Relative Status and Well-Being: Evidence from U.S. Suicide Deaths

Mary C. Daly[a], Daniel J. Wilson[a], and Norman J. Johnson[b]

First Draft: May 2006
Current Draft: April 2007

Please cite as:
Daly, Mary C., Daniel J. Wilson, and Norman J. Johnson. "Relative Status and Well-Being: Evidence from U.S. Suicide Deaths." Federal Reserve Bank of San Francisco Working Paper, March 2007.

[a] Federal Reserve Bank of San Francisco
[b] U.S. Census Bureau

Corresponding author's email: Mary.Daly@sf.frb.org

We gratefully acknowledge research funding provided by the National Institute of Aging. This paper benefited from helpful comments from Sandy Black, Andrew Clark, Dora Costa, John Ham, Justin Wolfers, and seminar participants at SOLE 2006, Federal Reserve Bank of New York, and U. of Nevada-Reno. We thank Ann Lucas, Jeremy Gerst, and Eric Backlund for excellent research assistance. Research results and conclusions expressed are those of the authors and do not necessarily indicate concurrence by the National Institute of Aging, the Bureau of the Census, the Federal Reserve Bank of San Francisco, or the Federal Reserve System.

Relative Status and Well-Being: Evidence from U.S. Suicide Deaths

Abstract:

This paper empirically assesses the theory of interpersonal income comparison using individual level data on suicide deaths in the United States. We model suicide as a choice variable, conditional on exogenous risk factors, reflecting an individual's assessment of current and expected future utility. Our empirical analysis considers whether suicide risk is systematically related to the income of others, holding own income and other individual factors fixed. We estimate proportional hazards and probit models of the suicide hazard using two separate and independent data sets: (1) the National Longitudinal Mortality Study and (2) the Detailed Mortality Files combined with the 5 percent Public Use Micro Sample of the 1990 decennial census. Results from both data sources show that controlling for own income and individual characteristics, individual suicide risk rises with reference group income. This result holds for reference groups defined broadly, such as by county, and more narrowly by county and one demographic marker (e.g., age, sex, race). These findings are robust to alternative specifications and cannot be explained by geographic variation in cost of living, access to emergency medical care, mismeasurement of deaths by suicide, or by bias due to endogeneity of own income. Our results confirm those using self-reported happiness data and are consistent with models of individuals' preferences regarding interpersonal income comparison, consistent with an "external habit" or "Keeping Up with the Joneses" model of utility.

Keywords: Relative income, interpersonal comparisons, interdependent preferences, suicide, happiness, Keeping Up with the Joneses.

JEL Codes: I31, D6, H0, J0

Relative Status and Well-Being: Evidence from U.S. Suicide Deaths

I. Introduction

The idea that individuals compare themselves to others is not a new one in economics. In fact, reference to relative position can be found in Adam Smith (1759). While more recent empirical studies point to a role for relative income in the utility function, the generalization of these findings and their incorporation into standard economic theory has been hampered by concerns regarding the measurement of relative utility that is based on subjective surveys or recovered from artificial experimental environments.

We explore an alternative and more objective measure of utility (disutility)–suicide deaths. Treating suicide as a choice variable regarding current life satisfaction and assessed value of future life, we develop a theoretical framework showing how suicide outcomes can be used to reveal preferences in the utility function. We then estimate empirical models derived from this framework and show that, holding own income and other characteristics constant, individual suicide risk is systematically related to the income of distinct reference groups. These findings support those from studies using more subjective happiness surveys or experimental data. With these findings in hand, we expand the literature on interpersonal comparisons by examining the magnitude and scope of the average effect across the income distribution and by considering the relative importance of competing reference groups.

We rely on two independent individual level data sets in our empirical work: the National Longitudinal Mortality Study (NLMS) and (2) the Detailed Mortality Files (DMF) combined with the 5 percent Public Use Micro Sample (PUMS) of the 1990 decennial census. The results from both data sets strongly support the idea that individual utility is affected by relative income. Specifically, we find that local area (county) income, holding own income constant, has a statistically and economically significant effect on suicide risk–suicide risk rises as reference group income rises. Importantly, this effect seems to persist across the income distribution. While we find a steep negative gradient in the effect of own income on suicide risk there is little difference in the magnitude of the effect of reference group income across individuals at different points in the income distribution. We further find that narrowing the reference group from county to county*race does not noticeably change the estimated reference income effect, though narrowing to county*age does appear to increase the effect somewhat. Lastly, we find little or no effect of state income on individual suicide risk suggesting that one's reference group is geographically limited.

The remainder of the paper is organized as follows. In Section 2 we review the empirical work on relative income and utility and discuss how information on suicide fits into and expands the literature. We lay out our theoretical framework and motivate our empirical strategy in Section 3. The data sets we construct and use are described in Section 4. In Section 5, we present our main results and assess their robustness. A summary of our findings and the path for future work are laid out in Section 6.

2. Previous Research

2.1 Existing Literature

Following early recognition of the importance of relative comparisons by Adam Smith, several economists have composed fuller treatments of the issue, including Veblen (1899), Duesenberry (1952), Easterlin (1974), Abel (1990), Gali (1994), Kahneman and Tversky (1996), Frank (2000), Becker and Rayo (2005), and others. These models of interdependent preferences generally posit that individuals care about their own socioeconomic status (generally defined by income, consumption, or wealth), and that of others. A growing empirical literature on the subject has found evidence consistent with this view. Empirical investigations generally can be grouped into two types. The first set consists of controlled experiments contrived to elicit participants' reactions to imposed hierarchies.

In these experiments, performed on human and primate subjects, researchers have looked for the subjects' negative reactions to the presence of a hierarchy, i.e., "inequality aversion," and for reactions to subjects' relative placement within a hierarchy, i.e., "interdependent preferences" (Engelmann and Strobel 2004; Brosnan and deWaal 2003; Alpizar, Carlsson, and Johansen-Stenman 2005). Although such experiments consistently find that inequality and relative income matter, the relatively small sample sizes and artificial environments of these experiments make their results difficult to generalize. Moreover, their contrived nature frequently makes it difficult to distinguish inequality aversion from relative income.

A second vein of the literature on interpersonal income comparisons comes from research on responses to questions from subjective well-being (happiness and/or life satisfaction) surveys. A number of researchers have used the responses from these surveys to study the extent to which self-reported happiness or satisfaction is correlated with relative position, holding other factors such as own income constant.[1] For example, Clark and Oswald (1996) use data on 5,000 British workers to investigate whether worker satisfaction rates are inversely related to relative wages. A similar examination is done in Brown, et al., and Qian (2005), focusing on relative rankings of workers' wages rather than the relative wage ratio. Both studies find evidence that relative income matters to self-reported satisfaction. Along the same lines, several papers have expanded the potential reference group to which individuals are compared by combining individual data on happiness and income with variables on local, regional, and national income (Helliwell 2002; Luttmer 2005; Tomes 1986; and Ferrer-i-Carbonell 2005). In general, these papers have found empirical support for the hypothesis of interpersonal income comparisons.

Still, serious concerns have been raised about the quality of data on self-reported happiness

[1]There also is a recent cross-national literature using surveys of happiness. These studies generally compare average reported happiness to average income across countries and find little correlation (Di Tella, MacCulloch, Oswald 2001; Alesina, Di Tella, and MacCulloch 2001; Easterlin 1973, 1995; Oswald 1997).

(see, e.g., Brekke 1997 and Osmani 1993; see Bertrand and Mullainathan 2001 for a broader critique of subjective survey data). These concerns involve language ambiguities (respondents may not all agree on the exact meaning of terms like “happiness” and “life satisfaction”), scale comparability (one person’s “very satisfied” may be higher, lower, or equal to another person’s “satisfied”), ambiguity regarding the time period over which respondents base their answers, and respondent candidness.

Although the results of these two types of studies seem to confirm a role for theories of interdependent preferences, concerns about how representative the underlying data are have hindered broader acceptance of the results. The suggestive findings coupled with concerns about experiments and self-reported measures of happiness suggest that additional methods of addressing the role of relative income are needed.

2.2 Suicide Data as an Alternative

Suicide data provide an alternative measure of happiness (unhappiness) with several advantages over experiments or surveys of happiness.[2] First, suicide can be thought of as a revealed choice made by individuals who have examined the value of continuing to live versus exiting. In studies of consumer choice, using observed choices to infer preferences has long been preferable to relying on individual self-reports of preferences. Second, suicide data are comparably measured across individuals and regions and over time. Third, in the United States, data on suicides are publicly available and complete, covering the universe of reported suicides by year.[3]

Despite these advantages, suicide data obviously are not perfect. Suicide victims presumably

[2]As Oswald (1997) puts it, “Suicides represent choices in response to (un)happiness that are intrinsically more compelling than replies made to happiness survey questions, and data that, by their nature, cannot be generated in a laboratory experiment.”

[3]Reported suicides may undercount all true suicides; many experts believe that a significant share of true suicides are misclassified as accidents or “undetermined injuries” (see Moyer, Boyle, and Pollock 1989; Rockett and Smith 1999; and Mohler and Earls 2001). We address this possibility in our empirical analysis.

are at the extreme tail of the distribution of life satisfaction over the population, and their preferences may not reflect the preferences of the non-suicide population. We address this concern by comparing our empirical results to those obtained by studies using subjective survey data. Our results are consistent with these previous studies, reinforcing the reliability of inferences based on suicide data. It also is possible that suicide decisions are largely idiosyncratic and unrelated, systematically, to the variables that affect happiness or life satisfaction. While this concern cannot be eliminated *a priori*, if it is binding we should find no correlation between relative income and suicide risk–a non-finding.

We are not the first to consider the influence of economic variables in general on suicide risk. Hamermesh and Soss (1974) develop an economic theory of suicide and, using cross-country and cross-state data, find that suicide risk is significantly related to unemployment and decreases in permanent income. More recently, Ruhm (2000) considers suicide as one of several causes of death and finds that, unlike other negative health outcomes that decline during times of recession, suicide risk is either increased or unaffected. In other work, Helliwell (2004) investigates the empirical association between subjective well-being and suicide rates using cross-country panel data and finds a strong negative relationship.[4] In a related survey article on happiness and economic factors, Oswald (1997) notes that many variables positively (negatively) associated with reported happiness are negatively (positively) associated with suicide risk.[5] To our knowledge, though, we are the first to use information on suicide risk to study the existence and nature of interpersonal comparison.

---

[4]Similarly, Koivumaa-Honkanen, et al. (2001) find that individual self-reports of life satisfaction have significant predictive power for suicide over the subsequent 20 years.

[5]Other recent examples of economists trying to explain suicide behavior include Cutler, Glaeser, and Norberg (2000), Brainerd (2001), Marcotte (2003), Stevenson and Wolfers (2000), Chuang and Huang (1997), Huang (1996), Kimenyi and Shughart (1986), Hamermesh (1974), and Schapiro and Ahlburg (1982-83). There have also been a number of recent studies in the psychiatry and public health literatures exploring the empirical links between suicide and socioeconomic factors (see, e.g., Blakely et al. 2003, Lewis and Sloggett 1998, and Kposawa 2001).

3. Theoretical Framework and Empirical Strategy

In this section we describe the theoretical framework underpinning our empirical analysis. The theoretical framework embeds a dynamic programming model of the individual's suicide decision within the random utility model (RUM), the standard economic framework for modeling discrete choice. We then lay out the empirical formulation and estimation strategy.

3.1 Theoretical Model

We begin with the premise that individuals regularly assess the value of future life against the current value of exit. For the vast majority of the population, this evaluation never nears the lower threshold defining exit but rather determines the extent to which someone experiences being happy or not so happy. For the marginal person, the threshold is binding and the evaluation ends in suicide. Assuming that the factors affecting this continuum of happiness can be evaluated using the subset of individuals who actually cross the suicide threshold (marginal individuals), we can use data on suicide deaths to directly test hypotheses related to the utility function, such as the extent to which neighbors' income or socioeconomic status affects utility.

This intuition fits nicely into the framework of a random utility model. The basic random utility model, $u(\mathbf{x}_{it}) + \theta_i$, lets contemporaneous utility for individual i in period t be a function of a deterministic component, $u()$, and an unobserved idiosyncratic component, θ_i, which can be treated as a random variable.[6] The deterministic component $u()$ depends on various state variables contained in the vector $\mathbf{x}$. As is standard, we assume $u()$ is monotonic in each variable in $\mathbf{x}$, strictly concave, and twice-differentiable. Mathematically, an individual's decision in each period whether or not to commit suicide can be expressed by the following dynamic programming problem:

[6]Our formulation of the random utility model happens to be quite consistent with theories in the fields of "positive psychology" (Peterson, Park, and Seligman 2005) and behavioral genetics (Lykken, 1999), which argue that happiness is a function of an individual-specific set point (analogous to θ_i), exogenous life conditions, and choice variables like volunteering and leisure time.

$$V(\mathbf{x}_t \mid \theta_i) = \max_{S_{it}} \left\{ (1 - S_{it}) \left[u(\mathbf{x}_{it}) + \theta_i + \beta_i E_t V(\mathbf{x}_{i,t+1} \mid \theta_i) \right] \right\} \tag{1}$$

subject to the boundary conditions

$$V(\mathbf{x}_{iT} \mid \theta_i) = 0;\ S_{it} = 1 \Rightarrow (S_{i,t+s} = 1)\ \forall\ s > 0 .^{7}$$

In the above equations, S_{it} is the choice variable. It takes on the value 1 if suicide is chosen, 0 otherwise. The objective function above assumes that the individual receives zero instantaneous utility if he or she chooses suicide. Setting the instantaneous utility from suicide equal to zero is an innocuous normalization; individual differences in this utility value can be thought of as part of θ_i, the individual-specific component of utility.[8] β is the discount factor and $E_t V(\mathbf{x}_{i,t+1} | \theta_i)$ is the expected value of future utility given this period's decision, S_t. Hereafter, let V_{it} denote $V(\mathbf{x}_{it} | \theta_i)$.

The first boundary condition states that the value function, V, is equal to zero in the final period, T, of the individual's natural life span. The second boundary condition states that suicide is an irreversible decision: choosing death today guarantees death in all future periods.

This dynamic programming problem can be solved recursively. Let $V_{i,t+1}^{c}$ denote the maximal value in period t+1 conditional on $S_{it} = 0$, and let $S_{i,t+1,t}$ denote the expectation as of t of the suicide decision next period conditional on $S_{it} = 0$, i.e., $S_{i,t+1,t} = E_t [S_{it+1} | S_{it} = 0]$. This can be thought of as the individual's self-assessed probability of committing suicide next period. The solution for any period t

[7] In the present setup, there is no explicit role for age. To incorporate age effects, as in Hamermesh and Soss (1974), the Bellman equation could be augmented as follows to account for the probability of dying next period (by nonsuicide):

$$V(\mathbf{x}_{it} \mid \theta_i) = \max_{S_{it}} \left\{ (1 - S_{it}) \left[u(\mathbf{x}_{it}) + \theta_i + \beta_i P(T-t) E_t V(\mathbf{x}_{it+1} \mid \theta_i) \right] \right\},$$

where $P(T - t)$ is the probability of surviving to the next period given the individual's maximum remaining lifespan, $T - t$. It should be the case that $P(0) = 0$, $P(T\text{-}t) \geq 0$, and $P\,'(\,) > 0$. This implies that $V(\mathbf{x}_t)$ is decreasing in t, which is equivalent to age in this context. However, age could itself be a variable in **x** affecting utility. Therefore, our empirical analysis will allow for age effects with no restriction on sign.

[8] The instantaneous utility from suicide may differ across individuals and could arguably be positive or negative (relative to natural death). Religious beliefs regarding the sinfulness of suicide or compassion for mourning loved ones left behind, for instance, would yield a negative instantaneous utility from suicide. On the other hand, one might receive some satisfaction from the sympathy and attention one might receive posthumously, implying a positive value.

< T is:

$$S_{it}^{*} = \begin{Bmatrix} 1, \text{ if } u(x_{it}) + \theta_i + \beta E_t V_{it+1}^{c} < 0 \\ 0, \text{ otherwise} \end{Bmatrix}, \text{ where} \tag{2}$$

$$E_t V_{t+1}^{c} = E_t \left\{ \sum_{j=0}^{T-(t+1)} \beta^j \left[u_{t+1+j}(\mathbf{x}_{t+1+j}) + \theta_i \right] \left(1 - S_{t+1+j,t+j}\right) \right\}. \tag{3}$$

This solution equation indicates that an individual will choose suicide when present and expected future utility (conditional on not choosing suicide this period) is less than the utility received from choosing suicide (recall that θ_i is the idiosyncratic utility component net of whatever instantaneous utility, either positive or negative, one receives upon committing suicide). The conditional expected future utility is the expected value of the discounted sum of future utilities, where each future utility value is weighted by the probability of not committing suicide in that period.

A key parameter in this model is θ_i, an unobserved, individual-specific component that incorporates all unobserved exogenous risk factors that determine an individual's predisposition to commit suicide. Conditional on $\mathbf{x}_{it}$ and β, whether $u(\mathbf{x}_{it}) + \theta_i + \beta E_t V_{it+1}^{c}(\mathbf{x}_{it} \mid \theta_i)$ is positive or negative will be completely determined by the value of the idiosyncratic component θ_i. Lower levels of θ_i lower utility and increase the probability of suicide. In our framework, θ_i incorporates the various genetic, biochemical, psychological, and neurological preconditions that have been found to be prevalent in suicidal individuals.[9] More generally, the individual-specific component of the randomized utility model allows for the fact that not all individuals will receive the same level of utility from the same level of $\mathbf{x}$, even though $\mathbf{x}$ affects every individual in the same way. In other words, even if all individuals had the same levels of consumption, leisure, etc., some individuals

[9]For example, neurologists have found that suicidal patients and suicide victims tend to have particularly low cerebrospinal fluid levels of 5-hydroxyindoleacetic acid (5-HIAA), a metabolite of serotonin (serotonin transmits nerve impulses across synapses). Low levels of 5-HIAA also have been found in perpetrators of murder, arson, and other violent behavior linked to impulsiveness (see Hendin 1995). Such characteristics of the individual's brain are thus captured by θ_i. Also in θ_i would be psychological conditions such as clinical depression.

would still be "happier" than others and some presumably would still commit suicide. The predisposition factors in θ_i are assumed to be pre-determined (genetically or formatively) for the adult age range on which we focus. Under this assumption, θ_i can be thought of as a random disturbance term that is predetermined and orthogonal to x. For a given $\mathbf{x}_{it}$, there exists a critical value $\theta^*(\mathbf{x}_{it})$ below which $u(\mathbf{x}_{it}) + \theta_i + \beta E_t V^c_{it+1}(\mathbf{x}_{it} \mid \theta_i)$ is negative and the individual chooses to commit suicide. The effect of any variable contained in the $\mathbf{x}_{it}$ vector on the suicide decision can be found by partially differentiating $\theta^*(\mathbf{x}_{it})$ with respect to that variable using the implicit function theorem[10]:

$$\frac{\partial \theta_t^*}{\partial x_{it}} = -\left[\underbrace{\frac{\partial u_t}{\partial x_{it}}}_{\mathrm{A}} + \beta \underbrace{\frac{\partial E_t V^c_{t+1}}{\partial x_{it}}}_{\mathrm{B}}\right] / \frac{\partial F}{\partial \theta_t^*}, \tag{4}$$

where from (3) we get

$$\frac{\partial E_t V^c_{it+1}}{\partial x_{it}} = \sum_{j=0}^{T-(t+1)} \beta^j \left[\frac{\partial u_{t+1+j}}{\partial x_{it}}\left(1 - S_{i,t+1+j,t+j}\right) - \frac{\partial S_{i,t+1+j,t+j}}{\partial x_{it}} u_{i,t+1+j}\right].$$

By construction, $\partial F / \partial \theta_t^* > 0$, so the sign of $\partial \theta_t^* / \partial x_{it}$ will equal the negative of the sign of the sum of the two expressions in the brackets of equation (4), which we've labeled **A** and **B**. **A** represents the effect of the variable x_{it} on contemporaneous utility; we call this the contemporaneous effect. **B** represents the effect that x_{it} has on future values of x_i as well as other variables in **x**; we call this the signal effect. An example of the signal effect is that current income may serve as a signal of future income; thus, part of the negative effect of current income on suicide risk that we estimate in our forthcoming regressions may be due to the fact that current income is thought to be a good signal of future income and expectations of high future income may lower current suicide risk. For the

[10]The implicit function is the solution for θ^* of the equation:
$F(x_t, \theta^*) = \left(u(\mathbf{x}_{it}) + \theta^* + \beta E_t V^c(\mathbf{x}_{it+1} \mid \theta^*)\right) = 0$.

variables considered in the empirical analysis of this paper, the signal effect is likely to be small or near zero and hence the overall effect will be dominated by the contemporaneous effect. Nonetheless, it is important to recognize that the effects of variables on utility and suicide that we identify represent the combined effect of these two forces.[11]

The probability that an individual commits suicide is simply the probability that his/her θ_i is below the threshold level $\theta^*(\mathbf{x}_i)$. Letting G denote the CDF of θ_i, then $\Pr[\theta_i < \theta^*(\mathbf{x}_i)] = G[\theta^*(\mathbf{x}_i)] = F(\mathbf{x}_i)$. Parameterizing $F(\mathbf{x}_i)$ a researcher equipped with individual level data on suicide decisions and the variables in $\mathbf{x}$ can estimate the average effect of any variable in $\mathbf{x}$ on the likelihood of suicide, and thus infer the effect of the variable on utility.[12] In the empirical section below, we estimate models based on two alternative parameterizations. The first is the proportional hazards model,

$$h(\tau \mid \mathbf{x}_{i,0}) \equiv \frac{f(\tau)}{1-F(\tau)} = \psi(\tau)\exp\left(\mathbf{x}'_{i,0}\beta\right), \tag{5}$$

where $h()$ is the suicide hazard at duration τ from when $\mathbf{x}_{i,0}$ is observed. The second parameterization is the probit model,

$$F(\mathbf{x}_i) = \Phi(\mathbf{x}'_i\beta), \tag{6}$$

where $\Phi()$ is the standard normal CDF.

3.2 Empirical Strategy

Our objective is to estimate the effects of own income and reference-group income on the probability of suicide. Given the longitudinal nature of the NLMS data and the cross-sectional nature

[11]The potential existence of the signal effect would not be recognized if one modeled individual behavior as depending on a single function representing remaining lifetime utility, as in Hamermesh and Soss (1974). In that case, the effect of a variable on lifetime utility is assumed to be unambiguous.

[12]Note the disturbance term in the estimation can be interpreted as an estimate of θ_i.

of the DMF-PUMS data, we estimate a Cox proportional hazards model with the former and a probit model with the latter. The probit model essentially regresses individual suicide probability on a function of own income, reference-group income, and a set of control variables. Similarly, using the NLMS data, we estimate Cox proportional hazards models of suicide risk–i.e., the hazard rate of suicide in a given period–as an exponential function of own income, reference-group income, and a set of controls. The Cox proportional hazard models allows us to easily control for the duration that sample members are observed (exposed) and for right-censoring, that is the fact that suicide is not observed for most individuals due to either nonsuicide death or the end of the follow-up window.

4. Data and Variable Specification

This paper uses two alternative individual-level data sets, each with their own comparative advantages, to analyze the relationship between relative income and suicide. The first data set is the National Longitudinal Mortality Study (NLMS) augmented with data on county and state income from the U.S. Census Bureau. The second data set we constructed by combining the Detailed Mortality Files (DMF) for years 1989-1992, from the National Center for Health Statistics with data from the 1990 Public Use Micro Sample (PUMS), which is a 5% random sample from the 1990 decennial census data. We will refer to this data set as the DMF-PUMS data. The DMF-PUMS data have the advantage of containing a very large number of observations on suicide victims (as well as on the general population), whereas the NLMS data have a much smaller sample of suicide and nonsuicide records from which to draw inferences. On the other hand, the mortality records in the DMF data do not include income and do not identify county of residence for sparsely populated counties, whereas the NLMS contains actual, reported income and has no limitations on geographic coverage. We choose to restrict both data sets to working-age adults (20-64), for whom relative income concerns are likely to be most relevant. Each of these two data sets are described in detail below.

4.1 NLMS

The NLMS is a confidential, restricted-use database developed and maintained by the U.S. Census Bureau to facilitate research on the effects of demographic and socioeconomic factors on mortality (see U.S. Bureau of the Census 2005). It has been used extensively by epidemiologists and public health experts in recent years, for example to study cancer and heart disease, but it has not previously been used by economists (to our knowledge). The NLMS consists of a set of cohort files, primarily from Current Population Surveys (CPS), matched to the National Death Index (NDI), a national database containing the universe of U.S. death certificates since 1979. The cohort files included in our analysis–those with sufficient information on income–are all March CPS files from 1979 to 1998, plus CPS files for February 1978, April 1980, August 1980, and December 1980. At the time of this writing, the mortality follow-up (i.e., the matching to the NDI) from the cohort files covered deaths occurring from January 1, 1979, through December 31, 1998. The matching process appends to individual CPS records (1) whether the person has died within the follow-up period, (2) date of death (if deceased), and (3) cause of death (if deceased).

For our analysis, we restrict our sample to non-Hispanic working-age adults. We exclude Hispanics because of definitional changes in the Hispanic status variable over time and because of concern that a nontrivial share of Hispanic CPS respondents may have moved out of the United States prior to the end of the follow-up period, in which case their deaths would not be observed. Given the richness of information available from death certificates, we are able to identify whether a suicide victim had a diagnosed severe mental illness.[13] As our goal is to infer preferences of the general population, we exclude these (160) suicide cases from our primary sample.[14] The final data set, after

[13]Known mental illnesses are listed as "contributing factors" on the death certificate.

[14]We note, however, that results based on the full sample are quite similar to ours though the estimated coefficients generally are closer to zero (as one would expect if severely mentally ill individuals place less weight on socioeconomic factors in the utility function and/or simply behave less "rationally").

excluding a relatively small number of records with missing values for key variables, contains 957,934 total records, including 74,786 nonsuicide deaths and 1,401 suicide deaths within the follow-up period (the remainder were still alive as of December 31, 1998).

4.2 DMF-PUMS

The public use DMF, available from the Inter-university Consortium for Political and Social Research for a given year are essentially the data from all death certificates recorded in the United States in that year (see U.S. Department of Health and Human Services 1992).[15] For the years 1989-1992, we extract the records where suicide is the cause of death (i.e., International Classification of Death, Rev. 9 (ICD9) codes E950-E959) and combine them with the individual records from the PUMS 5 percent sample of the 1990 decennial census (Ruggles et al. 2004), which we treat as nonsuicide observations. We extract suicides for years other than 1990 to maximize the number of suicide observations, given that suicide is a relatively infrequent event. In our empirical analysis, we adjust for the fact that nonsuicides are undersampled relative to suicides in our data, both because we include four years of suicide records (versus one year from PUMS) and because the nonsuicides are a 5 percent sample of the overall population.

The variables jointly available in the DMF and the PUMS are age, race, sex, county and state of residence, marital status, education, and Hispanic status. Income, on the other hand, is not recorded on death certificates. We therefore estimate income by matching suicide records in the DMF to individuals or groups of individuals in the PUMS data, where income is available. The matching procedure works as follows: (1) for each suicide record, find all matching observations in the PUMS,

[15]For later years, these data are called Multiple Cause of Death files. These data are also sometimes referred to as the Mortality Detail Files.

with its roughly 7 million records, matching on county[16], age, race, sex, Hispanic status, education, and marital status; (2) calculate average family income for this matching cell; and (3) assign this average income to the suicide observation. This procedure provides a reasonably accurate estimate of income: over the 7,202,093 working-age observations in PUMS, county, age, race, sex, Hispanic status, education, and marital status jointly explain 24 percent of the individual level variation in family income.[17] A variance decomposition (not shown) reveals that county, education, and marital status (in decreasing importance) have the greatest explanatory power, together accounting for 16 percent of variation.

With this matching procedure, we are able to estimate family income for 57 percent of U.S. working-age suicide records from 1989-1992, totaling 50,328 suicides.[18] We use the same matching procedure to generate an analogous predicted income variable for the nonsuicide records; this is the "own" income variable used in our regression analyses. The final data set has 4,360,747 observations.

We additionally use the PUMS data to construct several control variables at the county-age-race-sex-education-Hispanic-marital status cell level: share of cell that owns a home, share of cell that are Vietnam veterans (found in previous research to be a correlate with suicide), and the unemployment rate within the cell. Another control we include in our analysis is a state-level measure of firearm availability from Miller et al. (2002); specifically, it is the share of suicides in the state that

[16]In order to prevent disclosure of individuals' identities, the Census Bureau does not actually provide county of residence in the PUMS for counties with population under 100,000. For individuals from such counties, the Census Bureau instead provides the Public Use Micro Area (PUMA), which is an aggregate of multiple counties. Additionally, in a small number of cases, there is a many-to-many mapping between counties and PUMAs.

[17]Including occupation and industry in the income estimation would modestly improve the model fit to 28 percent. However, less than half of the suicide records report occupation and industry (as many states do not include them on death certificates). Therefore, we omit these variables from the matching procedure.

[18]The main constraining factors here in terms of coverage are county of residence and education. Education is simply unknown or unreported on many death certificates. For confidentiality reasons, county of residence (or occurrence) is not identified on the public-use DMF data if the county has a population below 100,000. This occurs for roughly a quarter of U.S. counties in 1990, covering slightly more than a quarter of all suicides. It should also be noted that some death records include occupation and industry of the deceased, but not enough records contain this information for us to include these variables usefully in our matching procedure.

are committed via firearm. (Using the share of homicides committed via firearm yields similar results.)

5. Descriptive Statistics and Main Results

5.1 Descriptive Statistics: Suicide Risk and Model Variables

National statistics show that the U.S. suicide rate has been relatively constant since 1950, averaging about 12 per 100,000 persons (see WHO 2005).[19] Table 1 reports suicide risk overall and by our model variables for the NLMS and DMF-PUMS samples. Recall that both samples exclude Hispanics and cover only working-age adults. The overall suicide rates in the NLMS and DMF-PUMS are quite similar to each other, at approximately 13 per 100,000, and are comparable to the national statistics. Furthermore, national data indicate considerable variation in suicide risk by gender, age, and race. These patterns are mirrored in the NLMS and DMF-PUMS samples. For example, suicide rates are far higher for males than for females and higher for whites than for other races. Suicide rates decline slightly with age according to the DMF-PUMS while having no clear age trend in the NLMS sample, which may simply be due to the relatively small sample size of the NLMS. In both samples, married individuals have a lower suicide rate on average relative to those who are single/never married or divorced/separated. Suicide rates generally fall, though not monotonically, with educational attainment. Although rudimentary, these categorical suicide rates suggest that the two data sources used in our analysis produce patterns consistent with the stylized facts regarding suicide reported in the epidemiology/public health, psychology, and sociology literatures.[20]

The key variables in our analysis are own and reference group income. To assess the extent to

---

[19]From 1950 to 2000, the overall U.S. suicide rate has fluctuated within the narrow range of 10.4 to 13.5 per 100,000. The typical rate for the working-age adult population is somewhat higher, around 12 to 15 per 100,000.

[20]Partial correlations of the type reported here are actually quite prevalent in these literatures, often even the standard way of summarizing the data. Our multivariate results show that some of these stylized facts are not robust to the inclusion of additional variables.

which preferences of the general population can be inferred from the revealed preferences of suicide victims, it is helpful to first compare these two populations along the key dimension of income. Figures 1 and 2 plot the distribution of predicted family income for working-age suicide victims in our two samples against the income distribution for the general U.S. working-age population.[21] Figure 1 shows the distributions of reported income (adjusted to 1990 dollars) for the total sample and for the subset of those who eventually commit suicide, according to the NLMS data. Note that the NLMS data are survey reports reflecting income at the time the individual was surveyed rather than income at the time the suicide was committed. The income distribution of suicide victims is slightly left of that for the general population. That said, the bulk of the suicide population has income in the middle range of the distribution. We take this as supporting evidence for the notion that suicide victims are broadly representative of the general population, at least in terms of income. This will aid us when we offer an interpretation for our findings.

Figure 2 reports income figures for the DMF-PUMS sample; the figure shows the distribution of estimated family income (estimated as described in Section 4 above) of suicide victims compared to that of the general population. The estimated distributions suggest that the modal suicide victim sits slightly to the left of the modal member of the general population, but overall the two distributions are quite similar. Importantly, there is little difference in the lower tail of the income distribution and overall the shapes for the two populations are roughly similar.[22] The fact that the DMF-PUMS data show a pattern similar to the NLMS data suggests that our estimated data in the DMF-PUMS files are reasonably accurate.

Descriptive statistics for other model variables are reported in Tables A1 (NLMS) and A2

[21]Note that both the suicide and general populations in the DMF-PUMS sample exclude individuals from counties with population under 100,000, since such counties are not identified in the data for confidentiality reasons.

[22]We also did this matching using education alone and obtained similar results. Full details of both estimation strategies are available from the authors upon request.

(DMF-PUMS) of the Appendix. Again, the key variables in our analysis are of similar magnitudes and have similar patterns in both data sets.

5.2 NLMS Regression Results

In this subsection we describe the results from estimating the Cox proportional hazards model, equation (5) above, using the NLMS sample. We use Cox proportional hazard models to accommodate the unbalanced nature of the NLMS data. The NLMS data records information on individuals from the time of their initial CPS response (period 0). Thus the information is summarized in terms of duration from the original interview rather than in chronological time. The proportional hazards model allows us to characterize the suicide hazard (probability of suicide after τ periods given it has not already occurred) over the interval from 0 to T, where T is maximum duration, conditional on individual covariates recorded at period 0. In the NLMS, T is 7,633 days, which is the difference between Dec. 31, 1998, the end of the NLMS follow-up window, and Feb. 1, 1978, the date of the earliest CPS response in the sample. Note that the vast majority of observations (individuals) are censored. Observations are left-censored due either to non-suicide death prior to the end of the follow-up period or to participating in a later CPS survey than February 1978. Observations are right-censored due to the individual still being alive at the end of the follow-up period. (Note that most observations are both left- and right-censored.) The estimation procedure accounts for both left- and right-censoring.

The estimated coefficients and associated p-values for our baseline models are reported in Table 2. Before turning our attention to the estimated effects of income variables, we first briefly discuss the effects of the various control variables included in the model. The bottom portion (panel C) of Table 2 confirms that sociodemographic factors are important determinants of suicide risk.[23]

[23]In unreported regressions, we reestimated the models that follow including year dummies for years of the CPS response. The estimated coefficients on the model variables are quite similar to those reported below, though the standard errors are moderately higher. Results are available upon request.

Consistent with the raw categorical suicide rates in Table 1, being female or nonwhite lowers suicide risk, while being divorced or widowed, separated, or never married raises suicide risk (relative to being married). Given the other controls, educational attainment does not have a separate statistically significant influence on suicide risk, though the point estimates suggest a mildly negative relationship between education and suicide risk. Veterans are found to be more likely to commit suicide than nonveterans. Controlling for these other factors, labor market status has an additional influence on suicide risk; being unemployed or out of the labor force (for any reason) raises suicide risk relative to being employed.[24] The magnitude and statistical significance of the control variables is little affected by which income variables are included in the model. Thus, for the remainder of the discussion we turn our attention solely to the income variables.

The key variables in our analysis–own income and relative income–are shown in the upper portion of the table. Column 1 focuses on the importance of own income, measured as a categorical variable to highlight the importance of the income gradient in determining suicide risk.[25] The results show that individuals with family incomes below \$20,000 in 1990 dollars (which, by way of reference, is equivalent to about \$31,000 in 2006 dollars, according to the CPI-U) are significantly more likely to commit suicide than those with incomes above \$60,000 (≈\$92,500 in 2006 dollars). In contrast, for those with incomes over \$20,000, own income has no significant effect on suicide risk.[26] The point estimates of the coefficients on the categorical income variables imply hazards ratios of 1.52, 1.47,

[24]The high relative risk of suicide for unemployed individuals has been found previously using similar data (Kposawa 2001, Blakely, et al. 2003).

[25]Our results are qualitatively similar to using a specification that instead uses continuous own income and own income squared.

[26]Previous research on the individual effects of own income on suicide is inconclusive. Similar to our finding, Kposawa (2001), using an earlier version of the NLMS, found that in a multivariate regression, suicide risk decreases with income. Lewis and Sloggett (1998) and Blakely et al. (2003), however, using British and New Zealand data, respectively, found no significant effect of income after other determinants of socioeconomic status had been controlled for.

1.12, and 0.99, respectively, for income categories [0,10K], [10K, 20K], [20K, 40K], and [40K, 60K]. A hazard ratio of 1.52, for instance, means that an individual with family income less than $10,000 (in 1990 dollars) is 52 percent more likely to commit suicide (within the remaining sample window) than an individual with income above $60,000 (the omitted income category). This finding is consistent with the standard assumption of diminishing marginal utility of income/consumption. Column 2 shows the results of adding reference group income. Following previous work on interpersonal income comparisons, our baseline specification considers county of residence to be one's reference group. The results show that county income has a positive effect on suicide risk controlling for own income, implying that a loss of relative position leads to a reduction in individual happiness. This finding is consistent with the results of studies using happiness survey data. Our estimated coefficient of 0.392 on log county income implies that, holding own income constant, a 10 percent higher county income is associated with a 3.9 percent higher suicide hazard relative to the baseline hazard (conditional mean hazard).[27]

Before concluding that our baseline specification confirms the findings from happiness studies, we consider whether our results simply reflect differences in county demographic composition that are correlated with both increased suicide risk and higher county income. These results are reported in column 3. Including variables for county population shares by age and race does not alter the qualitative result; in fact, including the shares increases the magnitude of the coefficient on county income.

The robustness of the results in Table 3 strongly suggest that county income has a positive effect on suicide risk controlling for own income. However, before assigning a behavioral interpretation to this result, there are a number of alternative explanations that must be considered.

[27]The proportional hazards function is $h(t) = h(0)e^{\alpha \ln(\bar{y})}e^{X\beta}$, where $\bar{y}$ is county income and X is a vector of all other model variables. The elasticity of the hazard with respect to county income is then: $\mathrm{dlog}(h(t))/\mathrm{dlog}(\bar{y}) = \alpha \mathrm{dlog}(\bar{y})$. We estimate $\hat{\alpha}$ = 0.392.

First, it may be that own income is endogenous in the sense that there may be unobserved factors, particularly mental health status, that affect both suicide risk and own income.[28] The resulting bias on our baseline estimator could potentially bias both the coefficient on own income (likely negative) as well as the coefficient on county income (and other variables) depending on the correlation between own income and county income.[29] To assess this possibility, we use a two-stage estimator in which own income is treated as endogenous and instrumented for using state of residence dummies (along with all other variables included in the model). For simplicity, we apply this estimator to a model containing a single continuous own income variable rather than the multiple categorical income variables. The results of the baseline model in which own income is treated as exogenous is shown in column 1 of Table 3. Column 2 shows the results of the two-stage estimator. Surprisingly, the two-stage results indicate that the baseline effect of own income was biased upward rather than downward as expected: the two-stage results show a much steeper own income gradient.[30] Most importantly, though, they also show that county income remains positive and significant even after controlling for the endogeneity of own income. In fact, the coefficient on county income from the two-stage estimation is somewhat larger than in the baseline model.

A second alternative explanation for the positive effect of county income on suicide probability is that county income is proxying for cost of living and our results simply reflect that, conditional on nominal own income, individuals are made worse-off by living in areas with a higher cost of living. To test this hypothesis, we exploit the fact that house prices–the most important

---

[28]Tekin and Markowitz found that suicide ideation has a negative effect on one's wages, suggesting that mental health (illness) may have a positive (negative) effect on income. We also note that when we add the 160 observations from suicide victims for whom a mental illness was identified on their death certificate to our sample (the exclusion of these cases in our main sample is discussed in Section 4.1), the estimated own income gradient becomes steeper, suggesting that severe mental illness is negatively associated with income.

[29]Note that our probit regressions using the DMF-PUMS sample are immune from this potential bias since our own income measure is already a fitted value from a first-stage regression using PUMS data.

[30]In addition, a Durbin-Wu-Hausman test rejects the exogeneity of own income.

component of cost of living–differentially effect house owners and renters. High house prices both contribute to the wealth of house owners as well as increase their cost of living. For renters, on the other hand, high house prices only translate into high cost of living, with no offsetting effect on wealth. Thus, if county income simply proxies for cost of living, then the effect of county income on the suicide hazard of renters should be higher than the effect on owners. Column 3 of Table 3 shows the results of interacting county income with a renter dummy variable. We find no significant difference in the effect of county income on renters' suicide hazard relative to owners, refuting the notion that county income is just a proxy for cost of living.

A third potential concern is that our estimated effect of county income on suicide risk reflects spurious correlation associated with some unknown and/or unmeasured relationship between county income and mortality in general or quality of emergency medical care. Moreover, one might be concerned that the universe of reported suicides represents a selected sample of all suicides and one that is not reflective of a more comprehensive measure. The results in Table 4 are designed to address these concerns. The first column simply redisplays our baseline results from Table 2, column 3. The second column repeats the analysis adding deaths from "injuries of undetermined cause" (ICD9 codes E980-E989), which some have argued primarily capture unreported suicides, to the reported suicide records. The results are quite similar to those for reported suicides alone and confirm the notion that relative income matters for individual happiness (unhappiness).

The third column of Table 4 reports results using death rates from heart attacks (acute myocardial infarction, ICD9 code 410) as the dependent variable. Our use of heart attack deaths is meant to test whether our results on suicide risk owe to differential quality or access to emergency room care or paramedical care rather than to reactions to relative income. Research has shown that heart attack deaths are correlated with time to treatment (e.g., proximity to emergency rooms). If our results on suicide are due to unequal access to emergency rooms such that attempted suicides more frequently end in death, then we should see the same pattern for heart attack deaths. This is not the case. Indeed, while the mortality hazard from heart attacks falls monotonically with own income, as with suicide, it also falls with county income, contrary to suicide. This is consistent with work by Daly, et al. (1998) that posits that richer communities create medical infrastructures that spillover to all individuals living in the county. Finally, looking at all causes of mortality, our findings concur with the standard result in the literature (see, e.g., Miller and Paxson 2006 and Gerdtham and Johannesson 2004): mortality falls monotonically with own income and is unaffected by relative income.

Based on these results, we conclude that our finding of a positive effect of local area income on suicide, after controlling for own income, reflects a behavioral response to unfavorable interpersonal income comparisons.[31] These individual-level results are consistent with earlier, semi-aggregate results for suicide risk (Daly and Wilson 2006) and with recent empirical analyses using self-reported, subjective well-being survey data (Luttmer 2005).

We now turn to extensions on our basic specification. We first consider expanding the reference group to the state level.[32] Column 1 of Table 5 again redisplays the baseline results from Table 2, column 3, while column 2 shows the results from replacing county income per family with state income per family. The results show that state income has no significant effect on suicide risk, suggesting that the state is too large to be considered a reference group for interpersonal comparison. The third column considers whether or not the relative income effect varies over the income distribution. While the small sample size limits the statistical power in this regression, the results are suggestive of a stronger effect for those at the bottom of the income distribution relative to those at the

[31]One might worry that our results are reflecting unmeasured correlation between county income and unobserved county characteristics, such as fraction of population with untreated depression or mental illness, that also are correlated with county income. While we cannot rule this out completely, we note that concerns along these lines likely would produce a downward bias on the county income effect. For example, in the case of untreated depression, it is more likely that mental health services and treatment, which are typically provided out of local government budgets, are better funded in richer counties than in poorer counties.

[32]Consideration of reference groups at a finer disaggregation than county is not possible with our NLMS sample due to lack of income data availability over time. We do, however, investigate narrower reference groups below with our DMF-PUMS sample, which requires reference group income data only for 1990, a decennial census year.

top.

5.3 DMF-PUMS Regression Results

Despite the remarkably strong evidence of interpersonal income comparisons coming out of the NLMS data, one might still be skeptical of this result given the relatively small number of suicides in the NLMS data. To assess whether this result is unique to the NLMS sample, and hence in some way spurious, and to further explore the relevance of alternative reference group definitions, we now consider a second, separate data set we call DMF-PUMS. As described in Section 4.2, the DMF-PUMS data are produced by combining suicide records from death certificate data with individual records from the PUMS 5 percent sample of the 1990 decennial census. We estimate a probit model equation (6) of the probability of committing suicide as a function of (log) estimated own income, (log) average county income, and various controls. Note that in these models we use a pared down set of sociodemographic controls that does not include the three most important income predictors used in our estimation of family income: county, education, and marital status. Otherwise, due to multicollinearity between these variables and predicted income, there would be little independent variation with which to identify the coefficient of own income. Our strategy in these regressions thus amounts to treating estimated income as a summary statistic for socioeconomic status.

Table 6 shows results for our full non-Hispanic sample. The table reports the estimated marginal effects evaluated at the mean (adjusted for the oversampling of suicides (DMF) relative to nonsuicides (PUMS) in our sample). The first column shows a baseline regression with our control variables and estimated own income.[33] Note the conditional mean probability of suicide is estimated to be 0.000103, or 10.3 per 100,000, which is similar to the sample suicide rate (unconditional mean

[33]In robustness checks not shown here we adjusted a subset of the models for the fact that income is an estimated variable using the technique developed by Murphy and Topel (1985). In each of the cases we tried, the adjustment had a very modest effect and made no material difference in our findings.

probability) of 13.4 per 100,000 reported in Table 1. The coefficients on the control variables are consistent with our NLMS results and with previous research: suicide risk is considerably higher for males and for whites and exhibits an inverted-U age profile. The Vietnam veteran share and the unemployment rate, measured for an individual's county, age, race, sex, Hispanic status, education, and marital status cell group, both have an upward effect on suicide risk, while cell-level homeownership has a downward effect. The firearm availability in one's state (proxied by the share of suicides committed by firearm) is positively associated with suicide risk. As in the NLMS/proportional hazards estimation, we find that suicide risk is decreasing in own income. The point estimate of -0.0000497 on log income implies that 10 percent higher income is associated with a reduction of suicide probability of about 5 per 100,000, or about half of the conditional mean suicide probability (10.3 per 100,000).

Building on this baseline regression, the remaining columns in the table report results from introducing reference income values computed over different reference groups. Column 2 shows the results of introducing county-level income. We confirm the key finding from the NLMS sample: holding own income constant, suicide risk is increasing with average county income. Both own and reference group income are statistically significant at conventional levels. Moreoever, it is worth noting that the point estimates on (log) own income and (log) county income are of roughly equal magnitudes, suggesting that the an equiproportional increase in all incomes would have little effect on the overall suicide rate.

The next two columns examine the effect of changing the reference group definition–in this case narrowing it. The results suggest that, while others in one's county or others of the same race in one's county are relevant reference groups, others in the same age range in one's county may be the most relevant reference group. The final column shows that the average state income has a positive but statistically insignificant effect on an individual's suicide risk–again, consistent with the NLMS results–implying that the relevance of others in one's reference group declines with distance and one's effective reference group may be geographically narrower than the state.

6. Summary and Conclusions

Using individual level data on suicide risk, we find compelling evidence in support of the idea that individuals care not only about their own income but also about the income of others in their local area. This finding is obtained using two separate and independent data sets, proving that it is not an artifact of the particular sample design of either data set. Furthermore, the finding is robust to alternative specifications and cannot be explained by geographic variation in cost of living, access to emergency medical care, or suicide reporting, or by bias due to the endogeneity of own income. In addition, it is worth noting that other plausible stories of potential bias that we are unable to test generally imply a *downward* bias on county income. For instance, it is possible that the quality of local mental health care negatively affects suicide hazard and is positively correlated with county income, leading to a downward bias on county income's effect on suicide. Another possibility is that individuals are mobile and endogenously select their county of residence in response to their income relative to the county's average. This would suggest that suicide outcomes underestimate the true relevance of interpersonal income comparisons because individuals are able avoid the negative utility impact of low relative income by simply moving to a location where they have higher relative income. More generally, any story involving classical measurement error in our reference group income measures (relative to the unobserved true reference income) will imply attenuation bias (toward zero). Finally, regarding the proportional hazards estimations, a common concern in such survival analysis is attentuation bias from unobserved individual heterogeneity. The concern is that individuals with especially negative individual effects ("frailty" in the parlance of survival analysis)–i.e., the θ_i term in our theoretical model–are more likely to exit the sample early via suicide; since there are no observations from these individuals for the remaining years of the sample, they receive less weight than survivors in the estimation, hence underestimating the effects of all variables on exit probability. Again, though, this bias only argues that the true effect of reference group income is in fact larger than what we find.

Our results confirm those obtained in semiaggregate analysis (Daly and Wilson 2006) on group suicide risk and income dispersion and also are broadly consistent with results using happiness surveys. The finding that reference income, holding own income constant, increases in suicide risk holds for reference groups ranging from simple geographic areas to near neighbors (evaluated as living in the same county and having one demographic marker in common) to simple geographical areas like county, with some evidence that age is particularly relevant for comparisons. State appears to be too broad as a measure of reference group.

This paper has focused on static interpersonal income comparisons. Models of this kind are known by various names such as "external habit formation" and "Keeping Up with the Jones". Future research using suicide data may consider dynamic models of preferences such as "internal habit formation" or "Catching Up with the Jones". The evidence in this paper regarding the usefulness of suicide data for evaluating the nature of the utility function and preferences suggests that such research could indeed be fruitful.

References

Abel, Andrew B. “Asset Prices under Habit Formation and Catching up with the Joneses.” *American Economic Review*, May 1990, 80(2), pp. 38-42.

Adler, Nancy E., W. Thomas Boyce, Margaret A. Chesney, Susan Folkman, and S. Leonard Syme. “Socioeconomic Inequalities in Health. No Easy Solution.” *JAMA*, June 23-30, 1993, 269(24) pp. 3140-5.

Alesina, Alberto, Rafael Di Tella, and Robert J. MacCulloch. "Inequality and Happiness: Are Europeans and Americans Different?" *Mimeo*, 2001.

Becker, Gary S. and Luis Rayo. “Evolutionary Efficiency and Happiness.” Mimeo, University of Chicago, 2005.

Bertrand, Marianne, and Sendhil Mullainathan. "Do People Mean What They Say? Implications for Subjective Survey Data." *American Economic Review*, 2001, 91(2), pp. 67-72.

Blakely, T. A., S. C. D. Collings, and J. Atkinson. "Unemployment and Suicide: Evidence for a Causal Association?" *Journal of Epidemiology and Community Health*, 2003, 57, pp. 594-600.

Brainerd, Elizabeth. "Economic Reform and Mortality in the Former Soviet Union: A Study of the Suicide Epidemic in the 1990s." *European Economic Review*, 2001, 45, no. 4-6, pp. 1007-19.

Brekke, KA. (1997), *Economic Growth and the Environment: On the Measurement of Income and Welfare* (Cheltenham, Brookfield: Edward Elgar).

Brown, Gordon D.A., Jonathan Gardner, Andrew Oswald, and Jing Qian. "Does Wage Rank Affect Employees’ Well-being?" *IZA Discussion Papers,* March 2005, 1505, Institute for the Study of Labor (IZA).

Chuang, Hwei Lin, and Wei Chiao Huang. "Economic and Social Correlates of Regional Suicide Rates: A Pooled Cross-Section and Time-Series Analysis." *Journal of Socio Economics*, 1997, 26, no. 3 277-89.

Clark, Andrew E., and Andrew J. Oswald. "Satisfaction and Comparison Income." *Journal of Public Economics,* September 1996, 61(3), pp. 359-81.

Cutler, David, Edward Glaeser, and Karen Norberg. "Explaining the Rise in Teenage Suicide." *National Bureau of Economic Research Working Paper*, 2000, 7713.

Daly, Mary C., Greg J Duncan, George A. Kaplan, and John W. Lynch. “Macro-to Micro Links in the Relation between Income Inequality and Mortality.” *The Milbank Quarterly,* September 1998, 76(3), pp. 315-39.

Daly, Mary C., and Daniel J. Wilson. “Keeping Up with the Joneses and Staying Ahead of the Smiths: Evidence from Suicide Data.” Federal Reserve Bank of San Francisco Working Paper, 2006-12 (2006).

Di Tella, Rafael, Robert J. MacCulloch, and Andrew J. Oswald. "Preferences over Inflation and Unemployment: Evidence From Surveys of Happiness." *American Economic Review*, 2001, 91, no. 1 335-41.

Di Tella, Rafael, Robert J. MacCulloch, and Andrew J. Oswald. "The Macroeconomics of Happiness." *Review of Economics and Statistics*, Forthcoming.

Duesenberry, James S. *Income, Saving, and the Theory of Consumer Behavior*. Cambridge, MA: Harvard University Press, 1949.

Duesenberry, James S. *Income, Saving, and the Theory of Consumer Behavior*. Cambridge, MA: Harvard University Press, 1952.

Easterlin, Richard A. "Does Money Buy Happiness?" *Public Interest*, 1973, XXX 3-10.

Easterlin, Richard A. "Does Economic Growth Improve the Human Lot? Some Empirical Evidence." *Nations and Households in Economic Growth: Essays in Honour of Moses Abramovitz.* Editors Paul A. David and M. W. RederNew York and London: Academic Press, 1974.

Easterlin, Richard A. "Will Raising the Incomes of All Increase the Happiness of All?" *Journal of Economic Behavior and Organization*, 1995, 27, pp. 35-48.

Engelmann, Dirk, and Martin Strobel. "Inequality Aversion, Efficiency, and Maximin Preferences in Simple Distribution Experiments." *American Economic Review*, September 2004, 94(4), pp. 857-69.

Ferrer-i-Carbonell, Ada. "Income and well-being: an empirical analysis of the comparison income effect." *Journal of Public Economics*, June 2005, 89(5-6), pp. 997-1019.

Frank, Robert H. *Luxury Fever: Money and Happiness in an Era of Excess*. New York: The Free Press, 2000.

Gali, Jordi. "Keeping Up with the Joneses: Consumption Externalities, Portfolio Choice, and Asset Prices." *Journal of Money, Credit, and Banking*, February 1994, 26(1) pp. 1-8.

Gerdtham, Ulf-G., and Magnus Johannesson. "Absolute Income, Relative Income, Income Inequality, and Mortality." *Journal of Human Resources*, 2004, 39(1), pp. 228-47.

Haan, Mary N., George A. Kaplan, and S. Leonard Syme. "Socioeconomic Status and Health: Old Observations and New Thoughts." In: J. P. Bunker, D. S. Gomby, & B. H. Kehrer, (Eds.), *Pathways to Health: The Role of Social Factors*, 1989, pp.76-135. Menlo Park, CA., HJ Kaiser Family Foundation.

Hamermesh, Daniel S. "The Economics of Black Suicide." *Southern Economic Journal*, 1974, 41, no. 2 188-99.

Hamermesh, Daniel S., and Neal M. Soss. "An Economic Theory of Suicide." *Journal of Political Economy*, January/February 1974, 82(1), pp. 83-98. (In *Mercurio* as "Una Teoria Economica del Suicidia.")

Helliwell, John F. "How's Life? Combining Individual and National Variables to Explain Subjective Well-Being." *National Bureau of Economic Research Working Paper*, 2002, 9065.

Helliwell, John F. "Well-Being and Social Capital: Does Suicide Pose a Puzzle?" *National Bureau of Economic Research Working Paper,* 2004, no. 10896.

Hendin, Herbert. "Suicide in America." New York: W.W. Norton & Company, 1995.

Huang, Wei Chiao. "Religion, Culture, Economic and Sociological Correlates of Suicide Rates: A Cross-National Analysis." *Applied Economics Letters*, 1996, 3, no. 12 779-82.

Kahneman, Daniel and Amos Tversky. "On the Reality of Cognitive Illusions: A Reply to Gigerenzer's Critique." *Psychological Review*, 1996, 103, pp. 582-91.

Kimenyi, Mwangi S., and William F. II Shughart. "Economics of Suicide: Rational or Irrational Choice." *Atlantic Economic Journal*, 1986, 14, no. 1 121.

Koivumaa-Honkanen, Heli, Risto Honkanen, Heimo Viinamaki, Kauko Heikkila, Jaakko Kaprio, and Markku Koskenvuo. "Life Satisfaction and Suicide: a 20-Year Follow-Up Study." *American Journal of Psychiatry*, 2001, 158(3), pp. 433-9.

Kposawa, Augustine J. "Unemployment and Suicide: A Cohort Analysis of Social Factors Predicting Suicide in the US National Longitudinal Mortality Study." *Psychological Medicine*, 2001, 31(1), pp. 127-38.

Lewis, Glyn, and Andy Sloggett. "Suicide, deprivation, and unemployment: record linkage study." *BMJ*, 1998, 317, pp. 1283-86.

Luttmer, Erzo F.P. "Neighbors as Negatives: Relative Earnings and Well-Being." *Quarterly Journal of Economics*, 2005, 102(3), pp. 963-1002.

Lykken, David. 1999 *Happiness: What Studies of Twins Show Us? Nature, Nurture, and the Happiness Set Point.* Golden Books Co. Inc.

Marcotte, Dave E. "The Economics of Suicide, Revisited." *Southern Economic Journal*, 2003, 69, no. 3, pp. 628-43.

Miller, Douglas, and Christina Paxson. "Relative Income, Race, and Mortality." *Journal of Health Economics*, 2006, 25(5), pp. 979-1003.

Miller, Mathew, Deborah Azrael, and David Hemenway. "Firearm Availability and Unintentional Firearm Deaths, Suicide, and Homicide Among 5-14 Year Olds." *The Journal of Trauma*, 2002, 52(2), pp. 267-75.

Mohler, B., and F. Earls. "Trends in Adolescent Suicide: Misclassification Bias?" *American Journal of Public Health*, 2001, 91, no. 1 150-3.

Moore, David E. and Mark D. Hayward. "Occupational Careers and Mortality of Elderly Men." *Demography*, February 1990, 27(1), pp. 31-53.

Moyer, L. A., C. A. Boyle, and D. A. Pollock. “Validity of Death Certificates for Injury-Related Causes of Death.” *American Journal of Epidemiology*, 1989, 130 1024-32.

Murphy, Kevin M., and Robert H. Topel. "Estimation and Inference in Two-Step Econometric Models." *Journal of Business and Economic Statistics*, 1985, 3, no. 4 370-79.

Osmani, S. (1993). ‘Comment on B.M.S. van Praag: The relativity of the welfare concept’, in (M. Nussbaum and A. Sen, eds.) *The Quality of Life*, Oxford: Oxford University Press.

Oswald, Andrew J. “Happiness and Economic Performance.” *Economic Journal*, November 1997, 107(445), pp. 1815-31.

Peterson, C., N. Park, and M.E.P. Seligman. “Orientations to Happiness and Life Satisfaction: the Full Life Versus the Empty Life.” *Journal of Happiness Studies*, 2005 6(1), 24-41.

Rockett, Ian, and G. S. Smith. "Suicide Misclassification in an International Context." How It's Done: An Invitation to Social Research. Editors B. S. Adler and R. D. Clark, 122-29. Belmont, CA: Wadsworth, 1999.

Steven Ruggles, Matthew Sobek, Trent Alexander, Catherine A. Fitch, Ronald Goeken, Patricia Kelly Hall, Miriam King, and Chad Ronnander. *Integrated Public Use Microdata Series: Version 3.0* [Machine-readable database]. Minneapolis, MN: Minnesota Population Center [producer and distributor], 2004. Available at http://www.ipums.org.

Ruhm, Christopher J. "Are Recessions Good For Your Health." *Quarterly Journal of Economics*, 2000, 115(2), pp. 617-50.

Schapiro, Morton Owen, and Dennis A. Ahlburg. "Suicide: The Ultimate Cost of Unemployment." *Journal of Post Keynesian Economics*, 1982-1983, 5(2) pp. 276-80.

Stevenson, Betsey, and Justin Wolfers. “'Til Death Do Us Part: Effects of Divorce Laws on Suicide, Domestic Violence and Spousal Murder.” Unpublished Manuscript, 2000.

Tekin, Erdal, and Sara Markowitz. "Suicidal Behavior and the Labor Market Productivity of Young Adults." NBER Working Paper #11238, 2005.

Tomes, Nigel. "Income Distribution, Happiness and Satisfaction: A Direct Test of the Interdependent Preferences Model." *Journal of Economic Psychology*, 1986, 7, no. 4 425-46.

Veblen, Thorsten (1899). *The Theory of the Leisure Class*, New York: Macmillan.

World Health Organization. WHO Mortaility Database, January 2005. Available online: http://www3.who.int/whosis/menu.cfm?path=whosis,mort

U.S. Bureau of the Census. "National Longitudinal Mortality Study, Reference Manual, Version 2." August 1, 2005. Avaiable online at: http://www.census.gov/nlms/reference.html.

U.S. Department of Health and Human Services. National Center for Health Statistics. Mortality Detail File, 1992 [Computer file]. Hyattsville, MD: U.S. Dept. of Health and Human Services, National Center for Health Statistics [producer], 1994. Ann Arbor, MI: Interuniversity Consortium for Political and Social Research [distributor], 1996.