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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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An OSF Pre-Analysis Plan for "Does Wealth Inhibit Criminal Behavior? Evidence from Swedish Lottery Winners and Their Children" is available at: https://osf.io/9wvdg

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).

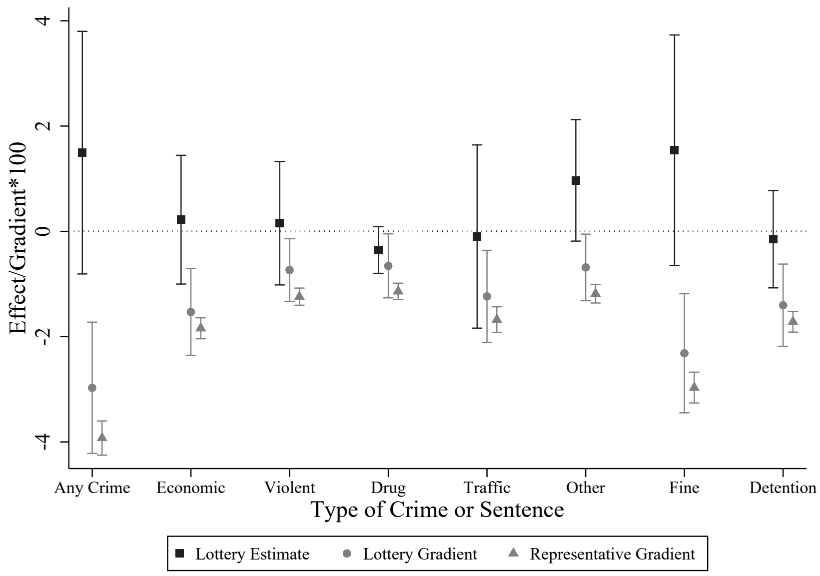
the Plan and proceed in four steps. First, we calculate, for each lottery prize, the annual payout it would sustain if it were annuitized over a 20-year period with an annual real return of 2%. For example, a 1 million SEK prize corresponds to an increase in net annual income of SEK 59,960. Second, as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