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ABSTRACT

This paper investigates whether individuals feel worse off when others around them earn more. In other words, do people care about relative position and does "lagging behind the Joneses" diminish well-being? To answer this question, I match individual-level panel data containing a number of indicators of well-being to information about local average earnings. I find that, controlling for an individual's own income, higher earnings of neighbors are associated with lower levels of self-reported happiness. The data's panel nature and rich set of measures of well-being and behavior indicate that this association is not driven by selection or by changes in the way people define happiness. There is suggestive evidence that the negative effect of increases in neighbors' earnings on own well-being is most likely caused by interpersonal preferences, i.e. people having utility functions that depend on relative consumption in addition to absolute consumption.

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1. Introduction

Classical economists understood that individuals are motivated at least partly by concerns about relative position. Adam Smith (1759), for example, wrote: "*Nothing is so mortifying as to be obliged to expose our distress to the view of the public, and to feel, that though our situation is open to the eyes of all mankind, no mortal conceives for us the half of what we suffer. Nay, it is chiefly from this regard to the sentiments of mankind, that we pursue riches and avoid poverty."* Arthur Pigou (1920) approvingly quotes John Stuart Mill's observation that "*men do not desire to be rich, but richer than other men."*[1] Of course, the belief that people may compare themselves to others around them goes back much further. After all, the framer of the Ten Commandments apparently judged it necessary to forbid humans from coveting their neighbor's possessions. Not all humans, however, appear to abide by this Commandment and possible effects of social comparisons on consumption and savings behavior are analyzed in the classic works of Veblen (1899) and Duesenberry (1949).

Though contemporary economists are aware that individuals may care about relative position, the accepted mainstream model states that individuals derive utility from their own consumption, U(C), rather than from a combination of own and relative consumption, U(C, $C/\overline{C}$), where $\overline{C}$ denotes some measure of the consumption of relevant others.[2] For many applications it does not matter whether utility has a relative component; whenever $\overline{C}$ is fixed or given, U(C) and U(C, $C/\overline{C}$) are isomorphic. Indeed, unless an individual's behavior can affect

[1] This is quoted in Graham and Pettinato (2002).

[2] Becker (1974) introduces a more general framework for incorporating social considerations into a utility function. $\overline{C}$ need not be the simple average of the consumption of relevant others, but could be some weighted average with the weight depending on the similarity of the respondent to the other person or on the level of consumption of the other person.

$\overline{C}$, U(C) and U(C, $C/\overline{C}$) cannot be distinguished by individual behavior without placing additional structure on the utility function.[3] In light of this, it is perhaps not surprising that most economists tend to rely on an absolute formulation of utility: U(C).

Whereas individuals may in many cases take $\overline{C}$ as given, policy decisions often affect $\overline{C}$. Hence, the distinction between absolute and relative formulations of utility has important implications for tax and expenditure policy, as analyzed by Boskin and Sheshinski (1978), Layard (1980), Oswald (1983), Ng (1987), Seidman (1987), Ireland (1998), Ljungqvist and Uhlig (2000) and Abel (2003). In particular, if utility depends on relative consumption, one person's increase in consumption has a negative externality on others because it lowers the relative consumption of others. In this case, taxes that discourage consumption are not as distortionary as previously thought because they also serve to internalize the negative externality of consumption on others.

When utility depends on one's consumption relative to nearby others, residential sorting by consumption level will reduce inequality of utility because people will compare themselves with others more like themselves. Comparing oneself to similar others reduces inequality in perceived relative position, which is one component of inequality of utility. If utility is linear in relative consumption and additive in absolute and relative consumption, then residential sorting by consumption level does not affect average utility, but for other functional forms it can increase or decrease average utility. Thus, when people care about relative position, the degree

[3] A structure that specifies that relative concerns are more important for some goods (e.g. present consumption or luxury consumption items) than for other goods (e.g. leisure or future consumption) yields behavioral implications. See, for example, Pollak (1976) or Frank (1985). As Dupor and Liu (2003) make clear, if the consumption of others affects own *marginal* utility rather the *level* of own utility, the consumption of others can influence asset pricing, risk taking, savings, intensity of job search, work effort, economic growth and income inequality. See, e.g., Abel (1990), Robson (1992), Galí (1994), Carroll, Overland and Weil (1997), Campbell and Cochrane (1999), Stutzer and Lalive (2001), Becker, Murphy and Werning (2003) and Zeckhauser and Rizzo (2003).

of residential sorting has direct effects on the distribution of well-being in addition to the indirect effects usually identified (e.g. due to the financing of local public goods).

The distinction between relative and absolute formulations of utility is also pertinent to the longstanding debate on whether the poverty line should be absolute (a fixed consumption basket), as it is in the U.S. and in many developing countries, or relative (a fraction of mean or median income), as it is in much of Europe (Sen, 1983). It also matters for the question whether increases in inequality due to a rise in the top incomes are a matter of policy concern. Feldstein (1998), for example, argues forcefully that only poverty, not increases in inequality due to increased top incomes, warrant policy attention because the latter is a pure Pareto improvement under the assumption that people only derive utility from their own consumption. He notes that this argument would not hold for "spiteful egalitarians," or people who say "It makes me worse off to see the rich getting richer." While one may reject spiteful egalitarianism on moral grounds (and therefore use a paternalistic argument to exclude this spiteful utility component from any social welfare function), it is an empirical question to determine whether people actually suffer from this affliction. In other words, do people actually feel worse off when those around them are richer?[4]

This paper tries to answer that question and finds evidence of "spiteful egalitarianism." I use panel data on individuals' self-reported happiness, other measures of well-being and other characteristics from the 1987-88 and the 1992-94 waves of the National Survey of Families and Households (NSFH). I match this data to information on local earnings, where localities are so-called Public Use Microdata Areas ("PUMAs"), which have about 150,000 inhabitants on average. Average annual earnings in each PUMA are estimated by applying national earnings by

[4] Samuelson (2004) and Rayo and Becker (2004) offer evolutionary explanations of relative consumption effects. Postewaite (1998) discusses the advantages and drawbacks of modeling relative position as an argument of the utility function rather than as an instrument to getting more own consumption in the future.

industry, occupation and year from the Current Population Survey to the industry and occupation mix of that PUMA from the 1990 Census five percent Public Use Micro Sample. I find that higher PUMA-level earnings are associated with lower levels of happiness, controlling for a host of individual characteristics including income.[5] This effect is large, robust to changes in specification and highly statistically significant. An increase in neighbors' earnings and a similarly sized decrease in own income each lead to a reduction in happiness of about the same order of magnitude.

This paper builds on previous papers that have empirically examined the relationship between relative position and well-being.[6] In a series of papers, Easterlin (1974, 1995 and 2001) notes that income and self-reported happiness are positively correlated across individuals within a country but that average happiness within countries does not seem to rise over time as countries become richer. Easterlin interprets these findings as evidence that relative income rather than the level of income matters for well-being. Veenhoven (1991) and Diener et al. (1993) show that happiness is not purely a relative concept, but they cannot rule out that concerns about relative position matter. Van de Stadt, Kapteyn and Van de Geer (1985) find that the income level that respondents say they need to reach a certain level of satisfaction rises both with own income and with income in one's reference group as defined by education and age. They interpret this as

[5] Though, at a conceptual level, relative consumption rather than relative income or earnings affects well-being, I use measures of earnings and income as proxies for consumption in the empirical section because of data availability.

[6] Frey and Stutzer (2002) provide an excellent review of this literature. Layard (2003) also discusses much of this literature as part of his engaging Lionel Robbins Memorial Lectures on happiness. Relative position may affect outcome variables other than subjective well-being. Clark and Oswald (1996) show evidence on job satisfaction while Neumark and Postlewaite (1998) and Bowles and Park (2002) relate it to labor supply decisions. There is a long literature on the effects of inequality on health outcomes. Deaton (2003) surveys this literature and concludes that the evidence on the relation between income inequality and health needs to be treated skeptically though he believes there is convincing biological evidence that increases in rank can be protective of health. Eibner and Evans (2001) find evidence for the U.S. that relative deprivation (which is a measure of rank and the income gap with those who are richer) increases mortality. Similarly, local variables other than income may affect well-being. Clark (2003) finds effects of local unemployment on happiness that may be explained by concerns about relative position. Alesina, Di Tella and MacCulloch (2001) investigate how inequality affects well-being and, intriguingly, find negative effects in Europe but not in the U.S.

evidence that utility is (partly) relative, but the findings could also be driven by reference group income proxying for poorly measured own income.[7] Using a self-reported measure of relative position, Graham and Pettinato (2002) find suggestive evidence in developing countries that well-being is influenced by relative income concerns though their measure of relative position might also proxy for own income.

Using Canadian data, Tomes (1986) relates self-reported happiness and life-satisfaction to own income and income in the local community. In a number of specifications, he finds that increases in income in certain parts of the community income distribution reduce well-being and he concludes that his "results support the interdependent preferences model, but defy any simple characterization in terms of inequality aversion or relative economic status." Using the German Socio-Economic Panel, Ferrer-i-Carbonell (2004) finds compelling evidence that, controlling for own income, subjective well-being is decreasing in income of the reference group defined by education × age × region cells where region is East or West Germany. Stutzer (2004) shows that self-reported happiness of Swiss individuals depends negatively on income aspirations when own income is held constant. Income aspirations, in turn, depend positively on the average income in one's community, thus providing a mechanism by which average local income reduces self-reported happiness. Using U.S. data, McBride (2001) and Oswald and Clark (2004) both find tantalizing evidence that relative income affects subjective well-being but they caution about the statistical reliability of their findings.[8]

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[7] Van Praag and Kapteyn (1973) found previously that income needed to reach a certain level of satisfaction rises with own income and interpret this as "preference drift," or evidence of habit formation. See Van Praag and Frijters (1999) for more details on the approach to measuring welfare using income need questions.

[8] Using a sample of 324 individuals from the General Social Survey, McBride (2001) finds that, controlling for own income, self-reported happiness depends negatively on the average income in one's age cohort defined as those within 5 years of the respondent's age though this effect is only just significant at the 5% level in one of his two specifications and not significant in the other. Oswald and Clark (2004) find that, controlling for own income, there is a sizeable but statistically insignificant negative effect of per capita state income on self-reported happiness, providing suggestive evidence that individuals care about relative position. They also find that relative income

This paper contributes in three ways to this literature. First, it takes seriously the concern that living in a prosperous area might affect one's *definition* of happiness even if it does not affect one's true or experienced well-being. I use other outcome measures that are less prone to definition shifts in response to neighbors' earnings to investigate this concern and conclude that this concern is not driving the results.

Second, the paper examines whether the inverse relationship between happiness and neighbors' earnings might be spurious due to omitted individual or local characteristics. The panel nature of the NSFH data enables me to run specifications with individual fixed effects, its detailed geographical information allows for the inclusion of state×time-specific fixed effects, and the use of a predicted measure of local earnings filters out many local earnings shocks caused by unobserved local factors that might simultaneously influence happiness. The results hold up under these specifications, reducing the concern that they are due to omitted variable bias.

Third, the paper offers suggestive evidence concerning the mechanism mediating the negative relationship neighbors' earnings and happiness. Is it a psychological externality of the form $U(C, C/\overline{C})$ as laid out above? Or might there be market interactions (e.g. in the housing market) that give rise to this pattern? In contrast to what one would expect if the findings were driven by effects in local housing markets, I find no evidence that there is any difference in the effect for renters and owners. Yet, I do find evidence that the results are stronger for people who socialize more with neighbors but not for those who socialize more with friends outside the neighborhood. I also find that the effects on happiness are mostly driven by changes in one's

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enters significantly if entered as household income per capita / state income per capita. However, because this regression controls for the log of household income per capita rather than the level of household income per capita, the relative income term may be significant because it offers an alternative functional form for own household income and not because of variation in state income per capita.

satisfaction with one's material position (e.g. one's financial situation) rather than changes with one's satisfaction with other aspects of life (e.g. one's family life). Finally, individuals' happiness decreases if neighbors with the same educational attainment earn more but hardly responds to earnings of neighbors with different levels of educational attainment. Together, these findings indicate that interpersonal preferences that incorporate relative income concerns, rather than other mechanisms, drive the negative association between neighbors' earnings and own well-being.

2. Empirical Strategy

Can data on behavior reveal whether people's well-being is affected by the incomes of others around them? Unless one assumes that neighbors' incomes affect an individual's *marginal* utility of other goods, the only behavior affected is the individual's choice of reference group implicit in the decision where to locate.[9] Individuals' concerns about relative position might then be capitalized in house prices with houses in high-income neighborhoods costing relatively less than similar houses in low-income neighborhoods because a homeowner in a rich neighborhood needs to be compensated for being relatively poor.[10] This prediction, of course, only holds if individuals are both (i) aware that their utility depends on relative position and (ii) correctly forecast the utility effect of the change in reference group associated with moving.

[9] See, Falk and Knell (2003) for evidence on reference groups choices from a questionnaire study.
[10] This prediction is derived from Frank's (1984) model in which he analyzes the effects of relative income concerns on wage distributions. He assumes that the reference group consists of coworkers and deduces that a worker in firm with highly productive (and highly paid) workers must be paid more than a similar worker in a low productivity firm because the worker needs to be compensated for the utility loss of being a relatively low earner if he joins the firm with the high earners.

Loewenstein, O'Donoghue and Rabin (2003) describe a number of experiments that show systematic biases in individuals' predictions of their future utility. With respect to endogenous reference groups, they note that "*when people make decisions that cause their comparison groups to change – such as switching jobs or buying a house in a new neighborhood – projection bias predicts that people will underappreciate the effects of a change in comparison groups and hence, consistent with Smith's assertion, overestimate the long-term satisfaction that would accompany such a change. As a result, people may be prone to make reference-group-changing decisions that give them a sensation of status relative to their current reference group. If a person buys a small house in a wealthy neighborhood in part because it has a certain status value in her apartment building, she may not fully appreciate that her frame of references may quickly become the larger houses and bigger cars that her new neighbors have.*" These considerations make credible identification of relative income concerns from mobility decisions or housing price information very challenging.

The identification of relative income concerns therefore probably falls in the limited set of research questions for which one needs to turn to a proxy for utility to answer it (see, Di Tella et al., 2001, Gruber and Mullainathan, 2002, or Frey et al., 2004, for other examples of such questions).[11] Though some skepticism towards self-reported measures of well-being is warranted (see, Bertrand and Mullainathan, 2001, and Ravallion and Lokshin, 2001), there is ample psychological evidence that confirms the validity and reliability of self-reported happiness as a measure of well-being (see, e.g. Kahneman, 1999). Frey and Stutzer (2002) therefore conclude that "the existing research suggests that, for many purposes, happiness or reported subjective well-being is a satisfactory empirical approximation to individual utility"

[11] Zeckhauser (1991), Solnick and Hemenway (1998) and Johansson-Stenman (2002) find evidence for positional concerns by asking subjects to make choices over hypothetic scenarios with different levels of absolute and relative income.

To determine whether well-being depends partly on relative income concerns, one might then estimate an equation of the form:[12]

(1) *self-reported well-being* = f(*own income, average income in locality, controls*)

To make the discussion of the empirical strategy more concrete, a very basic linear OLS regression with self-reported happiness (on a 1-7 scale) as the outcome variable yields a coefficient of 0.20 (s.e. of 0.014) on log own household income and a coefficient of -0.20 (s.e. of 0.04) on log average per capita income in one's locality (PUMA).[13] This regression previews the general findings of the more elaborate regressions presented in detail in the results section below. Can this finding of a negative coefficient on *average income in locality* (and a positive one on *own income*) be interpreted as evidence that utility is at least partly determined by relative income? Or are there plausible alternative stories that could give rise to the same result even if utility is purely a function of own income? Below, I discuss the three most serious threats to a causal interpretation of the coefficient on *average income in locality* and consider ways of testing them.

The first alternative story is that the definition of happiness shifts: people answer the question about their happiness in relative rather than absolute terms (Tversky and Griffin, 1991, and Frederick and Loewenstein, 1999).[14] In this case, self-reported happiness would be a proxy for relative experienced well-being rather than absolute experienced well-being. Suppose, for example, that each individual's experienced well-being, U_i, is equal to her income, Y_i, and that individuals are asked whether they are happy or not. Individuals now face the task of translating

[12] I enter *average income in locality* separately in this specification rather than in form of the ratio of *own income* to *average income in locality*. I do this because, in practice, I have a number of proxies for own income instead of a single measure.

[13] For simplicity, no other controls are included in this illustrative regression. Standard errors are adjusted for clustering at the PUMA level.

[14] King et al. (2004) explain how vignettes can be used to anchor the scale of subjective questions thereby making the answers comparable across people. No such vignettes, however, were used in the NSFH.

experienced well-being into an answer to this question. If people respond that they are happy whenever U_i exceeds some *fixed* (but possibly individual-specific) cutoff value, then they answer the question in absolute terms. In this case, an increase of everyone's income by the same factor would increase the fraction of people answering they are happy. However, if people respond that they are happy whenever their U_i exceeds some cutoff value that is a function of the population distribution of U_i (such a the mean or median U_i), they are answering the question in relative terms. In this case, an increase of everyone's income by the same factor may not affect the proportion of individuals answering that they are happy even though every individual's experienced utility is higher.[15] I address this concern by using alternative indicators of well-being that have a relatively objective definition, such as the frequency of financial worries or the frequency of marital disagreements.

The second alternative story is that the results are driven by unobserved local area characteristics that are both correlated with average local income and self-reported happiness. One might expect most of this type of omitted variable bias to go in the other direction; e.g., one would expect higher income areas to have less crime, better local schools and other positive amenities that raise happiness. The concern about local omitted variables driving the result is addressed in three ways. First, if the results hold up after inclusion of state×time fixed effects, they cannot be driven by unobservables that operate at that level, such as climate, state policies, or regional shocks. Second, one may be concerned that local incomes just proxy for the local price level. Thus, conditional on nominal income, an individual living in a high-income area would face higher prices and thus have less real income, reducing happiness.[16] However, if we

[15] This is also a potential explanation for the findings in a number of studies that levels of self-reported happiness or life satisfaction remain remarkably constant in a country over time even as incomes rise. See, e.g. Easterlin (1974).
[16] Note however that if higher price levels reflect positive local amenities, they do *not* reduce real income; in effect, the individual is purchasing the local amenity by locating in an expensive area. This means that only unobserved

control for real income instead, there is no longer a role for local area incomes to proxy for local price levels, and any negative effect of local income on happiness cannot be explained by this price level story. To control for real income, I include only those individual controls that do not vary with local prices. This excludes nominal earnings and home value, but still includes education, age, and average national earnings for someone in the same industry and occupation as the respondent. Third, instead of using actual local income, I use a predicted measure of local earnings. The predictor is based on the industry×occupation composition of the locality at *one point in time* (1990) and *national* industry×occupation earnings trends (excluding data from one's own state).[17] Thus, predicted local earnings vary across areas for two reasons: (i) the industry×occupation mix at a point in time, which we can control for using area fixed effects, and (ii) national earnings trends by industry and occupation, which we have no reason to believe to be correlated with unobserved local shocks. This predictor therefore filters out any local shocks (such as quality of local government) that may both affect local incomes and happiness. I refer to this measure as *LnPumaEarnings* or more informally as neighbors' earnings and use it as the measure of local earnings throughout this paper unless otherwise noted. The use of predicted local earnings, however, does not rule out that higher local earnings affect an unobserved local variable, which in turn reduces happiness. Two obvious candidates for such unobserved variables are quality of local schooling and changes to the housing market. If this is the case, we expect to see different effects by homeownership and by presence of children in the household.

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variation in local prices due to transportation cost or local production costs could possibly explain the findings. Since we can control for state×time fixed effects, the transportation or production cost differences should be *within* states to explain away the results. The scope for such variation is considerably less than the scope of transportation and production cost differences in the nation as a whole.

[17] This predictor follows similar predictors used by Bartik (1991), Blanchard and Katz (1992), Bound and Holzer (2000) and Autor and Duggan (2003). See appendix B for details on the construction of this predictor.

The third story is that the results are driven by omitted individual characteristics that influence both the decision where to live and self-reported happiness. In particular, selection of individuals with unobservables that make them relatively happy (or relatively likely to report being happy) into localities with relatively low incomes would also result in a negative coefficient on *average income in locality.* Though one might expect that most selection would go in the opposite direction (high income in one's locality proxying for higher unobserved own income), there could be selection effects that lead to a spurious negative effect of *average income in locality.* For example, individuals receiving an inheritance may be relatively unhappy (because of the unobserved death of a relative) but able to move to a relatively high-income locality, or intrinsically happy people might be better able to deal with the rougher aspects of low-income areas thus selecting to live there. This paper exploits the panel aspect of the NSFH data to deal with this concern. If, after inclusion of individual fixed effects, average income in the locality still matters, then we know that time-invariant unobserved individual characteristics cannot be driving the results.

To preserve statistical power, the baseline specification to test for relative income concerns is a pooled cross-section OLS regression of the form:

$$(2) \qquad Happiness_{ipt} = LnPumaEarnings_{pt}\,\beta_1 + \boldsymbol{X}_{it}\,\boldsymbol{\beta}_2 + wave_t\,\beta_3 + \varepsilon_{ipt}$$

where *i* indexes individuals, *p* indexes PUMAs, and *t* indexes the wave of the survey. In the baseline specification, *Happiness* is self-reported happiness (measured on a seven-point scale), but other correlates of well-being are used in alternative specifications. *LnPumaEarnings* are average predicted earnings in the PUMA of the respondent where the prediction is based on the PUMA's industry×occupation composition and national earnings trends. The vector $\boldsymbol{X}_{it}$ is a set of individual-specific controls that include a number of proxies for income as well as basic

demographics. Finally, $wave_t$ is a dummy for the wave of the survey and ε_{ipt} is an error term that may be clustered within PUMAs. If individuals derive utility in part from relative position, we would expect β_1 to be negative.

The baseline sample consists of individuals who are married or cohabitating in both waves of the NSFH. I limit the sample to married or cohabiting individuals for two reasons. First, for these observations, we also have information about spouses or interactions with one's spouse, which are useful in a number of further tests of the baseline results. Second, it turns out that married individuals drive the baseline results, though neighbors' earnings still have a negative and significant effect on happiness in the full sample that includes non-married individuals.

Since most questions are asked of both the main respondent and his or her spouse, it makes sense to also exploit the spousal information. Adding the spousal information as a separate observation to the regression may bias standard errors downwards because the error term of the respondent and the error term of his or her spouse are likely to be correlated.[18] Instead, I average the values of the individual level variables for the main respondent and his or her spouse, and enter those as a single observation in the regression. I thus do not exploit intra-household level variation, but this does not matter since the main explanatory variables of interest (neighbors' earnings and own household income) do not vary within households.

In the results section, I will present various modifications of this baseline regression to investigate whether the baseline results are spurious, whether they are robust, and what mechanisms drive them. These modifications will be explained in detail later and include adding

[18] I already cluster at the PUMA level (the level at which neighbors' earnings vary), and therefore cannot easily cluster at the household level at the same time.

individual fixed effects, adding state×wave fixed effects, exploring other outcome or control variables, and interacting *LnPumaEarnings* with other variables.

3. Data

National Survey of Families and Households

The data on subjective well-being as well as the individual-level control variables come from the National Survey of Families and Households.[19] The NSFH consists of a nationally representative sample of individuals, age 19 or older (unless married or living in a household with no one age 19 or older), living in households, and able to speak English or Spanish. The first wave of interviews took place in 1987-88 and a second wave of interviews took place in 1992-94. Though the questionnaires are not identical in both waves, many questions were asked twice making it possible to treat the data as a panel of about 10,000 individuals. What makes this dataset particularly well-suited for this paper is that, in addition to being a panel with both measures of well-being and extensive demographic information, it can be merged with detailed geographic information. The respondents of the baseline sample live in 580 separate Public Use Microdata Areas in the first wave, in 965 PUMAs in the second wave, while 555 PUMAs have respondents living there in both waves (more about the definition of PUMAs later).

The main outcome variable is self-reported happiness, which is the answer to the question: "*Next are some questions about how you see yourself and your life. First taking things all together, how would you say things are these days?*" Respondents answer on a seven-point

[19] The NSFH is a survey that was primarily designed for demographers interested in family and household issues. More information on the NSFH can be found in Sweet, Bumpass and Call (1988), in Sweet and Bumpass (1996) or at the NSFH website: http://www.ssc.wisc.edu/nsfh/home.htm

scale where 1 is defined as "very unhappy," 7 is defined as "very happy" but intermediate values are not explicitly defined. Other measures of well-being include the frequency of financial worries, the frequency of open disagreements with one's spouse on a number of topics, items from Lenore Radloff's (1977) depression scale, and, only in the second wave, self-reported satisfaction with various aspects of one's life. Detailed descriptions of these variables are included in appendix A.

The individual-level controls in the baseline specification consist of income proxies, labor market variables, demographic characteristics and religious affiliation. The main income variable is log household income while log value of the home and a dummy for being a renter may also proxy for own income. Any missing values are dummied out as are logarithms of any dollar amounts smaller than \$100/year.[20] The demographic controls consist of gender, a set of 4 race/ethnicity dummies, log household size, and a 3-segment spline in age (breakpoints at 35 and 60). Religious affiliation is controlled for by 12 dummy variables.

Census and Current Population Survey

The smallest geographical area in the 1990 Census 5% Public Use Micro Sample (PUMS) is the so-called Public Use Microdata Area. PUMAs consist of neighborhoods, towns or counties aggregated up, or subdivided, until they contain at least 100,000 inhabitants. In 1990, there were 1726 PUMAs in the U.S. and the median and mean size of a PUMA was respectively 127,000 and 144,000 inhabitants. The 1990 Census microdata are used to estimate the 3-digit industry × 3-digit occupation composition of each PUMA, which is later used to predict PUMA earnings. In addition, I use the Census to estimate average 1989 earnings for each PUMA, which will serve as a check on the predictor.

[20] All \$ amounts are in real 1982-84 dollars using the CPI-U.

I use the Merged Outgoing Rotation Groups (MORG) from the Current Population Survey (CPS) in the years 1987-88 and 1992-94 to estimate average earnings by 3-digit industry × 3-digit occupation cell in each of the two time periods when NSFH interviews took place. For each PUMA, I calculate these average national earnings by time-period×industry×occupation cell excluding data from the state in which this PUMA lies.

Predicted PUMA earnings for each wave are formed by applying average national earnings by industry×occupation cell during that time period to each observation in the corresponding industry×occupation cell in the PUMA. Details of this procedure are found in appendix B. Appendix A gives summary statistics and precise definitions of all variables.

4. Results

Basic results

Column 1 of Table 1 shows the baseline specification in full. This is a pooled cross-section OLS regression of self-reported happiness on log predicted PUMA earnings and individual controls. Individual level variables in this regression are averages of the main respondent and his or her spouse. Standard errors are corrected for clustered error terms at the PUMA level and the sample includes all NSFH respondents who are married or cohabiting in both waves. The first row shows that, controlling for own income and other own characteristics, predicted PUMA earnings have a significantly negative effect on self-reported happiness. In other words, individuals with richer neighbors report being less happy. As expected, own household income has a positive effect on happiness but its coefficient may be relatively small

because the regression includes other proxies for income, such as the value of one's home and a dummy variable for renting.[21] Usual working hours has an insignificant negative effect, unemployment status has a large and significant negative effect while a dummy for being out of the labor force has a significant positive effect on happiness.[22] The other demographic controls yield few surprising insights.

Column 2 shows the same regression using only data from the main respondent rather than averaging the respondent's data with that of the spouse. This regression confirms that the results are not sensitive to the averaging procedure used in the baseline regression though power increases moderately when the data of the main respondent and the spouse are averaged.[23]

The coefficient on own household income in the baseline regression is likely biased downwards due to measurement error in income and the inclusion of other income proxies. In specification 3, I try to get a more accurate estimate of the effect of own income by eliminating two other income proxies (home value and the dummy for renter) and by instrumenting log household income by the predicted household earnings, where the prediction based on the industry×occupation information of the respondent and the spouse and national earnings information (excluding the own state) by industry×occupation and time period from the CPS MORG.[24] Instrumenting results in an estimated effect of own household income on happiness that is about three times as large as the estimate in the baseline specification. Moreover, the estimate is now larger than the absolute value of the estimate of neighbors' earnings, though this difference is not statistically significant. The point estimates imply that if both own income and

[21] *Ln Value of home* has been demeaned. Hence, the dummy for renter is relative to a homeowner with a home of average value.
[22] *Ln Usual working hours* has been demeaned. Hence, the dummies for unemployment and out of the labor force are relative to an employed person working the average number of hours.
[23] I also stacked the data for the main respondent and the spouse, thus treating each as a separate observation in the regression. This yields essentially the same coefficients but the standard errors are smaller than those of the baseline regression.
[24] See appendix B for details on the construction of this predictor.

neighbors' earnings rise by the same percentage, a person would feel better off, indicating one's absolute economic situation matters for happiness in addition to one's relative position.

Could the results be spurious?

Table 2 investigates whether selection or omitted area characteristics could be driving the results. The first row of Table 2 replicates the baseline regression.

The second row includes individual-specific fixed effects, thus controlling for all time-invariant individual characteristics. The coefficient on neighbors' earnings remains negative and similar in magnitude though it is now only significant at the 10% level. This finding discounts the possibility that the cross-section results are driven by selection of people who are happier by nature into areas that are relatively poor. Of course, this specification does not rule out selection based on unobserved time-variant characteristics. The third row shows a regression that is identical to the baseline regression, except that a full set of state×wave fixed effects, a dummy for living in a metropolitan area and log metropolitan population size are added as controls. Both the coefficients on neighbors' earnings and on own income change little and remain statistically significant. Hence, the results are driven by variation in neighbors' earnings *within* states at each point in time. This specification thus rules out that the baseline results are spurious due to unobserved variables that operate at the state level or above, such as a poorer (southern) states having happier residents on average, for example because of unobserved better weather. One might worry that movers have something unobserved happen to them and that perhaps this unobserved factor causes the happiness to be inversely related to average neighbors' earnings. The fourth row tests this by showing the baseline regression excluding all respondents who

moved to a different PUMA. Again the coefficients on neighbors' earnings and own income are hardly affected, showing that the baseline results are not just driven by movers.

The regression in the fifth line is identical to the one in the fourth row, except that it includes individual fixed effects. Because the sample is limited to non-movers, the individual fixed effects also serve as PUMA fixed effects (i.e., any PUMA fixed effects would be absorbed by the individual fixed effects). Thus, the coefficient on neighbors' income in this regression is purely identified off of changes in neighbors' earnings that are solely due to different national trends in earnings in different industry×occupation cells. The individual fixed effects absorb any time-invariant individual characteristics as well as any effect correlated with the industry×occupation composition of each PUMA. Time-varying unobserved characteristics cannot affect one's neighbors' earnings because the sample is limited to non-movers. Unfortunately, the standard error in this specification is too large for this regression to provide any evidence on selection.

Could neighbors' earnings proxy for local price levels? In this case, the negative coefficient on neighbors' earnings would simply reflect that happiness falls as real incomes fall. Recall that neighbors' earnings are measured by *LnPumaEarnings*, which is a predictor of local earnings based on the *local* industry×occupation mix but *national* earnings data (excluding data from one's own state). Hence, local price variation would only be picked up by *LnPumaEarnings* to the extent it is correlated with the local industry×occupation mix. Moreover, we saw in row 3 that the estimates are robust to the inclusion of state×wave fixed effects, implying that only local price variation *within* states at a point in time could possibly be driving the results. Since migration within states is relatively easy, one would wonder what the source of such price variation is. If higher prices merely reflect better local amenities (such as

better schools or less crime), one shouldn't deflate incomes by local prices, because the higher local prices simply imply that one substitutes amenities for physical goods. Thus, in that case, higher local prices should not reduce well-being. On the other hand, if higher local prices reflect transportation costs or higher local production cost, local wages would need to rise proportionally to prevent an outflow of labor. In this case, individuals in an area with high local prices would earn more but not be any better off than similar individuals in an area with low local prices. Thus, if we don't measure a respondent's income by monetary variables (such as household income, home value) but instead use proxies that don't respond to local wage levels (national earnings in the respondent's industry ×occupation cell), then there would be no role for *LnPumaEarnings* to serve as a control for local prices. Hence, the coefficient on *LnPumaEarnings* should become insignificant if it were just spuriously picking up local price variation. The sixth row of table 2 estimates the baseline regression purged of any controls that proxy for the respondent's nominal income. The coefficient on *LnPumaEarnings* drops in size but remains negative and significant. This rules out that *LnPumaEarnings* is just picking up variation in local price levels.

Table 3 investigates the robustness of the baseline results. The first row again reproduces the baseline regression. One might be concerned that the results are driven by the somewhat complicated procedure used to predict *LnPumaEarnings.* The second row alleviates this concern; if anything, the estimate on *LnPumaEarnings* becomes more negative and more significant if we replace the predicted value by the actual value in 1989. Similarly, the third row shows that using log PUMA income instead of earnings yields similar results. The fourth row runs the baseline regression on all the observations in the balanced panel (rather than only the ones married in both waves). The coefficient on neighbors' earnings remains highly significant,

but drops somewhat in magnitude, giving a first indication that the estimates are primarily driven by the married subsample. This issue will be explored further in Table 7, discussed below. Could neighbors' earnings proxy for non-linearities in the effect of own income? This concern is ruled out by the fifth row, which shows that the estimate on neighbors' earnings hardly changes after the inclusion of a 5th-order polynomial in log household income. Since the outcome variable, self-reported happiness, is ordinal rather than cardinal, an OLS regression may not be appropriate. Specification six estimates the baseline regression using an ordered Probit and finds that the coefficient on neighbors' earnings remains negative and highly significant. Moreover, the ratio of the coefficient on neighbors' earnings to the coefficient on own income remains roughly constant.[25] Finally, the seventh row shows that the results are insensitive to replacing log own household income by the couple's log total earnings.

Do neighbors' earnings affect other outcomes?

Could an increase in neighbors' earnings merely change what individuals *define* as happy rather than their true underlying well-being? This concern is hard to rule out definitively, but using other outcome measures that may be less prone to shifts in definition yields some insights.

The first specification in Table 4 uses a measure of financial worries as an alternative outcome measure. If increases in neighbors' earnings reduce happiness because one cannot afford the goods that neighbors consume, one would expect respondents to have more financial worries. Financial worries are measured by the question "*How often do you worry that your total family income will not be enough to meet your family's expenses and bills?*" I find that respondents in areas with higher average earnings indeed have more financial worries after

[25] The coefficients in the ordered Probit turn out to be similar in magnitude to those in the baseline regression partly because the root mean square error of the baseline regression is 1.06 and thus close to unity, to which the error term in the latent model of the ordered Probit is normalized.

controlling for own income and other own characteristics. Because this question is less prone to a definition shift in response to neighbors' earnings, this finding alleviates the concern about a shifting definition in the happiness question.

One might also expect that a couple surrounded by neighbors earning more, would have more disagreements about material issues as their aspirations might be shaped by spending patterns of those around them (Stutzer, 2004). The regressions in specification 2 show that higher neighbors' earnings are significantly associated with more frequent disagreements about money, marginally significantly associated with more frequent disagreements about household tasks, but not significantly with the frequency of fights about the children, sex, in-laws or spending time together. It may be less clear why neighbors' earnings would be associated with disagreements about household tasks, though one can make up plausible stories to explain this finding.[26] Because the questions about open disagreements seem less prone to a shift in definition in response to neighbors' earnings, this finding also offers suggestive evidence that the estimated effect of neighbors' earnings on self-reported happiness is not solely due to a shift in the definition of happiness.

The third specification in Table 4 considers a measure of depression, which is the sum of the 12 items of the Radloff depression scale that are included in both waves of the survey. Since many of the items have more of an absolute definition (e.g. "having a poor appetite," "sleeping restlessly") or at least a definition for which it might be hard to use neighbors' behavior as a reference, the depression scale may be less prone to shifting definitions. On the other hand, depression and well-being, though correlated, are two distinct concepts and it is very well possible that increases in neighbors' earnings reduce true well-being without increasing

[26] For example, financial stress could reduce the room for contracting out household tasks or buying appliances that ease household chores, leading to more disagreements about household tasks.

depression. As the regression shows, neighbors' earnings have no significant impact on the depression index. Because the depression index is quite skewed, I also looked at the effect of neighbors' earnings on the probability of being in each part of the distribution of depression index. As specification four shows, higher neighbors' earnings significantly increase the probability of being in the top four quintiles of the depression index but do not significantly affect the probability of being in the top three, top two or top quintile of the depression index. Thus, the effect of neighbors' earnings seems to be limited to the bottom of the depression index distribution (i.e., only those furthest removed from being depressed come somewhat closer to being depressed). Overall, the findings with depression as an outcome variable only slightly alleviate concerns about shifting definitions of happiness, though depression might be a sufficiently different concept from well-being to pick up relative position effects.[27]

Since a large literature examines the effect of relative position on health outcomes, the fifth specification uses self-reported health status (relative to one's age group) as an outcome measure.[28] I find no significant relation between average neighbors' earnings and self-reported health. This finding, of course, does not rule out that such a relationship might exist, but it does not show up using my baseline specification.[29]

Mechanisms behind the association between neighbors' earnings and happiness

[27] In their study of the randomized Moving to Opportunity experiment, Kling et al. (2004) find that individuals moving to lower poverty census tracts report lower levels of psychological distress. Thus, for mental health measures such as depression or psychological distress the benefits associated with richer areas (such as lower crime) apparently outweigh any relative position effect.

[28] Self-reported health is measured by the question "*Compared with other people your age, how would you describe your health?*" with possible answers being *very poor, poor, fair, good* and *excellent*. Since the question about health *explicitly* asks respondents to compare themselves to other people of their age, this outcome cannot be used to address any concerns about a shifting definition of happiness.

[29] Many papers on relative position and health use relative deprivation (the income gap with those earning more) rather than average neighbors' income to measure status. In addition, my sample might be too small or the measure of self-reported health too noisy to pick up an effect.

One can think of overall self-reported happiness as being driven by one's satisfaction with various domains of life, such as one's family life, financial situation or friendships (Van Praag, Frijters and Ferrer-i-Carbonell, 2003). Only in wave 2, the NSFH asks respondents to rate their satisfaction with 11 such domains on a 7-point scale. Table 5 shows a regression of self-reported happiness on these 11 measures of satisfaction (but no other controls) to give the reader a sense of the relative importance of these satisfaction components for self-reported happiness. According to this regression, the top three predictors of self-reported happiness are satisfaction with family life, financial situation and sex life (in that order).

Table 6 explores which mechanisms underlie the relationship between happiness and neighbors' earnings by adding satisfaction measures as additional control variables. If the addition of a control variable reduces the magnitude of the coefficient on neighbors' earnings, some of the effect of neighbors' earnings apparently runs through that control variable. The first row of the table replicates the baseline regression for wave 2 because the satisfaction measures are only available in wave 2. Specification 2 shows the effects of including satisfaction measures as additional controls. None of the satisfaction measures alone can render the effect of neighbors' earnings insignificant, but the effect of neighbors' earnings is reduced most strongly if satisfaction with one's home, financial situation or amount of leisure time is included as a control. Simultaneously controlling for these three satisfaction measures makes the relationship between neighbors' earnings and happiness statistically and economically insignificant. Thus, apparently, higher neighbors' earnings reduce happiness for a large part through their effect on satisfaction with one's home, finances and leisure time. If neighbors' consumption patterns shape one's aspirations, it is not surprising that higher neighbors' earnings reduce one's satisfaction with one's financial situation or one's home. Perhaps less expected is that

satisfaction amount of leisure can account for part of the relationship between neighbors' earnings and happiness, but higher neighbors' earnings could reduce satisfaction with the amount of leisure time because one might cut back on the purchase of certain services (cleaning, yard, maintenance) in order to keep up with the neighbors in the consumption of more visible goods.

Even if status influences happiness, one would not expect it to affect happiness with aspects of life that are largely unaffected by comparisons with neighbors. Indeed, the remaining eight questions on life satisfaction cannot account for much of the relationship between neighbors' earnings and happiness, even though they include important general predictors of happiness such as satisfaction with family life and health (recall Table 5).[30] Even controlling for all eight simultaneously leaves the relationship between neighbors' earnings and happiness significant at the 5% level. Thus it seems that, by and large, neighbors' earnings affect happiness primarily through those components of happiness for which one would expect relative earnings to matter most.

Interaction effects

Table 7 investigates whether the relationship between neighbors' earnings and happiness operates across a range of demographic subgroups. Specification 1 shows that both females and males report being less happy if earnings in their area rise. Specification 2 shows that the happiness of individuals in various age ranges is negatively related to neighbors' earnings. Even though the coefficients on neighbors' earnings are not statistically significantly different from each other at the 10% level, the effect seems least strong for individuals aged 30 or younger,

[30] One might have expected satisfaction with present job to be influenced by neighbors' earnings. Perhaps this question measures job aspects (such as corporate culture) that are less subject to comparison with neighbors.

which may not be surprising because this group is probably less settled and therefore less inclined to compare themselves to their current neighbors.

Specification 3 finds that both those with more and with less education report lower levels of happiness when surrounded by richer neighbors. For each educational group, the point estimate is negative and the point estimates are not significantly different from each other at the 10 percent level. Specification 4 shows the relationship between local earnings and happiness across groups based on marital status. Though the hypothesis that the coefficients on neighbors' earnings are all equal to each other cannot be rejected at the 10% level, there seems to be little or no effect of neighbors' earnings on happiness for never-married individuals or those experiencing marital status transitions. This is not surprising because these individuals are likely to be less settled and thus less inclined to consider their neighbors as their reference group.

Might neighbors' earnings not matter by themselves but only affect people's happiness because they are correlated with the price of housing? In this case, we would expect to see a differential effect between renters (who dislike increases in the price of housing) and homeowners (most of whom like seeing the value of their home rise). Specification 5 shows that this is not the case. Or might neighbors' earnings somehow operate through the quality of schooling (e.g. richer neighbors leaving the public school system)? In this case, we should see different effects for those with and without children, which, as specification 6 shows, is not the case. Finally, specification 7 finds that the effect of neighbors' earnings seems to be somewhat stronger for relatively new residents (less than 7 years) than for longer-term residents, but the difference is not statistically significant.[31]

To investigate which parts of the happiness distribution are affected by neighbors' earnings, I create four dummy variables corresponding respectively to reporting a level of

[31] Looking at finer partitions of duration of residence did not reveal any interesting patterns.

happiness of at least 4, 5, 6 and 7.[32] Regressions (not-reported) of these dummy variables on neighbors' earnings and the remaining controls of the baseline regression yield a significantly negative coefficient on neighbors' earnings in all four cases, showing that the effect of neighbors' earnings operates throughout the happiness distribution. In additional unreported regressions, I also examine whether the relation between neighbors' earnings and happiness displays non-linearities but I find no evidence of such effects. The point estimates of neighbors' earnings on self-reported happiness are similar and not significantly different for those above mean earnings in their PUMA compared to those below. Other specifications show that the point estimates on a quadratic term in neighbors' earnings or the interaction between own household income and neighbors' earnings are both small and statistically insignificant.

If the neighbors' earnings reduce self-reported happiness because people engage in social comparisons, we would expect a stronger effect for those with more contacts with their neighbors. Table 8 investigates this hypothesis. The NSFH asks all main respondents about the frequency of social interactions with neighbors, relatives, friends living outside the neighborhood and people they work with. Each specification in Table 8 compares the effect of neighbors' earnings for those who have infrequent social contacts (less than once a month) to those with frequent social contacts with the type of person indicated, controlling for the direct effect of social interactions.[33] The table shows that the effect of neighbors' earnings is significantly stronger for those who socialize frequently with neighbors but not for those who socialize more frequently with relatives, friends outside the neighborhood or people they work with. These

[32] I don't partition the happiness distribution at 3 and 2 because only 3.5% and 1.0% of the observations respectively report levels of happiness lower than these values.

[33] The direct effect is positive in all cases but only significant for socializing with neighbors or relatives. Of course, the direct effect should not be interpreted causally.

findings are consistent with what one would expect if social comparisons with neighbors partly determine people's happiness.

Table 9 explores whether individuals predominantly compare themselves with neighbors who have a similar level of education. For each PUMA, I constructed predicted earnings for PUMA residents with a college degree and for those without.[34] Specification 1 shows the regression of self-reported happiness on the interaction of these two measures of local earnings with own educational attainment. The regression also includes all the controls of the baseline regression. Though standard errors are large, the point estimates indicate that the happiness of those without a college degree declines with the earnings of neighbors without a college degree but is insensitive to the earnings of neighbors with a college degree. Similarly, the happiness of those with a college degree declines with the earnings of neighbors with a college degree but is relatively insensitive to the earnings of neighbors without one. Moreover, for both those with and without a college degree, the point estimates on the earnings of one's own education group are similar (the diagonal coefficients are -0.28 and -0.40) as are the point estimates on the earnings of the other group (the off-diagonal coefficients are 0.02 and -0.07). In specification 2, I restrict the diagonal coefficients to be the same and the off-diagonal coefficients to be the same. Now there is a marginally significant relationship between happiness and earnings of neighbors with the same level of education but not between happiness and earnings of neighbors with a different level of education, though the test of equality of all coefficients is not rejected at the 10% level. Though the results in table 9 should be treated with caution because of low statistical power, they are suggestive of respondents comparing themselves mostly with neighbors who are similar in terms of educational attainment.

[34] This measure is constructed the same way as the predicted Ln PUMA earnings measure used in the baseline regression, except that everything is done separately by level of education.

5. Conclusion

This paper shows that individuals' self-reported happiness is negatively affected by the earnings of others in their area. By looking at alternative outcome measures, such as frequency of financial worries, I provide evidence that this finding is not simply an artifact of the way people report happiness. I investigate the concern that the finding could be driven by omitted variables, but find no evidence of selection in a number of specification tests. Though the mechanism by which increases in neighbors' earnings reduce happiness is hard to identify precisely, I provide suggestive evidence that interpersonal preferences are likely to be responsible for them. Increased neighbors' earnings by and large reduce satisfaction with material (rather than immaterial) aspects of one's life and have the strongest negative effect on happiness for those who socialize more in their neighborhood. I therefore conclude that the negative effect of neighbors' earnings on well-being is real and that it is most likely caused by a psychological externality, i.e. people having utility functions that depend on relative consumption in addition to absolute consumption.

The size of the effect is economically meaningful. An increase in neighbors' earnings and a similarly sized decrease in own income each have roughly about the same negative effect on well-being. This suggests that an increase in own income leads to a negative externality on neighbors' well-being that is of the same order of magnitude as the positive effects on own well-being. Unless one chooses to disallow these negative externalities on the ground that they appear to stem from an interpersonal preference component that is morally questionable, externalities of this size can in principle substantially affect the optimal policies dealing with income taxation, consumption taxation and residential sorting.

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Appendices:

Appendix A: Variable Definitions and Summary Statistics

The baseline sample consists of individuals who are married or cohabitating in both waves of the NSFH and have non-missing information on own household income. Unless otherwise noted, the variables in the baseline sample consist of the average of the non-missing values of that variable for the main respondent and his or her spouse. All dollar amounts are converted to 1982-84 real dollars using the CPI-U from Bureau of Labor Statistics.

Table A1: Variable Definitions

Variable name	Description
Self-reported happiness	The answer to the question: *"Next are some questions about how you see yourself and your life. Taking things all together, how would you say things are these days?*" where 1 denotes "*very unhappy"* and 7 denotes "*very happy*". Values in between did not have explicit labels.
Ln PUMA earnings (predicted)	Log average predicted pre-tax earnings of all non-institutionalized working age persons ($16 \leq age < 65$) with non-missing industry and occupation codes living in the PUMA of the NSFH respondent. See Appendix B for details.
Ln PUMA earnings (actual)	Log average actual pre-tax earnings of all non-institutionalized working age persons ($16 \leq age < 65$) with non-missing industry and occupation codes living in the PUMA of the NSFH respondent.
Ln PUMA income (actual)	Log average actual pre-tax per-capita income of all non-institutionalized persons living in the PUMA of the NSFH respondent.
Ln PUMA earnings (pred.)|no college, Ln PUMA earnings (pred.)| college	Same as *Ln PUMA earnings (predicted)* described above, except that the prediction is done separately for people in the PUMA with and without a college degree. See Appendix B for details
Ln Household income	Log household total pre-tax income constructed by the NSFH using information from both the main respondent and his or her spouse. NSFH variable names: "IHTOT2" in wave 1 and "MUHHTOT" in wave 2.
Ln Predicted earnings of R and spouse	Log pre-tax earnings of the main respondent and his/her spouse based on the industry×occupation codes of the respondent and the spouse and the earnings by industry×occupation×time-period from the CPS MORG. See Appendix B for details.
Ln Actual earnings of R and spouse	Log pre-tax earnings of the main respondent and his/her spouse.
Ln Value of home	Log current value of the respondent's home if the respondent is a homeowner. Answer to the question "*How much do you think your home would sell for now?*".
Renter	Dummy for renting one's home.
Ln Usual working hours	Log total usual working hours in one's main and secondary job. Non-working individuals are assigned the mean log working hours to make the dummies for unemployment and not-in-the-labor force more easily interpretable.
Unemployed	Dummy for being unemployed defined as not currently employed and having looked for work during the past 4 weeks.
Not in the labor force	Dummy for those neither currently employed nor unemployed.
Non-Hispanic white; Black; Hispanic; Asian; Other race or race n/a	Answer to the question "*Which of the groups on this card best describes you? 01-Black; 02-White-not of Hispanic origin; 03-Mexican American; Chicano, Mexicano; 04-Puerto Rican; 05-Cuban; 06-Other Hispanic; 07-American Indian; 08-Asian; 09-Other*". Categories 3,4,5 and 6 are combined into "*Hispanic*" and categories 7,9 and no answer are combined into "*Other race or race n/a"*.
Married or cohabiting; Separated; Divorced;	Cohabiting individuals (even if separated, divorced or widowed) are coded into the *Married or cohabitating* category.

Widowed; Never married; Missing marital status	
Educational attainment (years)	Years of completed education at the time of the wave 1 interview as defined by the NSFH constructed variable "EDUCAT".
Religious affiliation dummies (13)	Answer to the question: "*What is your religious preference? (IF PROTESTANT, ASK): What specific denomination is that?"*. I grouped the 65 possible responses to this question into 13 major categories.
Wave	The wave of the NSFH data. Wave 1 was fielded between March 1988 and May 1989 with most of the interviews conducted in the summer of 1988. Wave 2 was fielded between July 1992 and July 1994 with most of the interviews conducted in 1993.
Spousal record exists	Dummy if a spouse was interviewed.
Frequency of financial worries	The answer to the question: *"How often do you worry that your total family income will not be enough to meet your family's expenses and bills. Would you say: 1-Almost all the time; 2-Often; 3-Once in a while; 4-Hardly ever; 5-Never."* This question was only asked in wave 2.
Frequency of open marital disagreements about [household tasks; money; spending time together; sex; in-laws; the children]:	The answer to the question: "*The following is a list of subjects on which couples often have disagreements. How often, if at all, in the last year have you had open disagreements about each of the following [household tasks; money; spending time together; sex; in-laws; the children]: 01-Never; 02-Less than once a month; 03-Several times a month; 04-About once a week; 05-Several times a week; 06-Almost everyday*."
Depression Index (based on Radloff)	Twelve items from the Radloff depression index were asked in both waves: "*Next is a list of the ways you might have felt or behaved during the past week. On how many days during the past week did you: [Feel bothered by things that usually don't bother you?; Not feel like eating; your appetite was poor?; Feel that you could not shake off the blues, even with help from your family or friends?; Have trouble keeping your mind on what you were doing?; Feel depressed?; Feel that everything you did was an effort?; Feel fearful?; Sleep restlessly?; Talk less than usual?; Feel lonely?; Feel sad?; Feel you could not get going?] (number of days)."* The answer to each question is the number of days (0-7) on which the condition applied. The depression index is the sum of the answers to these 12 questions. This variable is only based on the responses by the main respondent because the depression items were not asked of the spouse in the first wave.
Self-reported health status	The answer to the question: *"Compared with other people your age, how would you describe your health? 1-Very poor; 2-Poor; 3-Fair; 4-Good; 5-Excellent"*
Satisfaction with [home; neighborhood; city or town; financial situation; amount of leisure; health; physical appearance; friendships; sex life; family life; present job]	The answer to the question: "*Overall, how satisfied are you with... [Your home?; Your neighborhood?; Your city or town?; Your financial situation?; The amount of leisure time that you have?; Your health?; Your physical appearance?; Your friendships?; Your sex life?; Your family life?; Your present job?]* where 1 denotes "*very dissatisfied"* and 7 denotes "*very satisfied*". Values in between did not have explicit labels. These questions were only asked in wave 2.

Table A2: Summary Statistics

	Whole Sample N=23,010			Baseline Sample Only N=8944		
Variable:	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Self-reported happiness	5.346	1.38	19795	5.553	1.08	8944
Ln PUMA earnings (predicted)	9.824	0.27	22979	9.843	0.26	8944
Ln PUMA earnings (actual)	9.820	0.24	22979	9.835	0.24	8944
Ln PUMA income (actual)	9.471	0.30	22979	9.492	0.29	8944
Ln PUMA earnings (pred.)|no college	9.451	0.15	22979	9.471	0.14	8944
Ln PUMA earnings (pred.)| college	10.904	0.16	22979	10.917	0.15	8944
Ln Household income	9.886	1.10	19037	10.346	0.75	8944
Ln Predicted earnings of R and spouse	9.825	1.12	16984	10.197	0.92	8125
Ln Actual earnings of R and spouse	9.804	0.74	19676	10.131	0.62	8091
Ln Value of home	10.871	0.88	12408	10.979	0.82	6788
Renter	0.409	0.49	23010	0.217	0.41	8944
Ln Usual working hours	3.692	0.44	13416	3.694	0.39	7255
Unemployed	0.029	0.17	23010	0.019	0.10	8944
Not in the labor force	0.342	0.47	23010	0.317	0.36	8944
Female	0.605	0.49	23010	n/a	n/a	n/a
Age	44.669	16.91	23010	43.282	14.03	8944
Non-Hispanic white	0.734	0.44	23010	0.828	0.36	8944
Black	0.179	0.38	23010	0.091	0.28	8944
Hispanic	0.073	0.26	23010	0.059	0.22	8944
Asian	0.009	0.10	23010	0.010	0.09	8944
Other race or race n/a	0.005	0.07	23010	0.012	0.08	8944
Married or cohabiting	0.601	0.49	23010	1.000	0.00	8944
Separated	0.041	0.20	23010	0.000	0.00	8944
Divorced	0.121	0.33	23010	0.000	0.00	8944
Widowed	0.104	0.31	23010	0.000	0.00	8944
Never married	0.133	0.34	23010	0.000	0.00	8944
Missing marital status	0.0002	0.01	23010	0.000	0.00	8944
Educational attainment (years)	12.518	3.11	22924	12.997	2.62	8940
Dummy for college or more	0.178	0.38	23010	0.232	0.37	8944
Ln Household size	0.914	0.56	23010	1.139	0.39	8944
No religion	0.084	0.28	23010	0.084	0.23	8944
Catholic	0.240	0.43	23010	0.242	0.40	8944
Jewish	0.020	0.14	23010	0.023	0.14	8944
Baptist	0.235	0.42	23010	0.201	0.37	8944
Episcopalian	0.020	0.14	23010	0.021	0.13	8944
Lutheran	0.054	0.23	23010	0.063	0.22	8944
Methodist	0.097	0.30	23010	0.106	0.28	8944
Mormon	0.022	0.15	23010	0.028	0.16	8944
Presbyterian	0.033	0.18	23010	0.041	0.18	8944
Congregational	0.017	0.13	23010	0.021	0.12	8944
Protestant, no denomination	0.043	0.20	23010	0.030	0.13	8944
Other Christian	0.114	0.32	23010	0.118	0.29	8944
Other religions / missing	0.022	0.15	23010	0.023	0.12	8944

Wave	1.435	0.50	23010	1.538	0.50	8944
Spousal record exists	0.503	0.50	23010	0.893	0.31	8944
Additional variables for table 4						
Frequency of financial worries (wave 2 only)	3.093	1.21	9713	2.997	1.02	5001
Frequency of open marital disagreements about:						
money	2.069	1.15	13019	2.079	0.99	9125
the children	2.097	1.27	10277	2.366	1.67	8664
household tasks	1.992	1.07	13026	2.022	0.93	9122
sex	1.748	1.11	12700	1.782	0.95	9044
spending time together	1.997	1.29	12974	1.998	1.07	9120
in-laws	1.494	0.88	12765	1.568	1.50	9156
Depression Index (based on Radloff)	14.913	16.68	21511	12.215	14.49	8782
Self-reported health status	3.949	0.86	21992	4.021	0.65	9173
Additional variables for tables 5 & 6 (wave 2 only)	Observations in wave 2: 10004			Baseline observations in wave 2: 5067		
Satisfaction with family life	5.749	1.42	9682	5.881	1.06	4806
Satisfaction with financial situation	4.525	1.71	9658	4.780	1.40	4800
Satisfaction with sex life	5.011	1.87	9232	5.276	1.39	4766
Satisfaction with home	5.491	1.52	9742	5.622	1.18	4806
Satisfaction with health	5.384	1.54	9694	5.436	1.16	4805
Satisfaction with present job	5.161	1.68	7036	5.301	1.36	4150
Satisfaction with amount of leisure	4.558	1.80	9685	4.514	1.47	4805
Satisfaction with friendships	5.697	1.35	9688	5.676	1.03	4803
Satisfaction with city or town	5.317	1.54	9643	5.383	1.23	4803
Satisfaction with physical appearance	5.144	1.47	9692	5.118	1.12	4805
Satisfaction with neighborhood	5.420	1.60	9719	5.532	1.28	4805

Appendix B: Construction of Predictor for PUMA- and Industry×Occupation Earnings

This appendix provides a detailed description of the construction of both the PUMA-level earnings predictor and the predictor of average earnings at the industry×occupation level. The construction of these predictors is described in seven steps.

Step 1: Creation of a common set of industry×occupation codes

The industry and occupation codes in the first wave of the NSFH as well as in the 1987 and 1988 CPS MORG files are 3-digit 1980 Census codes, while 3-digit 1990 Census codes were used in the second NSFH wave, the 1992-1994 CPS MORG files and (obviously) the 1990 Census PUMS. I create a crosswalk to make these two sets of codes compatible. In general, the 1980 and 1990 Census industry and occupation classification is very similar. In a few cases, I need to merge several 3-digit occupations or industries together in order to make the codes comparable. I exclude the active-duty military. The crosswalk transforms 231 industry codes from the 1980 Census and 236 industry codes from the 1990 Census into a set of 225 unique and comparable industry codes. Similarly, it transforms 503 occupation codes from the 1980 Census and 501 occupation codes from the 1990 Census into a set of 496 unique and comparable occupation codes.

Step 2: Calculation of average industry×occupation×time-period earnings from CPS MORG:

I use the NBER CPS labor abstracts to obtain usual weekly earnings ("earnwke") by industry×occupation for the years 1987-88 and 1992-94 for all employees (self-employed individuals are excluded because their earnings data is often unreliable). To ensure compatibility over time, all earnings are deflated by the 1982-84 CPI-U, all topcoded earnings are replaced by twice the value of the topcode[35], and the sum of weights is made identical across years. Step 2 yields two variables: $LnEarn_1$, average log annual earnings by industry×occupation cell in 1987-1988, and $LnEarn_2$, average log annual earnings by industry×occupation cell in 1992-1994. These variables differ by state because data from the own state is excluded in calculating the average.

Step 3: Prediction of average earnings by industry×occupation×time-period cell

Because many industry×occupation cells have very few observations or are empty in the CPS data but are non-empty in the NSFH or Census PUMS, I use a linear regression to predict earnings in each industry×occupation cell in each time period. I regress $LnEarn_1$ on a full set of 3-digit industry dummies and a full set of 3-digit occupation dummies where each industry×occupation cell is weighted by the weighted number of observation in that cell in the MORG. I run this regression separately for each state (because of the exclusion of the own state information). On average, these regressions have about 20,000 observations. The regressions yield the earnings predictor $LnEarnPred_1$. Similar regressions for wave 2 yield the earnings predictor $LnEarnPred_2$.

Step 4: Taking a weighted average between actual and predicted earnings

The benefit of the predictor described in step 3 is that it yields predicted earnings for industry×occupation×time-period cells that are empty in the CPS MORG and that it increases the

[35] This is the expectation of a topcoded earnings variable if the tail of the earnings distribution follows a Pareto distribution with a parameter of 2, which seems to be the case empirically (Saez, 2001).

precision of the earnings estimate for industry×occupation×time-period cells with very few observations (in wave 1 (2), 75% of the industry×occupation cells have 8 (9) or fewer observations in them). However, by not allowing industry×occupation interactions, the regression may not be the best predictor of earnings for cells with a large number of observations. To balance these two concerns, I take a weighted average, $LnEarnHat_t$, between predicted earnings and actual earnings, where the weight depends on n, the number of observations in that cell. Specifically, for wave 1:

$$LnEarnHat_1 = \left(\frac{n_1^{eff}}{n_1^{eff}+n}\right) LnEarnPred_1 + \left(\frac{n}{n_1^{eff}+n}\right) LnEarn_1$$

and similarly for wave 2. The parameter, n_t^{eff} or the relative weight on predicted earnings is estimated by a non-linear least squares regression of log average earnings from the PUMS on the left hand side of the equation above, where industry×occupation cells are weighted by the weighted number of observations in them in the PUMS. This yields n^{eff} of 22.9 and 25.1 for wave 1 and wave 2 respectively. The R^2 of these regressions are 0.74 and 73 respectively. The variable $LnEarnHat_t$ is merged to the NSFH data (based on the wave of the survey and the respondent's industry, occupation and state) and is the instrument for own household income used in specification 3 of table 1.

Step 6: Prediction of PUMA-level earnings

Using data from the 1990 Census 5% PUMS, I assign to each non-institutionalized person aged 16-65 the level of the predicted earnings, exp(*LnEarnHat*), based on that person's industry, occupation and state. I do this both for predicted earnings in 1987-88 and in 1992-94. Next, I calculate the average predicted level of earnings for each PUMA for each time period. The log of the average predicted level of earnings for each time period and PUMA is the variable *LnPumaPred.*

Step 7: Rescaling of $LnPumaPred_t$

To test the predictive power of $LnPumaPred_t$, I regress both $LnPumaPred_1$ and $LnPumaPred_2$ on log actual PUMA-level earnings in 1989 (from the 1990 Census) for the sample of 1315 PUMAs that occur in the NSFH data.[36] Though both regressions have a high R^2 (0.846 and 0.861 respectively), the coefficient on log actual 1989 PUMA level earnings is only around a half. Thus, while the predictor apparently does a good job ranking PUMAs by earnings, it underestimates earnings differences. To correct for this underprediction, I multiply each predictor by 2.19. After this rescaling, a regression of the average of $LnPumaPred_1$ and $LnPumaPred_2$ on log actual PUMA-level earnings in 1989 yields a coefficient of exactly one. This rescaling reduces the coefficient on PUMA-level earnings as an independent variable by a factor of about two but it of course does not affect the statistical significance of any of the results in the paper. In the rest of the paper, I refer to the rescaled predictor of PUMA-level earnings as *Ln Puma Earnings (predicted)* or, when there is no risk of confusion, *Ln Puma Earnings* for short.

[36] Some of these PUMAs do not contain NFSH *main* respondents but only secondary NSFH respondents (such as ex-spouses). Hence, there are more PUMAs in this sample than in the baseline sample described in the data section.

Validation using NSFH data

As a check on both the PUMA-level and industry×occupation-level predictors of earnings, I regress log earnings of NSFH respondents (with earnings of at least $100/year) on these predictors. The first row shows that the predictor of PUMA level earnings is highly significant, has a coefficient of about 0.75 and explains about 3.5% of the individual-level earnings variation. The second row shows that results are similar if we use the log of average actual earnings in the PUMA of the respondent in 1989. This shows that the use of predicted rather than actual earnings does not entail a huge loss of predictive power. The third, fourth and fifth row show that the results are similar for each wave and hold up if state fixed effects are included.

Rows (6) through (10), repeat these checks for the measure of industry × occupation earnings constructed in step 5 above. This measure is highly significant and has a coefficient reasonably close to one in all cases. Row (7) shows that results remain similar if we instead use log average earnings in 1989 by industry×occupation cell from the 1990 Census. This confirms that results are not driven by any peculiarities of my procedure of creating the measure for industry×occupation earnings.

Table A2: Validation of Earnings Predictor by PUMA and by Industry × Occupation

Dependent variable: *Ln Respondent's Real Earnings*

	Independent variable	Comment	Coefficient	S.E.	Adj. R^2	N
(1)	*Ln PUMA earnings (predicted)*	baseline	0.751	0.04	0.0341	14845
(2)	*Ln PUMA earnings (actual, 1989)*	actual 1989, not predicted	0.931	0.05	0.0418	14845
(3)	*Ln PUMA earnings (predicted)*	only wave 1	0.702	0.05	0.0320	7899
(4)	*Ln PUMA earnings (predicted)*	only wave 2	0.852	0.05	0.0400	6946
(5)	*Ln PUMA earnings (predicted)*	including state fixed effects	0.676	0.04	0.0434	14845
(6)	*Ln Industry×occupation earnings*	baseline	0.859	0.02	0.2732	13980
(7)	*Ln Census Industry×occupation earnings in 1989*	Average 1989 earnings by ind.×occ. from 1990 Census	0.721	0.02	0.2513	12541
(8)	*Ln Industry×occupation earnings*	only wave 1	0.852	0.02	0.2754	7644
(9)	*Ln Industry×occupation earnings*	only wave 2	0.854	0.03	0.2622	6336
(10)	*Ln Industry×occupation earnings*	including state fixed effects	0.849	0.02	0.2836	13980

Note: In specifications (1)-(5), standard errors are corrected for clustering at the PUMA level while in specifications (6)-(10) they are adjusted for clustering at the industry × occupation level.

Table 1: Baseline Regression

Dependent variable: *Self-reported happiness*	(1) Baseline		(2) Only main Respondent		(3) IV for own income	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Ln PUMA earnings (predicted)	**-0.208****	**0.052**	**-0.249****	**0.069**	**-0.279****	**0.067**
Ln Household Income	0.121**	0.020	0.110**	0.024	0.365**	0.100
Ln Value of home	0.052**	0.021	0.051**	0.024		
Renter	-0.182**	0.032	-0.222**	0.038		
Ln Usual working hours	-0.068	0.044	-0.111**	0.036	-0.137**	0.052
Unemployed	-0.428**	0.124	-0.255**	0.114	-0.352**	0.148
Not in the labor force	0.149**	0.043	0.066	0.043	0.252**	0.065
Female			-0.046	0.034		
Age spline (effect p.y. on segment 19-35)	-0.018**	0.004	-0.019**	0.005	-0.020**	0.005
Age spline (effect p.y. on segment 35-60)	0.002	0.002	0.002	0.003	0.001	0.002
Age spline (effect p.y. on segment 60 plus)	0.013**	0.004	0.009*	0.005	0.018**	0.005
White(omitted)						
Black	-0.060	0.050	-0.012	0.060	-0.054	0.052
Hispanic	0.278**	0.063	0.189**	0.074	0.286**	0.067
Asian	-0.067	0.117	-0.150	0.129	-0.045	0.117
Other race / ethnicity	-0.136	0.216	0.085	0.405	-0.093	0.210
Years of education	0.011	0.007	0.010	0.007	-0.007	0.012
Ln Household size	-0.166**	0.038	-0.176**	0.044	-0.180**	0.040
Catholic (omitted)						
No religion	0.170**	0.058	-0.155**	0.061	-0.162**	0.058
Jewish	-0.310**	0.098	-0.288**	0.112	-0.314**	0.098
Baptist	0.130**	0.041	0.058	0.045	0.147**	0.041
Episcopalian	-0.063	0.091	-0.008	0.097	-0.066	0.092
Lutheran	0.003	0.055	-0.089	0.064	0.003	0.056
Methodist	0.048	0.045	0.063	0.051	0.049	0.045
Mormon	0.041	0.079	-0.037	0.088	0.048	0.085
Presbyterian	-0.013	0.073	-0.103	0.077	-0.008	0.072
Congregational	0.040	0.095	-0.055	0.103	0.072	0.098
Protestant, no denomination	0.042	0.103	0.035	0.078	0.065	0.104
Other Christian	0.105**	0.047	0.025	0.054	0.122**	0.049
Other religions / missing	0.041	0.104	0.070	0.121	-0.070	0.108
Adjusted R^2	0.0371		0.0238		…	
Number of observations	8944		8023		8944	

Notes: Robust standard errors adjusted for clustering on PUMAs (1000 clusters in specifications 1 and 3; 974 clusters in specification 2). *Ln usual hours* and *Ln home value* are demeaned. All regressions also include dummy variables for independent variables with missing values. *Self-reported happiness* is measured on a scale of 1 to 7, with 7 representing "very happy." The sample consists of respondents of NSFH waves 1 and 2 that are married or cohabiting in both waves. In specifications (1) and (3) the variables are the average of the respondent's value and that of his or her spouse. In specification (3), *ln household income* is instrumented by predicted ln household earnings where predicted earnings are based on the industry × occupation of the respondent and his or her spouse.

Table 2: Testing for Selection

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) Baseline	-0.208**	0.052	0.121**	0.020	0.0371	8944
(2) Individual Fixed Effect	-0.232*	0.135	0.053	0.052	0.3102	8944
(3) Including State×Wave fixed effects, a dummy for metropolitan areas and log metropolitan area population	-0.189**	0.065	0.119**	0.020	0.0391	8944
(4) Observations remaining in same PUMA	-0.204**	0.063	0.129**	0.024	0.0381	6894
(5) Observations remaining in same PUMA and individual fixed effects	0.212	1.421	0.047	0.066	0.3170	6894
(6) Using predicted household income instead of household income and home value	-0.123**	0.050	…	…	0.0320	8944

Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in table 1, column 1. Whenever individual fixed effects are included, spousal variables are only used if the identity of the spouse remains the same in both waves.

Table 3: Robustness Checks

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) Baseline	-0.208**	0.052	0.121**	0.020	0.0371	8944
(2) 1989 <u>*actual*</u> *Ln PUMA earnings* as control (instead of predicted *Ln PUMA earnings*)	-0.266**	0.057	0.124**	0.020	0.0377	8944
(3) 1989 *Ln PUMA p.c.* <u>*income*</u> as control (instead of predicted *Ln PUMA earnings*)	-0.204**	0.052	0.123**	0.020	0.0372	8944
(4) Full balanced panel sample (including non-married individuals)	-0.155**	0.042	0.121**	0.017	0.0651	15568
(5) Controlling for 5th order polynomial in *Ln household income*	-0.212**	0.052	0.160**	0.030	0.0375	8944
(6) Ordered probit	-0.209**	0.051	0.110**	0.020	0.0107	8944
(7) *Ln earnings of R and spouse* as control (instead of *Ln HH income*)	-0.195**	0.052	0.082**	0.017	0.0370	8944

Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in table 1, column 1. The terms in the polynomial in specification 5 are demeaned. Hence, the coefficient on the first term (reported in the table) is the slope of *Ln household income* for someone with mean *Ln household income*. The regression in specification (4) also included 4 marital status dummies as controls.

Table 4: Other Outcome Measures

Dependent variable:	*Ln PUMA earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) *Financial worries*	0.221**	0.063	-0.388**	0.034	0.2199	5001
(2) Frequency of open disagreements about:						
a. money	0.129**	0.044	-0.104**	0.018	0.2261	9125
b. the children	0.096	0.062	-0.034	0.029	0.0781	8522
c. household tasks	0.080*	0.044	-0.049**	0.015	0.1865	9122
d. spending time together	0.059	0.055	-0.049**	0.020	0.1484	9120
e. sex	0.053	0.044	-0.046**	0.016	0.1481	9044
f. in-laws	0.020	0.051	-0.046**	0.025	0.0743	9096
(3) Depression Index						
Index (sum of 12 Radloff items)	-0.065	0.764	-1.334**	0.251	0.0654	8782
(4) Distribution of the Depression Index						
a. Dummy for being in the top quintile of the depression index	-0.015	0.023	-0.037	0.007	0.0495	8782
b. Dummy for being in the top two quintiles of the depression index	0.011	0.025	-0.030	0.008	0.0517	8782
c. Dummy for being in the top three quintiles of the depression index	0.012	0.023	-0.017	0.009	0.0470	8782
d. Dummy for being in the top four quintiles of the depression index	0.067**	0.018	-0.003	0.007	0.0367	8782
(5) *Health status relative to age group*	-0.008	0.034	0.095**	0.013	0.1489	9173

Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in table 1, column 1 except for the regressions in specification 2, which also control for self-reported quality of relationship with spouse. The variable *Financial worries* exists only in wave 2 and is the answer to the question "How often do you worry that your total family income will not be enough to meet your family's expenses and bills?" where 1 corresponds to "never" and 5 to "almost all the time." The frequency of open disagreements is measured on a scale of 1 ("never") to 6 ("almost every day"). The depression index is the sum of the 12 Radloff items that appear in both waves of the NSFH. Each item is the number of days in the past week that the respondent felt or experienced a symptom related to depression. Examples of such symptoms are "sleeping restlessly," "talking less than usual," and "feeling sad." Self-reported health status is the answer to the question "Compared with other people your age, how would you describe your health?", where 1 corresponds to "very poor" and 5 to "excellent." Specifications 3 and 4 only use data from the main respondent because the depression questions were only asked of the main respondent in all waves.

Table 5: Components of Happiness

Dependent variable:
Self-reported happiness

	Coeff.	S.E.
satisfaction with family life	0.278**	0.019
satisfaction with financial situation	0.144**	0.013
satisfaction with sex life	0.098**	0.011
satisfaction with home	0.092**	0.016
satisfaction with health	0.088**	0.019
satisfaction with present job	0.053**	0.014
satisfaction with amount of leisure time	0.039**	0.013
satisfaction with friendships	0.042**	0.018
satisfaction with city or town	0.022	0.015
satisfaction with physical appearance	-0.015	0.018
satisfaction with neighborhood	-0.024	0.015
R^2	0.3989	
Number of observations	4808	

Robust standard errors are adjusted for clustering at the PUMA level. Not reported but included in the regression are dummies variables for missing values of the satisfaction questions. The satisfaction variables are answers to the question; "Overall, how satisfied are you with... X," where X is "your family life," "your financial situation," etc. The answers are recorded on a 7-point scale with 1 corresponding to "very dissatisfied" and 7 corresponding to "very satisfied".

Table 6: Mechanisms

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) Baseline (wave 2 only)	-0.280**	0.072	0.160**	0.031	0.0298	4808
(2) Additional controls for *Satisfaction with* (wave 2 only):						
a. *home*	-0.139**	0.064	0.138**	0.030	0.1768	4808
b. *financial situation*	-0.146**	0.063	-0.038	0.029	0.1918	4808
c. *amount of leisure time*	-0.165**	0.066	0.130**	0.030	0.1340	4808
d. *sex life*	-0.180**	0.062	0.120**	0.029	0.2171	4808
e. *present job*	-0.190**	0.068	0.111**	0.030	0.1165	4808
f. *friendships*	-0.198**	0.064	0.142**	0.029	0.1635	4808
g. *health*	-0.203**	0.063	0.104**	0.029	0.1556	4808
h. *family life*	-0.205**	0.059	0.140**	0.027	0.2974	4808
i. *physical appearance*	-0.237**	0.065	0.141**	0.030	0.1305	4808
j. *neighborhood*	-0.241**	0.068	0.152**	0.030	0.0916	4808
k. *city or town*	-0.307**	0.065	0.149**	0.031	0.0946	4808
l. Top 3 *(home, finance, leisure)*	-0.058	0.058	0.011	0.028	0.2663	4808
m. Bottom 8 *(sex – city or town)*	-0.139**	0.055	0.094**	0.026	0.3630	4808

Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in table 1, column 1.

Table 7: Interactions With Respondent Characteristics

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) Gender					0.0236	8023
Male	-0.210**	0.093	0.114**	0.033		[3636]
Female	-0.283**	0.092	0.107**	0.033		[4387]
(2) Age					0.0376	8944
age ≤ 30	-0.089	0.114	0.086**	0.042		[1660]
30 < age ≤ 40	-0.229**	0.089	0.105**	0.039		[2943]
40 < age ≤ 60	-0.250**	0.090	0.172**	0.034		[3000]
60 < age	-0.225	0.151	0.099**	0.045		[1342]
(3) Education					0.0377	8944
Missing	-0.540	0.340	0.018	0.130		[291]
High school dropout	-0.203	0.189	0.130**	0.049		[1401]
High school degree	-0.254**	0.120	0.178**	0.037		[3263]
Some college	-0.101	0.130	0.088*	0.049		[1915]
College degree or more	-0.195**	0.096	0.052	0.040		[2075]
(4) Marital status transitions					0.0536	14500
Remains married or cohabiting	-0.200**	0.067	0.120**	0.023		[8023]
Remains divorced or separated	-0.392**	0.154	0.13**	0.048		[1524]
Remains widowed	-0.159	0.180	0.074	0.058		[1032]
Remains never married	-0.116	0.179	0.116**	0.051		[995]
Marital status change	-0.019	0.093	0.125**	0.034		[2926]
(5) Home ownership					0.0376	8944
Rents	-0.191*	0.102	0.185**	0.036		[1939]
Owns	-0.235**	0.060	0.092**	0.024		[7005]
(6) Presence of child(ren) aged ≤ 18					0.0372	8944
No child(ren) present in HH	-0.212**	0.077	0.127**	0.025		[3853]
Child(ren) present in HH	-0.206**	0.067	0.111**	0.028		[5091]
(7) Lives in current home…					0.0375	8944
less than 7 years	-0.207**	0.070	0.096**	0.026		[4193]
7 years or more	-0.156**	0.076	0.144**	0.029		[3870]
duration is missing	-0.431**	0.141	0.141**	0.063		[881]

Robust standard errors are adjusted for clustering at the PUMA level. Each specification is a single OLS regression in which *Ln PUMA earnings* and *Ln household income* are interacted with an exhaustive set of dummies. All regressions also include as controls the uninteracted set of dummy variables as well as the same controls as the baseline regression reported in table 1, column 1. Except for specification (4), the baseline sample consisting of individuals married or cohabiting in both waves is used. In specification (1) only information from the main respondent is used (because there is virtually no variation in the average gender of couples). In specification (4), no interactions with PUMA earnings or own household income are included for 2 observations with missing marital status transitions. The hypothesis that the coefficients on *Ln PUMA earnings* are all equal to each other cannot be rejected at a significance level of 0.10 or lower for any of the specifications. The number of observations in each category is denoted between square brackets.

Table 8: Interactions With Frequency of Respondent's Social Contacts

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	S.E. [p-value]	*Ln HH income* Coeff.	S.E. [p-value]	Adj. R^2	N
(1) Socialize with a neighbor					0.0400	8944
Less than once a month or missing	-0.121*	0.071	0.129**	0.026		[5076]
Once a month or more frequently	-0.314**	0.064	0.110**	0.027		[3868]
P-value on test of equal coefficients		[0.029]		[0.575]		
(2) Socialize with relatives					0.0383	8944
Less than once a month or missing	-0.140*	0.077	0.139**	0.033		[2927]
Once a month or more frequently	-0.243**	0.061	0.110**	0.023		[6017]
P-value on test of equal coefficients		[0.257]		[0.452]		
(3) Socialize with friends who live outside the neighborhood					0.0376	8944
Less than once a month or missing	-0.233**	0.083	0.154**	0.031		[4119]
Once a month or more frequently	-0.185**	0.064	0.088**	0.026		[4825]
P-value on test of equal coefficients		[0.640]		[0.106]		
(4) Socialize with people one works with					0.0374	8944
Less than once a month or missing	-0.187**	0.064	0.137**	0.023		[6325]
Once a month or more frequently	-0.249**	0.081	0.070*	0.036		[2619]
P-value on test of equal coefficients		[0.536]		[0.110]		

Robust standard errors are adjusted for clustering at the PUMA level. Each specification is a single OLS regression in which *Ln PUMA earnings* and *Ln household income* are interacted with an exhaustive set of dummies. All regressions also include as controls the uninteracted set of dummy variables as well as the same controls as the baseline regression reported in table 1, column 1. In wave 1, respondents were asked how often they "spend a social evening" with various types of people while in wave 2 they were asked how often they "get together socially" with these types of people. The number of observations in each category is denoted between square brackets. Frequency of social contacts is the frequency of social contacts of the main respondent because this variable was not collected of spouses in both waves.

Table 9: Neighbors' Earnings by Education Interacted with Own Education

Dependent variable:
Self-reported happiness

(1) Unrestricted Interaction

	Ln Predicted PUMA Earnings of neighbors with:	
Own Education	Less than college	College or more
Less than College	-0.284 (0.225)	0.017 (0.305)
College or more	-0.069 (0.201)	-0.402 (0.304)
Adjusted R^2	0.0249	
N	7523	

(2) Restricted Interaction

	Ln Predicted PUMA Earnings of neighbors with:	
Own Education	Less than college	College or more
Less than College	-0.316 * (0.190)	-0.048 (0.176)
College or more	-0.048 (0.176)	-0.316 * (0.190)
Adjusted R^2	0.0251	
N	7523	

Robust standard errors are adjusted for clustering at the PUMA level. All regressions also include as controls the uninteracted variables as well as the same controls as the baseline regression reported in table 1, column 1. The sample is limited to the baseline sample for which the respondent and his or her spouse both have non-missing values for educational attainment.