NBER WORKING PAPER SERIES

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Working Paper 24667
http://www.nber.org/papers/w24667

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2018

We thank Agneta Berge, Ed Kong, Chanwook Lee, Tuan Nguyen and Becky Royer for excellent research assistance. We thank seminar participants at Columbia, Essex, IFN, HECER, LSE, Michigan, NIPE (Braga), SOFI, Uppsala, Vienna and Princeton for comments. Daniel Benjamin, Fredrik Bergdahl, Martin Berlin, Samantha Cherney, David Laibson, Erik Mohlin, Abhijeet Singh and Lise Vesterlund provided especially helpful feedback at various stages of the project. The study was supported by the Swedish Research Council (B0213903) and the Hedelius Wallander Foundation (P2011:0032:1). The collection and analysis of the survey data was approved by the Regional Ethical Review Board in Stockholm on April 7, 2016 (2016/524-31/5). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 24667
May 2018
JEL No. D69,I31

ABSTRACT

We surveyed a large sample of Swedish lottery players about their psychological well-being and analyzed the data following pre-registered procedures. Relative to matched controls, large-prize winners experience sustained increases in overall life satisfaction that persist for over a decade and show no evidence of dissipating with time. The estimated treatment effects on happiness and mental health are significantly smaller, suggesting that wealth has greater long-run effects on evaluative measures of well-being than on affective ones. Follow-up analyses of domain-specific aspects of life satisfaction clearly implicate financial life satisfaction as an important mediator for the long-run increase in overall life satisfaction.

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A data appendix is available at http://www.nber.org/data-appendix/w24667

1 Introduction

Observational studies consistently find that happiness, life satisfaction and other facets of well-being are positively correlated with wealth and income (Diener et al. 1999, Diener & Biswas-Diener 2002, Biswas-Diener 2008, Sacks, Stevenson & Wolfers 2012, Deaton 2008). However, the extent to which these associations arise due to causal pathways from wealth to well-being remains poorly understood (e.g., Frey & Stutzer 2002, Clark, Frijters & Shields 2008, Dolan, Peasgood & White 2008). Considerable uncertainty therefore remains about the magnitude and persistence of any income or wealth effects on subjective well-being. A large literature on hedonic adaptation argues that people adjust their aspirations upwards when their economic conditions improve (e.g., Brickman & Campbell 1971, Frederick & Lowenstein 1999), implying the long-term effect of positive economic shocks may be small (Frey & Stutzer 2002, Clark, Frijters & Shields 2008).

A better understanding of how wealth and income impact long-run well-being is important for both societal and individual priorities. At the individual level, people may exaggerate the importance of financial conditions for well-being (e.g., Kahneman et al. 2006). Estimates of the effect of wealth may therefore help people who value subjective well-being make more accurate tradeoffs between pecuniary and non-pecuniary aspects of life (e.g., Layard 2006). At the societal level, subjective well-being data are increasingly being used in welfare analyses, a topic of ongoing discussion (e.g., Frey & Stutzer 2002, Fleurbaey 2009, Benjamin et al. 2014). In such settings, estimates of wealth effects may prove valuable, e.g. in cost-benefit analyses that rely on subjective well-being data to elicit willingness-to-pay for non-market goods (surveyed in Dolan & Fujiwara 2016).

To credibly estimate the causal effects of wealth it is necessary to isolate a source of variation in wealth that is plausibly unrelated to other determinants of well-being. Studies in developed countries have exploited variation in wealth or income induced by lotteries (Brickman, Coates & Janoff-Bulman 1978, Lindahl 2005, Gardner & Oswald 2007, Kuhn et al. 2011, Apouey & Clark 2015), tax rebates (Lachowska 2017), or within-person changes over time (e.g., Frijters, Haisken-DeNew & Shields 2004, Frijters et al. 2006). These studies overwhelmingly conclude effects are positive, but the effect sizes vary substantially in magnitude.[1] Moreover, with the exception of Lindahl (2005), these studies do not consider long-term effects.

In this paper, we study long-run effects of wealth on well-being by leveraging the randomized assignment of lottery prizes in a sample of Swedish lottery players. We surveyed lottery players about their well-being 5 to 22 years after the lottery event. Our study has several methodological strengths. First, our data allow us classify players into groups within which we know the prize amount won is randomly assigned. Our estimates are based entirely on comparisons of players who are in the same group but were awarded prizes of different magnitudes. Second, because of the large sample size (3,362 players) and substantial prize pool ($277 million), our estimates have high precision relative to other work. Third, all main results are based on pre-registered analyses described in a publicly archived Analysis Plan (Östling, Lindqvist & Cesarini 2016).

We find that the long-run effects of wealth vary depending on the exact dimension of well-being.[2] There is clear evidence that wealth improves people's evaluations of their lives as a whole. According to our estimate, an after-tax prize of $100,000 improves life satisfaction by 0.037 standard-deviation (SD) units. We find no evidence that the effect varies by years-since-win, suggesting a limited role for hedonic adaptation over the time horizon we analyze. Our results suggest improved financial circumstances is the key mechanism behind the increase in life satisfaction. In contrast, the estimated effects on our measures with a

[1]Most studies in low- and middle income countries also find positive effects of wealth or income on well-being. For example, unconditional cash grants have been shown to improve short-run subjective well-being in Kenya (Haushofer & Shapiro 2016) and Malawi (Baird, de Hoop & Özler 2013), though not in Ecuador (Paxson & Schady 2010). Relatedly, a study of Chinese twins reports that the positive relationship between income and happiness persists in within-family analyses (Li et al. 2014). Hariri, Bjørnskov & Justesen (2015) finds that a decrease in real income induced by a currency devaluation in Botswana had large negative effects on subjective well-being.

[2]In the psychometric literature, it is common to make a distinction between affective and evaluative measures of well-being (Diener et al. 1999, Schimmack 2008). Measures derived from responses to questions about the frequency of various positive or negative feelings are classified as affective whereas questions that require respondents to report their evaluation of their life (or some aspect of their life) are often referred to as cognitive or evaluative.

stronger affective component – happiness and an index of mental health – are smaller and not statistically distinguishable from zero. Despite the strong correlation between happiness and life satisfaction in our data (0.86), we can statistically reject equal treatment effects of lottery wealth on these outcomes.

To help benchmark our results, we rescale our lottery-based estimates and compare them to gradients with respect to annual income (averaged over multiple years to smooth out transitory fluctuations). For happiness and mental health, our rescaled estimates are about one third the magnitude of the corresponding gradients. For life satisfaction, we find that our rescaled estimate is similar in magnitude to the income gradient. We also compare our main results to those reported in previous quasi-experimental studies of lottery players' well-being and show our study compares favorably both in terms of statistical power and the credibility of our causal inference.

Our paper is structured as follows. Section 2 describes our survey of lottery players and describes the representativeness of our estimation sample. Section 3 describes our identification strategy and provides evidence in support of our key identifying assumption that lottery prizes are randomly assigned conditional on factors we observe. Section 4 summarizes the results from our main analyses and benchmark our estimates against the cross-sectional gradients and previous lottery studies. Section 5 concludes with a broader discussion of our findings and their limitations. The Online Appendix contains appendix figures and tables and additional details about our analyses.

2 Data and Study Design

Our study was conducted in three stages. First, we identified a *Survey Population* composed of individuals from a large administrative sample of lottery players. Second, Statistics Sweden surveyed these individuals on our behalf. Third, Statistics Sweden supplied us with an anonymized data set with subjects' survey responses and administrative variables. For all members of the *Survey Population*, including non-respondents, we have information about a set of basic demographic characteristics from Swedish registers and lottery-specific variables needed to implement our empirical strategy.

The analyses reported in this paper follow the procedures specified in an Analysis Plan (Östling, Lindqvist & Cesarini 2016) publicly accessible via the URL `https://osf.io/t3qb5/` and archived before the survey data were made available to us. The purpose of preregistration was to minimize readers' concerns about data-mining and undisclosed specification searches and to make transparent the distinction between pre-registered and post hoc analyses. All aspects of the main analyses were fully specified before the survey data were delivered to us. Specifically, we pre-specified the criteria for inclusion in the estimation sample; three diagnostic tests for endogenous attrition; our set of primary outcomes; variable coding (including handling of missing values and outliers); the estimating equation; heterogeneity and robustness analyses, and how we intended to adjust the p-values for our primary outcomes for multiple-hypotheses testing.

In formulating the plan, our goal was not only to reduce the number of investigator degrees of freedom in our main analyses, but to eliminate them altogether. We successfully executed the pre-registered analyses without having to make any additional judgment calls due to omissions or ambiguities in the Analysis Plan. The Analysis Plan also described our intention to benchmark our final estimates to household-income gradients and estimates previously reported in quasi-experimental studies. These comparisons were conducted according to the pre-registered procedures. However, we made no attempt to fully specify every detail of these comparisons before accessing the survey data.

2.1 Survey Population

The *Survey Population* was drawn from a large administrative sample that has been used in several previous studies on the impact of wealth on register-based outcomes such as health, mortality and children's outcomes (Cesarini et al. 2016), labor supply (Cesarini et al. 2017) and participation in financial markets (Briggs et al. 2015). In determining which members of the administrative sample to survey, a primary goal was to retain as much as possible of the lottery-prize variation.

We survey players from three of the four lotteries in the administrative sample: Kombi, Triss-Monthly and Triss-Lumpsum.[3] Kombi is a monthly subscription lottery with approximately 500,000 subscribers, the proceeds of which are donated to the Swedish Social Democratic Party. The administrative sample contains information on the number of lottery tickets and large prizes won for all Kombi participants between 1998 and 2011. Triss is a highly popular scratch-off lottery run by the Swedish government-owned gaming operator, Svenska Spel. We have information on two types of Triss prizes which qualify the winner to a daily TV show where the size of the prize is determined by a new lottery draw. At the show, Triss-Lumpsum winners (1994 to 2011) win a lump-sum prize between $7,000 and $700,000. Winners of the Triss-Monthly prize (1997 to 2011) win a monthly income supplement. The size ($1,400 to $7,000) and duration (10 to 50 years) of the supplement are determined by separate tickets which are drawn independently. We convert the Triss-Monthly to net-present value using a discount rate of 2 percent.

To define the *Survey Population*, we first identified all winners from the Triss lotteries and all large-prize winners from Kombi (defined as players who won at least 1M SEK). We then imposed a number of sample restrictions summarized in Table A1. The Analysis Plan contains a detailed description of, and motivation for, each restriction. By far the most important restriction is that we only survey individuals aged at most 75 in 2016, the year of the survey. Applying the full set of sample restrictions left 259 large prizes from Kombi, 3,294 Triss-Lumpsum prizes and 608 Triss-Monthly prizes. We supplied information about these winners to Statistics Sweden, who dropped prizes won by individuals who were deceased or lacked an official Swedish address of residence in 2016. In a final step, they added four

[3]We elected not to survey participants in the fourth lottery used in our prior studies – the prize-linked savings accounts (PLS) – because nearly all of the large lottery prizes in this sample were awarded in the 1980s and 1990s, making it less likely that we would be able to detect treatment effect on an outcome measured in 2016. An additional consideration was that a substantial fraction of the PLS players are deceased.

controls for each large-prize winner in Kombi to the *Survey Population.* The four controls were randomly selected from the set of non-winning Kombi players whose sex, year of birth and number of tickets owned exactly matched those of the winner in the month of win. This leaves our *Survey Population* of 4,840 observations: 241 Kombi large-prize events and 964 (241×4) matched controls, 3,065 Triss-Lumpsum prizes and 570 Triss-Monthly prizes. Because a small number of individuals appear more than once, these 4,840 observations correspond to 4,820 unique individuals.[4]

2.2 Survey Protocol

In early fall of 2016, Statistics Sweden mailed a letter of invitation to all members of the *Survey Population* (see Figure A1 for summary of the timeline). The letter was accompanied by the survey, a return envelope, and a 100 SEK gift certificate. To reduce experimenter demand effects, the letter made no mention of lotteries.[5] Subjects who failed to return the survey after the first mailing were sent three reminders. Triss-Monthly players who had failed to return a survey after the third reminder were also contacted by telephone and asked to return the mail-in survey. (For budgetary reasons, we limited the telephone reminders to non-respondents from Triss-Monthly). Three weeks after the end of the regular data-collection via mail, Statistics Sweden tried to reach 501 randomly selected non-respondents by telephone. Subjects who answered the phone were invited to participate in an abbreviated phone version of the survey.

[4]Individuals may appear in the data more than once because they won the lottery on more than one occasion, because we draw the controls in Kombi with replacement, or because of overlap between the lottery samples.

[5]The final data set delivered to us contains subjects' survey responses and some basic socioeconomic variables from administrative registers. Statistics Sweden required that information about these registers be available to interested subjects, along with information about the selection of the *Survey Population.* To accommodate this requirement, the cover letter referred survey invitees interested in learning more to a website with information about the registers and details on the selection of the *Survey Population.* Unbeknownst to the subjects, each letter's website URL was unique, and the final data delivered to us therefore contains information about which subjects accessed the website. Only six subjects did, implying any resulting biases are likely to be negligible.

2.3 Respondents Sample

Statistics Sweden received mail-in surveys from individuals corresponding to 3,251 of the 4,840 observations of the original *Survey Population*. Another 111 players (out of 501) participated in the abbreviated telephone survey, bringing the total response rate to 69%.[6] We refer to the survey respondents as our *Respondents Sample*. Table 1 shows the survey response rate and the distribution of prizes won for each lottery and our pooled sample. Here and in all that follows, lottery prizes are net of taxes and measured in units of year-2011 dollars. Although the majority of prizes are modest, most of our identifying variation comes from prizes in the range \$100,000 – \$800,000.[7] Even though the *Respondents Sample* constitute less than 1% of the pooled lottery sample analyzed in Cesarini et al. (2016), the oversampling of large-prize winners ensures that about one third of the identifying variation in lottery wealth in the administrative sample is retained.

Table 2 compares the distribution of pre-lottery baseline characteristics of the individuals in the *Respondents Sample* and the *Survey Population* with a random sample of Swedish adults. The representative sample has been reweighted to match the sex- and age distribution in the *Respondents Sample*. Players are substantially more likely to be born in Sweden (92.4% versus 83.8%). However, the representative sample was drawn in 2010 and the fraction of the Swedish population that is foreign-born grew steadily in the lottery years. Therefore, the observed difference understates the representativeness of players in most lottery years. Players are similar to the Swedish population in terms of marital status and number of children residing in their household. They are less likely to have attended college but have higher labor incomes, on average. In both cases, the differences are modest (25.8% versus 30.1% and \$35,000 versus \$32,000, respectively). Overall, the similarity in baseline characteristics is

[6]The effective response rate varies between outcomes because not all respondents respond to all questions in the mail-in survey and because the abbreviated phone survey did not include all questions.

[7]One way to quantify the importance of large prizes is to consider the change in treatment variation (the number of observations times the variance in lottery prizes demeaned at the level of the groups defined in Section 3) when prizes above some cutoff are dropped. For example, dropping the 415 prizes above \$200,000 (column (5) of Table 1) reduces treatment variation by 91%.

reassuring, though we cannot rule out that players who select into the lottery differ from the population in unobservables in ways that could impair the generalizability of our findings.

2.4 Primary Outcomes

The Analysis Plan defined four primary outcomes. The first outcome, *Happiness,* is based on the respondent's answer to the question "All things considered, how happy would you say that you are?" The respondent is asked to select one response alternative among 11 numerically coded options ranging from 0 ("Extremely unhappy") to 10 ("Extremely happy"). Our second outcome, *Overall Life Satisfaction* (*Overall LS*, for short), is derived from the answer to the question "Taking all things together in your life, how satisfied would you say that you are with your life these days?" The respondent is asked to select an option from an 11-point scale ranging from 0 ("Extremely dissatisfied") to 10 ("Extremely satisfied").

Our third outcome, *Mental Health*, is constructed from responses to the 12-item version of the General Health Questionnaire (Golberg & Williams 1988). Originally developed as a screening instrument for mental health, the GHQ-12 is commonly used to measure an individual's level of psychological well-being. Each item requires respondents to indicate, on a four-point scale, how often during the last two weeks he or she has experienced a specific positive or negative emotion. The response category chosen on each item is then converted to an integer between 1 and 4, with higher values indicating greater well-being. The final variable is defined as the sum of the 12 numerical values, and is hence in the range of 12 to 48, with higher values denoting greater well-being.

Our fourth primary outcome is *Financial Life Satisfaction* (*Financial LS*, for short), one of nine domain-specific aspects of life satisfaction measured in our survey. Each domain was measured by a single question with a six-point response scale ranging from 1 ("Very dissatisfied") to 6 ("Very satisfied").

Researchers often make a conceptual distinction between evaluative (sometimes referred to as cognitive) and affective components of subjective well-being (Diener et al. 1999, Schimmack

2008). *Happiness* is a hybrid of these two dimensions, because our measure is based on a question with a clear evaluative component ("All things considered..."), yet at the same time asks about pleasant feelings. By contrast, *Overall LS* and *Financial LS* are evaluative: respondents are required to form an assessment, either of their life as a whole, or of their overall financial situation. Finally, our measure of *Mental Health* is affective, as the items included in the battery all ask about the frequency with which the respondent has recently experienced a range of pleasant and unpleasant feelings. Despite their differences, *Overall LS* and *Happiness* are highly correlated both with one another (0.86) and with *Mental Health* (0.70 in both cases). *Financial LS* is modestly positively correlated with each of the three other primary outcomes, with correlations ranging from 0.39 (*Mental Health*) to 0.46 (*Overall LS*).

3 Analytic Framework

3.1 Estimation and Identification Strategy

Our identification strategy exploits the fact that the lottery prizes in our samples are randomly assigned conditional on player characteristics we observe. We estimate the long-run causal impact of lottery wealth by ordinary least squares, using the following estimating equation:

$$(1) \quad y_{is} = \alpha L_{i,0} + \mathbf{Z}_{i,-1}\boldsymbol{\gamma} + \mathbf{X}_i\boldsymbol{\beta} + \varepsilon_i,$$

where the time of the lottery event is normalized to $t = 0$. y_{is} is a measure of well-being standardized to unit variance for respondent i measured s years after the lottery event. Because we survey people who participated in lotteries between 1994 and 2011 in 2016, s varies between 5 and 22. $L_{i,0}$ is the prize (in \$100,000) awarded to individual i at $t = 0$ and $\mathbf{Z}_{i,-1}$ is a vector of baseline characteristics measured at year-end in the year prior to the lottery event. $\mathbf{X}_i$ is a set of indicator variables for groups of lottery players within which the prize money is randomly assigned. We control for the "baseline controls" $\mathbf{Z}_{i,-1}$ solely to improve statistical precision.

In Kombi, we construct the group identifiers in $\mathbf{X}_i$ by assigning each large-prize winner to the same group as his or her matched controls (occasionally, large prize winners in the same draw have identical ticket balances, are of the same sex and share a year of birth; when this happens we assign multiple winners to the same group identifier). In the two Triss lotteries, two players share a group identifier if and only if they won the same type of prize (Lumpsum or Monthly) in the same year and under the same prize plan. Because the prize plan determines the distribution from which prizes of either type are drawn, conditioning on prize plan guarantees the size of the lottery win is random.

Throughout, we report p-values based on analytical standard errors that have been clustered (Zeger & Liang 1986) at the individual level. In our main analysis of the primary outcomes, we also report permutation-based p-values constructed by simulating the distribution of the relevant test statistic under the null hypothesis of zero treatment effects (Young 2017). In each simulation iteration, we independently permute the prize column in each group. We next use Equation (1) to generate an estimate of the treatment effect of wealth. Repeating this process 10,000 times gives us a simulated distribution that we use to calculate the probability of observing a test statistic as extreme as the one observed under the null hypothesis. Finally, in our main analyses of the primary outcomes, we also report p-values that have been adjusted to account for the fact that we examined four primary outcomes. To calculate these family-wise error rate adjusted p-values, we apply the free step-down resampling method of Westfall & Young (1993). In the tables, we refer to the resulting p-values as FWER-adjusted p-values.

In our estimating equation, y_{is} depends linearly on $L_{i,0}$ even though the true relationship is likely to be concave. However, from a life-cycle perspective, the concavity is modest as long as the lottery prize is not very large relative to lifetime income. For example, suppose well-being is linear in log lifetime income and consider a household with remaining pre-win lifetime income of \$1.37 million, the approximate median in our sample.[8] For a \$400,000 prize, the derivative of y_{is} with respect to lifetime income is 1/1.37 for the first dollar won and 1/(1.37+0.4) for the last dollar, implying a linear specification offers a decent first approximation to the data.

3.2 Survey Non-Response and Tests of Endogenous Attrition

A potential concern about our identification strategy is that lottery wealth directly influences survey participation, potentially introducing endogeneity in the *Respondents Sample*, even if our identifying assumption holds in the *Survey Population.* To test for such selection biases, we conducted three pre-registered diagnostic tests for endogenous selection. All were conducted and reported exactly as described in the Analysis Plan (pp. 8-13). In test one, we found no evidence that survey participation is affected by lottery wealth (Table A2). In test two, we found no evidence of imbalance in baseline covariates measured prior to the lottery in neither the *Survey Population* nor the *Respondents Sample* (Table A3). In test three, we found that the estimated effects of lottery wealth on net wealth, debt, capital income and labor income do not change systematically when we restrict attention to the *Respondent Sample* by omitting the survey non-respondents from the estimation sample (Table A4). Overall, the results from these diagnostic tests bolster the credibility of our causal estimates, to which we now turn.

4 Results

4.1 Primary Outcomes

Figure 1 displays our estimates of the long-run effect of lottery wealth on each of the primary outcomes (see Table 3 for the underlying data). For all outcomes, we estimate positive

[8] The median annual household disposable income in year $t = -1$ was \$47,000 in our sample. The lifetime income we use in our heApriluristic calculation is simply the product of this income figure and 29, the median remaining lifespan of lottery players in their year-of-win assuming a lifespan of exactly 80 years.

effects of lottery wealth. The estimated effects on *Overall LS* and *Financial LS* are, respectively, 0.037 SD units and 0.067 SD units per $100,000 won, and remain significant after our multiple-hypothesis adjustment. For *Happiness* and *Mental Health*, the corresponding point estimates are 0.016 and 0.013, respectively.[9] Neither estimate is statistically distinguishable from zero, but for both outcomes, we can reject treatment effects equal to those found for *Overall LS* and *Financial LS*.[10] It is noteworthy that we can rule out equally sized treatment effects of wealth on *Overall LS* and *Happiness* ($p < 0.001$), despite their very high pairwise correlation (0.86).

Table A5 reports the results from two pre-specified robustness tests. In the first, we reweight the sample so that the share of phone-survey respondents in the estimation sample matches the population share of mail-in survey non-respondents (33%). The reweighted estimates for the two primary outcomes measured by the telephone survey – *Overall LS* and *Happiness* – are similar to the main results. In the second, we rerun the analyses omitting players who won prizes above 4M SEK ($580,000). For all outcomes the coefficient estimates are similar to the baseline results, though with larger standard errors.

To explore potential mechanisms, we conducted post hoc analyses of seven domain-specific measures of life satisfaction. The results of these analyses are shown in in Figure 1 (see Table A6 for the underlying estimates). For each of the seven outcomes – health, spare time, friends, relatives, home, neighborhood and society overall – we can rule out treatment effects as large as those found for *Financial LS* and, except for a marginally significant effect on spare time, none of the estimated effects are statistically distinguishable from zero. Overall, these post hoc analyses suggest that *Financial LS* mediates much of the observed

[9]We note that our estimate of the effect on *Mental Health* (0.013) is similar to the appropriately rescaled reduction in consumption of prescribed mental health drugs of 0.023 SD units in our previous work on lottery winners' health (Cesarini et al. 2016).

[10]In post hoc analyses, we also reran the analyses of *Happiness*, *Overall LS* and *Financial LS* using the "blow-up and cluster" conditional logit estimator proposed by Mukherjee et al. (2008) which has recently been shown to work well in a related context (Baetschmann, Staub & Winkelmann 2015). For *Happiness* and *Overall LS* the point estimates are nearly identical, whereas the effect of wealth on *Financial LS* increases modestly (from 0.067 to 0.080).

long-run treatment effect of lottery wealth on *Overall LS*.[11]

The claim that the long-run, positive, effect of lottery wealth on *Overall LS* is mediated by improved *Financial LS* may seem hard to reconcile with a common folk wisdom according to which lottery winners routinely squander their wealth. Yet previous analyses of the Swedish administrative sample have found little evidence in support of the hypothesis that winners often consume frivolously following a win. Large-prize winners appear to enjoy sustained improvement in economic conditions that are robustly detectable for well over a decade after the windfall (Cesarini et al. 2016). Winners reduce their labor supply and gradually spend down the windfalls, but the reductions are modest, do not seem to depend on the type of prize (lump-sum or monthly installments), and spread out quite evenly over the entire time horizon for which we have post-lottery outcomes (Cesarini et al. 2017). They also invest a substantial share of the wealth in financial assets, often opting for low-risk bond products over equities (Briggs et al. 2015).

This evidence is well in line with conclusions from interview-based research on lottery winners in multiple countries (Kaplan 1987, Furåker & Hedenus 2009, Eckblad & Lippe 1994, Larsson 2011). For example, one study of American lottery winners concludes matter-of-factly that "contrary to popular beliefs, winners did not engage in lavish spending sprees" (Kaplan 1987, p. 168).

4.2 Heterogeneity

Again following pre-registered procedures, we reran our analyses in subsamples stratified by sex, age-at-win (below or above median), pre-lottery income (below or above median), years-since-win (before or after 2005) and type of prize (Triss-Monthly vs Triss-Lumpsum).[12] The results are shown in Figure 2 (see Table A7 for underlying data). Overall, the estimated

[11]Including *Financial LS* as an additional control in Equation (1) (similar to a Sobel mediation test) reduces the estimated effect of lottery wealth on *Overall LS* by 73%.

[12]As explained in our Analysis Plan, we exclude Kombi altogether in the heterogeneity analysis by type of prize because Triss-Lumpsum and Triss-Monthly winners are drawn from the same underlying population (people who procure Triss scratch-off lottery tickets). Excluding Kombi makes it less likely that any observed heterogeneity is due to factors correlated with winning a lumpsum prize.

treatment effects are similar across subsamples. For example, the long-run effects of lottery wealth on *Financial LS* and *Overall LS* show up quite consistently, with significant treatment effects ($p < 0.05$) on *Financial LS* in all eight subsamples.

We performed 20 tests of homogeneous effects (4 outcomes $\times$ 5 dimensions of heterogeneity) and we only reject the null hypothesis of equal effects (at nominal $p < 0.05$) in two instances: *Overall LS* by type of prize (Triss-Monthly vs Triss-Lumpsum) and *Mental Health* by years-since-win (before or after 2005). This is only one more rejection than expected by chance under the null hypothesis of homogeneous effects and overall, our analyses therefore provide no strong evidence of heterogeneous effects. We note that in our analyses by type of prize, the overall pattern of results is in the opposite direction to what one would expect if prize money paid as monthly installments helped winners with self-control smooth consumption. Our subsample analyses only yield clear evidence of a positive treatment effects among players who won lumpsum prizes.[13]

One notable finding is that the positive effects show little evidence of fading with the passage of time. Even when we restrict the sample to players surveyed at least 11 years after the lottery event ("Pre 2005") the treatment-effect estimates range from 0.038 SD units ($p = 0.062$) for *Happiness* to 0.058 SD units ($p = 0.004$) for *Overall LS*. To further explore how treatment effects vary by years-since-win, we conducted post hoc analyses, the results of which are summarized in Figure 3 (see Table A8 for underlying estimates). The estimated treatment effects on *Financial LS* decay with the passage of time, but for the remaining three outcomes, the pattern is in the opposite direction. The absence of fade-out suggests that there is little adaptation to the lottery win over the time window for which we have data (5-22 years after the lottery event). But this conclusion is subject to the caveat that year-of-win is not randomly assigned, so it is possible that early and late winners differ along some dimension that moderates the effect of wealth. Nevertheless, there is little doubt that

[13]The comparison between Triss-Lumpsum and Triss-Monthly is potentially confounded by non-linear effects of wealth. Since Triss-Monthly players win larger prizes, on average, non-linear effects of lottery wealth could produce heterogeneous effects across the Triss samples even if prizes with identical net present values have identical effects.

adaptation to the windfall is incomplete well over a decade after the lottery event.

4.3 Household-Income Gradients

Since lump-sum lottery prizes represent one-time increases in lifetime wealth, there is no unassailable method for comparing our causal estimates to the cross-sectional income correlations that have been the focus in much of the literature. However, the evidence that many players choose to spread out the gains fairly evenly and over long time horizons suggests that players often treat the windfall as a long-run supplement to annual income flows from other sources (Cesarini et al. 2016, Cesarini et al. 2017, Briggs et al. 2015). Following our Analysis Plan, we therefore convert each lottery prize to the annual payout it could sustain if it were annuitized over a 20-year period at an actuarially fair price, and rerun our main analyses with this alternative scaling. For example, a $100,000 prize corresponds to an increase in net annual income of $5,996.

We compare our annuity-rescaled treatment effects for each primary outcome to gradients estimated using a measure of household permanent income (average disposable income over the period 2004-2014), controlling for sex, a fourth-order polynomial in age and sex-by-age interactions. Because income is endogenous to the lottery outcome (Cesarini et al. 2017), we estimate the gradients only for individuals in the *Respondents Sample* who won prizes below $20K. The average prize won in this sample ($8,491) is small enough that any endogeneity is likely to be negligibly small. In preliminary analyses, we verified that the cross-sectional relationship between permanent annual income and our primary outcomes replicate standard patterns from the literature. Figure A2 shows that in our sample, the cross-sectional relationship between permanent annual income and each of our primary outcomes is positive and concave (Deaton 2008, Stevenson & Wolfers 2013). We also compare our rescaled treatment effects to gradients for Swedish respondents in two waves of the European Social Survey.[14]

[14] See Section 2 in the Online Appendix for details on the ESS gradients.

We compare our lottery estimates to the cross-sectional gradients in three different analyses, the first two of which are shown graphically in Figure 4 (see Table A9 and A10 for the underlying data from all three analyses). The upper panel of Figure 4 shows the rescaled estimates and gradients when well-being is assumed to be linear in household income. The rescaled estimates for *Happiness* and *Mental Health* are about one third as large as the gradients, whereas the rescaled estimates for *Overall LS* and *Financial LS* are similar in magnitude to the gradients. For both *Happiness* and *Mental Health*, we reject the null hypothesis that the causal effect is equal to the gradient.

It is common in the literature to assume well-being is linear in log income. To better compare our results to previous work, we therefore further rescale our lottery-based estimates to make them comparable to log-income gradients.[15] The lower panel of Figure 4 shows the log-income gradients fall within the normal range previously reported in rich countries (Stevenson & Wolfers 2013), and that the relationship between gradients and our lottery-based estimates is similar to the linear case. The causal effect of log income on *Overall LS* implied by our estimate (0.377) is thus similar to the log income gradient, while the implied effect for *Happiness* is substantially lower (0.165).

Finally, in Figure 5, we repeat the original linear analysis, but in subsamples stratified by permanent-income tertile. Here, the gradients are estimated using a piece-wise linear spline regression with two knots, one at each of the cutoff points that define the permanent-income tertiles. In the bottom income tertile, our treatment-effect estimates are bounded away from the income gradients (all $p < 0.045$), as shown in Figure 5. At medium and high incomes, the gradients are similar in magnitude to the causal estimates.

[15] To accommodate the linear-log functional form assumption, we calculated the natural logarithm of the sum of permanent income (based on pre-lottery income data only) and the annuitized prize. Our final estimates are from an instrumental variable analysis that uses lottery prizes to instrument for the log of the sum of permanent income and the annuitized prize. (We also tried alternative methods to accommodate the functional-form assumption with very similar results.)

4.4 Previous Lottery Studies

We identified five previous quasi-experimental studies of lottery players' well-being. Table 4 provides summary information about how our study compares to these studies along some key dimensions: outcome variables analyzed, lottery data used, effect sizes reported and identification strategy. To facilitate comparisons, the effect-size estimates have been rescaled for comparability with our main results in Table 3 (effects of $100,000 on an outcome with unit variance). Section 3 in the Online Appendix provides further details on the calculations underlying the data in Table 4. Here, we emphasize that cross-study comparisons based on data in the table are subject to two important interpretational caveats. First, even though most studies used survey measures similar (in several cases, identical) to ours, only one (Lindahl 2005) analyzed a long-run measure of lottery players' well-being. Second, the rescaled estimates are calculated under the simplifying assumption that the effect is linear in prize amount.

The first study listed (Brickman, Coates & Janoff-Bulman 1978) famously compared the happiness of 22 major lottery winners of the Illinois State Lottery to that of 22 controls domiciled in the same regions as the winners. The study found no statistically significant differences between winners and controls in terms of happiness (past, present or expected future). After re-scaling, we obtained a treatment-effect estimate of 0.014 with a standard error of 0.025. These rescaled estimates are therefore quite similar to what we report for *Happiness*, both in magnitude (0.014 vs 0.016) and precision (0.025 vs 0.014). However, the prizes won by the 22 lottery players are very large compared to lottery winners in subsequent studies, including ours, with an average prize of $1.18M (range $123K to $2.46M). The rescaled estimates we report for Brickman, Coates & Janoff-Bulman (1978) are therefore likely to be the most sensitive to plausible violations of the linearity assumption.

The next two studies listed reported large and positive effects of wealth on mental health, one using data from Sweden (Lindahl 2005) and the second using British data (Gardner & Oswald 2007). Apouey & Clark (2015) updated and extended the analysis of Gardner &

Oswald (2007) in several ways, including controlling for individual fixed effects in the analyses and adding data from survey waves that have subsequently become available. The follow-up study reported positive and statistically significant effects on life satisfaction and mental health measured two years after the lottery (but not on outcomes measured sooner). The next row shows information from a study of Dutch Postcode Lottery winners (Kuhn et al. 2011), finding a negative but statistically insignificant effect of lottery wins on happiness. The four studies that appeared after Brickman, Coates & Janoff-Bulman (1978) had rescaled estimates with standard errors *at least* 7 times larger than ours. Therefore, conditional on finding a statistically significant effect, the effects reported were very large compared to ours. If the true effect-parameters of these studies are not dramatically different from the effects we can rule out with high statistical confidence, these studies were under-powered.[16]

Of course, it may be inappropriate to use our estimates to inform calculations of the likely power of these other studies. For example, short-run effects of wealth may be substantially larger than long-run effects. The pattern of results is not easy to reconcile with this theory, however, since Kuhn et al. (2011) report a negative effect on happiness six months after the lottery and Apouey & Clark (2015) report larger treatment effects on outcomes measured two years after the lottery than on outcomes measured in the post-lottery year. This theory also fails to account for the results in the study that, like us, analyzed a long-run measure of well-being (Lindahl 2005). A second possibility suggested by the prize data in Table 4 is that the studies relied to a greater extent on identifying variation generated by small and modestly sized prizes. When we drop the largest prizes from our data, the estimated treatment effects increase for two out of four outcomes (Table A5). However, the implied non-linearity for these two outcomes is not nearly large enough for this factor alone to rationalize the stark

[16] To illustrate, suppose the true treatment effect on *Mental Health* in the previous studies was 0.044 SD units, the upper limit of the 95% confidence interval of our estimate. Then the statistical power of the three previous studies reporting statistically significant effects on mental health (Lindahl 2005, Gardner & Oswald 2007, Apouey & Clark 2015) ranged from 5.02% to 6.55% (at $\alpha = 0.05$). Conditional on finding a statistically significant effect, design calculations (Gelman & Carlin 2014) show that studies with such power will incorrectly sign the effect ("type S error") between 15% to 45% of the time, and overestimate (absolute value of) the effect size ("type M error") by a factor of at least five.

differences in effect sizes across studies.

The final column of Table 4 summarizes each study's identification strategy, yet another potential source of between-study heterogeneity in effect-size estimates. Brickman, Coates & Janoff-Bulman (1978) compared winners to controls from approximately the same area as the winners (recruited via phone books). Of the four remaining studies, only one (Kuhn et al. 2011) compares the outcomes of players from the same lottery, controlling for factors (e.g. lottery tickets) conditional on which the prizes in the lottery were randomly assigned.

5 Concluding Discussion

Our study leverages the randomized assignment of lottery prizes to generate estimates of the long-run effects of wealth on four facets of psychological well-being. Our estimates have strong internal validity and were obtained through pre-registered analyses. Overall, our study advances understanding of the broader question of why wealth and well-being often go hand in hand by providing credible and precise estimates of the long-run causal impacts of large changes in wealth in a sample of Swedish lottery players.

We find that lottery wealth causes sustained increases in *Overall LS*. Since we did not survey any players within five years of the lottery, our research design is not suitable for studying short-run adaptation, but our results do reject the strong hypothesis of complete adaptation. The effect shows no evidence of fading over the time horizon for which we have data and is robustly discernible over a decade after the lottery event. Our follow-up analyses suggest that the most important mechanism explaining the increase in *Overall LS* is increased satisfaction with personal finances. A sustained increase in *Financial LS* is not easy to reconcile with a common folk wisdom that lottery winners squander their wealth through wreckless spending. However, consistent with the previous qualitative evidence (Kaplan 1987, Eckblad & Lippe 1994, Hedenus 2011), we find little evidence of such behavior in our data (Cesarini et al. 2017). The long-run increases in *Overall LS* we document thus appear to reflect improvements in households' long-run financial circumstances.

The estimated effects on our well-being measures with a stronger affective component – *Happiness* and *Mental Health* – are smaller and not significantly different from zero. At high levels of income, some studies have reported that only evaluative measures of well-being increase with income (Kahneman & Deaton 2010). We find that at all levels of income, lottery wealth appears to impact affective and evaluative measures of well-being differently. This result further underscores the potential value of maintaining the conceptual distinctions between different facets of well-being.

We find that our annuity-rescaled treatment-effects on *Overall LS* and *Financial LS* are similar in magnitude to household-income gradients whereas the effects on *Happiness* and *Mental Health* are about one third as large as the estimated gradients for these outcomes. The rescaled estimates are at best reasonable approximations given the inherent uncertainty about the parameters used in the annuity-adjustment. But with this caveat in mind, the results suggest cross-sectional gradients overstate the causal effects of household income on affective but not evaluative measures of well-being. Another possibility is that different sources of income could have substantially different causal effects. To the extent that the key feature of lottery wealth that distinguishes it from household income is that it is unearned, our estimates may be most relevant for ongoing efforts to assess the likely costs and benefits of policy proposals that involve large, unconditional income transfers, such as basic income programs (Marinescu 2018).

We conclude by emphasizing three of our study's limitations that may inspire future research. A first is that in the spirited debate about the "Easterlin hypothesis" (e.g., Easterlin 1974, Easterlin 1995, Clark, Frijters & Shields 2008, Sacks, Stevenson & Wolfers 2012, Stevenson & Wolfers 2013) a key question is whether absolute or relative economic conditions are more important determinants of well-being. Since a lottery prize causes both relative and absolute wealth to increase, it is not clear that our results are relevant for resolving the controversy. Second, even though the demographic characteristics of individuals in our *Re-*

spondents Sample are overall similar to a representative sample of Swedish adults, lottery players may differ along unobserved dimensions in ways that limit the generalizability of our findings, especially in settings outside Sweden or very narrowly defined subsamples. Finally, previous research has found that financial distress (e.g., Berlin & Kaunitz 2015, Dobbie & Song 2015) and negative wealth shocks (e.g., McInerney, Mellor & Nicholas 2013) can have substantial adverse effects on well-being. Since all lottery prizes induce positive shocks to wealth, our data do not allow us to explore the intriguing possibility that the effects of negative and positive wealth shocks are asymmetric.

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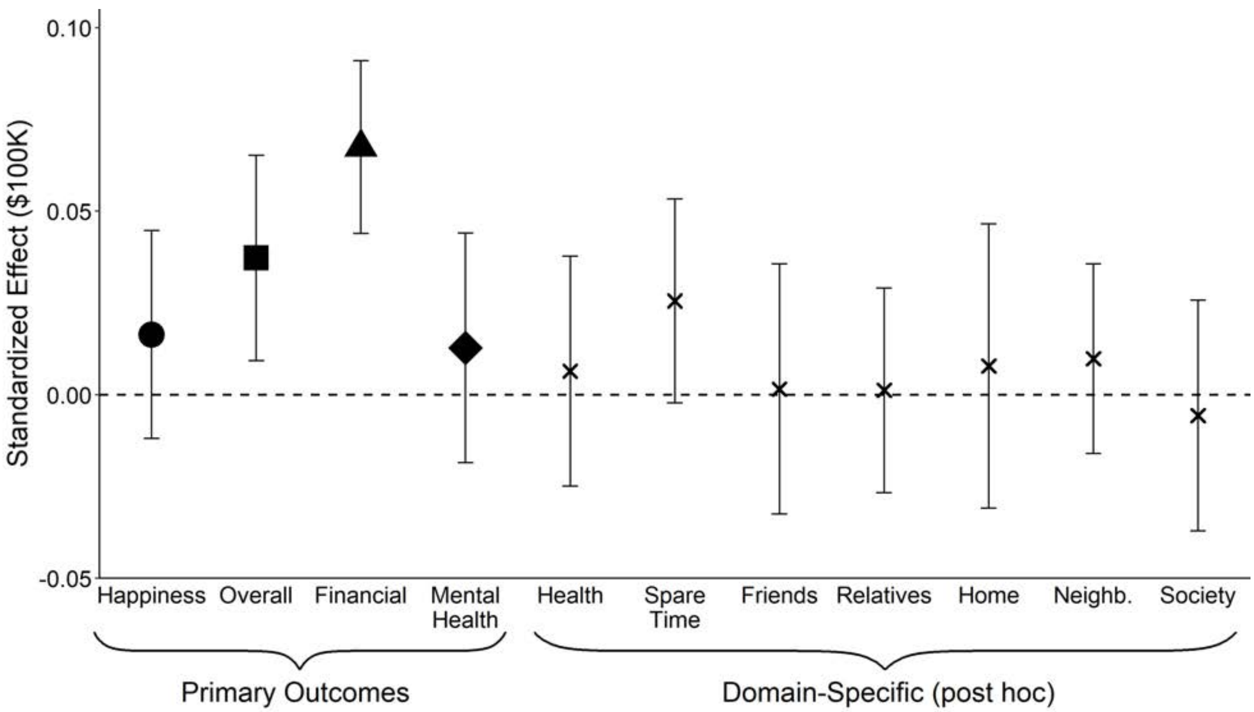

Figure 1: Causal Impact of Wealth on Primary Outcomes and Domain-Specific Measures of Life Satisfaction.

The figure shows estimated treatment effects of $100,000 USD (net of taxes) measured in SD units and coded such that higher values denote greater well-being. The lines show 95% CIs. The first four estimates are treatment-effect estimates from pre-registered analyses of primary outcomes. Family-wise-error corrected/nominal p-values 0.257/0.392 (*Happiness*), 0.009/0.025 (*Overall LS*), <0.001/<0.001 (*Financial LS*) and 0.423/0.397 (*Mental Health*). The seven estimates to the right are from post hoc analyses of domain-specific measures of life satisfaction. The figure omits one domain-specific outcome included on the survey – work – because one half of our respondents left this question blank (likely because they were retirees).

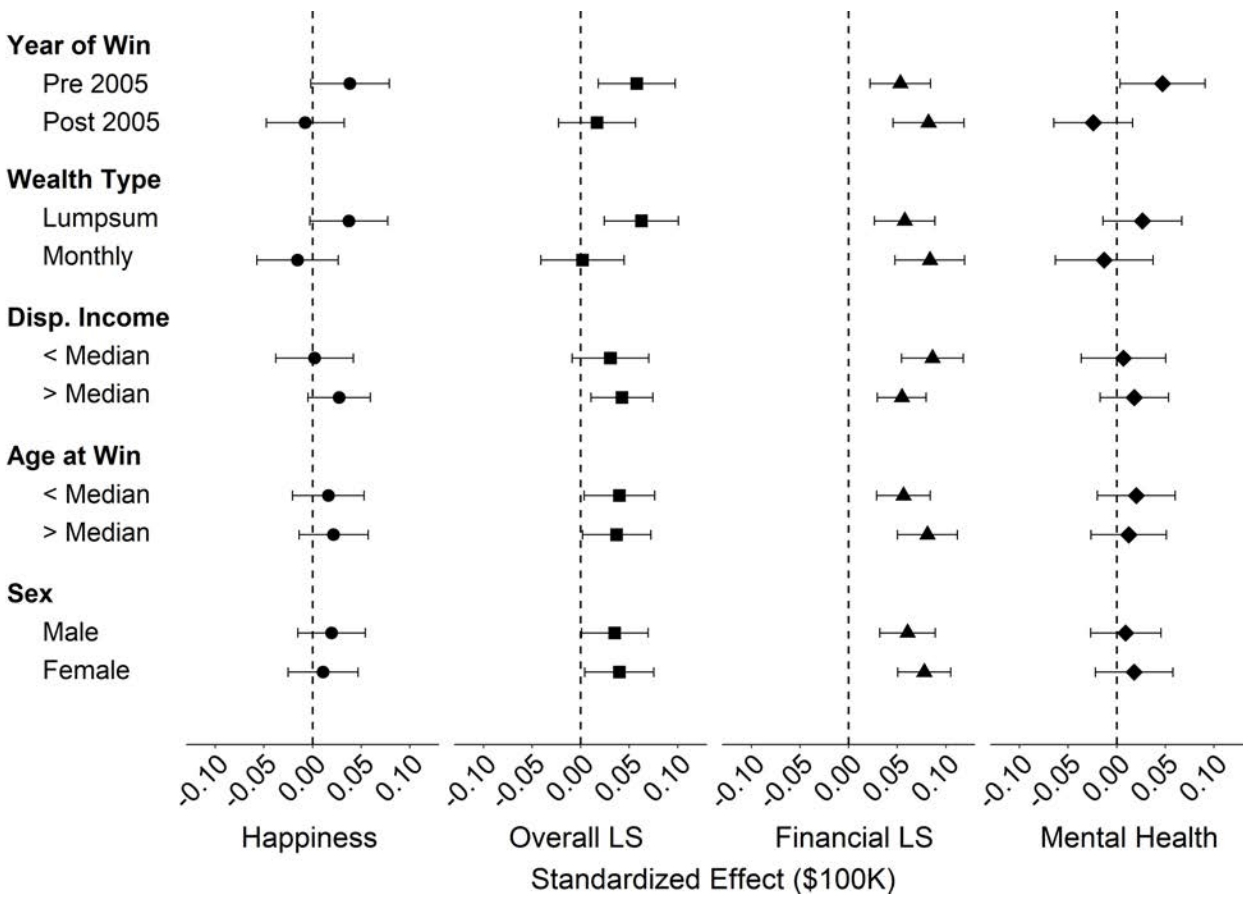

Figure 2: Treatment-Effect Heterogeneity

The figure shows estimated treatment effects of $100,000 USD (net of taxes) in subsamples defined in the Analysis Plan. For underlying data, see Table A7.

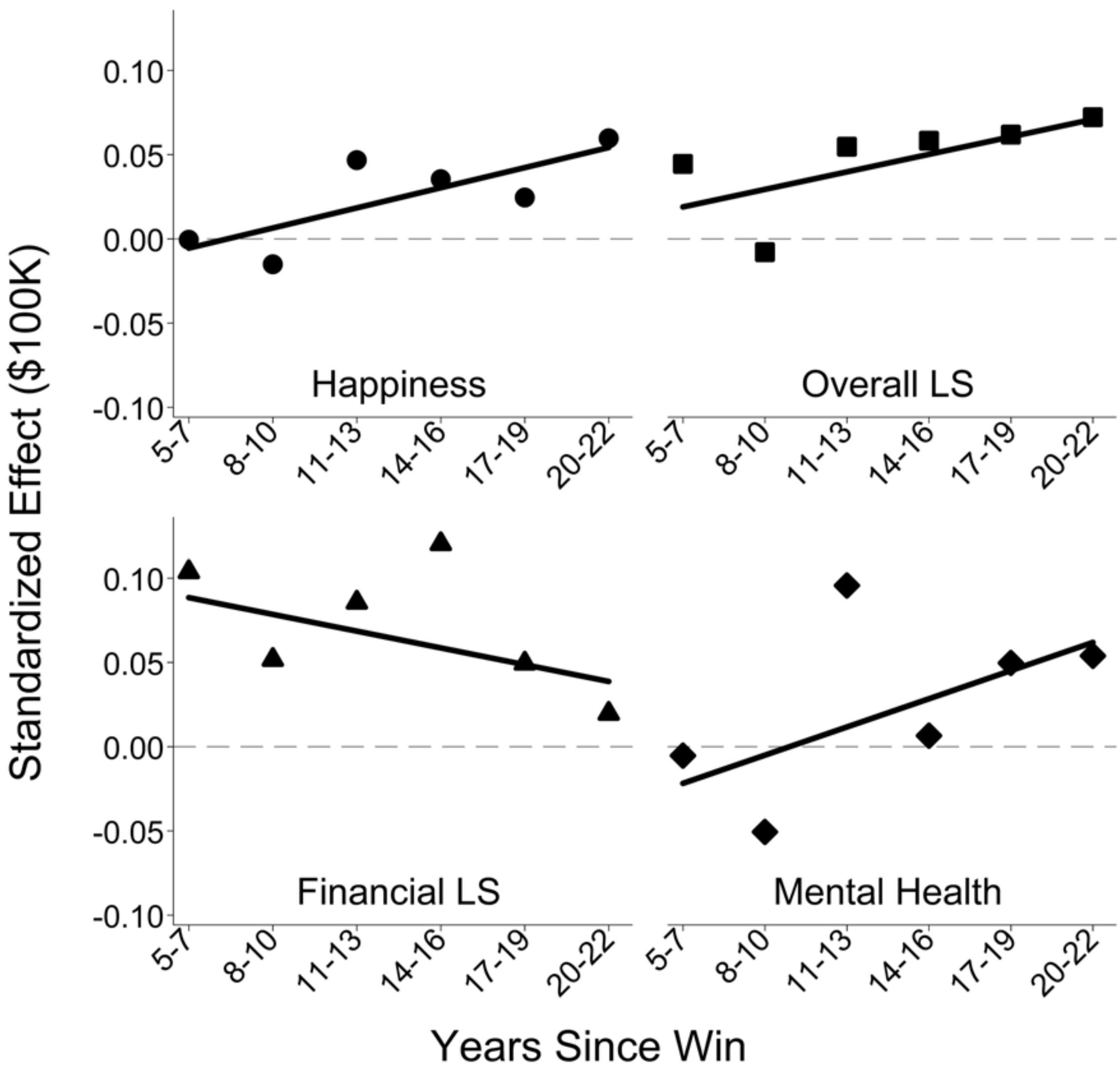

Figure 3: Treatment-Effect Heterogeneity by Years-Since-Win (Post Hoc)

This figure depicts estimates from post hoc analyses of treatment-effect heterogeneity by years-since-win. The line shown is from a regression of the treatment-effect estimate on average years-since-win in each group, weighting each point in proportion to the inverse of the variance of the estimate. The underlying data are in Table A8.

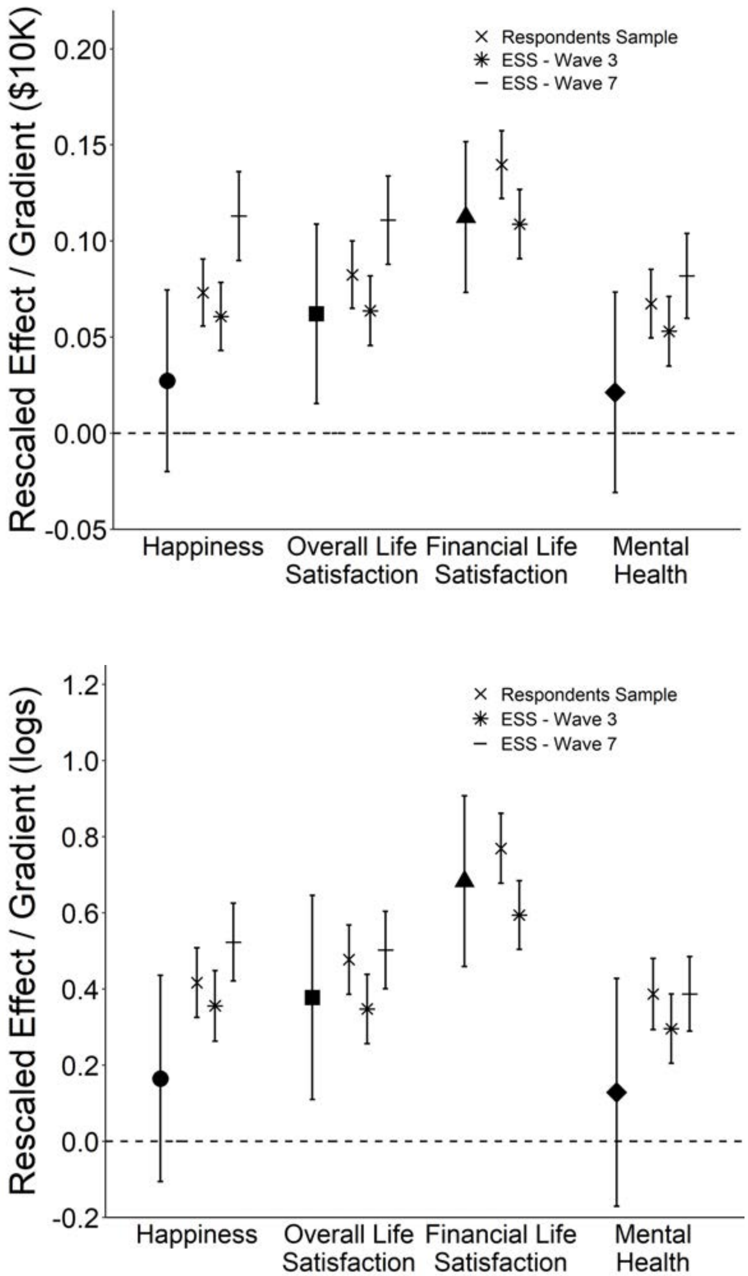

Figure 4: Comparing Annuity-Rescaled Treatment-Effect Estimates to Income Gradients

The figure shows annuity-rescaled causal estimates of the treatment effects and well-being log-income gradients estimated using similar methods in the *Respondents Sample* and two waves of the European Social Survey with comparable measures (ESS). In the *Respondent Sample*, gradients are estimated with large-prize winners (>$20K) omitted and household-permanent-income defined as the average of disposable, household income over the period 2004-2014. In the upper panel, income is measured in $10K. In the lower panel, we instead compare the causal estimates to log-income gradients. For additional details and underlying data, see Tables A9 and A10.

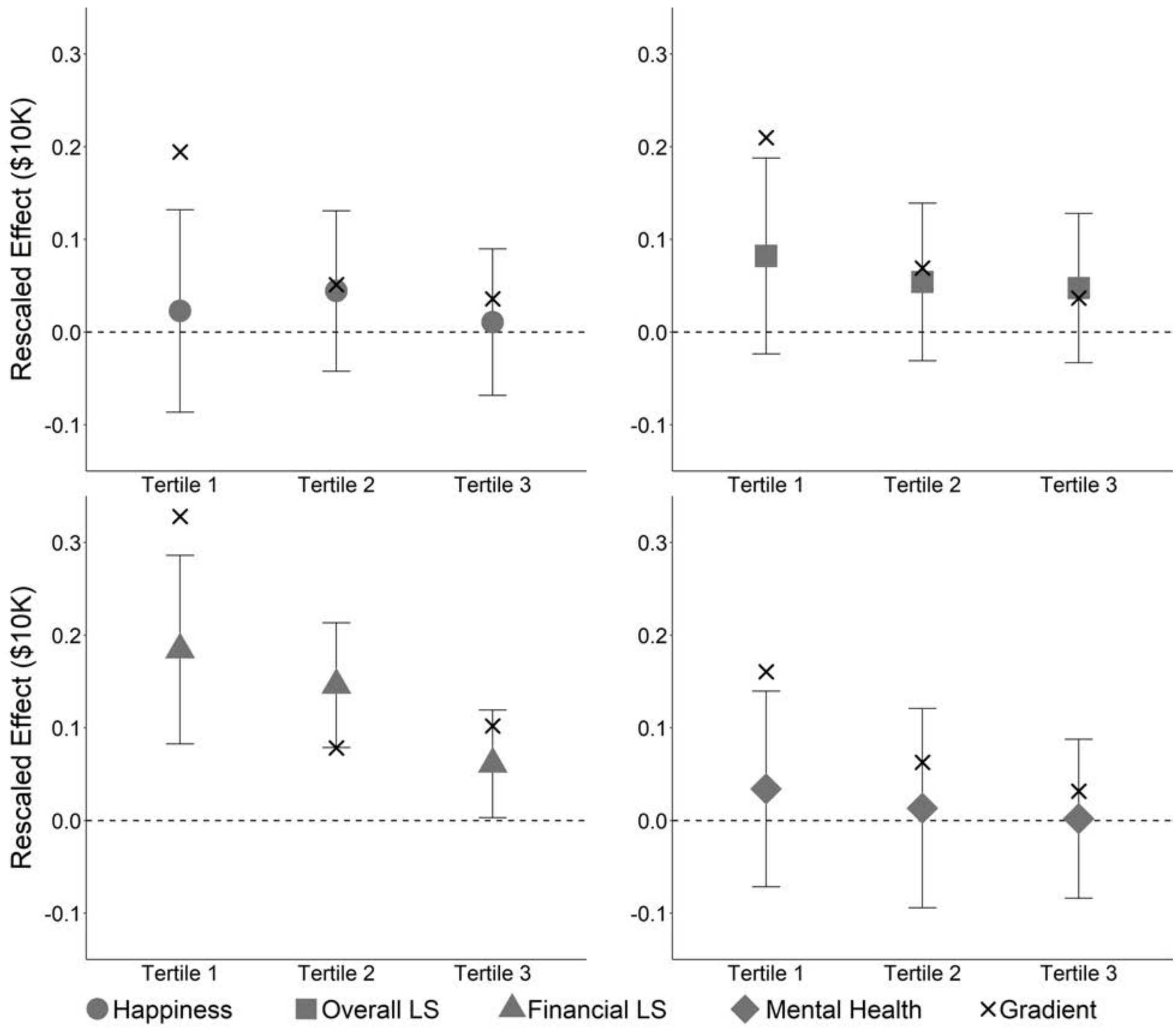

Figure 5: Comparisons of Rescaled Treatment Effects and Gradients by Permanent-income Tertile.

The figure shows the relationship between primary outcomes and household permanent income in the restricted *Respondents Sample* stratified by pre-lottery income tertile. The gradients reported are estimates from a single piecewise linear spline regression with two knots, one at each of the cutoff points that define the permanent-income tertiles.

Table 1: Distribution of Prizes Awarded.

	Survey Population				Respondents Sample			
			Triss...				Triss...	
	All	Kombi	Lumpsum	Monthly	All	Kombi	Lumpsum	Monthly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0	964	964	0	0	747	747	0	0
5K to 10K	811	0	811	0	554	0	554	0
10K to 50K	1,896	0	1,896	0	1,261	0	1,261	0
50K to 100K	211	0	211	0	138	0	138	0
100K to 200K	340	213	42	85	247	163	27	57
200K to 400K	322	21	43	258	216	14	34	168
400K to 600K	149	4	26	119	104	4	18	82
600K to 1M	135	2	36	97	87	0	23	64
>1M	12	1	0	11	8	1	0	7
Prize Sum ($M)	410.7	44.4	128.3	237.9	277.2	33.3	86.1	157.8
% of Survey Pop.					67%	75%	67%	66%
N	4,840	1,205	3,065	570	3,362	929	2,055	378
% of Survey Pop.					69%	77%	67%	66%

This table compares the distribution of prizes in the *Respondents Sample* and in the *Survey Population*. All prizes are after tax and measured in year-2011 USD. In Triss-Monthly, prize amount is defined as the net present value of the monthly installments won, assuming the annual discount rate is 2%.

Table 2: Representativeness of Survey Respondents.

	Respondents Sample					
	Kombi	Triss-Lumpsum	Triss-Monthly	Pooled	Survey Population	Representative Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Year of Birth	1951.1	1957.2	1957.5	1955.6	1957.3	1955.6
S.D.	8.0	11.5	11.6	11.0	11.7	11.0
Female	40.0%	52.1%	49.2%	48.4%	46.5%	48.4%
College	24.0%	26.1%	28.0%	25.8%	22.1%	30.1%
Swedish-born	95.2%	91.2%	91.5%	92.4%	90.7%	83.8%
Married	53.3%	53.8%	53.7%	53.7%	48.4%	51.0%
# Children	0.33	0.69	0.62	0.58	0.62	0.56
S.D.	0.73	1.00	0.94	0.94	0.97	0.95
Capital Income	-625	-978	-691.4	-848	-964	-26
S.D.	5,412	7,870	7,462	7,226	6,706	8,464
Labor Income	37,454	33,431	37,160	34,963	33,874	32,074
S.D.	22,598	21,748	22,277	22,123	21,893	24,671
N	929	2,055	378	3,362	4,840	373,276

This table reports descriptive statistics for the baseline controls in the *Respondents Sample*, both by lottery (columns 1-3), overall (4) and for the *Survey Population* (5). To help gauge representativeness, column 6 provides the same descriptive statistics for a representative sample draw in in 2010 after reweighting to match the sex and age distribution of the *Respondents Sample*. All time-varying variables are measured the year prior to the lottery event. The income variables are annual and measured in units of year-2011 $1,000.

Table 3: Happiness and Life Satisfaction (Primary Outcomes).

	Happiness	Overall Life Satisfaction	Financial Life Satisfaction	Mental Health
	(1)	(2)	(3)	(4)
Effect ($100K)	0.016	0.037	0.067	0.013
SE	(0.014)	(0.014)	(0.012)	(0.016)
p (analytical)	[0.257]	[0.009]	[<0.001]	[0.423]
p (resampling)	[0.257]	[0.011]	[<0.001]	[0.397]
FWER p	[0.392]	[0.025]	[<0.001]	[0.397]
N	3,327	3,331	3,216	3,147

This table reports the treatment effect of $100K (year-2011 prices) on the four primary outcomes measured in SD units. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual. The resampling-based p-values are obtained by simulating the distribution of coefficient estimates under the null hypothesis of zero treatment effects, as described in the main text. The family-wise error rate (FWER) is calculated using the free step-down resampling method of Westfall & Young (1993). Sample mean/SD in the *Respondents Sample* prior to standardization is: 7.14/1.77 (*Happiness*), 7.21/1.93 (*Overall Life Satisfaction*), 4.55/1.29 (*Financial Life Satisfaction*) and 38.1/5.18 (*Mental Health*).

Table 4: Comparison to Previous Lottery Studies.

	Outcome Variable			Lottery Prizes			Rescaled		Identification Strategy
	Dimension	t	N_{win}	Mean	Max	Sum	Effect	SE	
Current Study	Overall LS	5-22	2,589	106K	1,557K	275M	0.037	0.014	Within-lottery comparisons of players with identical pre-lottery odds of winning.
	Happiness	5-22	2,585	106K	1,557K	275M	0.016	0.014	
	Mental Health	5-22	2,439	105K	1,557K	257M	0.013	0.016	
Brickman et al. (1978)	Happiness	N/A	22	1,182K	2,464K	26M	0.014	0.025	Winners compared to non-lottery controls from similar areas (recruited via phone book).
Lindahl (2005)	Mental Health	0-12	626	5.6K	N/A	3.5M	0.280	0.124	Comparison of players (original lotteries unknown) who won different prize amounts (13-year period).
Gardner and Oswald (2007)	Mental Health	2	137	8.8K	245	1.2M	2.952	1.048	Comparison of players (original lotteries unknown) who won different prize amounts.
Apouey and Clark (2013)	Mental Health	1	674	5.8K	372	3.9M	0.297	0.455	Longitudinal analysis of within-person changes after win (original lotteries unknown).
	Mental Health	2	674	5.8K	372	3.9M	1.298	0.452	
	Overall LS	1	674	5.8K	372	3.9M	0.264	0.436	
	Overall LS	2	674	5.8K	372	3.9M	1.369	0.424	
Kuhn et al. (2011)	Happiness	0.5	223	22.5K	N/A	5.0M	-0.112	0.216	Within-lottery comparison with controls for quadratic in tickets.

N_{win} is number of lottery winners in sample (non-winners are excluded). Rescaled estimates are effects of \$100K (year-2011 prices) on the outcome measured in SD units. Lottery prizes have been converted to year-2011 prices. Further information about the studies and calculations underlying the rescaled effects are available in Section 3 in the Online Appendix.