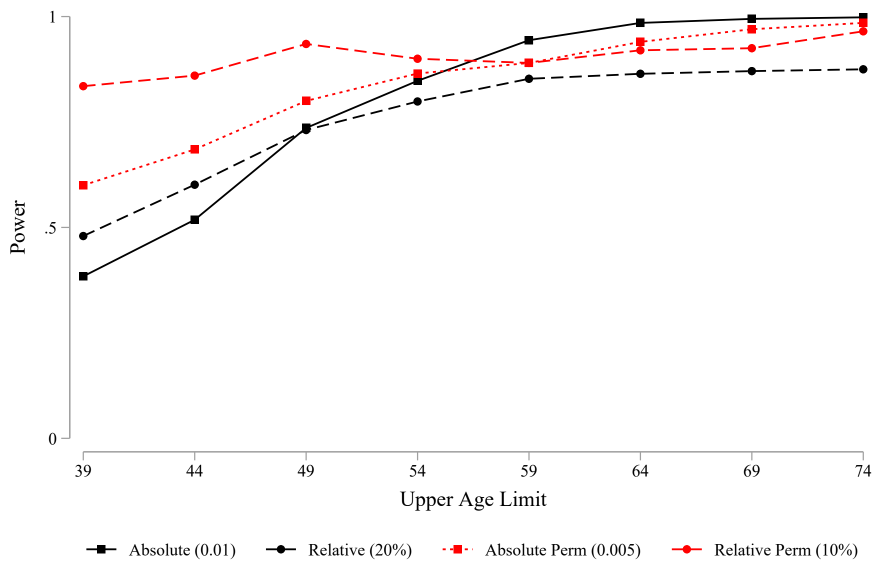
We start by considering how statistical power varies with age and gender, focusing on criminal behavior for the first five years after the lottery win. Figure 10 shows how power changes when we keep the lower age limit at 18 and increase the upper age limit from 39 up to 74. As expected, the power to reject an absolute effect of 1 percentage point monotonically increases as we expand the sample (from 51.8% to 99.8%). Less obvious, we also see power to reject a relative effect of 20 percent (the semi-elasticity) increasing from 60.1% to 87.5%. The reason for the less steep increase is that, though increasing sample size reduces standard errors of the absolute effect, the crime rate in the matched representative sample falls from 5.5% to 3.2% as the upper age limit is expanded. Figure 10 also shows the share coefficients inside a 10%-relative effect is highest when 74-year-olds are included in the sample, though the increase is not as steep as for power based on analytical standard errors. While the using the distribution of estimated coefficients thus suggest a smaller power-advantage than the analysis based on analytical standard errors, there is no evidenve of lower power due to expanding the upper age limit.

Figure 11 shows the corresponding analyses as Figure 10, but with the sample restricted to men. Except for a bump up for the last age group (age 74) in the permutation-based calculation of the relatively effect, power is systematically lower when the sample is restricted to men. Based on the results in Figure 10 and 11, we use the full sample of men and women between age 18 and 74 at the time of win as our baseline sample.

The analyses above are based on criminal behavior within five years after the lottery event. But what about a shorter or longer time horizon? Figure 12 shows how power for the full sample of men and women between 18 and 74 changes when we expand the time horizon from 1 to 10 years after the lottery event. Power with respect to the absolute effect falls monotically, reflecting the larger variance of the dependent variable as the crime rate increases with time and (from 7 years after the lottery) the exclusion of the latest cohorts of winners from the sample. Yet, more relevant for our purposes, power with respect to the relative effect increases with time since the lottery.

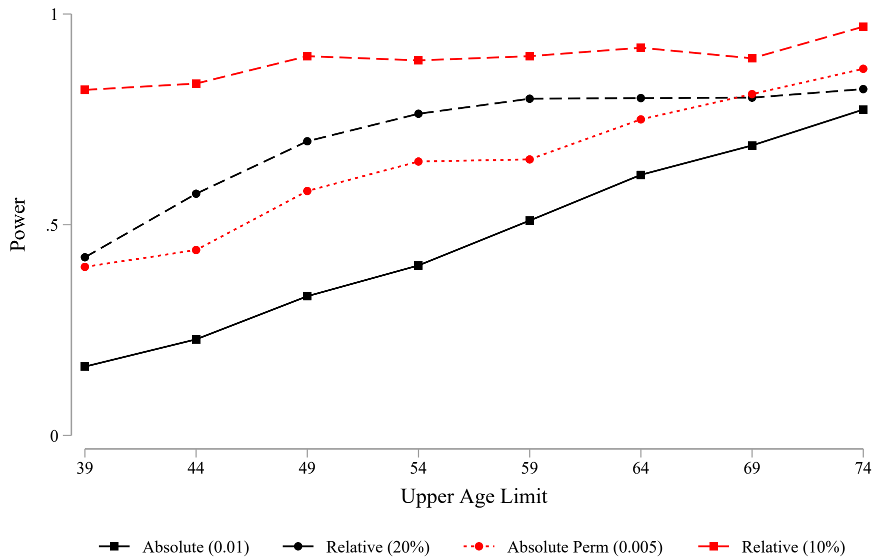
Having evaluated power for the binary case, we now turn to ways of specifying the

Figure 10: Power: Age restrictions



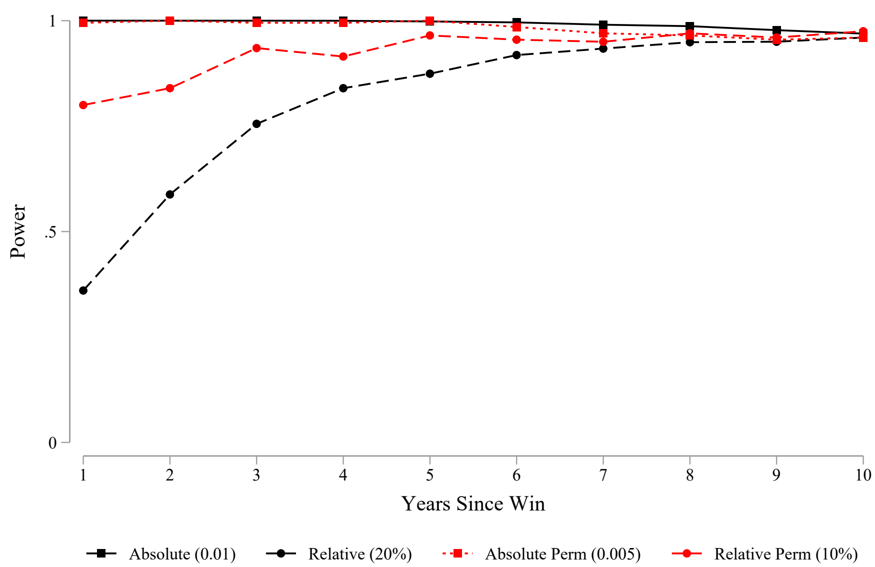
The figure shows how statistical power changes as the upper age limit of the estimation sample increases (with the lower age fixed at 18 for all analyses). Power is shown for both the absolute effect (+/- 1 percentage point) and the effect relative to the crime rate in the matched representative sample (+/- 20 percent). The black lines shows power based on analytical standard errors. The red lines show the share of perturbated coefficients within +/- 0.5 percentage point and +/- 10 percent.

Figure 11: Power: Age restrictions (only men)



Same as Figure 10, except the sample is restricted to men.

Figure 12: Power: Time horizon



The figure shows how statistical power changes as the time horizon expands from 1 up to 10 years after the lottery win. The sample includes all men and women in the lottery samples between the age of 18 and 74 at the time of the win. The definition of statistical power is the same as in Figure 10.

dependent variable which take criminal behavior on the intensive margin into account. As shown in Table 6, analytical standard errors imply substantial overrejection when we use the number of crimes as the dependent variable. Despite this, we find estimated statistical power for relative effects to be lower for all alternative specifications of the dependent variable. Using the analytical standard errors, power to reject the null is 87.5% for the relative effect in the binary case compared to between 44.1% (number of crimes) and 78.7% (IHS function of number of crimes) for the alternative specifications. The permutations-based test of power gives the same rankings.

Based on the analyses in this section, our main analyses will focus on the full sample of men and women between age 18 and 74 at the time of the lottery event. The outcome variables will be dummy variables equal to 1 in case an individual has committed a certain type of crime within 10 years after the lottery event.

6 Analyses

In this section, we pre-specify the main analyses in the paper.

6.1 Statistical Inference

As shown above, analytical standard errors typically induce overrejection when estimating different versions of regression 3 and 4. Throughout, we will therefore report for each regression the maximum of standard errors which are unadjusted; heteroskedasticity-robust (Huber-White) or adjusted for clustering at the level of the player, as well as and the EDF-corrected robust standard errors suggested by Young (2016). In addition, we will also report permutation-based p-values constructed by simulating the distribution of the relevant test statistic under the null hypothesis of zero treatment effects (Young 2018). Finally, in our main analyses of the primary outcomes, we also report p-values that have been adjusted to account for the fact that we examined four primary outcomes. To calculate these family-wise error rate adjusted p-values, we apply the free step-down resampling method of Westfall & Young (1993).

In the tables, we refer to the resulting p-values as FWER-adjusted p-values.

6.2 Primary Outcomes

For the adult analyses, the main outcome variable of interest is an indicator variables equal to 1 if an individual is convicted at least once in the 10 years after winning the lottery. We will also consider convictions by the six different types of crime defined in Table 1 and by type of sentence. FWER-adjusted p-values will be reported separately by type of crime and type of sentence. The format for reporting the main results is provided in Table 7. The child analyses follow the same pattern, with two exception. First, the outcome is defined as having ever committed a crime by year 2017. Second, to separate between criminal behavior in adolescence and adulthood, we consider two subcategories for "any crime" depending on the age of the child: 15-19 and age 20 and above.

Table 7: Main Analyses

		Type of Crime						Type of Sentence		
	Any Crime	Property	Violent	Drug	White collar	Traffic	Other	Fine	Detention	Jail
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(7)	(8)	(9)
Effect (M SEK)										
SE										
p (analytical)										
p (resampling)				To be estimated after publication of the plan						
FWER p										
N										

This table reports the effect of winning the lottery on criminal behavior. Each column reports results from a separate regression in which the dependent variable is an indicator variable equal to one in case of a conviction for a certain type of crime, or certain type of sentence, within 10 years after the lottery event. The sample includes lottery winners and controls between age 18 and 74 at the time of the win. In all specifications, we control for baseline characteristics measured at $t = -1$. Standard errors is equal to the maximum of conventional standard errors; Huber-White standard errors; standard errors adjusted for clustering at the level of the winner and the EDF-corrected robust standard errors suggested by Young (2016). The resampling-based p-values are constructed by performing 10,000 simulations. FWER p-values are calculated separately for the analyses in columns (2)-(6) and (7)-(9).

6.3 Exploratory analyses

To provide context to the main analyses listed above, we pre-specify a number of exploratory analyses below. All the analyses will be available for readers of the paper, though some may only be included in an online appendix.

First, Table 8 shows a number of alternative definitions of criminal behavior. To test for the possibility that wealth shocks affects the risk of getting convicted, conditional on committing a crime, we will test the effect of lottery wins on ever being suspected for a crime during the 10 years following the lottery event (column 1). Because traffic crimes constitute such a large share of all crimes, we also consider a supercategory which includes all crime except traffic crime (column 2). To test the theoretical predictions of a negative income effect on economically motivated crimes, we define a super-category of "production crimes", including all property and white collar crimes, and more serious drug crimes (column 3). The rationale for separating between petty and serious drug offences is our belief that petty drug crimes are more likely to reflect own consumption, whereas serious drug offenders are more likely to have sold or smuggled drugs, or handled larger quantities with the likely intention to sell. This assumption is supported by correspondence we have had with those within and familiar with the Swedish criminal justice system. Columns 4 and 5 separate between two subcategories of property crime – stealing vs. fraud and embezzlement – while columns 6 and 7 separate between serious and minor drug offences. We consider the same alternative definitions for the child sample.

Table 8: Alternative definitions of crime

			Property	Type of property crime		Type of drug crime	
	Suspect	Any crime - traffic	+ serious drug + white collar	Stealing	Fraud + embezzlement	Serious	Minor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect (M SEK)							
SE							
p (analytical)			To be estimated after publication of the plan				
p (resampling)							
N							

This table reports the results from alternative defintions of criminal behavior. The samle is the same as in Table 7. All outcome variables are indicator variables equal to 1 in case an individual has committed a certain type of crime within 10 years after the lottery event, except for *Suspect*, which is equal to 1 in case an individual appears in the Suspect registry. Fraud and Embezzlement includes chapter 9, 10 and 11 of the criminal code.

Second, we will test the sensitivity of our results in the main analyses with respect to dropping prizes exceeding 4 million SEK ($580K).

Third, we consider the evolution of the effect over time. To obtain acceptable statistical power, we define outcomes in four three-year periods (1-3; 4-6; 7-9 and 10-12 years after the lottery event) and only consider "any"-type of crime. To avoid changes in estimates due to changes in sample composition, we estimate these regressions only for winners who won in 2005 or earlier.

Fourth, we consider a number of heterogeneity analyses. In the adult analyses, we will consider the following dimensions:

- Any criminal conviction prior to winning (yes/no)
- Age (above or weakly below age 49)
- Gender (male/female)
- Income (above or below the median in the age-year-gender cell in the representative sample, with university students coded as "above median" regardless of their current income)
- Educational attainment (with or without a three-year post-secondary degree)

In the child analyses, we consider heterogeneity according to the following dimensions:

- Age at the time of the parents win (above or weakly below age 9)
- Parental income (combined parental income the year before the lottery event above or below median among parents in the representative sample in the same year)

6.4 Benchmarking the estimates

We compare the distribution of simulated coefficients to two different benchmarks: The gradient with respect to disposable income and income elasticities from previous work. To make the comparisons, we proceed in three steps, the first of which is to convert the lottery prizes to income streams. Because lump-sum lottery prizes represent one-time increases in wealth, converting them to income streams require us to make assumptions regarding the intertemporal behavior of lottery winners. The evidence from previous studies suggest winners spread out the gains over long time horizons (Cesarini et al. 2016) and often treat the windfall as a long-run supplement to annual income flows (Cesarini et al. 2017). We therefore follow previous studies on the same lottery data (Cesarini et al. 2016, Lindqvist, Östling & Cesarini 2020) and calculate, for each lottery prize, the annual payout it could sustain if it were annuitized over a 20-year period at an actuarially fair price. To illustrate, a \$100,000 prize corresponds to an increase in net annual income of \$5,996.

In the second step, we calculate average household disposable income during the five years prior to the lottery event. As in Section 3.3, we set annual household income to SEK 40,000 (\$6,000) in case reported income is lower.

In the third step, we use the lottery win as as an instrument for the log of the sum of permanent income and the annuitized prize, in a specification otherwise the same as regressions 3 and 4.

We compare the thus rescaled lottery-based estimates to income gradients for the matched representative sample calculated in the same fashion as Section 3.3, except we abstain from imposing any restrictions in addition to those in the lottery sample. Because our lottery-based estimates are now expressed in logs, dividing the estimated effect on the propensity to commit crime by the baseline rate (in the matched representative sample) implies our estimate is comparable to income-elasticities from previous work.

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