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

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May 28, 2021

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; the Swedish criminal justice system; our data on criminal convictions, and provide descriptive statistics for crime in Sweden. We then present our sample of lottery players and how they compare to the population in terms of crime risk. Next, we use Monte Carlo simulations to evaluate analytical standard errors and the statistical power of different sample restrictions and outcome variable definitions. Finally, we pre-specify the analyses we will report in the paper.

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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 behavior. 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.

We started working on the plan after having obtained access to both the lottery and crime data. Though all authors of this plan pledge that the true prize vector has not yet been matched to the data on criminal convictions, the fact that we have had access to the data implies credible evidence for this assertion cannot be presented. However, at a minimum, the plan shows that the specifications chosen outperform a large set of alternative specifications with respect to statistical power. Moreover, our evaluation of analytical tandard errors leads us to adopt methods for statistical inference which prove to be more conservative than what is 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 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 first generation of models of crime abstract from two potentially relevant mechanisms.[2] First, because total labor supply is assumed to be fixed, wealth has no effect on leisure and only affects the allocation of labor between legal and illegal activities. In more recent work, 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). 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. 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 to

[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 behavior (Lochner & Moretti 2004, Lochner 2004).

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

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. On the other hand, higher wealth may induce 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.

In sum, economic theory does not provide a clear prediction for how wealth affects criminal behavior. While the first generation of papers emphasized risk aversion, increasing demand for leisure from criminal activity and a higher utility loss from imprisonment suggest wealth reduces the propensity to commit crimes for economic gain. And while higher wealth implies as illicit goods, legal goods which are "inputs" in criminal acts, and fines all become more affordable, it is easy to come up with examples in which wealthy changes consumption patterns in a more law-abiding direction.

2.2 Sociological and Criminological Theories

While economic theory is ambigious, the leading theories in sociology and criminology emphasize poor economic and social circumstances as a leading cause behind criminal behavior. 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 perceived to be prevalent in American society at the time, and lack of economic resources the principle factor contributing to criminal behavior. In line with strain theory, subsequent works focus on social constructs and their interaction with economic disadvantage to explain crime, including Cohen's (1955) 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) and education (Lochner & Moretti 2004, Meghir, Palme & Schnabel 2014, Hjalmarsson, Holmlund & Lindquist 2015, Bennett 2016). These studies generally find that crime is positively associated with unemployment and negatively associated with education.

Much of the early empirical literature on the relationship between economic circumstances and criminal behavior 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.

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 behavior 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 exogenous sources of variation in economic means at the individual level, 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 behavior 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 while 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 labor market. As individuals become increasingly attached to the labor 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 behavior 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

We use 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) to measure criminal behavior. The unit of observation in this data set 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 do an initial classification of 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 into 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 include 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 laundering. 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 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. Examples of such crimes include arson, counterfeiting, rioting, incitement, and poaching. A list of the crimes we assign to each category is included in Table 1. Importantly, a given crime can belong to multiple categories. For instance, we classify driving under the influence of narcotics as both a traffic and a drug crime.

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

Table 1: Initial 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).

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.

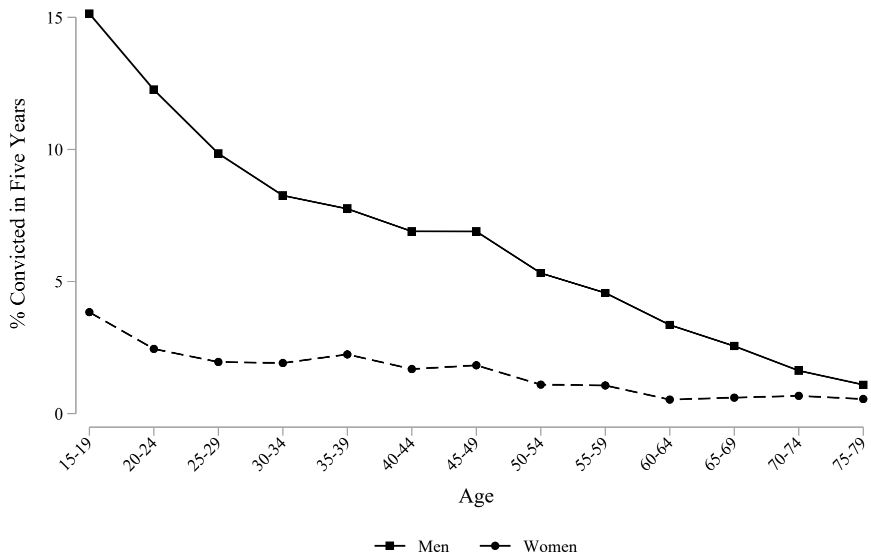
Apart from fines, most forms of punishment constitute some form of restriction of freedom. These range from to community service and probation for lesser crimes to long prison sentences for the most severe crimes. In many cases, underage offenders between the ages of 15-20 are sentenced to either juvenile 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 from the Suspects Registry (*Misstankeregistret*). This registry, which is compiled by the Swedish National Council for Crime Prevention, includes information on individuals suspected on reasonable grounds during 1995-2017 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 begin 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 79 for five years from the year the sample was drawn. People who die or move abroad within this five-year period are coded as missing. In line with previous research from Sweden (Wikström 1990), Figure 1 shows that men are much more likely to be convicted than

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.

women at all ages, and that the propensity to commit crimes falls 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 is traffic crime for men and property crime for women. 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. Yet this group is responsibe for 58 percent of all recorded crimes in our data.

Income Gradients

We now describe the relationship between criminal behavior 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 individuals aged 30-54 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 sampling year. To avoid simulaneity bias, we measure the share convicted during the five years after the sample was drawn.

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 for men is similar when we use their own disposable income instead of the household's, but considerably flatter for women.[4] We also find the gradients get steeper when we restrict attention to more severe 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.

[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)

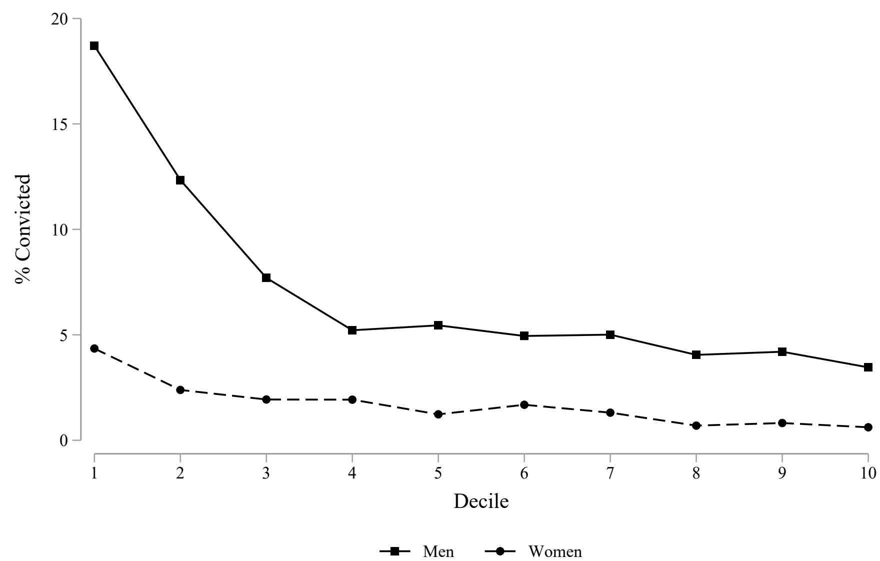
	Men	Women
Fine	5.93	1.32
Detention (including jail)	1.97	0.23
Jail	1.14	0.08

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.

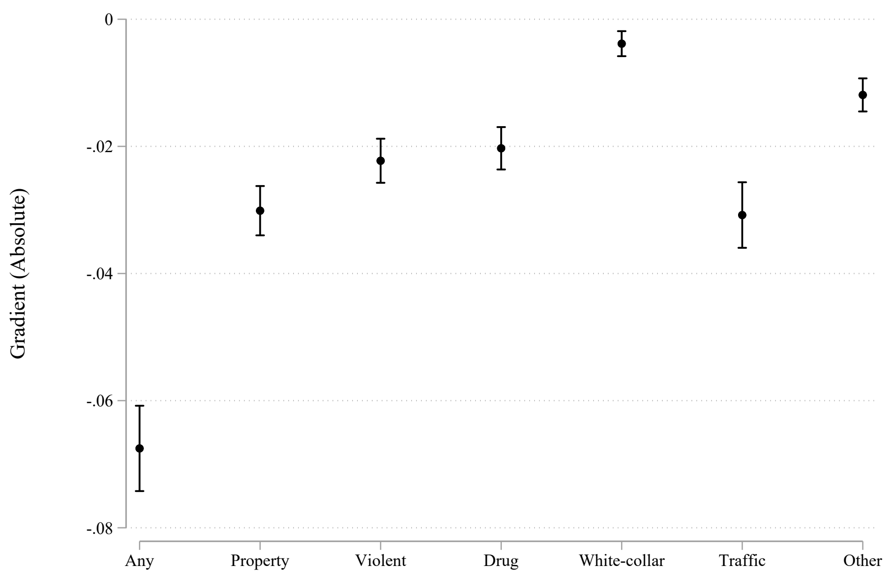
We now turn to the question of whether the crime-income gradients vary by type of crime. To investigate this, we restrict the sample to men between 30 and 54 and regress indicator variables for having been convicted for each type of crime on the log of average household income during the five years prior 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 percentage point lower risk of being convicted for any type of crime. The corresponding number for the 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 expressed in terms of an elasticity (though clearly a causal interpretation is uncalled for). The elasticity is in the ballpark of 1 for committing 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.

Parental Background and Children's Criminal Behavior

We now turn to the relationship between parental background and children's propensity to commit crime. We focus on the children born to parents in the representative samples used above. First, we consider a simple illustration of the intergenerational gradient in crime. Figure 5 shows how the conviction rate at age 15-19 vary with gender and parental convictions when the children were age 10-14. In line with previous research (Farrington 2003), boys for whom one parent was convicted of a crime are twice as likely to be convicted as teenagers compared to boys for whom neither parent were convicted. When both the parents were convicted, are three times as likely to be convicted. Though teen crime rates are substantially lower for girls, the difference in conviction risk by parental criminality is even starker than for boys. The intergenerational gradient in crime could be mediated by many factors, including lack economic resources. To get an idea about the relationship between parental income and children's criminal behavior,

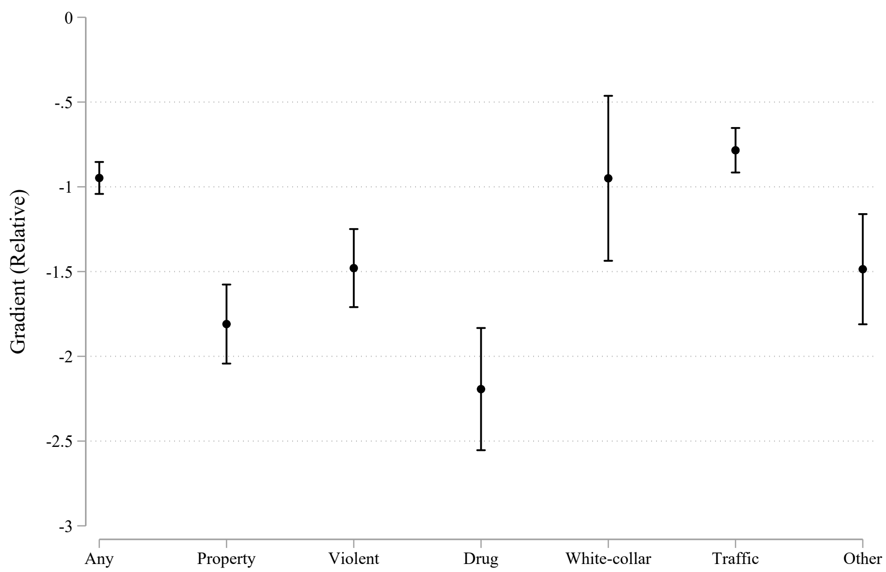
[5] The SEK/Dollar exchange rate was 6.72 on Dec 31st 2010.

Figure 3: Absolute Income Gradients by Type of Crime



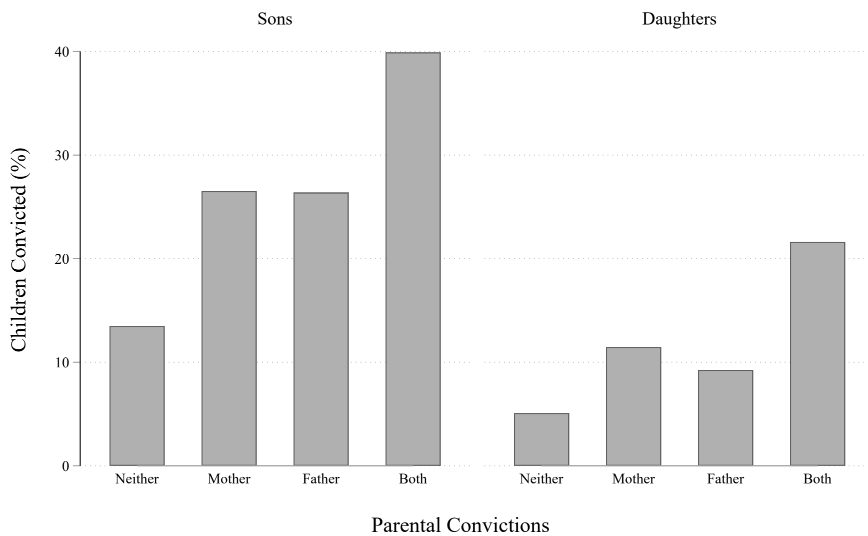
The figure shows the coefficients from regressing a dummies for being convicted for a given type of crime during a five-year period on the log of average household disposable income during the preceding five-year period. The sample consists of men between age 30 and 54 from representative samples drawn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

Figure 4: Relative Income Gradients by Type of Crime



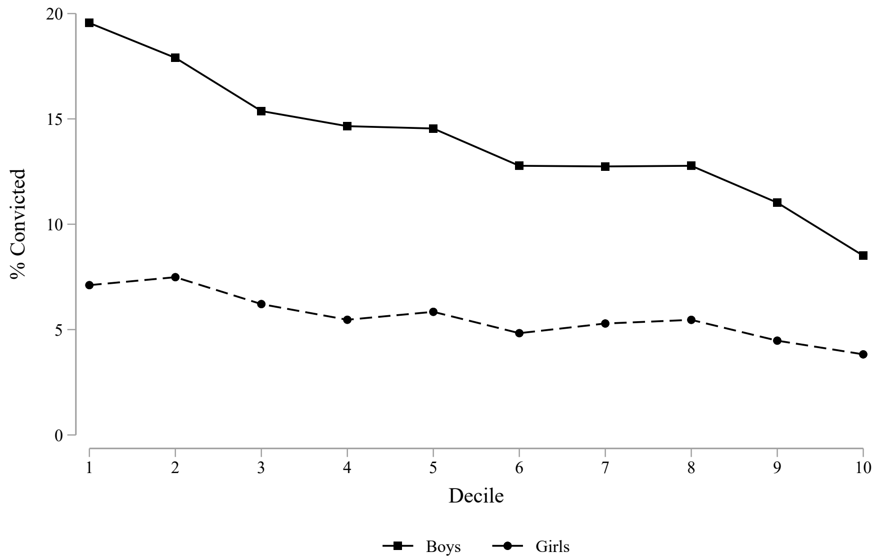
The figure shows relative income gradients based on regressing a dummies for being convicted for a given type of crime during a five-year period on the log of average household disposable income during the preceding five-year period. The coefficients have been divided by the average crime rate in the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

Figure 5: Crime at Age 15-19 by Parental Convictions



The figure shows the share of children from the representative samples drawn in 1990, 2000 and 2010 convicted for at least one crime at age 15-19 by parental convictions. Parental convictions are measured as whether either parent (or both) were ever convicted for a crime when the child was age 10-14.

Figure 6: Crime at Age 15-19 by Parental Income



The figure shows the share of children from the representative samples drawn in 1990, 2000 and 2010 convicted for at least one crime between age 15 to 19 by parental income decile. Parental income decile is based on the parents combined disposable income when the child was age 10-14 by three-year groups.

Figure 6 shows how the share of children convicted between age 15 and 19 varies with the parents' total income while the children were between 10 and 14 years of age. In the bottom decile of parental income, 19.6% of boys and 7.1% of girls are convicted for at least one crime, compared to 8.5% and 3.8% in the top decile. Though the conviction rate falls with age, the relative difference in the conviction rate increases. For instance, while boys from the lowest income decile are 2.3 times more likely to be convicted at age 15-19 compared to boys from the top decile, they are 2.9 times more likely to be convicted at age 25-29. Correspondingly, the 1.9 times higher rate for the girls from the bottom vs the top decile at age 15-19 increases to 3.4 at age 25-29.

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 data from a number of sources to place crime in Sweden in an international context. The United Nations Office on Drugs and Crime (UNODC) collects and publishes data documenting the pervasiveness of crime across countries. Figure 7 displays the number of persons brought in formal contact with the criminal justice system in 2005 for a sample of OECD countries. While Sweden appears in the bottom half of the ranking, it lies only slightly below the average for European countries in the sample.

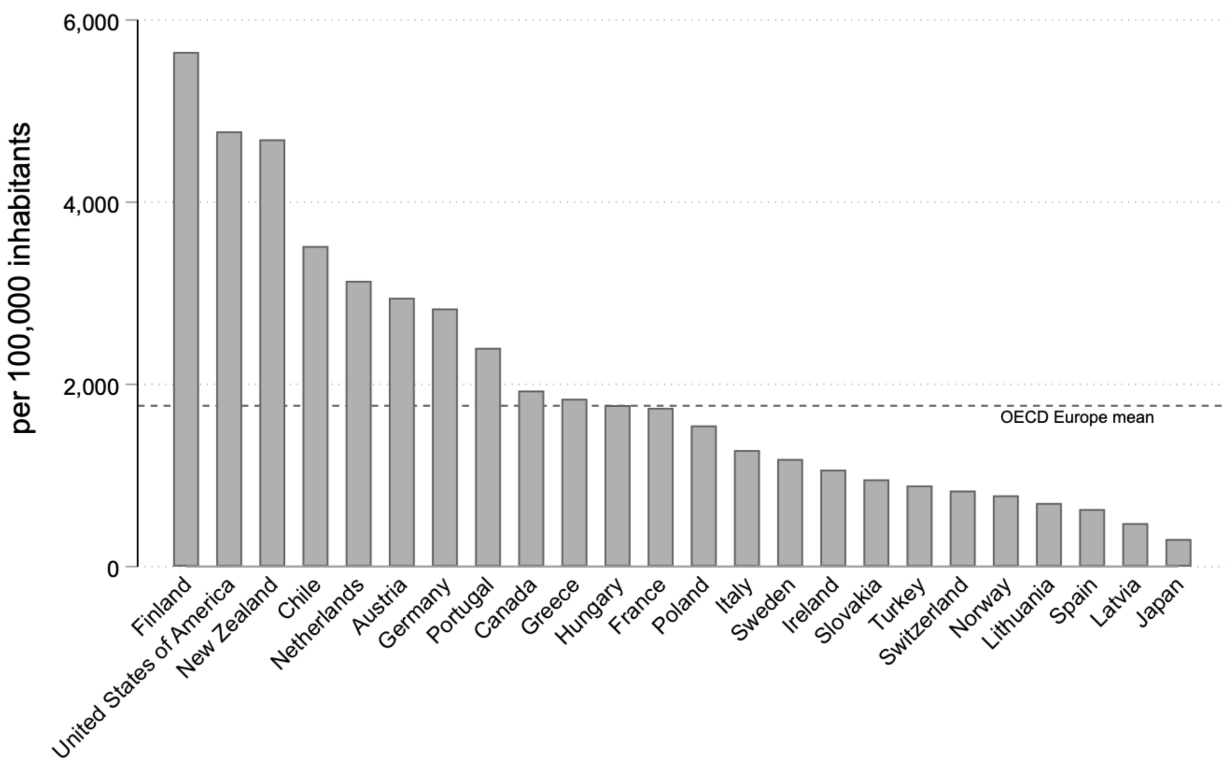
A major factor that affects crime statistics and hinders not only international comparisons, but also longitudinal studies of crime, are differences in willingness to report crimes across jurisdictions and time. In countries where crime is high, low willingness to report crimes through official channels will result in crime statistics that underestimate the true rate of criminality. In an attempt to bypass differences in police reporting rates, the International Crime Victim Survey (ICVS) elicits data on criminality by surveying households across countries directly. Figure 8 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.

To provide a picture of the relative willingness to report crimes in Sweden, Figure 9 plots the percentage of property crimes and assaults which survey respondents reported to police between 1994-1999. For both types of crime, Sweden falls roughly in the middle of the ranking of countries covered in the survey.

4 Lottery Samples

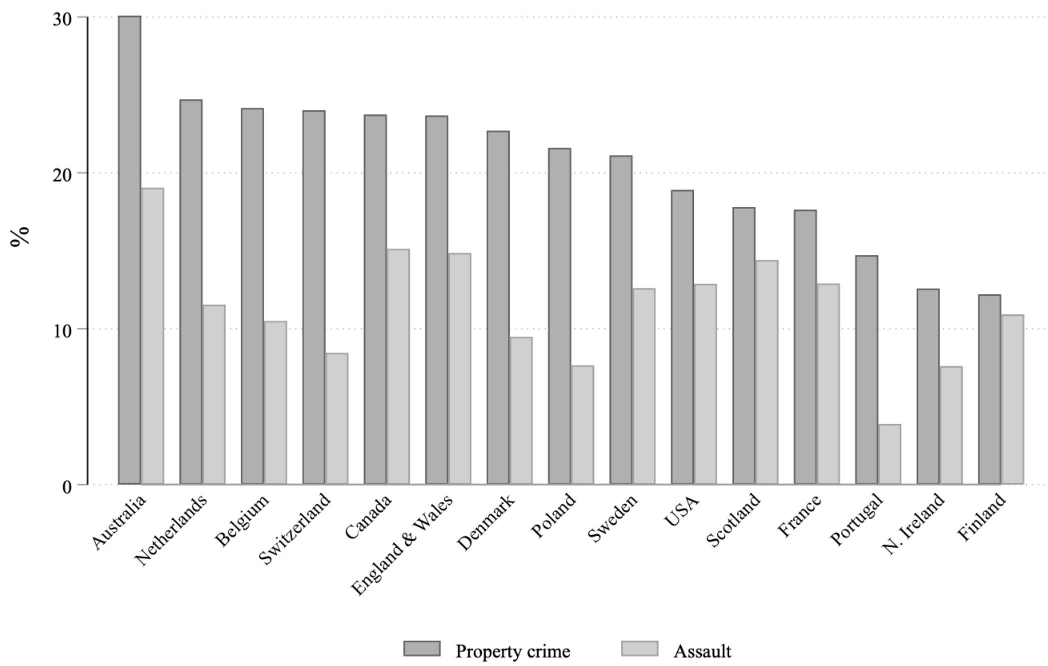
We construct our estimation samples by matching three samples of adult lottery players (age 18 and above) and their spouses to the crime data described above, as well

Figure 7: Persons Brought in Contact with the Criminal Justice System



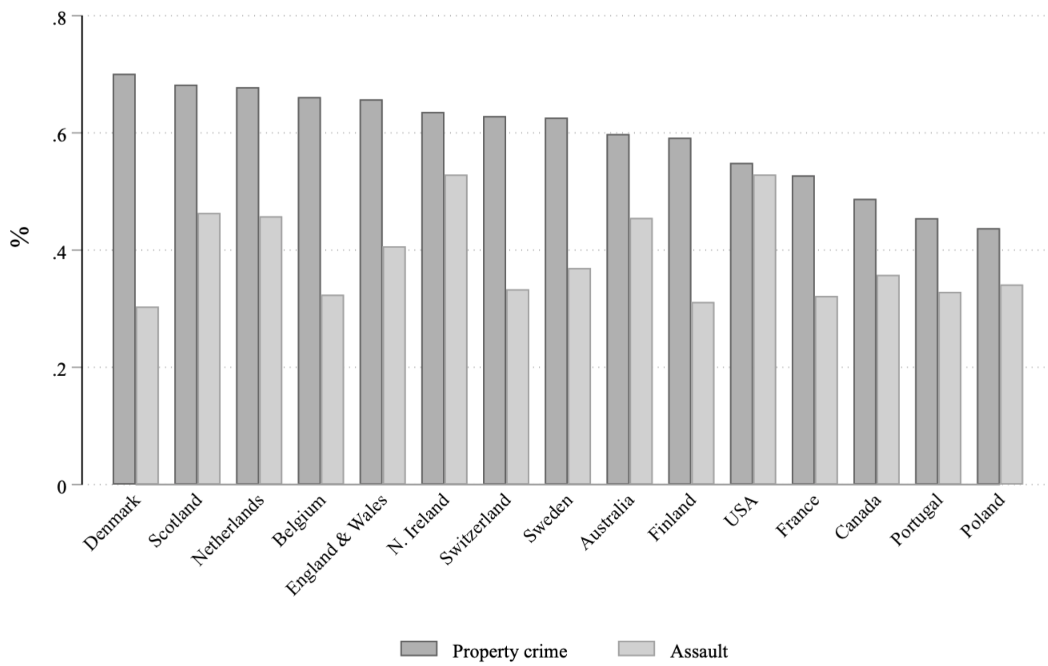
Source: United Nations Office on Drugs and Crime.

Figure 8: Percentage of Households Vicitim to Property Crime and Assault, 1994-1999



Source: International Crime Victim Survey.

Figure 9: Share of Crimes Reported, 1994-1999



Source: International Crime Victim Survey.

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

	Time Period	Treatment Variable	Cell construction: Adults	Cell construction: Children
PLS Fixed Prizes	1986-2003	Prize	Draw	Draw
PLS Odds Prizes	1986-1994	Prize	Draw ∗ Balance	Draw ∗ Balance
Kombi Lottery	1998-2011	Prize	Draw ∗ Balance ∗ Age ∗ Sex	Draw ∗ Balance ∗ #Children ∗ "Close" Child Age and Gender
Triss-Lumpsum	1994-2011	Prize	Year ∗ Prize Plan	Year ∗ Prize Plan
Triss-Monthly	1997-2011	NPV	Year ∗ Prize Plan	Year ∗ Prize Plan

as population-wide registers on socioeconomic outcomes from Statistics Sweden. Our 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) born no later than 2002. We impose the latter restriction as children born after 2002 are too young to reach the age of criminal responsibility of 15 during the period of study.

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 "cells" within which the amount won is random. We control for cell fixed effects in all analyses, thus ensuring all identifying variation comes from players (or children of players) in the same cell. Table 3 summarizes the cell construction, to be described in detail for 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.

The cell construction for the child sample is similar to the adult sample, except that we match children whose lottery-playing parents match with respect to number of fixed prizes or the account balance (odds-prize cells).

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 include information about the number of tickets held in each draw and information about prizes exceeding 1M SEK. Two individuals with the same number of tickets in a Kombi draw have the same chance of winning a large prize. We construct the Kombi cells by matching 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.

For the child sample, we match winning parents to control parents with the same number of lottery tickets and children. If more than 100 such "control families" are available, we choose the 100 families who are most similar to the winning family in terms of the age and sex of the children.

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. The construction of the child cells are analogous to the adult cells.

4.4 Baseline Estimation Samples

The baseline sample we use for evaluating analytical standard errors and statistical power consists of all winners and controls who turned at least 18 and no more than 74 in the year of the lottery draw. We impose the upper age restriction as crime rates above age 75 are low (see Figure 1). Merging the three lotteries gives us a sample of 356,960 lottery players within the relevant age range. Primarily because many PLS lottery players win small prizes several times, these observations correspond to 282,597 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 (iii) cells without variation in the amount won. After imposing these restrictions, we end up with an estimation sample of 354,053 observations (280,760 individuals).

We have 125,626 observations of children whose parents play the lottery, corresponding to 104,841 unique children. As for the adult sample, we exclude shared prizes in the Triss lotteries and cells without variation. We also restrict the sample to children whose both parents were i) alive the year before the lottery draw and ii) have non-missing values on basic socio-economic characteristics.[6] After imposing these restrictions, our child sample consists of 120,154 observations and 100,940 unique children of 60,058 lottery-playing parents (29,182 mothers and 30,876 fathers) who won a total of 69,256 prizes.

4.5 Prize Distribution

Table 4 shows the distribution of prizes in the adult and child samples. All lottery prizes are net of taxes and expressed in units of year-2010 SEK. Panel A shows the total prize amount in our adult sample is a little over 6 billion SEK (about $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 most of the within-cell variation in amount won (35%). Panel B shows the total prize pool in our child sample is a bit over 1.3 billion SEK ($900 million). Compared the adult sample, the Triss-Lumpsum sample is relatively more important for identification, while the Kombi lottery less important.

[6]We exclude disposable income from the set of socioeconomic characteristics we require from parents for children to be included in the data set.

Table 4: Distribution of Prizes Awarded

	A. Winners (adult analyses)					B. 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	37,139	0	37,139	0	0	4,265	0	4,265	0	
1K to 10K	286,767	286,767	0	0	0	58,259	58,259	0	0	
10K to 100K	22,964	21,738	0	1,226	0	5,011	4,691	0	320	
100K to 500K	4,715	2,244	0	2,471	0	1,210	500	0	710	
500K to 1M	498	275	20	203	0	101	52	0	49	
1M to 2M	1,311	643	358	63	247	245	132	44	13	56
2M to 4M	449	29	20	95	305	112	9	1	23	79
>4M	210	0	5	63	142	53	0	1	17	35
N	354,053	311,696	37,542	4,121	694	69,256	63,643	4,311	1,132	170
Sum (M SEK)	6,138	2,362	491	1,277	2,007	1,372	499	55	331	487
% of variation.	100.0	26.9	10.8	35.4	26.9	100.0	26.1	5.6	46.6	22.0

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 estimate the following model

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

where $L_{i,0}$ is the prize (in million SEK, about \$150,000) awarded to lottery player i at $t = 0$, $\mathbf{Z}_{i,-1}$ is a vector of pre-win socio-economic characteristics measured the year prior to the lottery, including a third-order polynomial in age interacted with gender; log of household disposable income, indicator variables for whether the individual was born in a Nordic country, was married and had a college degree.[7] $\mathbf{R}_{i,-1}$ 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

$$L_{i,0} = \mathbf{Z}_{p,-1}\lambda_p + \mathbf{R}_{p,-1}\rho_p + \mathbf{C}_{-1}\mu + \mathbf{X}_i\eta + \nu_i, \tag{2}$$

where $\mathbf{Z}_{p,-1}$ is a vectors of pre-win socio-economic characteristics of child j's biological parents and $\mathbf{R}_{p,-1}$ is a vector for the parents' criminal history. $\mathbf{Z}_{p,-1}$ includes third-order polynomials in the mother's and father's age, the log of the average of the parents' combined disposable income during the five years preceding the lottery draw, and indicator variables for whether the each parent was born in a Nordic country, was married

[7]Household disposable income is defined as the sum of own and (if married) spousal disposable income. Own and spousal disposable income is winsorized at the 0.5th and 99.5th percentile for the year in question before summing them us. To avoid a disproportionate influence for values close to zero, we winsorize household disposable income at SEK 40,000 (about \$6000) before taking the log.

and had a college degree. $\mathbf{R}_{p,-1}$ is the same vector of pre-win criminal behavior as in model 1 above, except we include the mother's and father's criminal record separately. $\mathbf{C}_{i,-1}$ is a vector of child-specific pre-win controls, including a third-order polynomial in age at the time of win interacted with gender and a dummy for born in the Nordic countries.

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 models 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 the individual lotteries. 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 where 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 and decide whether to make changes to the sample 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 differ 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 10 and 11 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.

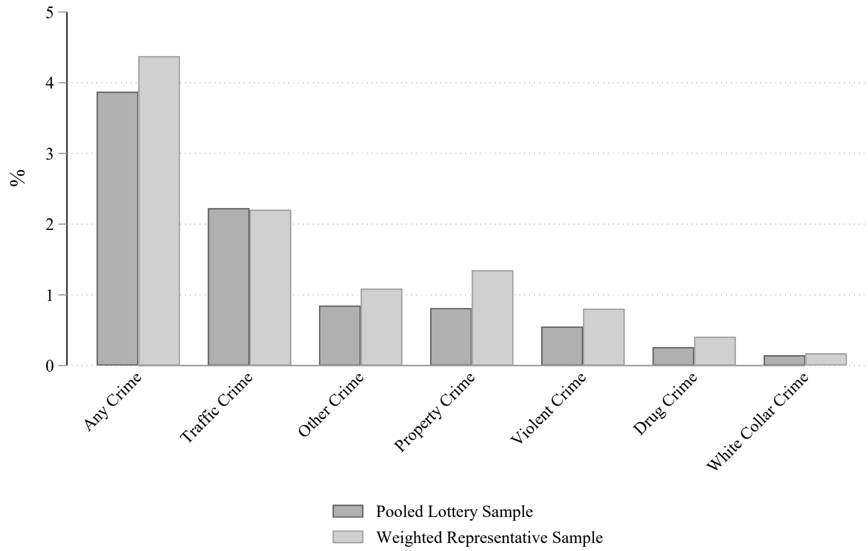
Table 5 also shows lottery players are more likely to be born in the Nordic countries and (except for the PLS lottery) have lower levels of education, but are quite similar with respect to marital status.

Table 5: Representativeness

	Samples							
	Pooled lottery	Matched repr.	PLS	Matched repr.	Kombi	Matched repr.	Triss lotteries	Matched repr.
Criminal record (%)								
Any crime	3.87	4.38	2.33	4.17	2.41	3.47	4.96	4.59
Property crime	0.81	1.35	0.57	1.39	0.39	0.88	1.21	1.40
Violent crime	0.55	0.80	0.19	0.65	0.30	0.53	0.91	0.90
Drug crime	0.26	0.41	0.02	0.17	0.05	0.24	0.46	0.52
White collar crime	0.15	0.17	0.07	0.14	0.10	0.18	0.21	0.17
Traffic crime	2.23	2.20	1.13	1.91	1.49	1.95	2.80	2.34
Other crime	0.85	1.09	0.59	1.16	0.43	0.62	1.12	1.13
Fine	3.20	3.62	2.07	3.51	2.00	2.89	4.13	3.78
Probation	0.79	1.02	0.17	0.79	0.49	0.77	1.10	1.13
Jail	0.46	0.62	0.12	0.57	0.27	0.48	0.67	0.65
Baseline characteristics								
Birth year	1950	1950	1940	1940	1945	1945	1954	1954
Female (%)	48.7	48.72	51.4	51.4	40.4	40.4	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	18.4	25.3	19.4	28.0
Married (%)	54.1	53.8	60.7	59.6	57.0	60.0	50.9	50.5
Log household disp. income	12.3	12.3	12.3	12.2	12.4	12.5	12.3	12.3

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 the Triss lotteries. 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 10: 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

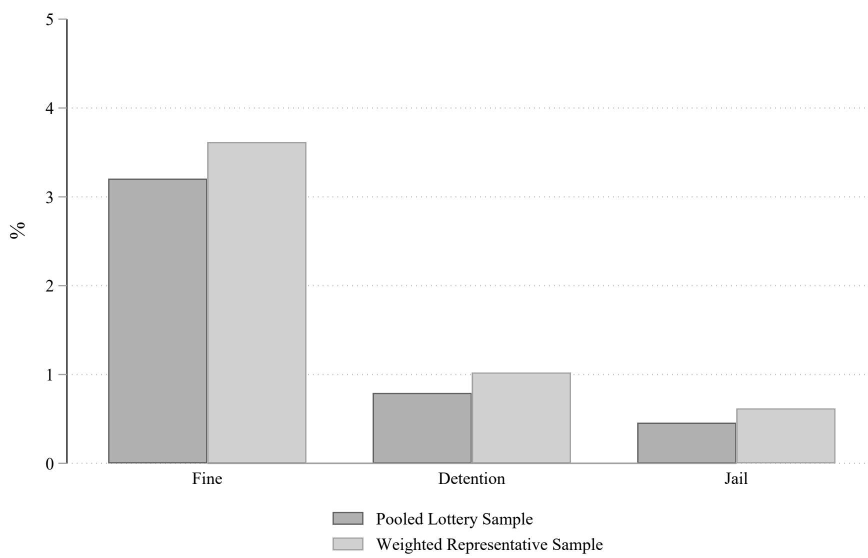
We begin by presenting the estimating equations, before turning to the evaluation of standard errors and statistical power.

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:

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

Figure 11: 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.

where $y_{i,t}$ is a measure of criminal activity within t years of winning the lottery. We set $y_{i,t}$ to missing for individuals who died or were registered as having migrated out of Sweden sometime before year t. $L_{i,0}$ is the prize (in million SEK, about $150,000) awarded to lottery player i at $t = 0$. The vectors $\mathbf{Z}_{i,-1}$, $\mathbf{R}_{i,-1}$ and $\mathbf{X}_i$ are identifical to model 1. $\mathbf{Z}_{i,-1}$ and $\mathbf{R}_{i,-1}$are included solely to improve statistical precision.

For our child sample analyses, the main estimating equation is

$$y_{ij,s} = \beta_c L_{i,0} + \mathbf{Z}_{p,-1}\gamma_p + \mathbf{R}_{p,-1}\phi_p + \mathbf{C}_{i,-1}\theta + \mathbf{X}_i\beta + \epsilon_i \tag{4}$$

where $y_{ij,s}$ is a measure of criminal activity of child j of player i. We follow each child for a maximum of s years after what happens latest of the lottery win and the child turning 15 (the age of criminal responsibility). For example, a child born in 1995 will be followed from 2010 if her parents played the lottery in 2009 or earlier, and otherwise from the year after the win. Because data on criminal behavior is not available after 2017, whether the restriction s is binding depends on child year of birth and when the parents played the lottery. As for the adult analyses, $L_{i,0}$ is the prize amount in million SEK. The vectors $\mathbf{Z}_{p,-1}$, $\mathbf{R}_{p,-1}$, $\mathbf{C}_{i,-1}$ and $\mathbf{X}_i$ are the same as in model 2.

In both models 3 and 4, we let the propensity to commit a crime be a linear function of the lottery win. While most theoretical 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 with this same 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 use permutation-based analyses to evaluate the performance of different types of analytical standard errors.

We start out with our full sample of adult lottery players between 18 and 74. We proceed by independently perturbing the prize vector within each lottery-cell 10,000 times. For each perturbation, we estimate regression 3 with either a binary indicator for any crime or the log of the number of crimes plus one within the first five post-lottery years as the dependent variable, and save the estimation results. We calculate four types of standard errors in each estimation: 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).[8] For each standard error, we calculate the standard two-sided p-value based on the t-statistic. Table 6 shows the share of p-values below 0.05 by type of dependent variable and standard error, including taking the largest standard error in each perturbation ("Maximum").

A first finding from Table 6 is that standard errors unadjusted for heteroskedasticity lead to overrejection. In the binary case, we reject the null hypothesis of zero effects in 8.9% of cases. Using the log number of crimes increases the rejection to 20.3%, more than four times too high. The reason for the overrejection is that the unadjusted standard errors are biased downward: the mean standard error in the binary case is 0.00178 to be compared to a standard deviation of $\hat{\beta}_w$of 0.00204. The three types of heteroskedasticity-adjusted standard errors significantly bring down the rejection rate, to around 6% for the binary case and 8% for when crime is measured in logs. Taking the maximum standard error from each perturbation gives a rejection rate of 4.9% in the binary case, implying a very slight underrejection. However, as shown in 6, adjusting for heteroskedasticity implies rejection is much more likely when the coefficient is negative. The difference is most dramatic in the log case, but the difference in the binary case is never smaller than a factor of four.

The over- and asymmetric rejection rates documented for the heteroskedasticity-

[8]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.

Table 6: Rejection Rate Depending on Type of Standard Errors and Coeffient Sign

	Binary indicator for any crime			Log number of crimes		
Type of standard error	$\hat{\beta}_w < 0$	$\hat{\beta}_w > 0$	$\hat{\beta}_w \neq 0$	$\hat{\beta}_w < 0$	$\hat{\beta}_w > 0$	$\hat{\beta}_w \neq 0$
Unadjusted	0.0399	0.0493	0.0892	0.0942	0.1094	0.2036
Robust	0.0497	0.0119	0.0616	0.0730	0.0075	0.0805
Clustered	0.0496	0.0119	0.0615	0.0729	0.0075	0.0804
EDF	0.0485	0.0106	0.0591	0.0697	0.0069	0.0766
Maximum	0.0386	0.0106	0.0492	0.0666	0.0069	0.0735

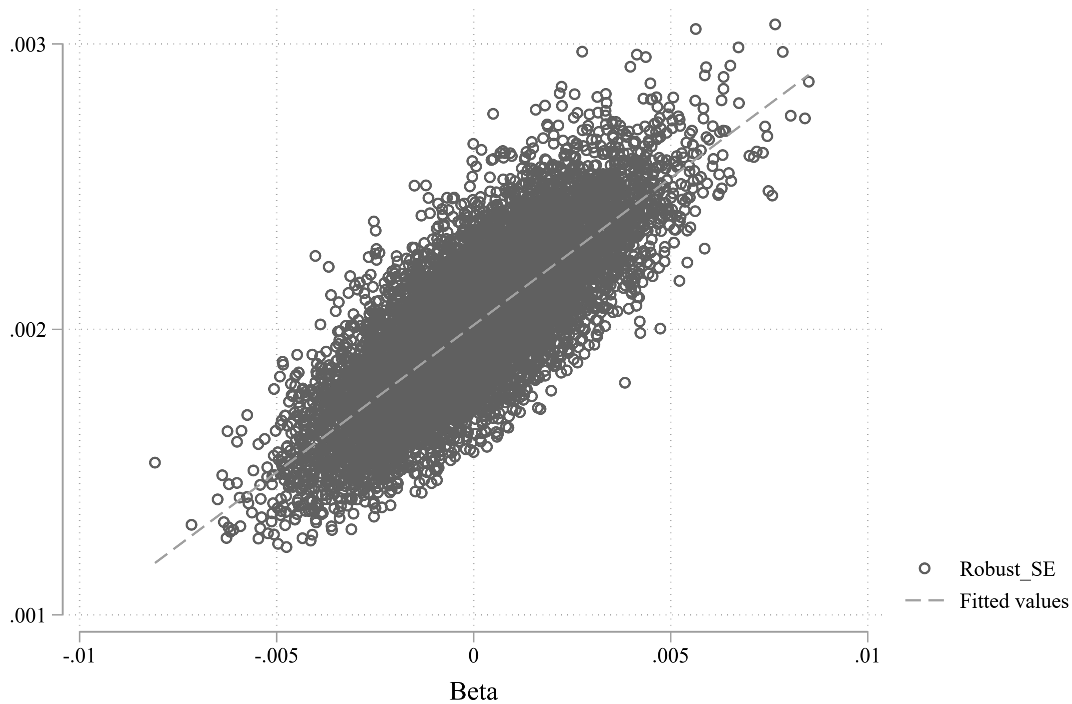
This table reports the share of p-values for the lottery-prize coefficient in regression 3 (two upper panels) which are below 0.05 for four different types of standard errors (conventional, heteroskedasticity-robust, clustered at the level of the player, EDF-corrected robust) and the largest of these four based on 10,000 perturbations of the lottery prize vector. The sample is restricted to individuals between age 18 and 74 at the time of winning and the dependent variables are measured 5 years after winning.

adjusted standard errors are not mainly due to the standard errors being biased. In fact, the mean value of the robust, clustered and EDF-standard errors from the permutations in are 0.00202, 0.00202 and 0.00204 in the binary case, almost identical to the standard deviation of the estimated lottery-effect 0.00204. However, while the variance of the unadjusted standard errors is close to 0, the heteroskedasticity-adjusted standard errors vary substantially between permutations. Moreover, as shown in Figure 12 for the binary case, the standard errors are strongly positively correlated with the estimated lottery-coefficients. The reason behind this pattern is the leverage of large-prize winners: Because the baseline crime rate is low, the estimated error term variance will be large in permutations where a relatively high fraction of people who commited a crime are assigned a large prize. As illustrated in Figure 13, the positive correlation between coefficients and standard errors creates a t-statistics distribution with a negative skew, implying the asymmetric rejection rate shown in Table 6. This is despite the distribution of estimated coefficients having a positive skew.

Finally, the analysis in this subsection has showed inference problems are greater when using the log of crimes compared to a binary indicator for any crime. The likely reason for the worse performance in the log case is that the increased skewness of the dependent variable (compared to the binary case) increases the variance of the heteroskedasticity-robust standard errors, which in turn implies the t-statistics distribution is more skewed than in the binary case. This is what explains the greater asymmetry in the rejection rate. In unshown analyses, we confirm that alternative ways of defining the dependent variable that takes the "intensive" margin into account also exacerbate inference problems.

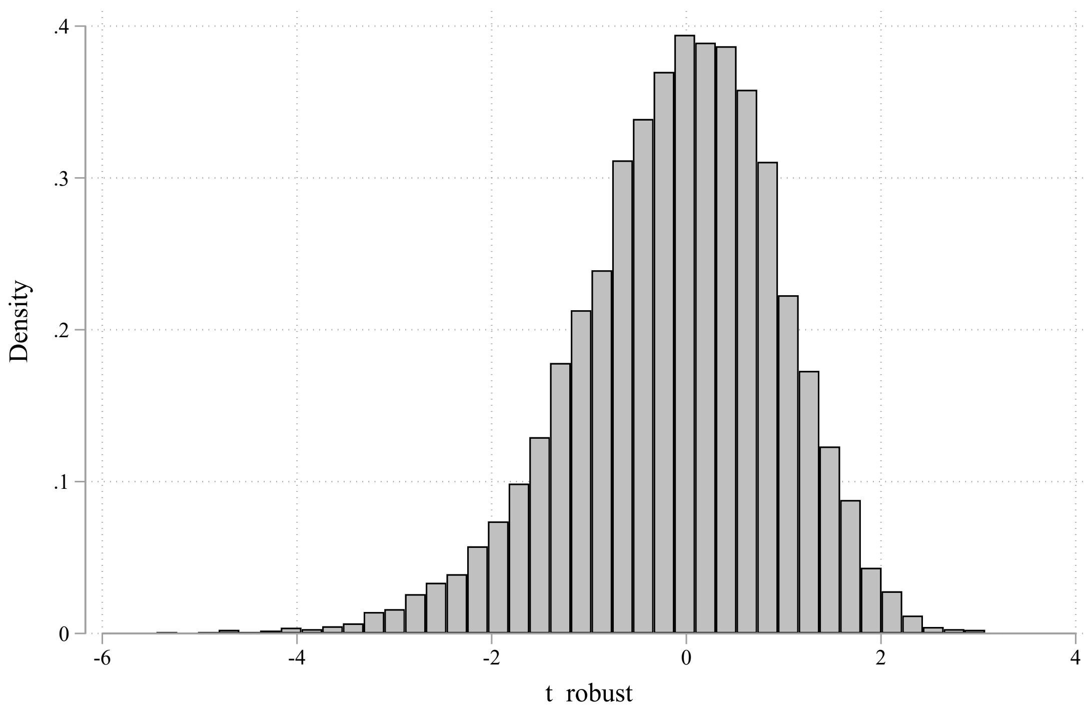
We now turn to the performance of the analytical standard errors in the child sample, focusing on a specification of model 4 with a binary indicator for any type of crime and s equal to 10. Apart from clustering standard errors at the level of the family instead of the player (using an iterative process that assigns half-siblings to the same cluster), we use the same procedure for calculating and standard errors in the child analyses. Our results, which are available upon request, show the asymmetric rejection rate is present (though less pronounced) also for the child sample. The likely reason the problem is less severe for the child sample despite the smaller sample size is that the dependent

Figure 12: Heteroskedasticity-robust Standard Errors and Coefficient Estimates



This figure shows the lottery-prize coefficients and heteroskedasticity-robust standard errors from regression 3 based on 10,000 perturbations of the lottery prize vector. The dependent variable is a binary indicator of any type of crime within five years after winning the lottery.

Figure 13: Permutation-based Heteroskedasticity-robust *t*-statistics



This figure shows the t-statistics from the coefficients and heteroskedasticity-robust standard errors shown in Figure 12.

variable is less skewed due to the higher baseline crime rate in the child sample.

5.3 Evaluating Statistical Power

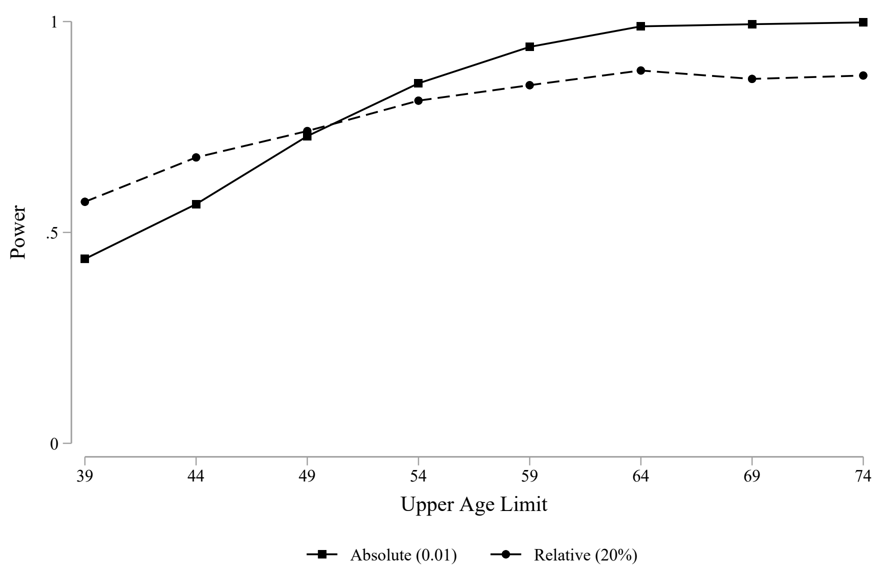
We now turn to an evaluation of statistical power for various samples and specifications. Because continuous measures of criminal behavior exacerbate inference problems, we focus on specifications where the dependent variable is a binary indicator for whether an individual has committed at least one crime within a certain time period after the lottery event.[9] As 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 crime risk due to winning the lottery – when evaluating power. Specifically, we evaluate power to reject the null of no effect under the alternative hypothesis that a SEK 1 million ($150 K) increase in wealth reduces the risk of committing a crime by 20 percent relative to the crime rate in the sample. For reference, we also report power to reject a null of no effect under the alternative hypothesis that a SEK 1 million increase in wealth reduces the propensity to commit a crime by 1 percentage point. We proceed as follows: 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 for each perturbation. We then use the average of these 200 maximum standard errors when calculating statistical power.[10]

We start by considering how statistical power varies with sample restrictions by age and gender, focusing on criminal behavior during the first five years after the lottery win. Figure 14 shows how power changes when we keep the lower age limit at 18 and increase the upper age limit from 39 to 74 in five-year increments. While the power to reject an absolute effect of 1 percentage point increases monotonically (from 43.7% to 99.8%), the power to reject a relative effect of 20 percent peaks at an upper age limit of 64 (88.4%).

[9] In unshown analyses, we have evaluated how power changes if we consider different functions of the number of crimes. Despite the inference problems described above, the estimated statistical power is smaller in these cases.

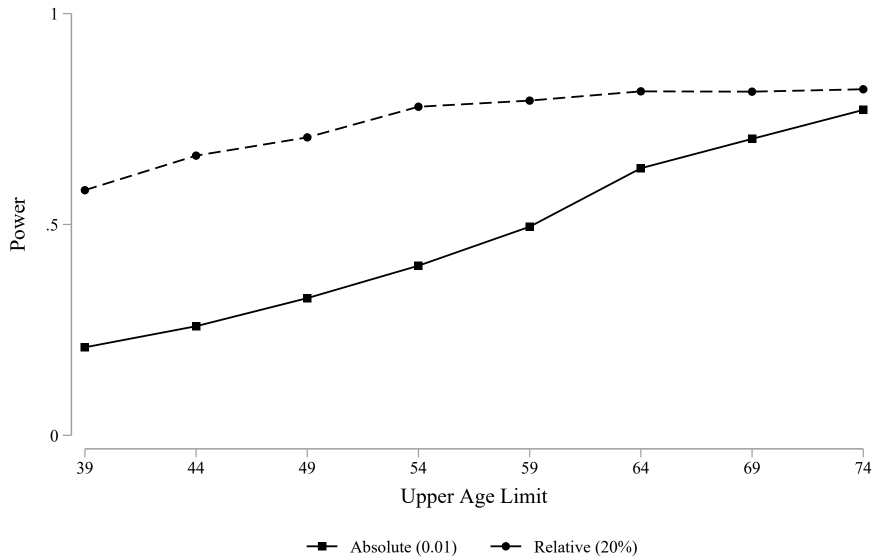
[10] An alternative to using the average of the standard errors is to use the standard deviation of estimated coefficients (see the discussion in Section 6.1). However, because the latter require a much larger number of perturbations, we use the former for our power analyses to save computation time.

Figure 14: 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 15: Power: Age Restrictions (Men Only)



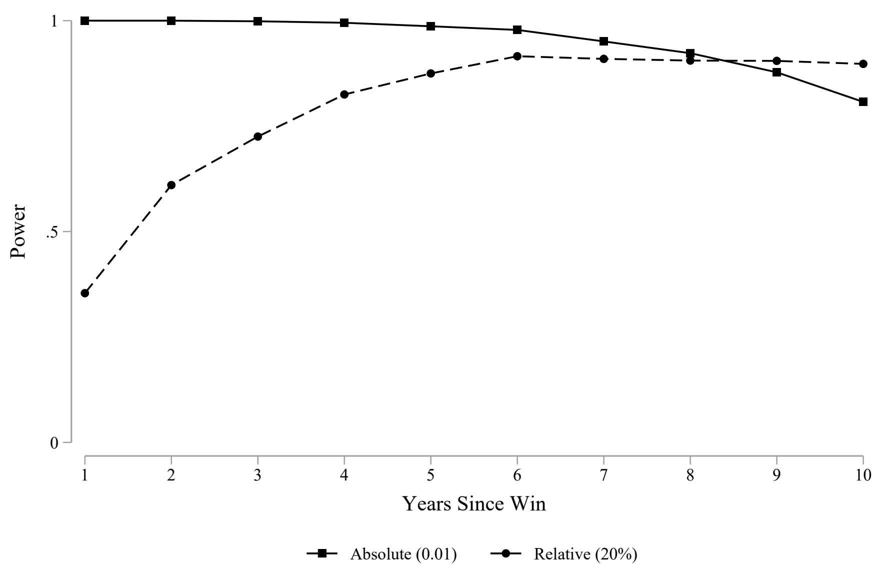
Same as Figure 14, except the sample is restricted to men.

Figure 15 mirrors the analyses in Figure 14, but with the sample restricted to men. Regardless of the upper age limit, statistical power is always lower when the sample is restricted to men. Because power was greatest for our full full sample of men and women aged 18-64, we henceforth use this sample in our evaluations of statistical power.

The analyses above are based on criminal behavior during the five years after a lottery event. But what about shorter or longer time horizons? Figure 16 shows how power is largest when we consider a six-year time horizon (91.6%). For shorter time horizons, the baseline crime rate is low, implying low power to detect relative changes in crime risk. For longer time horizons, the later cohorts of winners are excluded from the data.

Finally, we evaluate the statistical power of our initial subcategories of crime, considering the six-year time-horizon shown to maximize power for any type of crime. Table 7 shows statistical power is greatest for traffic crimes, by far the most common type of crime. Power is lower for less common types of crime, in particular for drug crimes and

Figure 16: 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 14.

Table 7: Statistical Power for Initial Crime Categories

Statistical power (%)

	Adult sample	Child sample
Property	31.7	54.2
Violent	28.2	39.8
Drug	27.1	42.2
White collar	16.8	5.6
Traffic	66.0	42.4
Other	26.2	43.5
Fine	84.9	82.1
Probation	40.0	40.0
Jail	21.4	12.8

This table shows the statistical power to reject a 20% relative decrease in crime risk from one million SEK in lottery wealth. Crime is measured by a binary indicator within six years after the lottery draw for the adult sample; and as a binary indicator within 10 years after age 15 or the lottery draw (depending on what happens latest) for the child sample.

white-collar crimes. Similarly, power is much lower for jail sentences compared to fines and probation.

Our evaluation of statistical power for the child analyses follow a somewhat different procedure compared to the analyses for the adult sample. Unlike the adult analyses, we do not consider different restricitions on the basic estimation sample. Our first step is instead to evaluate the effect of extending the maximum number of years we follow each child, s. In analyses available upon request, we vary s between 1 and 10 and find power is greatest for s equal to 10 (93.7%). Despite boys' higher crime rate, power is greater for the full sample of sons and daughters. Table 7 shows the pattern for the different categories is similar to the adult sample: power is by far lowest for white-collar crime, and also low for serving jail time.

6 Analyses

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

6.1 Statistical Inference

Because of the problems with analytical standard errors shown above, we rely on permutation-based p-values for statistical inference. As in Table 6 and similar to Young (2018), we will simulate the distribution of the relevant test statistic under the null hypothesis of zero treatment effects by perturbing the prize vector 10,000 times and running the relevant analyses for each perturbation. The p-value is then the percentile of the true test statistic in the distribution of simulated test statistics under the null of zero effect.

The exact test statistic depends on the context. When testing whether we can reject a zero effect of lottery prizes in equation 3 and 4, we compare the estimated coefficients of the true effect (i.e. $\hat{\beta}_w$ and $\hat{\beta}_c$) to their respective simulated distributions under the null. This is similar to what Young (2018) denotes "randomization-c", with one exception: To alleviate concerns about an asymmetric rejection rate, we will calculate the one-sided p-value and multiply by two. As pointed out by Fisher (1935), our procedure implies p-values could in theory be above one.

For tests of joint significance (e.g., the exogeneity tests in equation 1 and 2) we will compare the actual F-statistic with the distribution of simulated F-statistics under the null of no effect (a procedure similar to what Young (2018) refers to as "randomization-t").

The permutation-based p-values only allow us to test a null of zero effect. For reference, we will also report the standard deviation of simulated coefficients under the null of zero effects and the maximum of the four analytical standard errors considered in Table 6 from the actual estimation. It is not obvious a priori which of these two standard errors is most accurate: The validity of the standard deviation of the simulated coefficients hinges on the tenuous assumption that the null of zero effects is true. The standard errors from the actual estimation do not require the null oto be true, but as Figure 12 shows they display vary considerable variation across different permutations of the prize vector.

Our statistical inference is not only concerned about getting the p-values in single hypothesis-tests right. To adjust for multiple hypothesis-testing, we will report family-wise error rate adjusted p-values, we apply the free step-down resampling method of Westfall & Young (1993). We refer to the resulting p-values as FWER-adjusted p-values.

6.2 Primary Outcomes and Final Estimation Samples

Table 8 (adults) and 9 (children) show the main analyses we are going to report in the paper. For the adult analyses, we choose the sample and specification of the dependent variable that maximize power. As shown in Section 5.3, this implies we restrict the sample to men and women who were between age 18 and 64 at time of the lottery draw, and that our main outcome variable of interest is an indicator variable equal to one if an individual is convicted at least once in the six years after winning the lottery. The child sample is the same as specified in Section 4.4 and the dependent variable is defined in Section 5.1 with s equal to 10. The answers to the key questions we ask in the paper – whether wealth affects criminal behavior of adults and their children – will depend on the estimation of these two models.

As shown in Section 5.3, statistical power goes down when we consider different categories of crime. To somewhat reduce concerns of low power, we merge property crime and white-collar crime into a common category we denote "economic gain". We also discard jail as an outcome. There is nevertheless a risk that estimating the effect of lottery wealth on various types of crime will result in a chance finding. To mitigate this risk, FWER-adjusted p-values will be reported separately by type of crime and type of sentence.

Table 8: Main Adult Analyses

		Type of Crime					Type of Sentence	
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect (M SEK)								
p (resampling)								
SE (analytical)		To be estimated after publication of the plan						
SE (resampling)								
FWER p								
Mean dep. var.								
Effect/mean								
N								

This table reports the effect of winning the lottery on players' subsequent 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 six years after the lottery event. The sample includes lottery winners and controls between age 18 and 64 at the time of the win. In all specifications, we control for baseline characteristics measured the year before the lottery. The resampling-based p-values are constructed by performing 10,000 perturbations of the prize vector. The resampling-based standard errors equal the standard deviation of the estimated coefficients from the same perturbations. The analytical standard errors are equal to the maximum of conventional standard errors; Huber-White standard errors; standard errors adjusted for clustering at the level of the player and the EDF-corrected robust standard errors suggested by Young (2016). FWER p-values are calculated separately for the analyses in columns (2)-(6) and (7)-(8).

Table 9: Main Child Analyses

		Type of Crime					Type of Sentence	
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect (M SEK)								
p (resampling)								
SE (analytical)								
SE (resampling)		To be estimated after publication of the plan						
FWER p								
Mean dep. var.								
Effect/mean								
N								

This table reports the effect of winning the lottery on the criminal behavior of the players' children. 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 up to ten years after age 15 or the lottery draw (whatever happens latest), or year 2017. Children who were above age 18 at the time of the draw or born later than six months after the draw are excluded from the sample. In all specifications, we control for baseline child and parental characteristics measured the year before the lottery. The resampling-based p-values are constructed by performing 10,000 perturbations of the prize vector. The resampling-based standard errors equal the standard deviation of the estimated coefficients from the same perturbations. The analytical standard errors are equal to the maximum of conventional standard errors; Huber-White standard errors; standard errors adjusted for clustering at the level of the family (including half-siblings) and the EDF-corrected robust standard errors suggested by Young (2016). FWER p-values are calculated separately for the analyses in columns (2)-(5) and (6)-(7).

6.3 Robustness

We pre-specify two robustness checks for the results in Table 8 and 9. First, we will re-estimate these analyses dropping prizes exceeding 4 million SEK ($580K). Second, to account for the possibility that wealth affects the risk of getting convicted conditional on having committed a crime, we will replace the indicator for being convicted for any crime with an indicator of ever being suspected of a crime during the same period.

6.4 Exploratory Analyses

To provide context to the main analyses listed above, we pre-specify two types of exploratory analyses. All the analyses will be available for readers of the paper, though some might only be included in an online appendix. First, we consider the evolution of the effect over time. In the adult analyses, this means changing t in model 3 from 1 to 10. For the child analyses, we will estimate separate regressions for teen crime (age 15-19), crime in young adulthood (20-24) and crime as adults (age 25-29).

Second, we consider a number of heterogeneity analyses. Even though we view these analyses as exploratory, we believe there is a point in reducing the number of dimensions by which we test for heterogeneous effects. In the adult analyses, we will consider the following dimensions:

- Any criminal conviction prior to winning (yes/no)
- Age (up to age 49; age 50 and above)
- Sex (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)

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

- Sex (male/female)

- Age at the time of the parent's win (up to age 9; age 10 and above)
- Parental income (combined parental average income the year before the lottery event; above or below median among parents in the representative sample in the same year)

6.5 Benchmarking the Estimates

A natural way to get a sense of whether our estimates are "small" or "large" is to compare the estimated effect to the income-crime gradient. We here outline the way intend to make this comparison. We proceed in four steps and first consider the adult sample

The first step 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 draw. As in Section 3.3, we set annual household income to SEK 40,000 ($6,000) in case reported disposable income is below this threshold.

In the third step, we use the lottery win as as an instrument for the log of the sum of average household disposable and the annuitized prize, in a specification otherwise the same as models 3 and 4.

In the fourth step, we compare the rescaled lottery-based estimates to income gradients for the matched representative sample calculated in the same fashion as Section 3.3. 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 estimates will be comparable to income-elasticities from previous work.

The procedure for the child sample is similar, with a few differences. First, instead of the player's household income, we consider the log of the sum of the parent's disposable income. Second, we match the representative sample on the children's age and gender rather than that of the lottery player.

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