NEIGHBORS AS NEGATIVES: RELATIVE EARNINGS AND WELL-BEING*

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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 data containing various 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, that is, people having utility functions that depend on relative consumption in addition to absolute consumption.

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I. 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 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 $\overline{C}$, U(C) and U(C, $C/\overline{C}$) cannot be distinguished by individual behavior without placing

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. Samuelson [2004] and Rayo and Becker [2004] offer evolutionary explanations of relative consumption effects. Postlewaite [1998] discusses the advantages and drawbacks of modeling relative position as an argument of the utility function rather than as an instrument for getting greater consumption in the future.

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 [2005]. 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. The distinction between relative and absolute formulations of utility is also pertinent to the welfare implications of residential sorting by income and to the debate about whether the poverty line should be absolute (a fixed consumption basket) or relative (a fraction of mean or median income).

This paper provides evidence that suggests that utility depends in part on relative position. I use panel data on individuals' self-reported happiness, other measures of well-being and other characteristics from the 1987-1988 and the 1992-1994 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

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) has behavioral implications. See, for example, Pollak [1976] or Frank [1985, 1999]. 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, for example, Abel [1990], Robson [1992], Galí [1994], Carroll, Overland and Weil [1997], Campbell and Cochrane [1999], Stutzer and Lalive [2004] and Becker, Murphy and Werning [2005].

national earnings by industry, occupation and year from the Current Population Survey to the industry and occupation mix of that PUMA from the 1990 Census. 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 is 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, 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 absolute income matters for well-being but other studies have found that happiness is not purely a relative concept [Veenhoven 1991 and Diener et al. 1993]. Using European micro data, Van de Stadt, Kapteyn and Van de Geer [1985], Clark and Oswald [1996], Senik [2004] and Ferrer-i-Carbonell [2005] find that well-being is partly driven by relative position, where reference groups are defined by demographic characteristics.[6] Using U.S. data, McBride [2001] and Blanchflower and Oswald [2004] both find tantalizing evidence that relative income affects subjective well-being, but they caution about the statistical reliability of their findings.[7]

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. Layard [2003] also discusses much of this literature as part of his engaging Lionel Robbins Memorial Lectures on happiness.

6. In addition, Clark [2003] finds effects of local unemployment on happiness that may be explained by concerns about relative position.

7. 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, though this effect is only just significant at the 5 percent level in one of his two specifications and not significant in the other. Blanchflower and Oswald [2004] find that, controlling for own income, there is a sizeable but statistically

This paper contributes to this literature in three ways. First, it takes seriously the concern that living in an affluent 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 in order to investigate this concern and conclude that it is unlikely that this concern is 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 local housing prices and state 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 between neighbors' earnings and happiness. I 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. The paper's findings indicate that interpersonal preferences that incorporate relative income concerns drive the negative association between neighbors' earnings and own well-being.

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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 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. Using Canadian data, Tomes [1986] relates self-reported happiness to own income and income in the local community. He concludes that his results "defy any simple characterization in terms of inequality aversion or relative economic status."

II. 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 a subset of goods, the only behavior affected is the individual's choice of reference group implicit in the decision about 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 [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*

8. See Falk and Knell [2004] for evidence on reference group 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 a firm with highly paid workers must be paid more than a similar worker in a low productivity firm because the worker surrounded by highly paid workers needs to be compensated for the utility loss of being a relatively low earner.

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).[10] Though some skepticism towards self-reported measures of well-being is warranted (see, e.g., Bertrand and Mullainathan [2001] and Ravallion and Lokshin [2001]), Frey and Stutzer [2002] cite ample psychological evidence that confirms the validity and reliability of self-reported happiness as a measure of well-being and 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:[11]

(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 dependent variable yields a coefficient of 0.20 (s.e. of 0.014) on log own household income and a coefficient of -0.17 (s.e. of 0.04) on average log household income in one's locality (PUMA).[12] This regression previews the general findings of the more elaborate regressions presented in detail in the results section

10. 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.

11. I enter *average income in locality* separately in this equation rather than in the 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.

12. For simplicity, no other controls are included in this illustrative regression. This is a pooled cross-section regression with the sample consisting of all NSFH main respondents from the balanced panel with non-missing own income (N=16280). Average log household income in the PUMA is calculated from the 1990 Census Public Use Micro Sample and refers to income in 1989. Standard errors are adjusted for clustering at the PUMA level.

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? Ideally, we would have objective measures of utility, and all variation in neighbors' earnings would be due to exogenous shocks, such as national demand shocks to industries overrepresented in one's area. Lacking such an ideal experiment, I discuss below 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, Frederick and Loewenstein 1999].[13] 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 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 in everyone's income by the same factor would increase the fraction of people answering that 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 as the mean or median U_i), they are answering the question in relative terms. In this case, an increase in everyone's income by the same factor may not affect the proportion of individuals answering that they are happy, even though every individual's

13. 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 utility is higher.[14] I address this concern by using alternative outcome measures that have a relatively objective definition, such as the frequency of marital disagreements about various topics.

The second alternative story is that the results are driven by unobserved local area characteristics that are correlated with both 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 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 fixed effects, they cannot be driven by unobservables that operate at that level, such as climate, state policies, or regional shocks. Second, 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).[15] Thus, predicted local earnings vary across areas for two reasons: (i) the industry × occupation mix at a point in time 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 both affect happiness and local incomes conditional on industry × occupation mix. I refer to this measure as *PumaLnEarnings* 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 the possibility that areas with an overrepresentation of

14. 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.

15. This predictor follows similar predictors used by Bartik [1991], Blanchard and Katz [1992], Bound and Holzer [2000] and Autor and Duggan [2003]. See appendix 3 for details on the construction of this predictor.

high-paying industries and occupations may tend to have unobserved characteristics, such as higher housing prices, that reduce happiness. Third, to address the concern that neighbors' earnings simply proxy for local housing prices, I examine whether the results are robust to including local housing price measures as controls and whether the results hold up when we control for an individual's predicted real income (based on education, age and average national earnings for someone in the same industry and occupation as the respondent) instead of the person's actual income.

The third alternative 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 may be selection effects that lead to a spurious negative effect of *average income in locality.* For example, intrinsically happy people might be better able to deal with the rougher aspects of low-income areas, and thus choose 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:

$$(2) \qquad Happiness_{ipst} = PumaLnEarnings_{pt}\,\beta_1 + \boldsymbol{X}_{it}\,\boldsymbol{\beta}_2 + \boldsymbol{X}_p\,\boldsymbol{\beta}_3 + wave_t\,\beta_4 + \delta_s + \varepsilon_{ipst},$$

where i indexes individuals, p indexes PUMAs, s indexes states and t indexes the wave of the

survey. *Happiness* is self-reported happiness while *PumaLnEarnings* 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 while the vector $\boldsymbol{X}_p$ contains other PUMA characteristics such as its racial composition. Finally, $wave_t$ is a dummy for the wave of the survey, δ_s is a full set of state dummies and ε_{ipst} 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 cohabiting in both waves of the NSFH. I limit the sample to married or cohabiting individuals for two reasons. First, for these observations, we 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 survey questions are asked of both the main respondent and his or her spouse, it makes sense to exploit spousal information. Adding this 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.[16] 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 intrahousehold-level variation, but this does not matter since the main explanatory variables of interest (neighbors' earnings and

16. 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. In unreported regressions, I have entered the information of the main respondent and the spouse as separate observations. This yields basically the same point estimates with somewhat smaller standard errors.

own household income) do not vary within households.[17]

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 exploring other outcome or control variables, adding individual fixed effects and adding interactions between *PumaLnEarnings* and other variables.

III. Data

III.A. 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.[18] The NSFH consists of a nationally representative sample of individuals, aged 19 or older (unless married or living in a household with no one aged 19 or older), living in households and able to speak English or Spanish. The first wave of interviews took place in 1987-1988 and a second wave of interviews took place in 1992-1994. 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. This dataset is particularly well-suited for this paper because 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*

17. See Clark [1996] for a study of comparison effects within households.

18. 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

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," and intermediate values are not explicitly defined. Figure I shows the distribution of responses to this question from the main respondents and spouses in the baseline sample. Other interesting outcome measures include 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 and the frequency of financial worries. Appendices 1 and 2 contain detailed definitions and summary statistics for these outcome variables as well as for the control variables for income, labor market participation and other demographic characteristics.

III.B. Census and Current Population Survey

The smallest geographical area in the 1990 Census 5 percent 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 United States and the median and mean size of a PUMA was 127,000 and 144,000 inhabitants respectively. 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 earnings and income in 1989 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 1987-1988 and 1992-1994 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 the PUMA lies.

Predicted PUMA earnings for each time period are a weighted average of national earnings by industry × occupation cell during that time period, where the weights are the employment shares of each industry × occupation cell in the PUMA in 1990. The resulting predictor of PUMA earnings thus varies across PUMAs because of variation in the industry × occupation mix of PUMAs in 1990. As the industry × occupation employment shares are held constant within each PUMA, the predictor varies over time solely due to changes in national earnings by industry × occupation cell. Details of this procedure are found in Appendix 3.

IV. Results

IV.A. Basic results

The first column of Table I shows the baseline specification in full. This is a pooled cross-section OLS regression of self-reported happiness on predicted PUMA log earnings, individual controls, state fixed effects, and controls for metropolitan area size and racial composition in the PUMA. Individual level variables in this regression are averages of the main respondent and his or her spouse. Robust 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 predicted PUMA earnings have a significantly negative effect on self-reported happiness. In other words, controlling for other factors, individuals living in richer areas 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.[19] 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.[20] The other demographic controls yield few surprising insights.

Column two 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.

A one-standard deviation (0.27) increase in neighbors' earnings reduces self-reported happiness by 0.065 or 6 percent of a standard deviation. This effect may seem small, though one should keep in mind that there is a lot of idiosyncratic variation in self-reported happiness. It is instructive to compare the size of the effect of neighbors' earnings to that of one's own income. In the baseline specification, this comparison may be misleading because the coefficient on own household income is likely biased downwards due to measurement error in income and the inclusion of other income proxies. In specification three, 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 is 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.[21] 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

19. *Ln Value of home* has been demeaned. Hence, the dummy for renter is relative to a homeowner with a home of average value.

20. *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.

21. See Appendix 3 for details on the construction of this predictor.

specification.[22] Moreover, the estimate is now larger than the absolute value of the estimate of neighbors' earnings, though this difference is not statistically significant. Thus, the point estimates imply that if both own income and neighbors' earnings rise by the same percentage, a person would feel better off, indicating that one's absolute economic situation matters for happiness in addition to one's relative position, though I cannot reject the hypothesis that only relative position matters.

IV.B. Could the results be spurious?

Table II investigates whether omitted area characteristics or selection could be driving the results. The first row of the table reproduces the baseline results.

The second row includes individual-specific fixed effects, thus controlling for all time-invariant individual characteristics. The coefficient on neighbors' earnings remains negative and statistically significant. 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. One might worry that movers may have had something unobserved happen to them, and that perhaps this unobserved factor caused their happiness to be inversely related to average neighbors' earnings. The third 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.

22. The coefficient on own income rises to 0.16 if the controls for home value and the renter dummy are dropped. Thus, most of the rise of the coefficient on own income is due to the instrument. In the remainder of the paper, I use specification (1) rather than specification (3) as the baseline specification because it allows for a richer set of controls for own income.

The regression in the fourth row 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 from 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 meaningful evidence on selection.

Because the baseline regression already includes state fixed effects, log metropolitan population size, a dummy for non-metropolitan areas and the fraction of the PUMA population that is black, the results are driven by variation in neighbors' earnings that occurs *within* states and that is unrelated to metropolitan area size or the racial composition of the PUMA. Still, one may worry that neighbors' earnings might proxy for local omitted variables, in particular local price levels. In this case, the negative coefficient on neighbors' earnings would simply reflect that happiness is lower in areas where high price levels depress real income.[23] Because neighbors' earnings are predicted based on the *local* industry × occupation mix and *national* earnings data (excluding data from one's own state), local price variation would only be picked

23. Note, however, that if higher local prices 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 variation in prices due to transportation cost or local production costs could possibly explain the findings. Because of the state 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.

up by my measure of neighbors' earnings to the extent that it is correlated with the local industry × occupation mix. I examine this possibility in two ways.

First, in order for neighbors' earnings to proxy for local price levels, it must be the case that earnings for the average respondent are higher in nominal terms for respondents living in more expensive areas. Thus, if we don't measure a respondent's income using monetary variables (such as household income and 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 neighbors' earnings to serve as a control for local prices. The fifth row of Table II estimates the baseline regression purged of any controls that proxy for the respondent's nominal income. The coefficient on *PumaLnEarnings* drops in size but remains negative and significant, thus ruling out that *PumaLnEarnings* is just picking up variation in local price levels.

Second, I try to control directly for housing prices, which is probably the most important component of local prices. However, housing values vary because of variation in both the price of housing services and the quantity of housing services provided by the property. Ideally, only the former component should be included as a control because the latter component will also proxy for neighbors' incomes, since housing services are a normal good. To isolate the price component from the quantity component, I use the 1990 Census 5 percent PUMS to run a hedonic regression of log home value on a set of PUMA fixed effects and all available housing characteristics. To the extent that the measured housing characteristics adequately capture consumption of housing services, the PUMA fixed effects capture the price component. In row six, I control for local housing prices (as measured by these PUMA fixed effects), but the coefficient on *PumaLnEarnings* remains similar in size and statistically significant, confirming

that local price variation does not drive the effect of neighbors' earnings.[24]

Table III 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 *PumaLnEarnings.* The second row alleviates this concern; if anything, the estimate on *PumaLnEarnings* 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 PUMA log income instead of earnings yields similar results, while the fourth row shows that the results are insensitive to replacing log own household income with the couple's log total earnings. 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 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 V, 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 the 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.

24. The finding that local housing prices (adjusted for housing characteristics) do not enter significantly in the regression may seem surprising. This is true even if I use unadjusted average log home value as the measure of local housing prices, i.e., this result is not driven by some peculiarity of the hedonic home value regression. Instead, it appears that the state fixed effects and the control for metropolitan area size already absorb most of the housing price variation. Without the state fixed effects and the control for metropolitan area size, both the unadjusted and the adjusted measure of local housing prices enter negatively and significantly. In that case, the coefficient on neighbors' earnings remains negative and significant with adjusted local housing prices as a control, but it is no longer significant if unadjusted local housing prices are used as a control. This latter finding is not surprising since my predicted measure of neighbors' earnings and unadjusted local housing values are highly correlated (r=0.69) and both are imperfect measures of neighbors' true incomes.

Moreover, the ratio of the coefficient on neighbors' earnings to the coefficient on own income remains roughly constant.[25] An alternative way to deal with the ordinal nature of the happiness question is to create five dummy variables corresponding to a reported level of happiness of at least 3, 4, 5, 6 and 7 respectively.[26] Unreported regressions of these dummy variables on neighbors' earnings and the remaining controls of the baseline regression yield a significantly negative coefficient on neighbors' earnings, though the effect is only marginally significant in the regressions for attaining a happiness level of at least 3 or at least 7. These regressions show that the effect of neighbors' earnings operates throughout the happiness distribution.

IV.C. Do neighbors' earnings affect other outcomes?

One might be concerned that an increase in neighbors' earnings merely changes how individuals *define* happiness rather than their true underlying well-being. This concern is hard to rule out definitively, but using another outcome measure that is arguably less prone to shifts in definition yields some insights. One might expect that a couple surrounded by neighbors earning more would have more disagreements about material issues as their aspirations might be shaped by the spending patterns of those around them [Stutzer 2004]. The regressions in specification (1) show that higher neighbors' earnings are significantly associated with more frequent open disagreements about money, but not significantly with the frequency of disagreements about household tasks, the children, sex, in-laws or spending time together. Because the questions about open disagreements are about behavior, they seem less prone to a shift in definition in

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 squared 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.

26. I don't partition the happiness distribution at 2 because only a few percent of the observations report such low levels of happiness.

response to neighbors' earnings.[27] This finding therefore offers suggestive evidence that the estimated effect of neighbors' earnings on self-reported happiness is not simply due to a shift in the definition of happiness.

The second specification in Table IV considers a measure of depression, which is the sum of the 12 items from the Radloff depression scale that are included in both waves of the survey. Though many of the items also have subjective definitions (e.g., "feeling lonely," "sleeping restlessly"), to the extent that it is harder 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. 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 and find that 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 from being depressed come somewhat closer to being depressed). Overall, the findings using depression as an outcome variable only slightly alleviate concerns about shifting definitions of happiness, though depression may be a sufficiently different concept from well-being to pick up relative

27. The response categories for the frequency of disagreements ("Never; less than once a month; several times a month, about once a week; several times a week; almost every day") are defined by the survey instrument and thus less susceptible to subjective interpretation. However, what counts as "an open disagreement" remains somewhat subjective. This question is asked after the self-reported happiness question, so it is possible that answers to this question are correlated with self-reported happiness because of a bias towards giving answers consistent with previous answers. However, it is not clear why this bias would be more severe for disagreements about money than other disagreements.

position effects.[28]

Since a large literature examines the effect of relative position on health outcomes, the third specification uses self-reported health status (relative to one's age group) as an outcome measure.[29] 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.[30]

IV.D. Mechanisms behind the association between neighbors' earnings and happiness

One can think of overall self-reported happiness as being composed of one's satisfaction with various domains of life, such as 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. Specification (4) uses these satisfaction measures as outcome variables in order to understand which components of happiness drive the relationship between happiness and neighbors' earnings, though of course there may be drivers of happiness not covered by these satisfaction questions. As shown in specification (4), each measure of satisfaction increases with own income and, except for satisfaction with neighborhood and city or town, all satisfaction measures decrease with neighbors' earnings. However, in only three cases is the relationship with neighbors' earnings statistically significant

28. 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.

29. 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.

30. In his survey of this literature, Deaton [2003] 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 social rank can be protective of health. Eibner and Evans [2001] find evidence that in the United States, relative deprivation (which is a measure of rank and the income gap with those who are richer) increases mortality.

at the five percent level. Neighbors' earnings significantly increase satisfaction with one's city or town, which indicates that respondents are aware of the tangible benefits of living in an area with richer people. Neighbors' earnings significantly reduce satisfaction with the amount of leisure time (even though the regression controls for labor force participation and hours worked) and satisfaction with one's friendships.

If neighbors' consumption patterns shape one's aspirations, one might have expected that higher neighbors' earnings would significantly reduce one's satisfaction with material outcomes such as one's financial situation or one's home. This seems not to be the case and is confirmed by specification (5), which shows that the frequency of financial worries does not increase significantly with neighbors' earnings. Instead, people appear to be giving up leisure, to allow their friendships to suffer and to work more, perhaps in an attempt to mimic the material living standards of their neighbors.[31] This is consistent with Frey and Stutzer [2004] who argue that people substitute goods yielding extrinsic satisfaction (material possessions, status) for those yielding intrinsic satisfaction (time for family, friendships, hobbies), because predictions of future utility from the extrinsic attributes of consumption are systematically biased upwards. Furthermore, in unreported results, I run the baseline regression with all the satisfaction measures added as additional controls and find that the negative effect of neighbors' earnings remains significant at the five percent level. Hence, apparently a significant part of the negative effect of neighbors' earnings on happiness runs through drivers of happiness not captured by the satisfaction questions.

IV.E. Interaction effects

31. The labor supply effect, shown in specification (6), should be treated with caution since one would expect neighbors' earnings to be correlated with own earnings, of which labor supply is one component. Neumark and Postlewaite [1998] and Bowles and Park [2002] find effects of reference groups on labor supply.

Does the relationship between neighbors' earnings and happiness operate across a range of demographic subgroups? The first specification of Table V interacts both neighbors' earnings and own income with a set of marital status dummies. Though the hypothesis that the coefficients on neighbors' earnings are all equal to each other cannot be rejected at the 10 percent 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. In similar but unreported regressions, I tested whether the effect of neighbors' earnings on happiness varies by gender, age group, educational attainment, homeownership, presence of children in the household or length of time lived in one's current home. In all cases, the point estimates on neighbors' earnings are negative for each demographic subgroup, and in no case can the hypothesis that the effect is the same across subgroups be rejected at the 10 percent level.

One might wonder whether people predominantly compare themselves to neighbors from their own demographic subgroup. I explore this hypothesis for subgroups defined by college attendance. The point estimates of an unreported regression in which earnings of neighbors by educational attainment is interacted with own education indeed show that the happiness of those without a college degree declines with the earnings of neighbors without a college degree but is relatively 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. However, these point estimates are not significantly different from each other, thus the finding that individuals mostly compare themselves with neighbors with the same educational attainment is suggestive at best.

For two reasons, one might have expected individuals to be influenced more by neighbors' earnings if they are below the median PUMA income than if they are above the median. First, Rizzo and Zeckhauser [2003] show that individuals respond more strongly to shortfalls below their reference income. Thus, if neighbors' earnings serve as a reference income, one would expect a stronger response for those earning less than that. Second, to the extent that people also derive some happiness from "fitting in with the Joneses," an individual below the median is further negatively affected by an increase in neighbors' earnings while the negative effect is attenuated for a person above the median. Specification (2), however, shows that the effect of neighbors' earnings is almost identical for those above and below the median income in their PUMA. In additional unreported regressions, I examine other non-linearities but find no evidence of such effects. 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. In addition, adding various measures of inequality in one's PUMA to the baseline regression did not reveal any significant effects of inequality on happiness, in contrast to Alesina, Di Tella and MacCulloch [2004] who find that inequality in one's state affects happiness.

If 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. 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. Specifications (3a-d) of Table V compare 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.[32] The regressions show that the effect of neighbors' earnings is significantly stronger for those who socialize more frequently with neighbors but not for those who socialize more frequently with relatives, friends outside the neighborhood or people they work with. These findings are consistent with what one would expect if social comparisons with neighbors partly determine people's happiness.

V. 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 marital disagreements, I provide suggestive 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 find that increased neighbors' earnings have the strongest negative effect on happiness for those who socialize more in their neighborhood. I conclude that the negative effect of neighbors' earnings on well-being is real and that it is most likely caused by a psychological externality, that is, 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

32. 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.

to stem from an interpersonal preference component that may be morally questionable[33], externalities of this size can in principle substantially affect optimal income taxation, consumption taxation and residential sorting policies.

33. See Frank [2005] for arguments why these externalities should not be disallowed.

Appendix 1:

The baseline sample consists of individuals who are married or cohabiting 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 1982-1984 real dollars using the CPI-U from Bureau of Labor Statistics.

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.
PUMA ln earnings (predicted)	Average predicted pre-tax ln 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 3 for details.
PUMA ln earnings in 1989 (actual)	Average actual pre-tax ln 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. Source: 1990 Census PUMS.
PUMA ln income in 1989 (actual)	Average actual pre-tax ln household income of all non-institutionalized persons living in the PUMA of the NSFH respondent. Source: 1990 Census PUMS.
PUMA ln unadjusted house price	Average ln property value of owner-occupied units in the PUMA of the NSFH respondent. Source: 1990 Census PUMS.
PUMA ln adjusted house price	The PUMA fixed effects of an OLS regression of ln property value on all housing characteristics available in the 1990 Census PUMS.
Ln Metropolitan area population	Ln census population counts for the metropolitan area of which the PUMA is part. Source: 1990 Census Geographic Equivalency file.
Fraction black in PUMA	Fraction of the non-institutionalized population in the PUMA that is non-Hispanic black. Source: 1990 Census PUMS.
Ln household income	Ln 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	Ln 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 3 for details.
Ln actual earnings of R and spouse	Ln pre-tax earnings of the main respondent and his/her spouse.
Ln value of home	Ln 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	Ln 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; Widowed; Never married;	Cohabiting individuals (even if separated, divorced or widowed) are coded into the *Married or cohabiting* category.
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.
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]: 1-Never; 2-Less than once a month; 3-Several times a month; 4-About once a week; 5-Several times a week; 6-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.
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. The variable is reverse coded in the regressions.

Appendix 2:

Summary Statistics

	Whole Sample N=23,010			Baseline Sample Only N=8,944		
Variable:	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Self-reported happiness	5.346	1.38	19795	5.553	1.08	8944
PUMA ln earnings (predicted)	9.395	0.28	22979	9.422	0.27	8944
PUMA ln earnings in 1989 (actual)	9.390	0.24	22979	9.399	0.24	8944
PUMA ln income in 1989 (actual)	10.056	0.32	22979	10.091	0.31	8944
PUMA ln unadjusted house price	4.202	0.63	22979	4.198	0.62	8944
PUMA ln adjusted house price	-0.111	0.59	22979	-0.130	0.56	8944
Ln Metropolitan area population	13.802	1.25	18664	13.728	1.22	7021
Fraction black in PUMA	0.141	0.20	22979	0.106	0.16	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	9.801	0.74	19676	10.125	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
Educational attainment (years)	12.518	3.11	22924	12.997	2.62	8940
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
Additional variables for Table IV						
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
Variables only available in wave 2	Observations in wave 2: 10,004			Baseline observations in wave 2: 5,067		
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
Frequency of financial worries	3.093	1.21	9713	2.997	1.02	5001

Appendix 3:

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 five steps.

Step 1: Calculation of average industry × occupation × time-period earnings from CPS MORG

I use the NBER CPS labor abstracts to obtain usual weekly earnings by industry × occupation for the years 1987-1988 and 1992-1994 for all employees (self-employed individuals are excluded because their earnings data are not available). To ensure compatibility over time, all earnings are deflated by the 1982-1984 CPI-U and all topcoded earnings are replaced by twice the value of the topcode.[34] This yields the variables $LnEarn_1$ and $LnEarn_2$, ln average annual earnings by industry × occupation cell in 1987-1988 and in 1992-1994. These variables differ by state because data from the own state is excluded in calculating the average.

Step 2: 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 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.[35] 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 ×

34. 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].

35. 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 generally very similar codes compatible. I resolve any differences by merging several 3-digit occupations or industries together. This results in 225 unique and comparable industry codes and 496 unique and comparable occupation codes.

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 $LnEarnPred_1$ for 1987-1988 and $LnEarnPred_2$ for 1992-1994.

Step 3: Taking a weighted average between actual and predicted earnings

The benefit of the predictor described above is that it yields predicted earnings for industry × occupation × time-period cells that are empty in the CPS MORG and that it increases the precision of the earnings estimate for industry × occupation × time-period cells with very few observations. 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 ln 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 0.73 respectively. The variable $LnEarnHat_t$ is merged with 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 I.

Step 4: Taking averages by PUMA

Using data from the 1990 Census 5 percent PUMS, I assign to each non-institutionalized person aged 16-65 the predicted earnings, $LnEarnHat_t$, based on that person's industry, occupation and state. I do this separately for predicted earnings in 1987-1988 and in 1992-1994. Next, I calculate the average of this variable for each PUMA for each time period, yielding $PumaLnPred_t$.

Step 5: Rescaling of $PumaLnPred_t$

To test the predictive power of $PumaLnPred_t$, I regress both $PumaLnPred_1$ and $PumaLnPred_2$ on PUMA-level ln earnings in 1989 (from the 1990 Census) for all the PUMAs that occur in the NSFH data. Though both regressions have a reasonably high R^2 (0.75 and 0.76 respectively), the coefficient on 1989 PUMA ln 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 scale both predictors up by a factor (2.41) such that a regression of the average of $PumaLnPred_1$ and $PumaLnPred_2$ on actual PUMA ln 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 result in the paper. In the rest of the paper, I refer to the rescaled predictor of PUMA ln earnings as *PUMA ln earnings (predicted)* or, when there is no risk of confusion, *PUMA ln earnings* for short.

Validation using NSFH data

As a check on both the PUMA-level and industry × occupation-level predictors of earnings, I regress ln 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 3.7 percent of the individual-level earnings variation. The second row shows that results are similar if we use the actual average ln 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 results are similar for each wave separately and also hold up if state fixed effects are included (not reported).

Row three repeats this check for the measure of industry × occupation earnings constructed in step 3 above. This measure is highly significant and has a coefficient reasonably close to one. Row four shows that results remain similar if we instead use ln 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.

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)	*PUMA ln earnings (predicted)*	Baseline	0.753	0.04	0.0369	14845
(2)	*PUMA ln earnings (actual, 1989)*	Actual 1989, not predicted	0.941	0.05	0.0425	14845
(3)	*Industry×occupation ln earnings*	Baseline	0.859	0.02	0.2732	13980
(4)	*Census Industry×occupation ln earnings in 1989*	Average 1989 ln earnings by ind. × occ. from 1990 Census	0.721	0.02	0.2513	12541

In specifications (1) and (2), standard errors are corrected for clustering at the PUMA level while in specifications (3) and (4) they are adjusted for clustering at the industry × occupation level.

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TABLE I

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.
PUMA ln earnings (predicted)	**-0.239****	**0.066**	**-0.248****	**0.083**	**-0.296****	**0.076**
Ln Household Income	0.123**	0.020	0.111**	0.024	0.361**	0.102
Ln Value of home	0.068**	0.021	0.073**	0.025		
Renter	-0.172**	0.032	-0.209**	0.038		
Ln Usual working hours	-0.072	0.044	-0.113**	0.036	-0.138**	0.052
Unemployed	-0.431**	0.126	-0.254**	0.115	-0.355**	0.150
Not in the labor force	0.141**	0.043	0.059	0.043	0.240**	0.065
Female			-0.044	0.034		
Age	-0.031**	0.005	-0.029**	0.007	-0.038**	0.008
Age^2/100	0.033**	0.006	0.031**	0.007	-0.040**	0.008
White (omitted)						
Black	-0.044	0.060	0.019	0.068	-0.025	0.065
Hispanic	0.277**	0.069	0.197**	0.080	0.297**	0.071
Asian	-0.047	0.117	-0.125	0.131	-0.011	0.117
Other race / ethnicity	-0.115	0.212	0.153	0.396	-0.072	0.206
Years of education	0.010	0.007	0.010	0.007	-0.006	0.012
Ln Household size	-0.180**	0.036	-0.199**	0.042	-0.188**	0.037
Catholic (omitted)						
No religion	-0.168**	0.060	-0.161**	0.062	-0.162**	0.060
Jewish	-0.289**	0.098	-0.272**	0.111	-0.280**	0.098
Baptist	0.112**	0.042	0.050	0.047	0.118**	0.042
Episcopalian	-0.090	0.091	-0.027	0.099	-0.089	0.093
Lutheran	0.014	0.057	-0.106	0.066	0.009	0.057
Methodist	0.035	0.046	0.051	0.052	0.032	0.047
Mormon	0.041	0.108	-0.091	0.128	0.039	0.110
Presbyterian	-0.018	0.074	-0.109	0.079	-0.016	0.074
Congregational	0.018	0.097	-0.097	0.102	0.041	0.100
Protestant, no denomination	0.052	0.106	0.038	0.078	0.071	0.106
Other Christian	0.096**	0.048	0.017	0.054	0.107**	0.050
Other religions / missing	0.049	0.104	0.066	0.120	0.076	0.107
Ln Metropolitan area population	-0.005	0.014	-0.000	0.018	-0.007	0.014
Non-metropolitan area	-0.036	0.040	-0.058	0.050	-0.047	0.040
Fraction black in PUMA	-0.200	0.123	-0.213	0.154	-0.252**	0.125
State fixed effects	Yes		Yes		Yes	
Adjusted R^2	0.0388		0.0247		…	
Number of observations	8944		8023		8944	

Significance levels: *: 10 percent; **: 5 percent. Robust standard errors adjusted for clustering on PUMAs (1000 clusters in specifications (1)

and (3); 974 clusters in specification (2)). *Ln usual hours*, *Ln home value* and *Ln Metropolitan area population* are demeaned. All regressions also include dummy variables for independent variables with missing values and for logarithms of dollar values smaller than $100/year. *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 II

Testing for Selection

Dependent variable: *Self-reported happiness* Specification:	*PUMA ln earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	*PUMA ln housing price* Coeff.	S.E.	Adj. R^2	N
(1) Baseline	-0.239**	0.066	0.123**	0.020			0.0388	8944
(2) Individual fixed effects	-0.369**	0.165	0.054	0.053			0.3133	8944
(3) Observations remaining in same PUMA	-0.224**	0.084	0.129**	0.024			0.0423	6894
(4) Observations remaining in same PUMA and individual fixed effects	-0.414	1.460	0.046	0.065			0.3172	6894
(5) Using predicted household income instead of household income and home value	-0.164**	0.066					0.0333	8944
(6) Controlling for PUMA ln housing price (adjusted for housing characteristics)	-0.225**	0.079	0.123**	0.020	-0.018	0.057	0.0387	8944

Significance levels: *: 10 percent; **: 5 percent. Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in Table I, 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 III

Robustness Checks

Dependent variable: *Self-reported happiness* Specification:	*PUMA ln earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) Baseline	-0.239**	0.066	0.123**	0.020	0.0388	8944
(2) Actual *PUMA ln earnings in 1989* as control (instead of predicted *PUMA ln earnings*)	-0.333**	0.085	0.123**	0.020	0.0390	8944
(3) *PUMA ln income in 1989* as control (instead of predicted *PUMA ln earnings*)	-0.241**	0.069	0.122**	0.020	0.0387	8944
(4) *Ln earnings of R and spouse* as control (instead of *Ln HH income*)	-0.221**	0.066	0.080**	0.017	0.0387	8944
(5) Full balanced panel sample (including non-married individuals)	-0.145**	0.051	0.127**	0.017	0.0655	15568
(6) Controlling for 5th order polynomial in *Ln household income*	-0.243**	0.066	0.161**	0.030	0.0391	8944
(7) Ordered probit	-0.228**	0.065	0.112**	0.020	0.0126	8944

Significance levels: *: 10 percent; **: 5 percent. Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in Table I, column 1. The regression in specification (5) also includes four marital status dummies as controls. The terms in the polynomial in specification (6) 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*.

TABLE IV

Other Outcome Measures

Dependent variable:	*PUMA ln earnings* Coeff.	S.E.	*Ln HH income* Coeff.	S.E.	Adj. R^2	N
(1) Frequency of open disagreements about:						
a. money	0.189**	0.057	-0.116**	0.019	0.1478	9125
b. household tasks	0.084	0.057	-0.060**	0.016	0.1184	9122
c. the children	0.068	0.076	-0.045	0.030	0.0533	8522
d. sex	0.046	0.062	-0.053**	0.017	0.0863	9044
e. spending time together	0.027	0.065	-0.058**	0.021	0.0925	9120
f. in-laws	-0.008	0.057	-0.053**	0.025	0.0677	9096
(2) *Depression index (sum of 12 Radloff items)*	-0.570	0.856	-1.345**	0.255	0.0681	8782
(3) *Health status relative to age group*	-0.0002	0.041	0.097**	0.013	0.1503	9173
(4) *Satisfaction with* (wave 2 only):						
a. *amount of leisure time*	-0.274**	0.106	0.126**	0.037	0.2373	5010
b. *friendships*	-0.180**	0.085	0.060**	0.026	0.0595	5008
c. *sex life*	-0.171	0.104	0.141**	0.037	0.0300	4965
d. *financial situation*	-0.158	0.100	0.580**	0.044	0.2120	5006
e. *health*	-0.155*	0.090	0.179**	0.031	0.0515	5011
f. *home*	-0.151	0.100	0.078**	0.030	0.1478	5012
g. *family life*	-0.119	0.079	0.057**	0.027	0.0410	5010
h. *present job*	-0.097	0.117	0.259**	0.044	0.0606	4323
i. *physical appearance*	-0.093	0.083	0.063**	0.030	0.0444	5009
j. *neighborhood*	0.152	0.125	0.058*	0.035	0.0966	5010
k. *city or town*	0.366**	0.119	0.061*	0.036	0.0768	5008
(5) *Financial worries* (wave 2 only)	0.082	0.069	-0.389**	0.034	0.2262	5001
(6) *Labor supply (hours/week)*	2.224**	1.007	...		0.3041	8944

Significance levels: *: 10 percent; **: 5 percent. Robust standard errors are adjusted for clustering at the PUMA level. All regressions include the same controls as the baseline regression reported in Table I, column 1, except for specification (6), which does not include controls for own income, hours worked, unemployment and labor force participation. 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." Specification (2) uses only data from the main respondent because the depression questions were only asked of the main respondent in all waves. 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." Self-reported satisfaction is measured on a seven-point scale where 1 denotes "*very dissatisfied"* and 7 denotes "*very satisfied*". 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."

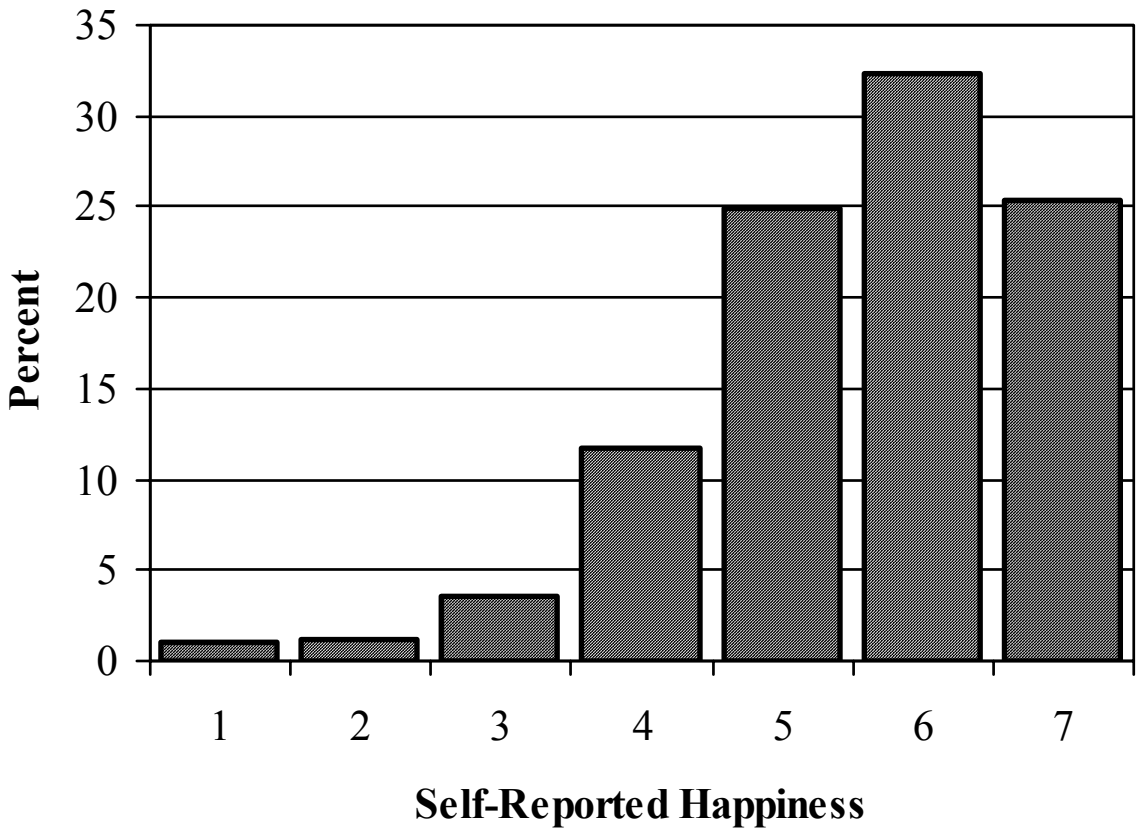
TABLE V

Interaction Effects

Dependent variable: *Self-reported happiness* Specification:	*PUMA ln earnings* Coeff.	S.E. [p-value]	*Ln HH income* Coeff.	S.E. [p-value]	Adj. R^2	N
(1) Marital status transitions					0.0537	14500
Remains married or cohabiting	-0.186**	0.076	0.122**	0.023		[8023]
Remains divorced or separated	-0.363**	0.155	0.157**	0.049		[1524]
Remains widowed	-0.103	0.182	0.087	0.058		[1032]
Remains never married	-0.076	0.171	0.122**	0.051		[995]
Marital status change	0.035	0.095	0.123**	0.034		[2926]
P-value on test of equal coefficients		[0.153]		[0.924]		
(2) Household income is …					0.0385	8944
below the PUMA median	-0.238**	0.095	0.119**	0.035		[3401]
above the PUMA median	-0.237**	0.074	0.124**	0.036		[5543]
P-value on test of equal coefficients		[0.993]		[0.915]		
(3a) Socialize with a neighbor					0.0416	8944
Less than once a month or missing	-0.161**	0.080	0.132**	0.026		[5076]
Once a month or more frequently	-0.335**	0.075	0.108**	0.027		[3868]
P-value on test of equal coefficients		[0.038]		[0.502]		
(3b) Socialize with relatives					0.0399	8944
Less than once a month or missing	-0.176**	0.086	0.145**	0.034		[2927]
Once a month or more frequently	-0.267**	0.074	0.109**	0.023		[6017]
P-value on test of equal coefficients		[0.300]		[0.353]		
(3c) Socialize with friends who live outside the neighborhood					0.0393	8944
Less than once a month or missing	-0.263**	0.090	0.157**	0.031		[4119]
Once a month or more frequently	-0.217**	0.075	0.088**	0.026		[4825]
P-value on test of equal coefficients		[0.644]		[0.092]		
(3d) Socialize with people one works with					0.0392	8944
Less than once a month or missing	-0.221**	0.077	0.140**	0.023		[6325]
Once a month or more frequently	-0.272**	0.089	0.065*	0.036		[2619]
P-value on test of equal coefficients		[0.607]		[0.077]		

Significance levels: *: 10 percent; **: 5 percent. Robust standard errors are adjusted for clustering at the PUMA level. Each specification is a single OLS regression in which *PUMA ln 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 I, column 1. All specifications use the baseline sample consisting of individuals married or cohabiting in both waves except for specification (1), where only information from the main respondent is used (in order allow for marital status transitions). 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. 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.

Figure I



Distribution of Self-Reported Happiness