Long-run Effects of Lottery Wealth on Psychological Well-being

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Abstract

We surveyed a large sample of Swedish lottery players about their psychological well-being 5-22 years after a major lottery event 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 over time. The estimated treatment effects on happiness and mental health are significantly smaller. Follow-up analyses of domain-specific aspects of life satisfaction implicate financial life satisfaction as an important mediator for the long-run increase in overall life satisfaction.

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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, Deaton 2008, Sacks, Stevenson & Wolfers 2012). 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). Moreover, 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 smaller than the short-term effect (Frey & Stutzer 2002, Clark, Frijters & Shields 2008). Considerable uncertainty therefore remains about the magnitude as well as the persistence of any income or wealth effects on subjective well-being.

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 make more accurate trade-offs between pecuniary and non-pecuniary aspects of life (e.g., Layard 2006). At the societal level, subjective well-being data are increasingly used in welfare analysis (e.g., Frey & Stutzer 2002, Fleurbaey 2009, Benjamin et al. 2014). For instance, estimates of wealth effects may prove valuable 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), and when deciding on pecuniary compensations for non-financial losses.

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), stock price fluctua-
tions (Schwandt 2018), or within-person changes over time (e.g., Frijters, Haisken-DeNew & Shields 2004, Frijters et al. 2006). Most studies 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 to 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 an 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 an important mechanism behind the increase in life satisfaction. In contrast, the estimated effects on our measures with a stronger affective component – happiness and an index of mental health – are smaller and

\(^1\)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).

\(^2\)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.
not statistically distinguishable from zero.

To help benchmark our results, we convert lottery prizes to annuity payouts and compare the resulting annuity-rescaled treatment effects 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 estimated in cross-sectional data. 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 that 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 Section 5 compares our results to income gradients and previous literature. Section 6 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 main 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/. The plan was posted before Statistics Sweden released any survey data 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 preregistered and post hoc analyses. Specifically, we pre-specified the criteria for inclusion in the Survey Population; three diagnostic tests for endogenous attrition; a set of primary outcomes; a set of baseline controls, variable coding (including handling of missing values and outliers); the estimating equation; heterogeneity and robustness analyses; and procedures for calculating \( p \)-values.

In formulating the plan, our goal was not only to reduce the number of investigator degrees of freedom in these analyses, but to eliminate them altogether. Unless explicitly noted otherwise, the results we report are based on analyses that were executed exactly according to the pre-registered procedures.

2.1 Survey Population

The Survey Population was drawn from a large administrative sample of lottery participants we have 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 elected to 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

\(^3\)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
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 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 in 2016. An additional consideration was that a substantial fraction of the PLS players are deceased. Like all other sample selection criteria, the decision to exclude PLS altogether was made ex ante.
correspond to 4,820 unique individuals.\textsuperscript{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 a 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.\textsuperscript{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.

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\%.\textsuperscript{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.

\textsuperscript{4}Individuals may appear in the data more than once for one of three reasons (i) they won multiple times (ii) they were selected as a Kombi control more than once (iii) sample overlap between the three lotteries.

\textsuperscript{5}Statistics Sweden required that information about the administrative registers, including the lottery data set, that were matched to survey responses should be provided to participants. 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.

\textsuperscript{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.
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 comparing non-winners and winners of small prizes with winners of prizes in the
range $100,000 – $800,000.\textsuperscript{7} Even though the Respondents Sample constitute less than a 1%
subsample of the pooled lottery sample analyzed in Cesarini et al. (2016), the oversampling
of large-prize winners allows us to retain approximately one third of the identifying variation
in lottery wealth.

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. Compared to the population, lottery players are older and somewhat more likely to
be male. Consequently, characteristics that vary between the sexes or over the life cycle will
differ between players and the Swedish adult population. To adjust for such compositional
differences, we reweight the representative sample to match the sex- and age distribution
of the Respondents Sample. Compared to the reweighted representative 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, re-
spectively). Overall, the similarity in baseline characteristics is reassuring, though we cannot
rule out that people who select into the lottery differ from the population in unobservables
in ways that could impair the generalizability of our findings.

\textsuperscript{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\%. 

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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 (Goldberg & 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 “Very dissatisfied” to “Very satisfied”, which we convert to integers from 1 to 6. Section 6 in the Online Appendix contains further information about the psychometric properties of our primary outcomes, and English translations of the survey questions from which they were derived.

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).

Following the Analysis Plan and the convention in much of the economics literature, we
treat all outcomes as measured on interval scales. This assumption is intuitively appealing
for Happiness and Overall LS which are based on numeric response scales, and it simplifies
quantitative comparisons across outcomes and with the previous literature. The interval-
scale assumption also allows us to use ordinary least squares (OLS) for estimation, which is
convenient due to the relatively large number of fixed effects required by our identification
strategy.

3 Analytic Framework

Here, we summarize the key features of the analytic framework described in the Analysis
Plan.

3.1 Estimation and Identification Strategy

We estimate the long-run causal impact of lottery wealth by OLS, using the following esti-
mating equation:

$$ y_{iis} = \alpha L_{i,0} + \mathbf{Z}_{i,-1} \gamma + \mathbf{X}_i \beta + \varepsilon_i, $$

(1)
where the time of the lottery event is normalized to $t = 0$. The dependent variable, $y_{i,s}$, is a measure of well-being standardized to unit variance for respondent $i$ measured $s$ years after the lottery event. Because the lottery events in our sample took place between 1994 and 2011, the value of $s$ varies between 5 and 22 for an outcome measured in 2016 (the year of the survey). $L_{i,0}$ is the prize (in $100,000) awarded to individual $i$ at $t = 0$ and $Z_{i,-1}$ is a vector of baseline characteristics measured in the year prior to the lottery event.

Our identification strategy exploits the fact that the lottery prizes in our samples are randomly assigned conditional on player characteristics we observe, $X_i$. In particular, $X_i$ is a set of indicator variables for groups of lottery players within which we know that prize money is randomly assigned under the lottery rules. The procedures used to define the groups differ across our three lotteries. In Kombi, we assign each large-prize winner a group identifier that is only shared with their matched controls. For purposes of illustration, consider a hypothetical Kombi player who is female, born in 1957 and had two tickets in the March 2003 lottery, winning a 1M SEK prize. Our procedure assigns this hypothetical winner to four controls randomly sampled from the population of female players born in 1957 who did not win a large prize in the March 2003 draw, but owned the same number of tickets as the winner.

In the two Triss lotteries, we do not have information about nonwinning players, so we instead rely on comparisons of players who won prizes of different magnitudes. In Triss-Lumpsum, two players share a group identifier if and only if they won exactly one lumpsum prize in the same year and under the same prize plan. Conditioning on the prize plan ensures that causal effects are identified exclusively off comparisons of players who happened to draw different prizes from the same underlying distribution. In Triss-Monthly, group identifiers are constructed using an analogous procedure. Controlling for $X_i$ in the final analyses thus ensures that all of our analyses appropriately account for potential conditional assignment of lottery prizes in our data. Section III of the Analysis Plan (Östling, Lindqvist & Cesarini 2016) contains additional information about the methodology used to define the
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 2018). 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). We refer to the resulting \( p \)-values as FWER-adjusted \( p \)-values.

We use an estimating equation in which \( y_{it} \) depends linearly on \( L_{i0} \) even though it is plausible that the true relationship is nonlinear. The previous literature finds that well-being increases approximately linearly in the logarithm of income. Since few of our players win prizes that are very large relative to their lifetime income, our linear specification may nevertheless offer a decent approximation to a log-linear relationship. To illustrate why, suppose well-being is linear in the logarithm of lifetime income and consider an individual who wins a prize of \$400,000 and whose remaining lifetime household income is \$1.37 million, approximately the median value in our sample. For this individual, the marginal impact of the first dollar won is \( 1/(1.37 \cdot 10^6) \approx 0.87 \cdot 10^{-6} \), whereas the marginal impact of the last dollar won is \( 1/((1.37 + 0.4) \cdot 10^6) \approx 0.56 \cdot 10^{-6} \). By contrast, a log-transformation of the prize variable is appropriate if the effect of a proportional increase in the lottery prize on

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8The median annual household disposable income in year \( t = -1 \) was \$47,000 in our sample. The lifetime income we use in our heuristic 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.
well-being is roughly constant across the prize distribution. Such proportionality stretches credibility for prizes in the range we consider in our analyses. For example, it requires that the marginal effect of a lottery dollar awarded to an individual who won a prize of $1,000 be 400 times larger than its effect on a $400,000-prize winner. To test for nonlinear effects, we instead report the results from a robustness test that omits large-prize winners.

### 3.2 Survey Non-Response and Tests of Endogenous Attrition

A potential concern about our identification strategy is that lottery wealth could directly influence the propensity to answer the survey. Such endogenous attrition could introduce endogeneity in the Respondents Sample, even if our identifying assumption holds in the Survey Population. To test for selection biases, we conducted three pre-registered diagnostic tests. 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 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 statistically significant after our multiple-hypothesis adjustment. For Happiness and Mental Health, the corresponding point estimates are 0.016 and 0.013, respectively.\footnote{Although statistically insignificant, our estimate of the effect on Mental Health (0.013) is quite similar to the appropriately rescaled reduction in consumption of prescribed mental health drugs of 0.023 SD units (SE = 0.007, $p = 0.002$) in our previous work on lottery winners’ health (Cesarini et al. 2016).} 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. In post hoc analyses, we find no evidence that the insignificant effect on Mental Health is due to counteracting effects on the survey items used to construct the index. For example, the estimated treatment effect on a standardized index restricted to positively phrased items is 0.014 (SE = 0.017), compared to 0.011 (SE = 0.016) for an index based on negatively phrased items (reverse coded, to ensure larger values of the index denote greater well-being).

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). Dropping the largest prizes leads to larger standard errors and coefficient estimates which are quite similar to the baseline results, although the treatment effect difference between Overall LS and Happiness is smaller.

We also conducted several post hoc analyses designed to examine if our results are sensitive to alternative cardinalizations. In the first, we reran the main analyses of Happiness, Overall LS and Financial LS using the “blow-up and cluster” conditional logit estimator proposed by Mukherjee et al. (2008) instead of OLS. 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). However, as shown by Schröder & Yitzhaki (2017) and Bond & Lang (2018), relaxing the distributional assumptions in standard ordered logit and ordered probit regression models can reverse the estimated treatment effect. In a second
analysis, suggested by Bloem (2019), we therefore examined if the original estimates are robust to a wide range of smooth convex and concave monotonic transformations of the outcome (see Figure A2). Following Bond & Lang (2018), we also estimated an ordered probit model which allows for heteroskedasticity and compute what log-normal transformation of the latent variable is needed to shift the estimated treatment effect on Overall LS to zero. We cannot reject homoscedasticity ($p = 0.111$) and find that a cardinalization that attaches extreme weight to observations at the bottom of the distribution of Overall LS is needed to shift the effect to zero: the implied difference between the 0.1th and 1st percentile is more than 100 times larger than the difference between the 10th and 99.9th percentiles.\footnote{It is not feasible to estimate this ordered probit model with our full set of indicator variables for lottery groups, so we only include dummies for lottery-by-year along with the demographic control variables.} In our final analysis, we only make use of the ordinal information in the survey responses. For each outcome, we defined and analyzed a series of indicator variables, one for each of the $K$ response categories. The indicator variable for response category $k$ is 1 if the respondent chose a response category indicating a level of well-being at least as great as the category. We ran one regression for each possible category. Figure A3 shows that the estimated effects on Overall LS and Financial LS are close to zero at the lower end of the well-being distribution, but positive and statistically significant at the higher end where the bulk of the response distribution is located.

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 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. Overall, these post hoc analyses suggest that Financial LS mediates much of the observed long-run effect of lottery wealth on Overall LS. For example, 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%.

A long-term impact on 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 spend down their windfalls, but lottery wealth dissipates slowly and is robustly detectable for well over a decade after the win (Cesarini et al. 2016). Winners also reduce their labor supply, yet 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). Winners also invest a substantial share of the wealth in financial assets, often opting for low-risk bond products over equities (Briggs et al. 2015).11

Several findings in our prior studies are also potentially relevant for evaluating hypotheses about other mechanisms. For example, the domain-specific analyses in Figure 1 provide some evidence that winners are more satisfied with their spare time. Even though the confidence interval is just wide enough to include zero, the prior evidence that winners modestly reduce their labor supply over a very long horizon makes us reluctant to dismiss this as a chance finding (Cesarini et al. 2017). Instead, we interpret this result as suggestive evidence that more, or higher-quality, leisure time contribute to the rise in overall life satisfaction. On the other hand, the administrative studies provide little evidence that lottery wealth impacts health, child outcomes, and occupational choice, making it less likely that any of these potential channels is quantitatively important.

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),

11Our evidence regarding winner behavior is well in line with conclusions from interview-based research on lottery winners in multiple countries (Kaplan 1987, Furäker & 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).
years-since-win (before or after 2005) and type of prize (Triss-Monthly vs Triss-Lumpsum). The results are shown in Figure 2 (see Table A7 for underlying data). Overall, the estimated 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 problems smooth consumption. Our subsample analyses only yield clear evidence of positive treatment effects among players who won lumpsum prizes.

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

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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.

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 the same net present values have identical effects.
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
adaptation to the windfall is incomplete well over a decade after the lottery event.

5 Benchmarking

To provide some additional context for our findings, we next compare our results to household-
income gradients and to previous quasi-experimental estimates of lottery players’ well-being.
The Analysis Plan stated such a comparative analysis would be reported but did not attempt
to fully specify the procedures by which we would arrive at the final quantitative benchmarks.
Following the suggestion of a referee, we also include a comparison to previously reported
estimates of several major life events. Additional methodological details about all of these
analyses are provided in the Online Appendix.

5.1 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 cor-
relations 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 sug-
gests 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 calculate, for each lottery prize, 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 ($8,483) 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 (Deaton 2008, Stevenson & Wolfers 2013). Figure A4 shows that in our sample, the cross-sectional relationship between permanent annual income and each of our primary outcomes is positive and concave. We also compare our rescaled treatment effects to gradients for Swedish respondents in two waves of the European Social Survey (see Section 2 in the Online Appendix for details).

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.\footnote{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.)} The lower panel of Figure 4 shows the
log-income gradients fall within the normal range reported in previous literature (Stevenson & Wolfers 2008, Stevenson & Wolfers 2013). For example, our life-satisfaction gradients range from 0.35 to 0.50, whereas Stevenson & Wolfers (2008) reports an average gradient for 113 countries of 0.38. As in the linear case, the causal effect of log income on Overall LS implied by our estimate (0.38) is similar to the log income gradient, while the implied effect for Happiness is substantially lower (0.17).

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, all treatment-effect estimates are smaller than the income gradients, as shown in Figure 5 (p-values ranging from 0.012 to 0.064 for tests of equality between the estimated gradients and rescaled effect estimates). At medium and high incomes, the gradients are similar in magnitude to the causal estimates.

5.2 Previous Lottery Studies

We identified five previous quasi-experimental studies of lottery players’ well-being. Table 4 provides a summary overview of how our study compares to these along some key dimensions: outcome variables, lottery data, effect sizes 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 two important interpretational caveats that apply when making cross-study comparisons based on data in the table. First, only one previous study (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. This rescaled estimate is 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 had 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 final row shows information from a study of Dutch Postcode Lottery winners (Kuhn et al. 2011) which reported nonsignificant results from an analysis of how lottery wealth impacts happiness.

Notably, all four studies that appeared after Brickman, Coates & Janoff-Bulman (1978) had rescaled estimates with standard errors at least 7 times larger than ours. Figure 6 provides a graphical illustration for one of our primary outcomes, Mental Health. All three previous studies of lottery players’ mental health claimed a positive finding and are routinely cited as having demonstrated one. Unfortunately, there are reasons to believe the studies were underpowered, perhaps dramatically so, and consequently that the value of their results for helping us determine if, and by how much, wealth impacts well-being is limited. This is true for any underpowered study, irrespective of whether or not the findings
reached statistical significance, but statistically significant findings are especially prone to misinterpretation.

To illustrate the problem, consider first the study by Lindahl (2005) which, like the present study, analyzed the long-run impact of lottery wealth on mental health in a Swedish sample. Based on our findings, the true long-run effect of $100K on mental health is unlikely to be larger than 0.044 (the upper limit of our 95% confidence interval). To put this effect size in perspective, note that it would imply a causal effect 25-30% larger than the household-income gradient based on our back-of-the-envelope calculations in Section 5.1. Assuming a true effect of 0.044, Lindahl’s statistical power to detect a significant effect was 6.5% (at $\alpha = 0.05$). An implication of large standard errors is that estimated effects must be far away from zero to be statistically significant. Consequently, conditional on finding a statistically significant effect, a study with power as low as Lindahl’s will incorrectly sign the effect (“type S error”) 16% of the time, and overestimate (the absolute value of) the effect size (“type M error”) by an average factor of 6.7 (Gelman & Carlin 2014). Since Gardner & Oswald (2007) and Apouey & Clark (2015) estimates had standard errors much larger than Lindahl’s, analogous calculations based on the same effect-size assumption yield even more dramatic type S and M error rates for these studies.

Of course, it may be inappropriate to use our estimates or household-income gradients 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. However, the assumptions about short-run effect sizes needed for previous lottery studies to have been well-powered are hard to justify. To see why, consider the study by Kuhn et al. (2011), which reported a nonsignificant result for happiness measured six months after a lottery event. Assuming a true short-run effect equal to the upper limit of their 95% confidence interval, 0.31, the statistical power of the studies by Gardner & Oswald (2007) and Apouey & Clark (2015) were still only 6% and 11%, respectively. Thus, the studies were underpowered even under assumptions about short-run effects we consider generous: an effect size of 0.31 is more than
twenty times greater than our estimated long-run effect.

A second difference between our and previous lottery papers is that previous studies have to a greater extent relied on identifying variation generated by small and modestly sized prizes. If lottery wealth has diminishing marginal effects, it could help explain the larger estimates of previous studies. When we drop the largest prizes from our data, the estimated treatment effects increase for two out of four outcomes, consistent with diminishing marginal returns (Table A5). But even for these two outcomes, the implied degree of non-linearity is not nearly large enough for this factor alone to contribute in a quantitatively meaningful way to the stark differences in effect sizes. A third possibility is that the effect of wealth varies greatly across countries. Although we cannot rule out this explanation, it does not square easily with the pattern of country-specific cross-sectional gradients reported by Stevenson & Wolfers (2008). Whereas Gardner & Oswald (2007) and Apouey & Clark (2015) estimate large positive effects of lottery wealth on wellbeing in the UK, the gradient is in fact flatter for the UK than for Sweden. And while Kuhn et al. (2011) report a negative point estimate in their study of Dutch lottery winners’ happiness, the Netherlands has one of the steepest gradients between income well-being among all countries in the world.

The final column of Table 4 summarizes each study’s identification strategy, yet another potential source of between-study heterogeneity in effect-size estimates. The study by Brickman, Coates & Janoff-Bulman (1978) compared winners to controls recruited from the general population via phone books in approximately the same areas as the winners. 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. Moreover, none of the previous studies reported results from pre-registered analyses. In summary, our study compares favorably to earlier work along several methodological dimensions, including power, pre-registration of analyses, and the credibility of the identification strategy.
5.3 Life Events

Our final benchmark are some previously reported estimates of how life changes and commuting time impact well-being (see Section 4 and 5 in the Online Appendix for additional methodological details).

In a first analysis, we compared our results to the estimated effects of five major life events on well-being reported in a study of British longitudinal data (Clark & Georgellis 2012). The study relied on a common event-study methodology and considered two measures of well-being: life satisfaction and mental health. For expositional ease, we limit the comparison to estimated effects on “short-run” well-being (measured within a year of the event) and “long-run” well-being (measured 5-17 years after the event). Figure 7 summarizes the results. For both mental health (upper panel) and life satisfaction (lower panel), the estimated short-run impact of each major life event — unemployment, marriage, divorce, widowhood and birth of a child — is large compared to the long-run effect of a $100K windfall (and the difference is usually statistically significant). The long-run event-study estimates are quite imprecisely estimated but with the exception of unemployment, their magnitudes appear to be more aligned with the relevant lottery estimate.

In a second analysis, we used results in Stutzer & Frey (2008) to compare our estimates to the magnitude of the negative association between commuting time and subjective well-being. Stutzer & Frey (2008) report several analyses of longitudinal data from the Germany Socioeconomic Panel. Overall, their estimates suggest that the long-run effects of $100K are comparable in magnitude to a reduction in daily commute time of 25-40 minutes (12.5-20 minutes for each one-way commute).

6 Concluding Discussion

Our study leverages the randomized assignment of lottery prizes to estimate 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 an affective component – Happiness and Mental Health – are also positive but smaller in magnitude and not significantly different from zero. These smaller effects are interesting in light of prior work which often finds that cross-sectional relationships with income are not the same for affective and evaluative measures (e.g., Kahneman & Deaton 2010, Jebb et al. 2018). For example, Kahneman & Deaton (2010) find that beyond a point of satiation, evaluative well-being rises with income whereas affective well-being does not. Overall, our results suggest that wealth impacts people’s satisfaction with their lives more than their affective well-being. Since our Happiness measure has an evaluative component and is strongly correlated with Overall LS in our sample and elsewhere (e.g., Stevenson & Wolfers 2013), the magnitude of the effect-size divergence is nevertheless surprisingly large. Overall, our results underscore 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 caveat is that different sources of income may have 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. A first is that in the spirited debate about the “Easterlin hypothesis” (e.g., Easterlin 1974, Easterlin 1995, Clark, Frijters & Shields 2008, Stevenson & Wolfers 2008, Sacks, Stevenson & Wolfers 2012) a key question is whether absolute or relative economic conditions are more important determinants of well-being (see also Luttmer 2005). 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 Respondents 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.
References


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; for underlying data, see Table 3. 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; for underlying data, see Table A6. 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 define 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 Respondents 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. For underlying data, see Table A10.
Figure 6: Comparisons of Rescaled Treatment Effects for Mental Health

This figure compares our estimated treatment effect on Mental Health to those in previous quasi-experimental studies of lottery players’ mental health. See Online Appendix for details on these studies and how the originally reported estimates were rescaled for comparability. Error bars denote 95% confidence intervals.
Figure 7: Comparison to Previously Reported Estimates of Life Events

This figure compares our results to published estimates (Clark & Georgellis 2012) of the effects of five major life events on “short-run” well-being (defined as a measure obtained within a year of the event) and “long-run” well-being (5-17 years after the event). In the upper panel, well-being is Mental Health and in the lower panel, it is Overall LS. See Online Appendix for details on the event-study estimates.
### Table 1: Distribution of Prizes Awarded

<table>
<thead>
<tr>
<th>Prize Sum ($M)</th>
<th>Triss...</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Survey Population</td>
<td>Respondents Sample</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>All</td>
<td>Kombi</td>
<td>Lumpsum</td>
<td>Monthly</td>
</tr>
<tr>
<td>0</td>
<td>964</td>
<td>964</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>5K to 10K</td>
<td>811</td>
<td>0</td>
<td>811</td>
<td>0</td>
</tr>
<tr>
<td>10K to 50K</td>
<td>1,896</td>
<td>0</td>
<td>1,896</td>
<td>0</td>
</tr>
<tr>
<td>50K to 100K</td>
<td>211</td>
<td>0</td>
<td>211</td>
<td>0</td>
</tr>
<tr>
<td>100K to 200K</td>
<td>340</td>
<td>213</td>
<td>42</td>
<td>85</td>
</tr>
<tr>
<td>200K to 400K</td>
<td>322</td>
<td>21</td>
<td>43</td>
<td>258</td>
</tr>
<tr>
<td>400K to 600K</td>
<td>149</td>
<td>4</td>
<td>26</td>
<td>119</td>
</tr>
<tr>
<td>600K to 1M</td>
<td>135</td>
<td>2</td>
<td>36</td>
<td>97</td>
</tr>
<tr>
<td>&gt;1M</td>
<td>12</td>
<td>1</td>
<td>0</td>
<td>11</td>
</tr>
<tr>
<td>% of Survey Pop</td>
<td>410.7</td>
<td>44.4</td>
<td>128.3</td>
<td>237.9</td>
</tr>
<tr>
<td>N</td>
<td>4,840</td>
<td>1,205</td>
<td>3,065</td>
<td>570</td>
</tr>
<tr>
<td>% of Survey Pop</td>
<td>67%</td>
<td>75%</td>
<td>67%</td>
<td>66%</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Respondents Sample</th>
<th>Survey Population</th>
<th>Representative Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Kombi Lumpsum</td>
<td>Triss-Monthly</td>
<td>Pooled</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Year of Birth</td>
<td>1951.1</td>
<td>1957.2</td>
<td>1957.5</td>
</tr>
<tr>
<td>S.D.</td>
<td>8.0</td>
<td>11.5</td>
<td>11.6</td>
</tr>
<tr>
<td>Female</td>
<td>40.0%</td>
<td>52.1%</td>
<td>49.2%</td>
</tr>
<tr>
<td>College</td>
<td>24.0%</td>
<td>26.1%</td>
<td>28.0%</td>
</tr>
<tr>
<td>Swedish-born</td>
<td>95.2%</td>
<td>91.2%</td>
<td>91.5%</td>
</tr>
<tr>
<td>Married</td>
<td>53.3%</td>
<td>53.8%</td>
<td>53.7%</td>
</tr>
<tr>
<td># Children</td>
<td>0.33</td>
<td>0.69</td>
<td>0.62</td>
</tr>
<tr>
<td>S.D.</td>
<td>0.73</td>
<td>1.00</td>
<td>0.94</td>
</tr>
<tr>
<td>Capital Income</td>
<td>-625</td>
<td>-978</td>
<td>-691.4</td>
</tr>
<tr>
<td>S.D.</td>
<td>5,412</td>
<td>7,870</td>
<td>7,462</td>
</tr>
<tr>
<td>Labor Income</td>
<td>37,454</td>
<td>33,431</td>
<td>37,160</td>
</tr>
<tr>
<td>S.D.</td>
<td>22,598</td>
<td>21,748</td>
<td>22,277</td>
</tr>
<tr>
<td>N</td>
<td>929</td>
<td>2,055</td>
<td>378</td>
</tr>
</tbody>
</table>

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 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)

<table>
<thead>
<tr>
<th></th>
<th>Happiness</th>
<th>Overall Life Satisfaction</th>
<th>Financial Life Satisfaction</th>
<th>Mental Health</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Effect (100K)</strong></td>
<td>0.016</td>
<td>0.037</td>
<td>0.067</td>
<td>0.013</td>
</tr>
<tr>
<td><strong>SE</strong></td>
<td>(0.014)</td>
<td>(0.014)</td>
<td>(0.012)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>** p (analytical)**</td>
<td>[0.257]</td>
<td>[0.009]</td>
<td>[&lt;0.001]</td>
<td>[0.423]</td>
</tr>
<tr>
<td>** p (resampling)**</td>
<td>[0.263]</td>
<td>[0.011]</td>
<td>[&lt;0.001]</td>
<td>[0.396]</td>
</tr>
<tr>
<td><strong>FWER p</strong></td>
<td>[0.396]</td>
<td>[0.026]</td>
<td>[&lt;0.001]</td>
<td>[0.396]</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>3,327</td>
<td>3,331</td>
<td>3,216</td>
<td>3,147</td>
</tr>
</tbody>
</table>

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**

<table>
<thead>
<tr>
<th>Outcome Variable</th>
<th>Lottery Prizes</th>
<th>Rescaled Effect</th>
<th>Identification Strategy</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Dimension</td>
<td>$N_{win}$</td>
<td>Mean</td>
</tr>
<tr>
<td>Current Study</td>
<td>Overall LS</td>
<td>5-22</td>
<td>2,589</td>
</tr>
<tr>
<td></td>
<td>Happiness</td>
<td>5-22</td>
<td>2,585</td>
</tr>
<tr>
<td></td>
<td>Mental Health</td>
<td>5-22</td>
<td>2,439</td>
</tr>
<tr>
<td>Brickman et al. (1978)</td>
<td>Happiness</td>
<td>N/A</td>
<td>22</td>
</tr>
<tr>
<td>Lindahl (2005)</td>
<td>Mental Health</td>
<td>0-12</td>
<td>626</td>
</tr>
<tr>
<td>Gardner and Oswald (2007)</td>
<td>Mental Health</td>
<td>2</td>
<td>137</td>
</tr>
<tr>
<td>Apouey and Clark (2013)</td>
<td>Mental Health</td>
<td>1</td>
<td>674</td>
</tr>
<tr>
<td></td>
<td>Mental Health</td>
<td>2</td>
<td>674</td>
</tr>
<tr>
<td></td>
<td>Overall LS</td>
<td>1</td>
<td>674</td>
</tr>
<tr>
<td></td>
<td>Overall LS</td>
<td>2</td>
<td>674</td>
</tr>
<tr>
<td>Kuhn et al. (2011)</td>
<td>Happiness</td>
<td>0.5</td>
<td>223</td>
</tr>
</tbody>
</table>

$N_{win}$ 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 4 in the Online Appendix.