

# Long-Run Effects of Lottery Wealth on Psychological Well-Being

ERIK LINDQVIST

*Swedish Institute for Social Research (SOFI), Stockholm University, and Research Institute of Industrial Economics (IFN)*

ROBERT ÖSTLING

*Stockholm School of Economics and IFN*

and

DAVID CESARINI

*New York University, NBER, and IFN*

*First version received November 2018; Editorial decision July 2019; Accepted November 2019 (Eds.)*

We surveyed a large sample of Swedish lottery players about their psychological well-being 5–22 years after a major lottery event and analysed 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.

*Key words:* Happiness, well-being, mental health, lottery.

*JEL Codes:* I31, I39, C31

## 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 and Biswas-Diener, 2002; Biswas-Diener, 2008; Deaton, 2008; Sacks *et al.*, 2012). However, the extent to which these associations arise due to causal pathways from wealth to well-being remains poorly understood (*e.g.* Frey and Stutzer, 2002; Clark *et al.*, 2008; Dolan *et al.*, 2008). Moreover, a large literature on hedonic adaptation argues that people adjust their aspirations upwards when their economic conditions improve (*e.g.* Brickman and Campbell, 1971; Frederick and Lowenstein, 1999), implying the long-term effect of positive economic shocks may be smaller than the short-term effect (Frey and Stutzer, 2002; Clark *et al.*, 2008).

---

*The editor in charge of this paper was Uta Schoenberg.*

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 tradeoffs 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 and Stutzer, 2002](#); [Fleurbay, 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 and 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 et al., 1978](#); [Lindahl, 2005](#); [Gardner and Oswald, 2007](#); [Kuhn et al., 2011](#); [Apouey and Clark, 2015](#)), tax rebates ([Lachowska, 2017](#)), stock price fluctuations ([Schwandt, 2018](#)), or within-person changes over time (e.g. [Frijters et al., 2004, 2006](#)). Most studies conclude effects are positive, but the effect sizes vary substantially in magnitude.<sup>1</sup> Moreover, with the exception of [Lindahl \(2005\)](#), these studies do not consider long-term effects.

In this article, 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 5–22 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 et al., 2016](#)).

We find that the long-run effects of wealth vary depending on the exact dimension of well-being.<sup>2</sup> 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 analyse. 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 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

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 and Shapiro, 2016](#)) and Malawi ([Baird et al., 2013](#)), though not in Ecuador ([Paxson and 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.

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 favourably both in terms of statistical power and the credibility of our causal inference.

Our article 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 [Supplementary 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 article follow the procedures specified in an Analysis Plan (Östling *et al.*, 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 pre-registered 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 also 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), labour supply (Cesarini *et al.*, 2017), and participation in financial markets (Briggs *et al.*, 2020). 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.<sup>3</sup> 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,

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 (1994–2011) win a lump-sum prize between \$7,000 and \$700,000. Winners of the Triss-Monthly prize (1997–2011) win a monthly income supplement. The size (\$1,400–\$7,000) and duration (10–50 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%.

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 [Supplementary 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 ( $241 \times 4$ ) 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.<sup>4</sup>

## 2.2. Survey protocol

In early fall of 2016, Statistics Sweden mailed a letter of invitation to all members of the *Survey Population* (see [Supplementary 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.<sup>5</sup> 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.

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. Like all other sample selection criteria, the decision to exclude PLS altogether was made *ex ante*.

4. Individuals may appear in the data more than once for one of three reasons: (1) they won multiple times, (2) they were selected as a Kombi control more than once, and (3) sample overlap between the three lotteries.

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.

TABLE 1  
*Distribution of prizes awarded*

	Survey population				Respondents sample			
	All	Kombi	Triss...		All	Kombi	Triss...	
			Lumpsum	Monthly			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%
<i>N</i>	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 instalments won, assuming the annual discount rate is 2%.

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%.<sup>6</sup> 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 comparing non-winners and winners of small prizes with winners of prizes in the range \$100,000–\$800,000.<sup>7</sup> Even though the *Respondents Sample* constitute less than a 1% subsample of the pooled lottery sample analysed 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

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 cut-off are dropped. For example, dropping the 415 prizes above \$200,000 (column 5 of Table 1) reduces treatment variation by 91%.

TABLE 2  
*Representativeness of survey respondents*

	Respondents sample				Survey population (5)	Representative sample (6)
	Kombi	Triss-lumpsum	Triss-monthly	Pooled		
	(1)	(2)	(3)	(4)		
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
No. 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
Labour 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
<i>N</i>	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 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.

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 labour 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 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.

#### 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 and 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 2 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 12–48, 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 [Supplementary 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 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  $t = 0$ . The dependent variable,  $y_{is}$ , 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  $\mathbf{Z}_{i,-1}$  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,  $\mathbf{X}_i$ . In particular,  $\mathbf{X}_i$  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 non-winning 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  $\mathbf{X}_i$  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 *et al.*, 2016) contains additional information about the methodology used to define the group identifiers.

Throughout, we report  $p$ -values based on analytical standard errors that have been clustered (Zeger and 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 and Young (1993). We refer to the resulting  $p$ -values as family-wise error rate (FWER)-adjusted  $p$ -values.

We use an estimating equation in which  $y_{i,t}$  depends linearly on  $L_{i,0}$  even though it is plausible that the true relationship is non-linear. 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.<sup>8</sup> 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 well-being is roughly constant across the prize distribution. Such proportionality stretches credulity 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 non-linear effects, we instead report the results from a robustness test that omits large-prize winners.

8. The 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.



### 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 ([Supplementary 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* ([Supplementary Table A3](#)). In test three, we found that the estimated effects of lottery wealth on net wealth, debt, capital income, and labour income do not change systematically when we restrict attention to the *Respondent Sample* by omitting the survey non-respondents from the estimation sample ([Supplementary 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.<sup>9</sup> 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).

[Supplementary 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 and Yitzhaki \(2017\)](#) and [Bond and Lang \(2018\)](#),

9. 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](#)).

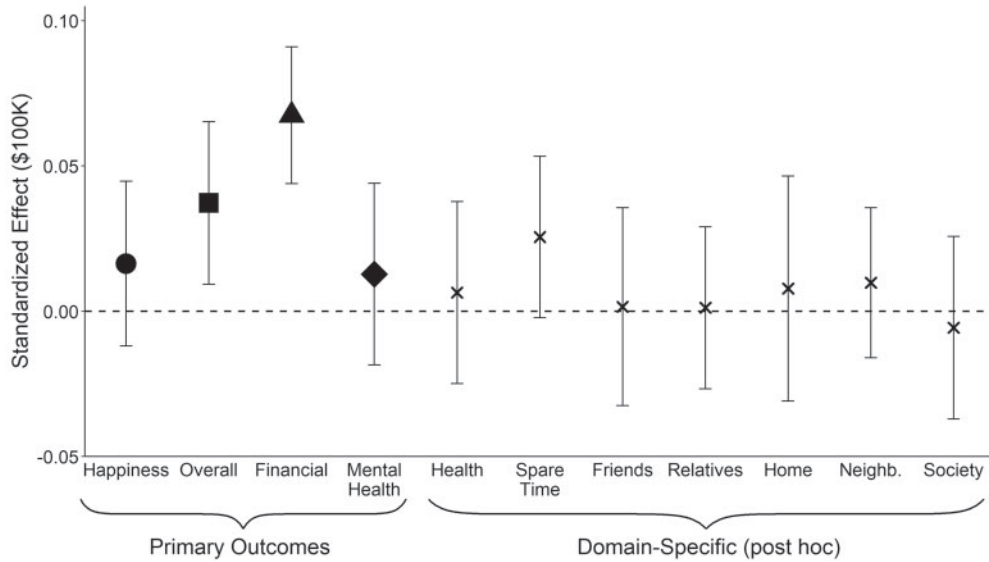


FIGURE 1

## Causal impact of wealth on primary outcomes and domain-specific measures of life satisfaction

Notes: 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 Supplementary 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).

TABLE 3  
*Happiness and life satisfaction (primary outcomes)*

	Happiness (1)	Overall Life Satisfaction (2)	Financial Life Satisfaction (3)	Mental Health (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)	[0.263]	[0.011]	[ $<0.001$ ]	[0.396]
FWER $p$	[0.396]	[0.026]	[ $<0.001$ ]	[0.396]
$N$	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 FWER is calculated using the free step-down resampling method of Westfall and 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*).

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 Supplementary Figure A2).

Following [Bond and 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.<sup>10</sup> In our final analysis, we only make use of the ordinal information in the survey responses. For each outcome, we defined and analysed 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. [Supplementary 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 [Supplementary Table A6](#) for the underlying estimates). For each of the seven outcomes—health, spare time, friends, relatives, home, neighbourhood, 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 labour supply, yet the reductions are modest, do not seem to depend on the type of prize (lump-sum or monthly instalments), 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., 2020](#)).<sup>11</sup>

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 labour 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.

10. 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.

11. Our evidence regarding winner behaviour is well in line with conclusions from interview-based research on lottery winners in multiple countries ([Kaplan, 1987](#); [Eckblad and Lippe, 1994](#); [Furåker and Hedenus, 2009](#); [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).

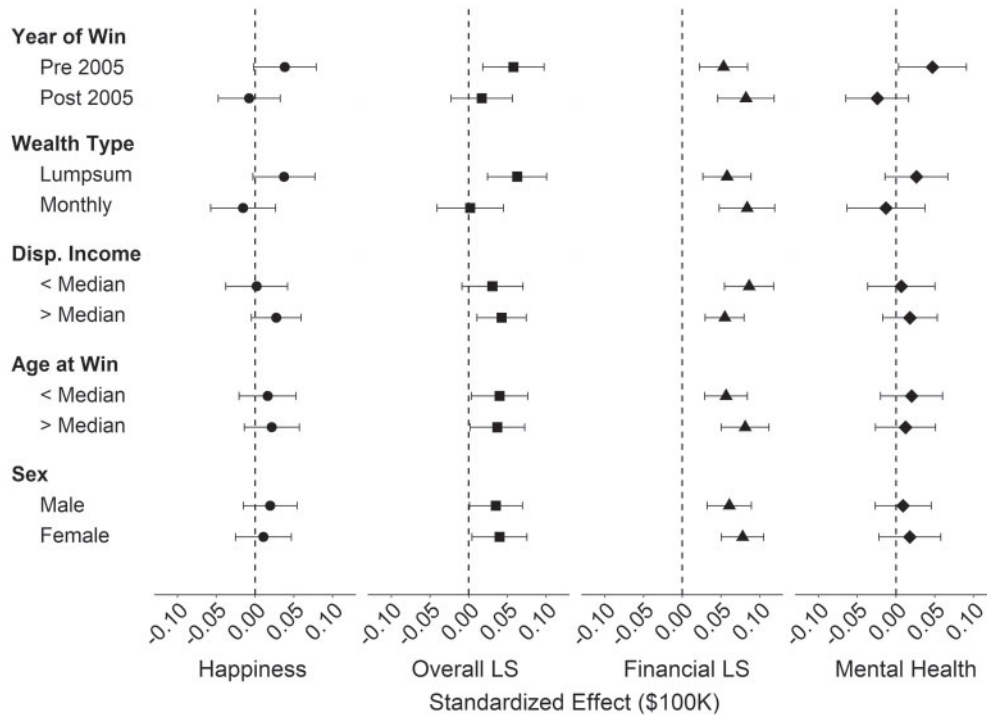


FIGURE 2  
Treatment-effect heterogeneity

Notes: The figure shows estimated treatment effects of \$100,000 USD (net of taxes) in subsamples define in the Analysis Plan. For underlying data, see [Supplementary Table A7](#).

#### 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 versus Triss-Lumpsum).<sup>12</sup> The results are shown in Figure 2 (see [Supplementary 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 versus 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

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.

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.<sup>13</sup>

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 [Supplementary 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 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 [Supplementary 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 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 ([Briggs \*et al.\*, 2020](#); [Cesarini \*et al.\*, 2016](#); [Cesarini \*et al.\*, 2017](#)). 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 2004–14), 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

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.

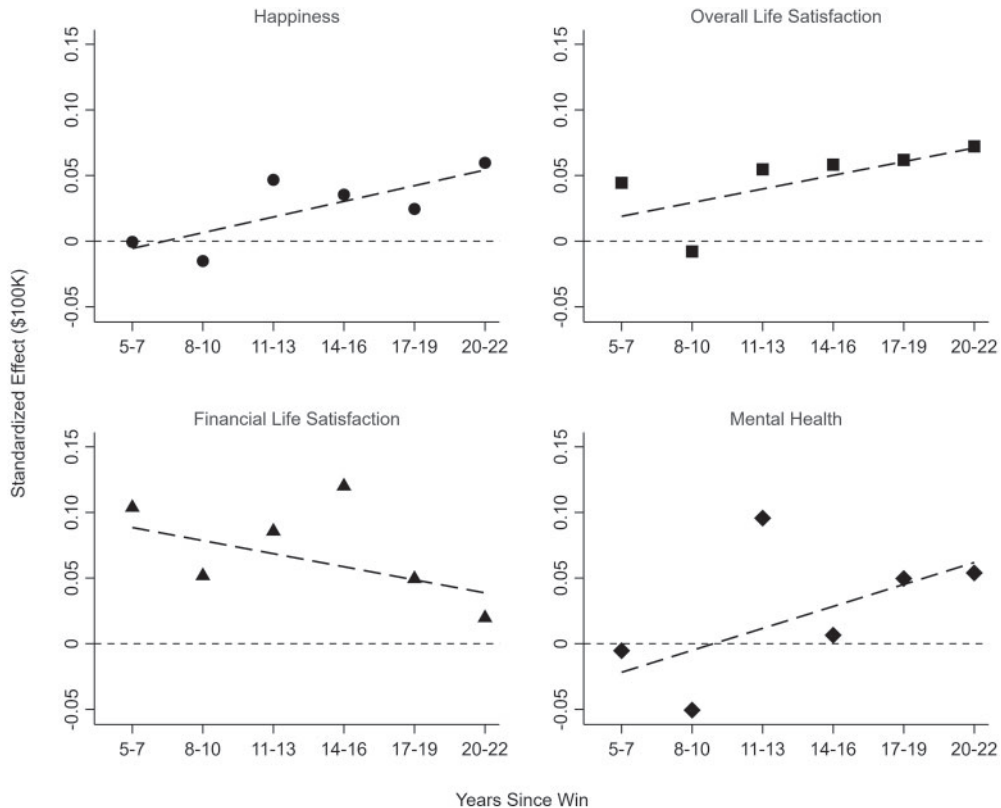


FIGURE 3

Treatment-effect heterogeneity by years-since-win (*post hoc*)

Notes: 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 [Supplementary Table A8](#).

between permanent annual income and our primary outcomes replicate standard patterns from the literature (Deaton, 2008; Stevenson and Wolfers, 2013). [Supplementary 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 (ESS; see Section 2 in the [Supplementary 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 [Supplementary Tables 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

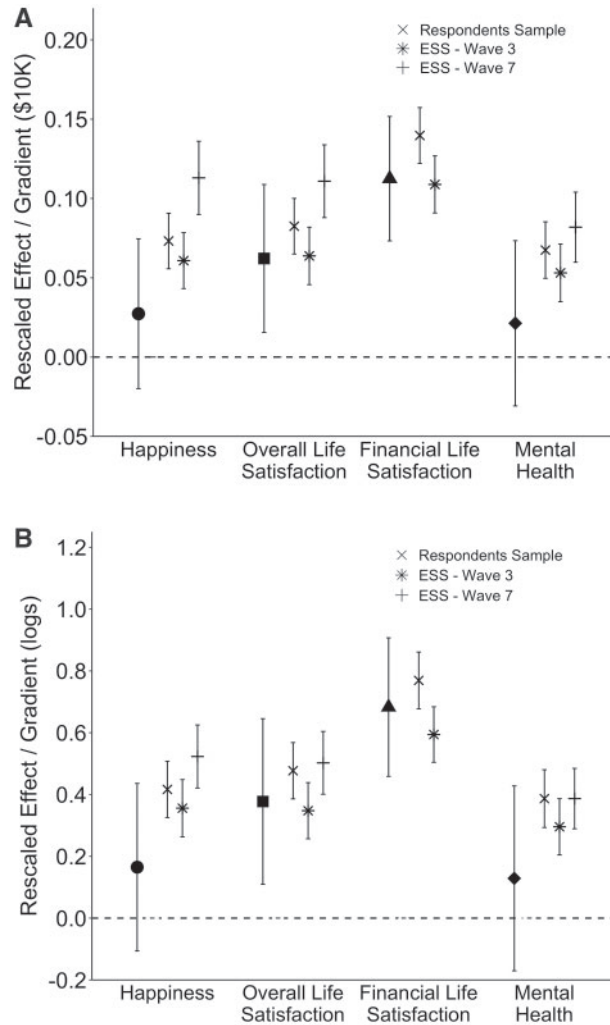


FIGURE 4

## Comparing annuity-rescaled treatment-effect estimates to income gradients

*Notes:* 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 ESS with comparable measures. 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 2004–14. 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 [Supplementary Tables A9 and A10](#).

them comparable to log-income gradients.<sup>14</sup> The lower panel of Figure 4 shows the log-income gradients fall within the normal range reported in previous literature ([Stevenson and Wolfers, 2008](#); [Stevenson and Wolfers, 2013](#)). For example, our life-satisfaction gradients range from 0.35

14. 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.)

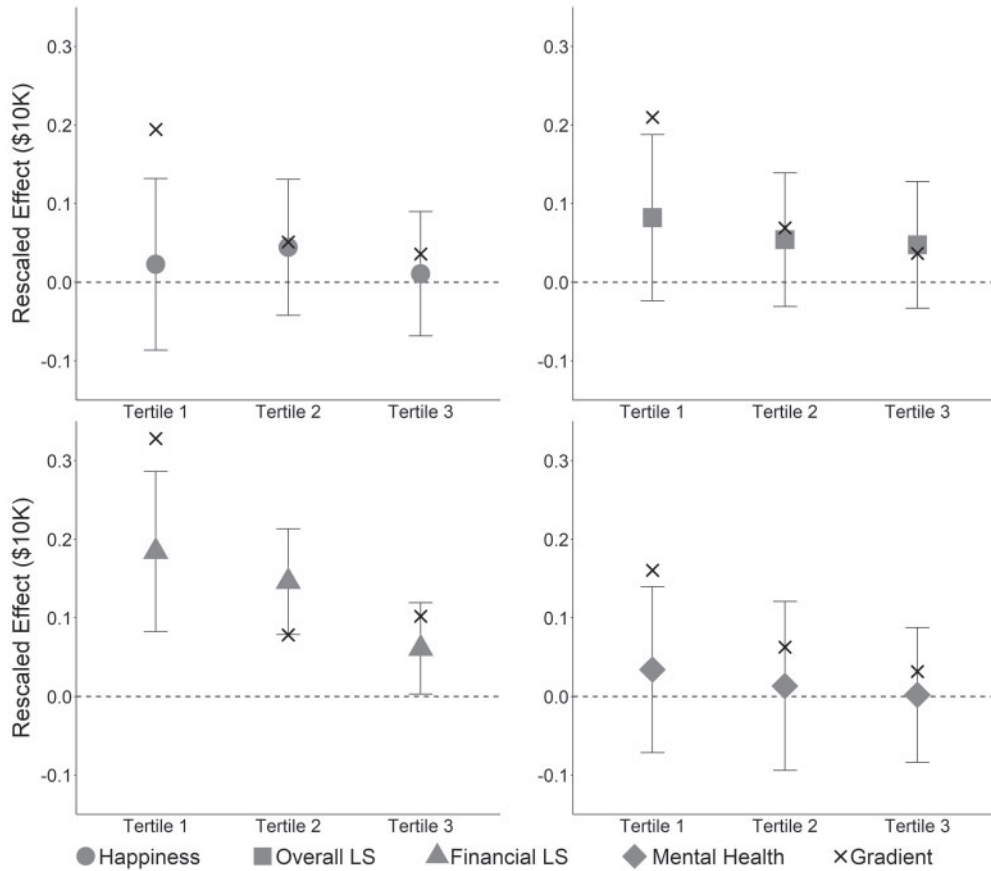


FIGURE 5

Comparisons of rescaled treatment effects and gradients by permanent-income tertile

Notes: 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 cut-off points that define the permanent-income tertiles. For underlying data, see [Supplementary Table A10](#).

to 0.50, whereas [Stevenson and 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 cut-off 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 [Supplementary 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](#)) analysed 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 \*et al.\*, 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 versus 0.016) and precision (0.025 versus 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 \*et al.\* \(1978\)](#) 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 and Oswald, 2007](#)). [Apouey and Clark \(2015\)](#) updated and extended the analysis of [Gardner and 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 2 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 non-significant results from an analysis of how lottery wealth impacts happiness.

Notably, all four studies that appeared after [Brickman \*et al.\* \(1978\)](#) 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, analysed 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 and Carlin, 2014](#)). Since [Gardner and Oswald \(2007\)](#) and [Apouey and Clark \(2015\)](#)

TABLE 4  
*Comparison to previous lottery studies*

	Outcome variable		Lottery prizes				Rescaled		Identification strategy
	Dimension	<i>t</i>	$N_{win}$	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 <i>et al.</i> (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	
Clark (2013)	Overall LS	1	674	5.8K	372	3.9M	0.264	0.436	Comparison of players (original lotteries unknown) who won different prize amounts.
	Overall LS	2	674	5.8K	372	3.9M	1.369	0.424	
Kuhn <i>et al.</i> (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 4 in the [Supplementary Appendix](#).

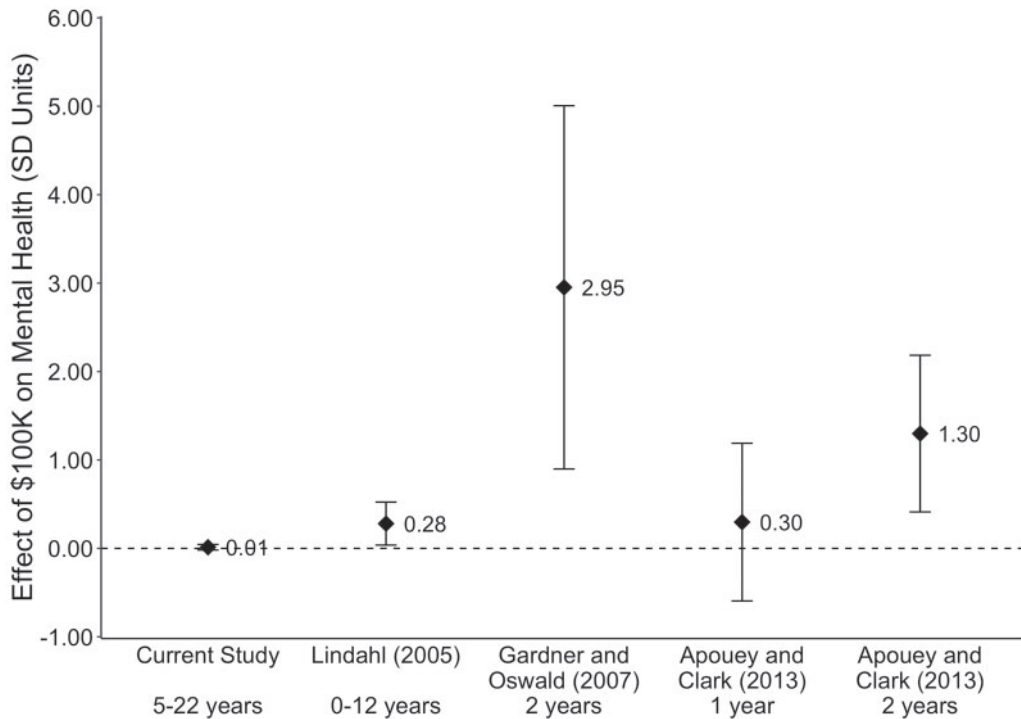


FIGURE 6

Comparisons of rescaled treatment effects for mental health

*Notes:* This figure compares our estimated treatment effect on *Mental Health* to those in previous quasi-experimental studies of lottery players' mental health. See [Supplementary Appendix](#) for details on these studies and how the originally reported estimates were rescaled for comparability. Error bars denote 95% confidence intervals.

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 non-significant result for happiness measured 6 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 and Oswald \(2007\)](#) and [Apouey and 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 ([Supplementary 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 and Wolfers \(2008\)](#). Whereas [Gardner and Oswald \(2007\)](#) and [Apouey and Clark \(2015\)](#) estimate large positive effects of lottery wealth on wellbeing in the U.K., the gradient is in fact flatter for the U.K. 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 \*et al.\* \(1978\)](#) 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 favourably 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 [Supplementary 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 and 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 and Frey \(2008\)](#) to compare our estimates to the magnitude of the negative association between commuting time and subjective well-being. [Stutzer and 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

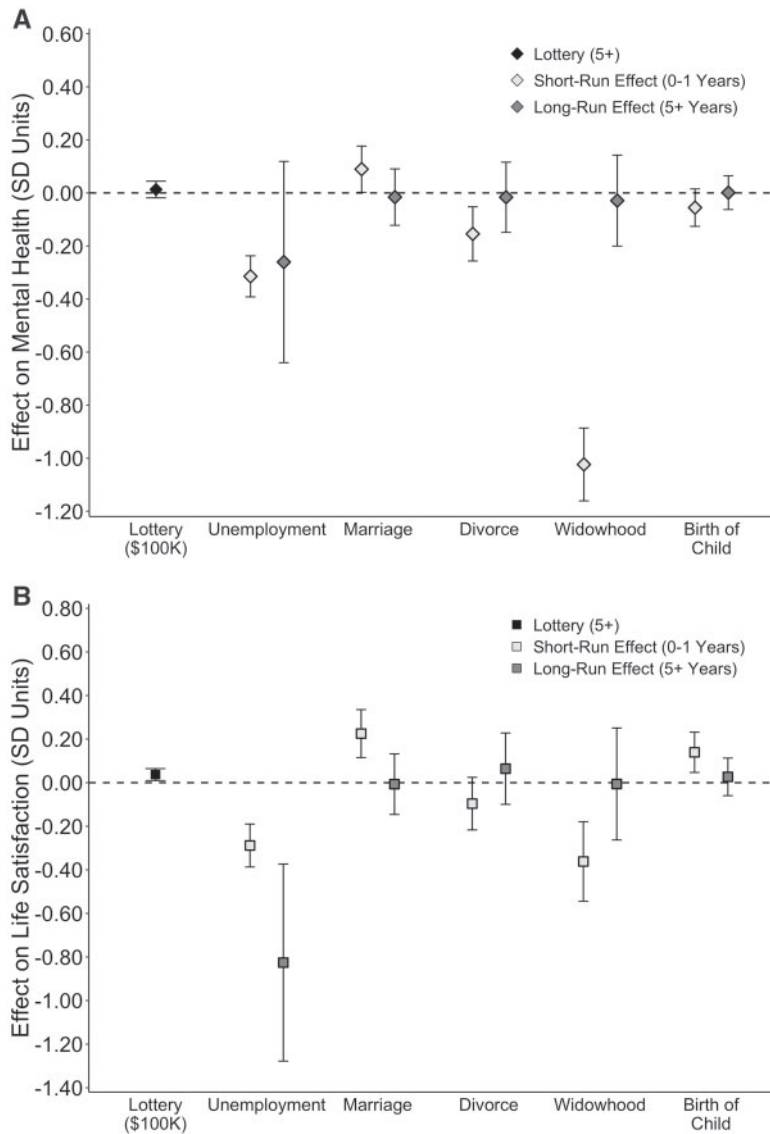


FIGURE 7

Comparison to previously reported estimates of life events

*Notes:* This figure compares our results to published estimates (Clark and 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 [Supplementary Appendix](#) for details on the event-study estimates.

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 5 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 reckless spending. However, consistent with the previous qualitative evidence (Kaplan, 1987; Eckblad and Lippe, 1994; Hedenus, 2011), we find little evidence of such behaviour 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 and Deaton, 2010; Jebb *et al.*, 2018). For example, Kahneman and 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 and 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 *et al.*, 2008; Stevenson and Wolfers, 2008; Sacks *et al.*, 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 and Kaunitz, 2015; Dobbie and Song, 2015) and negative wealth shocks (e.g. McInerney *et al.*, 2013) can have substantial adverse effects on well-being. Since all lottery prizes induce positive changes in wealth. Our data do not allow us to explore the intriguing possibility that the effects of negative and positive wealth shocks are asymmetric.

*Acknowledgments.* All authors contributed equally to this work. We thank Agneta Berge, Ed Kong, Chanwook Lee, Tuan Nguyen, and Becky Royer for excellent research assistance. We thank seminar participants at Advances with Field Experiments 2018, Columbia, CReAM Workshop on Topics in Labour Economics, Essex, IFN, HECER, HKUST, LSE, Michigan, NIPE (Braga), OsloMet, SMU, SOFI, Stanford, Sussex, Uppsala, Vienna and Princeton for comments. Daniel Benjamin, Fredrik Bergdahl, Martin Berlin, Samantha Cherney, Bruno Frey, 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), the Hedelius Wallander Foundation (P2011:0032:1), and Riksbankens

Jubileumsfond (P15-0615:1). The collection and analysis of the survey data was approved by the Regional Ethical Review Board in Stockholm on 7 April 2016 (2016/524-31/5).

### Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

### REFERENCES

- APOUEY, B. and CLARK, A. E. (2015), "Winning Big but Feeling No Better? The Effect of Lottery Prizes on Physical and Mental Health", *Health Economics*, **24**, 516–538.
- BAIRD, S., DE HOOP, J. and ÖZLER, B. (2013), "Income Shocks and Adolescent Mental Health", *Journal of Human Resources*, **48**, 370–403.
- BENJAMIN, D. J., HEFFETZ, O., KIMBALL, M. S. et al. (2014), "Beyond Happiness and Satisfaction: Toward Well-Being Indices Based on Stated Preference", *American Economic Review*, **104**, 2698–2735.
- BERLIN, M. and KAUNITZ, N. (2015), "Beyond Income: The Importance for Life Satisfaction of Having Access to a Cash Margin", *Journal of Happiness Studies*, **16**, 1557–1573.
- BISWAS-DIENER, R. M. (2008), "Material Wealth and Subjective Well-Being", in Eid, M. and Larsen, R. J. (eds) *The Science of Subjective Well-Being*, Chapter 15 (New York: The Guilford Press) 307–322.
- BLOEM, J. R. (2019), "How Much Does the Cardinal Treatment of Ordinal Variables Matter? An Empirical Investigation" (Unpublished working paper).
- BOND, T. N. and LANG, K. (2018), "The Sad Truth about Happiness Scales", *Journal of Political Economy*, **127**, 1629–1640.
- BRICKMAN, P. and CAMPBELL, D. T. (1971), "Hedonic Relativism and Planning the Good Society", in Appley, M. H. (ed.) *Adaptation-level Theory* (New York: Academic Press) 287–305.
- BRICKMAN, P., COATES, D. and JANOFF-BULMAN, R. (1978), "Lottery Winners and Accident Victims: Is Happiness Relative?", *Journal of Personality and Social Psychology*, **36**, 917–927.
- BRIGGS, J., CESARINI, D., LINDQVIST, E. et al. (2020), "Windfall Gains and Stock Market Participation", *Journal of Financial Economics*, forthcoming.
- CESARINI, D., LINDQVIST, E., NOTOWIDIGDO, M. et al. (2017), "The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries", *American Economic Review*, **107**, 3917–3946.
- CESARINI, D., LINDQVIST, E., ÖSTLING, R. et al. (2016), "Wealth, Health, and Child Development: Evidence from Administrative Data on Swedish Lottery Players", *Quarterly Journal of Economics*, **131**, 687–738.
- CLARK, A. E. and GEORGELLIS, Y. (2012), "Back to Baseline in Britain: Adaptation in the British Household Panel Survey", *Economica*, **80**, 496–512.
- CLARK, A. E., FRIJTERS, P. and SHIELDS, M. A. (2008), "Relative Income, Happiness, and Utility: An Explanation for the Easterlin Paradox and Other Puzzles", *Journal of Economic Literature*, **46**, 95–144.
- DEATON, A. (2008), "Income, Health, and Well-Being Around the World: Evidence from the Gallup World Poll", *Journal of Economic Perspectives*, **22**, 53–72.
- DIENER, E. and BISWAS-DIENER, R. (2002), "Will Money Increase Subjective Well-Being?", *Social Indicators Research*, **57**, 119–169.
- DIENER, E., SUH, E. M., LUCAS, R. E. et al. (1999), "Subjective Well-Being: Three Decades of Progress", *Psychological Bulletin*, **125**, 276–302.
- DOBBIE, W. and SONG, J. (2015), "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection", *American Economic Review*, **105**, 1272–1311.
- DOLAN, P. and FUJIWARA, D. (2016), "Happiness-Based Policy Analysis", in Adler, M. and Fleurbaey, M. (eds) *The Oxford Handbook of Well-Being and Public Policy* (Oxford: Oxford University Press).
- DOLAN, P., PEASGOOD, T. and WHITE, M. (2008), "Do We Really Know What Makes Us Happy? A Review of the Economic Literature on the Factors Associated with Subjective Well-Being", *Journal of Economic Psychology*, **29**, 94–122.
- EASTERLIN, R. (1974), "Does Economic Growth Improve the Human Lot? Some Empirical Evidence", in David, P. A. and Reeder, M. W. (eds) *Nations and Households in Economic Growth: Essays in Honor of Moses Abramovitz* (New York: Academic Press) 89–125.
- EASTERLIN, R. A. (1995), "Will Raising the Incomes of All Increase the Happiness of All?", *Journal of Economic Behavior & Organization*, **27**, 35–47.
- ECKBLAD, G. and LIPPE, A. (1994), "Norwegian Lottery Winners: Cautious Realists", *Journal of Gambling Studies*, **10**, 305–322.
- FLEURBAEY, M. (2009), "Beyond GDP: The Quest for a Measure of Social Welfare", *Journal of Economic Literature*, **47**, 1029–1075.
- FREDERICK, S. and LOWENSTEIN, G. (1999), "Hedonic Adaptation", in Kahneman, D., Diener, E. and Schwartz, N. (eds) *Well-Being: The Foundations of Hedonic Psychology* (New York: Russell Sage) 302–329.
- FREY, B. and STUTZER, A. (2002), "What Can Economists Learn from Happiness Research?", *Journal of Economic Literature*, **40**, 402–435.

- FRIJTERS, P., GEISHECKER, I., HAISKEN-DENEW, J. et al. (2006), "Can the Large Swings in Russian Life Satisfaction Be Explained by Ups and Downs in Real Incomes?", *Scandinavian Journal of Economics*, **108**, 433–458.
- FRIJTERS, P., HAISKEN-DENEW, J. P. and SHIELDS, M. A. (2004), "Money Does Matter! Evidence from Increasing Real Income and Life Satisfaction in East Germany Following Reunification", *American Economic Review*, **94**, 730–740.
- FURÅKER, B. and HEDENUS, A. (2009), "Gambling Windfall Decisions: Lottery Winners and Employment Behavior", *UNLV Gaming Research & Review Journal*, **13**, 1–15.
- GARDNER, J. and OSWALD, A. J. (2007), "Money and Mental Wellbeing: A Longitudinal Study of Medium-sized Lottery Wins", *Journal of Health Economics*, **26**, 49–60.
- GELMAN, A. and CARLIN, J. (2014), "Beyond Power Calculations: Assessing Type S (Sign) and Type M (Magnitude) Errors", *Perspectives on Psychological Science*, **9**, 641–651.
- GOLDBERG, D. P. and WILLIAMS, P. (1988), *A User's Guide to the General Health Questionnaire* (Berkshire: NFER-NELSON).
- HAUSHOFER, J. and SHAPIRO, J. (2016), "The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya", *Quarterly Journal of Economics*, **131**, 1973–2042.
- HEDENUS, A. (2011), "Finding Prosperity as a Lottery Winner: Presentations of Self after Acquisition of Sudden Wealth", *Sociology*, **45**, 22–37.
- JEBB, A. T., TAY, L., DIENER, E. et al. (2018), "Happiness, Income Satiation and Turning Points Around the World", *Nature Human Behaviour*, **2**, 33–38.
- KAHNEMAN, D., KRUEGER, A. B., SCHKADE, D. et al. (2006), "Would You Be Happier If You Were Richer? A Focusing Illusion?", *Science*, **312**, 1908–1910.
- KAHNEMAN, D. and DEATON, A. (2010), "High Income Improves Evaluation of Life but not Emotional Well-Being", *Proceedings of the National Academy of Sciences*, **107**, 16489.
- KAPLAN, H. (1987), "Lottery Winners: The Myth and Reality", *Journal of Gambling Behavior*, **3**, 168–178.
- KUHN, P., KOOREMAN, P., SOETEVENT, A. et al. (2011), "The Effects of Lottery Prizes on Winners and Their Neighbors: Evidence from the Dutch Postcode Lottery", *American Economic Review*, **101**, 2226–2247.
- LACHOWSKA, M. (2017), "The Effect of Income on Subjective Well-Being: Evidence from the 2008 Economic Stimulus Tax Rebates", *Journal of Human Resources*, **52**, 374–417.
- LARSSON, B. (2011), "Becoming a Winner But Staying the Same: Identities and Consumption of Lottery Winners", *American Journal of Economics and Sociology*, **70**, 187–209.
- LAYARD, R. (2006), "Happiness and Public Policy: a Challenge to the Profession", *Economic Journal*, **116**, C24–C33.
- LINDAHL, M. (2005) "Estimating the Effect of Income on Health and Mortality Using Lottery Prizes as an Exogenous Source of Variation in Income", *Journal of Human Resources*, **40**, 144–168.
- LUTTMER, E. F. P. (2005), "Neighbors as Negatives: Relative Earnings and Well-Being", *Quarterly Journal of Economics*, **120**, 963–1002.
- MARINESCU, I. (2018), "No Strings Attached: The Behavioral Effects of U.S. Unconditional Cash Transfer Programs", (NBER Working Paper No. 24337).
- MCINERNEY, M., MELLOR, J. M. and NICHOLAS, L. H. (2013), "Recession Depression: Mental Health Effects of the 2008 Stockmarket Crash", *Journal of Health Economics*, **32**, 1090–1104.
- MUKHERJEE, B., AHN, J., LIU, I. et al. (2008), "Fitting Stratified Proportional Odds Models by Amalgamating Conditional Likelihoods", *Statistics in Medicine*, **27**, 4950–4971.
- ÖSTLING, R., LINDQVIST, E. and CESARINI, D. (2016), "The Effect of Wealth on Survey Outcomes", *Analysis Plan Archived on through the Open Science Framework*. <https://osf.io/t3qb5/>.
- PAXSON, C. and SCHADY, N. (2010), "Does Money Matter? The Effects of Cash Transfers on Child Development in Rural Ecuador", *Economic Development and Cultural Change*, **59**, 187–229.
- SACKS, D. W., STEVENSON, B. and WOLFERS, J. (2012), "The New Stylized Facts about Income and Subjective Well-Being", *Emotion*, **12**, 1181–1187.
- SCHIMMACK, U. (2008), "The Structure of Subjective Well-Being", in Eid, M. and Larsen, R. J. (eds) *The Science of Subjective Well-Being*, Chapter 6 (New York: Guilford Press) 97–123.
- SCHRÖDER, C. and YITZHAKI, S. (2017), "Revisiting the Evidence for Cardinal Treatment of Ordinal Variables", *European Economic Review*, **92**, 337–358.
- SCHWANDT, H. (2018), "Wealth Shocks and Health Outcomes: Evidence from Stock Market Fluctuations", *American Economic Journal: Applied Economics*, **10**, 349–77.
- STEVENSON, B. and WOLFERS, J. (2008), "Economic Growth and Subjective Well-Being: Reassessing the Easterlin Paradox", (Brookings Papers on Economic Activity, Spring 2008) 1–87.
- STEVENSON, B. and WOLFERS, J. (2013), "Subjective Well-Being and Income: Is There Any Evidence of Satiation", *American Economic Review*, **103**, 598–604.
- STUTZER, A. and FREY, B. S. (2008), "Stress that Doesn't Pay: The Commuting Paradox", *Scandinavian Journal of Economics*, **110**, 339–366.
- WESTFALL, P. H. and STANLEY YOUNG, S. (1993), *Resampling-Based Multiple Testing: Examples and Methods for P-value Adjustment* (New York: Wiley).
- YOUNG, A. (2018), "Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results." *The Quarterly Journal of Economics*, **134**, 557–598.
- ZEGER, S. L. and LIANG, K.-Y. (1986), "Longitudinal Data Analysis for Discrete and Continuous Outcomes", *Biometrics*, **42**, 121–130.