Fortunate Families? The Effects of Wealth on Marriage and Fertility*

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Abstract

We estimate the effects of large, positive wealth shocks on marriage and fertility in a sample of Swedish lottery players. For male winners, wealth increases marriage formation and fertility, and there is suggestive evidence that divorce risk goes down. For female winners, the only discernible effect of wealth is that it increases short-run (but not long-run) divorce risk. Overall, the pattern of gendered treatment effects we document closely mirror the gender differences in income gradients in observational data. The gendered effects on divorce risk are consistent with a model where the wealthier spouse retains most of his/her wealth following a marital disruption. In support of this assumption, we show divorce settlements in Sweden often favor the richer spouse. *JEL Codes:* D1, J12, J13.

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1 Introduction

In a series of landmark papers, Becker famously argued that the basic tools of consumer theory could be used to understand choices in domains traditionally assumed to lie outside the scope of economic theory, such as fertility (Becker, 1960) and marriage (Becker, 1973, 1974; Becker et al., 1977). Becker argued that price and income changes were essential for understanding many of the findings from the observational literature. While his work helped launch an enormous literature on family economics, it has proven notoriously challenging to stringently test some of the core predictions of the models in question. The basic difficulty is that the models make quantitative predictions about the effects of exogenous changes in prices, wages and unearned income. It is widely understood that identifying such variation in observational data remains a major challenge for efforts to design and implement credible tests of theoretical predictions (or credibly pinning down key elasticities). For example, Hotz et al. (1997) conclude a comprehensive review of economic models of fertility by remarking that "theory and econometric methods are...much better developed than the empirical literature" and proceed to call for intensified efforts to identify "plausibly exogenous variation in proxies for the price and income concepts appearing in the theories". Lundberg and Pollak (1996) and Burstein (2007) emphasize that similar difficulties arise when testing economic models of marriage and divorce.

In this paper, we seek to make progress on the question of causality by providing credible estimates of the direction and magnitude of the long-run effects of unearned wealth on marital and fertility outcomes. Specifically, we leverage the randomized assignment of lottery prizes to analyze how large, positive wealth shocks impact marriage formation, divorce risk and fertility up to 10 years after a lottery windfall. Methodologically, our work is most closely related to previous studies that, following Imbens et al. (2001), leverage lottery wins to try to estimate the causal effects of unearned wealth. A small subset of these have considered one or more outcomes resembling the family-formation variables that we focus on here (Bulman et al., 2022; Tsai et al., 2022; Golosov et al., 2023; Bleakley and Ferrie, 2016; Boertien, 2012; Hankins and Hoekstra, 2011). The paper most closely related to ours is a a contemporaneous study of American lottery players by Bulman et al. (2022) which reports a rich and credible set of analyses of how unearned wealth impacts marriage and fertility outcomes measured up to five years after the event.

More generally, our study is broadly related to a quasi-experimental literature concerned with estimating income effects on fertility and marriage outcomes. Previous studies have relied on a range of identification strategies to try to isolate plausibly exogenous variation in income. These include leveraging income loss from job displacement (Lindo, 2010; Amialchuk, 2013; Huttunen and Kellokumpu, 2016), asset price fluctuations (Schultz, 1985; Black et al., 2013; Lovenheim and Mumford, 2013; Klein, 2017), gender-specific components of labor demand shocks (Schaller, 2016; Kearney and Wilson, 2018; Autor et al., 2019), changes in income taxes (Groeneveld et al., 1980; Cain and Wissoker, 1990; Weiss and Willis, 1997; Baughman and Dickert-Conlin, 2003, 2009; Azmat and González, 2010), or to the welfare system (Moffitt, 1990; Rosenzweig, 1999; Hu, 2003; Bitler et al., 2004; Cohen et al., 2013; Berniell et al., 2020; Yonzan et al., 2020).

Our study has several methodological strengths that allow us to go beyond this earlier work. A first is that our data allow us to classify players into groups within which the prize amount was randomly assigned under the rules of the lottery, effectively replicating the conditions of a randomized control trial. Our subsequent causal inferences are based exclusively on comparisons of the outcomes of players who were in the same group but were randomly assigned different prize amounts. Second, the prize pool is almost $265 million, allowing us to estimate treatment effects with high precision, both in the overall sample and in many interesting subsamples. Third, the rich registry data allows us to observe outcomes realized many years after the lottery, in a sample virtually free of attrition. A systematic and comprehensive comparison of our findings to those of previous lottery studies of family-formation outcomes identifies several dimensions along which our work compares favorably and helps advance the literature in substantively important ways. A final contribution is that we try to go beyond earlier work in designing and reporting numerous analyses to identify potential mechanisms underlying our findings and inform theories of the family.

In our main analyses, we study marriage (for players unmarried at the time of winning), divorce (for married players) and fertility (for all players, regardless of marital status) over three event windows: the short run (up to two years after the lottery), the medium run (five years) and the long run (ten years). We conduct our analyses in a sample of female and male players, and in the combined sample. In our pooled sample, we find that lottery wealth increases the marriage probability and fertility rate in the short and median run, while the effect on divorce risk is statistically insignificant. Our separate analyses of male and female winners suggests that the effects in the pooled sample often mask interesting heterogeneity by gender. Among unmarried men, our point estimate suggests that a one-million SEK lottery win($\approx$\$140,000), measured net of taxes, increases the probability of marriage within a five-year horizon by 4.7 percentage points, a 30% increase.[1] Lottery wealth also increases male fertility at all time horizons. Ten years after the lottery draw, male winners have 0.056 children more per million SEK, equal to a 13.5 percent increase in fertility during this period. For female winners, we find no statistically significant effects on marriage or fertility outcomes measured up to ten years after the event.

The only exception to the pattern of null results for female winners is that lottery wealth almost doubles their short-run probability of divorce. One interpretation of the absence of a discernible long-run increase in divorce risk among these women is that wealth accelerates the dissolution of marriages that were already underway. In contrast, lottery windfalls have, if anything, a tendency to stabilize male winners' marriages. Our point estimates for male winner's divorces are negative and statistically distinguishable from the effects for females in both the short and medium run.

Both in our combined and sex-stratified analyses, a striking finding is that the sign and magnitude of our estimated treatment effects often track the cross-sectional income gradients for the outcomes surprisingly closely. For example, our results on marriage formation are consistent with the literature on gender differences in partner selection (Fisman et al., 2006; Bertrand et al., 2015; Almås et al., 2023), which has found that wealth appears to improve the marriage market prospects of men more than women, on average. The "gendered" treatment effects we report for divorce are directionally consistent with evidence that the association between husbands' and wives' incomes and divorce risk have opposite signs (Berniell et al., 2020; Boheim and Ermisch, 2001; Burstein, 2007; Doiron and Mendolia, 2012; Folke and Rickne, 2020; Killewald, 2016; Weiss and Willis,

[1] We are using the 2010 average exchange rate of 7.2 SEK, as we are reporting the estimated effects in 2010 SEK. All prize amounts are net of taxes.

1997). Our follow-up analyses suggest the improvements are not uniform over the income distribution: lottery wins cause the largest increase in marriage rates and the biggest reduction in divorce rates among unmarried men with low incomes.

To provide some further insight into potential mechanisms underlying the gendered effect of wealth on divorce, we examine under what conditions our results align with economic theories of marriage. The predictions of divorce-threat models of marriage often hinge critically on assumptions about how wealth is split in the event of divorce. If wealth is split equally between spouses, each spouse's single-state utility is independent of the identity of the winning spouse. Gender differences in the effects of lottery wealth can then only arise if the utility of remaining married depends on whether the husband or wife wins the lottery. Such differential impacts could arise for a variety of reasons, for example because there is a strong social norm idealizing a male breadwinner (Bertrand et al., 2015). If the winner instead retains most of the lottery prize in divorce, an alternative explanation for the gendered divorce pattern is that the wife has greater marginal utility of consumption in the single-state than in the married state, and the husband greater marginal utility of consumption in a married state than in the single state. Intuitively, a lottery win may give a discontent wife economic opportunity to leave the marriage, while men use the prize money in a way that increases the gains from marriage.

To make some progress on distinguishing between these broad classes of explanations, we conduct a number of follow-up analyses. Under Swedish marital law, the default rule is that all assets are shared equally between spouses in the event of divorce. We show that actual divorce settlements in Sweden are, in fact, often deviate from this default. While the richer spouse redistributes some assets to the poorer spouse in the typical settlement, the amount of redistribution is smaller than the amount required to ensure an equal split of the assets. This empirical finding suggests that the spouses in our samples may anticipate retaining a larger share of the prize money in the event of divorce. Such non-equal splits probably reflect a combination of social norms, nuptial agreements and a stronger bargaining position in divorce settlements for the richer spouse.

There are additional patterns in the data which are easier to reconcile in a world of non-equal splits and gender-based preferences. First, the winning spouse retains most of the lottery wealth within marriage and increases consumption of leisure more (Cesarini et al., 2017). Because a stronger outside option also implies greater bargaining power also inside the marriage, larger consumption increases for the winning spouse follows naturally from a model with non-equal divorce splits. Other explanations appear more far-fetched. For example, a model with a male breadwinner norm and equal divorce-splits generates a larger increase in the winner's consumption only if the wife espouses the breadwinner norm more strongly than the husband. Second, the spike in divorces when wives win the lottery is only present for couples where the woman earns relatively little, consistent with the notion some women find the financial means to pursue a life as singles if they win the lottery (but not if their husbands do).

Our results also inform a debate about how fertility choices depend on income. Following Becker (1960), children are often introduced into economic models as normal durable goods. Studies on the cross-sectional relationship between income and fertility have reached different conclusions about both the sign and magnitude of any association (Anderson, 2008; Black et al., 2013; Kolk, 2019; Jones and Tertilt, 2008). Overall, our results suggest children are a normal good, even though income effects are not large, and may be stronger for men than for women. Because wealth increases men's marriage rate and lowers their divorce rate, the effect on male fertility might be partly mediated by a higher marriage rate (a back-of-the-envelope calculation suggests 20-40% of the effect). Suppose, further, that the reason wealth impacts men's marriage rate is that wealth makes men more attractive as spouses compared to other men. Then it may be misleading to extrapolate our estimates to a setting where everyone enjoys a large windfall gain. Under such a scenario, any effect of wealth on attractiveness is likely to be much smaller, and the wealth shock may have very little impact on marital choices that in turn could affect fertility.

The article is structured as follows. Section 2 reviews the lottery and the register data and describes how our estimation sample was constructed. Section 3 describes our identification strategy and provides evidence in support of our key identifying assumption. In Section 4 we report the results from our main analyses and compare our estimates to cross-sectional gradients and other studies using lottery data. Section 6 discusses how our results fit into previous theoretical and empirical literature. Section 7 concludes.

2 Data

Following Cesarini et al. (2016; 2017), we construct our estimation sample by matching three samples of lottery players, and their family members, to population-wide registers with annual information about labor market outcomes and demographic characteristics. Our basic identification strategy leverages our institutional knowledge of how prizes were awarded in each lottery to assign players to groups within which the lottery prize amounts are randomly assigned under the rules of the lottery. The group construction is almost identical to Cesarini et al. (2016), but to clarify and motivate our approach we provide a summary overview of the lottery data below.[2] We then explain the process by which we arrived at our final estimation sample and provide some summary information about the distribution of lottery prizes in this sample. All prizes are measured net of any taxes.

2.1 Kombi

Our first sample consists of about half a million individuals who participated in a monthly ticket-subscription lottery called Kombilotteriet ("Kombi"). The proceeds from Kombi go to the Swedish Social Democratic Party, Sweden's main political party during the post-war era. Subscribers choose their desired number of subscription tickets and are billed monthly. Our data set contains information about all draws conducted between 1998 and 2011. For each subscriber, the data contain information about the number of tickets held in each draw and information about prizes exceeding a million SEK. The Kombi rules are simple: two individuals who purchase the same number of tickets in a given draw face the same probability of winning a large prize. To construct the Kombi group identifiers, we therefore match each large-prize winner to (up to) 100 non-winning players who held the same number of tickets in the month of the draw and are similar to the winner in sex and age.[3] The winning player and the non-winning matched "controls" are then assigned

[2] Our procedure for generating the group identifiers is identical to that in Cesarini et al. (2016), except that all main analyses in this paper are restricted to a sample of individuals aged 18-44 at the time of the lottery event. A detailed description of the institutional features of the lottery samples, the primary sources of lottery data processing, data quality control, and group identifiers construction is provided in the Online Appendix to Cesarini et al. (2016).

[3] We set the amount won for Kombi controls to 0, even though some of them might have won modest prizes. However, because the expected amount won in the control group is approximately 100 SEK (less than half of the amount spent on buying tickets), this choice is completely inconsequential for the

to the same group.

2.2 Triss

Triss is a scratch-ticket lottery run by Svenska Spel, the Swedish government-owned gaming operator. Our Triss data set consists of two types of winners: those who presented a scratchoff ticket with three matching television symbols (Triss-Lumpsum, 1994-2011) or those presenting a ticket with three matching clover symbols (Triss-Monthly, 1997-2011). Both types of winners are invited to appear on a daily TV show where the value of their prize is determined. Each Triss-Lumpsum prize is determined by letting the winning player draw a prize on live television from a distribution with support from 50,000 to 5M SEK. The exact prize distribution is known and specified in a regulatory document called a prize plan. Triss-Monthly winners' prizes are determined similarly but through two independent draws. The first determines the size of the monthly installment won (10,000 to 50,000 SEK) and the second determines the duration (10 to 50 years). These distributions are also specified in prize plans which are subject to occasional revision.In all analyses below, we convert the Triss-Monthly prizes to present value by using a 2 percent annual discount rate.

Lacking information about non-winning Triss players, we rely on an identification strategy that compares players who won prizes of different magnitudes for reasons that are random. In Triss-Lumpsum, two players share a group identifier if and only if they won exactly one lump-sum prize in the same year and under the same prize plan. In Triss-Monthly, two players share a group identifier if and only if they won exactly one prize paid in monthly installments in the same year and under the same prize plan. We exclude a small number of players who won two prizes of the same type in a single year and under the same prize plan, since it is difficult to identify suitable controls for such winners. We also exclude shared prizes.

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estimation results.

2.3 PLS

Our final lottery sample contains information about Swedish individuals with "prize-linked savings" (PLS) accounts. PLS account holders automatically participate in monthly lottery draws where they have a chance of winning substantial monetary prizes. In most systems, banks fund the lottery prizes by reducing the interest paid on PLS account balances: PLS account owners effectively choose to forego some interest in return for the thrill of participating in the lotteries.

Under the Swedish PLS system, each account was assigned one ticket per 100 SEK in account balance, and automatically enrolled in a lottery that was held monthly for most of our study period. Each lottery ticket had the same chance of winning a prize, so a higher account balance increased the chance of winning. PLS account holders have the chance to win two types of prizes: fixed prizes and odds prizes. Fixed prizes were regular lump-sum lottery prizes that varied between 1000 and 2 million SEK. Odds prizes, on the other hand, paid a multiple of 1, 10, or 100 times the account balance to the winner (with the prize capped at 1 million SEK during most of the sample period). To define our PLS groups, we rely on one approach for fixed prizes and another one for odds prizes.

For fixed-prize winners, our identification strategy exploits the fact that the total prize amount is independent of the account balance among players who won the same number of fixed prizes in a given draw. We therefore assign two winners to the same group if they won the same number of fixed prizes in the same draw. Notice that this procedure excludes non-winning accounts from the sample. In practice, this is of little consequence, since the overwhelming majority of fixed prizes won are tiny compared to the largest prizes. Therefore, our approach effectively boils down to selecting our control group for winners from a sample of players who won very small prizes, rather than players who did not win any prizes at all. The main advantage of constructing a control group from small-prize winners rather than non-winners is that our information about prize-winning accounts is of very high quality. For the period 1986 through 1994, we observe all prize-winning accounts and can identify the owner of over 98% of them.

Among odds-prizes winners, there is no compelling justification for assuming that the total prize amount is randomly assigned among players who won the same number of odds prizes. On the contrary, since accounts with larger balances will, on average, win larger odds prizes, it is plausible that the owner of the account with larger balance differs along unobservable dimensions from the owner of the account with lower balance. For odds-prizes, we therefore proceed by matching each individual who won exactly one odds prize to an individual who also won exactly one prize (odds or fixed) in the same draw and whose account balances was similar to the winner's. A fixed-prize winner who is successfully matched to an odds-prize winner is hence assigned to the new odds-prize group and removed from the original fixed-prize group.

2.4 Estimation Sample

In this section, we summarize the key steps through which we arrived at our final estimation sample. We begin by pooling our three lottery datasets and matching players in this pooled sample to population-wide registers. This matching was performed by Statistics Sweden, using information about players' personal identification numbers (PINs) supplied by us. A complete description of how we processed the lottery data shared with Statistics Sweden and the quality-control filters we subsequently applied to the data set they delivered to us is available in the Online Appendix of Cesarini et al. (2016).

In the merged data set, the unit of observation is a lottery event: a player who wins on two occasions will therefore appear twice as two separate observations. For each lottery event, we have detailed information about any prize won (date, amount, type of prize, etc) and the characteristics of the lottery player. Since changes in marital status and fertility are strongly age-dependent (see Figure A.1), we restrict our main analyses to individuals aged 18-44 at the time of the lottery. Imposing the age restriction leaves 86,768 observations. Next, we drop observations with a missing marital status in the year prior to the lottery, which leaves us with 86,180 observations – 84,015 PLS prizes, 2,131 Triss prizes and 34 Kombi prizes. We then proceed by creating group identifiers for PLS and Triss observations in this sample, according to the previously described procedures.

In PLS, we drop observations assigned to groups with zero within-group variance in the magnitude of the prize (i.e. all players won the same prize amount) and observations assigned to an odds-prize group in which the total prize pool is below 100,000 SEK.

These restrictions leave us with 83,199 PLS observations assigned to 543 groups (164 and 379 fixed-prize and odds-prize groups, respectively). In Triss, we drop 277 prizes where we have reason to believe the winning ticket was jointly owned or the information on prize share was missing, and one group with only one observation, leaving 1,854 Triss observations assigned to 38 distinct groups (19 for each type of lottery).

Table 1: Overview of Identification Strategy

				Group Identifiers	
Lottery	Period	Type	N_p	Construction	N_G
PLS Fixed	1986-2003	Lumpsum	80,253	Draw × # Fixed Prizes	164
PLS Odds	1986-1994	Lumpsum	2,617	Draw × # Prizes × # Tickets	379
Kombi	1998-2011	Lumpsum	3,410	Draw × # Tickets	33
Triss-Lumpsum	1994-2011	Lumpsum	1,580	Year × Prize Plan	19
Triss-Monthly	1997-2011	Monthly	266	Year × Prize Plan	19

Notes. This table provides a summary overview of our identification strategy which assigns players to different groups defined by group identifiers. Groups are defined so that the magnitude of each prize won is randomly assigned within groups under the lottery. We assign two players to the same group identifier if and only if they share the characteristics listed in the column labelled Construction. N_p is number of prizes and N_G is number of unique group identifiers in our main estimation sample composed of players aged 18 through 44 at the time of the lottery. PLS Fixed and PLS Odds are for players assigned to PLS fixed groups and to PLS odds groups.

Next, we augment the data set with controls for each of the large-prize winners in Kombi. To select each winner's controls, we first identified all non-winning Kombi players who had purchased the same number of tickets as the winner in the month of win. For most winners, the number of exact matches exceeded 100, so we chose the 100 most similar in sex and age as the controls. When the number of exact matches was below 100, we retained them all as controls. Adding all Kombi controls to the sample expands the number of Kombi observations in the sample from 34 to 3,426 (34 large-prize winners and 3,392 controls) assigned to 33 groups. In one draw, two large-prize winners had identical ticket balances and therefore we assigned them to the same group. Each of 33 players were matched to 100 controls, one player was matched to 96 controls, and 4 controls (each from different cell) were eliminated because of missing pre-lottery marital status. These restrictions leave us with 88,479 observations.

Finally, we eliminate 353 observations where all outcome variables are missing. Table 1 reports the number of observations, the number of groups, and the study period for each lottery sample. Our final estimation sample consists of 88,126 observations – 82,879 from PLS, 1,846 from Triss and 3,410 from Kombi. These observations correspond to 76,866 unique individuals.

2.5 Lottery Prize Distribution

To help interpret our treatment-effect parameter, Table 2 provides some basic information about the prize distribution in our final estimation sample, both overall and separately by lottery. The prizes are expressed in 2010 SEK (deflated by the consumer price index) and net of taxes. To convey a sense of the magnitudes, the median annual disposable income of an adult Swede in 2010 was 178,100 SEK.

The bottom row of Table 2 reports the share of identifying variation contributed by each lottery to our final sample. Triss-Lumpsum and Triss-Monthly contribute the largest share of identifying variation (39.7% and 34.4%, respectively), followed by PLS fixed and odds prizes (10.6% and 12.3%). Kombi prizes contribute the lowest share of identifying variation. Clearly, the share of observations contributed to the final sample by each lottery is a very misleading estimate of the lottery's contribution to the final estimates. A related observation is that even though most prizes are small – Kombi controls and PLS small-prize winners (<10,000 SEK) account for 90% of the observations – it does not follow that our estimates are mostly informative about the treatment effects of small or modest changes in wealth. On the contrary, our estimates as primarily informative about the causal effects of positive windfall gains that are large, even from a life-cycle point. When we drop the 156 prizes above 2.5 million SEK, the total amount of identifying variation falls by 35% even though the sample size only drops by 0.2%.

Table 2: Distribution of Prizes

	(1)	(2)	(3)	(4)	(5)	(6)
				Triss...		
	PLS Fixed	PLS Odds	Kombi	Lumpsum	Monthly	All
0	0	0	3,376	0	0	3,376
1K to 10K	74,034	2,005	0	0	0	76,039
10K to 100K	5,737	222	0	527	0	6,486
100K to 500K	398	222	0	923	0	1,543
500K to 1M	18	52	2	65	0	137
1M to 2.5M	66	116	31	32	144	389
2.5M to 5M	0	0	1	19	90	110
>5M	0	0	0	14	32	46
N	80,253	2,617	3,410	1,580	266	88,126
Prize Sum (100M SEK)	3.68	2.69	0.41	4.38	7.68	18.84
% Treatment Variation	10.62	12.26	3.06	39.71	34.35	100.00

Notes. This table reports the distribution of lottery prizes for the pooled sample and the lottery subsamples. A lottery's share of treatment variation is calculated in a two-step process. First, we subtract each prize by its group-level mean and square the demeaned variable. Second, we calculate the sum of the squared variables for the lottery and divided by the sum of squares for all lotteries. *PLS Fixed* and *PLS Odds* are for players assigned to *PLS fixed* groups and to *PLS odds* groups.

3 Empirical Framework

3.1 Estimating Equation

We estimate the effect of unearned wealth by ordinary least squares (OLS), using the following estimating equation:

$$Y_{i,t} \quad = \quad \beta_0 + \beta_t L_{i,0} + X_{i,0}\delta_t + Z_{i,-1}\gamma_t + \epsilon_{i,t}, \tag{1}$$

where $Y_{i,t}$ is an outcome of player i measured t periods after the win. $L_{i,0}$ denotes the lottery prize won (measured in units of year-2010 millions of SEK), and $X_{i,0}$ is a vector of group identifiers.[4] The term $Z_{i,-1}$ is a vector of baseline characteristics measured at year-end in $t = -1$. It includes indicator variables for (i) sex, (ii) college completion, (iii) being born in a Nordic country (iv) marital status as well as a third-order degree polynomial in age-at-win and a discrete numerical variable equal to i's number of children

[4]One million SEK in 2010 is equivalent to 138.8 thousand US dollars using the 2010 average exchange rate of 7.2.

(see Section B of the Appendix for additional details). In line with previous work using the same data, we assume the effect of lottery wealth is linear, but we complement our main analyses with robustness checks omitting small or large prizes (see (Lindqvist et al., 2020) for a discussion of the linearity assumption).

Our key identifying assumption is that among players assigned the same group identifier, $L_{0,t}$ is independent of potential outcomes. If this assumption holds, we expect prize amount to be uncorrelated with $Z_{i,-1}$ conditional on the group-identifier fixed effects, and we expect OLS estimates of the β_t parameter to be unbiased both with and without the controls in $Z_{i,-1}$. We control for the pre-lottery characteristics since they absorb some of the residual variance, improving the precision of our estimates. In our main analyses we estimate the effect of the lottery wealth shock on fertility, marriage, and divorce $t = 2, 5, 10$ years after the lottery. We refer to these event windows as the short ($t = 2$), medium ($t = 5$) and long run ($t = 10$).

3.2 Outcomes and Estimation Sample

Our outcome variables are derived from data in two government registers: the *Longitudinal Integrated Database for Health Insurance and Labour Market Studies* (usually referred to by its Swedish acronym, "LISA") and the *Total Population Registry* ("RTB") (Statistics Sweden, 2017). These registries contain annual information about the three domains of family-formation that are the focus of this paper: marriage, divorce and fertility. For each domain, we analyze three outcome variables, one for each of the event horizons.

To analyze marriage formation, we generate three binary variables, one for each event horizon, in the subsample of players who were unmarried at year-end in $t = -1$. In this subsample ($N = 54,020$), each variable takes the value 1 if there is at least one marriage event recorded for the player in question over the relevant time horizon (and 0 otherwise). The variable is set to missing if marital status is missing at least once over the relevant time horizon.[5] Table A.1 in the Appendix shows that the short-run ($t = 2$) marriage rate is 9% among previously unmarried players and rises to 27% in the long run ($t = 10$). Our divorce outcomes are defined analogously, except that the event of interest now a marital dissolution and that the outcomes are defined only for players who were married

[5] An individual's marital status is missing if they are not registered in Sweden in a given year.

at year-end in $t = -1$ ($N = 34,106$). Among previously married players, the probability of a divorce is 4% in the short run, rising to 14% in the long run. Finally, we analyze fertility by calculating the total number of post-lottery children born within 2, 5 and 10 years of the lottery event in the full estimation sample. We classify a child as "post-lottery" if the month of conception is the month of the lottery event or a later month. The variable is set to missing only for individuals who were not registered in Sweden in t and in all posterior years because, in this case, it is possible that some children born t years after the lottery event will not appear in the *Total Population Registry*. Table A.1 in the Appendix shows that the average number of children born within 2, 5 and 10 years of the lottery event is, respectively, 0.07, 0.21 and 0.40 in our sample.

Table A.1 shows how the size of the estimation sample varies for each of our nine outcomes. For instance, for long-run fertility outcomes, the estimation sample is marginally diminished ($N = 86,109$), due to a small number of players for whom we do not have ten complete years of post-lottery fertility information and/or who migrated from Sweden during the study period.

3.3 Inference

Throughout, we report two sets of p-values. The first are conventional p-values derived from analytical standard errors that have been clustered at the individual level (to adjust for non-independence that could potentially arise across observations when a player appears multiple times). We refer to these as analytical p-values. We also report permutation-based p-values constructed by simulating the distribution of the relevant test statistic under the null hypothesis that the treatment effect is zero. In the majority of cases, the two sets of p-values are close in magnitude. In the instances that we observe meaningful differences, it is typically the case that the estimation sample is small or the outcome is a binary outcome with low prevalence (or both). In such instances, we favor the permutation-based p-values, which are valid under much weaker assumptions. In particular, analytical standard errors rely on asymptotic approximations of the sampling distribution that can be misleading, especially in small samples.[6] Appendix Section D.1

[6]The results of the Monte Carlo simulations described on pp. 93-96 in the Online Appendix of Cesarini et al. (2016) suggest that for binary outcomes, meaningful biases in analytical p-values are a concern

contains a detailed description of how we generate the permutation-based p-values.

In our primary analyses, we analyze the effect of unearned wealth on three outcomes, measured over three event horizons, in three different samples (men only, women only and both sexes pooled). To address the concern that we conducted many statistical tests, we report a false discovery rate (FDR) adjusted p-value for each of the $3 \times 3 \times 3 = 27$ null hypotheses of a zero treatment effect. Our method for calculating FDR-adjusted p-values follows the two-stage procedure proposed by Benjamini et al. (2006). Briefly, the procedure ensures the expected proportion of rejections that are incorrect – the false-discovery rate (FDR) – is bounded above by q. We follow the convention of setting $q = 0.10$ (Efron, 2010). In other words, the decision rule we adopt to declare each of the 27 individual hypotheses tested to be either significant or insignificant ensures that the (expected) proportion of significant test results that correctly reject the null is at least 90%. Appendix Section D.2 contains a detailed description of our procedures for multiple-hypothesis adjustment.

3.4 Generalizability

Internal Validity

Our study's key identifying assumption is that within the groups described in Table 1, prize amounts are independent of potential outcomes. An implication of this assumption is that demographic characteristics determined before the lottery should not predict, individually or jointly, within-group differences in the lottery outcome. To test this assumption, we regressed the lottery outcome on group-identifier fixed effects and several pre-lottery characteristics measured at year-end in $t = -1$. The results of these balancing tests are shown in Columns (1-5) of Table A.2 in the Appendix. Consistent with our identifying assumption, we find none of the baseline characteristics are significantly associated with prize amount, either individually or jointly, once we condition on the group-identifier fixed effects. Columns (1) through (4) show that this conclusion holds in each of the individual lotteries and Column (5) shows that it holds in the combined

in small estimation samples where the dependent variables has low prevalence. A recent work of Young (2019), that relies on a sample of approximately 50 experimental papers, shows that permutation tests find 13 to 22 percent fewer statistically significant results than are found using conventional methods.

sample. Column (6) shows that without group-identifier fixed effects, it *is* possible to predict prize amounts from pre-lottery characteristics. The fact that we are able to construct and use group identifiers in our analyses is thus more than a theoretical curiosity. The results in Column (6) suggest that the group identifiers matter in practice.

External Validity

A common concern about studies of lottery players is that individuals who choose to participate in lotteries differ in important ways from the general population, making it difficult to generalize any findings to the general population. Even though the concern itself often derives from inaccurate beliefs about the type of individuals who tend to participate in lotteries (Kaplan, 1987), it may have merit nonetheless. In designing our study, we took a number of steps to address the concern as comprehensively as possible.

In Table 3 we compare the distribution of baseline characteristics in each lottery sample, and the pooled lottery sample, to a representative sample that has been reweighted to match the sex- and age- distribution of our pooled sample. Baseline characteristics are always measured the year before the lottery. Since certain lotteries contribute a larger portion of the identifying variation (see Table \ref{tab:prizes}), we present summary statistics weighted by the amount of identifying variation in each lottery. In general, the differences are small. The overall proportion of players who are married (33%) in our pooled sample is similar to the population proportion (35%), while the fraction of players who attended college is six percentage points lower than in the representative sample (24% compared to 30%). Finally, the average player's number of children in the year of win is similar to the population average (1.2 in both samples). The average income in the pooled lottery sample is strikingly similar to the average income in the representative sample (208.5 compared to 203.0).

A different type of concern is that the effect of lottery wealth may not be informative about other types of shocks to wealth or income. However, our previous work on Swedish lottery winners give little reason to think lottery prizes differ significantly from other types of wealth shocks. Winners spend their prize money over an extended period of time (Cesarini et al., 2016), are more satisfied with their personal finances even a decade after winning (Lindqvist et al., 2020), and mainly invest in safe assets (Briggs et al., 2021). In line with a standard model, winning the lottery leads to an immediate but modest reduction in labor supply that is quite stable over time (Cesarini et al., 2017). We further estimate a positive but statistically insignificant effect on self-assessed mental health (Lindqvist et al., 2020), a modest reduction in consumption of prescriptions drugs related to mental health (Cesarini et al., 2016) and no statistically significant effect on alcohol consumption (Östling et al., 2020).

Table 3: Summary Statistics for Baseline Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Triss...		Pooled Lottery Sample	Reweighted Representative Sample
	PLS	Kombi	Lumpsum	Monthly		
Age-at-Win	33.75	37.03	33.53	34.14	33.90	33.90
1 if Female	0.48	0.33	0.45	0.44	0.45	0.45
1 if Nordic Born	0.96	0.97	0.91	0.91	0.92	0.92
# Children	1.09	1.31	1.28	1.12	1.18	1.24
1 if College	0.27	0.23	0.21	0.25	0.24	0.30
1 if Married	0.39	0.32	0.32	0.30	0.33	0.35
Income	191.91	264.50	202.36	221.59	208.47	203.03
N	82,870	3,410	1,580	266	88,126	844,443

Notes. Sample averages are reported. Baseline characteristics are measured the year before the lottery event. *# Children* is the number of pre-lottery children (children born or conceived before the lottery event). Income represents the annual income measured in thousands of SEK. Summary statistics shown in Column (5) correspond to the pooled lottery sample weighted by the identifying variation amount. The summary statistics shown in Column (6) correspond to a representative sample of Swedish adults aged 18-44, reweighted to match the sex- and age- distribution of the combined lottery sample reported in Column (5).

4 Results

We turn now to our results. We begin by reporting estimates from our core analyses of how windfall gains impact each of our outcomes over our three time horizons, before we turn to analyses related to the robustness of our findings, heterogeneity and additional outcomes.

4.1 Main Findings

The results from our main analyses of how wealth impacts marriage, divorce and fertility in the short ($t = 2$), medium ($t = 5$) and long run ($t = 10$) are summarized in Figure 1, which depicts our key parameter estimates along with their 95% confidence intervals. A more detailed summary of the results for marriage, divorce and fertility are shown in panels A, B and C of Table 4. For interpretational ease, the coefficients in Panels A and B are expressed as percentage-point changes per unit of 1M SEK won, whereas the results for fertility in Panel C are scaled so that a value of 100 would imply that a 1M SEK (about $140,000) increases average post-lottery fertility by one child over the relevant time period. We also report each coefficient estimate after it has been normalized by the mean of the dependent variable (relative risk). Overall, the FDR-adjusted p-values in Table 4 show that we can reject the null hypothesis of a zero treatment in 9 out of the 27 cases (at $q = 0.10$). Below, we briefly discuss the results in each panel in greater detail.

Overall, the estimates in Panel A suggest that unmarried lottery players who unexpectedly receive a substantial windfall are more likely to get married. All nine coefficient estimates are positive, suggesting that wealth encourages marital formation. Our estimates from the pooled sample imply a 1M SEK windfall increases the probability of getting married within $t = 2, 5, 10$ years by 2.29 (SE = 1.09), 3.25 (SE = 1.20) and 2.52 (SE = 1.51) percentage points, respectively. Expressed as relative risks, these coefficients are quite sizable, corresponding to a 25% increase in the short-run probability of getting married, a 20% increase in the medium run, and an 9% increase in the long-run. Columns (2), (5) and (8) show that the estimated wealth effects in women are consistently smaller, but never statistically distinguishable from the estimates for men. Irrespective of the time horizon, a standard F-test of the null hypothesis that wealth effects are the same in men and women fails to reject at the 5% level.

Panel B shows the results from our analyses of divorce, which were conducted in the subsample of players who were married at the time of the lottery. In the pooled sample, all estimates are small in magnitude and statistically indistinguishable from zero. The results from the sex-stratified analyses nevertheless yield some intriguing evidence of gender heterogeneity. In the subsample of male winners, all point estimates suggest that

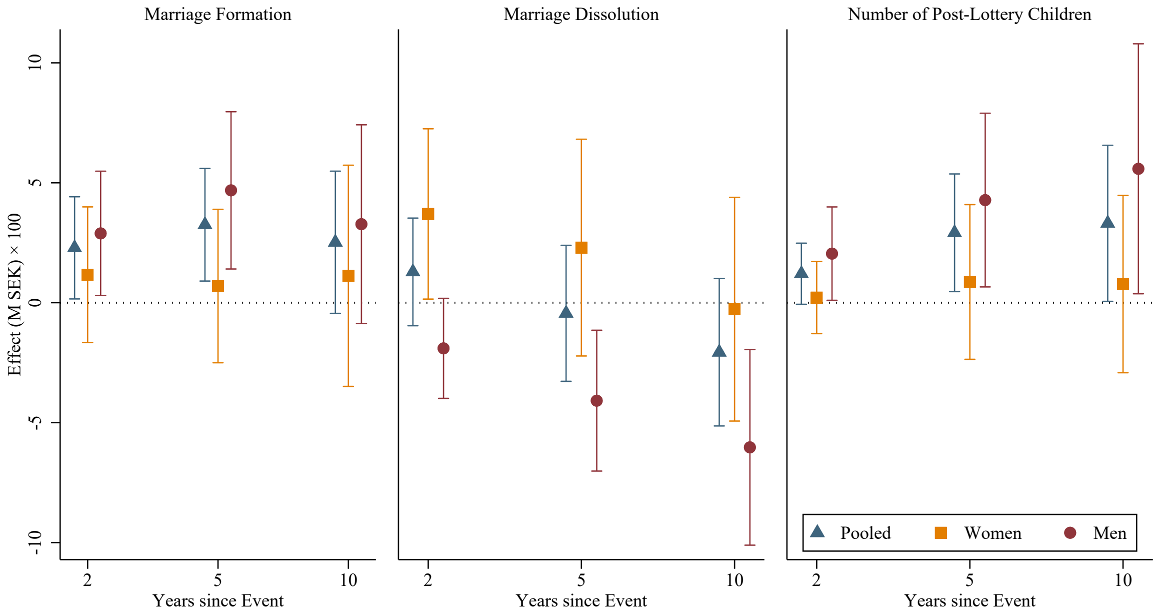
Table 4: Wealth Effects on Marriage, Divorce and Fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$t = 2$			$t = 5$			$t = 10$		
	All	By Sex		All	By Sex		All	By Sex	
		F	M		F	M		F	M
Panel A: Got Married by t (Unmarried at $t = -1$)									
Effect × 100	2.287	1.167	2.890	3.249	0.695	4.684	2.520	1.122	3.275
SE	(1.086)	(1.442)	(1.322)	(1.197)	(1.632)	(1.673)	(1.513)	(2.352)	(2.113)
p (analytical)	[0.035]	[0.418]	[0.029]	[0.007]	[0.670]	[0.005]	[0.096]	[0.634]	[0.121]
p (resampling)	[0.013]	[0.469]	[0.026]	[0.004]	[0.718]	[0.005]	[0.078]	[0.599]	[0.110]
p (FDR)	[0.051]	[0.633]	[0.078]	[0.045]	[0.819]	[0.045]	[0.162]	[0.736]	[0.198]
Baseline Mean	0.093	0.103	0.085	0.165	0.179	0.155	0.269	0.287	0.257
Relative Risk	0.247	0.113	0.340	0.197	0.039	0.302	0.094	0.039	0.128
N	53.805	22.636	31.169	53.191	22.378	30.813	51.867	21.863	30.004
Heterogeneity p		[0.378]			[0.088]			[0.496]	
Panel B: Got Divorced by t (Married at $t = -1$)									
Effect × 100	1.284	3.701	-1.903	-0.441	2.297	-4.084	-2.065	-0.270	-6.030
SE	(1.145)	(1.811)	(1.064)	(1.446)	(2.305)	(1.498)	(1.568)	(2.380)	(2.081)
p (analytical)	[0.262]	[0.041]	[0.074]	[0.760]	[0.319]	[0.006]	[0.188]	[0.910]	[0.004]
p (resampling)	[0.154]	[0.002]	[0.331]	[0.758]	[0.251]	[0.106]	[0.283]	[0.893]	[0.064]
p (FDR)	[0.260]	[0.045]	[0.470]	[0.819]	[0.399]	[0.198]	[0.425]	[0.894]	[0.144]
Baseline Mean	0.042	0.040	0.044	0.084	0.080	0.089	0.145	0.138	0.152
Relative Risk	0.307	0.927	-0.430	-0.052	0.286	-0.460	-0.143	-0.020	-0.396
N	33.994	18.750	15.244	33.740	18.617	15.123	33.094	18.320	14.774
Heterogeneity p		[0.008]			[0.020]			[0.068]	
Panel C: #Post-Lottery Children by t (All)									
Effect × 100	1.209	0.214	2.048	2.915	0.865	4.278	3.310	0.777	5.583
SE	(0.650)	(0.767)	(0.993)	(1.252)	(1.645)	(1.847)	(1.660)	(1.886)	(2.659)
p (analytical)	[0.063]	[0.780]	[0.039]	[0.020]	[0.599]	[0.021]	[0.046]	[0.680]	[0.036]
p (resampling)	[0.048]	[0.819]	[0.008]	[0.010]	[0.598]	[0.012]	[0.043]	[0.727]	[0.020]
p (FDR)	[0.118]	[0.851]	[0.051]	[0.051]	[0.736]	[0.051]	[0.116]	[0.819]	[0.068]
Baseline Mean	0.068	0.067	0.070	0.208	0.197	0.218	0.393	0.369	0.415
Relative Risk	0.177	0.032	0.294	0.140	0.044	0.196	0.084	0.021	0.135
N	88.113	41.539	46.574	87.635	41.319	46.316	86.109	40.700	45.409
Heterogeneity p		[0.144]			[0.168]			[0.140]	

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get married for unmarried at $t = -1$ players, to get divorced for married at $t = -1$ players, and on the number of post-lottery children by year-end of $t = 2$, $t = 5$ and $t = 10$ for the pooled lottery sample and by gender. All specification control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered at the individual level. Baseline mean is defined as the mean of the dependent variable of small-prize winners ($< 10,000$ SEK). The resampling based p-values are obtained from the resampling distribution of coefficients from 1,000 Monte Carlo simulations. $p(FDR)$ correspond to the false discovery rate adjusted resampling p-values computed using (Benjamini et al., 2006) and (Anderson, 2008) procedure. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Age-at-win: 18-44.

windfalls stabilize marriages. The $t = 10$ effect suggests a 1M SEK windfall reduces the probability of divorce by 6.03 percentage points (SE = 2.08), equivalent to a 40% reduction in relative risk. However, when we calculate resampling-based p-values and FDR-adjusted p-values, the impact of the husband's lottery wealth on the probability of divorce is statistically indistinguishable from zero. In the female subsample, the pattern is strikingly different. Whereas the long-run estimate is close to zero, married women who win the lottery are *more* likely to get divorced in the short run. In the female subsample, we estimate that a 1M SEK windfall increases short- and medium-run risks of divorce by 3.70 (SE = 1.81) and 2.30 (SE = 2.30) percentage points, respectively. Rescaling by the

Figure 1: Wealth and Family Formation



Notes. This figure reports the estimated treatment effects of a million SEK on the probability to get married for unmarried players, on the probability to get divorced for married players, and on the number of post-lottery children, all measured at year-end in 2, 5, and 10 years after the lottery. The results are reported in the pooled sample and by gender of the winner. The estimates are multiplied by 100. All specifications control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered by individual, and the error bar corresponds to 95 percent analytical confidence intervals. Age-at-win: 18-44.

baseline probability (4%), our estimate suggests a 1M SEK windfall effectively doubles the short-run risk of divorce. Our results thus suggest that among married women, wealth produces a large spike in short-run divorce risk that subsequently dissipates and may be close to zero in the long run. One interpretation of this pattern is that unearned wealth accelerates the termination of marriages whose dissolution were already underway. While only two of our nine coefficient estimates for divorce are statistically distinguishable from zero, conventional F-tests reject the null that male and female coefficients are identical at $t = 2$ ($p = 0.008$), $t = 5$ ($p = 0.020$) and $t = 10$ ($p = 0.068$).

Panel C reports our results for fertility, which were conducted in the full estimation sample. For each of the three horizons, the outcome variable is the number of post-lottery children born over the relevant time horizon. The point estimates in Panel C are positive, consistent with children being a normal good. In our pooled sample, we estimate that a 1M SEK windfall increases expected number of children by 0.012 (SE = 0.007), 0.029 (SE = 0.012) and 0.033 (SE = 0.017) children in the short-, medium- and long run, respectively. Expressed as relative risks, the $t = 5$ estimate suggests that a 1M SEK windfall gain increases medium-run fertility by approximately 14%. In our sex-stratified analyses, we find evidence that children are a normal good for men. For all event windows, the estimated effects in the male subsample are positive and economically meaningful. For example, the $t = 10$ effect of a 1M SEK windfall is estimated to be 0.056 (SE = 0.027), which is equivalent to a 13.5% increase in long-run fertility. In the female subsample, the coefficients are also positive, but smaller. Since none of the three female estimates is statistically distinguishable from zero, we conclude that the evidence that children are normal goods is considerably weaker in the female subsample than in the male or pooled sample. We also note that our estimates are too imprecise to reject equal treatment effects in men and women, so the differences between men and women are only suggestive.

4.2 Sensitivity Analyses

To probe the robustness of the findings in Table 4, we first examined if our main results are sensitive to alternative sample inclusion criteria with respect to age. Our main analyses were all conducted in a sample limited to players aged 18-44 at the time of the lottery event. For our fertility outcomes, these cutoffs are well within the range typically used in the literature (e.g. Hotz et al., 1997). While the exact values are arbitrary, Panel C of Figure A.1 provides a compelling graphical justification for an upper limit somewhere in the mid 40s. The probability of having at least one more child is strongly related to age, reaching a peak around the age of 25. By age 45, it is effectively zero. Panels A and B of Figure A.1 show that for marriage and divorce outcomes, there are qualitatively similar patterns by age although marriage and divorce probabilities do not decline to zero at older age. For example, in a random sample of unmarried (married) Swedes aged between 45 and 64, we find that 10.3% (6.7%) get married (divorced) within ten years. We therefore reran our main analyses of marriage and divorce (but not fertility) in an estimation sample constructed using a more liberal age-at-win restriction of 18-64. Appendix Table A.3 reports the results. For marriage, the results are similar to those in our primary specification. For divorce, we find that the results are sensitive to the alternative age cutoffs: the gender gap is smaller in magnitude and no longer statistically significant.

Second, we looked for evidence of nonlinear wealth effects by rerunning our original analyses omitting either small (<10,000 SEK for PLS winners) or large (>4 million SEK) prizes. If the marginal effects of wealth were everywhere diminishing or increasing, we expect the coefficient estimates to systematically move in opposite directions relative to the baseline estimate in Table 4. For example, under diminishing marginal returns, the specification with large prizes omitted is expected produce coefficient estimates closer to zero, whereas the specification with small prizes omitted should produce coefficients further away from zero. Under increasing returns, we expect the opposite pattern. Empirically, the results in Appendix Table A.4 provide little evidence of nonlinear effects for marriages and fertility: we fail to detect any systematic changes of coefficient estimates and the changes that we do observe are well within the range expected from sampling variation alone. In the sample of married male players, we find that omitting large prizes results in estimates closer to zero, suggesting that the negative and borderline significant estimates in the baseline sample are sensitive the inclusion of a small number of large prizes.

Third, we attempt to reassure that the heterogeneity by gender we document is not driven by differences in characteristics between couples where the wife played the lottery and couples where the husband played the lottery. In Tables A.5 and A.6 in the Appendix we reweight couples where the wife or the husband won in order to match the first and the second moments of pre-lottery characteristics in the pooled sample. That is, we construct the weights so that the reweighted sample of the couples where the wife won is similar to the reweighted sample of the couples where the husband won in terms of husband's and wife's age, income, the number of children, the year they played the lottery, and the type of the lottery they played (*i.e.*, Kombi, PLS, Triss). We use entropy balancing technique (Hainmueller, 2012) for the construction of the weights (the details are provided in Section E of the Appendix). The estimates of the lottery effect on the probability of marriage dissolution in the reweighted samples (reported in Table A.6 in the Appendix) confirm our main findings – we can reject equal treatment effects in the couples where wives win and where husbands win in the medium and long run, even when the estimates are adjusted for differences in characteristics between these subsamples.

4.3 Heterogeneous Effects

We now investigate heterogeneity along dimensions other than gender. For each of our three outcomes, we analyze whether the lottery effect varies in the pooled sample by the winner's (i) pre-lottery income (above/below the sample median), (ii) age-at-win (above/below age 35) and (iii) pre-lottery parental status (no children/some children). For fertility we also test whether effects differ between married and unmarried players. We estimate equation (1) for each subsample separately and report the results in Table A.7 (marriage formation), Table A.8 (divorce) and Table A.9 (fertility) in the Appendix.

Overall, we find little evidence of heterogeneous effects, but there are a few notable exceptions. First, the positive effect on marriage formation is statistically distinguishable from zero only among low-income players. In our analyses by gender above, we only find an effects on marriage formation of men. In a further exploratory analysis, we split the sample by both income and gender. Though statistical power goes down with the number of subgroups, Figure A.2 shows the effect of lottery wealth on marriage formation is positive for low-income men ($\hat{\beta}_5 = 7.84, SE = 2.29$) and it is not statistically distinguishable from zero for high-income men and women.

Second, the effect of lottery wealth on fertility is larger for winners who are young (when we split the sample by age) and unmarried (when we split by marital status), though we cannot always reject identical effects. The stronger effect for young winners is unsurprising given that the fertility rate declines with age (see Figure A.1). The stronger effect for unmarried players suggest the fertility response is driven by new marriages of unmarried players. Figure A.2 shows the results when we further split the age- and marital-status subsamples by gender. Notably, the gender difference in the fertility response is larger for older winners, suggesting declining female fertility may attenuate the female fertility response. Figure A.2 also shows estimated effects are close to zero for women regardless of their marital status. For men, the effects on fertility are also similar between unmarried and married players.

4.4 Additional Outcomes

We consider two additional outcomes: cohabitation and spousal quality. We observe cohabitation for unmarried couples with joint children. Panel A of Table A.10 in the Appendix shows the increase in unmarried men's probability of marriage is robust to excluding cohabiting men from the sample, suggesting the effect on marriage probability is not simply due to cohabiting couples changing their relationship status. Panel B shows the effect of lottery winnings on cohabitation for "single" winners (unmarried and not cohabiting with children prior to winning). The estimated effect is positive for women and negative for men, but neither effect is statistically different from zero. Consequently, the increase in men's marriage rates likely reflects an increased propensity to marry rather than a general increase in couple formation.

We next consider whether changes in the marriage rate coincide with changes in spousal quality. To avoid selection bias, we regress indicator variables equal to one in case an individual married a spouse with certain characteristics. This way of defining the outcome variable thus combines the "extensive" margin (whether an individual is married) and the "intensive" margin (spousal quality conditional on marriage). We consider two proxies for spousal quality: age and education. Figure A.3 in the Appendix suggests the likelihood of marrying a younger spouse does not increase in lottery wealth for female and male winners. In unreported analyses, we do not find that men or women are more likely to marry a partner with a college education.

5 Benchmarking Lottery Estimates

In this section, we compare our lottery estimates to two different benchmarks: income gradients and estimates from previously published studies of lottery players.

5.1 Income Gradients

We estimate cross-sectional income gradients using a representative sample of Swedish adults that has been weighted to match the sex-, age- and event-year distribution in the lottery sample. Our reweighting procedure ensures that in the representative sample, the outcomes are defined over the same distribution of event windows as in the lottery sample. To estimate the income gradients, we estimate the equation

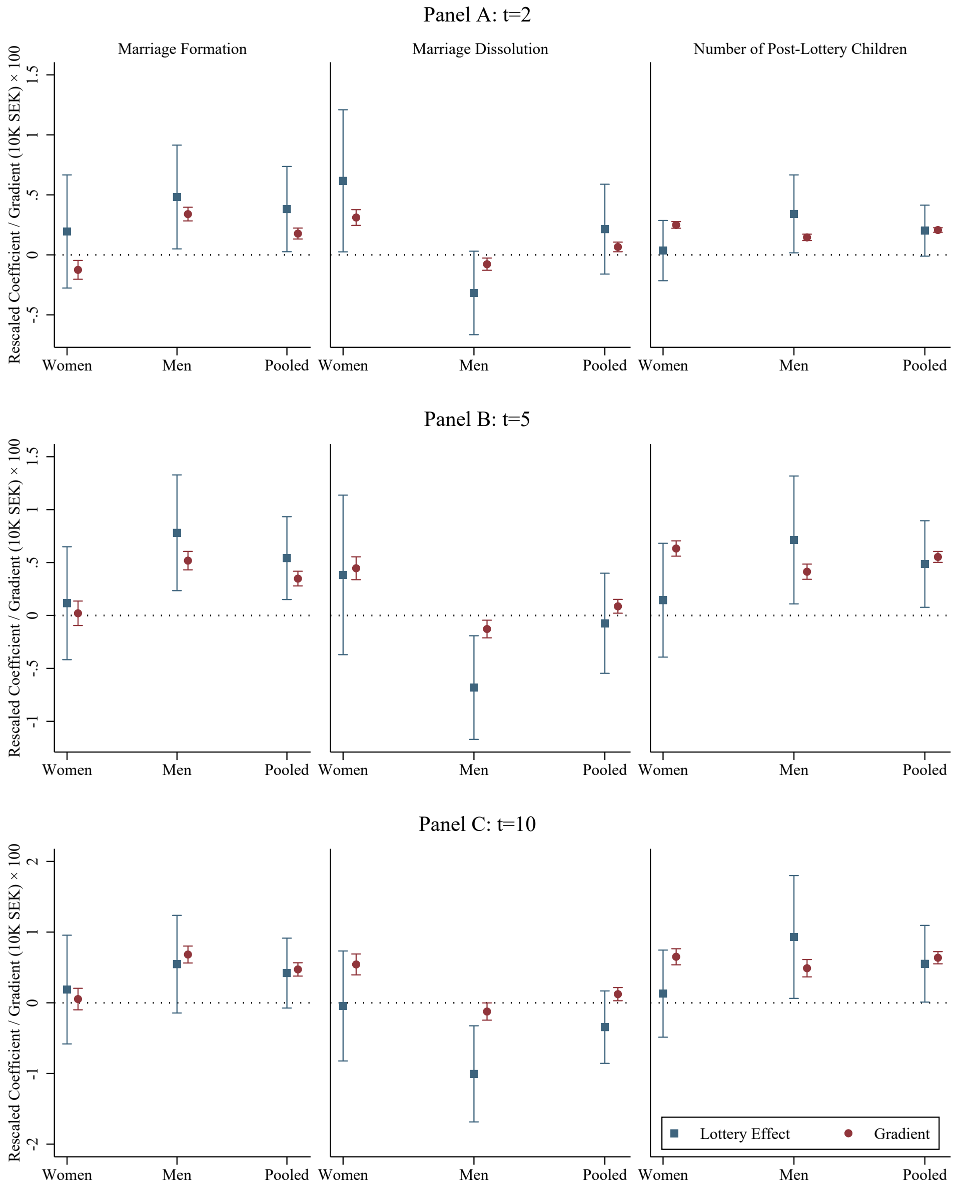
$$Y_{i,t} = \alpha_0 + \alpha_t I_{0,i} + Z_{i,-1}\theta_t + B_{0,i}\psi_t + u_{i,t} \tag{2}$$

where $Y_{i,t}$ is individual i's outcome measured t years after the event year, $Z_{i,-1}$ is the set of baseline covariates defined in the previous section, and $B_{0,i}$ is a vector of event-year fixed effects. To obtain a long-run measure of economic status purged of most transitory year-to-year fluctuations in income, $I_{0,i}$ is defined as the five-year average of all annual disposable incomes observed up to five years before the event year.

Our lottery estimates are not on a scale that easily permits comparisons to income gradients. To enable comparisons, we follow our previous research (Cesarini et al., 2016; Lindqvist et al., 2020) and convert the lump-sum prizes to annuity payouts, assuming a 20-year period and interest rate of 2%. For point of reference, a lump-sum prize of \$100,000 translates into an increase in permanent annual after-tax income of \$5,996.

Figure 2 and Table A.11 compares each of our annuity-rescaled lottery estimates to its corresponding income gradient. The lottery estimates are generally similar in magnitude to the income gradients. Even more strikingly, the patterns of heterogeneity by gender are often similar when we compare the quasi-experimental lottery estimates to the non-experimental gradients. For purposes of illustration, consider the gradient between short-run divorce risk and income. Panel A shows both the gradient and the lottery-estimate are positive in women and overall, but negative for men. Our comparison between lottery-based estimates and gradients thus bolster the credibility of previous studies that rely on observational data.

Figure 2: Comparison of Annuity-Rescaled Lottery Estimates to Income Gradients



Notes. This figure reports the estimated treatment effect of annuitized lottery prize and annual disposable income gradient (in 100,000 SEK) on the probability to get married for unmarried at $t = -1$ (Column 1), on the probability to get divorced for married at $t = -1$ (Column 2), and on the number of post-lottery children (Column 3), all measured at year-end in 2 (Panel A), 5 (Panel B), and 10 (Panel C) years after the event. The results are reported in the pooled sample and by gender of the winner. The rescaled lottery effects are estimated using equation (1), and the income gradients are estimated using equation (2). The estimates are multiplied by 100. All specifications control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered by individual, and the error bar corresponds to 95 percent analytical confidence intervals. Age-at-win: 18-44.

5.2 Comparison to Other Lottery Studies

We identified six quasi-experimental studies of lottery players that report estimates of wealth effects on outcomes similar to those in our primary analyses. Table A.12 provides summary information about each of these along some key dimensions, including identification strategy, sample, event-study windows, outcomes analyzed, and basic information about relevant estimates of reported in the original study.

We emphasize two dimensions along which our study compares favorably to this other work. First, our identification strategy compares the post-lottery outcomes of individuals *within* groups of ex ante identical participants from the same lottery. Within each group, we know that the magnitude of prizes won were randomly assigned under the rules of the lottery. This feature of our study distinguishes it from the remaining six studies, all of which make stronger identifying assumptions. Second, we can track our post-lottery outcomes for long periods after the win, allowing us to study effects for event-study windows as wide as a decade. This feature of our study allows us to go beyond earlier work in characterizing how wealth effects evolve over time.

Of the six studies, one analyzed lottery players in modern Taiwan (Tsai et al., 2022), one used data from the British Household Panel Survey (Boertien, 2012), one reported estimates of the effects of winning a land-lottery in early-19th century Georgia on long-run fertility (Bleakley and Ferrie, 2016), and the remaining three studied lottery winners in contemporary United States (Bulman et al., 2022; Golosov et al., 2023; Hankins and Hoekstra, 2011). Bulman et al. (2022) and Golosov et al. (2023) use multiple years of federal tax records for the entire US population to construct a panel with information about the winners of state lotteries. Since the studies use tax data from similar periods (2000-2019 and 1999-2016, respectively) the sample overlap is obviously very high. However, the papers differ substantially in their focus. While Golosov et al. (2023) analyze some marital outcomes in their full sample, their primary focus is on labor-market outcomes.

By contrast, Bulman et al. (2022) conduct a rich set of analyses of fertility and marital outcomes, many of which are directly relevant for and comparable to our study's findings. Their primary analyses are all conducted both in a full sample, and in men and women separately, applying several sample inclusion criteria that facilitate comparability with our results. For example, they impose an age restriction (25-44) similar to ours (18-44) and their paper's main specification excludes prizes greater than $500K. This restriction on prizes ensures that the identifying variation comes from prizes in a range similar to the range in our sample (Table 2). For these reasons, we generally prioritize comparisons of our results to Bulman et al. (2022) relative to those reported by Golosov et al. (2023).

The third US lottery study (Hankins and Hoekstra, 2011) analyzed data on state-lottery prizes won over a six-year period in a region of Florida. For several reasons, some of which are discussed below, we view the results in Hankins and Hoekstra (2011), and also Boertien (2012), as substantially less informative than those in the other four studies, and assign less weight to their results in our discussion below.

To facilitate comparisons across studies, we rescaled each of the originally reported effect-sizes to make them interpretable as estimates of the effect of a windfall measured in units of $100K. Below, any coefficients reported are harmonized coefficients, except where explicitly stated otherwise. See Appendix C for details on how the harmonized coefficients were generated.

Marriage

Four of the studies report estimates wealth effects on marriage (Bulman et al., 2022; Tsai et al., 2022; Golosov et al., 2023; Hankins and Hoekstra, 2011). For players unmarried at the time of win, Bulman et al. (2022) estimate that a $100,000 windfall increases the probability of being married two and five years after the lottery by 2.43 ($SE = 0.43$) and by 1.18 ($SE = 0.51$) percentage points, respectively. Since their data does not allow them to directly observe marriage or divorce events, they define an individual as married or not based on the marital status listed in the year-t tax filing (whereas we define the person as married if we observe at least one marriage event between the lottery and the end of year t). Rerunning our baseline model for $t = 2$ and $t = 5$ using Bulman *et al.*'s (2022) alternative definition of marriage, our harmonized $t = 2$ and $t = 5$ estimates were 1.67 ($SE = 0.78$) and 2.40 ($SE = 0.85$).[7] Tsai et al. (2022) also analyze wealth effects on marriage among players unmarried at the time of win. Their harmonized estimates

[7]These estimates are nearly identical to harmonized estimates from our main specification with marriage defined as described in Section 3.2, we conclude that the use of the alternative measure of marriage used by Bulman et al. (2022) is not a barrier to comparability.

for $t = 2$ and $t = 5$ respectively, are 1.16 ($SE = 0.34$) and 1.00 ($SE = 0.38$).

Overall, the results provide compelling evidence that unearned wealth increases the probability that single winners get married. For $t = 2$, the harmonized estimates range from 1.16 to 2.43, and an inverse-variance weighted average of the three coefficient estimates is 1.63, with a standard error of 0.25. For $t = 5$ the coefficients range from 1.00 to 2.40, now with an inverse-variance weighted average of 1.22 ($SE = 0.29$).

Next, we examine how Bulman *et al.*'s (2022) results broken down by gender compare to ours. In our sample, the wealth effects on marriage in the full sample appear to be primarily driven by unmarried men, especially those with low incomes. Bulman *et al.*'s (2022) harmonized estimate for $t = 5$ is 1.49 ($SE = 0.69$) among unmarried men, compared to 0.66 ($SE = 0.76$) for unmarried women. For point of reference, the analogous estimates in our sample are 3.47 ($SE = 1.15$) and 0.27 ($SE = 1.18$). Additionally, Bulman *et al.*'s (2022) follow-up analyses suggest that the effects are strongest among low-income men. Overall, Bulman *et al.*'s (2022) results on gender and marriage are thus qualitatively consistent with our main findings. However, we note that the magnitude of the estimated Swedish gender gaps are generally larger than those reported Bulman *et al.* (2022) and that both gender gaps are imprecisely estimated. Overall, a plausible interpretation of the evidence is that wealth effects on marriage may well be stronger among unmarried than among unmarried women, but that it is likely that the gender gaps we report are overestimates. An alternative possibility is that the actual gender gap are substantially larger in Sweden, but this strikes us as a less plausible first-order explanation.

Overall, the evidence from the three studies on how wealth impacts marriage rates up to five years after the lottery is congruent. There is clear and compelling evidence that the true harmonized wealth parameters are in the range 1-2 percentage points. There is also some evidence suggesting that wealth effects are larger for men. Finally, the evidence suggests that wealth effects on both unmarried men and women tend to be strongest among those with lower incomes.

The results reported in the fourth study on marriage, by Hankins and Hoekstra (2011), may seem to contradict our main conclusions about the sign and magnitude of wealth effects. The study's point estimates for marriage consistently go in the "wrong" direction, and the authors report a negative and statistically significant wealth effect on women that is highlighted in the abstract and interpreted as evidence that additional income makes single women less likely to marry. However, our examination of the results concludes that such conclusions may not be justified. In Online Appendix C.3, we show that Hankins and Hoekstra's harmonized estimates have large standard errors. Even under assumptions about the true effect sizes we consider optimistic, the study's statistical power was too low to provide meaningful information about the wealth effects of interest. Given this low power, we show that it is not uncommon, conditional on finding a statistically significant result, for the coefficient to have the "wrong" sign and for its magnitude to be implausibly large.

Divorce

Our literature review identified four studies of lottery players that estimate wealth effects on marital dissolutions (Bulman et al., 2022; Golosov et al., 2023; Boertien, 2012; Hankins and Hoekstra, 2011). Two of them (Hankins and Hoekstra, 2011; Boertien, 2012) are vulnerable to concerns about low power similar to those voiced about Hankins and Hoekstra's (2011) marriage analyses in the previous section (see also C.3). For example, Boertien (2012) uses data from the British Household Panel Survey and finds that lottery wealth causes a statistically significant decrease in the divorce risk for men, but not women. However, the small number of winners in the sample, the small prize amounts (average prize £402) and the use of an empirical specification with prizes expressed in logarithms makes us skeptical that a detailed comparison would be instructive. Therefore, we focus on relating our findings to those in Bulman et al. (2022).

In our full sample of players married at the time of the lottery, our harmonized estimates are consistently small and statistically indistinguishable from zero. For example, our harmonized estimate for $t = 5$ implies a 0.32 percentage-point *reduction* ($SE = 1.04$) in the probability that a player married at win files for divorce within five years of the lottery. The evidence reported by Bulman et al. (2022) is based on analyses of how wealth impacts the probability that players remain married. Their harmonized $t = 5$ estimate suggests that players married at $t = 0$ are 0.98 percentage points ($SE = 0.48$) *less* likely to be married five years later. Even though the harmonized estimate reported has the opposite sign of ours, both estimates are close to zero and we cannot reject the null hypothesis that the parameters are identical. Overall, the evidence is consistent with wealth effects on divorce risk being small overall.

Several findings suggest that wealth may impact the divorce risk of married men and women differently. For example, our harmonized $t = 5$ estimates imply that married women are 1.87 ($SE = 1.62$) percentage points *less* likely to remain married, whereas men in our sample are 3.11 ($SE = 0.89$) percentage points *more* likely to remain married. The analogous estimates reported by Bulman et al. (2022) are -1.57 ($SE = 0.78$) for women and -0.58 ($SE = 0.78$) for men. The gender gap implied by these estimates is approximately one percentage point. While the sign of the gap matches what we find, the magnitude of the gap is substantially smaller. Finally, Bulman *et al.* (p. 21 2022) report evidence that wealth causes larger increases in divorce risk among women with low baseline earnings, a finding consistent with the results from follow-up analyses described in Section 6.2 below.

Fertility

Finally, we identified three lottery studies examining fertility: Bulman et al. (2022), Tsai et al. (2022), and Bleakley and Ferrie (2016). For ease of interpretation, the harmonized wealth effects are multiplied by a factor of 100, so that an effect of 1.00 means that a $100K windfall increases the expected total number of children over the relevant event horizon by 0.01 (or equivalently, that a windfall of $100K increases the probability of having one more child during the relevant time period by one percentage point).

Bulman et al. (2022) analyze fertility outcomes up to five years after the lottery event. While they find clear evidence of a modest increase in the likelihood of having a child shortly after the win, they find no evidence that the wealth impacts cumulative births up to five years after the lottery. Their harmonized estimate for cumulative fertility at $t = 5$ is 0.07 ($SE = 0.47$). In our sample, the analogous estimate is 2.1 ($SE = 0.90$), whereas Tsai et al. (2022) report an estimate of 1.36 ($SE = 0.40$) on the same scale. An inverse-variance weighted meta-analysis of the three coefficients yields an estimate of 0.95 ($SE = 0.29$), providing strong evidence against the null that the effect is everywhere zero.

Finally, Bleakley and Ferrie (2016) exploit a land lottery that took place in Georgia in 1832 to study, inter alia, how wealth impacts long-run fertility outcomes. The obvious differences in time and place clearly hamper comparison to work based on more recent data and we make no effort to try to generate harmonized estimates for this study. That said, the authors find that winners – who, on average, were assigned a plot of land worth the approximate equivalent of five years salaries for an unskilled laborer at the time – had 0.18 more children than non-winners and demonstrate that the positive fertility effect persisted over the entire 18-year post-lottery period analyzed.

Overall, the weight of the evidence suggests to us that in most populations, unearned wealth has a small to modest positive effect effect on fertility.

6 Discussion

In this section, we discuss how our results fit into previous theoretical and empirical literature.

6.1 Marriage Formation

We find that lottery wealth has a positive effect on men's marriage formation, while the effect for women is close to zero. Splitting the sample by both gender and pre-win income, we further only find a positive effect for low-income men. How could these results be understood?

First, the fact that we see no effect of lottery wealth on the marriage rates of low-income women speaks against theories suggesting that young adults may delay marriage until they reach economic stability (Oppenheimer, 1988). Although classical economic models struggle to explain the documented gender heterogeneity, it aligns with the concept of hypergamy, i.e. the tendency for husbands to have a higher economic rank in the overall male population than their wives do in the overall female population (see Almås et al. 2023 for recent evidence from Norway). In the presence of hypergamy, high-income men have a larger pool of potential partners than their lower-income counterparts, which suggests that wealth may increase their probability of finding a match. Additionally, our results are consistent with earlier research findings indicating that there is a stronger relationship between income or other human-capital related factors and the propensity to marry in men than in women (Burgess et al., 2003; Ellwood and Jencks, 2004; Burstein, 2007; Bertrand et al., 2015; Killewald, 2016; Autor et al., 2019). One of the explanations for this pattern is that men and women may give different weights to different qualities when choosing a partner. This explanation fits with literature indicating that women value earning potential, intelligence, and social status of potential partner more than men do (Fisman et al. 2006; Hitsch et al. 2010b,a; Regan et al. 2000; Almås et al. 2023). A parsimonious interpretation of our results is therefore that lottery wealth increases the attractiveness of low-status men who would otherwise have a hard time finding a spouse.

6.2 Marriage Dissolution

We find that lottery wealth increases the short-run divorce probability in the couples where wives win, and our point estimates suggests a reduction of the long-term divorce probability in couples where the husband wins . These gendered treatment effects are consistent with a large body of empirical evidence showing higher husband's earnings or employment stabilize marriages, while wife's income or employment have an opposite effect (Berniell et al., 2020; Boheim and Ermisch, 2001; Burstein, 2007; Doiron and Mendolia, 2012; Folke and Rickne, 2020; Killewald, 2016; Weiss and Willis, 1997), though there are several exceptions (Hoffman and Duncan, 1995). Yet while gender differences in the effect of earnings on marriage dissolution can be explained by the differential effect of earnings of the husband and wife on the gains from specialization according to comparative advantage, gender differences in the effect of lottery wealth cannot be explained by this mechanism.

To interpret our results, in Section F of the Appendix we consider a symmetric cooperative bargaining model with singlehood as the threat point, as in Manser and Brown 1980 and McElroy and Horney 1981. A key assumption in the model is how wealth is split in a divorce settlement.[8] If lottery wealth is split equally, single-state utilities are independent of which spouse won the lottery. In this case, husbands' and wives' lottery wealth have different effects on marriage dissolution only if husbands' and wives'

[8]In Section F.1 of the Appendix, we show the key role of wealth splits in divorce holds also in a more general theoretical framework (as in Weiss 1997) without assumptions regarding the bargaining process.

wealth affect married-state utilities differently. One such potential mechanism is a male breadwinner norm (Bertrand et al., 2015). A male breadwinner norm implies gains from marriage increase when husbands win the lottery (decreasing divorce risk) but decreases when wives win (increasing divorce risk).

If the winner instead retains most of the lottery prize, an alternative explanation for the gendered divorce pattern is that the wife has greater marginal utility of consumption in the single-state than in the married state and the husband has greater marginal utility of consumption in the married state than in the single state. In this case, the gains from marriage increase in the husband's lottery wealth and decrease in the wife's lottery wealth. Intuitively, a lottery win may give a discontent wife economic opportunity to leave the marriage, while men use the prize money in a way that increases the gains from marriage.

We now turn to a discussion of which mechanism best fits patterns in the data. We first present descriptive evidence that divorce settlements in Sweden are often unequal and favor the richer spouse. We then present two additional empirical regularities. First, the winning spouse retains most of the lottery wealth and increases consumption more than the non-winning spouse. Second, both the increase in divorce risk when wives win and the decrease when the husband win are stronger in couples where the winner earns relatively little. We argue these regularities are easier to reconcile with our second mechanism, i.e. gender differences in preferences and a stronger bargaining position for the winning spouse, though other explanations (including a breadwinner norm) cannot be completely ruled out.[9]

The Division of Assets in Divorce

According to the Swedish Marriage Code, the default rule is that all assets (including inherited assets and pre-marital assets) should be split equally after divorce, unless spouses have a nuptial agreement or agree on another division of assets. As of today, 11% of Swedish married couples have a nuptial agreement.[10] In Appendix G.2, we present an

[9]Employing a method similar to Bertrand et al. (2015), Hederos and Stenberg (2022) find only weak evidence that Swedish couples adhere to a male breadwinner norm.

[10]The figure for nuptial agreements comes from e-mail correspondence with Henrik Bondesson of the Swedish credit reference agency *UC* on January 23rd, 2023.

investigation of a random sample of 997 Swedish nuptial agreements registered in 2013. Our investigation reveals one third of nuptial agreements state that all property is private property, implying there is no marital property to split in case of divorce. More than half (54%) of nuptial agreements specify that certain types of assets, typically real estate, inheritances and stocks in closely held corporations, are to be individual property. Notably, such contracts are often asymmetric in the sense that only one of the spouse's property is excluded from marital property.

Even absent a nuptial agreement, the identity of the lottery winner is likely to matter for bargaining power in divorce. Before a divorce settlement is reached, the prize money is controlled by the winning spouse unless a voluntary transfer is made, allowing the winner considerable freedom how to spend the money. And because a legal process that ensures the non-winning spouse his or her stipulated share in a divorce settlement is costly, bargaining theory suggest the winning spouse should be able to keep more than half the prize amount in divorces settled outside of court. Survey evidence from Brattström (2011) indicate only one fifth of divorcing couples in Sweden hired some kind of legal assistance to help with their divorce settlement. The survey also indicates spouses often agree on unequal divisions of assets even in the absence of nuptial agreements. Out of the thirty-four percent of respondents who said the division of assets was unequal, 53% said they "agreed to it" while 35% referred to a nuptial agreement.

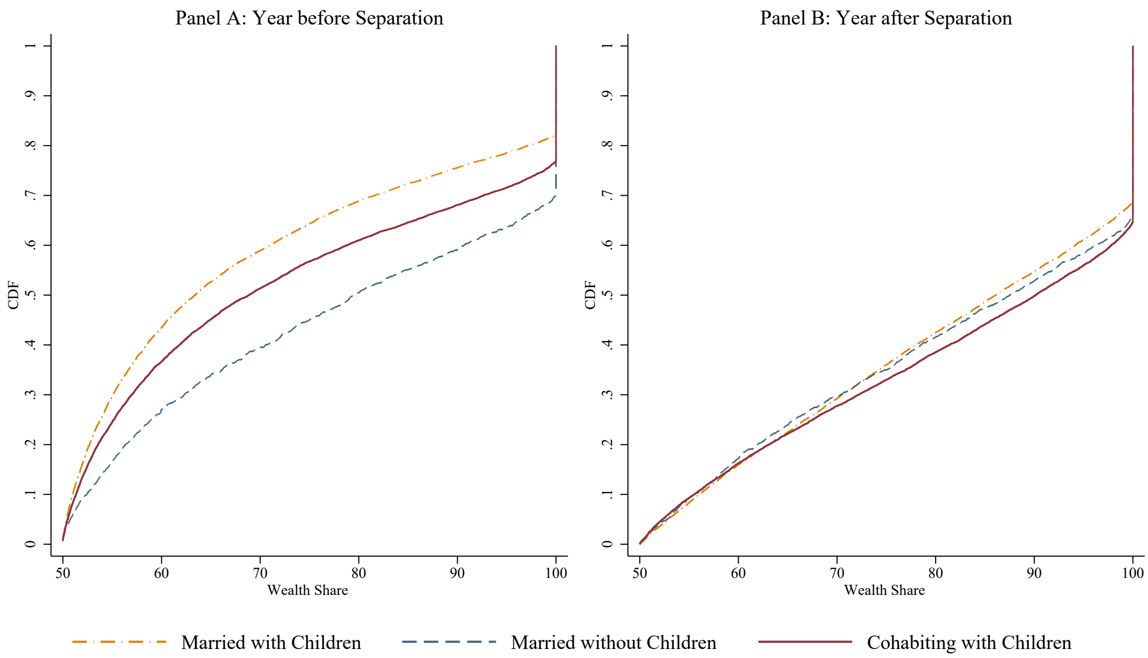
In order to quantify the significance of non-equal splits, we rely on descriptive evidence from the Swedish Wealth Registry 1999-2007. We restrict the sample to couples with a total net wealth of at least 100,000 SEK, who have been a couple for at least three years, and where both partners are between 25 and 44 years old. We differentiate between three different types of couples: married with or without children, and cohabiting couples with joint children. We include cohabiting couples in our analysis because the rules governing asset division of separating cohabitants does not impose an equal split of all assets.[11] Whether divorcing married couples split assets more equally than separating cohabitants is thus an indication of the extent to which the institution of marriage impact sharing of

[11] According to Swedish law, common property for cohabiting couples only include dwellings and associated property (e.g., furniture) procured for the purpose of living together. Property procured prior to cohabitation is not common property. As for married couples, cohabiting couples can regulate what is to be considered common property in a contract.

wealth between partners.

Figure 3 shows the wealth share of the wealthiest partner (at each point in time) the year prior to (panel A) and the year after (panel B) separation. Panel A shows within-couple wealth inequality prior to separation is often high. For instance, in 30% of married couples with children the wealthiest spouse holds at least 80% of total wealth, and inequality is higher for couples who are married without children or cohabit. Panel B shows measured wealth inequality is even higher after separation. Perhaps surprisingly, married couples do not seem to split their assets substantially more equally than cohabiting couples.

Figure 3: CDF of Wealth Shares of Wealthiest Partners



Notes. This figure shows the estimated cumulative distribution function (CDF) of the wealth share of the wealthiest partners in a couple, for the year before and after separation. The data is for couples where both partners are between the ages of 25 and 44 at the time of separation, and where the total combined wealth of the couple is above 100,000 SEK.

Because the identity of the wealthiest partner in a couple may change between Panel A (pre-separation) and B (post-separation), Figure 3 does not rule out redistribution of wealth from the wealthier partner at the time of separation. To estimate the extent of such redistribution, we regress each partner's post-separation share of assets on the pre-separation share. A coefficient of 1 implies the richer partner keeps his or her full share of assets post-divorce. A coefficient of 0 is consistent with an equal split of assets, but also with any other settlement where the post-separation wealth share is independent of the pre-separation wealth level. Table A.13 shows the coefficients from this regression are 0.62 for married couples with children, 0.68 for married couples without children and 0.75 for cohabiting couples. For each type of couple, we reject both equal splits (a coefficient of 0) and complete lack of redistribution from the richer to the poorer partner (a coefficient of 1). Although redistribution is higher for married couples, the small difference compared to cohabiting couples indicates marriage has a modest impact on the division of assets in divorce.

Appendix G.3 discusses the robustness of the Wealth Registry analyses discussed above. In particular, we argue idiosyncratic measurement error in asset values implies Figure 3 Panel B likely overstates wealth inequality after separation, but that such measurement error has limited implications for measured wealth inequality pre-separation and the estimated redistribution from the richer to the poorer spouse.

The Allocation of Lottery Wealth Within Marriage

Our empirical results suggest which spouse wins the lottery matters also for the allocation of consumption within the marriage. Table A.14 shows the winning spouse retains about two thirds of the lottery wealth and Cesarini et al. (2017) document stronger negative labor supply responses of the winning spouse. As shown in Section F.2, these empirical results are consistent with a divorce-threat model where the winner retains most of the lottery wealth in case of divorce.[12] The reason is the stronger outside option of the winning spouse increase bargaining power also inside the marriage.[13]

In contrast, a model with a male breadwinner norm and equal divorce-splits generates a larger increase in the winner's consumption only if the wife espouse the breadwinner norm more strongly than the husband. To see why, first note the spouse for which the norm is stronger will see a larger increase in utility when the husband wins, and a smaller increase (or a decrease) when the wife wins, for a given change in consumption.

[12] As shown in Section F.2, how large the winners' share of lottery wealth in divorce has to be in order for the winner to always consume more also within the marriage depends on spouses' marginal utilities of consumption in the single and married state.

[13] As pointed out by Cesarini et al. (2017), the non-equal division of lottery wealth might also be consistent with models with threat points internal to the marriage.

The norm-abiding spouse must thus receive a larger increase in consumption when the wife wins and a smaller increase when the husband wins for the marital surplus to be shared equally (which in turn follows from the assumption of equal splits of property in divorce). If the wife is more norm-abiding, her consumption must thus increase more when she wins the lottery, but less when her husband wins. If instead the husband is more norm-abiding, the breadwinner norm implies the consumption of the *non-winning* spouse should increase more, a prediction that contrasts sharply to the pattern in the data.

Exploratory Heterogeneity Analyses

We run a number of exploratory heterogeneity analyses to better understand the differential effects on divorce risk. Figure A.4 shows that the short-run effect of the wife's lottery wealth on the divorce probability is positive and statistically distinguishable from zero only in the couples where the wife earns less than the husband (Panel A) or below the sample median (Panel B), suggesting the treatment effect is stronger for women who are more likely to be financially dependent on their husband. These results thus support the notion that wives' lottery wins may increase short-term divorce risk because it gives some women the financial means to pursue a life as singles. Panel B of Figure A.4 also shows the reduction in divorce risk from husbands' lottery wealth is larger in couples where the husband earns below the sample median ($\hat{\beta}_5 = -6.36, SE = 2.57$) compared to couples where the husband earns above the sample median ($\hat{\beta}_5 = -1.93, SE = 1.77$), consistent with low economic status being detrimental to men's attractiveness as marital partners.

Beyond the divorce-threat model we discuss above, a general prediction of many marriage market models is that lottery wealth will facilitate or delay termination of marriages whose dissolution was already underway, while the effect on "stable" marriages will be negligible. To test this hypothesis, we test whether the effect of wife's wealth on marriage dissolution is stronger in (i) couples where the match quality is poor than in better matched couples and (ii) in newly married couples than in longer-term married couples. In order to analyze how the effect on wife's wealth on marriage dissolution varies with the match quality, we construct a proxy for the match quality defined as the absolute value of the difference in spouses age and education (larger distance suggesting lower match quality).[14] Panels C of Figure A.4 shows the estimated short-run effect on divorce when women win is positive in couples with match quality below the sample median but negative in couples with match quality above the sample median. Panel D of A.4 shows the effect of the wife's lottery wealth on the divorce probability by marriage duration, indicating that the short and the medium-run lottery effect is bigger in newly married couples (married for 3 years or less at the moment of win), though we cannot reject that the effects are identical. We do not find that the impact of a husband's wealth on marriage dissolution varies with the match quality or the length of the marriage.

6.3 Fertility

Following the seminal work of Becker (1960), children are often introduced into economic models as normal durable goods. However, the assumption that children are normal goods is seemingly at odds with the fact that economic growth has coincided with a transition to lower fertility in many countries (Doepke, 2004; Clark, 2005; Galor, 2005; Bar and Leukhina, 2010; Ager et al., 2020). Cross-sectional studies have not reached a consensus on the effect of income on fertility, with some studies suggesting that income and fertility are positively correlated (Anderson, 2008; Black et al., 2013; Kolk, 2019) and others showing that there is a negative association (Jones and Tertilt, 2008).

One possible explanation for the observed negative relationship between income and fertility is that wage increases may have two offsetting effects for parental decisions to have children. On the one hand, the income effect is expected to increase fertility if children are "normal" goods. On the other hand, higher wages imply higher opportunity cost of time and an increase in the "shadow price" of raising children, which may reduce the optimal level of fertility (the substitution effect).

Testing whether the income effect is positive is challenging. As noted by Hotz et al. (1997), labor market and fertility decisions are taken simultaneously and, therefore, the income-fertility association cannot be interpreted as a causal relationship. Moreover,

[14] Becker (1973) predicts that there is positive assortative mating with respect to complementary traits (e.g., education or physical attractiveness), while the negative assortative mating with respect to substitutionary traits, such as earning power, would be optimal. Therefore, the distance in complementary traits can be informative about the match quality. In our sample of pre-lottery married players, husband's and wife's education are positively correlated ($\hat{\rho} = 0.46$) while the correlation between husband's and wife income is small and negative ($\hat{\rho} = -0.04$).

studies which rely on exogenous variation in wages generally cannot separate the income effect from the substitution effect. In order to confront these challenges, some studies estimate the income effect on fertility exploiting income shocks generated by husband's job displacement (Amialchuk, 2013; Lindo, 2010) or other exogenous shocks that affect men's labor income (Black et al., 2013; Schaller, 2016; Kearney and Wilson, 2018; Autor et al., 2019), assuming that men do not contribute to home production and therefore the effect on fertility is due to the income effect rather than to the changes in the "shadow price" of raising children. The evidence usually indicate that fertility increases with men's income, though there are exceptions (Huttunen and Kellokumpu, 2016). Further studies have found increases in housing wealth (Lovenheim and Mumford, 2013) and lottery winnings (see the discussion in Section 5.2) to increase fertility.

Our results are consistent with children being normal goods, as we find clear evidence that lottery wealth increases fertility in the pooled sample and in the subsample of male winners. However, a back-of-the-envelope calculation suggests about 20-40 percent of male winner's fertility response can be accounted for by their higher marriage rate.[15] Moreover, if men's improved marriage prospects are mainly explained by them becoming relatively more attractive partners compared to other men, a general increase in income may have no effect on marriage rates.

The gendered fertility effects may suggest men and women have different fertility preferences. The agreement between partners is an important determinant of fertility (Doepke and Kindermann, 2019), and, as discussed in Section 6.2, lottery wealth may increase the bargaining power of the winner. In this case, our results would be consistent with men preferring to have more children than women. However, according to the OECD, the av-

[15] We calculate the change in fertility t years after the lottery, $\triangle C_t$, as

$$\triangle C_t = \hat{\beta}_t^m \triangle C_t^m (1 - s^m) + \hat{\beta}_t^d \triangle C_t^d s^m$$

where $\hat{\beta}_t^m$ and $\hat{\beta}_t^d$ are the estimated effects of lottery wealth on marriage and divorce probabilities t years after the lottery event, $\triangle C_t^m$ and $\triangle C_t^d$ denote the difference in the number of post-lottery children between pre-win unmarried men who are married at t and pre-win married men who divorce at t, and s^m denotes the share of men who are married at the time of the lottery event. Comparing $\triangle C_t$ to the estimated effect of lottery wealth on male winners' fertility indicates 21.5% ($t = 2$), 42.9% ($t = 5$) and 29.8% ($t = 10$) of the fertility response can be accounted for by the effect of lottery wealth on marital formation and dissolution. The validity of the calculation above hinges upon the strong assumption that the effects of lottery wealth on men's marriage formation and marital stability do not reflect an effect on desired fertility.

erage ideal number of children in Sweden is similar for men and women, suggesting there is no gender differences in preferences for children.[16] Moreover, to the extent the effect on male fertility is explained by increased marriage formation and stability, the fertility response reflects a relaxed constraint rather than preferences for children. In addition, the larger gender differences in the fertility response we found for older winners could be due to declining female fertility with age. In sum, the gendered effects we document may reflect the different constraints on fertility facing men and women rather than differences in preferences for children.

7 Conclusion

Our study leverages the randomized assignment of lottery prizes to estimate the effects of wealth on three important family outcomes – marriage formation, marriage dissolution, and fertility. Our estimates have strong internal validity and advance understanding of the broader question of how wealth affects family outcomes by providing credible and precise estimates for a large sample of Swedish lottery players. We find that lottery wealth increases the short- and medium-run probabilities of marriage. The overall wealth effect on marriage formation is driven by male winners. These gender asymmetries are consistent with previous literature that documents gender differences in partner selection, and indicates that women put higher weight on economic characteristics of potential partners than men do (Fisman et al., 2006; Hitsch et al., 2010b,a).

While the overall average treatment effect on marriage dissolution is not statistically distinguishable from zero, there is a consistent pattern of divergence between the estimated effects for husbands and wives. Specifically, when the winning player is a married woman, our estimates suggest that a 1M-SEK windfall almost doubles the baseline short-run divorce rate. This estimated effect appears to fade away in the long-run. We speculate that the positive wealth shock accelerates the exit from marriages whose dissolution was already underway. In contrast, the effect on long-term divorce risk in the sample of male

[16] According to the OECD, the average personal ideal number of children for Swedish women aged 15-64 is 2.41 while it is 2.33 for men. This difference is similar to the difference in other OECD countries. See OECD Family Database available at `http://www.oecd.org/els/family/database.htm`. The estimates are based on data from Eurobarometer 2011: Fertility and Social Climate.

winners is negative.We show a divorce-threat model where the winning spouse retains the bulk of the lottery prize in case of divorce can rationalize our findings, and present suggestive empirical evidence in support of this explanation. In particular, we show divorce settlements in Sweden are often unequal and favor the richer spouse.

Finally, consistent with theoretical models that introduce children as normal goods (Becker, 1960), we find that the evidence from quasi-experimental studies of lottery players as a whole Tsai et al. (2022); Bulman et al. (2022); Bleakley and Ferrie (2016) is consistent with small but positive wealth effects on completed fertility. This conclusion is also in line with the broader quasi-experimental literature (Black et al., 2013; Lindo, 2010; Lovenheim and Mumford, 2013). The positive effect on fertility in our sample appears to be driven by male winners and appears to be partly explained by the increase in marriage formation for men. If the increase in men's fertility is due to increased attractiveness in the marriage market relative to other men, the positive wealth effects we estimate are likely upper bound for the effects one should expect in response to a more general upward shift in overall living standards and wealth.

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Online Appendix

Fortunate Families? The Effects of Wealth on Marriage and Fertility

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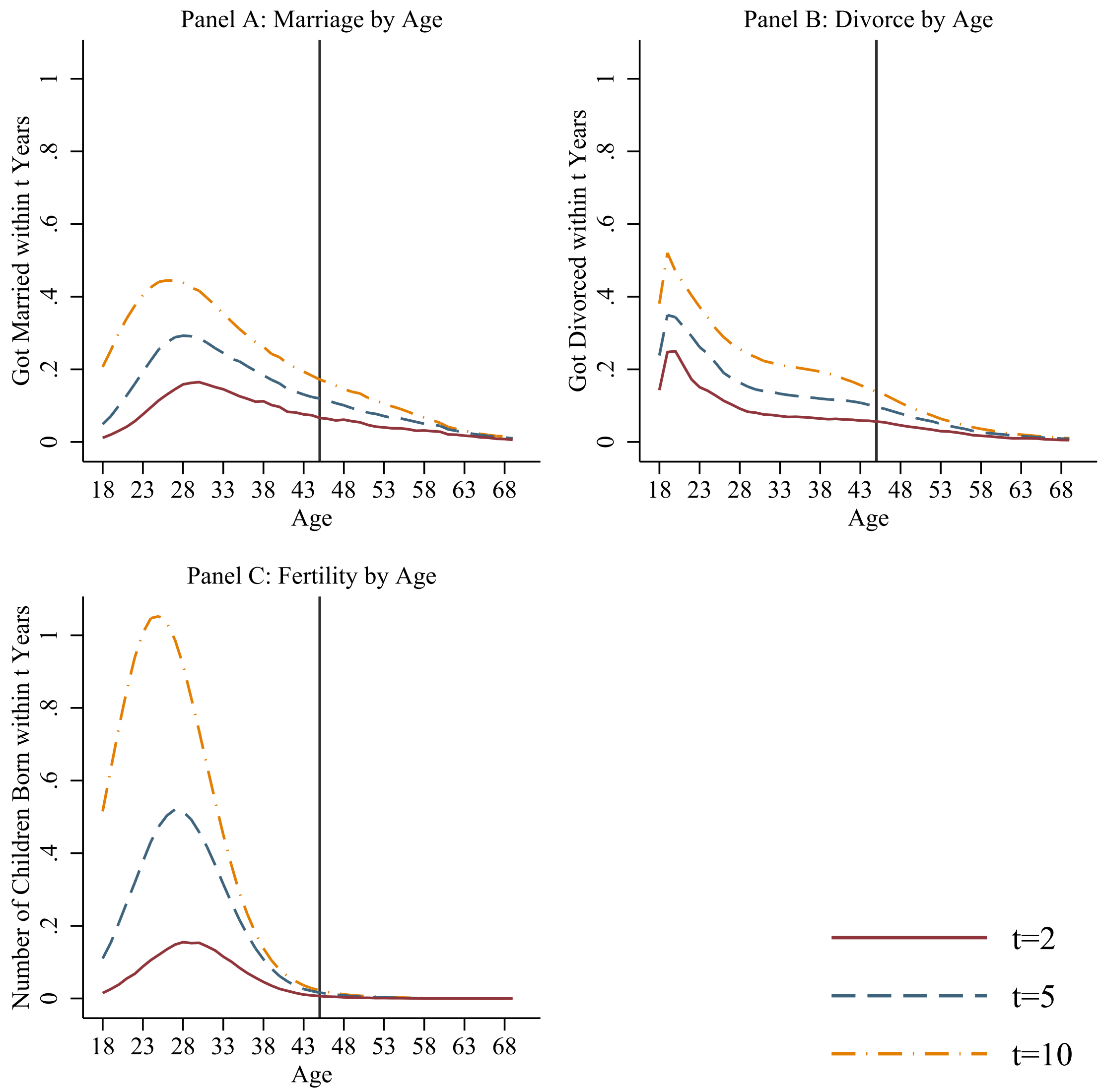
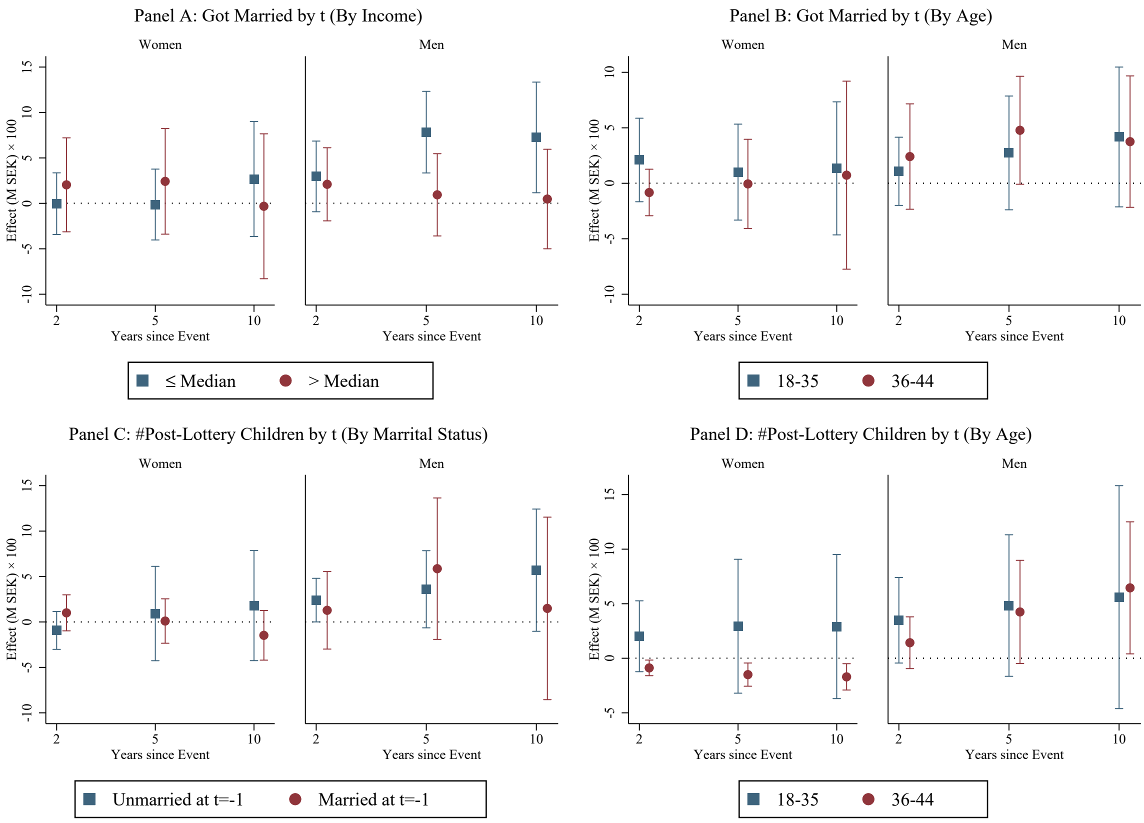
Appendix A Tables and Figures

Figure A.1: Outcome Variables by Age



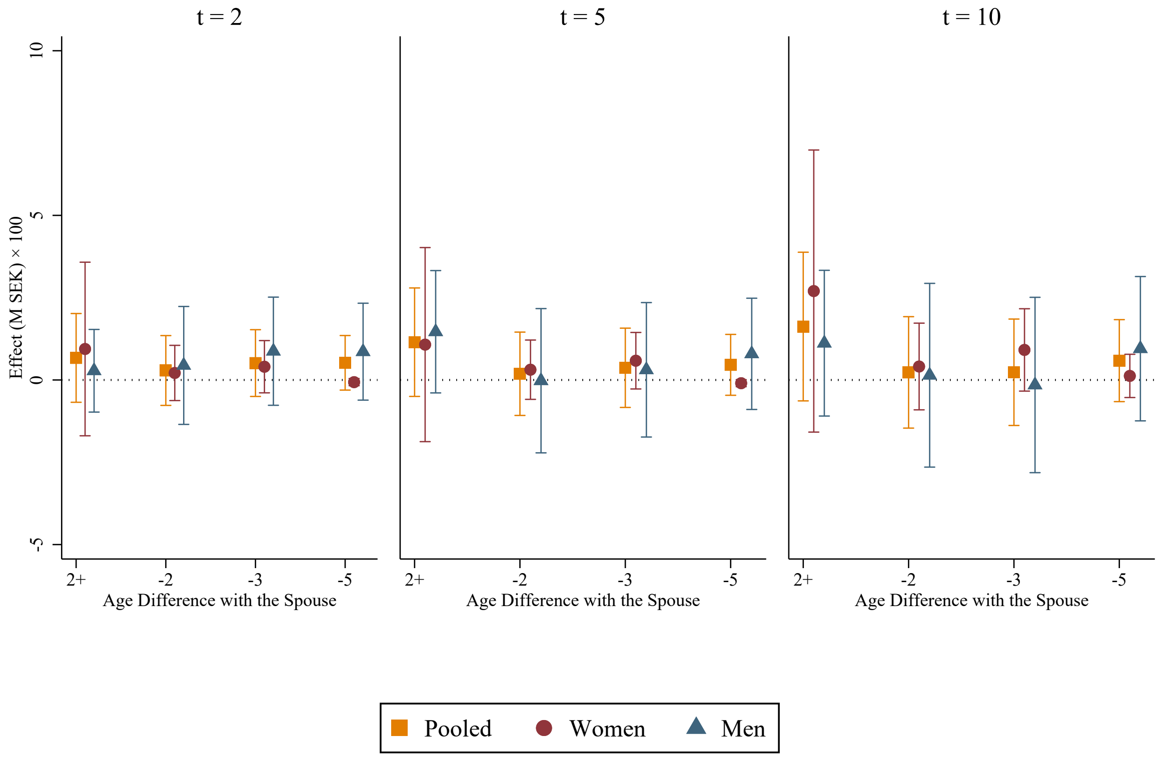
Note. The figure plots the average probability to get married (in unmarried individuals), to get divorced (in married individuals), and the average number of children born within 2, 5, and 10 years by age. The estimates are based on 1990 and 2000 Swedish represenative samples of unmarried individuals.

Figure A.2: Heterogeneous Effect of Wealth



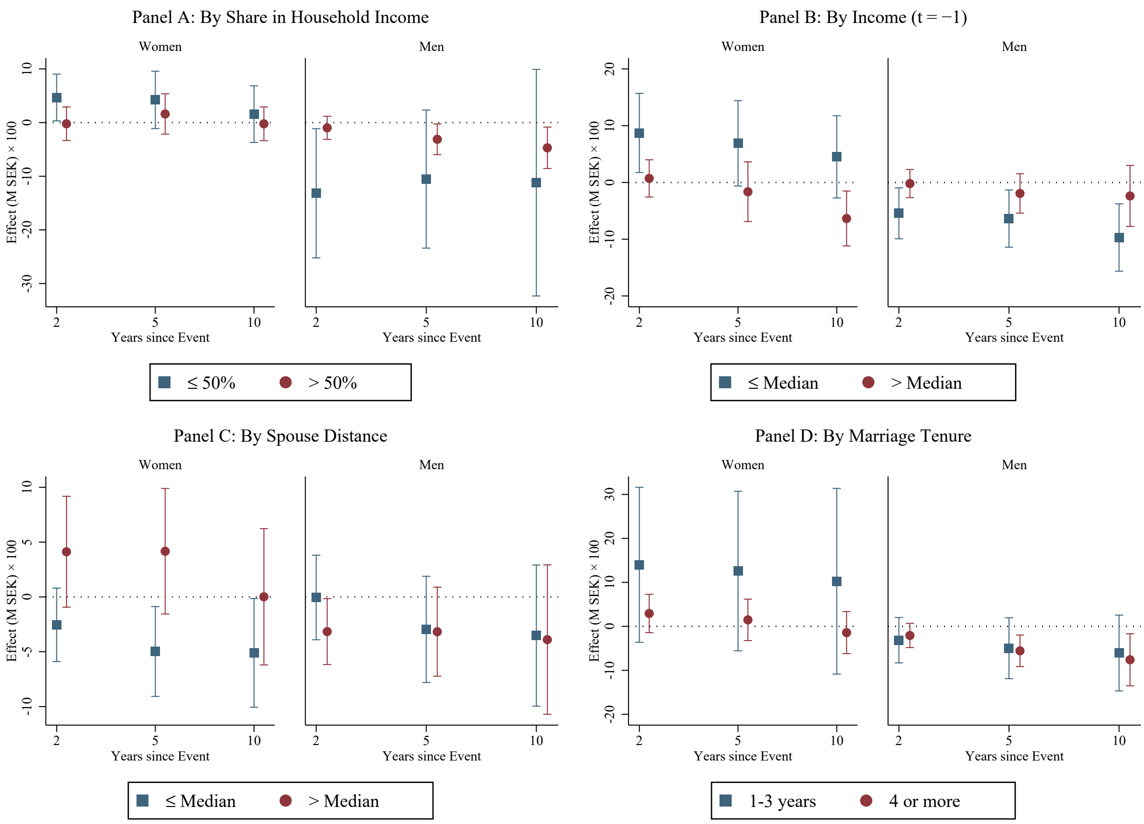
Notes. This figure reports the estimated treatment effect of a million SEK on the probability to get married for unmarried players by winners income at $t = -1$ and age-at-win (Panels A and B), and on the number of post-lottery children by winner's pre-lottery marital status and age-at-win (Panels C and D), all measured at year-end in 2, 5, and 10 years after the lottery. All specifications control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered by individual, and the error bar corresponds to 95 percent analytical confidence intervals. Age-at-win: 18-44.

Figure A.3: The Effect of Wealth on Marrying a Younger/Older Spouse



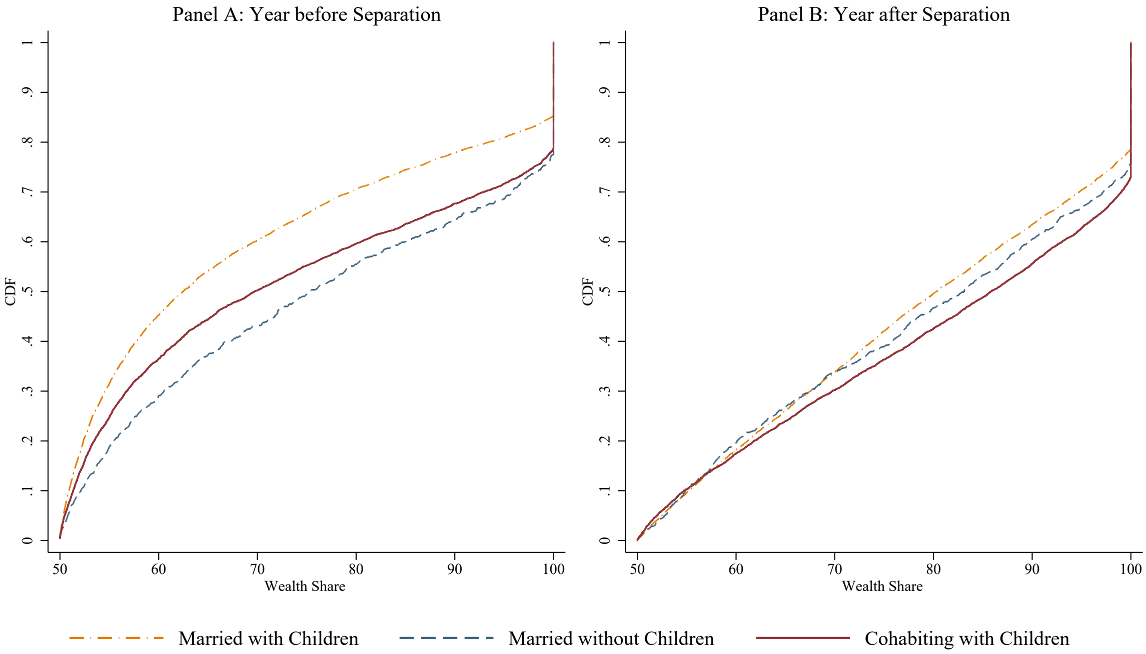
Notes. This figure reports the estimated treatment impact of a million SEK on the probability of getting married to a spouse who is at least 2 years older (+2), 2 years younger (-2), 3 years younger (-3), or 5 years younger (-5) for unmarried individuals at the time of the lottery ($t = -1$), measured at year-end in 2, 5, and 10 years after the lottery. All specifications control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered by individual, and the error bar corresponds to 95 percent analytical confidence intervals. Age-at-win: 18-44.

Figure A.4: Heterogeneous Effects of Wealth on Divorce (Married at $t = -1$)



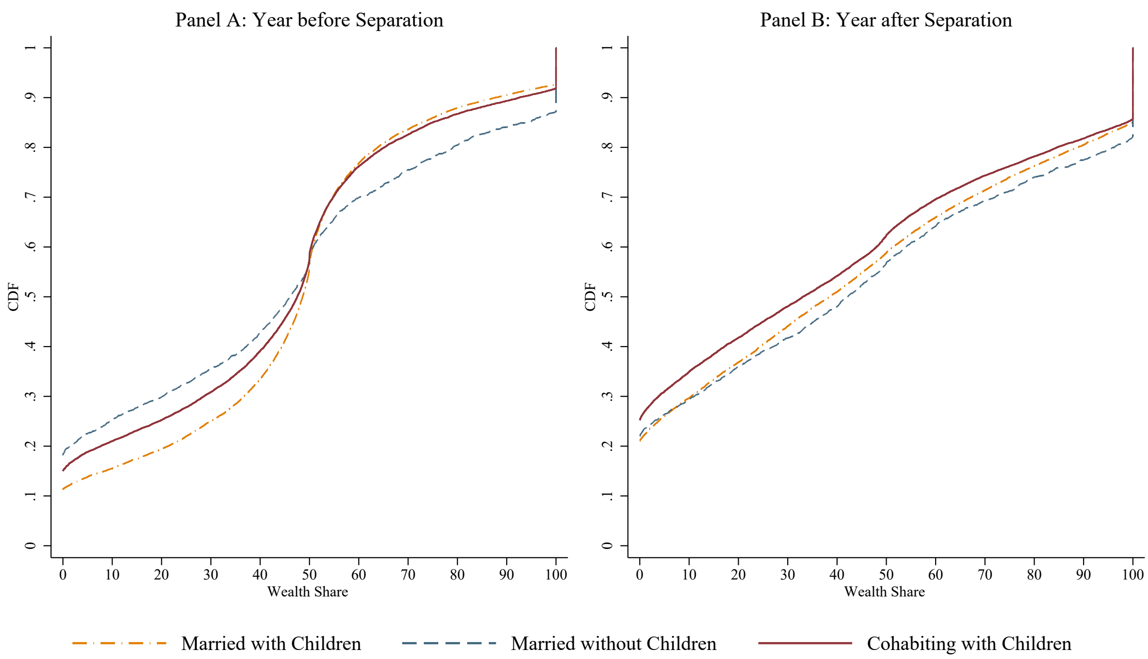
Notes. This figure reeports the estimated treatment effect of lottery wealth on the probability to get divorced by year-end of $t = 2$, $t = 5$ and $t = 10$ on men and women. Share in household income is computed as winner's income at $t = -1$ devided by the sum of own and spouse's income at $t = -1$. Spouse distance is defined as a sum of normalized distances between husband's and wife's age-at-win, and college graduation status at $t = -1$. Marriage tenure is defined as number of years married at $t = -1$. Standard errors are clustered by individual, and the error bar corresponds to 95 percent analytical confidence intervals. Age-at-win: 18-44.

Figure A.5: Cumulative Distribution of Wealth Shares of Wealthiest Partners. Total Net Wealth $\geq$ 500K SEK.



Notes. This figure shows the estimated cumulative distribution function (CDF) of the wealth share of the wealthiest partners in a couple, for the year before and after separation. The data is for couples where both partners are between the ages of 25 and 44 at the time of separation, and where the total combined wealth of the couple is above 500,000 SEK.

Figure A.6: Cumulative Distribution of Wealth Shares of Women.



Notes. This figure shows the estimated cumulative distribution function (CDF) of the wealth share for woman in a couple, for the year before and after separation. The data is for couples where both partners are between the ages of 25 and 44 at the time of separation, and where the total combined wealth of the couple is above 100,000 SEK.

Table A.1: Summary Statistics for Main Outcome Variables

	(1)	(2)	(3)	(4)	(5)	(6)
			Triss...			
	PLS	Kombi	Lumpsum	Monthly	Pooled Sample	
Marriage	Mean	Mean	Mean	Mean	Mean	N
$t = 2$	0.09	0.09	0.09	0.17	0.09	53,805
$t = 5$	0.17	0.16	0.19	0.23	0.17	53,191
$t = 10$	0.27	0.26	0.30	0.33	0.27	51,867
Divorce						
$t = 2$	0.04	0.07	0.07	0.05	0.04	33,994
$t = 5$	0.08	0.15	0.15	0.13	0.08	33,740
$t = 10$	0.14	0.22	0.24	0.16	0.15	33,094
Fertility	Mean/SD	Mean/SD	Mean/SD	Mean/SD	Mean/SD	N
$t = 2$	0.07/0.26	0.06/0.25	0.08/0.28	0.09/0.29	0.07/0.26	88,113
$t = 5$	0.21/0.50	0.18/0.47	0.26/0.56	0.20/0.50	0.21/0.50	87,635
$t = 10$	0.40/0.74	0.32/0.68	0.47/0.79	0.33/0.70	0.39/0.74	86,109

Notes. Sample averages are reported. The time of the lottery is normalized to $t = 0$. *Marriage* is defined for players unmarried at $t = -1$. For these lottery players, *Marriage* is an indicator variable equal to one if at least one marriage event was recorded for the player between the start of the lottery year and the end of year $t = 2, 5, 10$. *Divorce* is defined for players married at $t = -1$. For these lottery players, *Divorce* is equal to one if the player's $t = -1$ marriage had been dissolved by year-end in $t = 2, 5, 10$. *Fertility* is reported for the pooled lottery sample. *Fertility* is the total number of children born between lottery year and the end of year $t = 2, 5, 10$.

Table A.2: Randomization Tests

	(1)	(2)	(3)	(4)	(5)	(6)
			Triss...			
	PLS	Kombi	Lumpsum	Monthly	Pooled Sample	
Fixed Effects	Group ID	Group ID	Group ID	Group ID	Group ID	None
N	82,870	3,410	1,580	266	88,126	88,126
Baseline Covariates ($t = -1$)						
Age-at-Win	0.432	0.192	0.663	1.705	1.679	1.914
p (analytical)	[0.666]	[0.847]	[0.507]	[0.089]	[0.093]	[0.056]
Age-at-Win^2	0.328	0.381	0.708	1.568	1.598	1.481
p (analytical)	[0.743]	[0.704]	[0.479]	[0.118]	[0.110]	[0.139]
Age-at-Win^3	0.232	0.530	0.731	1.435	1.503	1.118
p (analytical)	[0.817]	[0.596]	[0.465]	[0.152]	[0.133]	[0.263]
1 if Female	1.256	−0.175	0.543	0.487	1.220	−0.376
p (analytical)	[0.209]	[0.861]	[0.587]	[0.627]	[0.222]	[0.707]
1 if Nordic Born	1.185	−0.620	−0.677	0.904	0.020	−3.290
p (analytical)	[0.236]	[0.536]	[0.499]	[0.367]	[0.984]	[0.001]
# Children	0.159	−2.048	0.861	0.364	0.592	1.783
p (analytical)	[0.874]	[0.041]	[0.389]	[0.716]	[0.554]	[0.075]
1 if College	−0.427	0.543	1.277	−0.113	0.670	−1.302
p (analytical)	[0.669]	[0.587]	[0.202]	[0.910]	[0.503]	[0.193]
1 if Married	0.944	−0.433	−0.142	0.207	0.406	−2.197
p (analytical)	[0.345]	[0.665]	[0.887]	[0.836]	[0.685]	[0.028]
Joint Test of Baseline Covariates						
$F - statistic$	0.719	1.242	0.507	0.761	0.444	7.882
p (analytical)	[0.656]	[0.270]	[0.852]	[0.637]	[0.875]	[< 0.001]
p (resampling)	[0.746]	[0.752]	[0.696]	[0.851]	[0.926]	[< 0.001]

Notes. Each column corresponds to a regression where the dependent variable is the size of the lottery prize won and the controls are the baseline characteristics measured prior to the lottery. Under the null hypothesis of conditional random assignment, variables determined before the lottery should not have any predictive power conditional on the group-identifier fixed effects. The table shows t statistics, i.e. coefficients divided by their standard error. The resampling based p-values are obtained from the resampling distribution of covariate coefficients from 1000 Monte Carlo simulations. In each simulation, we permute the prizes within each group.

Table A.3: Wealth Effects on Marriage, Divorce and Fertility. Age-at-win: 18-64

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$t=2$			$t=5$			$t=10$		
		By Sex			By Sex			By Sex	
	All	F	M	All	F	M	All	F	M
Panel A: Got Married by t (Unmarried at $t=-1$)									
Effect × 100	1.955	0.446	2.971	2.714	0.749	4.020	2.265	0.834	3.578
SE	(0.710)	(0.847)	(1.001)	(0.826)	(1.066)	(1.217)	(1.057)	(1.488)	(1.563)
p (analytical)	[0.006]	[0.599]	[0.003]	[0.001]	[0.482]	[<0.001]	[0.032]	[0.575]	[0.022]
p (resampling)	[0.003]	[0.615]	[<0.001]	[<0.001]	[0.499]	[0.002]	[0.017]	[0.577]	[0.006]
p (FDR)	[0.017]	[0.749]	[0.001]	[0.001]	[0.749]	[0.014]	[0.042]	[0.749]	[0.027]
Baseline Mean	0.060	0.060	0.060	0.106	0.103	0.109	0.174	0.167	0.179
Relative Risk	0.325	0.075	0.491	0.256	0.073	0.370	0.131	0.050	0.200
N	108.098	48.310	59.788	106.161	47.607	58.554	99.793	45.291	54.502
Heterogeneity p		[0.054]			[0.043]			[0.204]	
Panel B: Got Divorced by t (Married at $t=-1$)									
Effect × 100	0.864	1.594	0.086	0.265	0.787	-0.388	-0.357	-0.026	-0.798
SE	(0.411)	(0.657)	(0.499)	(0.488)	(0.738)	(0.663)	(0.551)	(0.796)	(0.824)
p (analytical)	[0.036]	[0.015]	[0.863]	[0.587]	[0.287]	[0.559]	[0.518]	[0.974]	[0.333]
p (resampling)	[0.012]	[<0.001]	[0.869]	[0.609]	[0.231]	[0.637]	[0.555]	[0.968]	[0.418]
p (FDR)	[0.033]	[0.009]	[0.903]	[0.749]	[0.416]	[0.749]	[0.749]	[0.969]	[0.705]
Baseline Mean	0.022	0.020	0.025	0.042	0.038	0.046	0.067	0.061	0.073
Relative Risk	0.387	0.787	0.035	0.064	0.208	-0.085	-0.053	-0.004	-0.109
N	148.451	78.154	70.297	146.195	77.193	69.002	139.007	74.280	64.727
Heterogeneity p		[0.068]			[0.237]			[0.500]	
Panel C: #Post-Lottery Children by t (Age-at-win: 18-44)									
Effect × 100	1.209	0.214	2.048	2.915	0.865	4.278	3.310	0.777	5.583
SE	(0.650)	(0.767)	(0.993)	(1.252)	(1.645)	(1.847)	(1.660)	(1.886)	(2.659)
p (analytical)	[0.063]	[0.780]	[0.039]	[0.020]	[0.599]	[0.021]	[0.046]	[0.680]	[0.036]
p (resampling)	[0.048]	[0.819]	[0.008]	[0.010]	[0.598]	[0.012]	[0.043]	[0.727]	[0.020]
p (FDR)	[0.118]	[0.851]	[0.051]	[0.051]	[0.736]	[0.051]	[0.116]	[0.819]	[0.068]
Baseline Mean	0.068	0.067	0.070	0.208	0.197	0.218	0.393	0.369	0.415
Relative Risk	0.177	0.032	0.294	0.140	0.044	0.196	0.084	0.021	0.135
N	88.113	41.539	46.574	87.635	41.319	46.316	86.109	40.700	45.409
Heterogeneity p		[0.144]			[0.168]			[0.140]	

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get married, to get divorced, and on the number of post-lottery children by year-end of t = 2, t = 5 and t = 10 for the pooled lottery sample and by gender. All specification control for baseline controls measured at $t=-1$ and group-identifier fixed effects. Standard errors are clustered at the individual level. Baseline mean is defined as the mean of the dependent variable of small-prize winners ($< 10,000$ SEK). The resampling based p-values are obtained from the resampling distribution of coefficients from 1,000 Monte Carlo simulations. $p(FDR)$ correspond to the false discovery rate adjusted resampling p-values computed using (Benjamini et al., 2006) and (Anderson, 2008) procedure. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Age-at-win: 18-64 (Panels A and B) and 18-44 (Panel C).

Table A.4: Non-Linear Wealth Effects on Marriage, Divorce and Fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$t = 2$			$t = 5$			$t = 10$		
	All	By Sex		All	By Sex		All	By Sex	
		F	M		F	M		F	M
Panel A: 1 if Got Married by t (Unmarried at $t = -1$)									
Excluding Prizes>4M									
Effect × 100	2.738	0.394	4.877	3.065	0.788	5.438	2.392	-0.875	5.516
SE	(1.486)	(2.198)	(2.042)	(1.721)	(2.687)	(2.361)	(2.026)	(3.217)	(2.767)
p (analytical)	[0.065]	[0.858]	[0.017]	[0.075]	[0.769]	[0.021]	[0.238]	[0.786]	[0.046]
p (resampling)	[0.052]	[0.849]	[0.012]	[0.073]	[0.799]	[0.022]	[0.271]	[0.805]	[0.063]
N	53.756	22.613	31.143	53.143	22.355	30.788	51.823	21.841	29.982
PLS Prizes<10K									
Effect × 100	1.920	0.537	2.015	2.812	-0.098	3.811	2.085	0.144	2.236
SE	(1.172)	(1.540)	(1.395)	(1.293)	(1.759)	(1.823)	(1.670)	(2.637)	(2.329)
p (analytical)	[0.102]	[0.727]	[0.149]	[0.030]	[0.956]	[0.037]	[0.212]	[0.956]	[0.337]
p (resampling)	[0.048]	[0.733]	[0.119]	[0.019]	[0.963]	[0.031]	[0.158]	[0.953]	[0.271]
N	7.515	2.918	4.597	7.442	2.895	4.547	6.898	2.713	4.185
Panel B: 1 if Got Divorced by t (Married at $t = -1$)									
Excluding Prizes>4M									
Effect × 100	3.942	6.103	1.131	2.777	4.295	0.559	0.149	0.613	-1.638
SE	(1.781)	(2.629)	(2.341)	(1.955)	(2.800)	(3.189)	(2.025)	(2.800)	(3.993)
p (analytical)	[0.027]	[0.020]	[0.629]	[0.156]	[0.125]	[0.861]	[0.941]	[0.827]	[0.682]
p (resampling)	[0.006]	[<0.001]	[0.623]	[0.142]	[0.106]	[0.879]	[0.946]	[0.858]	[0.741]
N	33.965	18.736	15.229	33.713	18.604	15.109	33.071	18.308	14.763
PLS Prizes<10K									
Effect × 100	1.239	3.978	-2.161	-0.723	3.230	-5.120	-1.962	0.819	-6.946
SE	(1.288)	(2.046)	(1.087)	(1.650)	(2.681)	(1.567)	(1.805)	(2.750)	(2.159)
p (analytical)	[0.336]	[0.052]	[0.047]	[0.661]	[0.228]	[0.001]	[0.277]	[0.766]	[0.001]
p (resampling)	[0.215]	[<0.001]	[0.290]	[0.667]	[0.085]	[0.056]	[0.358]	[0.738]	[0.039]
N	4.533	2.316	2.217	4.494	2.300	2.194	4.235	2.197	2.038
Panel C: Number of Post-lottery Children at t									
Excluding Prizes>4M									
Effect × 100	1.878	1.597	2.439	1.606	-0.058	3.497	1.499	-2.227	6.042
SE	(1.007)	(1.471)	(1.447)	(1.535)	(2.138)	(2.284)	(2.412)	(2.917)	(3.868)
p (analytical)	[0.062]	[0.278]	[0.092]	[0.296]	[0.978]	[0.126]	[0.534]	[0.445]	[0.118]
p (resampling)	[0.044]	[0.247]	[0.083]	[0.343]	[0.978]	[0.207]	[0.542]	[0.519]	[0.117]
N	88.035	41.502	46.533	87.559	41.283	46.276	86.041	40.666	45.375
PLS Prizes<10K									
Effect × 100	1.003	-0.277	1.710	2.546	-0.438	3.922	2.987	-0.493	5.170
SE	(0.687)	(0.762)	(1.089)	(1.364)	(1.799)	(2.048)	(1.822)	(2.072)	(2.992)
p (analytical)	[0.144]	[0.717]	[0.117]	[0.062]	[0.808]	[0.056]	[0.101]	[0.812]	[0.084]
p (resampling)	[0.114]	[0.771]	[0.058]	[0.028]	[0.802]	[0.020]	[0.085]	[0.830]	[0.050]
N	12.086	5.251	6.835	12.030	5.235	6.795	11.280	4.968	6.312

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get married, to get divorced, and on the number of post-lottery children at year-end in $t = 2$, $t = 5$ and $t = 10$ in the subsample of lottery players that excludes large prizes (>4M SEK) and small prizes (<10,000 SEK). All specification control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered at the level of the individual. The resampling based p-values are obtained from the resampling distribution of coefficients from 1000 Monte Carlo simulations. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Age-at-win 18-44.

Table A.5: Summary Statistics: Pre and Post Entropy-Balancing Reweighting (Married $t = -1$)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Before Reweighting				After Reweighting			
	Husband Wins		Wife Wins		Husband Wins		Wife Wins	
	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance
Husband's Age	37.409	20.491	40.085	37.451	38.883	31.623	38.886	31.631
Wife's age	35.569	29.590	36.853	24.241	36.276	27.057	36.277	27.049
1 if Husband College	0.315	0.216	0.318	0.208	0.316	0.216	0.316	0.211
1 if if Wife College	0.353	0.211	0.345	0.226	0.349	0.219	0.341	0.225
Husband's Income	287.028	18373.710	284.914	20989.084	285.857	19816.418	285.861	19817.896
Wife's Income	159.292	8024.935	170.088	8023.435	165.241	8052.897	165.246	8053.111
# of Children	1.995	1.087	2.016	1.038	2.006	1.060	2.007	1.060
PLS	0.936	0.059	0.963	0.035	0.951	0.046	0.951	0.046
Kombi	0.045	0.043	0.020	0.020	0.032	0.031	0.032	0.031
Triss Monthly	0.003	0.003	0.002	0.002	0.002	0.002	0.002	0.002

Notes. This table reports the summary statistics of pre-lottery characteristics (measured at $t = -1$) in the sample of couples where wives won and where husbands won. The estimates in Columns (5-8) correspond to the reweighted samples where weights are constructed using entropy balancing procedure that matches first and second moments of characteristics in each subsample with the corresponding moments in the polled sample of married at $t = -1$ lottery winners. Only pre-lottery married individuals between 18 and 44 years old at the moment of win are included.

Table A.6: Wealth Effects on Marital Dissolution: Reweighted (Married at $t = -1$)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$t = 2$			$t = 5$			$t = 10$		
	All	By Sex		All	By Sex		All	By Sex	
		F	M		F	M		F	M
Effect × 100	2.158	3.942	-0.385	-0.485	2.025	-6.128	-3.511	-1.301	-11.195
SE	(1.338)	(1.982)	(1.197)	(1.930)	(2.693)	(2.688)	(1.953)	(2.599)	(3.224)
p (analytical)	[0.107]	[0.047]	[0.748]	[0.801]	[0.452]	[0.023]	[0.072]	[0.617]	[<0.001]
p (resampling)	[0.033]	[0.002]	[0.861]	[0.779]	[0.353]	[0.161]	[0.103]	[0.581]	[0.030]
Baseline Mean	0.037	0.035	0.039	0.080	0.076	0.085	0.140	0.135	0.145
Relative Risk	0.590	1.141	-0.100	-0.060	0.267	-0.721	-0.250	-0.096	-0.770
N	33.994	18.750	15.244	33.740	18.617	15.123	33.094	18.320	14.774
Heterogeneity p		[0.062]			[0.032]			[0.017]	

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get divorced by year-end of $t = 2$, $t = 5$ and $t = 10$ for the pooled lottery sample and by gender. The samples by gender are reweighed in order to match the first and the second moments of pre-lottery characteristics (husband's and wife's age, husband's and wife's college graduation indicator, husband's and wife's income, number of children, the lottery indicators, the year of lottery indicators) in the pooled sample of married lottery players using entropy balancing reweighting method (see Appendix E). All specification control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered at the level of the individual. The baseline mean is defined as the mean of the dependent variable for small-prize winners (<10,000 SEK). The resampling based p-values are obtained from the resampling distribution of coefficients from 1000 Monte Carlo simulations. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Only pre-lottery married individuals between 18 and 44 years old at the moment of win are included.

Table A.7: Heterogeneous Effects of Wealth on Marriage (Unmarried at $t = -1$)

	(1)	(2)	(3)	(4)	(5)	(6)
	By Income		Age-at-win		Parent	
	≤Median	>Median	18-35	36-44	No	Yes
Panel A: 1 if Got Married by $t = 2$						
Effect × 100	2.127	1.597	2.302	2.420	0.955	2.592
SE	(1.371)	(1.761)	(1.277)	(1.858)	(1.221)	(1.909)
p (analytical)	[0.121]	[0.364]	[0.072]	[0.193]	[0.434]	[0.175]
p (resampling)	[0.123]	[0.267]	[0.056]	[0.080]	[0.363]	[0.078]
N	26,882	26,923	35,141	18,664	37,459	16,346
Heterogeneity p	[0.812]		[0.958]		[0.468]	
Panel B: 1 if Got Married by $t = 5$						
Effect × 100	4.872	1.323	2.602	4.513	2.297	2.854
SE	(1.720)	(1.691)	(1.678)	(1.658)	(1.551)	(1.804)
p (analytical)	[0.005]	[0.434]	[0.121]	[0.006]	[0.139]	[0.114]
p (resampling)	[0.004]	[0.429]	[0.118]	[<0.001]	[0.131]	[0.082]
N	26,483	26,708	34,694	18,497	36,943	16,248
Heterogeneity p	[0.141]		[0.417]		[0.814]	
Panel C: 1 if Got Married by $t = 10$						
Effect × 100	5.002	0.259	2.203	4.130	2.968	1.245
SE	(2.170)	(2.210)	(2.139)	(2.156)	(1.921)	(2.319)
p (analytical)	[0.021]	[0.907]	[0.303]	[0.055]	[0.122]	[0.591]
p (resampling)	[0.009]	[0.907]	[0.282]	[0.034]	[0.140]	[0.555]
N	25,777	26,090	33,883	17,984	36,008	15,859
Heterogeneity p	[0.126]		[0.526]		[0.566]	

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get married by year-end of $t = 1$, $t = 2$, $t = 5$ and $t = 10$. Standard errors are clustered at the level of the individual. The resampling based p-values are obtained from the resampling distribution of coefficients from 1000 Monte Carlo simulations. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Only pre-lottery unmarried individuals between 18 and 44 years old at the moment of win are included.

Table A.8: Heterogeneous Effects of Wealth on Divorces (Married $t = -1$)

	(1)	(2)	(3)	(4)	(5)	(6)
	By Income		Age-at-win		Parent	
	≤Median	>Median	18-35	36-44	No	Yes
Panel A: 1 if Got divorced after the lottery by $t = 2$						
Effect × 100	1.576	0.335	2.642	1.570	1.584	1.705
SE	(3.063)	(1.020)	(3.230)	(1.282)	(5.320)	(1.286)
p (analytical)	[0.607]	[0.743]	[0.414]	[0.221]	[0.766]	[0.185]
p (resampling)	[0.495]	[0.795]	[0.426]	[0.066]	[0.787]	[0.071]
N	16,995	16,999	9,494	24,500	3,147	30,847
Heterogeneity p	[0.701]		[0.756]		[0.982]	
Panel B: 1 if Got divorced after the lottery by $t = 5$						
Effect × 100	0.710	-1.805	0.922	-0.926	-1.184	-0.177
SE	(3.053)	(1.521)	(3.345)	(1.786)	(5.810)	(1.473)
p (analytical)	[0.816]	[0.235]	[0.783]	[0.604]	[0.838]	[0.904]
p (resampling)	[0.773]	[0.346]	[0.818]	[0.592]	[0.878]	[0.902]
N	16,871	16,869	9,424	24,316	3,111	30,629
Heterogeneity p	[0.461]		[0.624]		[0.862]	
Panel C: 1 if Got divorced after the lottery by $t = 10$						
Effect × 100	-1.203	-3.959	0.264	-3.010	-2.412	-1.994
SE	(3.105)	(1.588)	(3.745)	(1.913)	(5.718)	(1.606)
p (analytical)	[0.699]	[0.013]	[0.944]	[0.116]	[0.673]	[0.214]
p (resampling)	[0.682]	[0.101]	[0.955]	[0.169]	[0.784]	[0.281]
N	16,621	16,473	9,231	23,863	3,022	30,072
Heterogeneity p	[0.429]		[0.434]		[0.942]	

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get divorced by year-end of $t = 1$, $t = 2$, $t = 5$ and $t = 10$. Standard errors are clustered at the level of the individual. The resampling based p-values are obtained from the resampling distribution of coefficients from 1000 Monte Carlo simulations. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Only pre-lottery married individuals between 18 and 44 years old at the moment of win are included.

Table A.9: Heterogeneous Effects of Wealth on Fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Married		By Income		Age-at-win		Parent	
	No	Yes	≤Median	>Median	18-35	36-44	No	Yes
Panel A: Number of post-lottery children at $t = 2$								
Effect × 100	1.488	1.090	1.286	0.996	2.721	0.412	1.108	1.109
SE	(0.854)	(1.099)	(1.100)	(0.793)	(1.257)	(0.664)	(1.136)	(0.800)
p (analytical)	[0.081]	[0.321]	[0.242]	[0.209]	[0.030]	[0.535]	[0.329]	[0.166]
p (resampling)	[0.071]	[0.223]	[0.212]	[0.209]	[0.038]	[0.378]	[0.344]	[0.099]
N	54,017	34,096	44,070	44,043	44,859	43,254	40,809	47,304
Heterogeneity p	[0.775]		[0.830]		[0.104]		[1.000]	
Panel B: Number of post-lottery children at $t = 5$								
Effect × 100	3.316	2.406	2.264	4.134	4.898	1.655	4.604	1.711
SE	(1.666)	(2.035)	(1.912)	(1.678)	(2.268)	(1.384)	(2.504)	(1.434)
p (analytical)	[0.047]	[0.237]	[0.236]	[0.014]	[0.031]	[0.232]	[0.066]	[0.233]
p (resampling)	[0.030]	[0.102]	[0.215]	[0.002]	[0.032]	[0.039]	[0.051]	[0.118]
N	53,678	33,957	43,779	43,856	44,606	43,029	40,509	47,126
Heterogeneity p	[0.729]		[0.462]		[0.222]		[0.316]	
Panel C: Number of post-lottery children at $t = 10$								
Effect × 100	5.388	-0.417	4.412	3.531	5.522	2.440	7.886	0.450
SE	(2.320)	(2.419)	(2.611)	(2.172)	(2.910)	(1.762)	(3.207)	(1.808)
p (analytical)	[0.020]	[0.863]	[0.091]	[0.104]	[0.058]	[0.166]	[0.014]	[0.803]
p (resampling)	[0.023]	[0.828]	[0.092]	[0.080]	[0.092]	[0.040]	[0.036]	[0.731]
N	52,672	33,437	43,074	43,035	43,919	42,190	39,787	46,322
Heterogeneity p	[0.084]		[0.795]		[0.366]		[0.043]	

Notes. This table reports the estimated treatment effect of lottery wealth on the number of post-lottery children at year-end in $t = 1$, $t = 2$, $t = 5$ and $t = 10$. All specification control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered at the level of the individual. The resampling based p-values are obtained from the resampling distribution of coefficients from 1000 Monte Carlo simulations. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Age-at-win 18-44.

Table A.10: Wealth Effects on Marriage and Cohabitation: Unmarried and not Cohabiting with Children at $t = -1$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$t = 2$			$t = 5$			$t = 10$		
		By Sex			By Sex			By Sex	
	All	F	M	All	F	M	All	F	M
Panel A: Got Married by t									
Effect × 100	1.686	0.801	2.149	2.886	0.036	4.926	3.849	2.571	4.691
SE	(1.075)	(1.562)	(1.403)	(1.368)	(1.599)	(1.992)	(1.710)	(2.412)	(2.483)
p (analytical)	[0.117]	[0.608]	[0.126]	[0.035]	[0.982]	[0.013]	[0.024]	[0.286]	[0.059]
p (resampling)	[0.068]	[0.620]	[0.085]	[0.037]	[0.980]	[<0.001]	[0.019]	[0.293]	[0.040]
Baseline Mean	0.073	0.085	0.065	0.146	0.164	0.133	0.257	0.282	0.240
Relative Risk	0.230	0.094	0.330	0.198	0.002	0.370	0.150	0.091	0.196
N	44.557	18.112	26.445	43.984	17.877	26.107	42.854	17.434	25.420
Heterogeneity p		[0.521]			[0.056]			[0.540]	
Panel B: Started Cohabitation with Children by t									
Effect × 100	0.771	0.370	0.022	0.113	0.352	-1.706	1.815	2.022	0.595
SE	(0.913)	(1.321)	(1.384)	(1.099)	(1.686)	(1.374)	(1.482)	(2.225)	(2.004)
p (analytical)	[0.398]	[0.779]	[0.988]	[0.918]	[0.835]	[0.214]	[0.221]	[0.364]	[0.767]
p (resampling)	[0.478]	[0.849]	[0.995]	[0.932]	[0.856]	[0.367]	[0.244]	[0.405]	[0.794]
Baseline Mean	0.081	0.089	0.075	0.153	0.165	0.145	0.244	0.262	0.231
Relative Risk	0.096	0.042	0.003	0.007	0.021	-0.118	0.075	0.077	0.026
N	44.557	18.112	26.445	43.984	17.877	26.107	42.854	17.434	25.420
Heterogeneity p		[0.856]			[0.344]			[0.634]	

Notes. This table reports the estimated treatment effect of lottery wealth on the probability to get married for unmarried and not cohabiting with children at $t = -1$ players (Panel A) and to start cohabitation with children for unmarried and not cohabiting with children at $t = -1$ (Panel B) by year-end of $t = 2$, $t = 5$ and $t = 10$ for the pooled lottery sample and by gender. All specification control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered at the individual level. Baseline mean is defined as the mean of the dependent variable of small-prize winners ($< 10,000$ SEK). The resampling based p-values are obtained from the resampling distribution of coefficients from 1,000 Monte Carlo simulations. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in the subsamples. Age-at-win: 18-44.

Table A.11: Comparison of Annuity-Rescaled Lottery Estimates to Income Gradients.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$t = 2$			$t = 5$			$t = 10$		
	All	By Sex		All	By Sex		All	By Sex	
		F	M		F	M		F	M
Panel A: Got Married by t (Unmarried at $t = -1$)									
Effect (10K SEK) × 100	0.381	0.195	0.482	0.542	0.116	0.781	0.420	0.187	0.546
SE	(0.181)	(0.241)	(0.221)	(0.200)	(0.272)	(0.279)	(0.252)	(0.392)	(0.352)
p (analytical)	[0.035]	[0.418]	[0.029]	[0.007]	[0.670]	[0.005]	[0.096]	[0.634]	[0.121]
N	53,805	22,636	31,169	53,191	22,378	30,813	51,867	21,863	30,004
Heterogeneity p		[0.378]			[0.088]			[0.496]	
Gradient (10K SEK) × 100	0.178	-0.125	0.340	0.349	0.021	0.518	0.472	0.053	0.683
SE	(0.023)	(0.040)	(0.029)	(0.035)	(0.059)	(0.044)	(0.048)	(0.078)	(0.061)
p (analytical)	[<0.001]	[0.002]	[<0.001]	[<0.001]	[0.723]	[<0.001]	[<0.001]	[0.496]	[<0.001]
N	546,606	247,605	299,001	540,202	244,882	295,320	477,049	219,019	258,030
	[<0.001]			[<0.001]			[<0.001]		
Panel B: Got Divorced by t (Married at $t = -1$)									
Effect (10K SEK) × 100	0.214	0.617	-0.317	-0.074	0.383	-0.681	-0.344	-0.045	-1.006
SE	(0.191)	(0.302)	(0.177)	(0.241)	(0.384)	(0.250)	(0.261)	(0.397)	(0.347)
p (analytical)	[0.262]	[0.041]	[0.074]	[0.760]	[0.319]	[0.006]	[0.188]	[0.910]	[0.004]
N	33,994	18,750	15,244	33,740	18,617	15,123	33,094	18,320	14,774
Heterogeneity p		[0.008]			[0.020]			[0.068]	
Gradient (10K SEK) × 100	0.066	0.311	-0.078	0.087	0.446	-0.128	0.122	0.543	-0.124
SE	(0.020)	(0.034)	(0.026)	(0.033)	(0.055)	(0.042)	(0.048)	(0.076)	(0.062)
p (analytical)	[0.001]	[<0.001]	[0.003]	[0.009]	[<0.001]	[0.003]	[0.010]	[<0.001]	[0.047]
N	265,895	144,292	121,603	263,842	143,272	120,570	238,976	130,804	108,172
	[<0.001]			[<0.001]			[<0.001]		
Panel C: #Post-Lottery Children by t (All)									
Effect (10K SEK) × 100	0.202	0.036	0.342	0.486	0.144	0.714	0.552	0.130	0.931
SE	(0.108)	(0.128)	(0.166)	(0.209)	(0.274)	(0.308)	(0.277)	(0.315)	(0.443)
p (analytical)	[0.063]	[0.780]	[0.039]	[0.020]	[0.599]	[0.021]	[0.046]	[0.680]	[0.036]
N	88,113	41,539	46,574	87,635	41,319	46,316	86,109	40,700	45,409
Heterogeneity p		[0.144]			[0.168]			[0.140]	
Gradient (10K SEK) × 100	0.207	0.250	0.146	0.553	0.633	0.414	0.638	0.651	0.489
SE	(0.010)	(0.014)	(0.013)	(0.026)	(0.037)	(0.037)	(0.044)	(0.058)	(0.063)
p (analytical)	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
N	816,088	393,553	422,535	810,741	391,186	419,555	725,593	354,050	371,543
	[<0.001]			[<0.001]			[0.058]		

Notes. This table reports annuity-rescaled causal estimates of the lottery wealth and five-year average annual disposable income gradients estimated using representative samples of adults drawn in 1990 and 2000 and reweighted to match the sex and age-at-win distrubution of the lottery sample. The annuity-rescaled lottery wealth is computed assuming that the prizes were annuitized over a 20-year period using a discount rate of 2 percent. Income and annuitized wealth are measured in 10,000 SEK. All specification control for baseline controls measured at $t = -1$. Gradient estimation model controls from year fixed effect, and causal effect estimation model controls for group-identifier fixed effects. Standard errors are clustered at the level of the individual. The heterogeneity p-value is from a two-sided t-test of the null hypothesis that the treatment-effect parameters are identical in men and women subsamples.

Table A.12: Comparison to Other Lottery Studies

Study	Identification Strategy	Sample	Outcome	Rescaled Effects	Rescaled Current Study Effects
Bulman et al. (2022)	A triple-difference design to compare big and small-prize winners and and currect and future winners before and after the lottery.	2000-2019 US lottery winners who filled W-2G Form. Age-at-win: 25-44. $N \approx 888K$. $t = 1 - 5$.	Being married at $t = 5$ (unmarried at $t = -1$)	1.2. ($SE = 0.5$) for all, 0.66 ($SE = 0.76$) for females, 1.49 ($SE = 0.69$) for males.	2.4. ($SE = 0.85$) for all, 0.27 ($SE = 1.18$) for females, 3.47 ($SE = 1.15$) for males.
			Being married at $t = 5$ (married at $t = -1$)	-0.98 ($SE = 0.48$) for all, -1.57 ($SE = 0.78$) for females, -0.58 ($SE = 0.61$) for males.	0.44 ($SE = 0.98$) for all, -1.87 ($SE = 1.62$) for females, 3.11 ($SE = 0.89$) for males.
			Fertility at $t = 5$	0.07 ($SE = 0.47$) for all, -0.29 ($SE = 0.71$) for females, 0.31 ($SE = 0.63$) for males.	2.10 ($SE = 0.90$) for all, 0.62 ($SE = 1.18$) for females, 3.08 ($SE = 1.33$) for males.
Tsai et al. (2022)	A difference-in-differences design to compare big and small-prize winners before and after the lottery.	2004-2018 lottery winners in Taiwan who won more than 2K NT$. Age-at-win: 20-44. $N = 584,274$. $t = 1 - 6$.	Fertility at $t = 5$	1.36 ($SE = 0.40$)	2.10 ($SE = 0.90$)
			Being married at $t = 5$ (unmarried at $t = -1$)	1.00 ($SE = 0.38$)	2.4 ($SE = 0.85$)
Golosov et al. (2021)	First-difference and difference-in-difference estimation that exploits variation in the timing of lottery wins.	1999-2016 US lottery winners who filled W-2G Form and won at least $30K. Age-at-win: 21-64. $N = 90,731$. $t = 1 - 5$.	Got married by $t = 5$ (unmarried at $t = -1$).	0.77 ($SE = 0.08$)	2.34 ($SE = 0.86$)
			Got married by $t = 5$ (married at $t = -1$)	-0.67 ($SE = 0.10$).	-0.32 ($SE = 1.04$).
Bleakley and Ferrie (2016)	Random assignment of land from Georgia's Cherokee Land Lottery of 1832. Comparison of winners to non-winners.	Adult white males in 1850 who had been eligible to participate in the 1832 lottery. $N = 14,306$. $t = 18$.	Fertility at $t = 18$	Winners of land had 0.18 (SE=0.073) more children than non-winners (3.3% increase).	—
Boertien (2012)	Comparison of large and small prize winners of different lotteries/games.	Lottery and other games of chance players from the British Household Panel Survey (BHPS) in 1997-2005. $N = 3,043$. $t = 3$.	Got divorced by $t = 3$	The effect of 1 unit increase in $log(lottery\ prize)$ is -0.76 p.p. for men. The effect for women is non-significant (the value is not reported).	—
Hankins and Hoekstra (2011)	Comparison between large prize winners ($25,000-$50,000) and small prize winners ($600- $1,000) in Florida.	Miami-Dade and Palm Beach counties winners from 1988 through 2004. At least $600 prize. $N = 26,629$ for marriage and $N = 40,198$ for divorce. $t = 3$.	Got married by $t = 3$ (unmarried at t =-1)	-7.30 (SE = 5.62) for all, -19.83 (SE=7.53) for females, -1.39 (SE=8.00) for males	1.65 (SE=0.78) for all, 0.84 (SE=1.04) for females, 2.08 (SE=0.95) for males.
			Got divorced by $t = 3$ (married at t =-1)	-1.92 (SE=2.61)	0.92 (SE=0.82)

Notes. Rescaled estimates are effects of $100K × 100 (year-2010 prices). Estimates of Bleakley and Ferrie (2016) and Boertien (2012) are not rescaled and reported as in the original study. Further information about the studies and calculations underlying the rescaled effects are available in Section C in the Appendix.

Table A.13: Relationship between Spousal Wealth Pre- and Post Separation

	(1)	(2)	(3)
	Baseline	Total Wealth $\geq$ 500K SEK	Wife's Share $\geq$ 50%
Panel A: Married with Children			
Wealth share in $t = -1$	0.617	0.690	0.641
SE	(0.014)	(0.017)	(0.024)
N	13.153	7.471	5.677
Panel B: Married without children			
Wealth share in $t = -1$	0.677	0.733	0.658
SE	(0.039)	(0.046)	(0.066)
N	1.550	838	654
Panel C: Cohabiting with children			
Wealth share in $t = -1$	0.733	0.811	0.723
SE	(0.014)	(0.017)	(0.026)
N	10.326	5.127	4.245

Notes. This table shows the results of a regression analysis that examines the relationship between the net wealth share of the wealthiest spouse in the year prior to separation and the net wealth share of the same spouse in the year following separation. The analysis includes fixed effects for the year of separation, and only includes couples where both partners are between 25 and 44 years old and have a total net wealth of at least 100,000 SEK. Column 2 is a subset of this sample and only includes couples with a total net wealth of at least 500,000 SEK the year prior to separation. Column 3 is a subset of this sample, it only includes couples where the wife's wealth share was at least 50% the year prior to separation. Robust standard errors are in parentheses.

Table A.14: The Effect of Lottery on Own and Spouse's Wealth in the Year of the Lottery

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Women		Men	
	Own	Spouse's	Own	Spouse's	Own	Spouse's
Panel A: Net Wealth						
Effect × 100	48.413	22.098	51.905	19.654	45.019	25.589
SE	(10.123)	(8.483)	(11.907)	(12.205)	(14.774)	(4.383)
p (analytical)	[<0.001]	[0.009]	[<0.001]	[0.108]	[0.002]	[<0.001]
p (resampling)	[<0.001]	[0.059]	[<0.001]	[0.107]	[0.002]	[0.012]
N	2.564	2.521	1.262	1.241	1.302	1.280
Panel B: Real Assets						
Effect × 100	4.474	1.657	4.717	3.004	8.217	-7.224
SE	(7.312)	(6.808)	(7.173)	(9.437)	(18.601)	(5.745)
p (analytical)	[0.541]	[0.808]	[0.511]	[0.750]	[0.659]	[0.209]
p (resampling)	[0.459]	[0.817]	[0.430]	[0.771]	[0.536]	[0.532]
N	2.564	2.521	1.262	1.241	1.302	1.280
Panel C: Financial Assets						
Effect × 100	36.161	16.332	34.809	9.937	41.299	34.724
SE	(8.089)	(6.856)	(10.429)	(3.970)	(4.140)	(7.985)
p (analytical)	[<0.001]	[0.017]	[<0.001]	[0.012]	[<0.001]	[<0.001]
p (resampling)	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.009]	[0.003]
N	2.564	2.521	1.262	1.241	1.302	1.280
Panel D: Debt						
Effect × 100	-4.061	-5.575	-7.241	-8.979	4.362	1.180
SE	(2.488)	(2.581)	(2.338)	(2.166)	(2.988)	(4.804)
p (analytical)	[0.103]	[0.031]	[0.002]	[<0.001]	[0.145]	[0.806]
p (resampling)	[0.219]	[0.134]	[0.045]	[0.093]	[0.540]	[0.882]
N	2.564	2.521	1.262	1.241	1.302	1.280

Notes. This table reports the estimated treatment effect of lottery on own and spouse's wealth (in M SEK) at year-end of $t = 0$ for the pooled lottery sample and by gender of the winner. All specification control for baseline controls measured at $t = -1$ and group-identifier fixed effects. Standard errors are clustered at the level of the individual. Triss-monthly winners are excluded. Age-at-win: 18-44.

Appendix B Variable Construction

This section provides additional information about the definition and construction of the variables used in our analyses.

B.1 Outcome Variables

- *Marriage t* – Equals 1 if marriage status changed to "married" or if the spouse's ID changed at least once no later than $t \in \{2, 5, 10\}$ years after the lottery. For the period 1990-2018, the variable is derived using information in Statistics Sweden's (SCB) LISA database (Statistics Sweden, 2009; Ludvigsson et al., 2019). For earlier years, we use information in the *Total Population Register* (Statistics Sweden, 2017; Ludvigsson et al., 2016). Variable is defined only for players not married at year-end in $t = -1$. Variable is set to missing for individuals who do not appear in LISA or the *Total Population Register* at any year between the lottery event and $t \in \{2, 5, 10\}$ (as they are not registered in Sweden).

- *Divorced t* – Equals 1 if marriage status changed from "married" to "divorced" or if the spouse's ID changed at least once no later than $t \in \{2, 5, 10\}$ years after the lottery. The variable is derived using the same databases as the marriage variable described above. It is only defined for players who were married at year-end in $t = -1$. Variable is set to missing for individuals who do not appear in LISA or the *Total Population Register* at any year between the lottery event and $t \in \{2, 5, 10\}$ because they are not registered in Sweden.

- *Fertility t* – Equals the number of biological children whose estimated month of conception not prior to the month of the lottery and who are born no later than at year-end t years after the lottery event. Since our data set only contains information about each child's quarter and year of birth, we set each child's month and year of conception equal to nine months prior to the midpoint of the quarter of birth. For example, a child born in the first quarter of 2006 is assigned a month of conception of May 2005, since that is nine months before the midpoint of the midpoint of the first quarter of 2006. We obtain information about children of lottery players using data in the *Total Population Registry.* Variable is set to missing for individuals who do not appear in LISA or the *Total Population Register* at $t \in \{2, 5, 10\}$ and any posterior year because they are not registered in Sweden. This is because the *Total Population Registry* will collect information about all already born children of Swedish residents even if these children were born abroad.

- *Cohabitation with children t* – Equals 1 if an unmarried individual were living with a partner with whom they have common children within $t \in \{2, 5, 10\}$ years after the lottery. Information regarding children come from the *Total Population Registry.* We use the *Real Estate Registry* (1986 to 2010) or the LISA database (2011 onwards) for information about the couple's shared address. Therefore, a couple is considered to be cohabiting if they share the same address and have children together.

B.2 Income and Wealth Variables

All variables measured in monetary units are converted to units of year-2010 SEK, using Statistics Sweden's annual average Consumer Price Index (CPI) series. Below, we provide additional details on the income and wealth variables used in our analyses. As is conventional, all wealth and income variables used in our analyses are winsorized at the 1st and 99th percentiles.

Income Gradients The income measure we use to estimate the income gradients discussed in the main text is a multi-year average of individual disposable income. The average is taken over all pre-lottery years with non-missing data. Our annual measure of disposable income is measured net of taxes and includes wage earnings, self-employment income, pensions and other social transfers received.

Heterogeneity Analyses In some heterogeneity analysis, we analyze players with below- and above-median incomes separately. We also compare wealth effects among married players with relative incomes above and below 50%. In these analyses, our measures of income is pre-tax labor earnings in the year prior to the lottery event. Information about annual pre-tax labor earnings (original name: `ArbInk`) is obtained from the *Income and Taxation Registry.*

Wealth Variables In our analyses of how lottery prizes differentially impact the wealth of the winning and non-winning spouses, we use *Statistics Sweden's Wealth Registry*, which covers the period 1999-2007. The registry is bases on information provided by financial and governmental institutions on a mandatory basis. The registry provides the year-end value of different types of assets owned, including real assets, financial assets, and debt. Additional details on the *Statistics Sweden's Wealth Registry* are provided in Online Appendix of Cesarini et al. (2016) (p. 85-86) and in Statistics Sweden (2004).

Limitations of Wealth Data Here we discuss some limitations of the variables provided by the registry that are relevant for interpreting the analysis from section G of the Appendix.

The main limitation is that the measure of the value of real assets is less reliable than the value of financial assets, since information about conditions of the real estate is not always accurately provided to SCB. SCB imputed the value of the property using information about the unit, and the house prices in the neighborhood. Hence, some home improvements will not be reflected in the wealth registry unless the home is sold immediately.

Second, the ownership of condominium is difficult to determine so that some people can be mistakenly singled out as owners, and some apartment owners are not assigned any value. This is because there is no registry of persons owning a right of residence, and the ownership of condominium is determined using the data on property value and registered address. For example, persons renting condominium may be mistakenly classified as owners if they are registered in houses of condominium associations that do not provide control information of the wealth value of its members to the Swedish Tax Agency (Statistics Sweden, 2004).

Another limitation is that in most cases, the net and gross wealth variables include only financial and real assets. Other assets (*e.g.*, cars, art, or other similar assets) are included in the statistics only if these assets were included in the tax declaration, which mainly applies to persons who have assets whose value has exceeded the limit of the wealth tax. Therefore, the purchase of a car will be typically reflected in the register as a reduction in financial assets.

These limitations imply that that estimates reported in Section G and in A.14 of the Appendix should be interpreted with caution.

We use the following variables:

- Gross wealth (original name: `FSMMV`) – Equals to the year-end market value of total assets in millions of SEK .
- Real assets (original name: `FREALMV`) – Equals to the year-end market value of real assets in millions of SEK.
- Financial assets (original name: `FFINMV`) – Equals to the year-end market value of financial assets in millions of SEK.
- Debt (original name: `FSKULMV`) – Equals to the year-end total debt in millions of SEK.
- Net wealth (original name: `FNETTMV`) – Equals to the year-end market value of net wealth in millions of SEK, which is equal to the difference between the market value of total assets and debt.

All wealth variables are winsorized at 1st and 99th percentiles.

B.3 Baseline Covariates

Our baseline covariates are measured at year-end in the year prior to the lottery. Each variable is defined and constructed as in Cesarini et al. (2016) but using a longitudinal dataset updated with information for the years 2011-2018.

- Age-at-win– Derived by subtracting the individual's year of birth from the year of the lottery event.
- Female – 1 if the individual is classified as female in the *Total Population Registry* and 0 if classified as male.
- Nordic Born – 1 if individual was born in Nordic country. Constructed using the *Total Population Registry.*

- # of Pre-lottery Children – Derived by subtracting the number post-lottery children from lifetime fertility according to *Statistics Sweden's Child Registry*.

- College-educated – 1 if individual is classified as having completed at least three years of college no later than the year prior to the lottery.

- Married – 1 if the individual was married a year prior to the lottery. The variable is derived using information in LISA for the period 1990-2018. For earlier years, we use information in the *Total Population Register*.

Appendix C Comparison to Previous Lottery Studies

Here, we provide additional information about the section of the main text that discusses how our findings compare to those reported in previous studies using lottery players to estimate wealth effects on fertility and marriage.

C.1 Harmonizing Current Study's Estimates

Since all effect-size estimates in this paper are reported in units of 1M-SEK ($140,000), we obtain harmonized coefficients by simply dividing the original estimate of interest and its standard error by 1.4.

C.2 Harmonizing Tsai *et al.*'s (2022) Estimates

Marriage

Our harmonized estimates of marriage are derived from information in Figure 6A and Table 1 in Tsai et al. (2022). For a sample of players unmarried at the time of the lottery, the figure depicts the difference between the proportion of large-prize winners and small-prize winners married at $t = 1, 2, ..., 6$. The figure also reports 90% confidence intervals for each point estimate. For $t = 2$, the point estimate in the figure is ∼5.8, and the standard error needed to match the observed width of the confidence interval is ∼1.75. For $t = 5$, the original estimate is ∼5.0, with a standard error of 1.9. From Table 1, we have that the average large prize won by players in the large-prize group is

$500,000. We therefore divide the original estimates and their standard errors by 5 to obtain the harmonized coefficients of 1.16 ($SE = 0.34$) and 1.00 ($SE = 0.38$) reported in the main text for $t = 2$ and $t = 5$.

Fertility

Our harmonized estimates of fertility are derived using a procedure similar to the one described in the previous section. We combine information in Tsai *et al.*'s (2022) Figure 3 with descriptive statistics about prizes shown in Table 1. For $t = 2$, the point estimate depicted is ∼3.8, suggesting that large-prize winners have 0.038 more children than subjects in the control group. Setting the standard error to 2.15 yields a 90% confidence interval with a range ($3.8 \pm 1.645 \times 2.15$) that appears to match the figure. For $t = 5$, a similar procedure yields a point estimate of ∼6.8, with an implied standard error of ∼2.0. Since the average prize won by players in the large-prize group is approximately $500,000, the harmonized estimates in the main text are then obtained by dividing these approximate estimates and standard errors by 5. Doing so yields harmonized estimates of 0.76 ($SE = 0.43$) and 1.36 ($SE = 0.40$) for $t = 2$ and $t = 5$, respectively.

C.3 Harmonizing Hankins and Hoekstra (2011) Estimates

Marriage

Hankins and Hoekstra (2011), hereon HH, compared the marriage rates of Floridian lottery players who won prizes in the range $25K-$50K to those in a control group composed of players who won small prizes (below $1,000). In the main analysis, marriages were tracked over a three-year period after the lottery event. HH's estimate of -1.26 ($SE = 0.97$) suggests that large-prize winners were 1.26 percentage points *less* likely to be issued a marriage license than players in the control group. Splitting the sample by gender, they report an estimate of -0.24 ($SE = 1.38$) for men and a statistically significant estimate of -3.42 ($SE = 1.30$) in women.

There are two barriers to comparability that must be addressed to harmonize the originally reported estimates. First, HH do not observe players' marital status at time of win and therefore did not restrict their estimation sample to players who are unmarried at the time of the lottery. Under the plausible assumption that short-run wealth effects are very small among players who are married at the time of the lottery, we can nevertheless use the same procedure as HH to back out an approximate effect on the unmarried from the estimates reported in the paper. To do so, we follow HH in assuming that 46% of players in their sample were unmarried at baseline (see p. 409 in HH 2011). We then inflate the originally reported estimates by a factor of $1/0.46 = 2.17$.

Second, we rescale the effects to make them roughly interpretable as estimates of the effect of a \$100K windfall. Under the simplifying assumption that the value of each large prize won is equal to the midpoint of the prize interval (\$25K-\$50K), the appropriate adjustment factor is $100/37.5 = 2\frac{2}{3}$. Applying both adjustments to the original coefficient estimates for marriage reported in column (4) of Table 2 yields harmonized coefficients of -7.30 ($SE = 5.62$), -1.39 ($SE = 8.00$) and -19.83 ($SE = 7.53$) for the main sample, and the male and female subsamples, respectively. While the exact values of the rescaled estimates are sensitive to the assumptions made, the conclusion that HH's (2011) standard errors are much larger than those in the other three quasi-experimental lottery studies of marriage is very robust

Divorce

HH estimated that winners of \$25K-\$50K were 0.39 ($SE = 0.53$) percentage points less likely to be divorced at $t = 3$ than winners of less than \$1,000. We follow HH and assume that 54% of players in their sample were married at baseline (see p. 412 in HH 2011). Therefore, we inflated the originally reported estimates by a factor of $1/0.54{=}1.85$. Next, we applied the adjustment factor of $2\frac{2}{3}$ in order to make the estimates approximately interpretable as estimates of the effect of a \$100K windfall. Applying both adjustment factors implied the effect of \$100K of -1.92 ($SE = 2.61$) percentage points.

C.4 Design Calculations Based on HH

Here, we evaluate the statistical power of HH's study to detect effect sizes of a magnitude we consider plausible. We then proceed to discuss the implications of our findings about the informativeness and credibility of HH's findings.

We begin by discussing what effect sizes should be considered plausible. The parameter of interest is the effect of a cash windfall, measured in units of \$100K, on the three-year marriage rate among umarried lottery players. A useful starting point for selecting a plausible value is to look for external information in the form of previously published estimates of the parameter of interest, when available, or similar parameters. We proceed by summarizing the information from the three other lottery studies that examined the effect of wealth on marriage. Bulman et al. (2022, See Table 4) report a harmonized effects of 2.05 percentage points ($SE = 0.46$) at $t = 3$, in their sample of players married at win. Applying the procedure described in Section C.2 to the $t = 3$ depicted in Figure 6A yields a harmonized coefficient of 1.20 ($SE = 0.37$). Finally, the present study does not report estimates of wealth effects on marriage at $t = 3$, but the harmonized estimate for the closest available horizon ($t = 2$) is 2.32 ($SE = 0.86$).

Next, we conducted an inverse-variance weighted meta-analysis of the three harmonized estimates. The meta-analysis yielded a combined estimate of 1.61 ($SE = 0.27$). If we stipulate this to be a plausible, if only ballpark, value of the parameter estimated by HH, the study's power (at α =0.05) to detect an effect of 1.61 was 5.9% in the full sample. Moreover, design calculations following (Gelman and Carlin, 2014) suggest that given this power, conditional on finding a statistically significant the sign of the significant coefficient will be wrong 20% and the magnitude of the point estimate will overestimate the true parameter value in absolute value by a factor of 8, on average. Even under assumptions that we consider optimistic, the conclusion does not change. Suppose, for example, that in HH's sample of women who were single at win, the value of the harmonized parameter value is equal to the upper bound of the 95% confidence interval of the coefficient from our meta-analysis ($1.61 + 1.96 \times 0.27$)= 2.14. Then HH's power to detect the effect in their subsample of women (at α =0.05) given their standard error of 7.53 was 5.9%. Conditional on reporting a significant effect, the probability of a sign error is now 21% and the estimated coefficient will, on average, overestimate the true parameter in absolute value by a factor of over 8. Another way to assess how informative the HH estimates are is to examine what happens if its added to the meta-analysis. We find that adding including the harmonized estimate of -7.30 ($SE = 5.62$) changes the value of the inverse-variance weighted combined coefficient from 1.61 to 1.59 and reduces the standard error from 0.273 to 0.270. Overall, we conclude that the estimates provide little information about the sign and magnitude of wealth effects on marital outcomes. Of course, readers with very different priors about the sort of effect sizes that should be considered plausible in the Floridian sample studied by HH can easily apply the above framework using their preferred assumptions in lieu of those we have made.

Appendix D Inference

D.1 Permutation-Based Inference

Throughout, we supplement conventional p-values based on analytical standard errors with resampling-based p-values. The latter are obtained using a commonly used algorithm. Below, we describe the steps involved in applying it in our specific setting. For each outcome, run the regression described by equation (1) and save the estimated lottery effects β. Then start the permutation algorithm:

1. Within each group k, randomly permute the original prize column.
2. Estimate equation (1) using the permuted prize variable to obtain β_1^0.
3. Repeat the procedure N times to obtain an approximate final sample distribution of $\beta^0 = \beta_1^0, \beta_2^0, ..., \beta_N^0$ under the null that the wealth effect is zero.
4. Using the vector of β^0, obtain the vector of test statistics under the null as $T = T_1, T_{2,}..., T_N$, where $T_j = \beta_j^{0^2} \times Var(\beta^0)^{-1}$. Similarly, compute the observed value of the test statistic as $t_{obs} = \beta^2 \times Var(\beta^0)^{-1}$.
5. Compute the resampling-based p-value given by $P(T > t_{obs}) = \frac{1}{N}\sum_{j=1}^{N} I(T_j > t_{obs})$.

D.2 Multiple-Hypothesis Testing

In our main analyses, we tested 27 null hypotheses. To address concerns about multiple-hypothesis testing, we adopt a decision rule that ensures the (expected) proportion of true null hypotheses that are (incorrectly) declared to be significant does not exceed a desired threshold, q. This threshold, q, is known as the false discovery rate (FDR). In order to compute the FDR adjusted p-values reported in Table 4, we apply the two-step procedure proposed by Benjamini et al. (2006). In a preliminary step, sort the resampling-based p-values in ascending order so that $p_1 < p_2 < ... < p_M$ (where $M = 27$ in our setting). The algorithm is then applied

1. Compute adjusted $q' = \frac{q}{1-q}$. Find the largest r for which $p_r < q'r/M$, where r is the rank of the p-value and M is the total number of hypotheses. Reject all hypotheses with p-values smaller or equal than p_r at the level q. If no hypothesis is rejected ($r = 0$), stop. Otherwise, continue to the next step.

2. Let $M_0 = M - r$, where that r is the number of hypotheses rejected in the previous step. Repeat (1) for $q^* = q'M/M_0$.

The algorithm, which we implement using `Stata` code provided by Anderson (2008), generates p-values needed to identify all hypotheses rejected at some level q. For example, we can list all hypotheses rejected at level 0.05 or 0.1. In order to obtain the FDR adjusted p-values, we need to find the smallest q for which the hypothesis is rejected. Anderson (2008) suggests proceeding by first assuming all FDR adjusted p-values are equal to 1. Then apply the algorithm at $q = 0.999$, replacing the FDR adjusted p-values by 0.999 for all the hypotheses rejected at this level. Then reduce the value of q by 0.001, repeat the procedure, and replace the FDR adjusted p-value by 0.998 for all rejected hypotheses. Continue until $q = 0.001$.

Appendix E Entropy Balancing Reweighting

In order to produce the estimates reported in Table A.6, we first reweigh observations from the samples of couples where husband wins and couples where wife wins in order to match the moments of pre-lottery characteristics in the pooled sample of pre-lottery married winners. This procedure reassures that the first and seconds moments of the pre-lottery characteristics used for the weights construction in the reweighted samples of couples where husband wins and couples where wife wins are similar, so that the detected heterogeneity cannot be attributed to the differences in these characteristics between the subsamples. For the weighs construction we apply entropy balancing procedure proposed by Hainmueller (2012).

In the entropy balancing every observation in the sample of couples where wife wins $(i|W)$ and where husband wins $(i|H)$ gets a weight that satisfies a set of balance constraints and minimizes the loss function:

$$\min_{\omega_i} H(\omega) = \sum_{i|S} h(\omega_i) = \sum_{i|S} \omega_i log(\omega_i) \forall S \in W, H \tag{E.1}$$

subject to balance and normalizing constraints

$$\sum_{i|S} \omega_i c_{ri}(X_i) = m_r \text{ with } r \in 1, ..., R \quad \forall S \in W, H \tag{E.2}$$

$$\sum_{i|S} \omega_i = 1 \quad \forall S \in W, H \tag{E.3}$$

$$\omega_i \geq 0 \quad \forall i, \tag{E.4}$$

where $c_{ri}(X_i) = m_r$ describes a set of R balance constraints imposed on the covariate moments of the reweighted group. For this analysis, a balance constraint is formulated with m_{rj} containing the rth order moment of a given variable x_j in the pooled sample, whereas the moment functions $c_{ri}(X_i)$ are specified for the reweighted group (couples where husband wins or couples were wives wins). Therefore, weights (ω_i) are chosen in a way that the weighted $1_{st}, .., R_{th}$ moments of the pre-lottery characteristic in the reweighed group are equal to the corresponding moments in the pooled sample. The loss function $H(\omega)$ is non-negative and it decreases the closer ω is to the vector of ones.[1] These properties of the loss function imply that while weights are adjusted as far as needed to fulfill the balance constraints (E.2), they are maintained as close as possible to the base weights to sustain information about the reweighted group.

We impose the constraints on the first (for all variables) and second (for continuous variables) moments of the husband's and wife's age, the husband's and wife's income, the husband's and wife's college graduation indicators, the number of children, the year of win indicators, the lottery type indicators. The weights for the sample of couples

[1] Since the baseline weight of each observation is one.

where wife wins vary from 0.01 to 9.91 with the average weight of 1.29 and the median weight of 1. 90 percent of the weights lie in the interval between 1 and 2.28. The weights for the sample of couples where husbands win vary from approximately 0 to 133.9 with the average weight of 1.38 and the median weight of 1.90 percent of the weights lie in the interval between 0.78 and 3.81. Table A.5 reports the summary statistics of the variables used for the reweighting in both samples before and after the reweighting. The summary statistics suggest that the moments of the controls are similar in the reweighted subsamples.

Appendix F Models of Marital Dissolutions

F.1 General Framework

Here we provide a simple dynamic divorce decision model and show how it rationalizes our results.

Denote the value functions of the husband and wife in a married state by $V_h^M(z_t)$ and $V_w^M(z_t)$ respectively and the value functions of the husband and wife in a single state by $V_h^S(z_t)$ and $V_w^S(z_t)$ respectively, where z_t is a vector of state variables at t. State variables include the characteristics of the partners (e.g., earnings, wealth) denoted by x_{ht} and x_{wt} for the husband and wife respectively, the quality of their match denoted by θ_t, and the lottery wealth of the husband and wife denoted by L_{wt} and L_{ht} respectively, so that $z_t = \{x_{wt}, x_{ht}, \theta_t, L_{ht}, L_{wt}\}$.

In the initial period individuals are married and the optimal value function of $i \in \{h, w\}$ in a married state in t is specified as follows

$$V_i^M(z_t) = u_i^M(z_t) + \beta \mathbb{E}_t \text{Max}\left(V_i^M(z_{t+1}), V_i^S(z_{t+1})\right) \quad \forall i \in \{h, w\}, \tag{F.5}$$

where $u_i^M(z_t)$ is a current value in a married state of $i = \{h, w\}$ and $\mathbb{E}_t \text{Max}\left(V_i^M(z_{t+1}), V_i^S(z_{t+1})\right)$ is the expected future value of remaining married, which includes the future divorce prospects.

This value functions are written in terms of the state variables, since it is considered after all control variables (*e.g.*, allocation of time and goods) are set at their optimal values. Decisions about the allocation of time and goods are the result of household members' utility maximization and bargaining. The value of being single is defined similarly and it includes the future remarriage prospects. For simplicity, we assume that marriage dissolution is not costly.

The couple will remain married if and only if both spouses are better off when married than when single, i.e. if and only if

$$V_i^M(z_t) \geq V_i^S(z_t) \quad \forall i \in \{h, w\}, \tag{F.6}$$

and they divorce if at least one spouse is better off when married than when single.[2]

Let us define the gains from marriage of spouse i as $\Delta V_i(z_t) = V_i^M(z_t) - V_i^S(z_t) \;\; \forall i \in \{h, w\}$.

Divorce happens if the gains from marriage are negative for at least one of the two spouses. If the gains from marriage of both spouses are high, it is less likely that a shock to the state variables leads to divorce. Notice that if one of the two spouses has sufficiently high gains from marriage, they can compensate the partner through transfers: this would reduce the gains from marriage of the spouse who makes the transfer and increase those of the other, hence potentially preserving a marriage that would have otherwise come undone.

Given that in $t = 0$ all couples have no lottery wealth and all are married, in the initial period the following holds:

$$\Delta V_i(z_0 | L_{w0} = 0, L_{h0} = 0) \geq 0 \quad \forall i \in \{h, w\}. \tag{F.7}$$

We observe couples divorcing after the wife wins, which, in this model, means that

$$\exists i \in \{h, w\} : \Delta V_i(z_t | L_{wt} = L, L_{ht} = 0) < 0. \tag{F.8}$$

Conversely, we observe couples remaining married when the husband wins the lottery, which is consistent with

[2]Since 1973, the marriage law in Sweden does not require mutual agreement of the spouses for divorce. See Section G.1 of the Appendix for additional details of Swedish Marriage Code.

$$\Delta V_i(z_t|L_{wt}=0, L_{ht}=L) \geq 0 \quad \forall i \in \{h, w\}. \tag{F.9}$$

It is straightforward that our empirical results are consistent with this model if, for at least one spouse, the gains from marriage are lower when the wife wins the lottery than when nobody wins or when the husband wins.

To understand whether this gendered effect is driven by the asymmetric effect of the lottery on the single- or married-state utility, let us consider two different sharing rules of the lottery wealth after divorce: (i) the lottery prize is kept by the winner, (ii) the lottery prize is split equally between the spouses.

Scenario 1. Lottery prize is kept by the winner after divorce If the person who wins the lottery gets to keep the entire prize in case of divorce, the single-state utility of one spouse is not affected by the other spouse's lottery win. In the language of the model, this means that $V_i^S(z_t|L_i=0, L_{-i}=L) = V_i^S(z_t|L_i=0, L_{-i}=0) \quad \forall i \in \{h, w\}$.

If that is the case, couples get a divorce when the wife wins if and only if

$$\exists i \in \{h, w\} : \Delta V_i(z_t|L_{wt}=L, L_{ht}=0) < 0 < \Delta V_i(z_t|L_{wt}=0, L_{ht}=0), \tag{F.10}$$

i.e. for at least one of the spouses the gains from marriage go from positive to negative upon the wife's lottery win.

If $i = w$, then this scenario implies that wife's gains from marriage are lower when she wins than in the initial state (without the lottery win). This means that upon winning the lottery her single-state utility increases more than her married-state utility.

If, instead, $i = h$ this scenario implies that $V_h^M(z_t|L_{wt}=L, L_{ht}=0) < V_h^M(z_t|L_{wt}=0, L_{ht}=0)$, so that husband's married-state utility decreases when the wife wins. That's because his single-state utility is unaffected by her lottery win.

Remaining married when the husband wins is consistent with positive gains from marriage for both spouses, i.e.

$$\Delta V_i(z_t|L_{wt}=0, L_{ht}=L) > 0 \quad \forall i \in \{h, w\}. \tag{F.11}$$

This also implies that at least one of the spouses has greater gains from marriage when the husband wins than when the wife does, i.e.

$$\exists i \in \{h, w\} : \Delta V_i(z_t | L_{wt} = L, L_{ht} = 0) < \Delta V_i(z_t | L_{wt} = 0, L_{ht} = L).$$

If $i = w$, then the gains from marriage of the wife when she wins are lower than her gains from marriage when the husband wins. If $i = h$, then his married-state utility is lower when the wife wins than when he wins.

Scenario 2. Lottery prize is split equally between spouses after divorce If, upon divorce, each spouse's wealth, including the lottery win, is split equally, then the single-state utility of an individual is the same if he/she wins or if the spouse wins, so that $V_i^S(z_t | L_i = L, L_{-i} = 0) = V_i^S(z_t | L_i = 0, L_{-i} = L) \;\; \forall i \in \{h, w\}$.

Hence, the conditions that imply that the couple divorces if the wife wins, but stays together if the husband does ((F.8) and (F.9) in the previous scenario) can be written as

$$\exists i \in \{h, w\} : V_i^M(z_t | L_{wt} = L, L_{ht} = 0) < V_i^M(z_t | L_{wt} = 0, L_{ht} = L). \tag{F.12}$$

This means that there is at least one spouse for whom the married-state utility is lower when the wife wins than when the husband wins. This is inconsistent with the assumption of income pooling in the married state, since the source of income matters when the couple is together. In this scenario the effect of the lottery is gendered not because of the asymmetric effect of the lottery on the single-state utility but because of the asymmetric effect of the lottery on the married-state utility.

F.2 Symmetric Cooperative Bargaining Framework

Under both scenarios we have derived the corresponding conditions that make the general model consistent with our empirical findings. Are these conditions consistent with a model of bargaining among spouses? In this Section we develop one such model that, under appropriate assumptions about the parameters, delivers exactly the predictions described in the general framework, consistent with the data.

Consider a cooperative bargaining framework with singlehood as threat point, as in

Manser and Brown (1980) and McElroy and Horney (1981). For simplicity, let's assume that the husband's and wife's individual utilities depend only on private consumption. We denote the composite good consumed by the wife by Q_w and the composite good consumed by the husband by Q_h. The wealth of spouse $i = h, w$ is denoted by I_i. The pairs of these variable are $\mathbf{Q} = \{Q_h, Q_w\}$, and $\mathbf{I} = \{I_h, I_w\}$, respectively. In the notation of the general model in the previous section, $\mathbf{I}$ represents the state variable.

Let $V_i^S(\mathbf{I})$ be the single-state indirect utility obtained after maximization of the single-state utility function $U_i^S(Q_i)$, subject to the single-state budget constraint. Similarly, let the married-state utility function be denoted by $U_i^M(Q_i)$.

Each spouse solves the following optimization problem:

$$\max_Q U_i^M(Q_i) \text{ subject to} \tag{F.13}$$

$$U_{-i}^M(Q_{-i}) - V_{-i}^S(\mathbf{I}) \geq 0, \tag{F.14}$$

$$Q_w + Q_h \leq I_w + I_h \tag{F.15}$$

$$\mathbf{Q} \geq 0 \tag{F.16}$$

This amounts to suggesting a consumption allocation that satisfies the married-state budget constraint (F.15), in which no spouse consumes a negative amount (F.16), and in which the utility of spouse $-i$ in the married state (under the proposed allocation) is at least as high as the maximum single-state utility $V_i^S(\mathbf{I})$ (F.14). If that didn't hold, the couple would divorce, in contrast with the fact that spouse i is maximizing the married state utility.

To define the indirect utility in the single-state, assume that after divorce the wealth is split, so that each spouse keeps a share a of their own wealth and a share $1 - a$ of the spouse's wealth (if $a = 0.5$, then the wealth is split equally; if $a = 1$, then each spouse keeps their own wealth).

Hence, the single-state indirect utility is defined as follows[3]:

[3]For simplicity the prices of goods are normalized to one.

$$V_i^S(\mathbf{I}) = \max_{Q_i} U_i^S(Q_i) \text{ subject to}$$

$$Q_i \leq aI_i + (1-a)I_{-i}$$

If the utility function is increasing, each spouse consumes the entire single-state budget, so that the single-state utility maximization problem yields that the indirect single-state utility is $V_i^S(\mathbf{I}) = U_i^S(aI_i + (1-a)I_{-i})$.

Applying the Nash bargaining rule, which treats each individual equally (symmetric bargaining), the solution to the bargaining problem of the household is obtained by solving

$$\max_{Q} \left(U_h^M(Q_h) - V_h^S(\mathbf{I})\right)\left(U_w^M(Q_w) - V_w^S(\mathbf{I})\right) \text{ subject to} \tag{F.17}$$

$$Q_w + Q_h \leq I_w + I_h$$

$$\mathbf{Q} \geq 0$$

For simplicity let us assume that the utility functions are linear in consumption.

Furthermore, we allow husbands and/or wives to receive additional utility from the husband being a breadwinner—"male breadwinner norm".[4] Specifically, let us assume that the married-state utility of the husband/wife can be directly affected by the difference in income between the husband and the wife.

$$U_i^M = \alpha_i Q_i + g_i(\Delta I) \;\; \forall i \in \{h, w\}$$

where $\Delta I = I_h - I_w$ and $g_i(\Delta I)$ is increasing in ΔI. For simplicity, we assume that $g_i(\Delta I) = \gamma_i \Delta I$, where $\gamma_i \geq 0$.

Let us specify the utility functions of single individuals as:

$$U_i^S = \beta_i Q_i \;\; \forall i \in \{h, w\}$$

[4]Bertrand et al. (2015) demonstrates that there is an aversion to a situation where the husband earns more than the wife, driven by gender identity norms.

Notice that this implies that $V_i^S = \beta_i(aI_i + (1-a)I_{-i}) \quad \forall i \in \{h, w\}$, since U_i^S is increasing.

F.2.1 Gains from Marriage

Let us define the gains from marriage of spouse i as the difference between the indirect utility when married and when single denoted by $\Delta V_i = V_i^M(I_w, I_h) - V_i^S(I_w, I_h)$.

The solution to the optimization problem yields the following expressions for the gains from marriage

$$\Delta V_w = \frac{-\alpha_w\beta_h\left((1-a)I_w + aI_h\right) - \alpha_h\beta_w\left((1-a)I_h + aI_w\right) + \alpha_w\alpha_h(I_h + I_w) + (\gamma_h\alpha_w + \gamma_w\alpha_h)(I_h - I_w)}{2\alpha_h}$$

$$\Delta V_h = \frac{-\alpha_w\beta_h\left((1-a)I_w + aI_h\right) - \alpha_h\beta_w\left((1-a)I_h + aI_w\right) + \alpha_w\alpha_h(I_h + I_w) + (\gamma_h\alpha_w + \gamma_w\alpha_h)(I_h - I_w)}{2\alpha_w}$$

Case 1. No breadwinner norm ($\gamma_h = \gamma_w = 0$). In this case, the effect of husband's income on the gains from marriage of husband and wife is, respectively,

$$\frac{\partial \Delta V_h}{\partial I_h} = \frac{\alpha_w\alpha_h\big(1 - (a\beta_h/\alpha_h + (1-a)\beta_w/\alpha_w)\big)}{2\alpha_w}$$

$$\frac{\partial \Delta V_w}{\partial I_h} = \frac{\alpha_w\alpha_h\big(1 - (a\beta_h/\alpha_h + (1-a)\beta_w/\alpha_w)\big)}{2\alpha_h},$$

In this case, the effect of husband's income on gains from marriage of both spouses can be positive only if $a\beta_h/\alpha_h + (1-a)\beta_w/\alpha_w < 1$.

The effect of wife's income on the gains from marriage of husband and wife is, respectively,

$$\frac{\partial \Delta V_h}{\partial I_w} = \frac{\alpha_w\alpha_h\big(1 - (a\beta_w/\alpha_w + (1-a)\beta_h/\alpha_h)\big)}{2\alpha_w}$$

$$\frac{\partial \Delta V_w}{\partial I_w} = \frac{\alpha_w\alpha_h\big(1 - (a\beta_w/\alpha_w + (1-a)\beta_h/\alpha_h)\big)}{2\alpha_h},$$

These effect are negative only if $a\beta_w/\alpha_w + (1-a)\beta_h/\alpha_h > 1$.

Proposition 1. If there is no breadwinner norm, the impact of a wife's income on the benefits derived from marriage is negative, while the impact of a husband's income is positive, only if $a > 1/2$, $\beta_w/\alpha_w > 1$, and $\beta_h/\alpha_h < 1$.

Proof of Proposition 1. As demonstrated above, the impact of a husband's income is positive, and the impact of a wife's income is negative, only if both of the following inequalities are satisfied

1. $a\beta_h/\alpha_h + (1-a)\beta_w/\alpha_w < 1$
2. $a\beta_w/\alpha_w + (1-a)\beta_h/\alpha_h > 1$

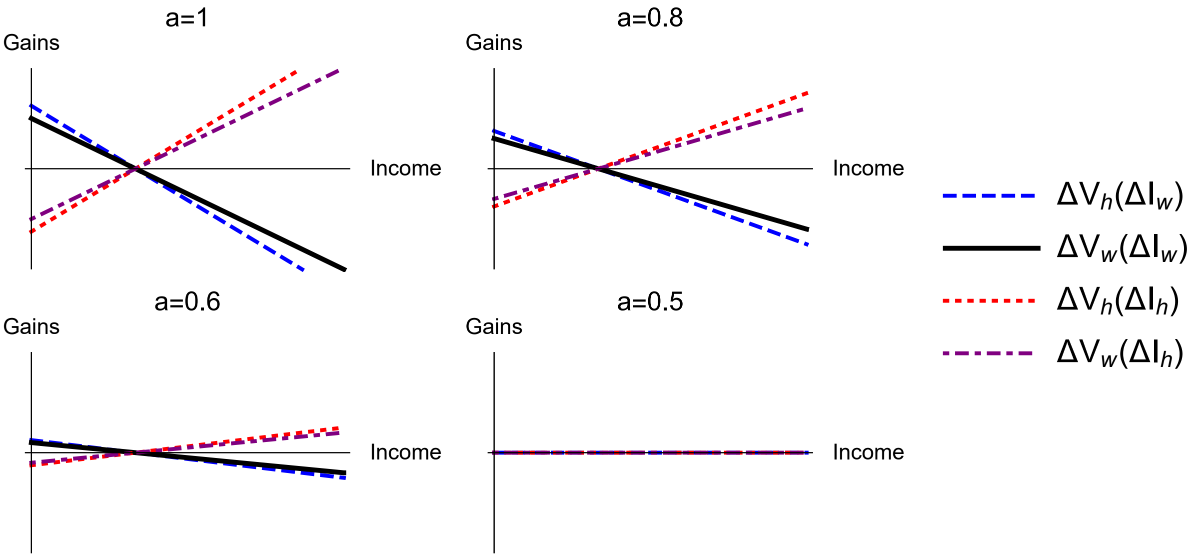
First, note that $a\beta_w/\alpha_w + (1-a)\beta_h/\alpha_h = a\beta_h/\alpha_h + (1-a)\beta_w/\alpha_w$ if $a = 1/2$, which implies that (1) and (2) cannot hold. Second, it is straightforward that both inequalities cannot hold if $\beta_i/\alpha_i \leq 1\ \forall i = \{w, h\}$ or $\beta_i/\alpha_i \geq 1\ \forall i = \{w, h\}$. Therefore, it must be that $\beta_i/\alpha_i > 1$ and $\beta_{-i}/\alpha_{-i} < 1$. Third, (1) and (2) imply that $a\beta_w/\alpha_w + (1-a)\beta_h/\alpha_h > a\beta_h/\alpha_h + (1-a)\beta_w/\alpha_w$, which implies that $(2a-1)\beta_w/\alpha_w > (2a-1)\beta_h/\alpha_h$. Given that $2a - 1 > 0$ for $a > 1/2$, $\beta_w/\alpha_w > \beta_h/\alpha_h$. Hence, it must be that $\beta_w/\alpha_w > 1$ and $\beta_h/\alpha_h < 1$. QED

Proposition 1 suggests that for the impact of a husband's income to be positive and the impact of a wife's income to be negative, it is crucial that $a > 1/2$, and that wives have a higher marginal utility in the single state as compared to the married state, while husbands experience the opposite. Figure F.7 depicts how the wife's and husband's gains from marriage change when either of the two sources of income (the wife's income or husband's income) increases, while keeping the other source of income constant, for different values of a between 0.5 and 1 and assuming that $\beta_w > \alpha_w$ and $\alpha_h > \beta_h$.

Case 2. With a breadwinner norm. We start by studying the case where $a = 1$, i.e. each spouse keeps their wealth upon divorce.

In this case, the effect of husband's income on the gains from marriage of husband and wife is, respectively,

Figure F.7: Changes in Gains from Marriage: Different Sharing Rules and no "Male Breadwinner Norm"



Notes. $x - axis$ denotes income of husband or wife. $y - axis$ denotes gains from marriage of husband and wife. $\beta_w = 1.2$, $\alpha_h = 1.25$, $\beta_h = \alpha_w = 1$, $\gamma_h = \gamma_w = 0$.

$$\frac{\partial \Delta V_h}{\partial I_h} = \frac{\alpha_w(\alpha_h - \beta_h + \gamma_h) + \alpha_h \gamma_w}{2\alpha_w}$$

$$\frac{\partial \Delta V_w}{\partial I_h} = \frac{\alpha_w(\alpha_h - \beta_h + \gamma_h) + \alpha_h \gamma_w}{2\alpha_h},$$

which is positive when $\alpha_h + \gamma_h + \frac{\gamma_w \alpha_h}{\alpha_w} > \beta_h$. Note that this inequality may hold even if $\beta_h > \alpha_h$ if γ_h and/or γ_w is big enough.

The effect of wife's income on husband's and wife's gains from marriage is, respectively,

$$\frac{\partial \Delta V_h}{\partial I_w} = \frac{\alpha_h(\alpha_w - \beta_w - \gamma_w) - \alpha_w \gamma_h}{2\alpha_w}$$

$$\frac{\partial \Delta V_w}{\partial I_w} = \frac{\alpha_h(\alpha_w - \beta_w - \gamma_w) - \alpha_w \gamma_h}{2\alpha_h},$$

which is negative when $\alpha_w < \beta_w + \gamma_w + \frac{\gamma_h \alpha_w}{\alpha_h}$. Again, this inequality may hold even if $\beta_w < \alpha_w$.

We next move to the case in which the wealth is split equally after the divorce ($a = \frac{1}{2}$). The effect of the husband's and wife's wealth on the husband's and wife's gains from marriage are respectively

$$\frac{\partial \Delta V_h}{\partial I_h} = \frac{\kappa}{\alpha_w} + 0.5\gamma_h + \frac{0.5\alpha_h\gamma_w}{\alpha_w}$$

$$\frac{\partial \Delta V_h}{\partial I_w} = \frac{\kappa}{\alpha_w} - 0.5\gamma_h - \frac{0.5\alpha_h\gamma_w}{\alpha_w}$$

$$\frac{\partial \Delta V_w}{\partial I_h} = \frac{\kappa}{\alpha_h} + 0.5\gamma_w + \frac{0.5\alpha_w\gamma_h}{\alpha_h}$$

$$\frac{\partial \Delta V_w}{\partial I_w} = \frac{\kappa}{\alpha_h} - 0.5\gamma_w - \frac{0.5\alpha_w\gamma_h}{\alpha_h}$$

where $\kappa = -\frac{1}{4}\beta_w\alpha_h + \alpha_w(\frac{1}{2}\alpha_h - \frac{1}{4}\beta_h)$.

Clearly, the two sources of income (I_h and I_w) can have different effects on the gains from marriage of the two spouses only if there is a breadwinner norm ($\gamma_h > 0$ and/or $\gamma_w > 0$). But when the breadwinner norm is introduced, our results can be fully accounted for by it.

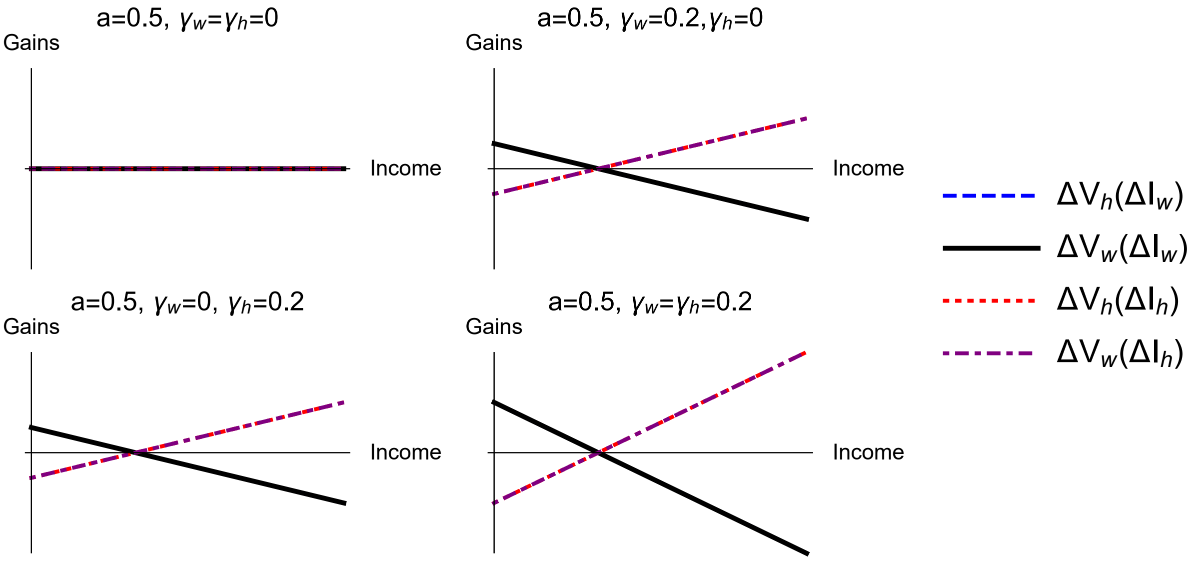
To see this, assume that the marginal utility of consumption is the same for both spouses and in both states, i.e. $\alpha_h = \alpha_w = \beta_w = \beta_h$. Figure F.8 shows the effect of the husband's and wife's income on the gains from marriage for different values of γ_w, and γ_h assuming that $\alpha_h = \alpha_w = \beta_w = \beta_h$ and $a = 1/2$. In all cases except when $\gamma_w = \gamma_h = 0$, the effect of wife's income on the gains from marriage of both spouses is negative, while the effect of husband's income on both gains from marriage is positive, which is consistent with the empirical results of the paper. In contrast, when $\gamma_w = \gamma_h = 0$, the source of income does not matter for the gains from marriage.

F.2.2 The Allocation of Consumption Within Marriage

We now analyze how consumption is allocated within the marriage under different assumptions about preferences and sharing of wealth in case of divorce.

First, assume that there is no "male breadwinner norm" ($\gamma_w = \gamma_h = 0$). It can be shown that

Figure F.8: Changes in Gains from Marriage: Male Breadwinner Norm under Different Sharing Rules.



Notes. $x - axis$ denotes income of husband or wife. $y - axis$ denotes gains from marriage of husband and wife. $\beta_w = \alpha_h = \beta_h = \alpha_w = 1$, $a = 1/2$.

$$\frac{\partial Q_w}{\partial I_w} - \frac{\partial Q_h}{\partial I_w} = (a-1)\beta_h/\alpha_h + a\beta_w/\alpha_w$$

$$\frac{\partial Q_h}{\partial I_h} - \frac{\partial Q_w}{\partial I_h} = a\beta_h/\alpha_h + (a-1)\beta_w/\alpha_w$$

These equations implies that the effect of one's own income on consumption will always be greater than the effect of the spouse's income on consumption when $min\{(a-1)\beta_h/\alpha_h + a\beta_w/\alpha_w, a\beta_h/\alpha_h + (a-1)\beta_w/\alpha_w\} > 0$. It can be shown that this condition is equivalent to $\frac{1-a}{a} < min\{\frac{\alpha_w\beta_h}{\alpha_h\beta_w}, \frac{\alpha_h\beta_w}{\alpha_w\beta_h}\}$. Hence, unless the marginal utility of consumption in the single and married state differ significantly between spouses, divorce splits which are not close to equal will imply the winning spouse increases consumption more also within the marriage. For instance, suppose one spouse has twice as much marginal utility in the single state than in the married state, whereas the other spouse has twice as much marginal utility in the married state than in the single state. Then, for one's own income to be more important than their spouse's income for individual consumption, the ratio of $1-a$ to a must be lower than $1/4$, implying a must be greater than $4/5$. In general, this condition implies that one's own income will be more important for an individual's consumption than their spouse's income when the individual retains most of their wealth in case of divorce.

Next, let us assume that there is a "male breadwinner norm" and equal divorce-splits ($a = 0.5$). Additionally, let us assume that husbands and wives have similar preferences ($\alpha_w = \alpha_h = \alpha$ and $\beta_w = \beta_h = \beta$). Then it can be shown that

$$\frac{\partial Q_w}{\partial I_w} - \frac{\partial Q_h}{\partial I_w} = \frac{\partial Q_h}{\partial I_h} - \frac{\partial Q_w}{\partial I_h} = \frac{\gamma_w - \gamma_h}{\alpha}$$

This equation implies that own income is more important for individual's consumption than spouse's income only if the wife espouse the breadwinner norm more strongly than the husband ($\gamma_w > \gamma_h$).

F.2.3 Summary

To sum up, we have developed a model of symmetric bargaining between two spouses. We show this model could rationalize our empirical results (e.g., divorce risk increasing in wives' lottery wealth but decreasing in husbands' lottery wealth) if the winner retains most of the lottery wealth in case of divorce and the husband has greater marginal utility of consumption when married, while the wife has greater marginal utility of consumption when single. If lottery wealth is instead split equally in case of divorce, a male breadwinner norm can generate similarly differential effects depending on which spouse won the lottery. We also show non-equal splits in case of divorce generates larger consumption increases for the winning spouse as long as the marginal utility of consumption in the married and single states are fairly similar for both spouses.

Appendix G Wealth Division after Separation

G.1 Swedish Divorce Law

Divorces in Sweden are governed by the Swedish Marriage Code of 1987, ("*Äktenskapsbalken*").[5] Below, we summarize some points relevant for the interpretation of our results (see also Boele-Woelki et al. (2004) and the Online Appendix in Cesarini et al. (2017)).

[5] `https://lagen.nu/1987:230#K5`

1. Divorce proceedings begin with a request that a court issue a divorce decree. No special grounds are needed to request divorce and the couple need not be in agreement.

2. The divorce decree will typically be approved only after a six-month "reconsideration period" has passed if any of the following conditions holds.

   (a) Only one of the spouses wishes for the marriage to be dissolved.

   (b) The couple have children under the age of 16.

   (c) Both spouses wished for the marriage to be dissolved, but requested a reconsideration period.

   If none of the above conditions hold, a divorce typically becomes effective within three weeks of the filing of the request for the divorce decree.

3. Property specified in a prenuptial agreement or acquired through gifts, inheritances or insurance payments is *not* considered marital property. Remaining property is generally considered marital property.

4. While a marriage is ongoing, each spouse has legal control over his/her property and is responsible for his/her debt. Hence, a lottery prize awarded to one of the spouses need to be shared equally.[6]

5. The law stipulates that in the event of divorce, all marital property be distributed between spouses. The default rule is that the marital property be divided equally between the spouses.

Figure A20 from Online Appendix to Cesarini et al. (2017) shows that most Swedish married couples do not have prenuptial agreement and therefore should divide their marital property if their marriage is dissolved. According to a survey of divorced couples conducted in Brattström (2011), in two of three cases couples reported that marital property was split equally between them. Couples who reported that their marital property was not split equally explained it by (i) agreement before the division, (2) existence of prenuptial agreement, (3) some property was a gift or inheritance.

[6]Indeed, the results in Appendix Table A.14 show that the prizes are not shared equally: lottery wealth has significantly larger effect on the wealth of the non-winning spouse.

G.2 Analysis of Nuptial Agreements

In Sweden, nuptial agreements have to be registered with the Swedish Tax Authority. Though the Tax Authority does not have individual-level data on nuptial agreements, the individual nuptial agreements are public information. To get a sense of what typical nuptial agreements look like, we requested the Swedish Tax Authority to send us 20 randomly selected nuptial agreements for each week in 2013. In the end, the Tax Authority sent us 997 nuptial agreements. This sample included both original nuptial agreements and changes and amendments to existing agreements.

The most common form of nuptial agreement (542, or 54.4%), listed specific property that should be exempted from marital property. Out of these, 160 listed property of both spouses (including three same-sex marriages), while 164 contracts listed only property of the husband while 218 only listed property of the wife. Typical property to denote private are real estate or stocks. Some contracts declare all property pertaining to the husband or wife is to be viewed as private. The second most common broad category (323, or 32.4%) states there should be no marital property, thus both including property the spouses have already acquired or may acquire in the future. Other, less common, types of contract stipulate property acquired before the signing of the contract or the marriage date should be private property (implying property acquired thereafter would be marital property). There are also nuptial agreements stating all property should be viewed as marital property, thus confirming the default rule (a fraction of such contracts could imply changing a previous nuptial agreement which indicated a non-equal split of assets).

G.3 Empirical Analysis of Wealth Splits

In this Section, we describe our analysis of wealth dynamics before and after separation in married and cohabiting couples. Specifically, we analyze how the spousal wealth gap changes around the time of separation. The goal of the descriptive analysis below is to gauge whether it is common to have unequal wealth levels within a marriage and after separation. It should be noted that the theoretical derivation in Section F.2 suggests that when wealth is not split equally between spouses after separation, our empirical results for marriage dissolution are consistent with different preferences of men and women in married and single states, as well as with social norms that suggest that the husband should be the primary breadwinner. However, if wealth is split equally, our results cannot be explained by different preferences of men and women, but rather by social norms. Therefore, understanding the dynamics of wealth before and after separation can provide insight into the potential mechanisms behind our results.

We use the entire population of couples (married or cohabiting with children) for which we have *Wealth Registry* data available for 1999-2007 before and after separation. Therefore, then limit the sample to individuals who experienced a *separation even*t between 2000 and 2006, who had been cohabiting with children or married for at least three years prior to the separation, and for whom we have at least one annual observation both before and after the separation event.[7] We then exclude individuals outside the age range 25-44.

For each couple, we calculate the share of net wealth held by the richer partner in the years before and after the separation. The division of wealth is set to 0:100 if one partner has negative wealth and the other spouse has positive wealth. We exclude couples with small values of net wealth (below 100,000 SEK).

Figure 3 shows the estimated cumulative distributions of the wealth share of the wealthiest partner (at each point of time) the year before (Panel A) and the year after the separation (Panel B). Figure A.5 replicates Figure 3 for couples with total net wealth in the year prior to separation of more than 500,000 SEK, displaying a similar pattern of within-couple wealth inequality. Figure A.6 shows the wealth share of the wife the year prior to separation and the year after separation. The results suggest that the median wife's wealth share is 47.8% the year prior to separation and it is 36.2% the year after separation, which again suggests that wealth inequality is higher after separation.

The results in Figure 3 are subject to two caveats. First, because as certain types of assets are excluded from the *Wealth Registry* (e.g., cash, cars and art) or lack exact market values (notably, real estate), wealth might be measured with error. As long as such measurement errors are perfectly correlated across partners, the impact on measured

[7]By separation, we mean either that a married individual's marital status changes from married to divorced, or that a cohabiting individual with children moved out from their partners during the analyzed period.

within-couple wealth inequality will be limited. However, if separations increase each partner's idiosyncratic measurement error – for instance because separating partners buy real estate on their own – post-separation wealth inequality will be overestimated. A second caveat is that, because which partner is wealthiest may change, higher inequality post-separation does not rule out redistribution of wealth from the wealthier partner at the time of separation.

To investigate the importance of these two caveats, we regress each partner's post-separation share of assets on the pre-separation share. A coefficient of one implies the richer partner keeps his or her full share of assets post-divorce. A coefficient of 0 implies the post-separation wealth share is independent of the pre-separation wealth level. An equal split of assets is a sufficient, but not necessary, condition for the coefficient to be 0. Idiosyncratic measurement error in post-separation wealth levels biases the coefficient toward 0. Table A.13 shows the coefficients from this regression are 0.62 for married couples with children, 0.68 for married couples without children and 0.73 for cohabiting couples. For each type of couple, we reject both equal splits (a coefficient of 0) and complete lack of redistribution from the richer to the poorer partner (a coefficient of 1). Although redistribution is higher for married couples, the small difference compared to cohabiting couples indicate marriage has a small impact on the division of assets.

Columns (2) and (3) of Table A.13 show the same analysis for wealthier couples (with total net wealth of 500,000 SEK of above) and for couples where wife had more than 50 percent of wealth prior to separation respectively. The results are similar to the baseline specification from column 1, confirming the robustness of the conclusion that couples do not split their wealth equally after separation.

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