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The consequences of the Swedish rent control system on labor income: Evidence from a randomized apartment lottery ☆

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ABSTRACT

Using a unique randomized rental apartment lottery in Stockholm metropolitan statistical area, this paper analyses behavioral effects on individuals receiving a rent-controlled contract in the Swedish rent control program. The result shows that receiving a rent-controlled contract reduces the annual labor income by 13 to 20 percent and employment by 8 to 13 percent. To some extent, these effects can be explained by an increased propensity to enter higher education.
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1. Introduction

Using a unique randomized rental apartment lottery in the Stockholm metropolitan statistical area (MSA), this paper analyses the behavioral effects on individuals receiving a rent-controlled contract in the Swedish rent control program. A substantial body of economic research has highlighted the potential negative consequences of keeping rents below market rates through rent control programs in terms of mismatch (cf. Glaeser & Luttmer 2003; Bulow & Klemperer 2012; Gyourko & Linneman 1989) and increased market rents in the long run (Autor et al. 2014; Diamond et al. 2019)[1].

This paper contributes to the literature by instead focusing on the behavioral effects of rent control connected to the extent of labor force participation, not previously discussed or analyzed in the literature. To understand these behavioral effects, one needs to understand the Swedish rent control program.

The Swedish rent control system differs mainly from the rent control programs in many other parts of the world (e.g. USA, UK and the Netherlands) as it is not targeted.[2] It applies uniformly across the entire rental sector, i.e. it embraces 1.4 million rentals with almost no other eligibility requirements. Thus, all rentals are under rent control, and all individuals are eligible for a rental contract without special needs taken into account. No distinction is made between new and sitting tenants as the Swedish rent control program aims to keep rents below the long-run equilibrium level of rents for both new and sitting tenants. The Swedish program can thus, to some extent, be seen as a non-targeted housing assistance program.

To analyze the consequences of the Swedish rent control system on labor income, we take advantage of a social experiment using lotteries to distribute rental apartments. The experiment consists of lotteries of a total of 638 apartments conducted from 2003 to 2013 in the Stockholm MSA. In each of the 638 lotteries, participants received a random number, and the person with random number one in each lottery was offered the apartment. If that person turned it down, the offer went to the person who received random number two, and so forth. The data for this analysis consists of all applicants who were offered and accepted an apartment and

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[1] Diamond et al. (2019) show that landlords in San Francisco exposed to rent control reduced rental housing supplies by 15 percent by selling to owner-occupants and redeveloping buildings. This result suggests an increase in market rents in the long run. Autor et al. (2014) analyzed an elimination of stringent rent controls in Cambridge, Massachusetts. They found that rent decontrol generated substantial, robust price appreciation at decontrolled units and nearby never-controlled units.

[2] The system shares to some extent the ideas with also a general social insurance system instead of a targeted.

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those who were not offered and received at most lottery number 15 in any of the 638 lotteries. In total, we have information on 7,423 lottery applicants. The sample is matched to a population register consisting of annual information on labor income and socio-economic variables over the period 2000 to 2015.

The design of the lottery shares all the important elements of the Swedish rent control program. Any individual was allowed to participate in the lottery, including individuals on the ordinary waiting list and those who saw this as an opportunity to receive a low-rent apartment in an attractive area of Stockholm. Furthermore, individuals could partake in several lotteries. The lottery enables us to analyze the consequences of the Swedish rent control system when there is a housing shortage. According to the Swedish National Board of Housing, the rents in Stockholm MSA would increase by approximately 68 percent if they instead were set in accordance with the market value (Sjöberg & Söderberg 2013).

Given the large subsidy, receiving a rent-controlled contract could be seen as an increase in income, which would reduce the labor supply for individuals receiving such a contract. However, winning the lottery could also impact education, family formation, and fertility (Enström Öst 2012), indirectly affecting labor supply as subsidized housing could compensate for labor income loss due to education or fertility. In this paper, we estimate the effects on labor supply, labor earnings, and education of complying with the offer of a rental contract each year up to six years after partaking in the lottery.

We find a consistent pattern of reduced yearly labor incomes when complying with the rental apartment offer. The effects are statistically significant at the one and five percent levels in year three to five after participating in the lottery. Receiving a rental apartment in a market with excess demand reduces the annual labor income after accepting the apartment with around 16,000 SEK (€ 1,700) each year, which implies a decrease in income of between 13 and 20 percent. We also see a reduction in the fraction employed of between 8 and 13 percent for the same years as the effects on labor income. Further analyses reveal that the effects can partly be explained by a short-term withdrawal from the labor market caused by an increased propensity to enter higher education due to the subsidized rent.

This paper adds new results on the behavioral effects of rent control programs. In most other parts of the world, only sitting tenants are included in the rent control programs. In this case, households would be less inclined to move, which could negatively affect labor supply. Diamond et al. (2019) found that rent control decreased renters' mobility by 20 percent, lowering the displacement from San Francisco. Similar results are observed in Navarro (1985), Hardman and Ioannides (1999), Munch and Svarer (2002), Glaeser and Luttmer (2003) and Krol and Svorny (2005). Van Dijk (2018) is the study most closely related to ours. She analyses a housing lottery in the Netherlands. The average effect on those receiving public housing is found to be negative and substantial in size on employment and individual and household earnings, while positive for assistance receipt.

The paper also adds to the body of empirical research on the effects of housing assistance on labor market outcomes. The experimental evidence on the effects of housing assistance programs on labor market outcomes finds similar results to ours, as using housing vouchers has been found to reduce labor force participation (cf. Rice & Sard 2006; Jacob & Ludwig 2012). In situations with assistance provided to targeted populations, i.e. families already living in public housing (Kling et al., 2004) or low-income families or families on welfare (Mills et al., 2006), the results indicate that vouchers reduce employment rates and earning amounts in the first year or two after random assignment.

The remainder of the paper is structured as follows. Section 2 describes the Swedish rental market and rent control system, and Section 3 describes the apartment lottery and discusses our data. Section 4 sets the empirical strategy, and the results are presented in section 5. Section 6 concludes with a discussion.

2. The Swedish rental market

More than three of ten million inhabitants in Sweden live in rental housing. The Swedish rental market is characterized by a form of rent control which is applied to all rental apartments on the housing market. The implication is that rents remain lower than the market rate, especially in attractive areas close to the city center. Therefore, the distribution of rental apartments is not based on tenants' willingness to pay for a specific apartment but instead on their position in a housing agency queue where they can sign on to a waiting list for a future apartment. The individual who is first on the waiting list will receive an offer when an apartment is available for rent. The individual can refuse the offer and remain on the waiting list to get a better offer. Generally, there is no cost, or a very low cost, to register on these lists. For attractive areas closer to the city center, the waiting time to get an apartment can be very long, up to 20 years,[3] especially in the center of Stockholm and areas close to the city center, like the one for this study. The waiting list system, thus, benefits older people and natives, especially in the more attractive areas. Over 700,000 people are currently registered in the housing queue in Stockholm, and the housing agency distributes approximately 10,000 rental apartments per year.

The Swedish rent control system is applied uniformly across the entire rental sector with no other eligibility requirements except for the requirement to have an income. Therefore, all rentals are under rent control, and all individuals are eligible for rental contracts without accounting for special needs, aiming for excellent and homogenous quality of housing regardless of the family situation. As a result, all individuals and families are eligible for rental contracts independent of family size and apartment size demanded.

The rents are based on annual collective bargaining at the municipality level between the Swedish Tenants Union, the municipal housing company, and the representatives of private property owners determining reference rents (Ellingsen & Englund 2003). The bargaining makes use of the so-called apartment utility value. The utility value is based on how tenants value various apartment attributes, e.g. balcony, elevator, and so on. Ordinary maintenance, i.e. measures that restore the original quality of the apartment, is included in the rent, and the rent is therefore not allowed to increase after ordinary maintenance. As the rental stock in Sweden is very homogenous regarding housing attributes, this implies a relatively small variation in per square meter rent. In theory, the main difference in utility values should concern the distance to the city center. However, in practice, the rents in the inner city do not differ much from those observed in the surrounding areas (cf. Andersson & Söderberg 2012). The average rent increase has been 1.8 percent per year over ten years.[4]

Sjöberg and Söderberg (2013) estimated that the rent levels in Stockholm, the housing market for this study, would increase by 68 percent if rents were set according to market value. Furthermore, there is a legal possibility that the tenant who holds a rental contract can get permission from the property owner to sublet the apartment for some time, a so-called second-hand rental. The rent for such a contract is allowed to be 10–15 percent higher if the apartment is furnished. However, a black market also exists where

[3] Source: The Stockholm Housing Agency https://bostad.stockholm.se/statistik/statistiktjansten/, 2019–12-18.

[4] Source: https://www.scb.se/hitta-statistik/statistik-efter-amne/boende-byggande-och-bebyggelse/bostads-och-hyresuppgifter/hyror-i-bostadslagenheter/pong/statistiknyhet/hyror-i-bostadslagenheter/, 2022–04-22.

the rents can be much higher (cf. Andersson & Söderberg 2012), and a flourishing market for these contracts exists in the Stockholm MSA. Furthermore, it is also possible to use a rental apartment in an exchange between two or three renters. For example, if a couple has two small apartments and they form a family, they can use these two apartments to find a larger apartment from a couple that just got divorced. In some instances, a rental apartment can also be part of the payment when buying an apartment or house. The idea is to reduce the house or apartment price as the seller of the house or apartment receives the contract for the rental apartment. For this reason, a rental apartment with regulated rent in Sweden may constitute a substantial capital asset. Another implication is that Landlords may sell to owner-occupants and redevelop buildings. The top listing was in 2008 when almost 18,000 rentals were sold to owner-occupants.[5,6]

For working individuals, the rent control is likely to reduce the labor supply as the subsidized rent and potential capital gain is a lump sum. There is a possibility that unemployed individuals could find jobs due to being offered subsidized apartments, with a resulting increase in labor supply. However, since there is an income requirement to obtain a rental contract in the waiting list system and the lottery, this potential effect cannot be studied with our data.

3. The lottery

Between 2003 and 2013, a municipal housing company in Solna, a municipality close to the Stockholm city center, decided to randomly distribute some of their apartments instead of using the waiting list. From the housing company's perspective, the lottery aimed to increase the heterogeneity of the tenants in the residential area. The long waiting time (about 10–14 years on average[7]) for an apartment was perceived to exclude certain groups from the opportunity to receive a home, for example, young adults and immigrants. However, individuals on the waiting list and those who already had an apartment were eligible to participate. The applicant needed to have a steady income and a gross income of at least three annual rents to be accepted as a tenant. In total, 638 rental apartments were distributed randomly through a lottery during a ten-year period, i.e., on average, 60 apartments each year.

Each apartment constituted a separate lottery. The apartments auctioned were advertised on the housing company's webpage on a Monday. On Sunday, seven days later, all applicants were awarded a random number through a computer program, and the applicant receiving random number one was first offered the apartment. If that individual turned down the offer, the apartment went to the individual with the random number two, and so on, until someone accepted the apartment.

3.1. Sample and summary statistics of lottery

The sample for this analysis consists of all individuals who were offered a rental apartment through a lottery and accepted the apartment, and those who were not offered an apartment and at most received the random lottery number 10 to 15 in any lottery. In total, we have information on 7,423 lottery applicants. Of those, 4,235 individuals are identified in our data once. The remaining 3,188 individuals are identified as having a lottery number between one and at most lottery number 15 at least two times. Fig. 1 shows the distribution of the number of winners and non-winner over the years in the 638 lotteries. Only seven apartments were offered through the lottery in the first year, but there was a gradual increase in apartment offers until 2011 when 116 apartments were offered. In 2013 only 14 apartments were offered.

We look at the individuals' outcomes at the latest of 2015, i.e., two years after the last lottery. The implication is that we cannot identify all the applicants in the data for the same length of time. For instance, those who participated in the lottery in 2003 can be followed for at most twelve years, while those that participated in 2013 can only be followed for two years. To reduce the problem that potential observed differences in effects over time stem from population changes, we restrict the follow-up period to six years. Also, since we are interested in the effect on labor supply, and as the public pension starts from the age of 61, we at most follow the participating individuals until they reach the age of 59. These two conditions mean that about half of the individuals can be followed throughout the analysis period.

As a consequence of the individual's right to abstain and receive an apartment through the waiting list, we cannot generally estimate the average treatment effect on labor supply and education from subsidized housing. Instead, we use randomization to estimate the effect for those complying with the offer. It is possible to use rankings (i.e., the local effect of having number one instead of two, two instead of three, etc.) to estimate a weighted average of each of these local average treatment effects (Imbens & Angrist 1994). However, as it is hard to interpret this estimand, we instead focus on the local average treatment effect from having a low lottery number against a higher number.

The distribution of lottery numbers for those who accepted the apartment is shown in Fig. 2. As a threshold, we chose the median lottery number for accepting the offer. Individuals with a lottery number of 1–3 are classified as low, and the remaining are classified as having a high lottery number. As the threshold is arbitrary, we provide a sensitivity analysis through the weighted local average treatment effect (WLATE) in Appendix A. The results from this WLATE analysis are qualitatively the same as the results from our main analysis presented in section 5.1. The results are also qualitatively similar to the results using thresholds one and two (the results from these analyses are shown in Appendix B).

3.2. Data and outcome variables

The data from the lottery are linked to a Swedish population register administrated by Statistics Sweden. These data reflect a set of annual information regarding socio-economic variables, i.e. age, gender, level of education, municipality of residence and labor income, for the period 2001–2015. We also have an estimate of the number of children younger than 18 years old living at the address the year before the lottery.[8] About 90 percent (6,652) of the sample's lottery applicants lived in the Stockholm municipality at the time of the lottery.

Labor income is the yearly income from work measured in thousands of SEK and adjusted with the consumer price index to the price level of 2017.

As no employment indicator is available in the register, we define employment using the labor income data. We use previous definitions for defining retirement in the Swedish context to this extent. Johansson et al. (2015, 2017) defined a person as retired

[5] Source: The Swedish National Board of Housing https://www.boverket.se/sv/samhallsplanering/bostadsmarknad/bostadsforsorjning/kommunernas-verktyg/allmannyttan/bestand/, 2022–04-20.

[6] Similar pattern are seen in the U.S. Diamond et al. (2019) found that landlords treated by rent control in San Francisco reduced their rental housing supply with 15 percent by selling to owner occupants and redeveloping buildings.

[7] Source: The Stockholm Housing Agency, 2019–12-18.

[8] The estimated number of children is based on information from the population registration address. This approach implies, for example, that a divorced parent with children registered at the other parent's address is presumed to have no children. If they live with another parent living with minors, these minors are seen as their minors.

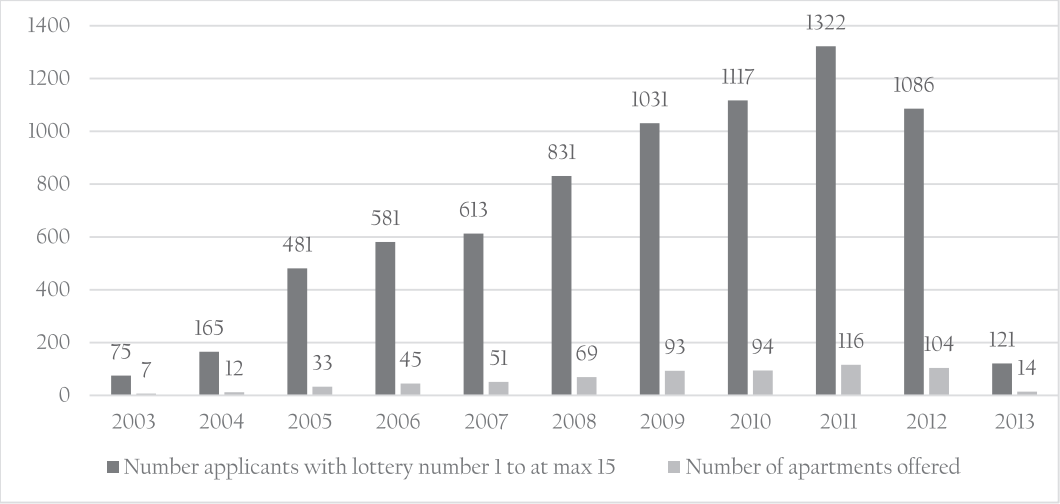

Fig. 1. The distribution of lottery applicants in the data and the number of offered apartments 2003–2013.

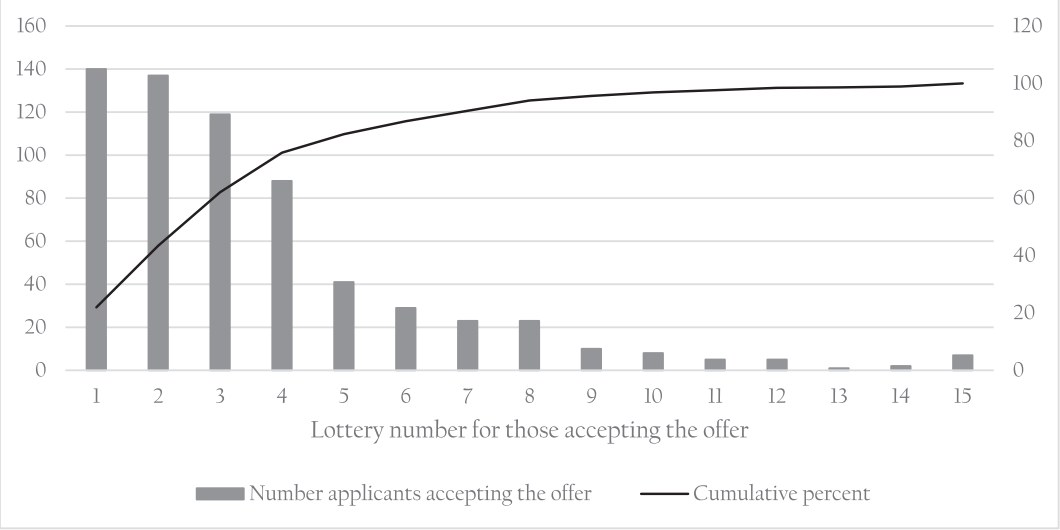

Fig. 2. The distribution of lottery numbers for those accepting the offer.

at year *t* if he or she has no income above one price base amount (PBA) at least in the two years after year *t*. The PBA tracks inflation and was 44,800 SEK (about €4,700) in 2017. The Swedish PBA is regulated by law and used for various types of benefits, such as insurance payments. We set the threshold at a half PBA, which is about 80 percent of the mean monthly wage for an individual to be defined as employed for a given year.

The level of education is measured according to the Swedish education nomenclature. It measures the highest completed education, consisting of six levels. According to this nomenclature, receiving a higher education degree is defined as an increase in the educational level.

Fig. 3 shows the number of observations available for each year of analysis. Everyone is observed for at least two years, and about 50 percent of the lottery applicants may be followed up to six years after the lottery. Thus after three years of the lottery, about 1000 lottery applicants are censored each year.

Table 1 provides descriptive statistics on covariates, measured one year before the partakers took part in the lottery for the population observed for the whole evaluation period (i.e. 3,372 individuals partaking in 2003–2009) and the 1,207 individuals that we observed for two or three years (partaking in 2011 and 2012). As shown, those observed for only three years are younger, have lower income, are less likely to be women, and have fewer children. The implication is that it is imperative to control for the year when the lottery takes place and the lottery.

Table 2 presents descriptive statistics for the two groups, "Low" and "High," together with a statistical test for differences in means between the two groups (p-value). From the first row in the table, we can see that the "Low" group has a substantially higher probability of receiving a rental apartment than those in the "High" group. Furthermore, we can see that the "Low" group is younger, less likely to be a woman, and has a lower income than the high group. These differences most likely stem from compositional changes in the population partaking in the lottery over the years exhibited in Table 1. In addition, the likelihood of winning an apartment is higher in the beginning, as seen in Fig. 1.

The third column in Table 2 displays the p-values for the equality of means between the "High" and "Low" groups when controlling for the lottery fixed effect. When comparing the within lottery

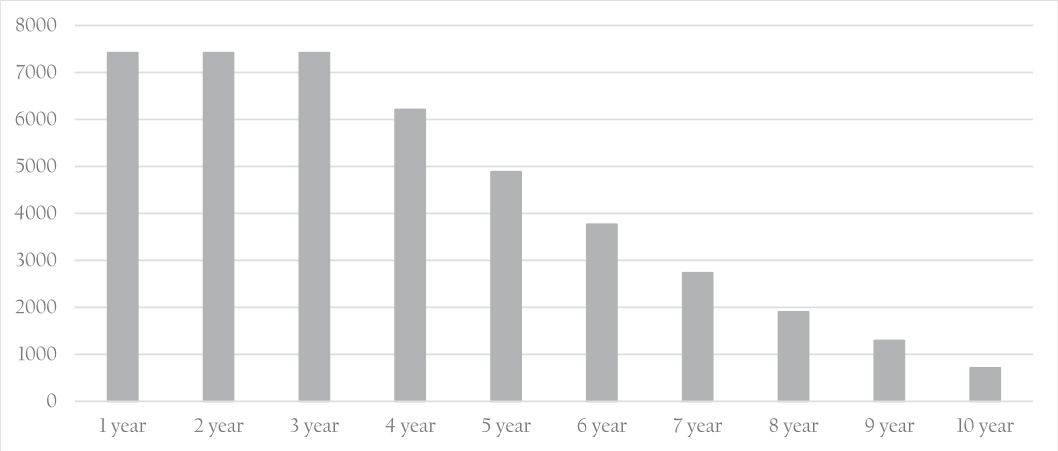

Fig. 3. The distribution of the number of lottery applicants available for each year in the analysis.

Table 1
Descriptive statistics of the sample observed at maximum 2–3 years and 6 years. Covariates measured the year before participating in the lottery.

	2–3 year	6 year	t-value
Percent receiving an apartment	9.86	8.31	
Age (years)	25.16	30.79	−24.69
	(6.01)	(9.08)	
Women = 1	0.45	0.51	−3.41
Born in Sweden = 1	0.65	0.66	−0.20
Number of children	0.24	0.33	−4.31
	(0.59)	(0.70)	
Income from work, (Yearly in 1000 SEK and in 2017 years price level)	160.73	180.01	−4.95
	(116.78)	(120.44)	
Highest education			
Pre-school education = 1	0.11	0.12	−0.48
Secondary education = 1	0.42	0.42	−0.34
Higher education = 1	0.47	0.46	0.64
Number of individuals	1,207	3,732	

Note: Standard deviation within parenthesis.

Table 2
Descriptive statistics of the sample by treatment status (Low (1–3) and High (4–15) lottery number). Covariates measured the year before participating in the lottery.

	Low	High	p-value
Percent receiving an apartment	48.71	3.66	
Age (years)	27.79	29.42	0.14
	(8.15)	(8.96)	
Women = 1	0.46	0.50	0.01
Born in Sweden = 1	0.64	0.65	0.33
Income from work, (Yearly in 1000 SEK and in 2017 years price level)	166.85	183.40	0.22
	(113.23)	(129.45)	
Number of children	0.28	0.30	0.83
	(0.02)	(0.01)	
Highest education			
Pre-school education = 1	0.13	0.11	0.29
Secondary education = 1	0.43	0.42	0.89
Higher education = 1	0.45	0.47	0.40
Number of individuals	813	6,610	

Note: Standard deviation within parenthesis. The p-values are estimated with fixed lottery effects.

balance, we find no statistically significant differences in these covariates except that the fraction of women is lower and statistically significant for the "Low" group. However, as an overall F-test (F-value = 0.57) does not reject that all variables are simultaneously equal between the two groups, we do not take this as a sign that the lottery was flawed.

4. Empirical strategy

Let Y_{ijtv} denote one of the outcomes observed in calendar year v for an individual i partaking in lottery j, measured t years after the lottery and let SA_{ij} be one if an individual accepts the subsidized apartment and zero otherwise.

The following model is used in the analysis for each one of the three outcomes

$$Y_{ijtv} = \gamma_t + \omega_j + \theta_v + \beta_t SA_i + \epsilon_{ijtv}. \quad (1)$$

Here ω_j, γ_t and θ_v respectively signifies fixed effects for the lottery, the number of years after partaking in the lottery, and the calendar year of the lottery, ϵ_{ijtv} are error terms and β_t $t = 1, \cdots, 6$ is the effect for the six years after partaking in the lottery. Let Low_i take value one if the individual has a random number less than four and zero otherwise. We estimate (1) using a two-stage least squares (2SLS) estimator where Low_i is interacted with the year after partaking in the lottery as an instrument. As individuals partake in several lotteries, we treat the error terms of these individuals as potentially correlated and use clustering robust standard errors in the inference.

The effect of having a low lottery number, in contrast to a high lottery number, and the "2SLS estimand" is defined as the average treatment effect for the sub-population of compliers (Angrist, Imbens & Rubin, 1996), or what is known as the local average treatment effect (LATE). To identify LATE, in addition to the exclusion restriction of the instruments and their relevance, we assume that an individual will not reject the offer if he/she has a high number, given that he/she will not reject with a low number and vice versa. This so-called no-defiers assumption is highly likely to be valid in this lottery. We also assume there here are no "never-takers" in the population. In other words, no person who would always reject an offer is partaking in the experiment. Given the lottery data, this is an innocuous assumption, as it is unlikely that anyone not intending ever to accept an apartment would partake in the experiment. We saw that around 80 percent of those having the highest lottery number denied the offer. The most likely reason for the rejection is that the apartment was unsuitable for their needs, so it was more rational to partake in another lottery. With these two assumptions, the population consists of "always-takers" and "compliers". The always-takers are those desperate to find an apartment and take whatever is offered. In contrast, the compliers are the ones that, to some degree, are optimizing.

Table 3
Estimates (labor income 1,000 SEK/year) from the reduced form (OLS) and two-stage least squares (2SLS), as well as the first step (OLS, in percent) for the six-year follow-up period.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. No control variables						
1. ITT	−2.86	−2.90	−15.46**	21.32***	−18.99**	−1.45
	(6.41)	(5.47)	(6.90)	(6.89)	(8.78)	(10.08)
2. LATE	−5.20	−5.27	−33.35**	−45.75***	−39.66**	−1.96
	(13.74)	(11.64)	(15.00)	(14.89)	(18.49)	(21.12)
F-value first step	200.18	200.17	194.13	163.74	135.01	101.57
B. With control variables						
1. ITT	−1.71	−2.06	−14.67**	−20.30***	−17.53**	0.13
	(6.25)	(5.30)	(6.77)	(6.77)	(8.63)	(9.74)
2. LATE	−2.79	−3.55	−31.72**	−43.65***	−36.65**	1.27
	(13.43)	(11.30)	(14.72)	(14.63)	(18.21)	(20.44)
F-value first step	197.22	197.09	191.04	160.97	133.20	99.71
Mean laborIncome (tSEK)	242.98	254.42	235.26	227.43	232.22	227.72
No. of observations	7,423	7,422	7,300	6,214	4,887	3,769

Notes: Total number of observations and clusters are 66,245 and 5,469, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalues are 3,663.99 (panel A) and 3,663.67 (panel B), respectively. ***/**/* p < 0.01/0.05/0.10.

5. Results

Section 5.1 presents the results of the labor supply analysis. Section 5.2 presents the results from the analysis of the effects on education, and section 5.3 examines who is a complier.

5.1. Labor supply

We first study the direct effects on labor income; then we analyse the effect on the extensive margin by estimating the effect on employment.

5.1.1. Labor income

Table 3 presents the results from the analysis up to six years after taking part in the lottery (columns one to six). The top panel (panel A) presents the results without any control variables, and the bottom panel (panel B) presents the results when we include the covariates. In both panels, row one presents the intent to treat (ITT), or reduced form, effect and row two presents the LATE.

The results in the table clearly show that the instruments are highly relevant (i.e., large F-statistics in all columns and the minimum eigenvalues are 3,663.99 (panel A) and 3,663.67 (panel B)). The minimum eigenvalue statistic is the F statistics for the joint significance of instruments in the first-stage regression (Stock & Yogo, 2005, p. 84).

The results in panels A and B are very similar. As precision improves when adding controls, the following discussion of point estimates refers to panel B. The ITT effect is negative for all years after the lottery except for the last year when the estimate is marginally positive. The effect is statistically significant three to five years after the lottery. The decrease in labor income is substantial in these years: a low lottery number reduces the labor income by around 16,000 SEK (€ 1,700) each year. The LATE is also statistically significant for these years. For the compliers, the decrease in annual labor income in these years is in the range of 31,720 to 45,750 SEK (€3,300 to €4,800). Under the assumption that compliers have a mean income of 235,260 and 227,430 SEK for these years, these effects amount to 13.5 percent and 20 percent reduction in labor income.[9] Thus, a substantial reduction in labor income.

The increasing effect from the first two years makes sense as it takes some time to adjust the labor supply. There is also a formal restriction as the housing company sets regular income requirements at least three months before the move-in date. The tapering off is potentially a consequence of those individuals who do not get an apartment through the lottery, receiving a rented apartment through the regular housing queue, or receiving an apartment when they participate in a new housing lottery. About half of the individuals who did not receive an apartment in the lottery moved to another rental apartment. The majority of these individuals moved within two years after the lottery, which confirms the speculation to some extent.

We repeated the analysis for a balanced sub-sample during the four years after the lottery. The result is qualitatively the same as for the full sample and is presented in Appendix C.

5.1.2. Employment

Table 4 presents the results from the employment analysis. As the results are qualitatively the same with and without covariates, we only present the results when covariates are included in the model. From this table, we can see a reduction in the fraction employed for all years as a consequence of a low lottery number. The reduction is substantial and statistically significant for the same years as for the effects on labor income. Three to five years after the lottery, the percentage points reduction for the compliers are 7.63 (16.84 percent), 13.07 (29.38 percent) and 12.25 (27.02 percent), respectively. The result for the balanced sub-sample during the four years after the lottery is qualitatively the same as for the full sample (see Appendix C).

5.1.3. Effects for those without own housing

Table 5 presents the results from the same analysis as presented in Tables 3 and 4, restricted to the sample of individuals younger than 29 without a rental contract or property ownership at the time of the lottery.[10] Thus, the analysis is of a group for whom the subsidy was initially intended. That is, a subsidy to enable individuals to find affordable housing in the absence of housing. As the results are qualitatively the same with and without covariates, we

[9] The result in section 5.3, where the compliers are only marginally less likely to have had an income below the average income, provides a justification for the assumption.

[10] We have no information on previous rental contracts or property ownership. We create the sample by using individuals younger than 29 and registered as living in the same address of, at least, one of the parents. This procedure of defining the sample should be absent of measurement errors as property ownership and having a rental contract requires that individuals are registered as living at this address.

Table 4
Estimates (employment in %) from the reduced form (OLS) and two-stage least squares (2SLS) (OLS, in percent) for the six-year follow-up period.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
1. ITT	−1.74	−1.31	−3.58**	−6.11***	−5.87**	−1.89
	(1.73)	(1.81)	(1.76)	(1.88)	(2.29)	(2.55)
2. LATE	−3.51	−2.54	−7.63**	−13.07***	−12.25**	−3.69
	(3.72)	(3.88)	(3.82)	(4.07)	(4.84)	(5.33)
F-value first step	197.22	197.09	191.04	160.97	133.20	99.71
Fraction employed (%)	42.92	47.76	45.30	44.48	45.34	44.52
No. of observations	7,423	7,422	7,300	6,214	4,887	3,769

Notes: Total number of observations and clusters are 66,245 and 5,469, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalue is 3,663.67.***/**/* p < 0.01/0.05/0.10.

Table 5
Estimates (labor income 1,000 SEK/year and employment (%)) for a sub-sample (individuals age < 29 with no rental contract before the lottery) from the reduced form (OLS) and two-stage least squares (2SLS) for the six-year follow-up period.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. Income						
1. ITT	−2.38	−1.48	−11.03	−27.60***	−9.53	−32.37**
	(6.70)	(7.16)	(8.40)	(10.27)	(14.57)	(15.63)
2. LATE	−4.28	−23.20	−23.33	−55.18***	−16.93	−59.73**
	(14.06)	(15.18)	(18.08)	(20.78)	(26.48)	(28.92)
B. Employment						
3. ITT	−3.20	−1.28	−2.78	−8.68***	−5.49	−10.94***
	(2.50)	(2.61)	(2.60)	(2.73)	(3.84)	(4.19)
4. LATE	−7.44	−2.80	−3.35	−12.14**	−19.38***	−26.80***
	(5.17)	(5.26)	(4.98)	(4.68)	(6.00)	(7.06)
F-value first step	72.82	69.99	68.12	63.32	54.43	42.85
Mean labor income (tSEK)	212.60	225.66	193.96	183.34	219.86	213.80
Fraction employed (%)	34.95	40.17	36.49	35.54	42.54	40.96
No. of observations	2,858	2,860	2,795	2,164	1,467	1,145

Notes: Total number of observations and clusters are 24,029 and 2,145, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalue is 1,388.47. ***/**/* p < 0.01/0.05/0.10.

only present the results when covariates are included in the model. Panel A presents the results on labor income, and Panel B the results on employment.

As in the main analysis, we find negative estimates on both income and employment throughout. The magnitude of the point estimates for both outcomes is generally larger in absolute value than those for the full sample. The estimates for year three are, however, no longer statistically significant. However, the reason for this is the inflated standard errors due to the smaller sample size. The main difference from the main analysis is the large, negative and statistically significant effect in the last year for both outcomes −59,730 SEK (€ 6,200), or 28 percent, for income and 26.80 percentage points, or 65.42 percent for employment. Thus, the tapering off of the effect is not seen in this sample. As it is likely that this sample is less likely to receive a rented apartment through the regular housing queue, these results support the speculation on the reason for the tapering-off effect in the main sample.

5.1.4. Effects on males and women

Table 6 presents the results from the same analysis as presented in Tables 3 and 4 but for women (panel A) and men (panel B) separately. Again, as the results are qualitatively the same with and without covariates, we only present the results when we control for covariates. To further enhance the presentation, we have not presented the F-statistic for every instrument and the sample size and cluster for each year. Again the effects are negative on both outcomes for both men and women except for year six on the income. However, the effects are substantially larger in absolute value for women than for men. For women, the negative effect is statistically significant on income for three to five years after partaking in the experiment and from year four to five for employment. The effect in year five is substantial for both income and employment: A reduction in income by 55,790 SEK (about € 5,800) or 27.40 percent and a reduction in employment by 19.72 percentage points or by 50.05 percent. The results for men are slightly more modest and only statistically significant for employment in year three, where the reduction amounts to 22.60 percent.

5.2. Effects on education

One possible explanation for the large effect on labor and employment and the tapering off effect is the temporary withdrawal from the labor market to pursue higher education. The costs for studying are low in Sweden,[11] but it is possible that having a rental contract at subsidized rent will affect the individual's perception of higher education.

To study the effects on education, we use the same modelling strategy as above, but with the indicator variable for receiving a higher education degree as an outcome. Individuals who already hold the highest degree, i.e. doctoral degree, at the lottery time are excluded from this analysis. Furthermore, those who achieve the highest degree after the lottery are excluded from this analysis the year after they acquired the highest degree.

[11] There are no admission fees for entering higher education in Sweden. In addition, the cost of forgone earning is subsidized by the provision of study allowance and low interest study loans. In 2007, the total amount per month was equal to about 7,400 SEK (€683) of which the study allowance covers 25 percent.

Table 6
Estimates (labor income 1,000 SEK/year and employment (%)) for the sub-samples women and men from the reduced form (OLS) and two-stage least squares (2SLS) for the six-year follow-up period.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. Women Income						
1.ITT	−4.86	−2.02	−16.89**	−29.09***	−26.19**	0.54
	(5.90)	(6.49)	(7.15)	(8.64)	(10.83)	(12.34)
2. LATE	−10.21	−3.43	−38.97**	−66.60***	−55.79**	2.14
	(13.60)	(14.94)	(16.68)	(20.38)	(23.14)	(26.57)
Employment						
3. ITT	−1.84	0.41	−2.33	−10.43***	−9.24***	−3.30
	(2.35)	(2.57)	(2.50)	(2.58)	(3.16)	(3.54)
4. LATE	−3.98	1.40	−5.16	−23.96***	−19.72***	−6.91
	(5.45)	(5.92)	(5.80)	(6.05)	(6.76)	(7.59)
Mean labor income (tSEK)	219.67	230.94	214,12	205.59	203.61	202,66
Fraction employed (%)	35.74	40.61	39.23	38.78	39.02	39.63
B. Men Income						
1. ITT	0.13	−2.96	−13.65	−13.46	−11.43	1.03
	(10.38)	(7.92)	11.07)	(10.10)	(13.01)	(14.23)
2. LATE	0.92	−5.55	−28.25	−27.54	−23.70	3.03
	(21.01)	(15.90)	(22.75)	(20.59)	(27.10)	(29.27)
Employment						
3. ITT	−2.73	−3.87	−5.65**	−3.27	−3.65	−0.24
	(2.642)	(2.47)	(2.43)	(2.68)	(3.26)	(3.51)
4. LATE	−5.37	−7.77	−11.61**	−6.50	−7.45	−0.17
	(4.91)	(5.00)	(4.99)	(5.45)	(6.80)	(7.22)
Mean labor income (tSEK)	266.17	277.77	256.38	249.98	261.96	253.47
Fraction employed (%)	50.07	54.88	51.37	50.36	51.92	49.54

Note: Clustered standard errors within parenthesis. Control variables: Age, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalues are 1,600.68 (panel A) and 1,850.34 (panel B), respectively.
***/**/* p < 0.01/0.05/0.10.

The results from the covariate-adjusted analysis are displayed in Table 7. To enhance the presentation and save space, we only present the total number of observations, clusters and the minimum eigenvalue for the overall relevance of the instruments in the note.

The results for the whole sample (Panel A) find statistically significant and positive effects in year three after participating in the lottery. For the compliers, we find an effect of 5.76 percentage points in year three, i.e. an increase that amounts to about 100 percent, thus a substantial effect. As it takes two to four years to obtain a higher education degree, these results indicate that complying individuals start a new education relatively soon after moving into the apartment; this increases their educational level three years after the lottery.

Separate analyses have been performed for women and men to see if the large effect on labor income and employment for women can be explained by education. For men, we find a significant effect in year three, similar to the result in panel A for the full sample, evidenced by an increase in education by 150 percent. The results for women point to the conclusion of an increase in education in years three and four, amounting to 77 percent and 116 percent. However, as none of these effects are statistically significant, the more considerable decrease in labor income for women than for men is a difference that is not convincingly explained by differences in education decisions.

To further understand the effects on education, we have repeated the analysis on education for the sample of individuals younger than 29 without a doctoral degree. The result is qualitatively the same as for the full sample in Table 7, and to save space, we do not present it here. However, the results can be obtained upon request.

5.3. Who are the compliers?

We cannot observe who is a complier or not, but the population of compliers in the sample can be described by their covariates, given in Table 2. The relative probability of binary attribute, X, for the compliers in comparison to the overall population, is obtained through the Bayes theorem.

$$\frac{\Pr(X=1|complier)}{\Pr(X=1)} = \frac{\Pr(complier|X=1)}{\Pr(complier)} =$$

$$\frac{\Pr(SA=1|Low,X=1) - \Pr(SA=1|High,X=1)}{\Pr(complier)}$$

The right-hand side is then easy to estimate using the sample analogue.

For the three continuous variables, age, labor income, and the number of children, we create dummies, taking value one if the age, the labor income and the number of children are below the mean age, mean income, and mean number of children, respectively, zero otherwise. Table 8 displays the results for the sample observed three years after the experiment (i.e., excluding the individuals in the last years of the study) but with covariates measured one year before the lottery. The same analysis is also conducted for all the other years. As the results are similar to the results displayed in Table 8, they are not presented here to save space.

The take-home message from Table 8 is that the compliers are not so different from the other individuals in the sample. However, the table shows that the compliers are somewhat younger, less likely to be women, less likely to have been born in Sweden, and fewer children the year before the lottery. The compliers are marginally less likely to have had an income below the average income and marginally less likely to have had no more than pre-school and secondary education.

6. Discussion

We have estimated the effect of rent control on labor supply (income and employment) and education using data from a randomized rental apartment lottery in the Stockholm MSA, Sweden.

Table 7
Estimates (receiving a higher education (%)),for full sample as well as for sub-samples women and men, excluding those holding highest degree, from the reduced form (OLS) and two-stage least squares (2SLS) for the six-year follow-up period.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. Full sample						
1. ITT	−0.12	−1.04	2.42**	1.07	−0.04	−0.12
	(1,13)	(0.98)	(1.16)	(1.22)	(1.11)	(1.24)
2. LATE	−0.33	−2.51	5.76**	2.45	−0.15	−0.34
	(2.58)	(2.27)	(2.73)	(2.81)	(2.53)	(2.83)
Fraction receiving higher degree (%)	6.97	6.04	5.60	4.83	3.43	2.76
B. Women						
1. ITT	2.03	−1,24	1.87	2.56	−0.41	1.01
	(2.02)	(1.64)	(1.86)	(2.05)	(1.81)	(2.14)
2. LATE	5.49	−3.61	5.09	6.89	−1.18	−2.42
	(5.38	(4.46)	(5.04)	(5.47)	(4.58)	(5.22)
Fraction receiving higher degree (%)	8.63	7.72	6.72	5.94	4.41	2.85
C. Men						
1. ITT	−1.29	−0.41	3.16**	0.26	0.90	−0.61
	(1.30)	(1.21)	(1.50)	(1.44)	(1.37)	(1.46)
2. LATE	−0.27	−0.91	6.76**	0.51	1.93	−1.40
	(2.62)	(2.44)	(3.13)	(2.92)	(2.90)	(3.17)
Fraction receiving higher degree (%)	5.33	4.39	4.50	3.71	2.45	2.66

Note: Total numbers of observation and cluster for panel A are 34,586 and 3,928, respectively. Total numbers of observation and cluster for panel B are 17,264 and 2,004, respectively and total numbers of observation and cluster for panel C are 17,322 and 1,929, respectively. Control variables: Age, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalues are 1,679.47 (panel A), 602.60 (panel B) and 919.59 (panel C), respectively.***/**/* p < 0.01/0.05/0.10.

Table 8
Complier characteristics ratios.

	$\Pr(X=1\|complier)$	$\Pr(X=1)$	$\frac{\Pr(X=1\|complier)}{\Pr(X=1)}$
Age less than mean	0.62	0.66	0.94
Women	0.44	0.50	0.87
Born in Sweden	0.60	0.65	0.93
Income less than mean	0.48	0.51	0.95
Number of children	0.71	0.79	0.90
Pre-school education	0.11	0.12	0.90
Secondary education	0.38	0.42	0.90

The lottery design is very similar to the waiting list system for subsidized housing in Sweden in case of excess demand, so the results are policy-relevant for the Swedish rent control system. We study the effects up to six years after partaking in the lottery.

We find a coherent picture of negative effects on the labor supply. The effects are statistically significant (at the one and five percent levels) three to five years after participation. Receiving a rental apartment in a market with excess demand reduces the annual labor income three to five years after accepting the apartment by around 16,000 SEK (€ 1,700) each year, which implies a decrease in income by between 13 and 20 percent. We also see a reduction in the fraction employed in the range of 8 to 13 percent for the same years as the effects on labor income.

As in the main analysis, we find negative estimates on income and employment for young adults without own housing. However, the magnitude of the point estimates for both outcomes is generally larger in absolute value than those for the full sample. The same goes for the results from the analysis performed separately for men and women. Again the effects are negative on labor income and employment for both men and women. The effects are, however, substantially larger for women than for men.

To some extent, the effect in these years may be explained by an increased propensity to enter higher education as a consequence of obtaining a subsidized rental apartment. Three years after participating in the lottery, there is a 100 percent increase in obtaining a higher degree for the individuals accepting the apartment. The result for education is also qualitative the same for young adults. As it takes, in general, two to four years to finish a higher degree, part of the income effect could stem from temporarily leaving the labor market during their education. The larger decrease in labor income for women compared to males cannot be explained by larger changes in education attainment by the women.

The effects need to be interpreted with an understanding of the Swedish rent control system. The aim is to keep rents for all rented apartments, *both* for new and sitting tenants, below the long-run equilibrium level. During times of substantial shortage, the level of subsidy can be very high, especially in attractive areas. The value of a rental apartment, estimated by property owners and managers, was around SEK 100,000–150,000 (€10,400-€ 5,600) per room in Stockholm in 2006. As most apartments were two-bedroom and one living room apartments, the value could be as high as 450,000 SEK (€47,000). Furthermore, the Swedish National Board of Housing estimated that the rents in Stockholm MSA would increase by approximately 68 percent if they instead were set in accordance with the market value (Sjöberg & Söderberg 2013). So the rent control system creates a considerable subsidy for all tenants without any eligibility criteria.

In an international context, the Swedish rent control system is quite unique as it covers all rental apartments. However, the results should be of interest also outside of Sweden as we show that there are long-term behavioral effects of receiving a contract. Almost all rent control ordinances in the U.S. limit how rents can be increased for existing tenants only. This practice creates a wedge between their housing costs if they stay in their existing unit and their housing costs if they move, which severely reduces mobility (see, e.g., Linneman (1987), Rapaport (1992), Glaeser & Luttmer (2003)) and then indirectly labor supply.[12] The Swedish rent control system, which covers all tenants, most likely creates lower lock-in effects than the US. Thus, considering the behavioral effects on labor earnings identified here, the total effect on labor supply in the U.S. is most likely greater.

A final concluding comment concerns the primary goal of the Swedish rent control system to reduce economic segregation. To evaluate this goal, the Swedish tenant's association compares the

[12] Linneman (1987), for example, finds that a tenant in a rent-controlled apartment in New York stays about twelve years longer than a renter in a non-controlled unit. Rapaport (1992) corrects for endogeneity in this calculation but still finds significant impacts on turnover from rent control.

average income of residents in rental apartments with residents in condominiums. Suppose the average income is lower for tenants in rental housing than for the residents in tenant-owned housing. In that case, this is perceived as an argument that the rent control system successfully reduces economic segregation (Bergenstråhle 2016). However, these analyses do not consider that income may be endogenous in relation to housing. Our analysis shows that this is the case and that the effect is not negligible. Thus, the argument that the Swedish rent control system reduces economic segregation can be misleading.

Data availability

The authors do not have permission to share data.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. The weighted local average treatment effect

In this setting $\log(number_{ij})$ interacted with the year after partaking in the lottery are the instruments in the two stage least squares (2SLS) estimator of β_t in equation (1). The estimand – which we denote WLATE – is implicitly defined in Imbens and Angrist (1994) as the weighted average effects for those that accept the apartment due to having a lower lottery number for all possible consecutive lottery numbers (i.e., one instead of two, two instead of three, . . ., and so on), that is, 14 local average treatment effects.

The results for the labor income are presented in Table A1, and the results for employment are presented in Table A2. As for the main analysis, we present the results with covariates (Panel B) and without covariates (Panel A) for the labor income analysis in Table A1, but we restrict the presentation to the analysis with covariates for the employment analysis as they are qualitatively the same as without covariates. From both tables, we can see that the instruments are relevant (i.e., large F-statistics and the minimum eigenvalue are 3,922.3 (Table A1 panel A), 3,925.27 (Table A1 panel B) and 3,925.27 (Table A2)). The minimum eigenvalue statistics is the F statistics for the joint significance of instruments in the first-stage regression (Stock & Yogo, 2005, p.84).

Table A1
Estimates (labor income 1000 SEK/year) from the reduced form (OLS) and 2SLS ((instruments: $\log(number_i)$ by year after the lottery) for the six-year follow-up period given.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. No control variables						
1. Reduced	−2.06	−2.96	8.53**	13.73***	13.24***	−2.08
	(2.93)	(2.98)	(3.55)	(3.72)	(4.99)	(5.44)
2. WLATE	9.58	13.18	−33.23**	−54.37***	−52.28***	9.84
	(11.24)	(11.37)	(13.78)	(14.66)	(19.77)	(21.54)
F-value first step	248.67	248.68	242.48	195.30	152.09	112.69
B. With control variables						
1. Reduced	−2.22	−3.13	8.33**	13.59***	12.17**	−2.60
	(2.84)	(2.88)	(3.48)	(3.66)	(4.94)	(5.30)
2.WLATE	−1.60	2.05	−44.31***	−66.33***	−59.06***	2.95
	(11.43)	(11.58)	(14.33)	(15.21)	(20.66)	(21.74)
F-value first step	193.06	192.92	188.54	153.57	118.78	90.36
Mean labor income (tSEK)	212.60	225.66	193.96	183.34	219.86	213.80
No. of observations	7,423	7,422	7,300	6,214	4,887	3,769

Notes: Total number of observations and clusters are 66,245 and 5,469, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalues are 3,922.3 (panel A) and 3,925.27 (panel B), respectively. ***/**/* p < 0.01/0.05/0.10.

Table A2
Estimates (employment %) from the reduced form (OLS) and 2SLS (instruments: $\log(number_i)$ by year after the lottery) for the six-year follow-up period given.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. With control variables						
1. Reduced	−0.33	−0.35	2.23**	3.09***	3.72***	−0.01
	(0.92)	(0.96)	(0.93)	(1.01)	(1.19)	(1.34)
2. WLATE	−2.17	−2.12	−12.61***	−16.45***	−18.25***	−2.77
	(3.77)	(3.89)	(3.77)	(4.15)	(4.89)	(5.49)
F-value first step	193.06	192.92	188.54	153.57	118.78	90.36
Fraction employed (%)	34.95	40.17	36.49	35.54	42.54	40.96
No. of observations	7,423	7,422	7,300	6,214	4,887	3,769

Notes: Total number of observations and clusters are 66,245 and 5,469, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalue is 3,925.27. ***/**/* p < 0.01/0.05/0.10.

Appendix B. Late effects with lottery number one and two.

Table B1 and Table B2

Table B1
Estimates (labor income 1,000 SEK/year) and employment from the reduced form (OLS) and two-stage least squares (2SLS) for the six-year follow-up period. Instrument lottery number = 1.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. With control variables Income						
1. ITT (income)	2.18	10.96	−14.54	−32.86**	−37.61*	−27.44
	(10.97)	(12.02)	(13.11)	(16.70)	(22.77)	(24.37)
2. LATE (income)	2.87	12.73	−15.90	−36.88*	−42.21	−30.72
	(12.01)	(13.15)	(14.39)	(18.82)	(25.73)	(27.61)
B. With control variables Employment						
1. ITT (employed)	2.39	0.33	−8.50**	−10.74**	−15.34***	−9.18
	(4.05)	(4.14)	(3.72)	(4.23)	(5.10)	(6.36)
2. LATE (employed)	2.88	0.57	−9.35**	−11.99**	−17.21***	−10.22
	(4.45)	(4.54)	(4.09)	(4.77)	(5.80)	(7.23)
F-value first step	712.59	710.72	687.08	376.26	251.32	154.78
Mean labor income (SEK)	212.60	225.66	193.96	183.34	219.86	213.80
Fraction employed (%)	34.95	40.17	36.49	35.54	42.54	40.96
No. of observations	7,423	7,422	7,300	6,214	4,887	3,769

Notes: Total number of observation and clusters are 66,245 and 5,469, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalue is 1,799.68. ***/**/* p < 0.01/0.05/0.10.

Table B2
Estimates (labor income 1,000 SEK/year) and employment from the reduced form (OLS) and two-stage least squares (2SLS) for the six-year follow-up period. Instrument lottery number = 1–2.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5	(6) Year 6
A. With control variables Income						
1. ITT	−5.50	1.23	−22.84***	−24.68***	−25.33**	3.07
	(6.49)	(7.09)	(7.62)	(9.37)	(12.36)	(13.84)
2. LATE	−8.14	2.93	−36.77***	−39.46***	−38.68**	5.61
	(10.32)	(11.28)	(12.25)	(15.13)	(18.99)	(21.08)
B. With control variables Employment						
1. ITT	−0.71	−2.16	−6.49***	−9.31***	−9.29***	−4.84
	(2.35)	(2.43)	(2.31)	(2.48)	(3.08)	(3.51)
2. LATE	−0.81	−3.20	−10.34***	−14.87***	−14.14***	−7.14
	(3.75)	(3.87)	(3.71)	(4.05)	(4.77)	(5.36)
F-value first step	244.81	243.55	239.66	191.10	180.95	132.55
Mean labor income (tSEK)	212.60	225.66	193.96	183.34	219.86	213.80
Fraction employed (%)	34.95	40.17	36.49	35.54	42.54	40.96
No. of observations	7,423	7,422	7,300	6,214	4,887	3,769

Notes: Total number of observations and clusters are 66,245 and 5,469, respectively. Clustered standard errors within parenthesis. Control variables: Age, Gender, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalue is 3,491.94. ***/**/* p < 0.01/0.05/0.10.

Appendix C. Four years balanced sample

Table C1

Table C1
Estimates (labor income 1000 SEK/year) from the reduced form (OLS) and two-stage least squares (2SLS) for the panel that is balanced during 4 years after the lottery.

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4
A. With control variables Income				
1. ITT	−5.40	−6.56	−4.97	−18.34***
	(7.46)	(6.09)	(7.51)	(6.54)
2. LATE	−10.14	−12.69	−9.20	−38.75***
	(15.36)	(12.35)	(15.42)	(13.80)
B. With control variables Employment				
1. ITT	−2.38	−2.25	−1.47	−5.28***
	(1.94)	(2.01)	(2.05)	(1.88)
2. LATE	−4.68	−4.39	−2.68	−11.09***
	(4.01)	(4.13)	(4.23)	(3.94)
F-value first step	233.27	233.00	233.16	233.29
Mean labor income (SEK)	235.12	259.96	275.66	226.78
Fraction employed (%)	42.29	48.64	53.13	44.34

Notes: Total number of observations and clusters are 24,672 and 4,780, respectively. Clustered standard errors within parenthesis. Control variables: Age, Women, Born in Sweden, Number of children, Education level. All models are estimated with lottery-fixed effects, lottery-year effects and year after lottery fixed effects. The Minimum eigenvalue is 2,001.49. ***/**/* p < 0.01/0.05/0.10.

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