Does Wealth Inhibit Criminal Behavior? Evidence from Swedish Lottery Winners and Their Children

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Abstract

There is a well-established negative gradient between economic status and crime, but its underlying causal mechanisms are not well understood. We use data on four Swedish lotteries matched to data on criminal convictions to gauge the causal effect of financial windfalls on player's own crime and their children's delinquency. We estimate a positive but statistically insignificant effect of lottery wealth on players' own conviction risk. Our estimates allow us to rule out effects one fifth as large as the cross-sectional gradient between income and crime. We also estimate a less precise null effect of parental lottery wealth on child delinquency.

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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). People who are relatively poor are more often convicted of criminal offenses, even in countries with relatively low levels of income inequality and extensive social safety nets. For example, Swedish men in the bottom income decile are five times more likely to be convicted for a crime over a five-year period than men in the top decile. Women commit fewer crimes, but the relative difference in crime rates across the income distribution is similar.

Social scientists have proposed a range of explanations for the observed relationship between crime and economic status. 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). Common to these theories is the notion that lack of financial resources causes crime.

Economic theory predicts poor labor market conditions increase crime for economic gain (Ehrlich 1973, Sjoquist 1973, Block & Heineke 1975), but the effect of changes in unearned income and 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 income.

In this paper, we use data from four samples of Swedish lottery players matched with data on the universe of criminal convictions to investigate how positive wealth shocks affect criminal behavior. Matching adult players to their children, we also estimate the effect of parental wealth on child delinquency. A key advantage of our data is that we observe the factors conditional on which lottery wins are randomly assigned.

In our sample of adult lottery players, we estimate a positive but statistically insignificant effect of lottery wealth on criminal behavior. The point estimate of our main outcome of interest — conviction for any type of crime within seven years of the lottery event — suggests 1 million SEK (about $150,000) increases conviction risk by 0.28 percentage points (10.2%). The 95% confidence interval allows us to reject reductions in conviction risk larger than 0.16 percentage points (5.8%). We find no clear evidence of differential effects across types of offenses.

To put our estimates in perspective, we rescale them so that they represent the causal effect of changes to log permanent income and compare them to the corresponding cross-sectional gradients. We reject effects as large as the crime-income gradient at all conventional levels of statistical significance, thus challenging theories that emphasize lack of economic resources as a key determinant of adult criminal behavior.

In our intergenerational analyses, we estimate an effect of parental financial resources on child delinquency close to zero, but non-trivial effects in either direction cannot be ruled out. The 95% confidence interval for the effect of 1 million SEK ranges from a 1.36-percentage-point reduction (12.9%) to a 1.54-percentage-point (14.6%) increase in conviction risk. Though we cannot rule out an effect of the same magnitude as the gradient between parental income and children's criminal record, our results suggest the causal effect of parental wealth in Sweden are smaller than the large protective effects Akee et al. (2010) estimate in a US sample.

Our paper contributes to a quasi-experimental literature on how economic circumstances affect crime. Previous research has shown that income support for ex-convicts (Mallar & Thornton 1978, Rossi, Berk & Lenihan 1980, Berk, Lenihan & Rossi 1980, Munyo & Rossi 2015, Tuttle 2019) and other disadvantaged groups (Andersen, Dustmann & Landersø 2019, Palmer, Phillips & Sullivan 2019, Deshpande & Mueller-Smith 2022) can reduce crime, in particular crime in pursuit of financial gain. Financially motivated crime also appears to increase toward the end of the payment cycle for government transfers, when recipients have poor liquidity (Foley 2011, Chioda, De Mello & Soares 2016, Carr & Packham 2019, Watson, Guettabi & Reimer 2019), whereas drug crime (Riddell & Riddell 2006, Dobkin & Puller 2007, Watson, Guettabi & Reimer 2019) and domestic violence (Hsu 2017) are higher at the time of payout. Among the few quasi-experimental studies on how parental financial resources affect children's crime, Akee et al. (2010) find increases in parental income decrease juvenile crime, while the evidence from housing voucher programs is mixed (Kling, Ludwig & Katz 2005, Sciandra et al. 2013, Jacob, Kapustin & Ludwig 2015) and studies using within-family variation in parental income find no effects (Sariaslan et al. 2014, Sariaslan et al. 2021).

In contrast to previous work, which has focused on disadvantaged groups, our sample is fairly representative of the overall population in terms of socio-economic characteristics and pre-lottery criminal behavior. Moreover, we study a different type of wealth shock. Whereas most of the previous literature has focused on changes in wealth or income of a more temporary nature, our estimates answer the question of whether a substantial, positive wealth shock affects criminal behavior. Our study is thus better suited for understanding the causal pathways underlying the crime-income gradient, but is less informative about the effects of redistributive programs targeting disadvantaged groups.

Before estimating the effect of lottery winnings on crime, we 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 main aim of the Plan was both to limit these degrees of freedom and to commit to analyses with high statistical power and sound statistical inference. When we began work on the Plan, we already had access to the 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.

2 Data on Crime

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. We also distinguish between two types of sentences: fines and detention, where detention indicates any kind of restriction of freedom.

3 Lottery Samples

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 but 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 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.2 for details and evidence that the deviations in the final estimation sample are minimal and completely inconsequential in terms of our substantive findings.

3.1 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 and 2 million SEK 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.

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

3.2 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.

3.3 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 to 5M SEK. 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) 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.

3.4 Estimation Samples

To construct the 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.

Table B2 in the Online Appendix shows the distribution of prizes in the adult and intergenerational samples. All lottery prizes are net of taxes and expressed in units of year-2010 SEK and comparisons to dollar amounts reflect the exchange rate by year-end 2010. Panel A shows the total prize amount in our adult sample is a little over 6 billion SEK (about $900 million). PLS and Triss-Monthly have the largest prize pools with over 2 billion SEK per lottery, yet Triss-Lumpsum is the lottery which provides most of the within-cell variation in amount won (36%). Panel B shows the total prize pool in our intergenerational sample is slightly over 1.3 billion SEK ($200 million).

3.5 Representativeness

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

Table 1 shows the share convicted in the Triss sample is similar to the representative sample, whereas the PLS and Kombi samples have lower conviction rates than the population at large. Because the two Triss lotteries contribute a large share of the overall identifying variation (see Table B2), however, the weighted pooled lottery sample is quite similar to the representative sample. Table 1 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 quite similar with respect to marital status. In Section A.3 of the Online Appendix, we further show crime rates in Sweden are in line with those in comparable countries.

A final concern is whether the effect of lottery wealth is informative about other types of shocks to wealth or permanent income. 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 their 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 standard model, winning the lottery leads to an immediate, 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). Despite playing the lottery, winners' post-win financial behavior does not indicate much appetite for risk (Briggs et al. 2021). Previous studies also estimate a positive but statistically insignificant effect on self-rated mental health (Lindqvist, Östling & Cesarini 2020); a modest reduction in consumption of prescriptions

Table 1: Representativeness

	Pooled lottery	Matched repr.	PLS	Matched repr.	Kombi	Matched repr.	Triss lotteries	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
Socio-economic characteristics								
Birth year	1950	1950	1940	1940	1945	1945	1954	1954
Female (%)	48.8	48.8	51.4	51.4	40.7	40.7	49.6	49.6
Nordic born (%)	95.0	91.9	96.8	94.4	98.1	91.9	93.7	90.8
College (%)	20.2	25.4	20.8	17.5	18.6	25.4	19.4	28.0
Married (%)	54.1	53.8	60.7	59.6	57.0	59.9	50.9	50.5
Log household disp. income	12.3	12.3	12.3	12.2	12.5	12.5	12.3	12.3

Notes: The table shows descriptive statistics for the pooled lottery sample and each of the three subsamples that it comprises. We weigh each of the three 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.

drugs related to anxiety and insomnia (Cesarini et al. 2016) and no statistically detectable effect on self-reported alcohol consumption (Östling, Cesarini & Lindqvist 2020).

4 Estimation and Inference

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 a measure of criminal activity within t years of winning the lottery. $L_{i,0}$ 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 sex, log of household disposable income, and indicator variables for marital status, completion of a college degree, and being born in a Nordic country.[1] $\mathbf{R}_{i,-1}$ 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. 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

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

where $y_{ij,s}$ is a measure of criminal activity of child j of player i. We follow each child for a maximum of s years after 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 in million SEK. $\mathbf{Z}_{j,-1}$ 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

[1] 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.

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.

The Plan also specifies the permutation-based p-values we use 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.[2] As specified in the Plan, we also report the maximum of four different analytical standard errors: unadjusted 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.

5 The Effect of Lottery Wealth on Crime

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

5.1 Adult Analyses

Table 2 shows the estimated effect of lottery wealth on crime in the adult sample. For our main outcome — an indicator for having at least one criminal conviction in the seven years after the lottery event — our point estimate suggests that a 1M-SEK windfall increases the conviction rate by 0.28 percentage points (SE = 0.22), corresponding to 10.2% of the sample crime rate. The effect is not statistically distinguishable from zero. The 95%

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

confidence interval allows us to reject that a 1M-SEK lottery windfall reduces crime risk by more than 0.16 percentage points, or 5.8%.

Columns (2) to (6) of Table 2 show the results for each of the five different crime categories. The estimated effects on crimes for economic gain, violent crime, and other types of crime are positive, while the estimated effects on drug crime and traffic crimes are negative, but none of these estimates are statistically significant. Columns (7) and (8) show the results by type of sentence. Though neither estimate is statistically significant, our estimates suggest winning the lottery increases the probability of being sentenced to pay a fine, but decreases the probability of being sentenced to some form of detention.

Table C1 in the Online Appendix shows the results from two sets of pre-specified robustness 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 replace the indicator for any type of crime with an indicator for being suspected of a crime up to $t = 7$. Because data on individuals suspected for offenses are only available from 1995, this estimation sample is different from that in Table 2. For reference, column (2) therefore reports the results for the any conviction-indicator using the same sample as in column (1). Though our results suggest lottery wealth reduces the risk of being a suspect by 0.39 percentage points per MSEK, the effect is not statistically significant (permutation-based p-value 0.251).

Second, in columns (3)–(10) of Table C1 we re-estimate the regressions from Table 2 dropping prizes exceeding 4 million SEK ($580,000). We estimate statistically significant positive 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 C1 reinforce our conclusion that wealth does not reduce the propensity to commit crime.

We now turn to two exploratory analyses. First, Figure C1 in the Online Appendix shows the evolution of the effect of lottery wealth on crime when we vary the time horizon from 1 to 10 years after the draw. The estimated effect is close to zero up to five years after the lottery, and then becomes positive (though never statistically significant). Second, we test for heterogeneous effects along four dimensions: age, sex, disposable income and any prior conviction. Table C2 in the Online Appendix shows the effect of lottery wealth 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.

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

Table 2: Adult Sample: Main Analyses

		Type of Crime					Type of Sentence	
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect (M SEK)*100	0.278	0.041	0.029	-0.066	-0.018	0.181	0.287	-0.028
SE	0.223	0.118	0.113	0.043	0.165	0.111	0.212	0.089
p (resampling)	0.243	0.691	0.711	0.397	0.951	0.122	0.206	0.861
p (analytical)	0.211	0.725	0.798	0.124	0.912	0.105	0.175	0.755
FWER p		0.911	0.911	0.857	0.951	0.458	0.362	0.861
Mean dep. var.*100	2.730	0.605	0.266	0.062	1.654	0.517	2.440	0.308
Effect/mean	0.102	0.068	0.109	-1.055	-0.011	0.349	0.118	-0.090
N	325,796	325,796	325,796	325,796	325,796	325,796	325,796	325,796

Notes: This table reports the effect of winning the lottery on players' subsequent criminal behavior. Each column reports results from a separate regression in which the dependent variable is an indicator variable equal to 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).

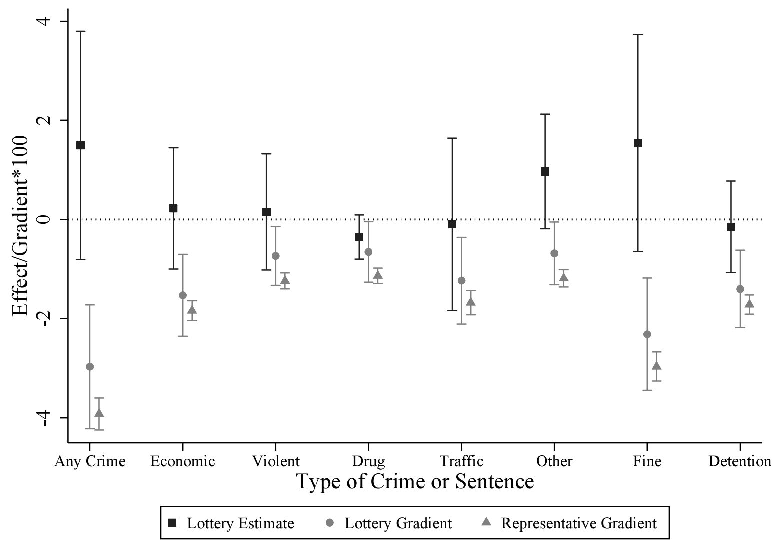
the Plan and proceed in four steps. First, we calculate, for each lottery prize, the annual payout it would sustain if it were annuitized over a 20-year period with an annual real return of 2%. For example, a 1 million SEK prize corresponds to an increase in net annual income of SEK 59,960. Second, as 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 thus 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).

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 follow the Plan and estimate gradients for lottery players who won less than SEK 200,000. Second, in a post-hoc analysis, we estimate the gradients for a representative sample weighted to match the age and gender distribution of the lottery sample. Figure 1 shows the causal, lottery-based estimates and the associated gradients (see Table C3 for the underlying estimates). The lottery-based estimate for any type of crime implies an increase in log permanent income by 1 increases conviction risk by 1.50 percentage points. The corresponding gradients are strongly negative (-2.97 and -3.93), and the null hypotheses that the gradients equal the causal effect are strongly rejected (p-values <0.001). The gradients are more negative than the causal estimates for all categories of crime, and the difference is statistically significant in two (lottery sample gradients) and four (representative sample gradient) out of five cases, respectively. Similarly, we reject the gradients for both types of sentences in both samples.

5.2 Intergenerational analyses

We now turn to our intergenerational analyses. Table 3 shows the estimated effect on our main measure of child delinquency — 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) — is close to zero: the point estimate suggests that a child's conviction risk increases by 0.09 percentage points (SE = 0.74) for each 1M-SEK won by its parent. Considering that 10.5% of children in our data are convicted at least once, the increase in relative crime risk is less than 1%. The 95% confidence interval allows us to reject that 1 million SEK in parental lottery wealth reduces crime risk by more than 1.36 percentage points (12.9%) or increases crime risk by more than 1.54 percentage points (14.6%).

Figure 1: Benchmarking (Effect of Log Income on Crime)



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 and did not receive study aid in the year prior to the lottery (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 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.

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 but statistically insignificant effects of parental lottery wealth on both fines and detention (columns (7)–(8)).

Table C4 shows the results for the same set of robustness tests as for the adult sample. The estimated effect on the risk of being a crime suspect is close to zero (0.02 percentage points per MSEK) and statistically insignificant. There is no clear pattern for how dropping prizes above 4M SEK changes the results, apart from making estimates less precise.

Table C5 reports the results from three pre-specified dimensions of heterogeneity: pre-win parental income, age at the time of the draw, and sex. In neither of these subsamples do we reject the null of no effect, nor do we reject treatment effect homogeneity across subsamples.

Table C6 compares rescaled lottery-estimates to cross-sectional gradients calculated 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. The rescaled causal effect for any type of crime (0.62) implies an increase in log parental disposable income by one increases the risk of conviction by 0.64 percentage points. Despite the stark difference compared to the gradients in the lottery sample (-2.16) and representative sample (-7.05), neither difference is statistically significant. 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 in the intergenerational sample.

Though non-trivial effects of parental wealth in either direction cannot be ruled out, our results suggest the effect of parental wealth in Sweden is smaller than the protective effects Akee et al. (2010) estimate for casino profits distributed to families in the Great Smoky Mountains Study of Youth in the US. 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 points ($SE = 8.9$). A simple rescaling of our main estimate suggests a similar wealth shock would reduce the 10-year conviction risk in our sample by 0.014 percentage points ($SE = 0.117$).[3]

[3]The estimates in Akee et al. (2010) reflect a total income supplement of about \$16,000 (4*\$4,000) in the price level of year 2000 (source: correspondence with Randall Akee). 1M SEK in year 2010-prices corresponds to about \$101,400 in year 2000, implying our estimates should be divided by 101,400/16,000 = 6.34 to be comparable to those of Akee et al. (2010). The comparison between our study and Akee et al. (2010) rests on several strong assumptions, e.g., that the effect is linear in the size of the wealth shock in both samples.

Table 3: Intergenerational Sample: Main Analyses

		Type of Crime					Type of Sentence	
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect (M SEK)*100	0.087	-0.285	-0.572	-0.213	0.346	-0.514	-0.435	-0.430
SE	0.740	0.410	0.296	0.345	0.470	0.381	0.578	0.277
p (resampling)	0.875	0.559	0.099	0.592	0.397	0.203	0.480	0.249
p (analytical)	0.906	0.487	0.053	0.538	0.462	0.177	0.451	0.121
FWER p		0.592	0.372	0.592	0.592	0.570	0.480	0.421
Mean dep. var.*100	10.543	3.943	2.017	1.538	4.110	3.384	8.450	1.766
Effect/mean	0.008	-0.072	-0.283	-0.139	0.084	-0.152	-0.052	-0.244
N	115,306	115,306	115,306	115,306	115,306	115,306	115,306	115,306

Notes: This table reports the effect of winning the lottery on the criminal behavior of the players' children. Each column reports results from a separate regression in which the dependent variable is an indicator variable equal to one in case of a conviction for a certain type of crime, or certain type of sentence, within 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).

6 Conclusions

We estimate a positive but statistically insignificant effect of lottery wealth on adults' conviction risk. Though small protective effects of wealth cannot be ruled out, we can reject causal effects one fifth as large as the cross-sectional crime-income gradient in a representative sample. The results from our intergenerational analyses are less precise but allow us to rule out large effects of parental wealth in either direction.

Although our results should not be casually extrapolated to other countries or segments of the population, Sweden is not distinguished by particularly low crime rates relative to comparable countries, and the crime rate in our sample of lottery players is only slightly lower than in the Swedish population at large. Additionally, there is a strong, negative cross-sectional relationship between crime and income, both in our sample of Swedish lottery players and in our representative sample. Our results therefore challenge the view that the relationship between crime and economic status reflects a causal effect of financial resources on adult offending.

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"Does Wealth Inhibit Criminal Behavior? Evidence from Swedish Lottery Winners"

Online Appendix

August 2023

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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 decisons 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 judgements.

A.2 Crime Data

We use the register of conviction decisions (*register över lagförda personer*) maintained and provided by the Swedish National Council for Crime Prevention (*Brottsförebyggande rådet,* or *BRÅ* for short) to measure criminal behavior. The unit of observation in this data set is a conviction, corresponding to either a court sentencing, a prosecutor imposed fine, or a waiver of prosecution. Prosecutor-imposed fines (*strafföreläggande*) are common for minor 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 occuring at the same time and place. In turn, each crime can be a violation of up to three sections of the law, including crimes against the Swedish 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) assualt, unlawful threats, defamation and sexual assault. We also include possession of illegal weapons in this category. Drug-related crimes include impaired driving, possession of illegal drugs, bootlegging and smuggling. White-collar crimes include various crimes related to tax evasion, violation of company law, benefit fraud and money laundering. Traffic crimes include, for example, impaired and reckless driving and driving without a license. Notably, many minor traffic 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

Table A1: Initial 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.

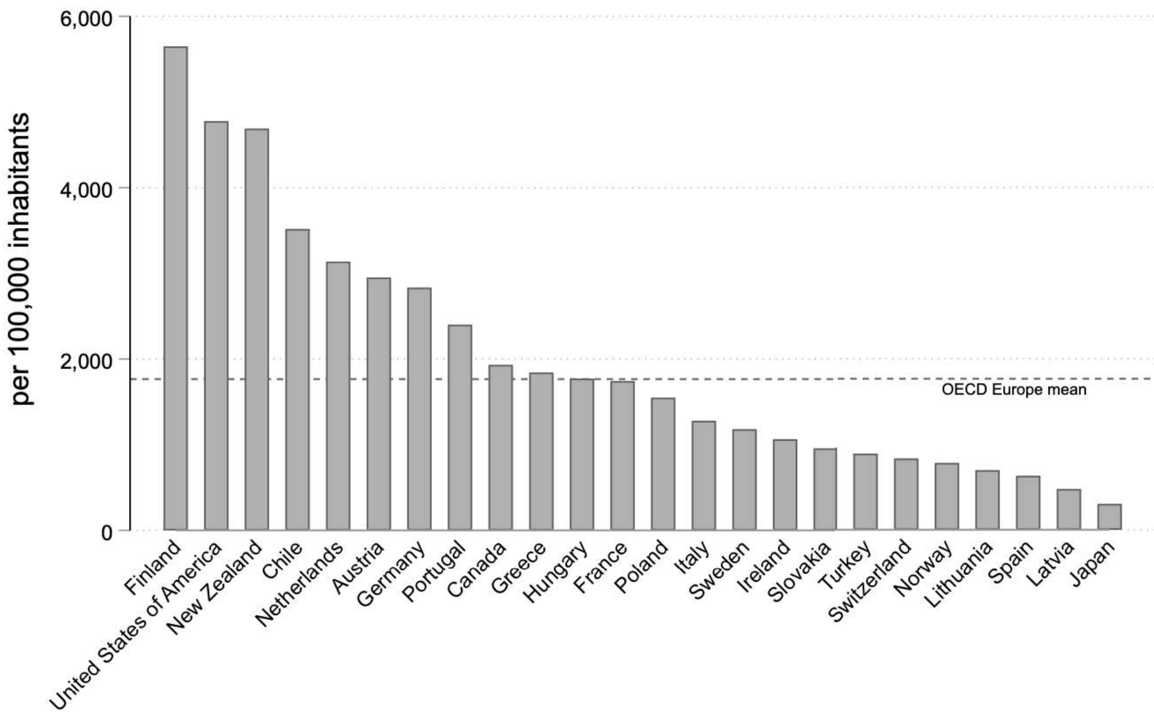
The table shows the exact coding of criminal code chapters (BRB) and the coding of the most common codes from the Swedish Code of Statutes (SFS).

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.

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



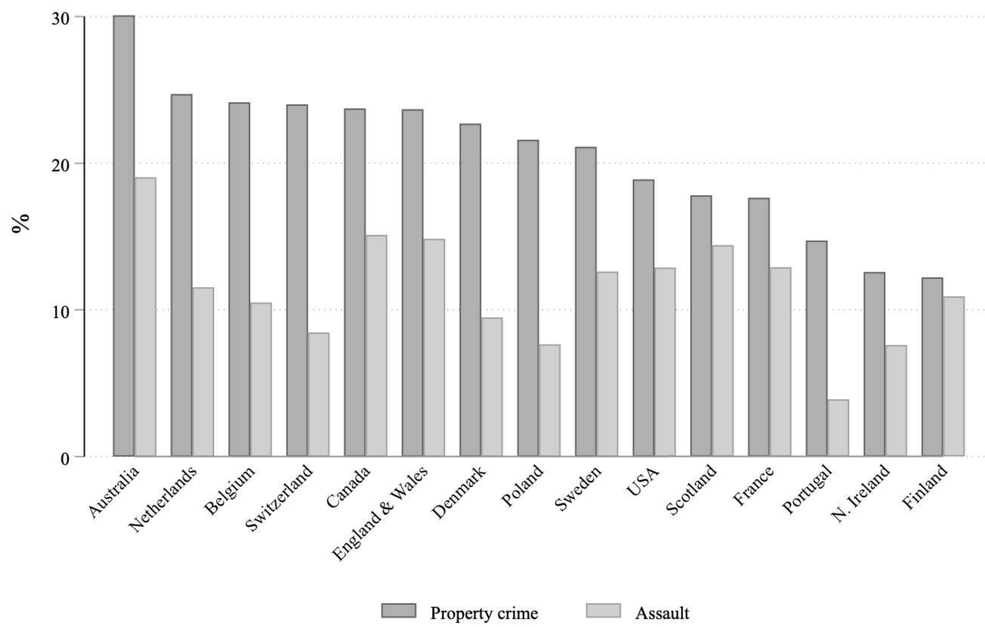
Source: United Nations Office on Drugs and Crime.

A.3 Crime in Sweden in an International Comparison

Although comparisons of criminality across boarders are difficult given differences in legal systems, enforcement, and record keeping practices, we can look to data from a number of sources to place crime in Sweden in an international context. The United Nations Office on Drugs and Crime (UNODC) collects and publishes data documenting the pervasiveness of crime across countries. Figure A1 displays the number of persons brought in formal contact 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).

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

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



Source: International Crime Victim Survey.

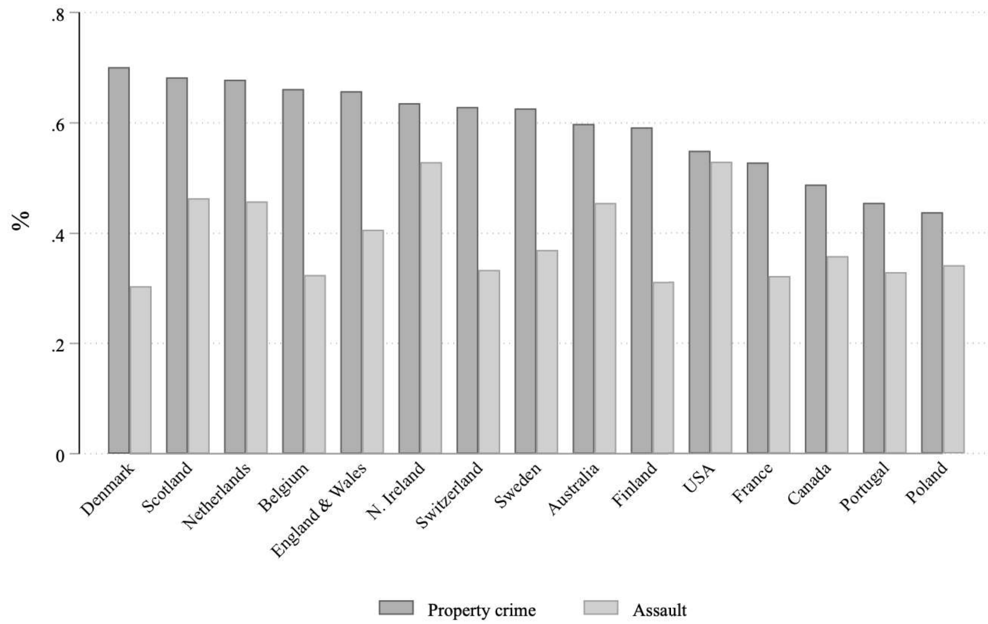
across countries directly. Figure A2 plots the percentage of households that are victims of crime between 1994 and 1999 for the sample of countries covered by the 2000 ICVS. For both property crime (9th out of 15) and assault (7th out of 15), Sweden falls roughly in the middle of the pack.

To provide a picture of the relative willingness to report crimes in Sweden, 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.

A.4 Descriptive Statistics of Crime in Sweden

This subsection documents basic patterns of crime in Sweden based on our data from the Swedish National Council for Crime Prevention. To this end, we use three representative samples of 50,000 Swedes each, drawn in 1990, 2000 and 2010 by Statistics Sweden. We

Figure A3: Share of Crimes Reported, 1994–1999



Source: International Crime Victim Survey.

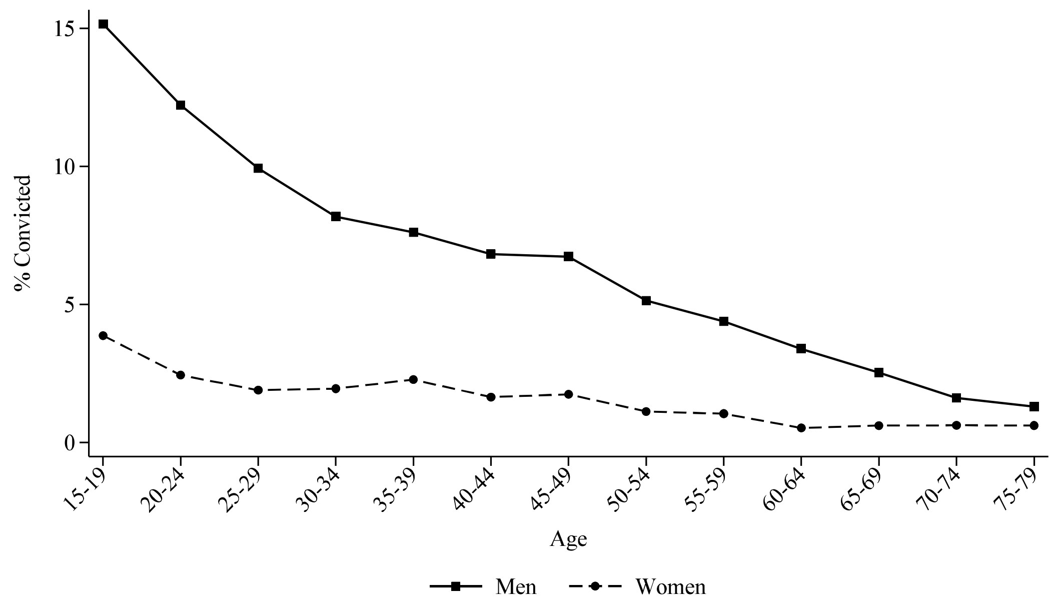
begin by showing how the fraction of the population convicted of a crime varies by sex and age For each sample, we follow all individuals ages 15–79 for five years from the year the sample was drawn. People who die or move abroad within this five-year period are coded as missing. In line with previous 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, 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

Figure A4: Criminal Activity by Age and Gender in the Representative Sample



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

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

Figure 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

Table A2: Descriptive Statistics of Convictions in a Representative Sample

A. By type of crime (% of sample)

	Men	Women
Any	7.24	1.58
Property	1.87	0.69
Violent	1.63	0.22
Drug	1.06	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)

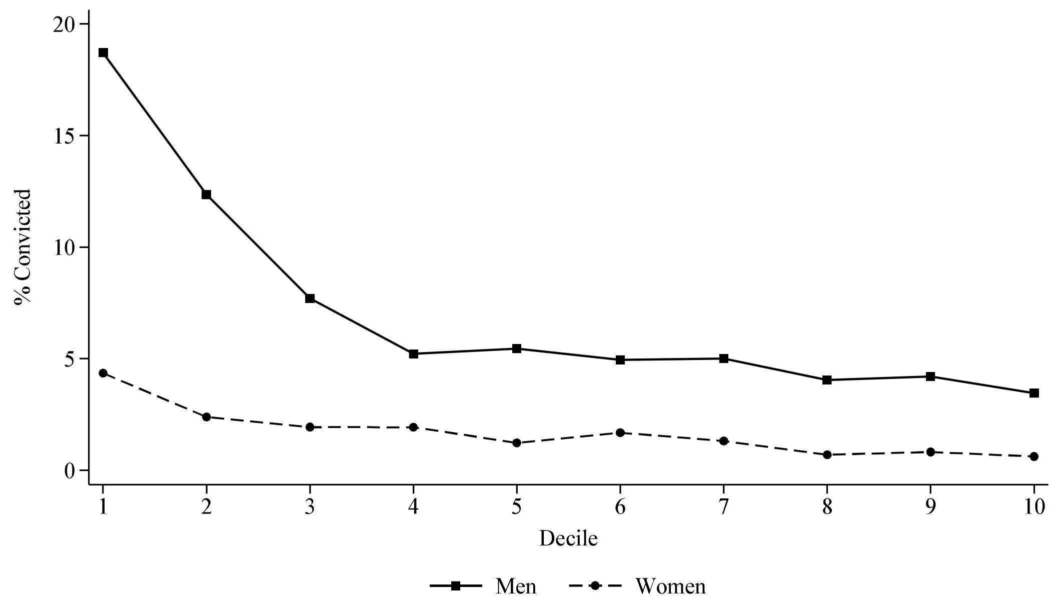
	Men	Women
Fine	5.95	1.32
Detention (including jail)	1.96	0.23
Jail	1.13	0.08

C. By perpertrator 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.

Figure A5: The 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.

of the household's, but is considerably flatter for women.[4] 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.

B Lottery Cells

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. Table B1 shows the cell construction described in Section 3 of the paper. Table B2 shows the distribution of prizes for the adult and intergenerational samples (the winning parents), respectively.

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

Table B1: Cell Construction Across 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 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:

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

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.[5] $\mathbf{R}_{i,-1}$ 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

[5] 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.

Table B2: Distribution of Prizes Awarded

	A. Winners (adult analyses)					B. Parents (intergenerational analyses)				
				Triss...					Triss...	
	All	PLS	Kombi	Lumpsum	Monthly	All	PLS	Kombi	Lumpsum	Monthly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0	37,041	0	37,041	0	0	4,272	0	4,272	0	
1K to 10K	286,828	286,828	0	0	0	58,259	58,259	0	0	
10K to 100K	23,005	21,766	0	1,239	0	5,011	4,691	0	320	
100K to 500K	4,715	2,237	0	2,478	0	1,211	500	0	711	
500K to 1M	497	279	20	198	0	101	52	0	49	
1M to 2M	1,290	638	356	54	242	245	132	44	13	56
2M to 4M	440	29	20	87	304	112	9	1	23	79
>4M	218	0	5	65	148	53	0	1	17	35
N	354,034	311,777	37,442	4,121	694	69,264	63,643	4,318	1,133	170
Sum (M SEK)	6,127	2,360	489	1,255	2,023	1,372	499	55	332	487
% of variation.	100.0	26.9	10.8	36.0	26.3	100.0	26.1	5.6	46.5	21.8

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

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

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 B3 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 B3 therefore also reports permutation-based p-values for F-statistics based on unadjusted standard errors. In this case, we reject the null of joint significance when the cell fixed effects are not included. Still, the p-values with cell fixed effects included are always above 0.05.

B.2 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

Table B3: Testing for Conditional Random Assignment of Lottery Prizes

	Adult Sample				
	All		Kombi	Triss	PLS
	(1)	(2)	(3)	(4)	(5)
p (clustered)	0.160	0.177	0.294	0.146	0.553
p (unadjusted)	0.000	0.618	0.060	0.159	0.226
N	354,034	354,034	37,442	4,815	311,777
Cell FE	No	Yes	Yes	Yes	Yes
	Intergenerational Sample				
	All		Kombi	Triss	PLS
	(6)	(7)	(8)	(9)	(10)
p (clustered)	0.665	0.073	0.145	0.147	0.912
p (unadjusted)	0.001	0.181	0.946	0.117	0.678
N	120,159	120,159	6,768	2,298	111,093
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).

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.

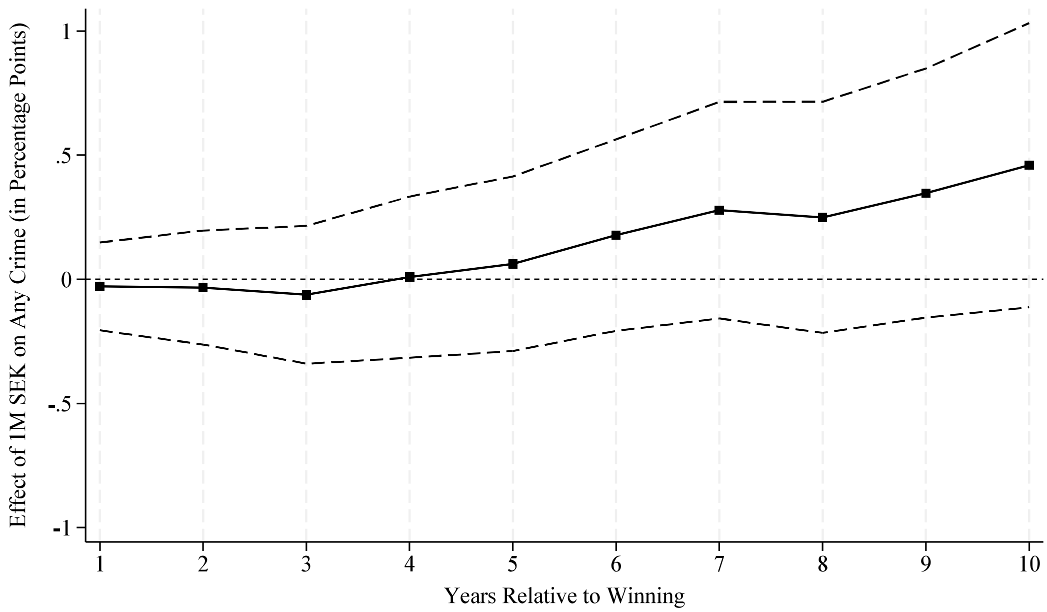
First, because a few restrictions are imposed on the sample after selecting the controls, sample sizes used in the paper differ slightly from those reported in the Plan. To be precise, there are 26 fewer observations in the adult estimation sample (354,034) compared to the Plan (354,060) in the adult 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 1 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 C1 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 Additional Results

Figure C1: Adult Sample: Effect over Time



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.

Table C1: Adult Sample: Robustness

				Type of Crime					Type of Sentence	
	Any Suspicion	Any Crime	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effect (M SEK)*100	-0.388	0.065	0.641	0.057	0.020	-0.061	0.070	0.393	0.609	0.079
SE	0.301	0.245	0.332	0.146	0.108	0.067	0.229	0.208	0.310	0.146
p (resampling)	0.251	0.791	0.046	0.681	0.813	0.516	0.734	0.012	0.049	0.524
p (analytical)	0.199	0.791	0.054	0.698	0.853	0.367	0.758	0.059	0.050	0.588
Mean dep. var.*100	2.930	2.451	2.729	0.605	0.265	0.062	1.654	0.517	2.439	0.308
Effect/mean	-0.133	0.027	0.235	0.094	0.075	-0.974	0.043	0.760	0.250	0.257
N	152,173	152,173	325,602	325,602	325,602	325,602	325,602	325,602	325,602	325,602
Sample	Suspect sample					Prize no more than 4M SEK				

Notes: This table reports results similar to Table 2, with the following differences: 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, and the sample in columns 3-10 is restricted to people who won 4M SEK or less.

Table C2: Adult Sample: Heterogeneous Effects

	Age		Sex		Disp. Income		Prior Crime	
	Below Age 50	At least Age 50	Male	Female	Below Median	Above Median	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect (MSEK)*100	0.226	0.252	0.545	0.014	0.279	0.303	0.354	-0.396
SE	0.371	0.264	0.409	0.160	0.410	0.245	0.207	0.821
p	0.584	0.624	0.217	0.994	0.491	0.541	0.106	0.760
p equal	0.961		0.265		0.963		0.534	
N	120,277	205,519	159,136	166,660	133,261	192,535	300,526	25,270

Notes: This table reports the results from four pre-registered heterogeneity analyses. Columns 1 and 2 show results separately for winners age 50 and younger at the time of the draw. Columns 3 and 4 show the results separately for male and female winners. Columns 5 and 6 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. 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.

Table C3: Adult Sample: Benchmarking (Effect of Log Income)

	Any Crime	Type of Crime: Economic Gain	Type of Crime: Violent	Type of Crime: Drug	Type of Crime: Traffic	Type of Crime: Other	Type of Sentence: Fine	Type of Sentence: Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lottery Estimate.*100	1.495	0.222	0.155	-0.353	-0.098	0.969	1.542	-0.149
SE	1.176	0.624	0.598	0.227	0.888	0.589	1.117	0.472
N	325,788	325,788	325,788	325,788	325,788	325,788	325,788	325,788
Lottery Gradient*100	-2.972	-1.531	-0.734	-0.655	-1.232	-0.684	-2.316	-1.403
SE	0.637	0.421	0.303	0.310	0.446	0.323	0.577	0.399
N	244,246	244,246	244,246	244,246	244,246	244,246	244,246	244,246
p equal effects	0.001	0.020	0.185	0.432	0.246	0.014	0.002	0.042
Rep. Gradient*100	-3.926	-1.841	-1.240	-1.139	-1.677	-1.185	-2.967	-1.718
SE	0.165	0.102	0.083	0.079	0.125	0.089	0.149	0.100
N	88,029	88,029	88,029	88,029	88,029	88,029	88,029	88,029
p equal effects	0.000	0.001	0.021	0.001	0.073	0.000	0.000	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. The lottery sample gradients are estimated from the sample of winners who won less than SEK 200K and did not receive study aid in the year prior to the lottery with observations weighted to match the identifying variation in each lottery (this weighting explains the larger standard errors for the 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). 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 2 is due to eight singleton observations being dropped from the observation count in the IV regression.

Table C4: Intergenerational Sample: Robustness

				Type of Crime					Type of Sentence	
	Any Suspicion	Any Crime	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effect (M SEK)*100	0.023	-0.099	0.430	-0.069	-0.325	-0.243	1.097	-0.768	0.056	0.126
SE	0.850	0.812	1.003	0.624	0.451	0.507	0.778	0.579	0.879	0.517
p (resampling)	0.965	0.915	0.664	0.940	0.534	0.624	0.098	0.170	0.939	0.766
p (analytical)	0.979	0.902	0.668	0.912	0.471	0.632	0.160	0.185	0.949	0.808
Mean dep. var.*100	13.584	9.913	10.545	3.944	2.019	1.537	4.112	3.386	8.454	1.767
Effect/mean	0.002	-0.010	0.041	-0.018	-0.161	-0.158	0.267	-0.227	0.007	0.071
N	83,144	83,144	115,210	115,210	115,210	115,210	115,210	115,210	115,210	115,210
Sample	Suspect sample					Prize no more than 4M SEK				

Notes: This table reports results similar to Table 3, 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)–(10) is restricted to children whose parent won 4M SEK or less.

Table C5: Intergenerational Sample: Heterogeneous Effects

	Parental Disp. Income		Age		Sex	
	Below Median	Above Median	Below Age 10	At least 10	Sons	Daughters
	(1)	(2)	(3)	(4)	(5)	(6)
Effect (MSEK)*100	0.311	0.019	-0.524	0.749	-0.817	1.031
SE	1.001	0.900	1.037	1.015	1.104	0.825
p	0.715	0.983	0.592	0.574	0.497	0.453
p equal	0.826		0.331		0.180	
N	57,698	57,608	52,085	63,221	58,648	56,658

Table C6: Intergenerational Sample: Benchmarking (Effect of Parental Log Income)

		Type of Crime					Type of Sentence	
	Any Crime	Economic Gain	Violent	Drug	Traffic	Other	Fine	Detention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Causal Estimate*100	0.618	-2.021	-4.054	-1.512	2.454	-3.643	-3.089	-3.052
SE	5.240	2.911	2.098	2.418	3.269	2.699	4.098	1.966
N	115,304	115,304	115,304	115,304	115,304	115,304	115,304	115,304
Lottery Gradient*100	-2.169	-1.534	-1.436	-1.000	-1.623	-0.635	-2.427	-1.099
SE	2.221	1.268	1.261	1.417	1.669	1.075	1.956	0.898
N	95,610	95,610	95,610	95,610	95,610	95,610	95,610	95,610
p equal effects	0.626	0.876	0.203	0.852	0.267	0.266	0.871	0.337
Rep. Gradient*100	-7.046	-3.956	-2.567	-2.004	-3.028	-2.006	-5.659	-2.499
SE	0.441	0.309	0.241	0.273	0.272	0.242	0.378	0.262
N	58,221	58,221	58,221	58,221	58,221	58,221	58,221	58,221
p equal effects	0.147	0.510	0.369	0.843	0.095	0.511	0.494	0.761

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. The lottery sample gradients are estimated from the sample of children whose parents won less that SEK 200K with observations weighted to match the identifying variation in each lottery (this weighting explains the larger standard errors for the 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). 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 3 is due to two singleton observations being dropped from the observation count in the IV regression.