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Do People Care How Much Their Neighbors Earn?

How relative earnings affect well-being.

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Abstract

This paper empirically investigates whether individuals feel worse off when the earnings of others around them rise. In other words, do people care about *relative* position and does "keeping up with 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 average earnings in the neighborhood. I find that, controlling for an individual's own income, higher earnings of neighbors are associated with lower levels of self-reported happiness. I exploit the data's rich set of measures of well-being and behavior as well as its panel nature to provide evidence that this association is not driven by selection or by changes in the way people define happiness. I find suggestive evidence that the negative effect of increases in neighbors' earnings on own well-being is most likely caused by interpersonal preferences or people having utility functions that depend on relative consumption in addition to absolute consumption.

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1. Introduction

The notion that individuals are motivated at least partly by concerns about relative position was prevalent among classical economists. 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 goods. 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 current economists are of course not unaware 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 relatively 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 $\overline{C}$, 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).

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[1] This quote is borrowed from Graham and Pettinato (2002).

[2] Becker (1974) introduces a more general framework for incorporating social considerations into a utility function.

[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), Frank (1985), Neumark and Postlewaite (1998), Kapteyn (2000) or Kamenica and Shapiro (2001). 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

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 is important for many policy questions. 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. As analyzed by Boskin and Sheshinski (1978), Layard (1980), Oswald (1983), Seidman (1987), Ireland (1998), and Ljungqvist and Uhlig (2000), this externality has important implications for tax policy. 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 basked), 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 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 others around them get richer?

This paper tries to answer that question. 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 Households and Families (NSHF). 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 earnings in each PUMA are estimated by applying national earnings trends by industry and occupation from the Current Population Survey to the industry and

influence asset pricing, savings and growth. See, e.g., Abel (1990), Galí (1994), Carroll, Overland and Weil (1997) and Campbell and Cochrane (1999).

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.[4] 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.[5] 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 as with income in one's reference group as defined by education and age. They interpret this as evidence that utility is (partly) relative, but the findings could also be driven by reference group income proxying for poorly measured own income.[6] Graham and Pettinato (2002) find suggestive evidence in developing countries that well-being is influenced by relative income concerns though they only use a self-reported measure of relative position. Using Canadian data, Tomes (1986) relates self-reported happiness and life-satisfaction to own income and income 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 concluded that his "results support the

[4] 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.

[5] Frey and Stutzer (2002) provide an excellent review of this literature.

[6] Van Praag and Kapteyn (1973) found previously that income needed to reach a certain level of satisfaction rises with own income and interpret this a "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.

interdependent preferences model, but defy any simple characterization in terms of inequality aversion or relative economic status."[7]

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 include individual fixed effects, its detailed geographical information allows for the inclusion of state-wave-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, it 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 affecting the 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 satisfaction with one's material position

[7] 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.

(e.g. one's financial situation) rather than changes with one's satisfaction with other aspects of life (e.g. one's family life). These findings provide suggestive evidence 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 behavioral data 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, the only behavior affected is the individual's choice of reference group implicit in the decision where to locate.[8] 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.[9] 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. Loewenstein, O'Donoghue and Rabin (2002) 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 changes – 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. 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*

[8] See, Falk and Knell (2003) for evidence on reference groups choices from a questionnaire study.
[9] 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.

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 it doubtful that relative income concerns can be credibly identified from mobility decisions or housing price information.

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, Gruber and Mullainathan, 2002, for another example of such a question). 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 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"

To determine whether well-being depends partly on relative income concerns, one might then estimate an equation of the form:[10]

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

Can a 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? Let me offer 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. 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

[10] 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.

experienced well-being, U_i, is equal to her income, Y_i, and that individuals are asked the question "Taking things all together, would you say your are happy or unhappy?" Individuals now face the task of translating experienced well-being into an answer to the happiness 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.[11] This concern can be addressed 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. Nevertheless, the concern about local omitted variables driving the result is addressed in three ways. First, if the results hold up after inclusion of state*wave 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.[12] However, if we

[11] This is also a potential explanation for the findings in a number of studies that as incomes in a country rise over time levels of self-reported happiness or life satisfaction remain remarkably constant. See, e.g. Easterlin (1974).

[12] 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 variation in local prices due to transportation cost or local production costs could possibly explain the findings. Since we can control for state*wave 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.

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.[13] 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. Unless otherwise noted, the measure of local income throughout this paper is this predictor of average earnings at the PUMA level. I refer to this measure as *LnPumaEarnings* or more informally as neighbors' earnings. 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.

The third story is that the results are driven by omitted individual characteristics that both influence 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 respond being happy) into localities with relative 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

[13] 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.

receiving an inheritance maybe 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. This still leaves open the possibility that time-varying unobserved individual characteristics drive the results. This possibility is investigated by limiting the sample to individuals who remain in the same area and including individual fixed effects. By limiting the sample to those who remain in the same area, we dramatically reduce the scope for changing unobserved individual characteristics to be correlated with the choice of which area to live in.[14] Moreover, because the individuals remain in the same area, the individual fixed effects also act as area fixed effects, thus absorbing any time-invariant characteristic of the area.

To preserve 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 happiness 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_{it} is an error term that may be clustered within PUMAs.

[14] Of course, it doesn't completely rule out such a correlation. If an area gets richer and primarily those whose unobservables make them *unhappier* decide to stay, there would be a negative correlation between unobserved determinants of happiness and local income. However, it seems reasonable to be most concerned about unobserved shocks among those individuals who move. After all, something must be going on in the lives of these individuals that prompt them to move.

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 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.

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 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.[15] 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.[16] Though the questionnaires are not identical in both waves, many questions were asked twice making it possible to treat the data as a

[15] The NSFH is 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

[16] A third wave, which was fielded in 2001-02, is expected to become public later in 2003.

panel of about 10,000 individuals.[17] 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.[18] The respondents of the baseline sample live in 601 separate Public Use Micro Areas in the first wave, in 951 PUMAs in the second wave, while 566 PUMAs have respondents living there in both waves (more about the definition of PUMAs later).

The main outcome variable used 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 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 items from the Lenore Radloff's (1977) depression scale, the frequency of open disagreements with one's spouse on a number of topics, self-reported health status, the frequency of financial worries and, only in the second wave, self-reported satisfaction with various aspects of one's life. Detailed descriptions of these variables are included in the data appendix.

The individual-level controls in the baseline specification consist of income proxies, demographic characteristics, health status and religious affiliation. The main income variable is log household income. Other control variables that may also proxy for income are national average earnings in the industry*occupation cell of the respondent, log value of the home, a set of 5 dummies for the type of home, usual hours of work, dummies for labor force status (employed, unemployed, not in labor force), years of own education and years of spousal education. Any missing values are dummied out as are logarithms of any dollar amount smaller than $100/year.[19] The demographic controls consist of gender, a set of 4 race/ethnicity dummies, log household size, 3 marital status dummies, a 3-segment spline in age (breakpoints at 35 and 60) and dummies for having moved since the first interview. Own and spousal self-reported health status are included as controls because self-reported health is an important determinant of happiness that

[17] XXX to be done: describe attrition (about 25%); causes of attrition.
[18] Geographic identifiers are not publicly available, but the collectors of the NSFH generously merge in geographic information at the zip-code level or above for academic researchers signing the confidentiality agreement.
[19] All $ amounts are in real 1982-84 dollars using the CPI-U.

turns out to be unrelated to neighbors' earnings. 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. In the Boston area, for example, Cambridge-Somerville or Brookline-Newton is a PUMA, while the city of Boston proper is divided into 5 PUMAs. The 1990 Census microdata are used to estimate the 3-digit industry x 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.

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 x 3-digit occupation cell in each of the two time periods when NSFH interviews took place.

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. T-statistics 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, if your neighbors' earnings increase while your situation remains unchanged, you report being less happy. In contrast, earnings in one's industry*occupation cell do not significantly affect self-reported happiness. 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 (more about this below). Usual working hours has a significant negative effect, unemployment status a marginally significant negative effect while a dummy for being out of the labor force has a marginally significant positive effect. The other demographic controls yield few surprising insights. Perhaps disturbing for romantics is the finding that own self-reported health status seems to be about ten times more important for own happiness than spousal health status. The insignificant negative coefficient on own education is in line with findings in the literature and might be explained by aspiration levels that rise with education. The coefficient on spousal education is positive, probably because spousal education also proxies for income.

Column 2 omits the controls for average earnings in the respondent's industry*occupation cell, self-reported health, education, home value, type of home because these controls might be acting as a proxy for household income which undoubtedly is measured with some error. Indeed, with these controls omitted, the coefficient on household income more than doubles and becomes roughly equal in magnitude (but of opposite sign) to the coefficient on neighbors' earnings. Taken literally, this would imply that if both own income and neighbors' earnings rose by the same percentage, a person would not feel any better or worse off. However, there is probably a fair amount of measurement error in both variables and it seems quite plausible that measurement error in an individual-level variable such as own income might be substantially larger than measurement error in a more aggregated variable such as neighbors' earnings. In that case, attenuation bias in the coefficient on household income would be larger than the attenuation bias in the coefficient on neighbors' earnings and the true effect of an increase of everybody's income by the same factor would make

people better off. Even if we don't know the exact amount of measurement error in each variable, it is probably fair to conclude from this table that the negative effect of neighbors' earnings on happiness is sizeable compared to the positive effect of own income.

Column 3 includes individual-specific fixed effects, thus controlling for all time-invariant individual characteristics. The coefficient on neighbors' earnings remains negative and significant. This finding rules out 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. Apart from changes in neighbors' earnings, the only other significant predictor of changes in happiness is the change in self-reported health status.

Could the results be spurious?

Table 2 investigates further whether selection or omitted area characteristics could be driving the results. The first row of Table 2 replicates the baseline regression. The second row shows a regression that is identical to the baseline regression, except that a full set of state*wave fixed effects and a control for being in a metropolitan area are added. Both the coefficients on neighbors' earnings and on own income remain virtually unchanged and statistically significant. Hence, the results are driven by variation in neighbor's 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 (after all, something causes them to move) and that perhaps this unobserved factor causes the happiness to be inversely related to average neighbors' earnings. The third row tests this by showing the baseline regression excluding all individuals 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 driven by movers.

The regression in the fourth line is identical to the one in the third 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. Any effect correlated with the industry*occupation composition of each PUMA is absorbed by the fixed effects. It is hard to imagine how national trends in industry*occupation earnings would be correlated with unobserved changes in PUMA characteristics that directly affect happiness. The individual fixed effects absorb any time-invariant individual characteristics while time-varying unobserved characteristics cannot affect one's neighbors' earnings because the sample is limited to non-movers. Though the estimated coefficient on neighbors' earnings is again very close to the one in the baseline regression, the standard error is very large which means that this is at best suggestive evidence against selection.

Could neighbors' earnings be a proxy for local price levels? In this case, the negative coefficient on neighbor's earnings would simply reflect that happiness falls as real incomes fall. Recall that neighbor's earnings are measured by *LnPumaEarnings*, which is a predictor of local earnings based on the *local* industry*occupation mix but *national* earnings data.[20] 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 2 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

[20] See Appendix B for details on the construction of *LnPumaEarnings*.

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 (age, education, 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 fifth 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 somewhat but remains negative and highly 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. The second and third rows show that the results remain virtually unchanged if the baseline regression is estimated on just observations in the first wave or just observations in the second wave. One might be concerned that the results are driven by the somewhat complicated procedure used to predict *LnPumaEarnings*. The fourth row removes 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. The fifth 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 substantially 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 sixth row, which shows that the estimate on neighbors' earnings hardly changes after inclusion of a 5^{th} 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 (7) 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.[21] Finally, the eighth row shows that household income is a much stronger predictor of happiness than the couple's earnings, but that it does not matter for the effect of neighbors' earnings which of these two measures is used.

Do neighbors' earnings affect other outcomes?

Could an increase in neighbors' earnings merely change what respondents *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 may yield some insights. The first 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 depression. As the regression shows, neighbors' earnings have no significant impact on the depression index.[22] This result therefore does little to alleviate concerns about shifting definitions of happiness, though depression might be a sufficiently different concept from well-being to pick up any effects of neighbors' earnings.

If increases in neighbors' earnings reduce happiness because one cannot afford the goods that neighbors can afford, 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?*" The second specification shows that respondents in areas with higher average earnings do have more financial worries after controlling for own income and other own

[21] 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 1.21 and thus close to unity, to which the error term in the latent model of the ordered Probit is normalized.

[22] Of the 12 Radloff items, only one ("Feeling irritable, or likely to fight or argue") was significant at the 5% level. Since, one out of every 20 items should be significant at the 5% level on average, not too much inference can be made from this finding. Experimenting with threshold effects in the Radloff index did not turn up any significant effects of neighbors' earnings.

characteristics. Because this question is less prone to a definition shift in response to neighbors earnings, this finding does alleviate 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. The regressions in specification 3 show that higher neighbors' earnings are associated with more frequent disagreements about money, household tasks and the children but not significantly with the frequency of fights about immaterial issues such as sex, in-laws and spending time together. It may be less clear why neighbors' earnings would be associated with disagreements about household tasks and the children, though one can make up plausible stories to explain this finding.[23] 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.

Since a large literature examines the effect of relative position on health outcomes, the fourth specification uses self-reported health status (relative to one's age group) as an outcome measure.[24] 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.[25]

Mechanisms behind the association between neighbors' earnings and happiness

[23] 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. Having children who interact with children of neighbors who earn more could confront parents with contentious issues such as whether their children should also get as high an allowance, be clothed as nicely and attend an equally fancy summer camp as their neighbors' kids.

[24] 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.

[25] Many papers on relative position and health use relative deprivation (the income gap with those earning more) rather than average neighbor's 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). 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 including or excluding control variables, such as the satisfaction questions. 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. Similarly, if excluding a control variable increases the magnitude of the coefficient of neighbors' earnings, that variable accounts for part of the relationship between neighbors' earnings and happiness. The first two rows of the table replicate the baseline regression for the whole sample and for just wave 2. Specification 3 shows the effects of including satisfaction questions as additional controls. The effect of neighbors' earnings is roughly cut in half and no longer significant at the 5% level if satisfaction with one's sex life, financial situation, amount of leisure time or one's home is included as a control. Thus, apparently, higher neighbors' earnings reduce happiness for a large part through their effect on these four variables. 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 with sex life or amount of leisure can account for part of the relationship between neighbors' earnings and happiness. The finding on sex life seems surprising but does square with experimental evidence on primates showing that status can affect various hormone levels including ones that affect sexual behavior.[26]

[26] See Frank (1985) for a lucid discussion and additional references on the experimental work on status and hormone levels in primates. In addition, running the baseline regression with frequency of sex with one's partner as outcome variable (instead of happiness) shows that higher neighbors' earnings are associated with less frequent sex, leading some plausibility to sex as one of the mechanisms.

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 other seven 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).[27] Controlling for each of these questions separately never makes the relationship between neighbors' earnings and happiness insignificant. Even controlling for all seven at the same time leaves the relationship between neighbors' earnings and happiness significant at the 5% level. On the other hand, simultaneously controlling for satisfaction with sex life, financial situation, leisure and home completely accounts for the relationship between neighbors' earnings and happiness. 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.

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. Alternatively, one might need to commute further (to get a better job) or increase work hours. These possible channels are explored in specifications 4,5 and 6. Including a control for commuting time does not reduce the effect of neighbors' earnings on happiness, while excluding controls for own household income or for usual working hours does not increase the effect. Thus, the relationship between neighbors' earnings and happiness does not seem to be running through commuting time or labor supply decisions.

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.

[27] 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. Or, perhaps, the fact that this variable is missing for those without a job (but with a working spouse) accounts for its relative lack of power.

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

Specifications 3 and 4 show the relationship between local earnings and happiness across groups based on current marital status and marital status transitions. In specification 3, the hypothesis that the coefficients on neighbors' earnings are all equal to each other cannot be rejected but the effect seems to operate most strongly for individuals who are married or divorced. These individuals may be more settled than never married individuals and may thus find their neighbors a more appropriate reference group. It is not immediately obvious why neighbors' earnings are unrelated to the happiness of widowed individuals, but one could imagine that after a death of a spouse one becomes less concerned with mundane matters such as relative earnings differences. Specification 4 confirms that the effect of neighbors' earnings on happiness operates most strongly for non-widowed individuals whose marital status remains the same. In fact, the hypothesis that the effect of neighbors' earnings is the same for all marital status groups can just be rejected at the 10% level and the difference in the effect between those remaining married and those with a status change is significant at 5% level. Apparently, those with marital status changes have other life events going on that either swamp or replace concerns with relative earnings.

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 increase 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 is about equally strong for relative new residents (less than 6 years) as for longer-term residents.[28]

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 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. The table shows that the effect of neighbors' earnings is significantly stronger for those who socialize frequently with neighbors or relatives but not for those who socialize more frequently with friends outside the neighborhood or people they work with. These findings are broadly consistent with what one would expect if social comparisons with neighbors partly determine people's happiness. The finding that neighbors' earnings have a greater negative effect on people who socialize more with their relatives seems perhaps somewhat surprising at first, but may make sense if frequent social contacts with relatives occur mostly for relatives who live in the neighborhood. In that case, *LnPumaEarnings* would also proxy for the income of these relatives and may be picking up the effect of social comparisons with relatives.

Non-linear effects

Table 9 investigates whether relation between neighbors' earnings and happiness displays non-linearities but finds no evidence of such effects. Specification 1 includes a quadratic in *LnPumaEarnings* and shows that the marginal effect of *LnPumaEarnings* is relatively constant over a wide range of local earnings. Specifications 2,3 and 4 examine whether relative position matters more for richer or poorer individuals. Specifications 2 and 3 show that the marginal effect of neighbors' earnings on happiness does not differ significantly by own household income. Specification 4 shows that the effect seems

[28] Looking at finer partitions of duration of residence did not reveal any interesting patterns. The magnitude of the effect is about half the size (-0.13) in the first year but the standard error sufficiently large to prevent making any precise inference from this.

somewhat stronger for those with incomes above the median in their locality, but this difference is not statistically significant (p-value of 0.33 on the test of equal effect).

5. Conclusion

This paper shows that increases in neighborhood earnings negatively affect self-reported happiness. 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 including ones with individual fixed effects. 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 ground that they appear to stem from an interpersonal preference component that is morally questionable, externalities of this size in principle offer considerable scope for policy interventions, especially in the area of taxation. In practice, however, the size of the externality, though statistically highly significant, would need to be estimated more precisely. Moreover, it would seem prudent to have further corroboration of this paper's results before they are used as a rationale for policy. I leave these tasks to future work.

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Appendix:

Appendix A: Descriptive Statistics and Variable Definitions

To Be Written.

Appendix B: Construction of Predictor for Puma Earnings

To Be Written.

Table 1: Baseline Regression

Dependent variable:	(1)		(2)		(3)	
Self-reported happiness	Baseline		Short		Indiv. F.E.	
	Coeff.	t–stat.	Coeff.	t–stat.	Coeff.	t–stat.
Ln PUMA earnings (predicted)	**-0.237**	**-3.74**	**-0.196**	**-3.08**	**-0.369**	**-2.36**
Ln Industry/occupation earnings	0.018	0.55			0.014	0.19
Ln Household Income	0.062	2.69	0.158	7.21	0.001	0.02
Ln Usual working hours	-0.098	-2.89			0.030	0.45
Unemployed	-0.188	-1.72	-0.238	-2.32	-0.123	-0.58
Not in the labor force	0.163	1.79	0.059	1.55	0.057	0.37
Self-reported health	0.362	17.66			0.237	4.75
Spouse's self-reported health	0.032	2.75			0.008	0.39
Ln Household size	-0.173	-4.37	-0.184	-4.43	-0.122	-1.06
Age spline (segment 19-35)	-0.013	-2.88	-0.012	-2.52	0.012	0.20
Age spline (segment 35-60)	0.004	1.72	0.003	1.03	0.024	0.38
Age spline (segment 60 plus)	0.007	1.63	0.013	2.72	0.022	0.33
Female	-0.059	-1.89	-0.034	-1.18		
Asian	-0.116	-0.95	-0.168	-1.34		
Black	0.015	0.27	-0.026	-0.46		
Hispanic	0.201	2.91	0.132	1.86		
Other race	0.089	0.27	0.056	0.15		
Years of education	-0.010	-1.36				
Spouse's years of education	0.015	2.08				
Ln Value of home	0.017	0.75			0.003	0.06
Mobile home	0.057	0.83			-0.160	-1.07
Multifamily home	0.066	1.05			0.120	0.77
Townhouse	-0.069	-0.80			0.001	0.01
Apartment (≤4 units)	0.017	0.16			0.012	0.06
Apartment (≥5 units)	0.048	0.61			0.133	0.88
No religion	-0.140	-2.52	-0.173	-2.96		
Jewish	-0.173	-1.79	-0.170	-1.62		
Baptist	0.060	1.41	0.030	0.68		
Episcopalian	0.016	0.16	0.044	0.46		
Lutheran	-0.108	-1.84	-0.090	-1.42		
Methodist	0.047	1.03	0.062	1.30		
Mormon	-0.033	-0.43	-0.013	-0.16		
Presbyterian	-0.132	-1.75	-0.103	-1.40		
Congregational	-0.035	-0.40	-0.010	-0.11		
Protestant, no denomination	-0.027	-0.39	-0.003	-0.04		
Other Christian	0.023	0.45	0.024	0.46		
Other religions / missing	0.001	0.01	0.052	0.45		

Moved within same city since wave 1	0.033	0.65	-0.007	-0.13	0.042	0.38
Moved to different city since wave 1	0.025	0.49	0.038	0.76	0.018	0.19
Wave	-0.078	-2.08	-0.131	-3.81	-0.181	-0.51
Individual fixed effects?	NO		NO		YES	
Adjusted R^2	0.0753		0.0183		0.2865	
Number of observations	8942		8942		8942	

Notes: Robust t-statistics adjusted for clustering on PUMAs (994 clusters). 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 omitted categories are: Male, White, Single Family Home, Catholic, Did Not Move. The sample consists of respondents of NSFH waves 1 and 2 that are married or cohabiting in both waves.

Table 2: Testing for Selection

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	t -stat	*Ln HH income* Coeff.	t-stat	Adj. R^2	N
(1) Baseline	-0.2374	-3.74	0.0621	2.69	0.0753	8942
(2) Including State*Wave fixed effects and a dummy for metropolitan areas	-0.2148	-3.10	0.0614	2.63	0.0784	8942
(3) Observations remaining in same PUMA	-0.2441	-3.10	0.0727	2.71	0.0774	6904
(4) Observations remaining in same PUMA and individual fixed effects	-0.2435	-0.20	-0.0191	-0.26	0.2769	6904
(5) Excluding controls expressed in $ terms (household income and home value)	-0.1896	-3.08			0.0708	8942

Robust t-statistics 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 3: Robustness Checks

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	t -stat	*Ln HH income* Coeff.	t-stat	Adj. R^2	N
(1) Baseline	-0.2374	-3.74	0.0621	2.69	0.0753	8942
(2) Only wave 1	-0.2116	-2.38	0.0446	1.42	0.0680	4563
(3) Only wave 2	-0.2474	-3.00	0.0792	2.14	0.0817	4379
(4) 1989 *actual Ln PUMA earnings* as control (instead of predicted *Ln PUMA earnings*)	-0.2773	-3.94	0.0642	2.78	0.0755	8942
(5) Full balanced panel sample (including non-married individuals)	-0.1510	-3.66	0.0744	4.14	0.1019	17190
(6) Controlling for 5^{th} order polynomial in ln household income	-0.2412	-3.78	0.0964	2.71	0.0753	8942
(7) Ordered probit	-0.2226	-4.03	0.0559	2.74	0.0292	8942
(8) Couple's *Ln earnings* as control (instead of *Ln HH income*)	-0.2275	-3.60	0.0217	1.24	0.0747	8942

Robust t-statistics 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 HH income for someone with the mean ln HH income. The coefficients in the ordered probit turn out to be similar in magnitude to those in the baseline regression partly because the rmse of the baseline regression is 1.21 and thus close to unity, to which the rmse is normalized in the latent model of the ordered probit.

Table 4: Other Self-Reported Measures of Well-Being

Dependent variable:	*Ln PUMA earnings* Coeff.	t-stat	*Ln HH income* Coeff.	t-stat	Adj. R^2	N
(1) *Depression index (sum of 12 Radloff items)*	0.1473	0.21	-0.5573	-2.44	0.1372	9784
(2) *Financial worries*	0.2503	3.90	-0.3630	-10.3	0.2028	5028
(3) Frequency of open disagreements about:						
a. household tasks	0.1339	2.80	-0.0542	-3.19	0.1411	9757
b. money	0.1414	3.15	-0.0878	-4.41	0.1758	9739
c. spending time together	0.0026	0.04	-0.0436	-1.86	0.0980	9717
d. sex	0.0772	1.47	-0.0524	-2.85	0.1088	9505
e. in-laws	0.0012	0.03	-0.0121	-0.75	0.0835	9560
f. the children	0.2240	3.30	-0.0121	-0.46	0.1435	7900
(4) *Health status relative to age group*	-0.0092	-0.24	0.0982	6.77	0.0977	9968

Robust t-statistics 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 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." The frequency of open disagreements is measured on a scale of 1 ("never") to 6 ("almost every day"). The regressions in specification 3 also control for self-reported quality of relationship with spouse. The variable *Financial worries* 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." 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."

Table 5: Components of Happiness

Dependent variable: Self-reported happiness	Coeff.	t-stat
satisfaction with family life	0.238	11.92
satisfaction with financial situation	0.139	9.76
satisfaction with sex life	0.105	8.01
satisfaction with home	0.095	5.81
satisfaction with health	0.083	4.83
satisfaction with present job	0.069	5.01
satisfaction with amount of leisure time	0.050	4.31
satisfaction with friendships	0.027	1.49
satisfaction with city or town	0.023	1.45
satisfaction with physical appearance	0.012	0.82
satisfaction with neighborhood	-0.011	-0.74
R^2	0.3868	
Number of observations	4379	

Robust t-statistics 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.	t-stat	*Ln HH income* Coeff.	t-stat	Adj. R^2	N
(1) Baseline	-0.2374	-3.74	0.0621	2.69	0.0753	8942
(2) Baseline (wave 2 only)	-0.2474	-3.00	0.0792	2.14	0.0817	4379
Additional controls for						
(3) *Satisfaction with (wave 2 only):*						
a. *sex life*	-0.1152	-1.58	0.0729	2.15	0.2264	4379
b. *financial situation*	-0.1322	-1.75	-0.0771	-2.22	0.2145	4379
c. *amount of leisure time*	-0.1343	-1.68	0.0609	1.74	0.1639	4379
d. *home*	-0.1434	-1.90	0.0763	2.20	0.2007	4379
e. *health*	-0.1791	-2.26	0.0712	1.94	0.1467	4379
f. *present job*	-0.1810	-2.26	0.0667	1.84	0.1471	4379
g. *family life*	-0.1856	-2.51	0.0786	2.46	0.2881	4379
h. *friendships*	-0.1974	-2.47	0.0726	2.06	0.1730	4379
i. *physical appearance*	-0.2156	-2.71	0.0879	2.45	0.1411	4379
j. *neighborhood*	-0.2307	-2.89	0.0769	2.11	0.1368	4379
k. *city or town*	-0.2721	-3.46	0.0799	2.15	0.1356	4379
l. Top 4 *(sex, finance, leisure, home)*	-0.0066	-0.10	-0.0146	-0.46	0.3351	4379
m. Bottom 7 *(health – city or town)*	-0.1393	-1.98	0.0716	2.20	0.3360	4379
(4) *Minutes of commuting time*	-0.2351	-3.67	0.0629	2.73	0.0752	8942
Exclude controls for:						
(5) *Ln Household income*	-0.2222	-3.51	n/a		0.0747	8942
(6) *Ln Usual working hours*	-0.2347	-3.69	0.0578	2.50	0.0747	8942

Robust t-statistics 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.	t-stat	*Ln HH income* Coeff.	t-stat	Adj. R^2	N
(1) Gender					0.0754	8942
Male	-0.2240	-2.64	0.0721	2.22		[3998]
Female	-0.2455	-2.82	0.0541	1.75		[4944]
(2) Age					0.0754	8942
age ≤ 30	-0.0118	-0.10	0.0363	0.87		[1651]
30 < age ≤ 40	-0.3329	-3.40	0.0420	0.97		[2883]
40 < age ≤ 60	-0.1972	-2.00	0.1051	2.48		[3003]
60 < age	-0.3735	-2.29	0.0336	0.63		[1405]
(3) Current marital status					0.1018	17190
Married or cohabiting	-0.1618	-2.94	0.0868	4.14		[10585]
Divorced or separated	-0.2684	-2.46	0.0771	2.09		[2827]
Widowed	-0.0250	-0.17	0.0381	0.77		[1620]
Never married	-0.0641	-0.58	0.0813	1.84		[2156]
(4) Marital status transition					0.1033	17190
Remains married or cohabiting	-0.2274	-3.73	0.0759	3.30		[8942]
Remains divorced or separated	-0.2900	-2.03	0.0834	1.75		[1804]
Remains widowed	0.0024	0.01	0.0459	0.79		[1267]
Remains never married	-0.1319	-1.04	0.0928	1.80		[1555]
Marital status change	0.0191	0.23	0.0912	2.75		[3618]
(5) Home ownership					0.0756	8942
Rents	-0.2689	-2.29	0.1447	3.20		[2044]
Owns	-0.2327	-3.21	0.0293	1.09		[6898]
(6) Presence of child(ren) aged ≤ 18					0.0754	8942
No child(ren) present in HH	-0.2821	-3.20	0.0534	1.71		[3976]
Child(ren) present in HH	-0.2017	-2.41	0.0656	2.06		[4966]
(7) Lives in current home...					0.0755	8942
≤ 72 months	-0.2364	-2.69	0.0467	1.47		[3826]
> 72 month	-0.2019	-2.23	0.0829	2.64		[4331]
duration is missing	-0.4089	-2.54	0.0333	0.43		[785]

Robust t-statistics 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 dummies variables as well as the same controls as the baseline regression reported in table 1, column 1. Except for specifications (3) and (4), the baseline sample consisting of individuals married in both waves is used. In specification (3), no interactions with PUMA earnings or own household income are included for 2 observations with missing current marital status. Similarly, interactions are not included for 4 observations with missing marital status transitions in specification (4). 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 except for specification 4 where the p-value of the hypothesis test is 0.0097. 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.	t-stat [p-value]	*Ln HH income* Coeff.	t-stat [p-value]	Adj. R^2	N
(1) Socialize with a neighbor					0.0783	8942
Less than once a month or missing	-0.1490	-1.77	0.0599	1.86		[5123]
Once a month or more frequently	-0.3331	-4.17	0.0664	2.23		[3819]
P-value on test of equal coefficients		[0.086]		[0.878]		
(2) Socialize with relatives					0.0777	8942
Less than once a month or missing	-0.0856	-0.87	0.0673	1.76		[2979]
Once a month or more frequently	-0.3109	-4.22	0.0566	2.14		[5963]
P-value on test of equal coefficients		[0.046]		[0.808]		
(3) Socialize with friends who live outside the neighborhood					0.0767	8942
Less than once a month or missing	-0.2625	-2.68	0.1004	2.86		[4164]
Once a month or more frequently	-0.2087	-2.64	0.0218	0.76		[4778]
P-value on test of equal coefficients		[0.661]		[0.078]		
(4) Socialize with people one works with					0.0755	8942
Less than once a month or missing	-0.2240	-2.96	0.0710	2.65		[6406]
Once a month or more frequently	-0.2585	-2.83	0.0255	0.57		[2536]
P-value on test of equal coefficients		[0.749]		[0.382]		

Robust t-statistics 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 dummies 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.

Table 9: Non-linear Effects of Ln PUMA Earnings

Dependent variable:
Self-reported happiness

Specification:	*Ln PUMA earnings* Coeff.	t-stat	*Ln HH income* Coeff.	t-stat	Adj. R^2	N
(1) Quadratic in Ln puma earnings					0.0753	8942
Direct effect	-0.2515	-3.68	0.0615	2.65		
Interaction w/ Ln puma earnings (demeaned)	0.1263	0.82				
(2) Interaction with own income					0.0752	8942
Direct effect	-0.2581	-3.72	0.1296	0.18		
Interaction w/ Ln HH income (demeaned)	-0.0065	-0.09				
(3) Interaction with own income and it square					0.0750	8942
Direct effect	-0.2571	-3.54	0.2003	0.26		
Interaction w/ Ln HH income (demeaned)	-0.0135	-0.17	0.0135	0.03		
Interaction w/ (Ln HH income)^2 (demeaned)	-0.0010	-0.02				
(4) By local financial position					0.07519	8942
Below median income in own PUMA	-0.1757	-1.79	0.0619	1.43		
Above median income in own PUMA	-0.2925	-3.67	0.0646	1.43		
P-value on test of equal coefficients		[0.325]				

Robust t-statistics 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 the variables indicated. 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. To convert mean PUMA level *earnings* to a proxy for median PUMA level *income*, I multiplied PUMA level earnings by a factor such that in each wave exactly 50% of the full weighted NSFH sample had household incomes above the PUMA level income proxy. This factor was 1.5312 in wave 1 and 1.3953 in wave 2 and resulted in 3346 observations in the baseline sample below the PUMA-level cutoff and 4996 above.