Does Wealth Inhibit Criminal Behavior? Evidence from Lottery Winners in Sweden and the United States*

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Abstract

We estimate the effect of financial resources on criminal behavior using data from lotteries in Sweden and the United States matched to criminal convictions (Sweden) and incarceration (United States). While institutional environments vary between the two countries, we fail to detect statistically significant effects on criminal behavior in either sample, including for at-risk populations. Moreover, our estimates allow us to rule out effects much smaller than the cross-sectional gradients between income and crime. We also estimate null effects of parental lottery wealth on child delinquency. The results challenge theories that emphasize the lack of economic resources as a key determinant of criminal behavior.

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1 Introduction

A ubiquitous finding in the study of crime is the negative relationship between criminal behavior and economic status (Heller, Jacob & Ludwig 2011). For example, men in the bottom income decile in the United States are 60 times more likely to be sentenced to prison during a five-year period compared to men in the top decile.[1] This steep gradient between income and crime is not only a U.S. phenomenon. In Sweden, a country with low income inequality and an extensive social safety net, the corresponding ratio between men in the top and bottom of the income distribution is 20. Though women commit fewer crimes, the relative difference in crime rates across the income distribution is similar to that of men in both Sweden and the United States. These robust negative relationships raise the question of how much is causally due to income, and if so, what drives this pattern.

Social scientists have proposed a range of explanations for the observed relationship between crime and economic resources.[2] In economics, theory predicts poor labor market conditions increase crime for economic gain (Ehrlich 1973, Sjoquist 1973, Block & Heineke 1975) due to substitution effects, but the effect of changes in unearned income or wealth is ambiguous. For example, crime can be increasing in wealth if individuals exhibit decreasing absolute risk aversion (Allingham & Sandmo 1972, Block & Heineke 1975), but may be decreasing if leisure from criminal activity is a normal good (Grogger 1998) or if the utility loss of imprisonment increases in wealth (Becker 1968). Economists have also highlighted that certain "consumption offenses" (Stigler 1970), such as illicit drug use, may be increasing in wealth. By relaxing financial frictions, wealth may also allow investments that lower the return to crime. However, it is difficult to isolate variation in resources that is not only uncorrelated with the propensity to commit crime but also independent of other mechanisms that affect the incentive to commit crime (i.e. substitution effects). Moreover,

[1] See Figure A6 for prison-income gradients for Sweden and the United States and Figure A5 for general crime-income gradients for Sweden.

[2] A prominent class of theories in sociology emphasize that lack of economic resources may cause "strain" — anger, frustration, and resentment — and induce individuals to resort to crime to obtain what they cannot obtain through legal means (Merton 1938, Cloward & Ohlin 1960, Agnew 1992). A related literature argues that low economic status may lead to selection into geographic areas with less social control, increasing the propensity for criminal behavior (Shaw & McKay 1942, Sampson & Groves 1989).

the relationship between crime and resources may be heavily reliant on the environment, which can vary widely even across developed countries, raising external validity questions.

To overcome these challenges, we match data from Swedish and U.S. lotteries to data on criminal convictions and incarcerations to investigate how positive wealth shocks affect criminal behavior. Matching adults to their children, we also estimate the effect of parental wealth on child delinquency. For Sweden, we have access to administrative data on four different lotteries. Because we observe the factors conditional on which lottery wins were randomly assigned (e.g. the number of lottery tickets), we are able to estimate the causal effect of wealth under weak identifying assumptions. Matching these data to the universe of criminal convictions, we can study the effect of wealth on different types of crime and severity of punishment.

For the U.S., our data consist of lottery winners from tax records which we match to records on incarceration in state and federal prisons. Because these lottery data do not include information on the probability of winning, we identify the effect of lottery wealth by relying on a differences-in-differences strategy where we leverage variation in prize amount and timing of the win. In short, this estimator calculates the difference in incarceration between winners of large and small amounts, and then subtracts the difference in current incarceration between future winners of large and small amounts. As the U.S. lottery data include many more winners and incarceration is more common in our U.S. sample, the U.S. data allow for a more precise estimation of the effect of lottery wealth on the probability of serving a prison sentence.

Despite the significant institutional differences between Sweden and the United States, we detect no effects of lottery wealth on criminal convictions in either country. The point estimate for our main outcome of interest in the Swedish adult sample—conviction for any type of crime within seven years of the lottery event—implies a $100,000 win net of taxes increases conviction risk by a statistically insignificant 0.19 percentage point (on a sample mean of 3.77%). The lower limit of the 95% confidence interval allows us to reject reductions in conviction risk larger than 0.11 percentage point. We find no evidence of differential effects across different types of crimes or sentences of different severity, including incarceration. We further estimate a null effect for the probability of being suspected of a crime, suggesting the null result for convictions is not due to lottery winners having better legal representation. In our sample of U.S. lottery players, we similarly estimate a positive but statistically insignificant effect of lottery wealth on incarceration and are able to rule out all but modest reductions in the risk of imprisonment. For both Sweden and the U.S., we find no evidence that the effect of lottery wealth varies with players' pre-win income, sex, age or predicted criminal behavior. Both sets of estimates are reasonably precise and, rescaling lottery prizes to corresponding income flows, we easily reject effects as large as the crime-income gradient at all conventional levels of statistical significance in both countries.

We continue to obtain positive but statistically insignificant estimates in our intergenerational analyses. The estimate for our main outcome in the Swedish sample—conviction for any type of crime by age 25—suggests children's conviction risk increases 0.06 percentage point per $100,000 in parental lottery wealth and allows us to reject reductions in conviction risk larger than 0.92 percentage point (on a sample mean of 10.78%). The corresponding estimates for the U.S. sample is an increase in incarceration risk of 0.02 percentage point and a lower bound of −0.06 percentage point (on a sample mean of 1.27%). As for the adult sample, we find no evidence of treatment effect heterogeneity with respect to type of crime and sentencing (Sweden), nor parental or child demographic characteristics (both countries). We reject the parental income-crime gradient for our U.S. sample, but not for the Swedish sample where precision is lower. Overall, the results in our paper challenge theories that emphasize lack of economic resources—in childhood or as an adult—as a key determinant of criminal behavior.

Our paper contributes to a literature on how shocks to wealth or income affect crime. In particular, we use sound identification to look at a number of outcomes for both adults and children from diverse populations from two different countries with generally sufficient precision even within key subgroups. In the prior literature, a seminal study by Mallar & Thornton (1978) found income support to ex-convicts reduced recidivism, but a follow-up study on a similar, larger program did not detect any effects (Rossi, Berk & Lenihan 1980, Berk, Lenihan & Rossi 1980).[3] Later work exploiting changes in drug convicts' eligibility for welfare and food stamps (SNAP) have either found revoked eligibility to increase crime (Yang 2017, Tuttle 2019), or null results (Luallen, Edgerton & Rabideau 2018, Mueller-Smith et al. 2023). Studies on other vulnerable populations have found cash transfers reduce crime (Deshpande & Mueller-Smith 2022, Dustmann, Landersø & Andersen 2024), mixed results (Palmer, Phillips & Sullivan 2019) or null results (Carr & Koppa 2020).[4] Most of these studies estimate policies that include both income and substitution effects. One of the distinguishing features of our study is that we are estimating the effect of pure income shocks that do not change implicit prices.

Back-of-the-envelope calculations suggest the crime-income elasticities implied by our estimates are statistically significantly smaller than elasticities based on previous studies that reject null effects, if these effects are interpreted as pure income shocks. While a number of studies do not reject zero effects, we consider what explains the discrepancies in findings for those that do. One potential explanation is that the lottery players we study are less negatively selected compared to previous studies, which have tended to focus on deprived segments of the population. Our Swedish sample is fairly representative of the overall population while the U.S. sample is negatively selected, though not to the same extent as some of the populations studied in previous literature. Thus, our study is well suited for understanding the causal pathways underlying the crime-income gradients in the population. However, it is also the case that when we focus on those with very low incomes or very high predicted crime risk, we continue to recover null estimates for both

[3]The TARP experiment considered four treatment groups with different combinations of financial aid, implicit tax rates on aid, and job training. The reduced-form evidence showed no detectable effects of these treatments on recidivism. However, based on a structural model, Berk, Rossi and Lenihan argue the TARP payments lead to fewer arrests. We here follow Ludwig & Schnepel (2024) in emphasizing the reduced-form evidence.

[4]A related literature studies crime over the payment cycle for government transfers. Financially motivated crime appears to increase toward the end of the payment cycle, when recipients have poor liquidity (Foley 2011, Chioda, De Mello & Soares 2016, Carr & Packham 2019, Watson, Guettabi & Reimer 2020), whereas drug crime (Riddell & Riddell 2006, Dobkin & Puller 2007, Watson, Guettabi & Reimer 2020) and domestic violence (Hsu 2017) are higher at the time of payout. While related, these studies do not address the question of how a shock to permanent income impacts criminal behavior.

Sweden and the U.S. that in most cases allow us to rule out substantial negative crime-income elasticities. The differences in results are thus more consistent with treatment beyond pure income shocks in previous studies driving the effects. For example, income support have been conditional on abstaining from criminal activity (Mallar & Thornton 1978, Tuttle 2019, Deshpande & Mueller-Smith 2022) and related to incentives to take part in integration programs (Dustmann, Landersø & Andersen 2024). Our results suggest that these incentives can be important.[5]

The evidence from the small set of quasi-experimental studies on how parental financial resources affect children's crime is also mixed. Akee et al. (2010) find installments from casino profits decrease minor juvenile crime and Dustmann, Landersø & Andersen (2024) find cutting aid to Danish refugees increased the number of convictions also for children. In contrast, Jacob, Kapustin & Ludwig (2015) find the children of housing voucher lottery winners in Chicago did not have significantly different criminal records compared to the children of non-winners. Studies using within-family variation in parental income have also obtained null findings (Sariaslan et al. 2014, Sariaslan et al. 2021). The point estimates in Dustmann, Landersø & Andersen (2024) and especially Akee et al. (2010) imply, again if interpreted as a pure income shock, elasticities outside the confidence interval we estimate for Sweden and the United States. Yet because of larger standard errors, their confidence intervals include an elasticity above (Dustmann et al. 2024) or just below (Akee et al. 2010) zero. Indeed, weighting by the inverse of each estimate's variance, a meta-analysis combining these two estimates with ours imply a small parental income-elasticity of -0.04. Moreover, as with the adult literature that finds significant effects, more than just the effects of resources are loaded into the treatment variation from these two papers (e.g., effects of better parental employment opportunities could operate in the case of Akee et al. 2010).

Before estimating the effect of lottery winnings on crime in our Swedish sample, we

[5]Two other potential explanations are differences in identification assumptions, which we do not speak to, and the possibility that lottery wealth is spend differently than other typical income shocks, for which there is little indication from the prior literature as discussed later.

specified the statistical analyses in a pre-analysis plan (henceforth, the Plan), uploaded on June 16th, 2021 and available at https://osf.io/9wvdg/. The aim of the Plan was to limit the number of researcher degrees of freedom (Simmons, Nelson & Simonsohn 2011) and commit to analyses with high statistical power and sound statistical inference. When we began work on the Plan, we already had access to the Swedish lottery data and a second data set with information about demographic characteristics and criminal convictions. However, we did *not* merge these two original data sets until the Plan had been publicly archived. The U.S. data and analyses were incorporated into our paper at a later stage and were therefore not pre-specified in the Plan, but we constrained the analysis to follow the Swedish analysis as closely as possible.

2 Crime Data

2.1 Sweden

We use the register of conviction decisions maintained and provided by the Swedish National Council for Crime Prevention to measure criminal behavior. The unit of observation in this data set is a conviction, corresponding to either a court sentencing (49.5% of all convictions), a prosecutor-imposed fine (35.7%), or a waiver of prosecution (14.8%).

Prosecutor-imposed fines are common for minor offenses and are issued when the offender accepts a fine suggested by the prosecutor. In exchange, the offender is not required to go to trial. A waiver of prosecution is issued when a prosecutor declines to press charges, despite overwhelming evidence that the accused committed the crime in question. Prosecution waivers are common for juvenile offenders. They are also sometimes used for adult offenders who are being charged with multiple crimes, some of which are much more serious than others. In such cases, the prosecutor may opt to issue a waiver for the less serious crimes, on the grounds that they are unlikely to impact the final prison sentence. The register does not include fines for minor offenses issued by police, customs, and other authorities. We consider all convictions listed in the register when constructing our outcome variables.

Our extract from the register spans the years 1975 to 2017 and contains all convictions of individuals aged 15 (the age of criminal responsibility) or older at the time of infraction. Individuals are identified by unique personal identification numbers, allowing us to match convictions with the lottery data and data on background characteristics from Statistics Sweden. In the data, each conviction can comprise multiple crimes, sometimes as many as 25. The Swedish judicial system defines crimes by the principle of instance such that a single crime typically corresponds to violations occurring at the same time and place. In the data, each crime could in turn be recorded as a violation of up to three sections of the law.

We classify crimes into five broad categories: crimes for economic gain, violent crimes, drug crimes, traffic crimes, and other crimes. A given crime can belong to multiple categories (see Section Online Appendix A.2 for further details). For instance, we classify driving under the influence of narcotics as both a traffic crime and a drug crime. In the Plan we also distinguish between two types of sentences: fines and detention, where detention indicates any kind of restriction of freedom (including but not limited to imprisonment). To facilitate the comparison to our U.S. estimates, we also consider imprisonment as a post hoc outcome.

2.2 United States

The incarceration data is collected by the Internal Revenue Service (IRS) for all incarceration spells from state and federal prisons that began (and/or ended) between 2011 and 2022. The records, which are periodically updated, contain the start and end date of each spell as well as the prisoner's Taxpayer Identification Number (TIN), which is typically their Social Security Number. Time spent in jail is not included in this data, and we do not observe the type of crime for which inmates have been convicted.

3 Lottery Samples

3.1 Swedish Lottery Data

We construct our estimation samples by matching four samples of adult lottery players (ages 18 and above) to the crime data described above, as well as population-wide registers on socioeconomic outcomes from Statistics Sweden. Our sample for the intergenerational analyses consists of all children of players who were conceived before and were below the age of 18 at the time of the lottery. We also restrict the sample to children born in 2002 or earlier, since later-born children are too young to reach the age of criminal responsibility of 15 during the period of study.

For each lottery, we construct cells within which the amount won is randomly assigned. 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. The construction of the cells is with minor adjustments (specified in the Plan) identical to Cesarini et al. (2016). Table B1 in the Online Appendix summarizes the cell construction, to be described in detail for each lottery below. In Section B.2 of the Online Appendix we discuss and show statistical tests that support the conditional random assignment of the lottery prizes.

Our original intention was to run the final analyses in exactly the same estimation sample as the one used in the Plan's analyses. Unfortunately, a minor coding oversight — failing to set the seed in one of the files used to process the raw data — prevents us from recreating the original sample exactly. See Online Appendix B.3 for details and evidence that the deviations in the final estimation sample are minimal and completely inconsequential in terms of our findings.

Prize-Linked Savings Accounts

Prize-linked savings accounts (PLS) are bank accounts that randomly award prizes to their owners (Kearney et al. 2011). Our data include two sources of information from the PLS program run by Swedish 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 of 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 allows us to identify PLS winners between 1986 and 2003 who held an account during the fiche period.

Draws in the PLS lottery were typically held monthly. Account holders were given one lottery ticket per 100 SEK in account balance. Each draw offered two types of prizes: fixed prizes and odds prizes. Fixed prizes varied in magnitude between 1,000 SEK ($149) and 2 MSEK ($298K) whereas odds prizes paid a multiple of 1, 10, or 100 times the account balance (capped at 1 million SEK during most of the sample period). We rely on somewhat different approaches to construct PLS cells depending on the type of prize won. 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 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.

For odds-prize winners, the amount won depends on the account balance in the month of win and it is therefore insufficient to compare to players who won the same number of odds prizes in the same draw. We therefore construct the odds-prize cells by matching each player who won exactly one odds prize to other players who won exactly one prize (odds or fixed) in the same draw and whose account balance was similar. Fixed-prize winners who are matched to an odds-prize winner this way are assigned to the new odds-prize cell and removed from any original fixed-prize cell they had originally been assigned to. 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 only consider odds-prize cells for which the total amount won is at least 100,000 SEK ($14,880).

The cell construction for the intergenerational sample is identical, except that the unit of observation is a child of a lottery-winning parent.

The Kombi Lottery

Kombilotteriet ("Kombi") is a subscription lottery run by a company owned by the Swedish Social Democratic Party. Kombi subscribers receive their desired number of tickets via mail once 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. We construct the Kombi cells by matching each large-prize winner with (up to) 100 non-winning players of the same age and sex as the winner and whose ticket balances in the month of win were identical to the winner's.

For the intergenerational 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.

The Triss Lotteries

Triss is a scratch-card lottery offered 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 prizes, here denoted Triss-Lumpsum and Triss-Monthly. Winners of either type of prize are invited to a 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 SEK ($7,440) to 5M SEK ($744K). Triss-Monthly winners participate in the same TV show, but draw two tickets. The first determines the size of a monthly installment (10,000–50,000 SEK; $1488-7,440) and the second its duration (10–50 years). The two tickets are drawn independently.

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).

Although the chance of winning a Triss-prize depends on the number of tickets bought, the amount won does not. We assign players to the same cell if they won exactly one prize of a given type in the same year and under the same prize plan. We exclude from the sample a few cases in which a player won more than one prize within the same year and prize plan. The construction of the cells for the intergenerational analyses is analogous to the adult cells.

3.2 U.S. Lottery Data

Lottery wins of at least $600 are reported by states to the IRS on Form W-2G, an information return covering the years 2000 to 2022. As in the Swedish case, we exploit variation in win size, but we cannot observe ticket purchases in the US data. As a result, win size variation reflects not only random variation induced from lotteries but other factors like the type of lottery played (since lotteries differ in payout size). To address this, we derive a comparison group of future winners to form a second difference (similar to e.g. Bulman et al 2021). To be able to examine incarceration through 7 years after the lottery win and derive a comparison group, we focus on individuals who won the lottery between 2011-2014. We then use same aged individuals from each of those years who won the lottery 8 years in the future as a control group (i.e. winners between 2019 and 2022), adding birth cohort by year of win (or placebo year of win for the control) fixed effects. We merge these current and future lottery winners to their incarceration records using their TINs. We construct a similar comparison group for the intergenerational sample, which we describe in detail in Section 4.2.

3.3 Estimation Samples

To construct the Swedish estimation sample for adult players, we started with all winners and control individuals who were at least 18 and no older than 74 years of age in the year of the lottery draw. We then excluded observations who (i) had not been assigned to a cell, or had been assigned to a cell without any variation in the magnitude of the size of the prize won; (ii) lacked information about basic socio-economic characteristics measured in government registers or (iii) shared prizes in the Triss lottery. Imposing these restrictions leaves an estimation sample of 354,034 observations (280,783 individuals).

As with the adult sample, we exclude children not matched to a cell, or matched to a cell without prize variation and children whose parents shared a prize in the Triss lottery. We also restrict the sample to children whose parents were both alive the year before the lottery draw and for whom none of our basic socio-economic characteristics are missing in the registers. Imposing these restrictions, our intergenerational sample consists of 120,159 observations corresponding to 100,953 unique children of 60,074 lottery-playing parents (29,189 mothers and 30,885 fathers) who won a total of 69,264 prizes.

To construct the U.S. sample for adults, we focused on lottery winners (and future winners) who similarly were at least 18 and no older than 74 years of age in the year of the lottery win (or placebo year of the win, i.e. 8 years prior to their win). This leaves an estimation sample of 2,043,527 observations (of which 1,168,465 are current winners). For the U.S. intergenerational sample, we have 5,145,045 children of 2,565,655 lottery-winning parents (1,211,596 mothers and 1,354,059 fathers). Of these, 3,400,478 are the children of 1,953,665 current winners.[6]

Table 1 shows the distribution of prizes in the adult and intergenerational samples for the Swedish and U.S. data (with only current winners included in the U.S. data). All lottery prizes are net of taxes and expressed in units of year-2010 USD and comparisons to dollar amounts reflect the exchange rate by year-end 2010. Panel A shows the total prize amount in our Swedish adult sample is a little over $900 million. PLS and Triss-Monthly have the largest prize pools with over $300 million per lottery, yet Triss-Lumpsum is the lottery which provides most of the within-cell variation in amount won (36%). Panel B shows the total prize pool in our intergenerational sample is slightly over $200 million. In the U.S. lottery data, the total prize pool is roughly $8.7 billion for the adult sample and $13.8 billion for the intergenerational sample. In our main U.S. estimation samples—where

[6]Following Bulman et al. (2021), those in the U.S. sample who win multiple prizes (in the same year or across several years) are excluded from the main sample for all but their first win as are those who take the lottery win as an annuity. We add these groups in a robustness check and the results are very similar.

we cap prizes at \$1 million—the total amounts are \$4.9 billion and \$7.9 billion, respectively.

As another illustration of the variation we use to identify the effect of lottery wealth in the Swedish sample, Figure B1 shows for the adult sample the amount won by the "controls" (the minimum amount won in each cell) and the treatment variation (the amount won above the minimum amount). Figure B1 shows the minimum amount is typically small, except for the Triss-Monthly sample where the lowest prize is still above \$150,000. The treatment variation is fairly uniformly distributed up to \$900,000, implying prizes that are substantial (though not enormous) from a life-cycle perspective help identify our estimates.

3.4 Representativeness

To gauge the representativeness of our estimation samples, we compare the Swedish lottery players' criminal behavior (in the five-year window preceding the lottery event) and socio-economic characteristics (the year before the lottery event) with those of representative population samples drawn in 1990 (PLS lottery) and 2000 (Kombi and the two Triss lotteries) from the Swedish population and U.S. lottery winners' incarceration rates with those of a representative sample drawn from the U.S. population, weighted to match the age and sex distribution of each lottery. We also compare the pooled Swedish lottery sample (with each lottery weighted by its share of the identifying variation) with a representative sample matched on age and sex.

Panel A in Table 2 shows how the Swedish lottery samples compare to the matched representative samples. The Triss sample is similar to the representative sample, whereas the PLS and Kombi samples have lower conviction rates than the Swedish population. Because the two Triss lotteries contribute a large share of the overall identifying variation (see Table 1), however, the weighted and pooled lottery sample is quite similar to the representative sample. Table 2 also shows lottery players are more likely to be born in a Nordic country and have lower levels of education (except for the PLS lottery), but are similar with respect to marital status.

Panel B in Table 2 shows how the U.S. lottery sample compare to the matched repre-

Table 1: Distribution of Prizes Awarded

	A. Winners (adult analyses)						B. Parents (intergenerational analyses)					
	Sweden					U.S.	Sweden					U.S.
				Triss...						Triss...		
	All	PLS	Kombi	Lumpsum	Monthly		All	PLS	Kombi	Lumpsum	Monthly	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
0	37,041	0	37,041	0	0		4,272	0	4,272	0	0	
\$100 to \$1K	286,762	286,762	0	0	0	690,974	58,255	58,255	0	0	0	1,017,431
\$1K to \$10K	22,829	21,686	0	1,143	0	433,044	4,988	4,668	0	320	0	858,654
\$10K to \$50K	4,704	2,231	0	2,473	0	30,742	1,171	496	0	675	0	56,413
\$50K to \$100K	551	261	0	290	0	6,164	140	55	0	85	0	10,244
\$100K to \$250K	1,205	593	375	59	178	4,476	206	114	44	13	35	6,936
\$250K to \$500K	637	244	19	73	301	1,197	161	55	1	20	85	1,725
\$500K to \$1M	285	0	6	83	196	1,303	68	0	1	20	47	1,417
> \$1M	20	0	1	0	19	560	3	0	0	0	3	845
N	354,034	311,777	37,442	4,121	694	1,168,460	69,264	63,643	4,318	1,133	170	1,953,665
Sum (Million \$)	911.8	351.3	72.8	186.7	301.0	8,747.7	204.2	74.3	8.2	49.3	72.4	13,805.4
% of variation.	100.0	26.9	10.8	36.0	26.3	100.0	100.0	26.1	5.6	46.5	21.8	100.0

Notes: 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 dollars. In Triss-Monthly, prize amount is defined as the net present value of the monthly installments won, assuming the annual discount rate is 2%.

sentative sample. The share of lottery players incarcerated is somewhat larger than the share incarcerated in the overall population. Mirroring these differences, individuals in the U.S. lottery sample are less likely to be American citizens, lower educated, less likely to be married, and have lower disposable income. Arguably this sample is primed to find an effect given the lower SES profile of lottery winners. At the same time, there is common support in the distribution of these characteristics between the lottery and representative sample. (Note that Table 2 understates the difference between the U.S. and Sweden in incarceration rates at *any point in time* because it mainly reflects the extensive margin of prison sentences and not the much longer sentences in the U.S.)

In Section A.3 of the Online Appendix, we provide evidence that, while the U.S. incarceration rate is much higher, the rates of assault and property crime in Sweden and the U.S. are similar and in line with those in comparable countries.

Apart from sample representativeness, external validity would be hampered if the effect of lottery wealth is different from the effects of other types of shocks to wealth or permanent income.[7] Previous work on Swedish lottery winners contradict the notion that there is something special about lottery wealth that impairs generalizability. Winners refrain from quickly spending the prize money (Cesarini et al. 2016) and show higher satisfaction with their personal finances even a decade after winning (Lindqvist, Östling & Cesarini 2020). In line with a wealth shock in a standard lifecycle model, winning the lottery leads to a persistent, though modest, reduction in labor supply, which does not seem to depend on whether prizes are paid out as lump-sum or monthly installments over many years (Cesarini et al. 2017). Evidence from U.S. lottery studies indicates similar savings and labor supply responses, though with potentially less persistence for small prizes and for winners of low socioeconomic status (Bulman et al. 2021, Bulman, Goodman & Isen 2022, Golosov et al. 2024).

[7] If there was significant heterogeneity in the effect of resources by the type of shock, a natural question then would be what, if anything, constitutes an "ordinary" shock.

Table 2: Representativeness

	A. Sweden								B. U.S.	
	Pooled lottery	Matched repr.	PLS	Matched repr.	Kombi	Matched repr.	Triss lotteries	Matched repr.	Lottery	Matched repr.
Criminal record (%)										
Any crime	3.88	4.38	2.32	4.17	2.47	3.47	4.96	4.59	-	-
Economic crime	0.94	1.47	0.64	1.50	0.47	1.01	1.37	1.53	-	-
Violent crime	0.56	0.80	0.19	0.65	0.30	0.52	0.91	0.90	-	-
Drug crime	0.27	0.41	0.02	0.17	0.08	0.24	0.46	0.52	-	-
Traffic crime	2.23	2.20	1.13	1.91	1.47	1.94	2.80	2.34	-	-
Other crime	0.85	1.09	0.59	1.16	0.43	0.63	1.12	1.13	-	-
Fine	3.22	3.62	2.06	3.51	2.09	2.90	4.13	3.78	-	-
Detention	0.79	1.02	0.17	0.79	0.46	0.77	1.10	1.13	-	-
Prison	0.46	0.62	0.12	0.57	0.25	0.48	0.67	0.65	1.26	0.83
Socio-economic characteristics										
Birth year	1950	1950	1940	1940	1945	1945	1954	1954	1967.5	1967.5
Female (%)	48.76	48.76	51.37	51.37	40.70	40.70	49.62	49.62	44.66	44.66
Nordic born/US citizen (%)	95.04	91.85	96.79	94.38	98.11	91.92	93.67	90.83	90.39	92.82
College (%)	20.17	25.35	20.77	17.52	18.61	25.37	19.42	28.01	44.22	53.00
Married (%)	54.06	53.80	60.70	59.65	57.03	59.88	50.95	50.51	46.06	58.10
Log household disp. income	10.43	10.39	10.36	10.29	10.57	10.56	10.42	10.40	10.33	10.53

Notes: The table shows descriptive statistics for the Swedish pooled lottery sample and each of the three subsamples that it comprises in panel A and the U.S. sample in panel B. We weigh each of the three Swedish subsamples by its identifying variation in amount won (the variation in prizes demeaned at the cell-level) when constructing the pooled lottery sample. The matched representative samples have the same distribution of age and sex 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 draw. Because U.S. incarceration in the five years preceding 2011-2014 is truncated by the prison data only including spells that overlapped with 2011-present, we present means for the 2016-2019 winner cohorts (using the truncated lottery cohorts reveals a similar gap between the lottery and the representative samples)..

4 Estimation and Inference

4.1 Swedish Lotteries

Our identification strategy exploits the fact that 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:

$$y_{i,t} = \beta_w L_{i,0} + \mathbf{Z}_{i,-1}\gamma_w + \mathbf{R}_{i,-1}\phi_w + \mathbf{X}_i\delta_w + \epsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is an indicator for some type of criminal sentence within t years of winning the lottery. $L_{i,0}$ is the prize 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 sex, log of household disposable income, and indicator variables for marital status, completion of a college degree, and being born in a Nordic country.[8] $\mathbf{R}_{i,-1}$ is a vector of pre-win criminal behavior, including dummy variables for being convicted for each of the categories of crime listed above during the five-year period prior to the lottery event 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. Because the amount won ($L_{i,0}$) is random conditional on the cell fixed effects ($\mathbf{X}_i$), the covariate vectors ($\mathbf{Z}_{i,-1}$ and $\mathbf{R}_{i,-1}$) are only included to increase the precision of our estimates. In our main analyses, we set $t = 7$. This event horizon was chosen based on power calculations reported in the Plan (p. 29-32).

For our intergenerational analyses, the main estimating equation is

$$y_{ij,s} = \beta_c L_{i,0} + \mathbf{Z}_{j,-1}\gamma_c + \mathbf{R}_{j,-1}\phi_c + \mathbf{C}_{j,-1}\theta_c + \mathbf{X}_i\delta_c + \epsilon_{ij,s}, \tag{2}$$

[8] Household disposable income is defined as the sum of own and (if married) spousal disposable income. Own and spousal disposable income are winsorized at the 0.5th and 99.5th percentile for the year in question before summing them. To avoid a disproportionate influence of values close to 0 we winsorize household disposable income at SEK 40,000 (about $6000) before applying the log transformation.

where $y_{ij,s}$ is an indicator for some type of criminal sentence for child j of player i. We follow each child for a maximum of s years after the lottery event if the child is 15 or older at the time of the event. If the child is younger, we follow the child s years after he or she turns 15 (the age of criminal responsibility). As in the adult analyses, $L_{i,0}$ is the prize amount. $\mathbf{Z}_{j,-1}$ is a vector of pre-win socio-economic characteristics of child j's biological parents (both player i and the non-playing parent), including third-order polynomials in the mother's and father's age, the log of average parental disposable income during the five years preceding the lottery draw, and indicator variables for whether each parent was born in a Nordic country, was married and had a college degree. $\mathbf{R}_{j,-1}$ includes the same indicators of pre-win criminal behavior as in model (1), but for child j's mother and father. $\mathbf{C}_{j,-1}$ is a vector of child-specific controls, including a third-order polynomial in age at the time of win interacted with gender and a dummy for being born in a Nordic country. $\mathbf{X}_i$ is the vector of cell fixed effects for the intergenerational sample.

Section 5.3 of the Plan evaluates statistical power for different values of s between 1 and 10. We found power to be maximized for $s = 10$, which is why we focus on this time horizon in the intergenerational analyses.

As discussed in Section 5.2 of the Plan, the skewness of our dependent variable (criminal behavior) and key independent variable (lottery prizes) implies inference based on analytical standard errors might be misleading, despite a large sample. To address this concern, the Plan specifies our use of permutation-based p-values for statistical inference. To calculate these, we simulate the distribution of the relevant test statistic under the null hypothesis of zero treatment effects by perturbing the lottery 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. Our approach is similar to what Young (2019) labels "randomization-c", with one exception: because the sampling distribution of our coefficients is often asymmetric, we calculate a one-sided p-value and multiply it by two.[9] As specified in the Plan, we

[9] More formally, let q be the percentile of the estimated coefficient in the distribution of simulated coefficients under the null of zero effect. The p-value is then $2q$ if the coefficient is negative and $2(1-q)$ if

also report the maximum of four different analytical standard errors: classical standard errors, heteroskedasticity-robust standard errors, standard errors adjusted for clustering at the level of the player (winner sample) or family (intergenerational sample), and the EDF-corrected robust standard errors suggested by Young (2016). To adjust for multiple-hypothesis testing, we report family-wise error rate (FWER) adjusted p-values from the free step-down resampling method of Westfall & Young (1993) for our main results.

4.2 U.S. Lotteries

Unlike the Swedish data, our U.S. lottery data do not include information on the probability of winning. We therefore leverage variation in the amount and timing of the win in a differences-in-differences strategy to estimate the effect of lottery wealth on crime. Specifially, we define *current winners* as people who won between 2011 and 2014 and *future winners* as winners between 2019 and 2022 and use the latter as a control group for the former.

In keeping with the Swedish analyses, we focus on incarceration spells that began at some point between the year of the win and 7 years following the win. Because we use future lottery winners as controls, we set the outcome variable to zero for incarceration spells that were not completed by the end of the 7th year subsequent to the win. Without this restriction, the control group would mechanically contain fewer incarcerated people as it is rare to win the lottery while incarcerated. The restriction removes less than 10% of prison sentences that began in the treatment window. We show later that adding in these prison sentences, likely stemming from convictions for the most severe crimes, has no material effect on the results.

We typically estimate regressions of the following form:

$$prison_{i,s,t} = \beta_1 W_i + \beta_2 L_i + \beta_3 (W_i \times L_i) + \lambda_{as} + \mathbf{X}_i \delta_w + e_i, \tag{3}$$

the coefficient is positive. As pointed out by Fisher (1935), our procedure implies p-values can be above one.

where $prison_{i,t+s}$ is an indicator for whether individual i served (and completed) a prison sentence within t years after year s. For current winners, year s is the year of the win. For future winners, year s is the year of the win for their corresponding current winners. Further, W_i equals 1 if winner i is a current winner (as opposed to a future winner), L_i is the after tax amount of the lottery win scaled by \$100,000 (in 2010 dollars), and the interaction of the two is the treatment effect. λ_{as} is a fixed effect for age at win (or age at placebo win) by year of the win (or placebo win) fixed effects to compare current lottery winners to same aged future lottery winners. We sometimes use a vector of predetermined variables to show robustness of the results and to test for balance. The predetermined variables include incarceration in the prior 5 years, ever incarcerated, logarithm of after tax income, sex, marital status, whether filed tax return, college educated (if under 30 in 1999, i.e. when the college data began), and whether a U.S. citizen. In the US context, lottery wins can reach over a billion dollars, so to account for possible concavity in the response function and to make the results more comparable to the maximum win size region in the Swedish context, we limit lottery wins to 1 million after tax dollars. Inference is based on the maximum of robust and non-robust standard errors.

This identification strategy makes several assumptions. First, it assumes that unobserved differences, if any, in the propensity to be incarcerated for winners of large versus small lottery amounts are similar for current versus future winners. In support of this assumption, Table B2 tests and finds evidence for covariate balance. The strategy also assumes that the effect is approximately linear over the win range we examine and that there is no meaningful treatment heterogeneity for winners of large versus small lotteries.[10] To explore the validity of these assumptions, respectively, we examine the sensitivity of our estimates to varying the maximum win size, and we reweight win amount bins to look the same along a rich set of characteristics. As we show later, the evidence from those exercises is supportive of these assumptions.

For the intergenerational sample, we use the children of parents who won the lottery

[10]While lottery wins occur throughout the sample period, we make no "forbidden" comparison that plague staggered DiD designs, i.e. previous winners are never in the comparison group.

between 2000 and 2022 provided they meet the below criteria. Since we look at incarceration of children between the ages of 15 and 25, we limit the sample to child birth cohorts for which we can theoretically observe prison spells for at least one year in that age range, namely those born between 1986 and 2007, and only include children whose parents won by the time they reached age 18 (if the parent won when the child was between 15 and 18, we exclude from the outcome the years prior to the lottery win). To construct a comparison group, we look at children of the same birth cohorts but whose parents won too late for the lottery win to have influenced the outcome, so long as they won when the child was age 19 or older (if the parent won when the child was between 19 and 25, we only include the years prior to the lottery win when constructing the outcome variable). Our regressions are of the following form:

$$prison_{ij,l,b} = \beta_1 W_{ij} + \beta_2 L_j + \beta_3 (W_{ij} * L_j) + \lambda_{yc} + \mathbf{X}_{ij}\delta_w + e_i, \tag{4}$$

where $prison_{ij,l,b}$is an indicator for whether child individual i of lottery-winning parent j has served a prison sentence between age l and b. W_{ij} is an indicator for whether child i was below 18 at the time parent j won the lottery, L_j is the amount won and λ_{yc} is a fixed effect for year of win and cohort. As for the U.S. adults, we sometimes also include a vector of controls for child and parental characteristics, $\mathbf{X}_{ij}$. Inference is based on the maximum of robust and non-robust standard errors and standard errors clustered at the level of the winning parent.

A potential concern in the U.S. intergenerational analyses is that the age span over which we observe incarceration differs between treated and control children (in the timing dimension but not the win amount dimension). First, the share of children assigned to treatment and control varies across cohorts because, in later cohorts, fewer children had reached 19 or more before the parents won the lottery. Second, within a given cohort, the observation window may differ between treated and controls. For example, for children born in 1996, control children whose parents won in 2012–2014 are observed from age 15 onward, while treated children whose parents won in 2015–2020 are only observed up until age 24. In Section 5.2, we present results from several robustness test which show the estimated effect of parental lottery wealth on child incarceration remains statistically insignificant and similarly precise when we balance the sample.

Aside from the issues related to the dependent variable, the identifying assumptions for the U.S. intergenerational analyses are analogous to the adult sample. However, Table B2 shows we reject the null hypothesis of covariate balance. This imbalance is quantitatively small and driven by the coefficient on a child's parents being married of 0.049 percentage point per $100,000 (SE = 0.018), which is statistically significant at the 0.004 level (with a sample of over 5 million children, we are powered to detect tiny amounts of imbalance). Notably, having parents that are married is *negatively* associated with crime, such that the positive relationship between winnings and marriage should, if anything, bias our estimate toward finding a negative effect. Section 5.2 below discusses a number of robustness tests that in concert strongly suggest little bias.

5 Results

In this section, we analyze the effect of lottery wealth on criminal behavior.

5.1 The Effect of Lottery Wealth on Adult Crime

Table 3 shows the estimated effect of lottery wealth on crime in the adult sample. For our main outcome in the Swedish analyses in Panel A—an indicator for having at least one criminal conviction in the seven years after the lottery event—our point estimate implies a $100,000 windfall increases the conviction rate by 0.187 percentage point (SE = 0.150), corresponding to 5.0% of the sample crime rate. The effect is not statistically distinguishable from zero. The 95% confidence interval allows us to reject that a $100,000 lottery windfall reduces crime risk by more than 0.107 percentage point, or 2.8%.

Columns (2) to (6) of Table 3 show the results for each of the five different crime categories. The effects on crimes for economic gain, violent crime, and other types of crime are positive while the effects on drug crime and traffic crimes are negative, but none of these estimates are statistically significant. Columns (7) to (9) show the results by type of sentence. Though the estimated effects suggest lottery wealth increases the risk of being sentences to a fine but decreases the risk of detention or prison, all estimates are small and statistically insignificant.

Column (10) shows the main results for the U.S. analysis. The point estimate implies a \$100,000 windfall increases the probability of incarceration by 0.043 percentage point (SE = 0.058), or 3.5% of the sample crime rate. This effect is not statistically distinguishable from zero and the 95% confidence interval allows us to rule out reductions in the incarceration rate bigger than 0.071 percentage point, or 6.2% of the sample incarceration rate.

Robustness

Table D1 in the Online Appendix shows the results from two sets of pre-specified robustness tests for the Swedish analyses. First, to account for the possibility that wealth affects the risk of conviction, rather than the incidence of criminal behavior, column (1) reports the results when we consider an indicator for being suspected (instead of convicted) of a crime.[11] Because data on individuals suspected for offenses are only available from 1995, the estimation sample is different from that in Table 2. For reference, column (2) therefore reports the results for convictions using the same sample as in column (1). Though our results suggest lottery wealth reduces the risk of being suspected by 0.261 percentage point per \$100K, the effect is not statistically significant (p-value 0.251) and we cannot reject the null hypothesis that the effect for being suspected and convicted are the same (p-value 0.240).

Second, in columns (3)–(10) of Table D1 we re-estimate the regressions from Table 3 dropping prizes exceeding 4 MSEK (\$595K). We estimate statistically significant positive

[11]There are three reasons for why there is a non-perfect overlap between being suspected and convicted for a crime. First, not all people suspected of a crime are convicted. Second, though coverage improves significantly over time and is almost complete toward the end of our study period, in particular lesser crimes are sometimes not entered into the Suspect Registry. Third, there might be a time gap between being suspected and convicted.

Table 3: Main Analyses for the Adult Sample

	A. Sweden									B. U.S.
		Type of Crime					Type of Sentence			
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention	Prison	Prison
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effect ($100K)*100	0.187	0.028	0.019	-0.044	-0.012	0.121	0.193	-0.019	-0.034	0.043
SE	0.150	0.079	0.076	0.029	0.111	0.075	0.142	0.060	0.041	0.058
p (resampling)	0.243	0.691	0.711	0.397	0.951	0.122	0.206	0.861	0.601	-
p (analytical)	0.211	0.725	0.798	0.124	0.912	0.105	0.175	0.755	0.399	0.455
FWER p		0.911	0.911	0.857	0.951	0.458	0.362	0.861	-	-
Mean dep. var.*100	3.774	0.948	0.637	0.322	2.239	0.625	3.210	0.844	0.381	1.154
Effect/mean	0.050	0.029	0.030	-0.137	-0.006	0.194	0.060	-0.022	-0.090	0.035
Pre-registered	Y	Y	Y	Y	Y	Y	Y	Y	-	-
N	325,796	325,796	325,796	325,796	325,796	325,796	325,796	325,796	325,796	2,043,527

Notes: This table reports the effect of winning the lottery on players' subsequent criminal behavior. Panel A: Each column reports results from a separate regression in which the dependent variable is an indicator variable equal to 1 in case of a conviction for a certain type of crime, or certain type of sentence, within seven years after the lottery draw. The sample includes lottery winners and controls between age 18 and 74 at the time of the win. In all specifications, we control for the factors listed in model 1. 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). The resampling-based p-values are constructed by performing 10,000 perturbations of the prize vector. FWER p-values are calculated separately for the analyses in columns (2)-(6) and (7)-(8). The mean of the dependent variable is calculated by weighting the sample by the treatment variation in each lottery. Panel B: Prison is an indicator variable equal to 1 in case of a spell of incarceration within seven years after the calendar year of the lottery win. Standard errors are equal to the maximum of conventional standard errors and Huber-White standard errors.

effects on any crime (permutation-based p-value 0.046), other types of crime (p-value 0.012) and for being convicted and required to pay a fine (p-value 0.049). The point estimates are generally larger compared to the full sample, suggesting the marginal effect of wealth on criminal behavior is decreasing in wealth, but also less precisely estimated. Still, the results in Table D1 reinforce our conclusion that wealth does not reduce the propensity to commit crime. As an additional test of non-linear effects, Panel A Figure D2 shows a non-parametric analysis where we split the sample into five prize categories. The estimated effects are more positive for winners of larger prizes, but never statistically significant.

We consider three additional post hoc robustness tests for the Swedish analyses. First, we remove from regression (1) the covariate vectors not needed for identification ($\mathbf{Z}_{i,-1}$ and $\mathbf{R}_{i,-1}$). Second, we consider an alternative indicator of criminal behavior equal to one for all types of crimes except traffic crimes (by far the most common type of crime). Finally, given potential concavity in the relationship between resources and crime, we remove lotteries where the "controls" are relatively wealthy, either because the minimum amount won was substantial (Triss-Monthly), or because the amount won is increasing in the number of lottery tickets (PLS odds prizes), which in turn correlates positively with income.[12] As shown in Table D2, the estimated effect of lottery wealth remain close to zero and statistically insignificant in all these tests.

Our robustness tests for the U.S. analyses are reported in Table D3. Panel A shows the results when we vary the floor and ceiling of the lottery prize range. The point estimates are broadly similar across all ranges and are never statistically significant. The lack of clear non-linearities is confirmed by Panel B of Figure D2 where we report results from a regression with five prize categories. Table D3 Panel B shows the results of alternate specifications to our identification approach, none of which gives a statistically significant estimate. Estimating the effect based only on within-prize variation among current winners makes the estimate more precise but slightly negative, consistent with winners of larger prizes being somewhat positive selected. Using triple difference-in-differences or including

[12]As explained in Section 3.1, we condition on the account balance when constructing the PLS odds prize cells, implying the identifying variation is exogenous also in the odds prize cells.

the full set of controls instead makes the estimate more positive. We also report the results from two reweighting exercises where we first reweight the sample to match socio-economic characteristics of the random, representative sample, and then reweight the individuals in each prize bin the same way. We further include winners of multiple prizes (instead of only the first win) and annuities, include sentences that go beyond seven years after the win and run separate regressions for below and above median sentence duration. None of these tests indicate that our results are not robust.

Figure D1 in the Online Appendix shows the estimated effects when we vary the time horizon from 1 to 10 years (Sweden) or 1 to 7 years (United States).[13] While the U.S. estimates are very stable, there is a slight tendency for the Swedish estimates to become more positive over time (though never statistically significant).

Treatment Effect Heterogeneity

We test for heterogeneous effects in our Swedish sample along four pre-specified dimensions: age, sex, disposable income (defined as above or below the median income in a representative sample-groups defined by age, sex and year) and any prior conviction. Panel A in Table 4 shows the effect of lottery wealth in the Swedish sample is larger for men and for players without a prior conviction, but none of these differences are statistically significant. There is no evidence of heterogeneity by age or income.

Table D4 reports the results from three sets of post hoc heterogeneity analyses for the Swedish sample. We first split the sample by future crime risk. To this end, we regress our indicator of a conviction at $t = 7$ on the pre-win covariate vectors $\mathbf{Z}_{i,-1}$ and $\mathbf{R}_{i,-1}$ from regression (1) using the representative sample and then split the lottery sample by the median of this predicted value. The key factors that predict high crime risk are being male, young, born outside of the Nordic countries and having prior convictions. For example, while the sample with below-median crime risk is 94% female and has a 0% share with a prior criminal record, the above-median sample is 82% male and 15.5% have

[13] As discussed above, extending the time horizon beyond 7 years could result in biased estimates for the U.S. sample.

a criminal record. Whereas 0.8% of the below-median sample are convicted for a crime within seven years of the lottery event, 4.6% in the above-median sample are convicted. In our second analysis, we split the sample into below- or above the group-specific median income in the representative sample, where groups are defined by sex and year (instead of sex, year and age as in Table 4). Refraining to define groups by age implies the low-income group disproportionally consists of young lottery players. In the third analysis, we estimate regression (1) for each lottery separately. As shown in Table D4, we fail to reject the null of homogeneous treatment effects across all three dimensions (crime risk, income and lotteries). The only nominally statistically significant effect in Table D4 is that winning the PLS lottery increases the probability of conviction.

Panel B in Table 4 shows heterogeneity analyses for our U.S. sample corresponding to the pre-registered Swedish analyses. Because incarceration is rare and we only have data on pre-win incarceration spells from a few years, we split the sample by predicted crime risk rather than prior incarceration. In none of the subsamples do we estimate an effect statistically distinguishable from zero. If anything, the pattern of results suggest lottery wealth is the opposite of one would expect if wealth has more protective effects in groups more prone to commit crime (the relatively young, men, and people with low income).

As shown above, neither the Swedish nor U.S. data provide evidence of more protective effects of wealth in population strata more prone to commit crime, such as men or people with low incomes. To investigate this point further, Figure 1 shows the results from exploratory analyses where we restrict the sample to households in the bottom 4, 3, 2 and 1 deciles of the income distribution or to households in the top 4, 3, 2 and 1 deciles of the distribution of predicted crime risk. The higher crime risk deciles essentially include men and women with a previous criminal record.[14] The sample restrictions imposed in Figure 1 implies estimation becomes less precise. This is particularly the case in our Swedish sample where identification is based on comparing people who won different amounts in relatively

[14]For example, the top crime risk decile in the Swedish sample is 94% male and has a 62% share with a previous criminal record. 10.1% of the people in this group commit a crime within seven years of the lottery event.

Table 4: Heterogeneous Effects in the Adult Sample

	A. Sweden							
	Age		Sex		Income		Prior Crime	
	< 50	≥ 50	Male	Female	< Median	≥ Median	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect ($100K)*100	0.152	0.169	0.366	0.010	0.183	0.198	0.238	-0.266
SE	0.249	0.178	0.275	0.108	0.273	0.166	0.139	0.552
p	0.584	0.624	0.217	0.994	0.491	0.541	0.106	0.760
p equal effects	0.961		0.265		0.963		0.534	
Mean dep. var.*100	5.333	2.568	6.288	1.237	4.384	3.306	2.132	13.168
Effect/mean	0.029	0.059	0.058	0.296	0.045	0.060	0.111	0.018
N	120,277	205,519	159,136	166,660	133,261	192,535	300,526	25,270

	B. United States							
	Age		Sex		Income		Crime Risk	
	< 50	≥ 50	Male	Female	< Median	≥ Median	< Median	≥ Median
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Effect ($100K)*100	0.094	-0.068	0.112	-0.029	0.070	0.011	0.001	0.035
SE	0.085	0.068	0.078	0.066	0.094	0.056	0.024	0.112
p	0.270	0.319	0.150	0.662	0.455	0.845	0.953	0.753
p equal effects	0.169		0.242		0.608		0.762	
Mean dep. var.*100	1.766	-0.302	1.666	0.520	1.623	0.493	0.105	2.173
Effect/mean	0.053	0.225	0.067	0.056	0.043	0.022	0.009	0.016
N	1,192,774	850,753	1,130,827	912,700	1,194.316	849,211	1,008,976	1,034,551

Notes: This table reports the results from four pre-registered heterogeneity analyses. Columns 1/9 and 2/10 show results separately for winners age 50 and younger at the time of the draw (Sweden) or win (United States). Columns 3/11 and 4/12 show the results separately for male and female winners. Columns 5/13 and 6/14 display results separately for those above or below the median disposable household income in the same age-year-sex cell in the representative sample (where age is defined by five-year intervals). Columns 7 and 8 show the results for winners depending on whether they have any recorded conviction from 1975 up to the year prior to the draw. Columns 15 and 16 show the results depending on whether predicted crime risk (based on the covariates) are below or above the median. Panel A: All regressions include the same set of covariates as in model 1 plus interactions between all covariates (including the cell fixed effects) and an indicator for the relevant dimension of heterogeneity. Standard errors are the maximum of unadjusted, heteroskedasticity-robust and clustered at the level of the player. The p-values for both individual coefficients and for equality between coefficients are based on 10,000 permutations of the prize vector. Panel B: All regressions add interactions for the relevant dimension of heterogeneity. Standard errors are equal to the maximum of conventional standard errors and; Huber-White standard errors.

small cells and where sample restrictions imply some cells are dropped. With this caveat in mind, Figure 1 shows little evidence of more protective effects in at-risk groups. Notably, the only statistically significant estimate is a nominally significant *increase* in criminal behavior for Swedish winners in the bottom income decile. Though the exploratory nature of these analyses implies nominal p-values are biased downward, we can rule out substantial protective effects at the very low end of the income distribution (as well as the high end of the risk distribution).

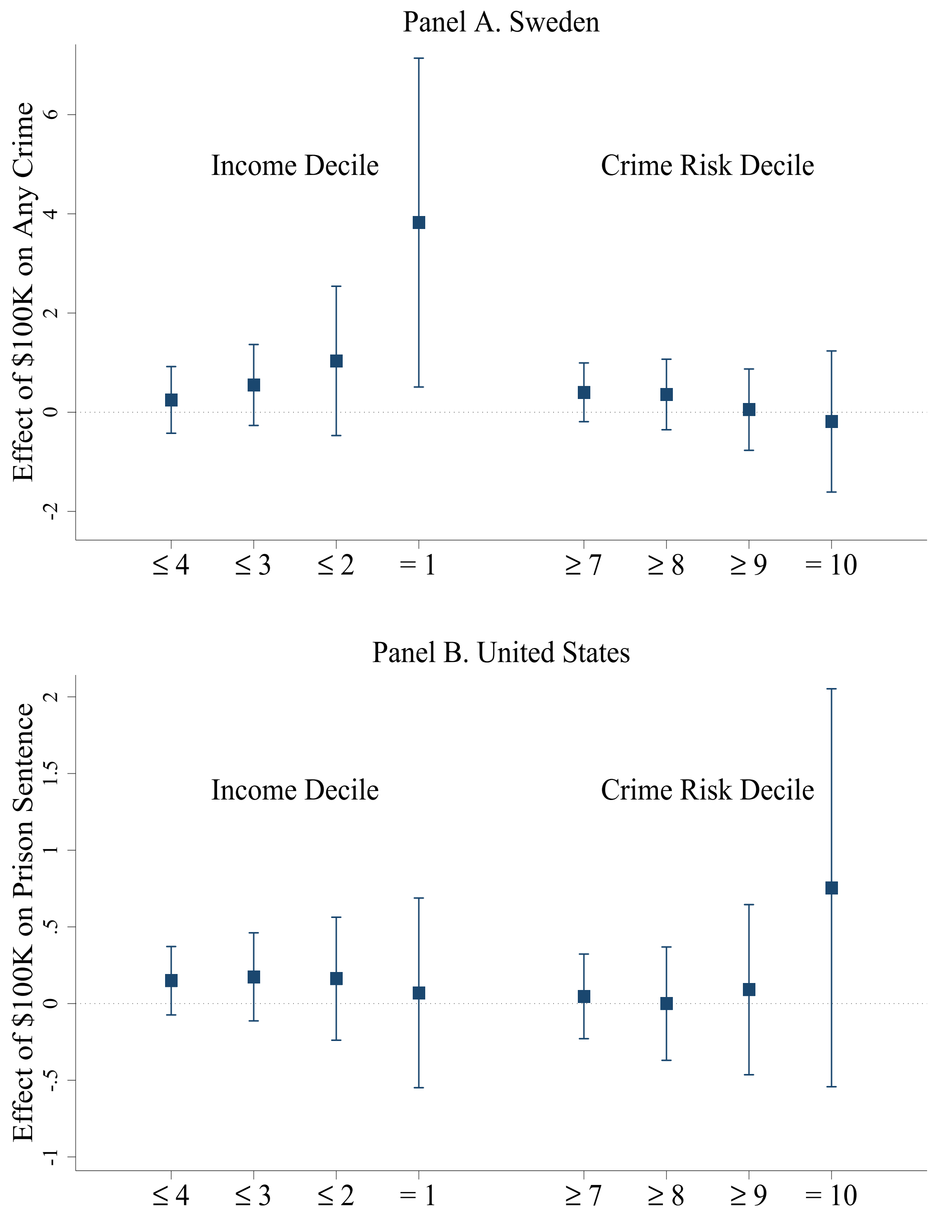
As a final, more agnostic, approach to test for treatment effect heterogeneity, we consider the cross-lottery cell variation in estimated treatment effects in the Swedish sample. First note that we can write the treatment effect β_w as

$$\beta_w = \sum w_k \beta_k, \tag{5}$$

where β_k is the treatment effect in cell k and w_k is the within-cell variation in lottery winnings. Formally, $w_k = \sum^{n_k} \left(\widetilde{L}_{ik,0} - \overline{\widetilde{L}}_{k,0}\right)^2$ where n_k is the number of players in cell k, $\widetilde{L}_{ik,0}$ is the lottery win of player i in cell k after residualizing with respect to the covariate vectors ($\mathbf{Z}_{i,-1}$ and $\mathbf{R}_{i,-1}$) and $\overline{\widetilde{L}}_{k,0}$ us the average residualized prize in cell k. In our adult sample, β_w is the weighted average of 2,421 different lottery cells. As shown above, we fail to reject the null that $\beta_w = 0$. However, if treatment effects vary across cells, then the distribution of $\hat{\beta}_k$ should have fatter tails compared to distributions simulated under the null that $\beta_i = 0$ for all i (which is implicitly assumed when perturbing the prize vector and which in turn implies $\beta_k = 0$ for all k). Comparing the actual and simulated distributions of $\hat{\beta}_k$ thus allows us to gauge the presence of heterogeneous treatment effects across cells without having to specify what factor may explain such heterogeneity.

The left panel of Figure D3 shows the actual distribution of $\hat{\beta}_k$ (weighted by w_k) does not deviate appreciably from the corresponding simulated distributions. More formally, the right panel of Figure D3 shows the percentiles of the actual distribution lies within the ranges of the simulated distributions, also at the low and high end of the distribution. If anything, the actual distribution of $\hat{\beta}_k$ appear somewhat less extreme than the distri-

Figure 1: Heterogeneity by Income and Crime Risk Deciles in the Adult Sample



Notes: Income deciles are defined within age-year-sex cells. Crime risk estimated based on the predicted values from a regression of of any conviction (Sweden) or imprisonment (United States) on the covariate vectors. Panel A: Standard errors are the maximum of unadjusted, heteroskedasticity-robust and clustered at the level of the player. Panel B: Standard errors are equal to the maximum of unadjusted and heteroskedasticity-robust.

butions simulated under the null of no treatment effect. In Figure D4, we show cell-level demographic characteristics (age, income, predicted crime risk and female share) are uncorrelated with the cell-level treatment effects ($\hat{\beta}_k$). In conclusion, we find no evidence of cross-cell treatment effect heterogeneity.

Gradient Comparison

To place our results in context, we rescale our lottery estimates in terms of log permanent income and compare them to the corresponding cross-sectional gradients. We follow the Plan and proceed in four steps. First, we calculate, for each lottery prize, the annual payout it would sustain if it were annuitized over a 20-year period with an annual real return of 2%. For example, a 1 million SEK prize corresponds to an increase in net annual income of SEK 59,960. Second, as a measure of permanent non-lottery income, we calculate average household disposable income during the five years prior to the lottery draw. In the third step, we add the annuitized lottery prize to our measure of permanent non-lottery income, thus getting a measure of total permanent income. In the final step, we instrument the log of total permanent income with the lottery prize, including the same set of controls as in model (1). Effectively, our IV regression implies we rescale the (reduced-form) lottery-based estimates reported above by the effect of winning the lottery on log permanent income (the first stage). The rescaled estimates can thus be interpreted as semi-elasticities where crime risk is expressed in percentage points but income in relative terms.

We compare the rescaled lottery-based estimates to log income gradients estimated using the same measure of permanent non-lottery household income as above, including controls for sex, a third-order polynomial in age and sex-by-age interactions. We estimate the gradients in two samples. First, we estimate gradients for Swedish lottery players who won less than 200,000 SEK ($30K) and U.S. players who won below $30K.[15] Second, in a

[15] The cutoff for the Swedish sample was stipulated in the Plan. We also exclude players who received study aid in the year prior to the lottery event from the Swedish lottery gradient-sample. When calculating the lottery gradients, we weigh each player by its share of the identifying variation. Formally, players in a lottery with N_l players which contributes a share s_l of the total identifying variation will get the weight s_l/N_l.

post hoc analysis, we estimate the gradients for a representative sample weighted to match the age and gender distribution of the lottery sample.

Figure 2 shows the rescaled lottery-based estimates and the associated gradients (see Table D5 for the underlying estimates). The lottery-based estimate for any type of crime in the Swedish sample implies an increase in permanent income by 10% increases conviction risk by approximately 0.15 percentage point with a lower bound of -0.08 percentage point. The Swedish gradients imply a 10% increase in income is associated with a reduction in conviction risk by about 0.30 or 0.39 percentage point, depending on whether we consider the lottery or representative sample. The null hypotheses that the gradients equal the rescaled lottery estimate are strongly rejected (p-values 0.001 or lower). The gradients are more negative than the rescaled lottery-based estimates also for all categories of crime, and the difference is statistically significant in two (lottery sample gradients) and four (representative sample gradients) out of five cases, respectively. Similarly, we reject the gradients for both types of pre-specified sentences in both samples.

The U.S. rescaled lottery estimate implies a 10% increase in income increases the risk of being convicted to prison by roughly 0.03 percentage point with a lower bound of -0.04 percentage point. As for Sweden, the gradients are strongly negative—corresponding to a 0.09 and 0.10 percentage point reduction in incarceration risk—and easily rejected at all conventional levels of statistical significance.

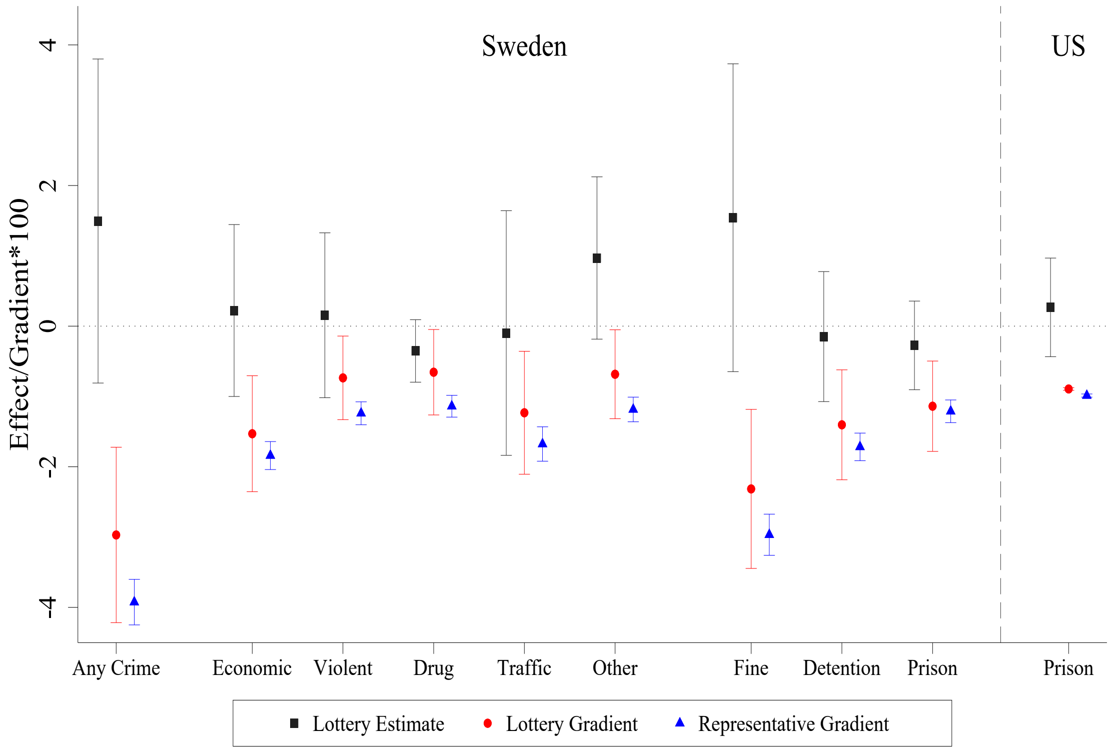
The similarity between the crime-income gradients in our lottery samples and in the representative samples broadly supports the external validity of our results. In particular, that the gradient is strongly negative also in the Swedish lottery sample suggests our failure to reject the null of zero effects of lottery wealth on convictions for criminal offenses is not due to these lottery players being so well off that income is irrelevant for predicting criminal activity. In short, while poor Swedish lottery players are significantly more likely to commit crime, windfall gains in the form of lottery prizes does not seem to reduce this probability. Still, a caveat to the gradient comparison in Figure 2 is that the income distribution for the lottery sample and the representative sample may differ. Though Table 2 shows average income is similar to the population average for the Swedish lottery sample (while lower for the U.S. sample), there are fewer lottery players in the lowest income deciles in the Swedish sample. Further, Figures A5 and A6 show the income-gradients are much steeper for the lower part of the income distribution. To address this issue, we reweight the lottery samples to match the income distribution of the representative sample. Figure D5 shows re-weighting does not appreciably change the rescaled lottery estimates for Sweden (or the United States).

Figure D5 also shows the re-scaled estimates for the lowest 4, 3, 2 and 1 income deciles (i.e., rescaled versions of the results shown in Figure 1), along with the gradient for the lowest four income deciles. As expected, restricting the sample to the lowest four income deciles implies substantially steeper gradients. In contrast, the rescaled lottery-based estimates either become more positive (Sweden) or stay roughly constant (United States). Though standard errors increase as sample size becomes smaller, we reject the null that the rescaled lottery estimates are equal to low-income gradients for all income groups for both countries.

5.2 Intergenerational analyses

We now turn to our intergenerational analyses. Table 5 shows the estimated effect on our main measure of child delinquency in the Swedish sample: whether children are convicted of any type of crime within 10 years after the lottery event (or 10 years after turning 15 if the child was younger at the time of win). The point estimate suggests that a child's conviction risk increases by 0.059 percentage point (SE = 0.497) for each $100K in parental lottery wealth. Considering that 11.9% of children in our data are convicted at least once, the insignificant increase in relative crime risk corresponds to just 0.5% of baseline risk. The 95% confidence interval allows us to reject that $100K in parental lottery wealth reduces crime risk by more than 0.92 percentage point (7.7% of baseline risk). Columns (2)–(6) show that, except for traffic crime, the estimated effects for all categories of crime are negative, though no estimate is statistically significant. We similarly estimate negative

Figure 2: Comparing Lottery Estimates to Log Income Gradients in the Adult Samples



Notes: The lottery-estimates are based on regressions where the log of average household income in the five years preceding the lottery draw plus an annuity for the lottery win (assuming prizes are annuitized over 20 years) is instrumented with the lottery win. The set of controls are the same as in model (1). The lottery sample gradients are estimated from the sample of winners who won less than SEK 200K (Sweden) or \$20K (U.S.), with observations weighted to match the identifying variation in each lottery. The representative sample have been weighted to match the age- and sex distribution of the lottery sample (weighted by the identifying variation in each lottery). The reported 95% confidence intervals for Sweden are based on standard errors which are the maximum of standard errors which are unadjusted, heteroskedasticity-robust and clustered at the level of the player. The reported 95% confidence intervals for U.S. are based on standard errors which are the maximum of standard errors which are unadjusted and heteroskedasticity-robust.

but statistically insignificant effects of parental lottery wealth on all types of sentences (columns (7)–(9)).

In the U.S. sample, the effect on incarceration among the children of lottery winners is similarly close to zero. The point estimate suggests \$100K in lottery wealth increases the risk of incarceration by 0.02 percentage point. The 95% confidence interval allows us to reject that \$100k in parental lottery wealth reduces incarceration by more than 0.06-percentage-point (4.5% of baseline risk).

Robustness

Table D6 shows the results for the same set of pre-registered robustness tests as for the Swedish adult sample. The estimated effect on the risk of being suspected of a crime is close to zero (0.015 percentage point per \$100K) and statistically insignificant. There is no clear pattern for how dropping prizes above \$595K (4M SEK) changes the results, apart from making estimates less precise. Panel A of Figure D7 shows the same post hoc analysis of non-linear effects as for the adult sample. The estimated effect of \$1M in parental lottery wealth is positive and marginally statistically significant, suggesting parental wealth increases child delinquency, but the post hoc nature and large confidence intervals of this result suggest it should be interpreted cautiously. Table D7 shows the results from the same type of post hoc robustness tests as for the Swedish adult sample (dropping the covariate vector, dropping traffic crime, dropping Triss-Monthly or PLS Odds prizes, or both). As for the adults, neither of these tests change the results appreciably.

We now turn to the U.S. intergenerational sample. Using the same tests as for the adult sample, we detect no evidence of non-linear effects (see Panel A of Table D8 and Panel B of Figure D7). Panel B of D8 reports specification tests which combined suggest little omitted variable bias. As for the adult sample, using only the within-treatment group variation in amount won gives a more precise but negative estimate. This is because the estimated "effect" of lottery wealth in the placebo group (the children of parents who won at too old an age for their incarceration to be affected by the shock) is slightly negative

Table 5: Main Analyses for the Intergenerational Sample

	A. Sweden									B. U.S.
		Type of Crime					Type of Sentence			
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention	Prison	Prison
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effect ($100K)*100	0.059	-0.192	-0.384	-0.143	0.232	-0.345	-0.293	-0.289	-0.126	0.021
SE	0.497	0.276	0.199	0.232	0.316	0.256	0.388	0.186	0.105	0.040
p (resampling)	0.875	0.559	0.099	0.592	0.397	0.203	0.480	0.249	0.395	-
p (analytical)	0.906	0.487	0.053	0.538	0.462	0.177	0.451	0.121	0.123	0.594
FWER p		0.592	0.372	0.592	0.592	0.570	0.480	0.421	-	-
Mean dep. var.*100	11.931	4.839	2.884	3.169	4.083	3.602	8.323	2.836	0.861	1.266
Effect/mean	0.005	-0.040	-0.133	-0.045	0.057	-0.096	-0.035	-0.102	-0.146	0.017
Pre-registered	Y	Y	Y	Y	Y	Y	Y	Y	-	-
N	115,306	115,306	115,306	115,306	115,306	115,306	115,306	115,306	115,306	5,145,045

Notes: This table reports the effect of winning the lottery on the criminal behavior of the players' children. Panel A: 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 ten years after age 15 or the lottery draw (whichever happens later), or year 2017. Children who were older than 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 the factors listed in model 2. 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) 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. FWER p-values are calculated separately for the analyses in columns (2)-(6) and (7)-(8). The mean of the dependent variable is calculated by weighting the sample by the treatment variation in each lottery. Panel B: Prison is an indicator variable equal to 1 in case of a spell of incarceration in the ten years between ages 15 and 25. Standard errors are equal to the maximum of conventional standard errors; Huber-White standard errors; and standard errors adjusted for clustering at the level of the winning parent.

but still small and insignificant (ruling out more than a 0.07 percent point reduction per \$100,000). The slight positive selection of large-prize winners is also apparent when we create a predicted incarceration measure using the control variables and estimated only among the placebo group. With our main specification, we estimate a negative but very small and insignificant relationship of this variable with winnings (result not in the table but it is -0.0036 per \$100K, SE $= 0.0046$). The results are also robust to including the full set of controls, which marginally increases the point estimate. These tests thus all suggest the covariate imbalance discussed in Section 4.2, if anything, bias our results toward finding protective effects of parental lottery wealth on children. Moreover, we estimate a positive and insignificant effect on incarceration in a specification with family fixed effects, which abstracts away from any differences across families, including from marriage.

Panel B of D8 further shows results are robust to reweighting the sample to match the distribution of socio-economic characteristics in the random, representative sample overall or within bin group. We also consider a number of robustness tests specific to the intergenerational sample. First, we reweight the sample so that each parent winner has the same weight (irrespective of his or her number of children). Second, we reweight the sample so that the age span for which we observe incarceration is the same in the treatment and control group. Third, we restrict the sample to cohorts where children are in both the treatment and control group (i.e. exclude the 2004 to 2007 birth cohorts). The estimated effect remains positive in all these tests, which demonstrates that any differences in the period over which we can observe treated and untreated children are not influencing our results. Finally, we report the results from a few additional tests analogous to the adult sample, including multiple prizes and varying sentence length, in all cases with null results.

Figure D6 shows the pattern of null results is robust to changing the age span during which we measure children's criminal behavior, for both Sweden and the United States.

Treatment Effect Heterogeneity

Table 6 Panel A reports the results from three pre-specified dimensions of heterogeneity in our Swedish sample: pre-win parental income, age at the time of the draw, and sex. In a post hoc analysis, we also split the sample crime risk. In neither of these subsamples do we reject the null of no effect, nor do we reject treatment effect homogeneity across subsamples. Table 6 Panel B shows we similarly do not reject the null of no effect or treatment effect homogeneity when the U.S. sample is split along the same dimensions. Table D9 shows there is no pattern of treatment effect heterogeneity across the four lotteries in the Swedish sample.

Figure 3 shows the results from a post hoc analysis where we restrict the sample to the bottom 4, 3, 2 and 1 deciles of the parental income distribution, and the top 4, 3, 2 and 1 decile of the child crime risk distribution. While we show the results for Sweden for completeness, the severe restrictions on sample size implies estimates become imprecise. Still, because estimates become more positive for children of low-income parents, we are able to rule out all but very small reductions in crime risk from lottery wealth for this group. In comparison, the U.S. estimates remain quite precise when sample size decreases. Similar to the Swedish case, there is no tendency for estimates to become negative as we go down the distribution of parental income or up the distribution of child crime risk. As we will discuss in the next section, the fact that the baseline level of crime is higher when parental income is low or predicted crime risk is high implies the lower bound of estimates scaled by baseline crime risk (such as the elasticity) remain stable despite larger confidence intervals on the absolute effect.

Gradient Comparison

Table D10 compares rescaled lottery-estimates to cross-sectional gradients calculated in the same way as for the adult sample, except we replace household income with the sum of the parents' disposable income and control for child age and gender, as well as the age of the mother and father, when estimating the gradients. In the Swedish sample, the

Table 6: Heterogeneous Effects in the Intergenerational Sample

A. Sweden

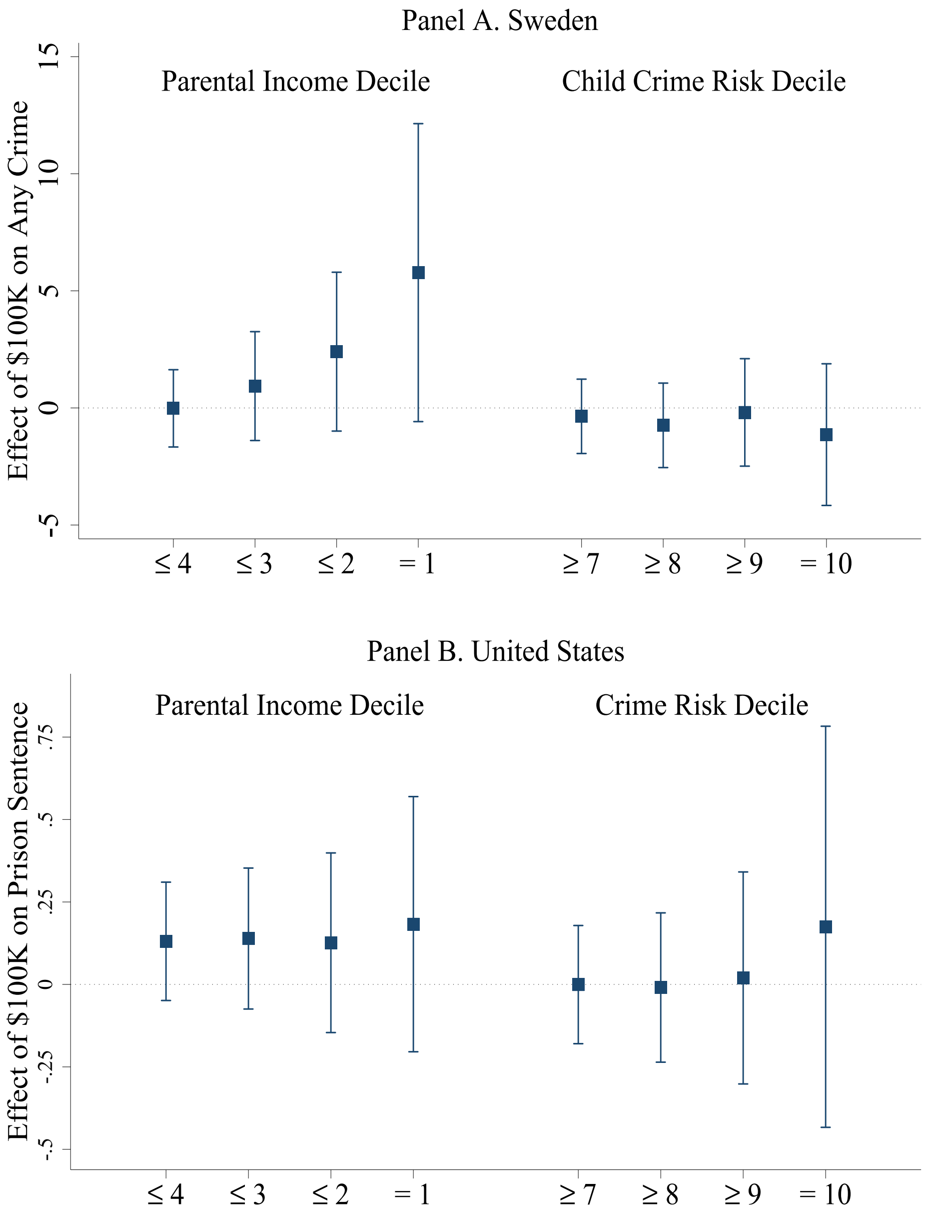
	Age		Sex		Parental Income		Crime Risk	
	< 10	≥ 10	Sons	Daughters	< Median	≥ Median	< Median	≥ Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect ($100K)*100	-0.352	0.503	-0.549	0.693	0.222	-0.234	0.289	-0.142
SE	0.697	0.682	0.742	0.554	0.743	0.569	0.522	0.806
p	0.583	0.564	0.478	0.450	0.813	0.694	0.516	0.872
p equal effects	0.335		0.167		0.990		0.622	
Mean dep. var.*100	10.066	13.206	17.339	6.399	12.696	11.254	5.387	16.682
Effect/mean	-0.035	0.038	-0.032	0.108	0.021	0.018	0.054	-0.009
N	52,085	63,221	58,648	56,658	48,821	66,485	57,653	57,653

B. United States

	Age		Sex		Parental Income		Crime Risk	
	< 10	≥ 10	Sons	Daughters	< Median	≥ Median	< Median	≥ Median
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Effect ($100K)*100	-0.003	0.032	0.023	0.015	0.097	-0.032	0.015	0.010
SE	0.054	0.046	0.073	0.028	0.077	0.039	0.025	0.083
p	0.950	0.488	0.755	0.592	0.210	0.412	0.547	0.903
p equal effects	0.531		0.923		0.118		0.953	
Mean dep. var.*100	1.164	1.410	2.196	0.295	1.782	0.892	0.265	2.701
Effect/mean	-0.003	0.023	0.010	0.051	0.054	-0.036	0.064	0.004
N	2,848,829	4,042,279	2,627,351	2,517,694	2,162,291	2,982,754	2,353,305	2,791,740

Notes: This table reports the results from three heterogeneity analyses which were pre-registered for the Swedish sample (age, sex and parental income) and one post hoc analysis (crime risk). Columns 1/9 and 2/10 display results separately by average combined parental disposable income for the five years before the lottery draw (above or below the median among parents in the representative sample in the same year and with children of the same age, using five-year intervals for child age). Columns 3/11 and 4/12 show results separately for children age 10 and younger at the time of the draw. Columns 5/13 and 6/14 show the results separately for sons and daughters. Columns 7/15 and 8/16 show the results by crime risk as predicted by the respective covariate vectors. Panel A: All regressions include the same set of covariates as in model 2 as well as interactions between all covariates and the cell fixed effects, and an indicator for the relevant dimension of heterogeneity. Standard errors reported are either unadjusted heteroskedasticity-robust, or clustered at the level of the player, whichever is largest. The p-values for both individual coefficients and for equality between coefficients are based on 10,000 permutations of the prize vector. Panel B: All regressions add full interactions of baseline control variables with an indicator for the relevant dimension of heterogeneity. Standard errors reported are either unadjusted, heteroskedasticity-robust, or clustered at the level of the player, whichever is largest, with the p-values for both individual coefficients and for equality derived from such adjustment.

Figure 3: Heterogeneous Effects by Income and Crime Risk in the Intergenerational Sample



Notes: Parental income decile are defined by average total parental disposable income in the five years before the lottery draw (above or below the median among parents in the representative sample in the same year and with children of the same age, using five-year intervals for child age). Panel A: Standard errors are the maximum of unadjusted, heteroskedasticity-robust and clustered at the level of the player. The p-values for equality between coefficients are based on 10,000 permutations of the prize vector. Panel B: Standard errors are equal to the maximum of conventional standard errors; Huber-White standard errors; and standard errors adjusted for clustering at the level of the winning parent. The p-value for equality between coefficients are derived from maximum standard errors.

rescaled causal effect for any type of crime (0.62) implies an increase in parental income by 10% increases the risk of conviction by about 0.06 percentage point. The gradients imply a 10% increase in parental income is associated with a 0.22 (lottery sample) and 0.70 (representative sample) percentage point reduction in conviction risk. However, only the latter gradient is statistically different from zero and in neither case can we reject that the gradients equal the rescaled lottery estimate. The same conclusion holds for the rescaled estimates with respect to type of crime and sentence: standard errors are too large to allow any strong conclusion regarding the causal effect relative to the gradient.

While the Swedish intergenerational analyses are only suggestive, the superior statistical power of the U.S. child sample allows for stronger conclusions. The rescaled U.S. lottery estimate implies an increase in parental income by 10% increases incarceration risk by 0.01 percentage point with a lower bound of -0.04 percentage point. The gradients imply a 10% increase in parental income is associated with a 0.07 (lottery sample) and 0.06 (representative sample) reduction in incarceration rate and are strongly rejected in both cases. Figure D8 shows we continue to reject the gradients also when restricting the sample to children of lottery winners in the lower part of the income distribution all the way down to the bottom decile.

6 Discussion

The previous section showed the effect of lottery wealth on adults' criminal behavior is significantly smaller than corresponding income gradients, and possibly zero. We similarly obtained null results for the effect of parental lottery wealth on children's criminal activity, though the gradient could only be rejected in the U.S. sample. In this section, we discuss these results in light of the previous literature. We focus on studies that estimate the effect of significant shocks to income or wealth.

The previous literature is not conclusive. For example, while an early study found income support to ex-convicts reduced recidivism (Mallar & Thornton 1978), a follow-up study on a similar, larger program found null results (Rossi, Berk & Lenihan 1980, Berk, Lenihan & Rossi 1980). In a similar vein, two studies found revoking drug convicts' eligibility for welfare and food stamps (SNAP) increased crime (Yang 2017, Tuttle 2019) while two studies obtained null results studying the same reforms (Luallen, Edgerton & Rabideau 2018, Mueller-Smith et al. 2023). In related work, Deshpande & Mueller-Smith (2022) found losing Supplemental Security Income at age 18 increased the number of criminal charges; Palmer, Phillips & Sullivan (2019) found access to emergency financial assistance in Chicago reduced arrests for violent crime but increased arrests for property crime (Palmer, Phillips & Sullivan 2019), whereas Carr & Koppa (2020) found no effect of housing vouchers on arrests. In a study on Danish refugees, Dustmann, Landersø & Andersen (2024) found cutting back financial aid increased property crime.

A first question to ask is whether our results are statistically distinguishable from previous studies that have found statistically significant results. Comparing estimates is difficult: studies consider different populations, different types of crime, different time horizons, and different types of shocks to wealth or income. To achieve some level of comparability across studies, we assume the variation represents a pure income shock with no substitution incentives and focus on crime-income elasticities, i.e., relating changes in relative risks of committing crime to changes in relative income. Detailed calculations are provided in Appendix C and the results summarized in Table D11.

The elasticities from our Swedish and U.S. samples are calculated by dividing the rescaled estimates in Figure 2 and Table D5 by the sample crime rate. For adults in Sweden, we obtain an elasticity of 0.375 with a confidence interval lower bound of -0.203. The adult elasticity for imprisonment in the U.S. sample is 0.231 with a lower bound of -0.377. For comparison, Table D11 also shows the elasticities implied by the representative sample gradients in Sweden and the U.S. are -0.865 and -0.890, respectively. Notably, as discussed in Section 5, our calculations of the lottery-based elasticities are based on the assumption that the prize money is spent over a 20 year period. A shorter time span would produce lottery-based elasticities that are closer to zero and have smaller standard errors.

The most straightforward comparison of our implied elasticities is to Deshpande and Mueller-Smith (2022). They estimate that losing Supplemental Security Income (SSI) at age 18 increased the risk of any conviction between age 18 to 38 by 0.062 (SE = 0.032) and the risk of incarceration by 0.029 (SE = 0.010). SSI eligibility is valued to $46,100 over 21 years, or $2,195 per year. Relating these changes to crime and income to their corresponding means, we obtain an elasticity of −0.470 for any type of crime (most comparable to our main outcome in Sweden) and −1.299 for imprisonment (most comparable to our U.S. estimates).

Comparing our estimates to other studies on adults is less straightforward. For example, Mallar & Thornton (1978) find $780 in total financial aid paid out over 13 weeks reduced the risk of being arrested within one year of release from prison. If we, in keeping with our assumptions for the lottery sample, assume financial aid is spent over a 20 year period and assume earnings remain fixed over this period, we get an implied elasticity well below −10, more than an order of magnitude larger than the elasticities implied by the U.S. income gradients. Of course, assuming a decades-long spending horizon in a sample of convicts just released from prison may be unreasonable. Still, much shorter spending horizons also imply elasticities outside of the lower bounds suggested by our lottery-based estimates. For example, a spending horizon of just one year gives an elasticity of −0.801.

The studies by Tuttle (2019) and Dustmann, Landersø & Andersen (2024) consider changes to income support eligibility that affect unearned income for several years, but the long-term duration of these effects are unclear.[16] If we scale the effect of unearned income by the concurrent income flows, we get an elasticity of −1.465 from Tuttle (2019) and −0.952 from Dustmann, Landersø & Andersen (2024).

In all of these instances, we can reject the null that our lottery-based elasticities and the implied elasticities from the studies above are the same. While we emphasize the rough nature of these comparisons, they raise the question of what factors could explain the

[16] Yang (2017) finds welfare eligibility decreases criminal behavior using the same reform as Tuttle (2019), but provide no estimates on the reform effect on unearned income, implying we cannot calculate the elasticity.

seemingly inconsistent results. One possible explanation is differences in study populations. While the winners in our Swedish lottery sample are quite similar to the population at large, the U.S. lottery sample is negatively selected in terms of incomes, educational attainment and propensity to commit crime. Still, the ex-convicts, welfare recipients and refugees for which previous literature has found significant effects are arguably more negatively selected than our U.S. sample of lottery winners. However, two factors suggest differences in sample characteristics is not an obvious explanation for the different results. First, as mentioned above, the results of our study are not unique as several other studies on vulnerable populations find either null (Berk, Lenihan & Rossi 1980, Luallen, Edgerton & Rabideau 2018, Carr & Koppa 2020, Mueller-Smith et al. 2023) or conflicting (Palmer, Phillips & Sullivan 2019) results. Second, the pattern of results in both our Swedish and U.S. data do not suggest more protective effects for winners with low income or high crime risk. To provide a sense of what kind of elasticities that can be ruled out for disadvantaged groups, Table D12 shows the lower bounds for the elasticities based on the estimates in Figure 1. Apart from the highest crime risk-decile in Sweden (or some comparisons to the minor-crime elasticity in Deshpande and Mueller-Smith 2022), we rule out elasticities substantially smaller than the elasticities suggested either by previous studies that reject the null or the crime-income gradients in our data. For example, the lower bound of the top crime risk decile in the U.S. sample is -0.206.

Another possible explanation is that treatment in other studies goes beyond pure income transfers. In particular, eligibility for income support is conditional on abstaining from criminal activity in all three studies based on U.S. data (Mallar & Thornton 1978, Tuttle 2019, Deshpande & Mueller-Smith 2022).[17] In Dustmann, Landersø & Andersen (2024), the reduction in income support implied weaker incentives to take part in integration programs. Thus, the stronger effect found in these studies could be due to the incentives

[17] Mallar & Thornton (1978) and Deshpande & Mueller-Smith (2022) consider cases where the treatment group receive income support which is discontinued in case they engage in serious crime. Tuttle (2019) instead consider a case where treatment implies SNAP eligibility is withdrawn, implying both a loss of income support and (compared to the control group) weaker incentives not to engage in crime as future criminal activity has no effect on eligibility.

provided by program eligibility rules affecting criminal behavior.

We now turn to the evidence regarding parental income and children's propensity to engage in crime. As for adults, we calculate income elasticities based on the assumption that (parental) lottery wealth is spent over 20 years. The elasticity for Sweden is then 0.052 with a lower bound of -0.807. The corresponding estimate for the U.S. lottery sample is 0.115 with a lower bound of -0.309. As for the semi-elasticities considered in the previous section, we reject the elasticity implied by the cross-sectional gradient in the U.S. but not in the Swedish sample.

The number of previous studies on children's criminal behavior with plausibly quasi-experimental variation in parents' unearned income or wealth is small and the evidence mixed. Jacob, Kapustin & Ludwig (2015) find winning a housing voucher lottery in Chicago had no effect on children's criminal behavior, despite implying a substantial boost to family income. In contrast, Dustmann, Landersø & Andersen (2024) found cutting aid to Danish refugees lead to a marginally statistically significant increase in the number of convictions for children up to the age 20. Using the same methodology as for parents, the implied elasticity is -1.157 ($SE = 0.649$), which is not statistically distinguishable from our lottery-based estimates. Based on the Great Smoky Mountains Study of Youth, Akee et al. (2010) estimate that a \$4,000 annual income supplement over four years decreases the probability of children having committed a minor crime by age 21 by 17.9 percentage point ($SE = 8.9$), while there is no discernible effect on moderate crime. If we, as for our lottery sample, assume recipients smooth consumption over a 20-year period, we get an implied elasticity of -16.09 ($SE = 8.00$) for minor crime. If we instead assume the income supplement is spent over four years (the duration of the supplement) and that these are the only formative years, we get an elasticity of -3.75 ($SE = 1.86$). However, the confidence interval in Akee et al (2010) includes elasticities just below zero (the upper bound is -0.10 in the four-year case). A simple meta-analysis of Dustmann, Landersø & Andersen (2024), the four-year minor-crime estimate in Akee et al. (2010), and our lottery-based estimates where each estimate is weighted by the inverse of its estimated variance gives an elasticity of -0.037. Adding Jacob, Kapustin & Ludwig (2015) to the meta-analysis would reinforce the conclusion of an elasticity close to zero. While we again emphasize the rough nature of these comparisons, we believe the broader point still stands: our null results for the effect of parental wealth on child crime is not inconsistent with the previous evidence if estimation uncertainty is taken into account.

Table D12 shows the lower bounds of the elasticities when we restrict the sample to children of low-income parents or children with high crime risk, i.e., the implied elasticities from Figure 3. Despite the comparably lower precision of the Swedish child estimates we always reject the elasticity suggested by the point estimate for minor crime in Akee et al. (2010), and typically also the implied elasticity from Dustmann, Landersø & Andersen (2024). For the U.S. lottery sample, we reject elasticities much smaller than implied by these previous studies in all subsamples.

7 Conclusions

The association between crime and income is one of the strongest gradients found in the social sciences. The goal of this paper is to cast light on the underlying casual pathways behind this crime-income gradient.

To this end, we have leveraged plausibly exogenous variation in wealth among Swedish and U.S. lottery winners. The Swedish sample allows us to obtain unbiased estimates of the casual effect of wealth under weak assumptions and estimate the effect on different types of crime and sentences. The U.S. data require stronger (though still plausible) identifying assumptions and is restricted to incarceration as an outcome, but allows for more precise estimation and in an institutional environment with fewer income transfers and higher rates of crime.

For both Sweden and the U.S., we detect no statistically significant effects of lottery wealth on criminal behavior. For winners, our estimates allow us to rule out effects substantially smaller than the cross-sectional gradients between income and crime in both countries. For the winners' children, we reject the gradient between parental income and child delinquency for the U.S., but not for Sweden. Although our results should not be casually extrapolated to other countries or segments of the population, a number of factors suggest our results may generalize beyond the specific contexts we consider. First, despite the institutional and demographic differences between Sweden and the United States, the results are strikingly similar. Second, our null finding are not an artifact of particularly law-abiding study populations. The level of crime in Sweden is similar to that of comparable countries, and our sample of Swedish lottery players has a crime rate that is only slightly lower than the Swedish population. For our U.S. lottery sample, the incarceration rate is higher than in the U.S. population at large. Moreover, heterogeneity analyses do not suggest more protective effects in sub-samples characterized by low income or high crime risk.

Of course, the conclusion of our paper is not that the effect of economic resources on crime is zero: for neither the Swedish nor the U.S. samples can we rule out modest protective effects. However, our study does suggest that the causal effect of resources is meaningfully smaller than what both crime-income gradients and previous quasi-experimental studies that reject null effects (when interpreted as purely a resource shock) have indicated, challenging the view that the lack of financial resources is a primary cause of criminal behavior.

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"Does Wealth Inhibit Criminal Behavior? Evidence from Lottery Winners in Sweden and the United States"

Online Appendix

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A Institutional Background and Data on Crime

A.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. SFS also contains the Swedish Penal Code (*Brottsbalken, BRB*) which is 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.

Criminal cases are tried in one of 48 district courts (tingsrätten). Appeals of decisions made in the district courts are heard before one of six courts of appeal (hovrätten). The Supreme Court (Högsta domstolen) is the highest court in the Swedish judiciary and the final instance for appeals. The Supreme Court typically hears high profile cases, and those that have the potential to set a precedent for future judgments.

A.2 Swedish 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 offenses and are used when a prosecutor offers an offender the opportunity to accept a fine in exchange for not taking the case 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 offenses, implying the crime in question is unlikely to affect the sentence. The register does not include fines for minor offenses issued by police, customs and related officials (*ordningsbot*).

Our extract from the register spans the years 1975–2017 and contains convictions of individuals aged 15 (the age of criminal responsibility in Sweden) or older at the time of infraction. Individuals are identified by unique personal identification numbers that allow matching to the lottery data, as well as data on individual background characteristics from Statistics Sweden. In the data, each conviction can comprises up to 25 crimes. The Swedish judicial system defines crimes by the principle of instance such that a single crime typically corresponds to violations occurring 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 Penal Code and violations of other laws in the 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 Penal Code, and forgery is a crime according to chapter 14, article 1 of the Swedish Penal Code.

For each section of the law, we observe the chapter, article, and paragraph for crimes against the Swedish Penal Code, and the exact statute and applicable paragraph for other crimes in the SFS. 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 the following broad initial categories: property crimes, violent crimes, drug crimes, white-collar crimes, traffic crimes, and other crimes. Property crimes include 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) assault, 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

Table A1: Swedish Crime Categories

Categories	Criminal 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 criminal code chapters (BRB) and the coding of the most common codes from the Swedish Code of Statutes (SFS).

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 offenses (e.g. moderate levels of speeding) do not result in entries in the registry. 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 more comprehensive list of the crimes we assign to each category is included in Table A1. 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 is day fines (*dagsböter*), which are typically handed out in convictions punishable by fine that are of a more serious nature. Day fines consist of two components: a number of fines and an amount that is calculated based on 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.

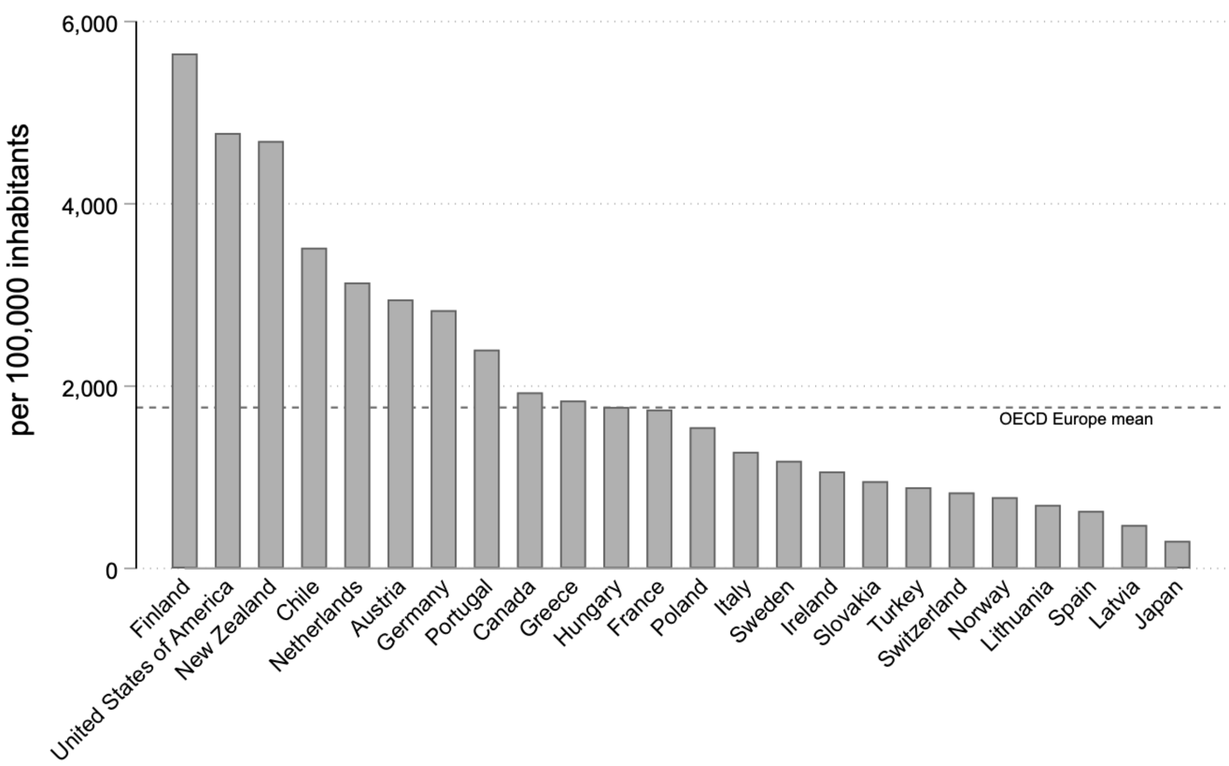
Apart from fines, most forms of punishment constitute some form of restriction of freedom. These punishments range from to community service and probation for lesser crimes to long prison sentences for the most severe crimes. In many cases, underage offenders ages 15–20 are sentenced to either juvenile care (*ungdomsvård*) or juvenile detention (*sluten ungdomsvård*) delivered outside of the adult correctional system. We define all sentences that involve some restriction of freedom as *detention* and the subset that involve serving time in prison as *jail*.

Although 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. 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.

A.3 Crime in Sweden and the U.S. 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 A1 displays the number of persons brought in formal contact

Figure A1: Persons Brought in Contact with the Criminal Justice System

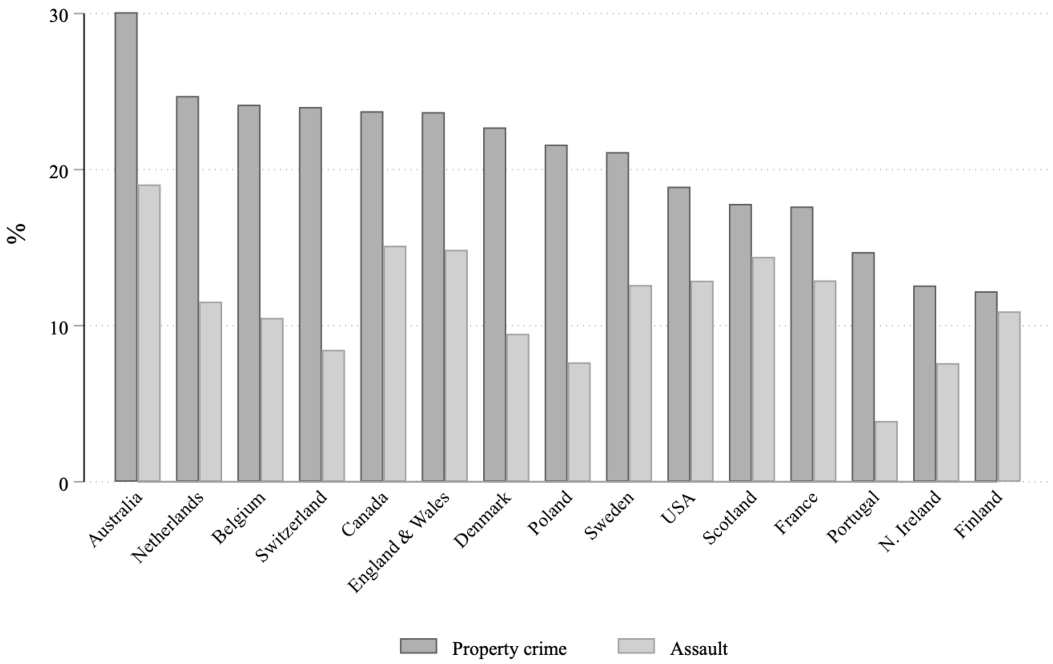


Source: United Nations Office on Drugs and Crime.

with the criminal justice system in 2005 for a sample of OECD countries. Although Sweden appears in the bottom half of the ranking, it is close to the median among the European countries in the sample (11th out of 19). The United States ranks second after Finland.

A major factor that affects crime statistics and hinders not only international comparisons, but also longitudinal studies of crime, is 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 A2 plots the percentage of households that are victims of crime between 1994 and 1999 for the sample of countries covered by the 2000 ICVS. For both property crime and assault, both Sweden and the U.S. falls roughly in the middle of

Figure A2: Percentage of Households Victim to Property Crime and Assault, 1994-1999



Source: International Crime Victim Survey.

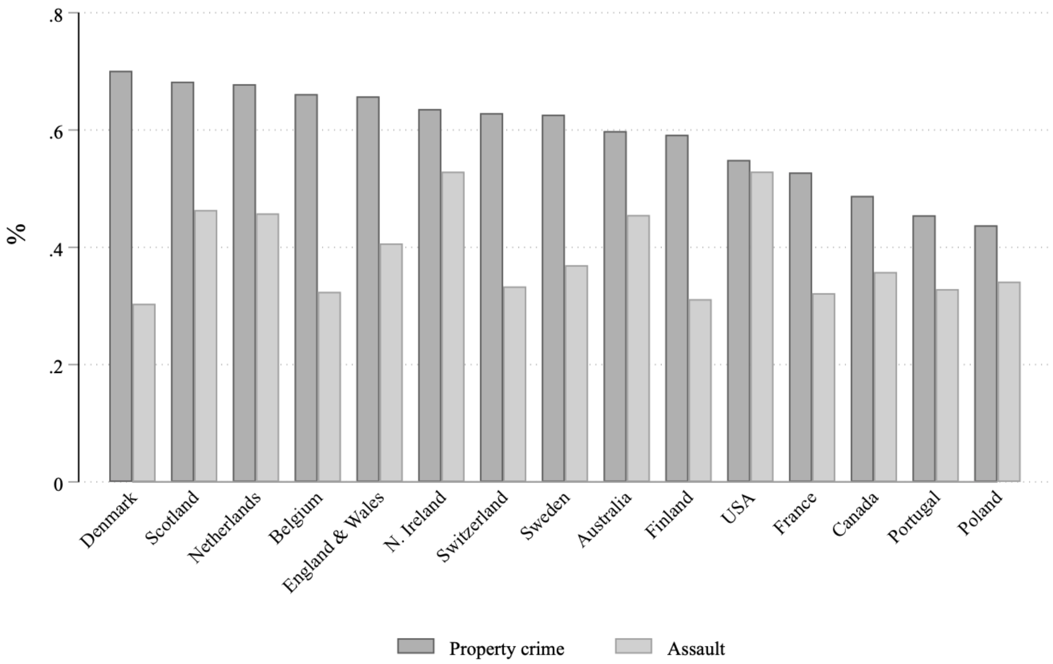
the pack.

To provide a picture of the relative willingness to report crimes, Figure A3 plots the percentage of property crimes and assaults that 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. The United States has a lower reporting rate of property crime, but a higher rate for assault.

A.4 Descriptive Statistics of Crime in Sweden and the U.S.

This subsection focuses on 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

Figure A3: Share of Crimes Reported, 1994–1999



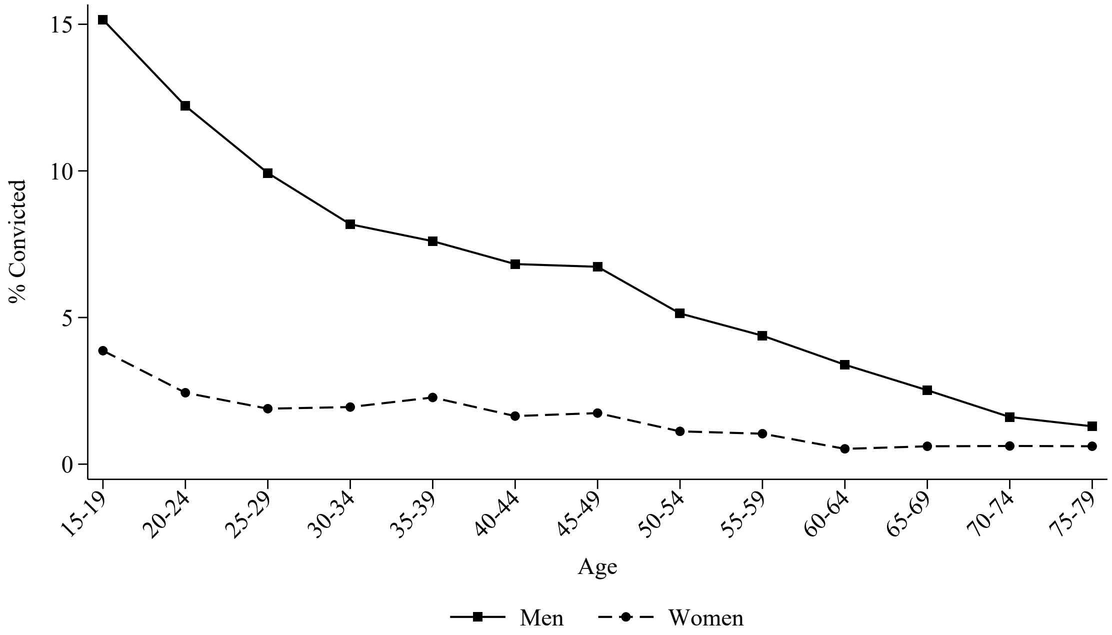
Source: International Crime Victim Survey.

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 of a crime varies by sex and age For each sample, we follow all individuals ages 15–79 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 A4 shows men are much more likely than women to commit crimes, and that the propensity to commit crimes decreases with age for both genders.

Panel A of Table A2 shows the share of 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 of at least one crime, compared with one out of 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 A2 shows fines are the most common form of punishment. Notably,

Figure A4: Criminal Activity by Age and Gender in the Swedish 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.

the share of women who receive a harsher sentence is smaller than the share of men who do. Whereas the relative risk of being sentenced to paying a fine is 4.5 times larger for men, the relative risk of serving jail time is more than 14 times larger.

Panel C shows the distribution of convicted individuals by number of crimes. More than half of convicted men and two thirds of convicted women are only convicted of one crime during the five-year period we study. A relatively small group of individuals are convicted of five crimes or more, yet this group is responsible for 57% of all recorded crimes in our data.

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

Table A2: Descriptive Statistics of Convictions in the Swedish 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	1.06	0.18
White collar	0.25	0.06
Traffic	3.78	0.53
Other	2.00	0.30

B. By type of sentence (% of sample)

	Men	Women
Fine	5.95	1.32
Detention (including jail)	1.96	0.23
Jail	1.13	0.08

C. By perpetrator number of crimes

	Men	Women
1	57.0	66.2
2	16.7	15.1
3	6.8	6.4
4	4.4	3.2
≥ 5	15.1	9.1

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.

relative to others of the same gender, age (five-year intervals) and sampling year. To avoid simultaneity bias, we measure the share convicted during the five years after the sample was drawn.

Figure A5 shows criminal behavior is strongly related to income. Whereas 18.7% of men in the lowest income decile are convicted of a crime, the same is true for only 3.5% of men in the highest decile. Though the level is much lower for women, the relative difference in criminal behavior is similar: women in the bottom decile are about seven times more likely to be convicted of a crime relative to women in the top decile. In unshown analyses, we find the gradient for men is similar when we use their own disposable income instead of the household's, but is considerably flatter for women.[18] We also find the gradients get steeper (in relative terms) 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 17 times more likely to be sentenced to detention and 21 times more likely to go to prison.

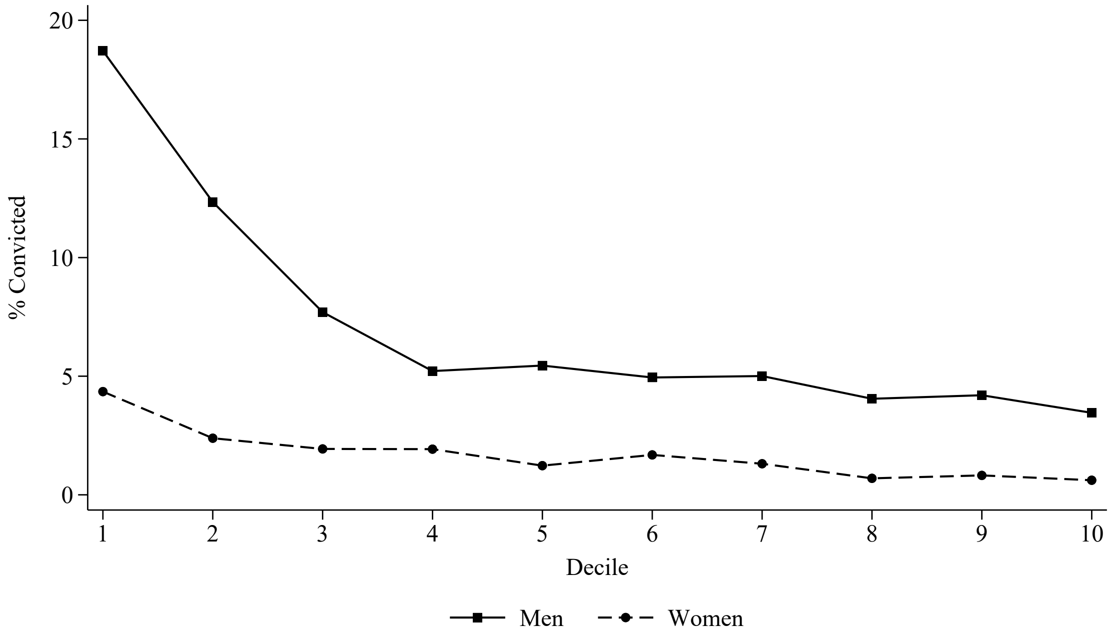
Finally, Figure A6 shows prison-income gradients for both Sweden and the United States. The gradients are calculated using the same method as for Figure A5. However, the incarceration rate for the bottom income decile for the U.S. is likely a lower bound. The reason is people who already are incarcerated 1) have low income and 2) are less likely to receive a new sentence. Excluding people who were incarcerated at the start of the five-year period or coding them as "sentenced" (thus changing the outcome from "no incarceration" to "incarcerated") makes the U.S. prison-income gradient even steeper.

B Lottery Data

In this section, we provide additional material regarding the construction of cells of lottery players, the prize distribution and tests of the conditional exogeneity of lottery prizes.

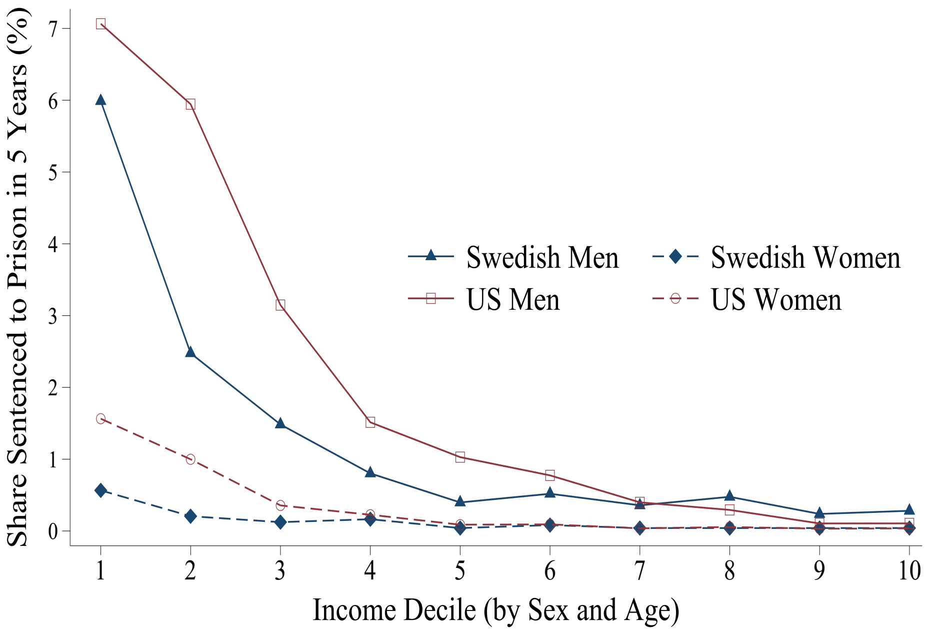
[18]A likely reason for the flatter own-income gradient for women is that female labor supply is decreasing in spousal income, pushing down the incomes of highly educated women (who are likely to be married to high-income men).

Figure A5: The Swedish Crime-income Gradient



The figure shows the share of men and women ages 30–54 from representative samples drawn in 1990, 2000 and 2010 who have been convicted of 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.

Figure A6: Prison-income Gradients in Sweden and the U.S.



Sweden: The figure shows the share of men and women ages 30–54 from representative samples drawn in 1990, 2000 and 2010 who have been convicted to serve time in prison 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 sex, age (five-year intervals), and the year the sample was drawn. U.S.: The figure shows the share of men and women ages 30–54 from representative samples drawn between 2011-2014 (the treatment years) who have been incarcerated 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 sex, age (five-year intervals), and the year the sample was drawn.

Table B1: Cell Construction Across Swedish Lottery Samples

	Time Period	Treatment Variable	Cell Construction: Adults	Cell Construction: Intergenerational
PLS Fixed Prizes	1986-2003	Prize	Draw×#Prizes	Draw×#Prizes
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

Notes: This table summarizes the cells constructed for each of the lotteries in the sample. Institutional knowledge of the way in which prizes were allocated in each of the lotteries allows us to construct groups of players (cells) of in which the lottery prize amounts were as good as randomly assigned. The cell construction column details the characteristics players must share to be placed in the same cell.

B.1 Lottery Cells (Sweden)

Table B1 shows the cell construction described in Section 3 of the paper.

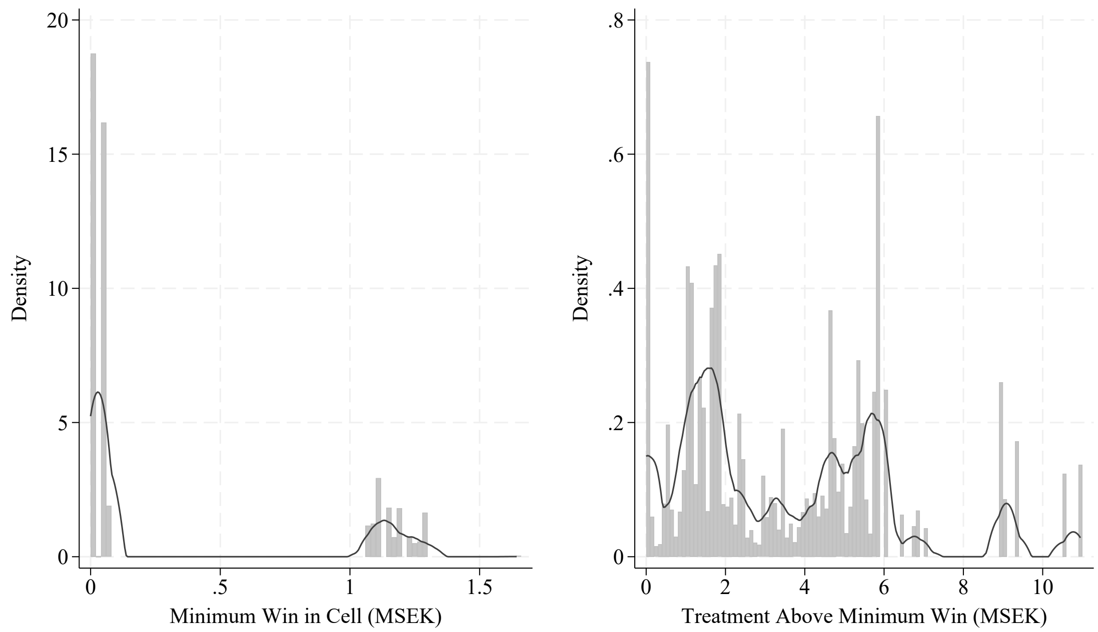
Figure B1 shows the within-cell level variation in amount won in the Swedish sample. The left panel shows the minimum amount won in each cell. The right panel shows the distribution of prizes above this minimum level.

B.2 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, 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 winner sample, we estimate the following model:

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

Figure B1: Cell-level Variation in Treatment in the Swedish Lottery Sample



Notes: This figure illustrates the cell-level variation in amount won in the Swedish lottery sample. The graph to the left shows the distribution of the minimum amount won in each cell. The graph to the right shows the distribution of the amount won above the minimum.

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.[19] $\mathbf{R}_{i,-1}$ 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 prior to the lottery draw 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 intergenerational sample, we estimate

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

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

where $\mathbf{Z}_{p,-1}$ is a vector of pre-win socio-economic characteristics of child j's biological parents and $\mathbf{R}_{p,-1}$ is a vector of 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 each parent was born in a Nordic country, was married and had a college degree. $\mathbf{R}_{p,-1}$ is the same vector of pre-win criminal behavior as in model B.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 being born in a Nordic country.

As stated in the Plan, our test of exogeneity in models B.1 and B.2 is whether we can reject the null hypothesis of joint insignificance of all predetermined covariates for all lotteries combined. As also stated in the Plan, we focus on the permutation-based p-values constructed by simulating the F-statistic for joint significance under the null hypothesis of zero treatment effects (Young 2019) and cluster the standard errors at the level of the player (adult sample) and family (intergenerational sample).

Table B2 shows that, for the adult sample, the p-values based on clustered standard errors are always above 0.05 (the cutoff stipulated in the Plan), regardless of whether we consider the full sample or each lottery individually. Although this finding is reassuring, the fact that we don't reject joint insignificance in the specification without cell fixed effects (column 1) raises the concern that our test may have limited power. As further discussed in Section 4 below, the combined skewness of both lottery prizes and criminal behavior implies statistical inference based on standard errors that adjust for heteroskedasticity may be unreliable. To the extent that F-statistics based on clustered standard errors exhibit high variability also under the null, actual differences across samples are harder to detect. As a post hoc supplement, Table B2 therefore also reports permutation-based p-values for F-statistics based on unadjusted standard errors. In this case, we reject the null of joint

Table B2: Testing for Conditional Random Assignment of Lottery Prizes

	Adult Sample					
	A. Sweden					B. U.S.
	All		Kombi	Triss	PLS	
	(1)	(2)	(3)	(4)	(5)	(6)
p (clustered)	0.160	0.177	0.294	0.146	0.553	-
p (unadjusted)	0.000	0.618	0.060	0.159	0.226	0.242
N	354,034	354,034	37,442	4,815	311,777	2,043,527
Cell FE	No	Yes	Yes	Yes	Yes	-
	Intergenerational Sample					
	C. Sweden					D. U.S.
	All		Kombi	Triss	PLS	
	(7)	(8)	(9)	(10)	(11)	(12)
p (clustered)	0.665	0.073	0.145	0.147	0.912	0.002
p (unadjusted)	0.001	0.181	0.946	0.117	0.678	0.001
N	120,159	120,159	6,768	2,298	111,093	5,145,045
Cell FE	No	Yes	Yes	Yes	Yes	-

Notes: The table reports resampling-based p-values for joint significance of the covariates in model B.1 (adult sample) and B.2 (intergenerational sample) from 10,000 perturbations of the prize vector, as described in the main text. Standard errors are either unadjusted or clustered at the level of the player (adult sample) or the family (intergenerational sample). In the U.S. sample standard errors are either unadjusted or robust (adult sample) or unadjusted, robust, or clustered at the winner level (intergenerational sample)

significance when the cell fixed effects are not included. Still, the p-values with cell fixed effects included are always above 0.05.

The U.S. exogeneity test is similar whereby the treatment variable of interest is regressed on the remaining variables from the main specification (fixed effects and amount of the win) along with the predetermined characteristics. The p-value reported in the table reflects whether we can reject the null hypothesis of joint insignificance of the predetermined characteristics, which we are unable to do in the adult sample but can do in the intergenerational sample. However, as discussed in the intergenerational robustness section, the preponderance of evidence is that our estimate is not meaningfully biased upwards.

B.3 Deviations from the Pre-analysis Plan

A coding mistake in the selection of Kombi controls implies that we cannot re-create the exact sample used in the Plan. As specified in the pre-analysis plan (henceforth, "the Plan"), we select up to 100 controls (matched on tickets in the month of the draw, age and gender) to each winners in the Kombi sample. When more than 100 controls are available, we select 100 controls randomly. Because of a missing a "sortseed" command in our Stata code, we are unable to generate exactly the same set of controls as used in the Plan. However, since the procedure for selecting the Kombi controls are unchanged, the ex ante sampling properties of both samples are the same. The coding mistake does thus not affect the credence of our identification, though it does imply minor differences in terms of sample size, descriptive statistics and the assessment of which specification is optimal. We comment on these issues below.

First, because a few restrictions are imposed on the sample after selecting the controls, sample sizes used in the paper differ slightly from those reported in the Plan. To be precise, there are 26 fewer observations in the adult estimation sample (354,034) compared to the Plan (354,060) in the adult sample and 8 more observations in the intergenerational sample (69,264 vs. 69,256), which includes one observation from the Triss-Lumpsum lottery which was excluded from the Plan due to another small coding mistake.

Second, comparing Table 2 with Table 5 in the Plan shows the descriptive statistics of the samples are very similar. For example, the share with any conviction in the previous five years is 3.88% in the estimation sample compared to 3.87% in the Plan. Demographic characteristics like share females (48.8% in both sample), share married (54.1% in both samples) and share with a college degree (20.2% vs. 20.1%) are also very similar.

Finally, re-running the analyses for statistical power (see Section 5.3 in the Plan) does not yield different conclusions regarding the adult sample (the full sample with age range 18-74 is still optimal), but suggest statistical power is somewhat higher if we consider a time horizon of $t = 9$ rather than of $t = 7$ (see the discussion in Section 4). However, the difference in power is tiny (91.9% vs. 92.4%). In the main analyses reported in the paper, we followed the Plan and focused on criminal behavior at $t = 7$. Figure D1 shows the main conclusion of the paper — that we can reject substantial reductions in criminal behavior following lottery wins — would be slightly strengthened were we to focus on criminal behavior at $t = 9$ instead of $t = 7$. The optimal specification for the child analyses is unchanged compared to the Plan.

C Comparison of Crime-income Elasticities

We here provide the calculations behind the elasticities discussed in Section 6 and shown in Table D11. Before commenting on the individual studies, a few comments are in order.

First, we focus on elasticities rather than absolute estimates (the effect of income or wealth shocks measured in dollars on risk expressed in percentage points), relative effects (scaling the effects by baseline risk) or semi-elasticities (scaling the wealth shock by some measure of baseline income). The reason is the vast difference in outcomes and sample characteristics across studies.

Second, the studies considered below have been chosen because they report statistically significant findings. Studies that report null (Berk, Lenihan & Rossi 1980, Jacob, Kapustin & Ludwig 2015, Luallen, Edgerton & Rabideau 2018, Carr & Koppa 2020, Mueller-Smith et al. 2023) or conflicting (Palmer, Phillips & Sullivan 2019) findings have not been considered. We also do not discuss all potentially relevant estimates from the studies we do consider, but focus on estimates which are statistically significant and most comparable to our lottery estimates. The upshot is that the estimates reported are far from comprising an encompassing view of the literature. Our objective is more modest.

Third, our calculations of standard errors are only based on the uncertainty in estimated treatment effects — the uncertainty in the relative size of the wealth/income shock (the "first stage") is not taken into account. The simple reason for this choice is that the information required for estimating uncertainty in the first stage is not available. Consequently, the standard errors reported below should be viewed as lower bounds (this is in contrast to the elasticities reported for our lottery samples where the IV-specification implies uncertainty from the estimation of the first stage is reflected in the standard errors).

Finally, we emphasize the uncertainty of our calculations. The implied elasticities should be viewed as indicative rather than definitive. That said, we have generally attempted to make choices that make implied elasticities closer to zero (and thus, in effect, closer to the elasticities based on our estimates). In particular, we typically assume shorter spending horizons compared to the 20-year horizon assumed for our lottery samples. A shorter spending horizon implies a given wealth shock is larger relative to income, which in turn makes elasticities smaller in absolute value.

C.1 Adult Elasticities

The elasticities from our Swedish and U.S. samples are calculated by dividing the rescaled estimates in Figure 2 and Table D5 by the sample crime rate. For example, the rescaled lottery estimate for any crime in Sweden reported in Figure 2 is 0.01495 with a standard error of 0.01176. Dividing this estimate by the sample crime rate of 0.03774 gives an elasticity of 0.396 with a confidence interval lower bound of -0.215. The corresponding implied elasticity for imprisonment in the U.S. sample is 0.231 with a lower bound of -0.377. For comparison, the elasticities implied by the representative sample gradients in Sweden and the U.S. are -0.865 and -0.890, respectively. Notably, as discussed in Section 5, this calculation of the lottery-based elasticity is based on the assumption that the prize money is spent over a 20 year period. A shorter time span would have given a more precisely estimated elasticity closer to zero.

Deshpande and Mueller-Smith (2022)

Deshpande and Mueller-Smith (henceforth DMS) estimate the effect on losing eligibility for Supplemental Security Income (SSI) at age 18 on future outcomes. Table 1 shows losing eligibility increases the risk of any conviction between age 18 to 38 by 0.062 (SE = 0.032) and the risk of incarceration by 0.029 (SE = 0.010). The control group conviction rate is

0.387 and the incarceration rate 0.047.

DMS value SSI eligibility to \$46,100 (p. 2303), or \$2,195 per year over 21 years.[20] DMS provide estimates for how the reform affects earnings, but not total income. To get an estimate of total income, we instead turn to Deshpande (2016) who study the same reform. Deshpande (2016, p. 3317) states mean total income in the control group is \$9,041 and that losing eligibility reduces total income by \$1,569 (the main reason for the smaller effect on total income is that losing eligibility increases earnings).[21] Because DMS consider a negative wealth shock, we calculate the elasticity based on the mean crime rates and incomes in the treatment group. That is, we relate the reduction in SSI income (\$2,195 or \$2,167) to the treatment group average total income of \$9,041 - \$1,569 = \$7,472.

We thus get an elasticity with respect to conviction risk of

$$(-0.062/(0.062 + 0.387))/(2,195/7,472) \approx -0.470$$

with SE $\approx$ 0.243 while the elasticity for incarceration is

$$(-0.029/(0.047 + 0.029))/(2,195/7,472) \approx -1.299$$

with SE $\approx$ 0.448.

Mallar and Thornton (1978)

Mallar and Thornton (henceforth MT) study a population of 432 ex-offenders with a high probability of committing theft crimes and with no known history of alcohol or narcotic abuse (p. 211). The sample is split into four groups. There are two treatments: financial aid of \$60 a week for 13 weeks (thus \$780 in total) and job-placement services. Group 1 and 2 receive financial aid while group 1 and 3 receive job placement services. Group 4 is

[20] We follow DMS in applying discounting when assessing the total value of eligibility. Because income support from SSI is front-loaded, discounting has a relatively limited impact on the WTP for eligibility.

[21] Deshpande (2016) states the effect of losing eligibility on SSI income is \$2,167, somewhat smaller than the number from DMS (\$2,195). Because the higher number gives elasticities slightly closer to zero, we use it in the calculations below.

the control group. Here, we only consider the effect of financial aid.

Table 4 in ML shows financial aid reduces the re-arrest rate by 0.0804 (SE = 0.0419), which corresponds to a reduction in relative crime risk by 0.263 (p. 217). Table 6 states the average dollars per week earned in each quarter is 45.37, 52.50, 48.42 and 43.84, implying an annual total of approximately 12.5*(45.37 + 52.50 + 48.42 + 43.84) ≈ \$2376.6. The key assumption when computing an elasticity from these estimates is the time horizon over which the financial aid is spent. A longer time horizon implies the financial aid is relatively smaller, resulting in a larger elasticity. Arguably, assuming a 20 year horizon, as we do in our sample of lottery winners might, be unreasonable in a sample of newly released convicts. Yet it is an open question what spending horizon might be reasonable. Here, we set the spending horizon to one year, giving us an elasticity of -0.263/(780/2376.6) ≈ -0.801 (SE ≈ 0.417).

Dustmann, Landersø and Andersen (2024)

Dustmann, Landersø and Andersen (henceforth DLA), study a reform that reduced benefits to refugees in Denmark. Table 2 states the reform reduced transfers by \$9,775 in the first year since residency; \$8,320 in the second year and \$4,956 on average in year 3-5, but increased labor earnings by \$1,144, \$1,567 and \$1,070 for each year, respectively. The pre-reform means for transfers for the corresponding years are \$18,431, \$17,979 and \$15,849, respectively, while the means of labor market earnings are \$1,852, \$4,182 and \$8,424. Estimates beyond the first five years are not provided. Table 6 shows the number of convictions four years after residency increases by 0.053 (SE = 0.018) for the treatment group, from a baseline level of 0.096. DLA also report a short-term crime rate (only the first year after residency) and estimates for specific sub-groups, but we focus on the longer-term crime rate for the overall sample.

Comparing the estimates reported by DLA to our estimates requires several strong assumptions. First, we assume the effect on transfers beyond the first five years after residency do not matter. Second, we assume the refugees in the sample do not smooth consumption beyond the five year period, but smooth perfectly within this period. These two assumptions imply we can relate the total negative income shock to the total counterfactual income flows during the five years after residency. Assuming a longer spending horizon — as for our sample of lottery winners — would make the income shock smaller as a share of income and thus imply an elasticity further away from zero. For simplicity, we relate the income shock to individual rather than household income.

Because the treatment implies reducing a benefit, we calculate elasticities based on the baseline crime rate and income of the treatment group post-treatment. The relative change in the crime rate is thus 0.053/(0.053 + 0.096) = 0.053/0.149 = 0.3557. The change in income is 9,775 + 8,320 + 3*4,956 = 32,963, the pre-treatment transfer income is 18,431 + 17,979 + 3*15,849 = 83,957, giving a post-transfer income of 83,957 - 32,963 = 50,994. To the post-treatment transfer income we add post-treatment labor earnings equal to 1,852 + 4,182 + 3*8,424 + 1,144, 1,567 + 3*1,070 = 37,227, implying a total post-treatment income of 50,994 + 37,227 = 88,221. The relative change in income is thus -32,963/88,221 = -0.3736. Dividing the relative change in crime with the relative change in income gives an elasticity of 0.3557/-0.3736 ≈ -0.952 with a standard error of (0.018/0.149)/0.3736 ≈ 0.323.

Tuttle (2019)

Tuttle studies the effect of removing eligibility for SNAP (Supplemental Nutrition Assistance Program) for drug offenders in Florida. Table 3 shows that removing eligibility increases recidivism by 0.0950 (SE = 0.0467). The control group mean is 0.1644. Table 2 reports estimates of the share of SNAP benefits of gross income for SNAP recipients to between 15.7 and 29.3% depending on year and family status, something Tuttle (p. 315) summarizes as "about 20%".

Like DLA, Tuttle consider a change to income support eligibility that affect unearned income for several years, but where the long-term duration of these effects are unclear. We follow the same approach as for DLA above and scale the effect by concurrent income flows. We use the 20%-figure mentioned by Tuttle for these calculations, but because Tuttle (like DMS and DLA) consider a case where a benefit is removed, we scale benefits by the income of the treatment group, for which SNAP benefits would give a 25% increase in income. (We are making the additional assumption here that SNAP benefits does not affect other sources of income, therefore likely overstating the effect of benefits on income and thus understating the absolute value of the elasticity). Notably, Tuttle reports the effect of losing eligibility — an intention-to-treat effect (ITT) — that should ideally be scaled by the effect of losing eligibility on SNAP take-up. In other words, the 20%-share benefit share referred to above (which we use in our calculations below) pertain to those who *receive* SNAP benefits not those who are *eligible*. Along with the other assumptions discussed above, the fact that we use the ITT-estimate likely implies the absolute value of the calculated elasticity is understated.

Given the assumptions above, we obtain an elasticity of $(0.095/(0.095 + 0.1644))/0.25 \approx -1.465$ (SE ≈ 0.720).

C.2 Intergenerational Elasticities

Akee et al. (2010)

Akee et al. estimate consider variation in family income from installments from casino profits in the Great Smoky Mountains Study of Youth. Akee compare children who resided in families who received installments for six or four years to children whose family income increased for only two years. The installments were approximately $4,000 per year per parent with American Indian background. We focus here on the difference between children whose families received the installment for six years versus the control group (comparing the difference between the "intermediate" group and the control group would give similar but somewhat attenuated implied elasticities).

Akee et al. report results for a number of different outcomes, but we here focus on whether children have ever been convicted for a crime by age 21 (Table 8). Akee et al. find the probability of children having committed a minor crime by age 21 by 17.9 percentage points ($SE = 8.9$), while there is no discernible effect on moderate crime. We focus here only on the estimate for minor crime. Table 1 shows the minor crime-rate in households with at least one American Indian in the household is 0.25 while average annual income is $20,919.

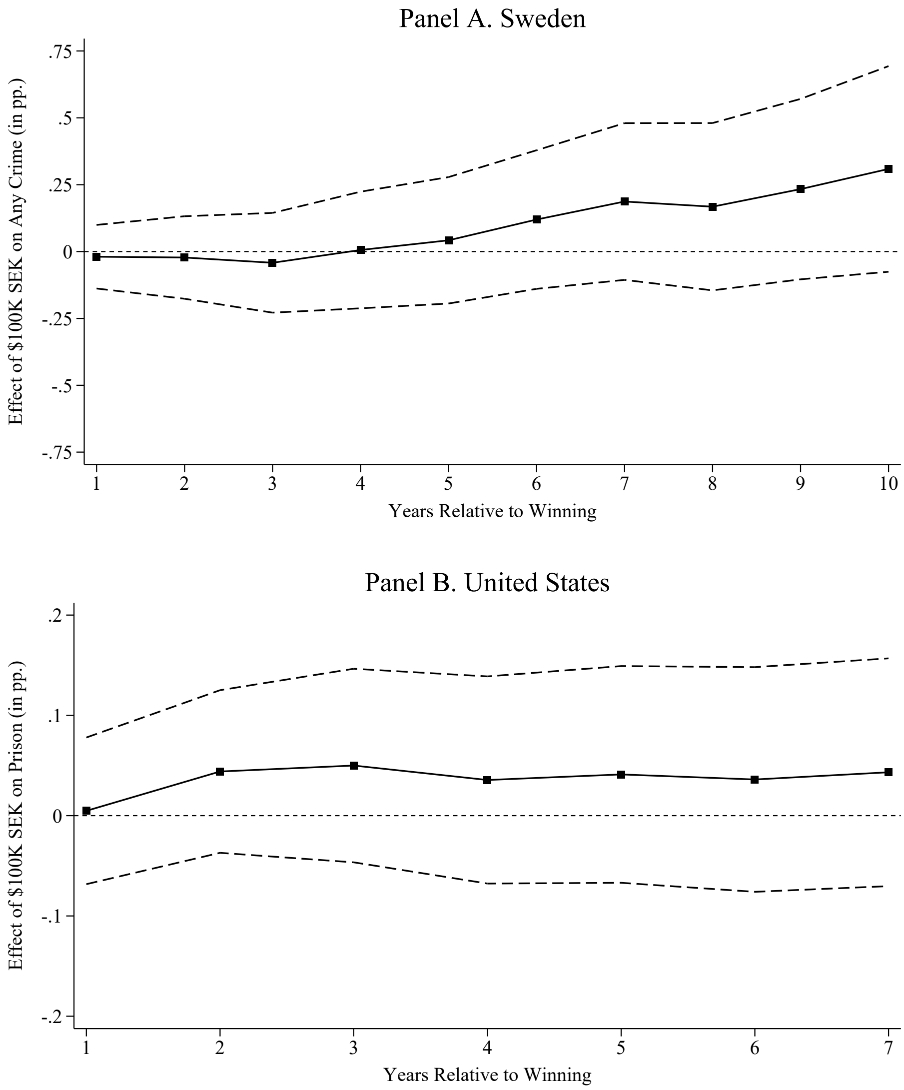
If we assume, as we do for our samples of lottery winners, that families spend the income supplement over a 20-year period, the annuity is $931, or 4.45% of average annual family income of $20,919 (Table 1). The implied elasticity is then (-0.179/0.25)/0.0445 ≈ -16.09 (SE ≈ 8.00). If we instead simply relate the $4,000 payout to average income, the boost to income is 4,000/20,919 = 19.12% and the implied elasticity (0.179/0.25)/0.1912 ≈ -3.745 (SE ≈ 1.862). Notably, this smaller elasticity is based on the very strong assumption that the four years of additional income support is all that matters for children's criminal behavior. Relaxing this assumption would result in an even more negative elasticity.

Dustmann, Landersø and Andersen (2024)

Table 9 of DLA shows cutting aid increased the number of convictions for the refugees' at age 20 by 0.269 (SE = 0.151). The pre-reform number of crimes was 0.416. Using the same methodology as for parents, we get an elasticity of (0.269/(0.416 + 0.269))/-0.3736 ≈ -1.157 (SE ≈ 0.649).

D Additional Results

Figure D1: Effects over Time in the Adult Samples



Panel A: The figure shows the results from model 1 with t varying from 1 to 10. 95 percent confidence intervals based on the maximum of the four types of standard errors discussed in Section 4. Panel B: The figure shows the results from model 3 with t varying from 1 to 7. 95 percent confidence intervals based on the maximum of unadjusted and robust standard errors.

Table D1: Robustness Tests for the Swedish Adult Sample

				Type of Crime					Type of Sentence		
	Any Suspicion	Any Crime	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention	Prison
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Effect ($100K)*100	-0.261	0.044	0.429	0.038	0.013	-0.041	0.047	0.263	0.407	0.053	-0.011
SE	0.202	0.165	0.222	0.098	0.072	0.045	0.153	0.139	0.207	0.098	0.072
p (resampling)	0.251	0.791	0.046	0.681	0.813	0.516	0.734	0.012	0.049	0.524	0.946
p (analytical)	0.199	0.791	0.054	0.698	0.853	0.367	0.758	0.059	0.050	0.589	0.876
p equal effects	0.240										
Mean dep. var.*100	6.011	4.102	3.222	0.773	0.465	0.209	1.921	0.570	2.786	0.598	0.304
Effect/mean	-0.043	0.011	0.133	0.050	0.028	-0.194	0.024	0.461	0.146	0.088	-0.037
N	152,173	152,173	325,602	325,602	325,602	325,602	325,602	325,602	325,602	325,602	325,602
Sample	Suspect sample		Prize no more than $595K (4M SEK)								

Notes: This table reports pre-registered robustness tests with respect to the Swedish sample analyses in Table 3. The dependent variable in column 1 is an indicator equal to one in case a player was suspected of a crime within six years of the lottery draw (data available from 1995 onwards). The sample in column 2 is restricted to the same sample as in column 1. The sample in columns 3-11 is restricted to people who won $595K/4M SEK or less. The p-value for equal effects between column 1 and 2 is based on a stacked regression (taking the largest of robust and clustered standard errors). The mean of the dependent variable is calculated by weighting the sample by the treatment variation in each lottery.

Table D2: Post Hoc Robustness Tests for the Swedish Adult Sample

	Any Crime		Any Crime Except Traffic		Any Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Effect ($100K)*100	0.187	0.148	0.157	0.198	0.030	-0.027
SE	0.150	0.159	0.113	0.181	0.158	0.199
p (resampling)	0.243	0.456	0.196	0.303	0.862	0.938
p (analytical)	0.211	0.355	0.164	0.272	0.852	0.893
Mean dep. var.*100	3.774	3.774	1.985	3.793	4.002	4.130
Effect/mean	0.050	0.039	0.079	0.052	0.007	-0.006
N	325,796	325,796	325,796	325,177	294,598	293,979
Lotteries	All	All	All	All Except Triss-Monthly	All Except PLS Odds	All Except Triss-Monthly & PLS Odds
Covariate Vector	Yes	No	Yes	Yes	Yes	Yes

Notes: This table reports post hoc robustness tests with respect to the results in column 1 of Table 3. The results in column 1 are identical to column 1 of Table 3 and only included for comparison. Column 2 shows the results when the covariate vector (but not the vector of lottery cell fixed effects) are dropped from the regression. The dependent variable in column 3 is a dummy for having committed any non-traffic crime up to 7 years after the lottery event. Columns 4-6 shows the results when we drop Triss-Monthly, PLS Odds prizes or both from the estimation sample. Standard errors and p-values are calculated as in Table 3. The mean of the dependent variable is calculated by weighting the sample by the treatment variation in each lottery.

Table D3: Robustness Tests for the U.S. Adult Sample

A. Different Prize Ranges

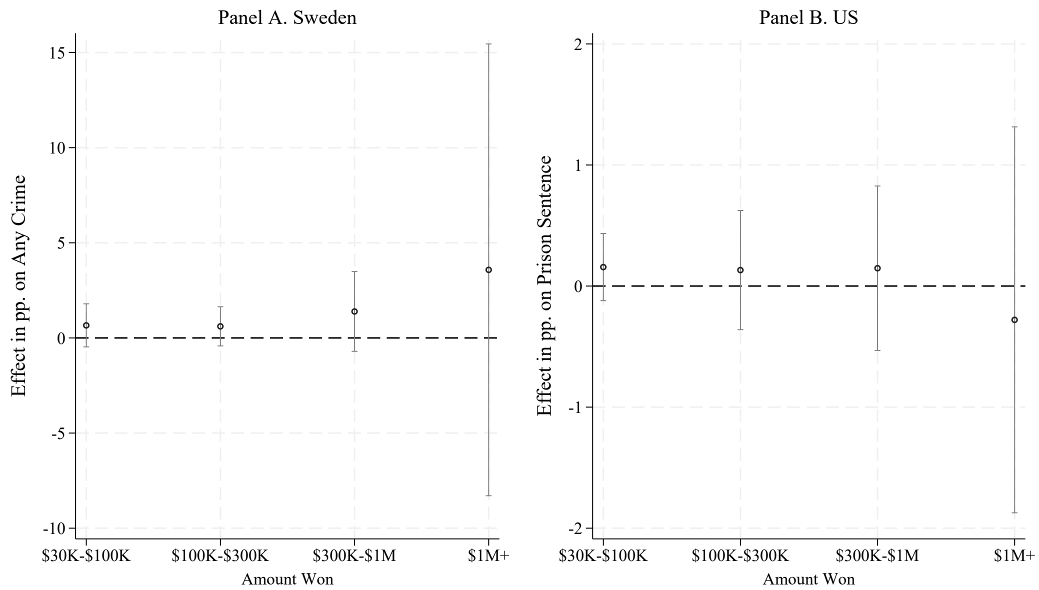
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Effect ($100K)*100	0.043	0.028	-0.074	-0.035	0.129	0.032	-0.011	0.010	0.013
SE	0.058	0.058	0.066	0.073	0.089	0.049	0.049	0.045	0.031
p	0.455	0.624	0.261	0.631	0.149	0.521	0.822	0.832	0.668
N	2,043,527	767,244	73,324	35,429	2,041,400	2,043,880	2,044,017	2,044,086	2,044,278
Max prize	$1M	$1M	$1M	$1M	$500K	$1.5M	$2M	$2.5M	$5M
Min prize	$600	$1K	$10K	$30K	$600	$600	$600	$600	$600

B. Different Specifications

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Effect ($100K)*100	-0.017	0.085	0.065	0.037	-0.014	0.064	0.022	0.000	0.043
SE	0.031	0.061	0.055	0.046	0.062	0.055	0.061	0.044	0.046
p	0.577	0.162	0.235	0.426	0.817	0.240	0.719	0.998	0.343
N	1,167,900	2,043,527	2,043,527	2,043,527	2,043,527	2,416,175	2,043,527	2,043,527	2,043,527
Outcome							Sentences that end $>$ 7 years after the win	Shorter sentences	Longer sentences
Specification	Only prize size variation	Triple difference	Full set of controls	Reweight to random sample	Reweight each bin to random sample	Including multi-year/subsequent prizes			

Notes: The columns in panel A vary the minimum or maximum win size in each regression relative to the baseline range reproduced in column 1. Panel B column 10 removes future winners from the specification, column 11 differences out the incarceration outcome variable with prior incarceration, column 12 adds the full set of controls (incarceration in the prior 5 years, ever incarcerated, logarithm of after tax income, sex, marital status, whether filed tax return, college educated, and whether a U.S. citizen.), column 13 reweights the lottery sample to look like the representative sample along these same control variables, column 14 reweights each of the five prize win range categories from Figure D2 to match the representative sample using the same control variables, column 15 adds future lottery wins of repeat winners and wins taken as an annuity, column 16 includes prison sentences that end 8 or more years after the lottery win, and columns 17-18 compare effects on below and above median prison sentence length. Standard errors are equal to the maximum of conventional standard errors and Huber-White standard errors.

Figure D2: Post Hoc Tests of Non-linear Effects



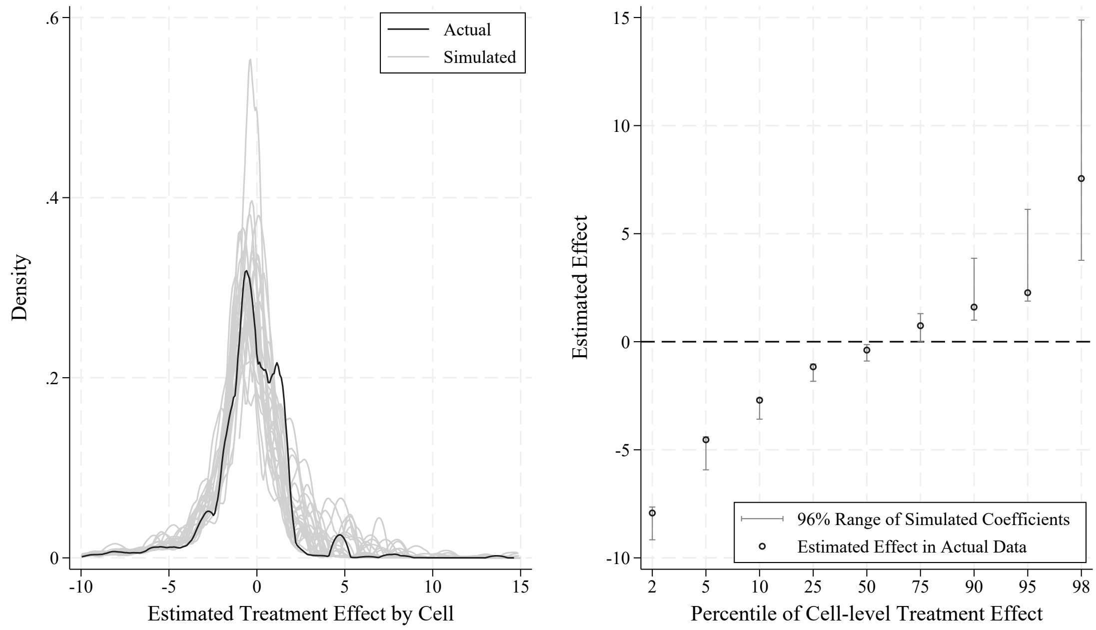
Panel A: The figure shows the results from model 1 where amount won is replaced with four indicator variables for amount won. The excluded category are prizes below $30K. 95 percent confidence intervals based on the maximum of the four types of standard errors discussed in Section 4. Panel B: The figure shows the results from model 3 with amount won replaced by the same four indicator variables as for Panel A and with prizes below $30K as the excluded category. 95 percent confidence intervals based on the maximum of unadjusted and robust standard errors.

Table D4: Post Hoc Tests of Heterogeneous Effects in the Swedish Adult Sample

	Crime Risk		Disp. Income*		Lotteries			
	Low	High	Below Median	Above Median	PLS	Kombi	Triss-Lumpsum	Triss-Monthly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect ($100K)*100	0.066	0.271	0.329	0.162	0.582	-0.260	0.072	0.120
SE	0.217	0.189	0.300	0.149	0.295	0.352	0.259	0.258
p	0.536	0.373	0.264	0.647	0.038	.586	0.777	0.665
p equal effects	0.501		0.606		0.616			
N	162,898	162,898	131,256	194,540	288,860	32,653	3,664	619

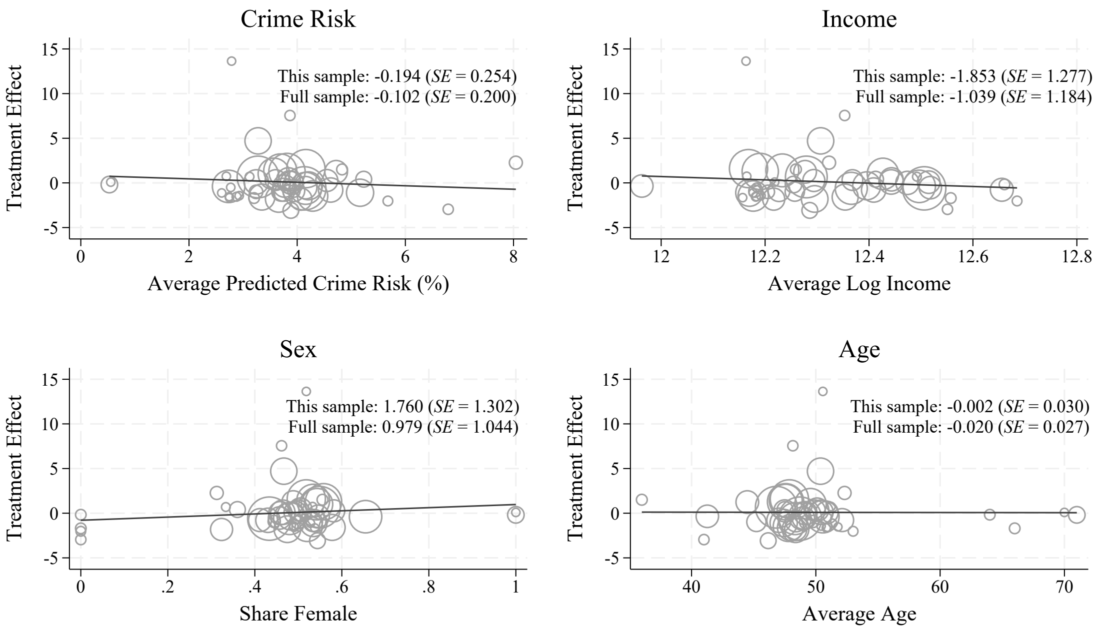
Notes: This table reports the results from post hoc heterogeneity analyses. Columns 1 and 2 show the results when the estimation samples are split by the median of predicted crime risk (based on the covariate vectors). Columns 3 and 4 show results separately for those above or below the median disposable household income in the corresponding year-sex cell in the representative sample between age 25 and 74. Columns 5-8 show the results by lottery. All regressions include the same set of covariates as in model 1 plus interactions between all covariates (including the cell fixed effects in columns 1 to 4) and an indicator for the relevant dimension of heterogeneity. Standard errors are the maximum of unadjusted, heteroskedasticity-robust and clustered at the level of the player. The p-values for both individual coefficients and for equality between coefficients are based on 10,000 permutations of the prize vector.

Figure D3: Distribution of Cell-level Treatment Effects in the Swedish Adult Sample



Notes: This figure shows the distribution of cell-level treatment effects in the Swedish lottery samples with each cell weighted by its share of the treatment variation. The left graph shows the actual density plotted against 25 distributions simulated under the null of zero treatment effects, smoothed using a kernel density. For visibility, treatment effects outside -10 and +15 are not included in the graph. The right graph plots the (weighted) cell-level treatment effect percentiles alongside the range of the percentiles of simulated cell-level treatment effects (with the top and bottom 2% of simulated percentiles being excluded).

Figure D4: Cross-cell Treatment Effects Heterogeneity in the Swedish Adult Sample



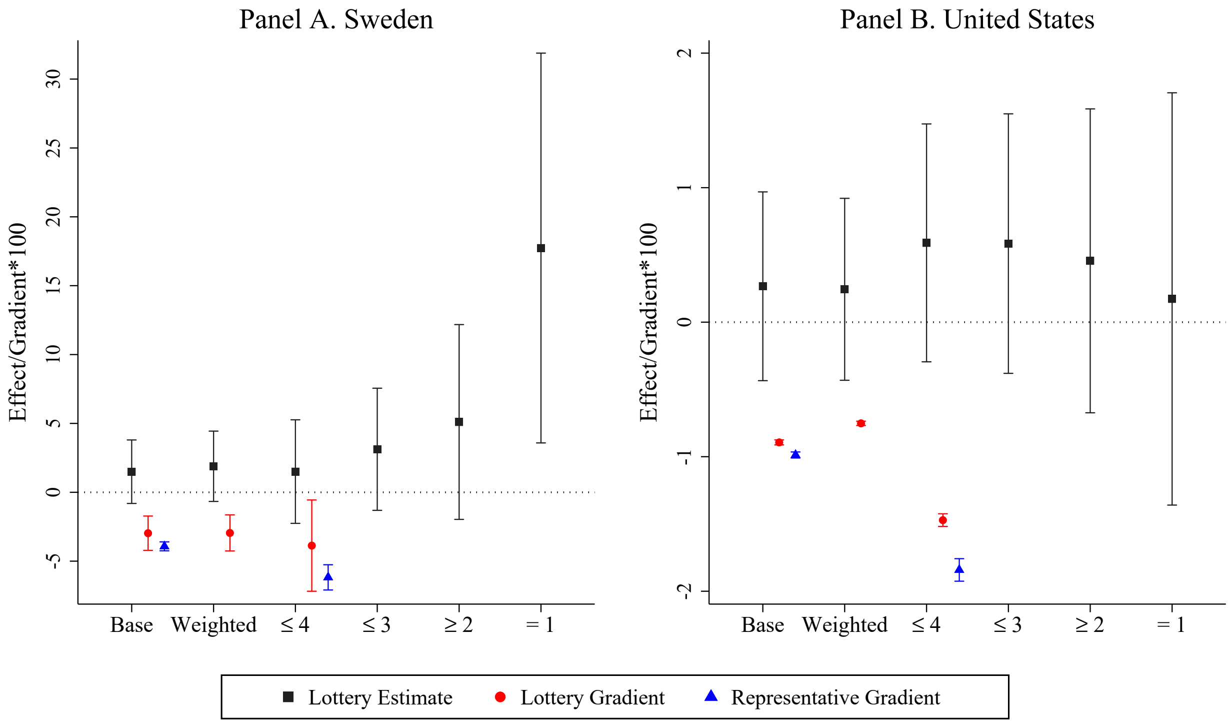
Notes: This figure shows the cell-level treatment effects in the Swedish lottery samples plotted against cell-level average characteristics. Each cell is weighted by its share of the treatment variation. For illustrative purposes, a few small cells with extreme values have been excluded from the graphs. Each graph shows the coefficients from regressions of cell-level treatment effects against average characteristics (weighted by treatment variation) for the cells shown in the graph ("this sample") and when all cells in the sample are included ("full sample").

Table D5: Comparison to Log Income Gradients in the Adult Samples

	A. Sweden									B. U.S.
		Type of Crime					Type of Sentence			
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention	Prison	Prison
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lottery Estimate.*100	1.495	0.222	0.155	-0.353	-0.098	0.969	1.542	-0.149	-0.274	0.267
SE	1.176	0.624	0.598	0.227	0.888	0.589	1.117	0.472	0.322	0.358
N	325,788	325,788	325,788	325,788	325,788	325,788	325,788	325,788	325,788	2,043,527
Lottery Gradient*100	-2.972	-1.531	-0.734	-0.655	-1.232	-0.684	-2.316	-1.403	-1.139	-0.894
SE	0.637	0.421	0.303	0.310	0.446	0.323	0.577	0.399	0.328	0.009
N	244,246	244,246	244,246	244,246	244,246	244,246	244,246	244,246	244,246	1,997,014
p equal effects	0.001	0.020	0.185	0.432	0.246	0.014	0.002	0.042	0.060	0.001
Rep. Gradient*100	-3.926	-1.841	-1.240	-1.139	-1.677	-1.185	-2.967	-1.718	-1.211	-0.987
SE	0.165	0.102	0.083	0.079	0.125	0.089	0.149	0.100	0.082	0.011
N	88,029	88,029	88,029	88,029	88,029	88,029	88,029	88,029	88,029	897,157
p equal effects	< 0.001	0.001	0.021	0.001	0.073	< 0.001	< 0.001	0.001	0.001	< 0.001

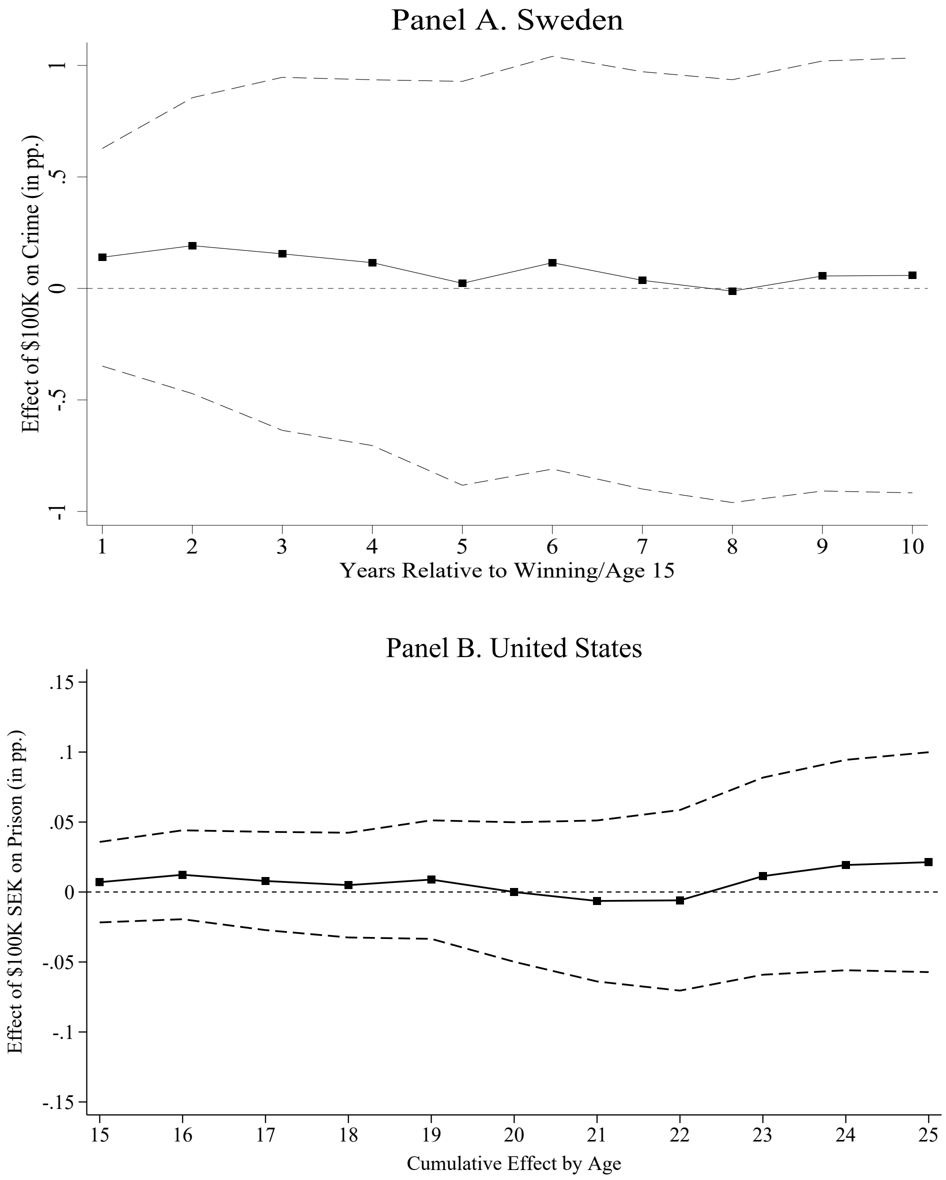
Notes: The lottery (causal) estimates are based on regressions where the log of average household income in the five years preceding the lottery draw plus an annuity for the lottery win (assuming prizes are annuitized over 20 years) is instrumented with the lottery win. The set of controls are the same as in model 1 (Panel A) and model 3 (United States). The lottery sample gradients are estimated from the sample of winners who won less than SEK 200K (Sweden) or $20K (US) with observations weighted to match the identifying variation in each lottery (this weighting explains the larger standard errors for the Swedish lottery sample gradients as the relatively few Triss winners get a large weight). The Swedish gradient sample also excludes individuals who received study aid in the year prior to the lottery. The representative sample gradients has been weighted to match the sex and age-distribution in the lottery samples (weighted by the identifying variation in each lottery). Panel A: The reported standard error is the maximum of standard errors which are unadjusted, heteroskedasticity-robust and clustered at the level of the player. The p-values for equal effects come from a stacked regression and are based on the maximum of standard errors which are unadjusted, heteroskedasticity-robust or clustered at the level of the player. The discrepancy in the number of observations for the rescaled lottery estimate compared to Table 3 is due to eight singleton observations being dropped from the observation count in the IV regression. Panel B: Standard errors reported are either unadjusted or heteroskedasticity-robust, whichever is largest. The p-values for equal effects come from a stacked regression and are derived from the largest of the two sets of standard errors.

Figure D5: Comparison to Log Income Gradients in the Adult Samples: Weighted & Low Income



The figure shows various rescaled lottery estimates and gradients for any crime (Sweden) and imprisonment (United States). "Base" refers to the same rescaled estimates and gradients as reported in Table D5. In "Weighted" the samples have been reweighted to match the income distribution in the representative sample. The four estimates shows the rescaled lottery estimates when the sample has been restricted to the bottom 4, 3, 2 and 1 deciles of the income distribution (defined by year, age and sex). Confidence intervals calculated as in Table D5.

Figure D6: Effects by Age/Time in the Intergenerational Samples



Panel A: The figure shows the results from model 1 with t varying from 1 to 10. The 95 percent confidence intervals are based on the maximum of the four types of standard errors discussed in Section 4. Panel B: The figure shows the results from model 4 with t varying from 0 to 10 (as we only observe lottery win year and not the exact day). The 95 percent confidence intervals are based on the maximum of the three types of standard errors discussed in Section 4.

Table D6: Robustness Tests for the Swedish Intergenerational Sample

				Type of Crime					Type of Sentence		
	Any Suspicion	Any Crime	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention	Prison
Detention	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Effect ($100K)*100	0.015	-0.067	0.289	-0.047	-0.219	-0.163	0.737	-0.516	0.038	0.085	0.030
SE	0.571	0.546	0.674	0.420	0.451	0.340	0.523	0.389	0.591	0.348	0.159
p (resampling)	0.965	0.915	0.664	0.940	0.534	0.624	0.098	0.170	0.939	0.766	0.755
p (analytical)	0.979	0.902	0.668	0.912	0.471	0.632	0.160	0.185	0.949	0.808	0.848
p equal effects	0.916										
Mean dep. var.*100	16.864	12.040	11.344	4.395	2.568	2.415	4.170	3.575	8.550	2.425	0.727
Effect/mean	0.001	-0.056	0.025	-0.011	-0.085	-0.068	0.177	-0.144	0.004	0.035	0.042
N	83,144	83,144	115,210	115,210	115,210	115,210	115,210	115,210	115,210	115,210	115,210
Sample	Suspect sample		Prize no more than $595K (4M SEK)								

Notes: This table reports results similar to Table 5, with the following differences: the dependent variable in column 1 is an indicator equal to one in in the event that a lottery player's child was suspected of a crime within six years of the lottery draw (data available from 1995 onwards); the sample in column 2 is restricted to the same sample as in column 1, and the sample in column (3)–(11) is restricted to children whose parent won $595K (4M SEK) or less. The p-value for equal effects between column 1 and 2 is based on a stacked regression (taking the largest of robust and clustered standard errors). The mean of the dependent variable is calculated by weighting the sample by the treatment variation in each lottery.

Table D7: Post Hoc Robustness Tests for the Swedish Intergenerational Sample

	Any Crime		Any Crime Except Traffic		Any Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Effect ($100K)*100	0.059	-0.147	-0.424	-0.059	0.103	-0.020
SE	0.497	0.504	0.456	0.518	0.547	0.585
p (resampling)	0.881	0.794	0.310	0.936	0.813	1.005
p (analytical)	0.906	0.770	0.352	0.905	0.868	0.970
Mean dep. var.*100	0.119	0.119	0.098	0.118	0.121	0.120
Effect/mean	0.005	-0.012	-0.043	-0.005	0.009	-0.002
N	115,306	115,306	115,306	115,025	112,041	111,760
Lotteries	All	All	All	All Except Triss-Monthly	All Except PLS Odds	All Except Triss-Monthly & PLS Odds
Covariate Vector	Yes	No	Yes	Yes	Yes	Yes

Notes: This table reports post hoc robustness tests with respect to the results in column 1 of Table 5. The results in column 1 are identical to column 1 of Table 3 and only included for comparison. Column 2 shows the results when the covariate vector (but not the vector of lottery cell fixed effects) are dropped from the regression. The dependent variable in column 3 is a dummy for having committed any non-traffic crime up to 7 years after the lottery event. Columns 4-6 shows the results when we drop Triss-Monthly, PLS Odds prizes or both from the estimation sample. Standard errors and p-values are calculated as in Table 3. The mean of the dependent variable is calculated by weighting the sample by the treatment variation in each lottery.

Table D8: Robustness Tests for the U.S. Intergenerational Sample

A. Different Prize Ranges

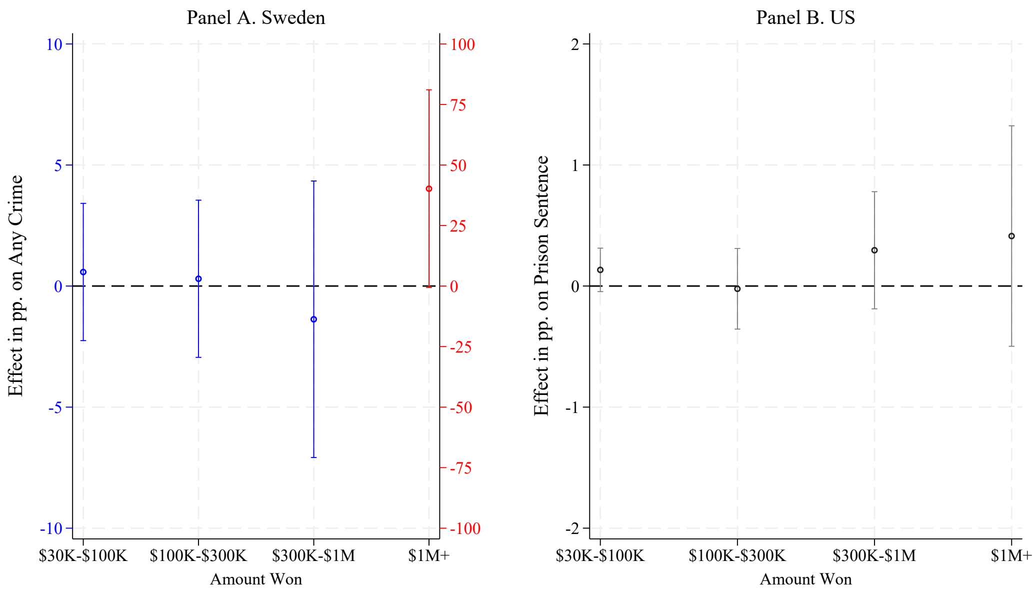
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Effect ($100K)*100	0.021	0.033	0.034	-0.019	0.061	-0.001	0.008	0.011	0.015
SE	0.040	0.044	0.045	0.049	0.063	0.034	0.031	0.028	0.020
p	0.594	0.458	0.452	0.698	0.333	0.974	0.801	0.692	0.462
N	5,145,045	2,301,533	200,808	92,189	5,149,504	5,145,851	5,146,163	5,146,361	5,146,854
Max prize	$1M	$1M	$1M	$1M	$500K	$1.5M	$2M	$2.5M	$5M
Min prize	$600	$1K	$10K	$30K	$600	$600	$600	$600	$600

B. Different Specifications

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Effect ($100K)*100	-0.029	0.027	0.040	0.038	0.026	0.020	0.013	0.084	0.001	-0.009	0.021
SE	0.025	0.040	0.046	0.041	0.040	0.045	0.046	0.083	0.038	0.029	0.023
p	0.244	0.503	0.386	0.359	0.515	0.654	0.766	0.312	0.971	0.763	0.363
N	3,398,982	5,145,045	5,145,045	5,145,045	5,145,045	4,388,584	4,388,584	5,145,045	5,734,424	5,145,045	5,145,045
Outcome										Shorter sentences	Longer sentences
Specification	Only prize size variation	Full set of controls	Reweight to random sample	Reweight each bin to random sample	Reweight to 1/No. kids per winner	Reweight to similar cohorts	Restrict to balanced cohorts	Winner fixed-effects	Including multi-year/subsequent prizes		

The columns in panel A vary the minimum or maximum win size in each regression relative to the baseline range reproduced in column 1. Panel B column 10 removes future winners from the specification, column 11 adds the full set of controls (incarceration in the prior 5 years, ever incarcerated, logarithm of after tax income, sex, marital status, whether filed tax return, college educated, and whether a U.S. citizen.), column 12 reweights the lottery sample to look like the representative sample using these same control variables, column 13 reweights each of the five prize win range categories from Figure D2 to match the representative sample using the same control variables, column 14 provides equal weighting to each lottery winning parent, column 15 reweights the control group to resemble match the treatment group in terms of cohorts, column 16 drops cohorts the 2004-2007 birth cohorts for which there are no control group children, column 17 includes winner fixed effects, column 18 adds future lottery wins of repeat winners and wins taken as an annuity, and columns 19-20 compare effects on below and above median prison sentence length. Standard errors are equal to the maximum of conventional standard errors; Huber-White standard errors; and standard errors adjusted for clustering at the level of the winning parent.

Figure D7: Post Hoc Tests of Non-linear Effects



Panel A: The figure shows the results from model 2 where amount won is replaced with four indicator variables for amount won. The excluded category are prizes below $30K. 95 percent confidence intervals based on the maximum of the four types of standard errors discussed in Section 4. Panel B: The figure shows the results from model 4 with amount won replaced by the same four indicator variables as for Panel A and with prizes below $30K as the excluded category. 95 percent confidence intervals based on the maximum of unadjusted and robust standard errors.

Table D9: Effects by Lottery in the Swedish Intergenerational Sample

	Lotteries			
	PLS	Kombi	Triss-Lumpsum	Triss-Monthly
	(5)	(6)	(7)	(8)
Effect ($100K)*100	0.311	-0.361	0.018	-0.266
SE	0.875	1.861	0.640	1.059
p	0.699	0.911	0.947	0.785
p equal effects		0.974		
N	106,494	6,571	1,960	281

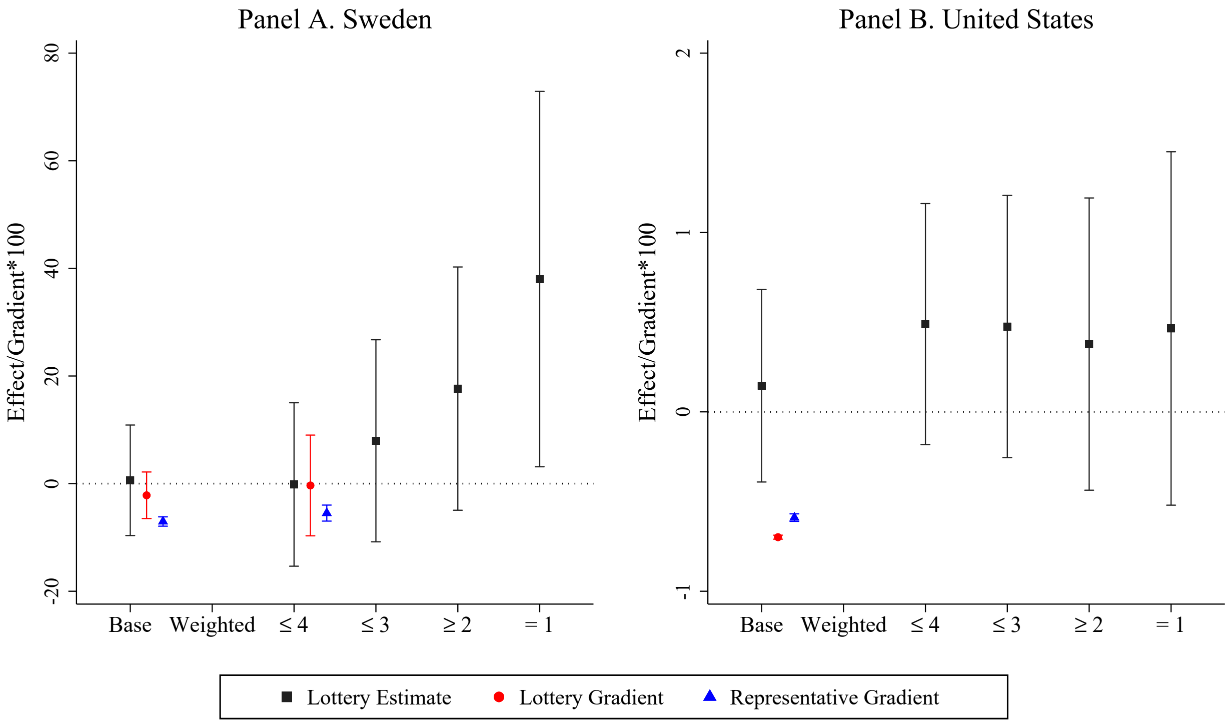
Notes: This table reports the results from post hoc heterogeneity analyses by lottery in the Swedish sample. All regressions include the same set of covariates as in model 1. Standard errors are the maximum of unadjusted, heteroskedasticity-robust and clustered at the level of the player. The p-values for both individual coefficients and for equality between coefficients are based on 10,000 permutations of the prize vector.

Table D10: Comparison to Parental Income Gradients in the Intergenerational Samples

	Sweden									U.S.
		Type of Crime					Type of Sentence			
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention	Prison	Prison
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Causal Estimate*100	0.618	-2.021	-4.054	-1.512	2.454	-3.643	-3.089	-3.052	-1.326	0.146
SE	5.240	2.911	2.098	2.418	3.269	2.699	4.098	1.966	1.110	0.274
N	115,304	115,304	115,304	115,304	115,304	115,304	115,304	115,304	115,304	5,145,045
Lottery Gradient*100	-2.169	-1.534	-1.436	-1.000	-1.623	-0.635	-2.427	-1.099	-0.698	-0.699
SE	2.221	1.268	1.261	1.417	1.669	1.075	1.956	0.898	0.425	0.006
N	95,610	95,610	95,610	95,610	95,610	95,610	95,610	95,610	95,610	5,020,536
p equal effects	0.626	0.876	0.203	0.852	0.267	0.266	0.871	0.337	0.564	0.002
Rep. Gradient*100	-7.046	-3.956	-2.567	-2.004	-3.028	-2.006	-5.659	-2.499	-1.105	-0.589
SE	0.441	0.309	0.241	0.273	0.272	0.242	0.378	0.262	0.154	0.011
N	58,221	58,221	58,221	58,221	58,221	58,221	58,221	58,221	58,221	1,439,535
p equal effects	0.147	0.510	0.369	0.843	0.095	0.511	0.494	0.761	0.825	0.007

Notes: This table presents estimates of the causal effect of lottery wealth on crime based on regressions where the log of average parental income in the five years prior to the lottery draw plus an annuity for the lottery win (assuming prizes are annuitized over 20 years) is instrumented with the lottery win. The set of controls are the same as in model 2 and model 4 (United States). The lottery sample gradients are estimated from the sample of children whose parents won less that SEK 200K (Sweden) or \$20K (U.S.) with observations weighted to match the identifying variation in each lottery (this weighting explains the larger standard errors for the Swedish lottery sample gradients as the relatively few Triss winners get a large weight). The representative sample gradients has been weighted to match the sex and age-distribution in the lottery samples (weighted by the identifying variation in each lottery). Panel A: Standard errors reported are either unadjusted heteroskedasticity-robust, or clustered at the level of the player, whichever is largest. The p-values for equal effects come from a stacked regression and are derived from the largest of the three sets of standard errors. The discrepancy in the number of observations for the rescaled lottery estimate compared to Table 5 is due to two singleton observations being dropped from the observation count in the IV regression. Panel B: Standard errors reported are either unadjusted, heteroskedasticity-robust, or clustered at the level of the winner, whichever is largest. The p-values for equal effects come from a stacked regression and are derived from the largest of the three sets of standard errors.

Figure D8: Comparison to Log Income Gradients in the Intergenerational Samples: Weighted & Low Income



Note: The figure shows various rescaled lottery estimates and gradients for any crime (Sweden) and imprisonment (United States). "Base" refers to the same rescaled estimates and gradients as reported in Table D10. In "Weighted" the samples have been reweighted to match the income distribution in the representative sample. The four estimates shows the rescaled lottery estimates when the sample has been restricted to the bottom 4, 3, 2 and 1 deciles of the income distribution (defined by year, age and sex). Confidence intervals calculated as in Table D10.

Table D11: Comparison of Implied Elasticities in Selected Studies

Study	Elasticity	Outcome	Country	Source of variation	Spending horizon	Population
Adults						
This paper	-0.865 (0.036)	Any crime	Sweden	Income (gradient)	-	Random sample
This paper	0.375 (0.295)	Any crime	Sweden	Lottery	20 years	Lottery players
This paper	-0.890 (0.010)	Prison	U.S.	Income (gradient)	-	Random sample
This paper	0.231 (0.310)	Prison	U.S.	Lottery	20 years	Lottery players
Mallar & Thornton (1978)	-0.801 (0.417)	Arrests	U.S.	Income support	1 year	Ex-convicts
Tuttle (2019)	-1.465 (0.720)	Prison	U.S.	SNAP	-	Ex-convicts
Deshpande & Mueller-Smith (2022)	-0.470 (0.243)	Any crime	U.S.	SSI benefits	21 years	SSI beneficiaries
Deshpande & Mueller-Smith (2022)	-1.299 (0.448)	Prison	U.S.	SSI benefits	21 years	SSI beneficiaries
Dustmann, Landersø & Andersen (2024)	-0.952 (0.323)	No. of convictions	Denmark	Benefits	-	Refugees
Children						
This paper	-0.588 (0.037)	Any crime	Sweden	Income (gradient)	-	Random sample
This paper	0.052 (0.439)	Any crime	Sweden	Lottery	20 years	Lottery players
This paper	-0.419 (0.008)	Prison	U.S.	Income (gradient)	-	Random sample
This paper	0.115 (0.216)	Prison	U.S.	Lottery	20 years	Lottery players
Akee et al. (2010)	-3.745 (1.862)	Minor crime	U.S.	Installments	4 years	Native Americans
Dustmann, Landersø & Andersen (2024)	-1.157 (0.649)	No. of convictions	Denmark	Benefits	-	Refugees

Notes: This table compares elasticities implied by our lottery-based estimates, the crime-income gradients in our representative samples, and a selected set of previous studies. The calculations underlying the estimates are shown in Section C.

Table D12: Lower Bounds of Implied Income Elasticities

	Full sample	Income decile ≤ 4	≤ 3	≤ 2	$= 1$	Crime risk decile ≥ 7	≥ 8	≥ 9	$= 10$
Adults									
Sweden	-0.203	-0.481	-0.255	-0.323	0.418	-0.209	-0.344	-0.698	-1.005
U.S.	-0.377	-0.160	-0.176	-0.261	-0.482	-0.384	-0.433	-0.353	-0.206
Children									
Sweden	-0.807	-1.197	-0.824	-0.393	0.194	-1.086	-1.281	-1.085	-1.618
U.S.	-0.309	-0.095	-0.126	-0.203	-0.234	-0.440	-0.437	-0.441	-0.422

Notes: This table shows the lower bound of the implied elasticities for our lottery-based estimates for the full sample as well as subsamples defined by income or crime risk.