The Short-Term Mortality Consequences of Income Receipt

William N. Evans
Department of Economics and Econometrics
University of Notre Dame
437 Flanner Hall
Notre Dame, IN 46556
Vmail: 574-631-7039
Email: wevans1@nd.edu

Timothy J. Moore
Department of Economics
University of Maryland
Tydings Hall
College Park, MD 20742
Vmail: 301-442-1785
Email: moore@econ.umd.edu

June 14, 2010

Abstract

Researchers and retailers have documented a "paycheck cycle" where consumption declines before the receipt of income, then rises afterwards. In this paper, we identify a related phenomenon, where mortality rises immediately after income receipt. We find that mortality increases following the arrival of monthly Social Security payments, regular wage payments for military personnel, the 2001 tax rebates, and Alaska Permanent Fund dividend payments. The increase in short-run mortality is large, potentially eliminating some of the protective benefits of additional income.

Keywords: mortality, income, consumption, life-cycle model, permanent-income hypothesis, liquidity constraints, tax rebates, wages, dividends, social security.

JEL classification: D91, H31, H55, I10, I12, I38

We wish to thank Hoyt Bleakley, Kasey Buckles, Tatyana Deryugina, Craig Garthwaite, Ashley Miller, Belén Sbrancia, David Segal, Jon Skinner, Dan Hungerman, and Mel Stephens for a number of helpful suggestions, Robert Bilgrad and Karen Davis of the NCHS for assistance with the use of restricted-use data used in this project, and seminar participants at Dartmouth College, the University of Chicago, Indiana University, University of California, Santa Barbara, the Federal Reserve Board at St. Louis, Georgia State, Harvard University, the Institute for Research on Poverty of the University of Wisconsin and the Upjohn Institute for being gracious hosts. Andrew Kenny and Corey McNeilly provided excellent research assistance throughout this project.

I. Introduction

A minority of families in the US have active savings plans. According to the 2007 Survey of Consumer Finances, only 42 percent of families save money on a regular basis.[1] Given this low savings rate, it is no surprise that in a recent online survey from CareerBuilders.com, 61 percent of respondents report that they "always or usually live paycheck to paycheck to make ends meet."[2] Even some higher-income families operate this way: in this survey, 30 percent of households with annual income over $100,000 report living paycheck to paycheck. This problem is not unique to the United States: a survey of 3,000 workers in the United Kingdom suggests that the average employee paid monthly is "broke" 23 days after payday.[3]

The lack of liquidity between paydays has been demonstrated to alter behavior among consumers. In particular, econometric evidence suggests consumers tend to reduce spending before income receipt and increase purchases right after income receipt. Stephens (2003) found that seniors increase their consumption of time-sensitive purchases, like perishable food and eating at restaurants, after the receipt of Social Security checks. Using data for the United Kingdom, Stephens (2006) found a similar increase in consumption after the receipt of paychecks. This bunching effect is particularly pronounced for those on federal income transfer programs and those with lower incomes. Among Food Stamp recipients, Shapiro (2005) found a drop in daily caloric consumption of 10-15 percent from when food stamps

---

[1] http://www.federalreserve.gov/pubs/bulletin/2009/pdf/scf09.pdf. Of the other respondents, 6 percent report usually spending more than they earn, 16 percent report that their spending and income are about the same, and 37 percent report only saving whatever income is not spent at the end of the year.
[2] http://msn.careerbuilder.com/Article/MSN-2061-Salaries-and-Promotions-Majority-of-Workers-Live-Paycheck-to-Paycheck/
[3] http://www.thesun.co.uk/sol/homepage/news/2902043/Brits-are-broke-before-payday.html?OTC-RSS&ATTR=News

are paid to just before they are next due. Likewise, Mastrobuoni and Weinberg (2009) found food consumption declined between Social Security payments among seniors with a high fraction of income coming from Social Security.

In the retail industry, the link between income receipt and spending is often referred to as the "paycheck cycle," where "… strapped consumers are showing a tendency to make their largest purchases when their salaries first come in and to cut back as that money runs out."[4] In a recent interview with Reuters, Tom Schoewe, the Chief Financial Officer for Wal-Mart, notes that "Many of our customers receive food stamps, welfare, other forms of government assistance. On the first of the month their EBT [electronic bank cards] would be charged, and we can see literally real-time once the clock strikes midnight and EBT cards are charged, you can see our results start to tick up."[5] Paycheck receipt is also met with some supply response. Hastings and Washington (2008) use store scanner data and find both grocery prices and the amount of goods purchased increases at the start of the month.

While the link between income receipt and consumer spending has received some attention, there has been little research to expand this area of inquiry beyond consumption behavior. To that end, in this paper we document that mortality increases immediately after income receipt. We use various versions of the Multiple Cause of Death (MCOD) data, a census of all deaths in the United States, to examine the income receipt/short-run mortality link. Taking our cue from research that tests predictions about the life-cycle/permanent income hypothesis using known dates of income receipt, we examine three cases of income

[4] http://online.wsj.com/article/SB123535246479645145.html
[5] http://blogs.reuters.com/shop-talk/2009/08/13/30-minutes-with-wal-marts-cfo/

receipt from that literature as well as two new tests.[6] We examine the mortality consequences of (1) the receipt of Social Security payments on the 3rd of each month, (2) changes in the Social Security payment schedule to one based on beneficiaries' dates of birth, (3) the receipt of military wages on the 1st and 15th day of each month, (4) the 2001 federal tax rebates, and (5) the annual Alaska Permanent Fund dividend payments.

In all cases, we find that mortality increases after the receipt of income. Seniors who enrolled in Social Security prior to May 1997 typically received their Social Security checks on the 3rd of the month. For this group, mortality declines just before paycheck receipt, and is highest the day after checks are received. For those who enrolled in Social Security after April 1997, benefits are paid on either the second, third or fourth Wednesday of the month, depending on beneficiaries' birth dates. Among this group, mortality is highest on the days checks arrive. Similar results are found in counties with a large military presence, with mortality among 17-64 year olds increasing by nearly 12 percent the day after mid-month paychecks arrive, while over the same period there is no change in mortality in counties with little military presence. During the week the 2001 tax rebate checks arrived, mortality among 25-64 year olds increased by 2.5 percent. During the week that direct deposits of Permanent Fund dividends are made, mortality among urban Alaskans increases by 13 percent.

[6] The life cycle-permanent income hypothesis (LC/PIH) is the standard model for inter-temporal choice in modern macroeconomics. A key implication of the model is that predictable and certain changes in income should have no effect on consumption once they occur. Over the past 15 years, authors have used high-frequency survey data on consumption and exact dates of income receipt to test this prediction. Three of our tests have been used in this way: Stephens (2003) examined the receipt of Social Security checks in the pre-1997 period; Johnson, Parker and Souleles (2006) examined the 2001 tax rebates; and Hsieh (2003) considered consumption after the receipt of Alaska Permanent Fund dividend payments.

These results are qualitatively large. In simulations using cross-sectional estimates of the impact of income on mortality, the change in daily mortality produced by the payday effect is equivalent to a 2.6 percent drop in income for the elderly and a more than 10 percent drop in income for everyone in counties with a large military presence.

A close reading of the existing medical literature suggests consumption does increase mortality, which is why we observe a link between income receipt and mortality. While this is most obvious in cases like traffic fatalities – since increased travel increases the likelihood of an accident – other causes of death also have well-documented links to consumption. For example, as discussed in the next section, many triggers for heart attacks are activity-related. If income payments increase economic activity, one may expect a higher incidence of heart attacks to result. Likewise, Ruhm (2000) shows that mortality is pro-cyclical, suggesting a deadly aspect to increased economic activity. Finally, as we show below, movements in aggregate mortality and in goods purchases are closely related.

Our work helps illuminate and broaden two literatures. The first is a group of papers that document an increase in substance abuse-related mortality following payments to welfare recipients. Sometimes called the “full wallets” hypothesis, this literature shows convincingly that problems associated with substance abuse increase after federal transfer program payments arrive. Our work demonstrates that the “full wallets” effect is broader than currently understood. The effect of income receipt on mortality is not limited to recipients of federal transfer programs or to deaths involving substance abuse.

Second, the results described below run counter to the large literature on income and health (Kitigawa and Hauser, 1973; Deaton, 2003). While this research has established a persistent positive correlation between income and health outcomes, it has failed to identify the causal nature of the relationship. The factors that lead one to have a high income or socioeconomic status (e.g. intelligence, discount rates) may also improve health outcomes. In fact, another literature has established that negative health shocks reduce earnings and increase health care spending,[7] suggesting that the direction of causation may run from health to income. Given the possibility of reverse causation and the lack of an obvious causal pathway from income to health, Deaton (2003, p. 118) notes that "…much of the economics literature has been skeptical about *any* causal link from income to health, and instead tends to emphasize causality in the opposite direction…"

In recent years, authors have tested whether socioeconomic status causally affects health by using exogenous variation in education[8] and income.[9] While the former group of studies has consistently found that education improves health, there are conflicting results among studies examining the role of income. Our results below may be instructive for this literature. First, some of the longer-term gains from an exogenous increase in income may be negated by the short-run phenomenon we detect. This may explain why consistent results have been hard to find. Second, these short-run effects may impact the efficacy of cash transfers, which some authors – despite the misgivings outlined by Deaton – have suggested as a way of reducing health inequalities between income levels. For example, a 1998 United Kingdom Government report recommended an increase in cash benefits as a direct way to improve health outcomes in the lowest income groups.[10] A number of scholars who have

[7] For example, see Bound, 1989, Haveman et al., 1995, and especially Smith, 1999.
[8] For example, authors have examined whether health outcomes are altered by increases in education generated by policies such as compulsory schooling (Lleras-Muney, 2005), an increase in access to colleges (Currie and Moretti, 2003) and the Vietnam Draft (de Walque, 2007; Grimand and Parent, 2007).
[9] Such work exploits variation in income produced by such factors as winning the lottery (Lindahl, 2005), German reunification (Fritjers, Hasken-DeNew and Shields, 2005), receiving an inheritance (Meer, Miller and Rosen, 2003), South African pensions (Case, 2004) and changes in Social Security (Snyder and Evans, 2006).
[10] http://www.archive.official-documents.co.uk/document/doh/ih/ih.htm

attempted to empirically measure the link between socioeconomic status and health have expressed similar sentiments.[11] Our results suggest that the negative short-run consequences of these transfers must be considered in such evaluations.

In the next section, we outline the existing literature from a variety of disciplines that suggests income receipt and mortality may be related in the short run. In Section III, we examine how regular payments to Social Security recipients and military personnel affect short-term mortality. In both cases we find mortality is much higher immediately after the receipt of income than beforehand, and that these increases are not just among deaths related to substance abuse.

To examine whether increases in mortality also occur following less regular income payments, in Section IV we consider the mortality effects of the one-time receipt of 2001 tax stimulus checks and the annual receipt of Alaska Permanent Fund dividends. The populations in these examples broaden the phenomenon beyond the elderly and active duty military. In both cases, there is a short-term increase in mortality that is partially offset by a subsequent decrease in deaths, suggesting that some of the immediate effect reflects short-term mortality displacement: that is, mortality has been hastened for people who would have died soon anyway. In section V, we discuss the implications of our work.

II. Consumption and Mortality in the Short-Run

There has been limited research linking changes in mortality to consumption. The largest and most direct literature is that surrounding what is called the "full wallets"

[11] Marmot (2002, p. 43) notes that redistribution would improve overall health by "relieving the fate of the poor more than it hurt the rich." Wilkinson (in Gly and Miliband, 1994) argues, "[t]he health evidence suggests that narrowing the gap in relative standards is now much more important to the quality of life in the developed world than further economic growth."

hypothesis, which suggests that the receipt of income encourages drug and alcohol abuse in some populations. Papers by Verhuel et al. (1997), Rosenheck et al. 2000, Maynard and Cox (2000), Halpern and Mechem (2001), Riddell and Riddell (2006), and Li et al. (2007) have found such a relationship. In the most detailed study to date, Dobkin and Puller (2007) use administrative records from California to show that hospital admissions and within-hospital mortality increases among Supplemental Security Income recipients immediately after they are paid. These increases are particularly pronounced for substance abuse-related cases.

There are reasons to think that the relationship between consumption and mortality is broader than just that produced by substance abuse. Some causes of death are obviously related to people's levels of activity. For example, the more one drives the higher the risks of an accident; in fact, the elasticity of motor vehicle mortality rates with respect to per capita vehicle miles of travel is close to one.[12] There are also other causes of death with extensive empirical evidence that an increase in activity temporarily raises mortality risks, with the most detailed evidence being for heart attacks. Most activities seem to increase the short-term risk of a heart attack, including exercise (Mittleman et al., 1993; Albert et al., 2000), sexual activity (Moller et al., 2001), eating a heavy meal (Lipovetsky et al., 2004), the busy Christmas holiday season (Phillips et al., 2004), returning to work on Mondays (Witte et al., 2005; Willich et al., 1994), and shoveling snow (Franklin et al., 1996; Heppell et al., 1991).

As we indicate below, much of the short-term mortality consequences of income receipt are concentrated in external causes (e.g., accidents, murders, etc.) and heart

[12] Using data from the Fatal Accident Reporting System, we calculate the total motor vehicle fatality rate (deaths per 100,000 people) at the state/year level for all states and the District of Columbia for 1975 to 1997. We regressed the natural log of this variable on state and year effects and the natural log of per capita vehicle miles of travel, a variable that can be constructed from data in the National Highway Traffic Safety Administration's annual *Highway Statistics* publication. The coefficient (standard error) on this final variable is 0.78 (0.06).

attacks/disease, results consistent with the existing literature outlined above. When we suggest a link between consumption and mortality, we are not limiting the discussion to the act of consuming *per se* but, rather, all activity associated with consumption. Receiving a paycheck may, for example, encourage people to see a movie that day, which by construction increases activity (and maybe the risk of a heart attack) and exposes the consumer to the hazards of driving in traffic.

As already mentioned, recent studies find people spend more immediately after they receive an income payment, even when it was certain and expected. Amongst seniors, for example, Stephens (2003) finds Social Security recipients consume more immediately after they are paid, while Mastrobuoni and Weinberg (2009) find seniors' caloric intake is highest after they receive their Social Security checks. Given what is known about the relationship between income payments and consumption, a broad-based relationship between consumption and mortality is likely to result in a short-term increase in mortality following income payments.

There are two patterns in aggregate mortality that indicate there may be a reduced-form relationship between income receipt and mortality. First, mortality is pro-cyclical: aggregate mortality increases in a boom and declines in a recession. Second, there is a within-month mortality cycle where daily mortality counts decline below the average in the last few days of a calendar month before increasing above the average for the first few days of the month. This pattern could be connected to income payments, which disproportionately occur at the start of the month: in addition to federal transfer programs, TANF benefits and monthly wages are commonly paid at the start of the month (Evans and Moore, 2009).

The pro-cyclic nature of mortality can be seen in Figure 1a, where we compare the unemployment rate and the natural logarithm of the mortality rate for the United States from 1973 to 2005.[13,14] This is an updated version of a figure that first appeared in Ruhm (2000); both series are de-trended using a linear trend and the residuals are normalized by dividing them by their standard deviation. The figure shows a strong inverse relationship between unemployment and mortality (ρ=-0.49). Ruhm (2000) found that this basic relationship remains in regressions of state-level mortality rates on unemployment rates, state and year effects, as well as some demographic covariates. Similar relationships between mortality and measures of economic activity have been documented for several OECD countries (Gerdtham and Johannesson, 2005; Neumayer, 2004; Tapia Granados, 2004), health habits (Ruhm, 2003) and health outcomes (Ruhm, 2005), as well as a wide variety of causes of death including heart disease, and murder (Ruhm, 2000), motor vehicle fatalities (Evans and Graham, 1988) and infant health (Dehejia and Lleras-Muney, 2004).

While, to date, authors have not provided an explanation for the pro-cyclic nature of mortality, a likely intervening factor is the changes in activity that occur over a business cycle. In Figure 1b, we present the de-trended and normalized unemployment rate from Figure 1a with a similarly de-trended and normalized plot of the natural log of real per capita goods purchases.[15] Not surprisingly, spending declines in recessions and the correlation coefficient between these two numbers is strongly negative. More interesting, however, is Figure 1c which shows that the de-trended and normalized natural log of goods spending

[13] The mortality date is from the Multiple Cause of Death (MCOD) file of the National Center for Health Statistics, and is explained in the next section. The annual average unemployment rate is from the Bureau of Labor Statistics.

[14] For ease of interpretation, throughout the paper we generally take the natural log of dependent variables.

[15] Spending on durable and nondurable goods in this section is from the National Income and Product Accounts, and is deflated using the GDP deflator.

(from Figure 1b) and the de-trended and normalized natural log of all-cause mortality follow similar patterns (ρ=0.21). The pattern is stronger for some death categories than others, and also when we focus on nondurable goods. In Figure 1d, we plot the de-trended, normalized series for the natural log of external causes (e.g., accidents, homicides, suicides) against a similar series for the natural log of per capita durable goods consumption. The patterns are very similar (ρ=0.69).

A second pattern in aggregate mortality is the within-month cycle, which is shown in Figure 2a for deaths in the United States between 1973 and 2005. Days are arranged in relation to the 1st of the calendar month, and average daily mortality risk is shown for the fourteen days prior to the 1st and the first fourteen days of the month, with 95 percent confidence intervals also shown.[16] Starting about twelve days before the 1st, daily deaths decline slowing, and fall to 0.8 percent below average the day before the 1st of the month. Deaths then increase on the 1st of the month to 0.6 percent above the daily average. The peak-to-trough represents about a 1.4 percent difference in daily mortality rates.

This pattern was first identified by Phillips, Christenfeld and Ryan (1999), who noted that the within-month mortality cycle is particularly pronounced for external causes and speculated that the payment of government transfers at the beginning of each month resulted in higher levels of substance abuse and increased mortality. Evans and Moore (2009) separate deaths possibly caused by substance abuse from other deaths and show that, while substance abuse deaths display the largest within-month cycle, they account for a minority of the overall pattern. They also establish that the within month mortality cycle is broad based,

[16] We use the delta method to construct the variance of the risk ratio. The variance of daily deaths is calculated as follows. Let N_t be the number of people alive at the start of day t, and the probability of death that day equal p_t. Since this is a set of Bernoulli trials, expected deaths (d_t) is $E[d_t] = N_t p_t$, and the variance of deaths is $V[d_t]=N_t p_t(1-p_t) = \sigma^2_t$. A consistent estimate of p_t is d_t/N_t. The risk of death on any single day is extremely low, such that $1-p_t$ is functionally one. Therefore an estimate of the variance of daily deaths is simply d_t.

appearing for many causes of death, including external causes, heart disease, heart attack, and stroke, but not cancer. The within-month cycle is also evident for both sexes and for all age groups, races, marital status groups, and education groups. The within-month cycle is mirrored by a similar cycle in activity. Using daily data on a number of different activities and purchases, Evans and Moore (2009) document that activities such as going to the mall, visiting retail establishments, purchasing lottery tickets, going to the movies, and the amounts spent on food and non-food retail purchases all show the same pattern, namely, that activity declines toward the end of the month and rebounds after the 1st of the month.

It is plausible that a short-term consumption-mortality relationship accounts for much of the within-month mortality cycle. In Figure 2b we plot, in relation to the 1st of the month, the normalized mean residuals of a regression where the natural log of the daily mortality counts is regressed against dummy variables for the different days of the week, synthetic months that begin fourteen days before the 1st of each month and synthetic years that begin fourteen days before the 1st of January,[17] as well as special days throughout the year, such as New Year's Day and Christmas.[18] The synthetic months and years are similar to those used in Stephens (2003). They are constructed to control for changes across seasons and time while avoiding a mechanical jump from the last day of one calendar month to the first day of the next month.

---

[17] For example, month 1 goes from December 18th to January 17th, month 2 goes from January 18th to February 14th in non-leap years (and to February 15th in leap years), and so on.

[18] We include unique dummies for a long list of reoccurring special days, including for January 1st and 2nd, the Friday through Monday associated with the all federal holidays occurring on Mondays (Presidents' Day, Martin Luther King Jr Day since 1986, Memorial Day, Labor Day, Columbus Day), Super Bowl Sunday and the Monday afterwards, Holy Thursday through Easter Sunday, July 4th, Veteran's Day, the Monday through Sunday of Thanksgiving, a dummy for all days from the day after Thanksgiving though New Year's Eve, plus single day dummies for December 24th through December 31st. We also reduce the number of homicides on September 11, 2001 by 2,902 deaths, which according to a Center for Disease Control report was the number of deaths on that date due to the terrorist attacks http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm.

The within-month cycle remains apparent in this plot, with the mean of the residuals prior to the 1st generally below zero and the residuals from the 1st above zero. Alongside those residuals, we plot the normalized residuals of the same regression when the dependent variable is the natural log of the average daily spending by participants in the Diary Survey component of the Consumer Expenditure Survey (CEX). The CEX is produced by the Bureau of Labor Statistics, and for the Diary Survey component households provide detailed information about their purchases over a 14-day period. We use data from 1986 and 1988 to 2005. Prior to 1986, detailed information on expenditure items were not included in the public use micro-data files, and in 1987 the Diary Survey was not conducted throughout the year.[19] Dollar values are converted to 2005 dollars using the CPI-U, with each day in a synthetic month deflated by the CPI-U value in which the 1st of the calendar month falls.[20] We drop purchases of more than $200, as well as payments for housing, insurance and utilities, as these expenditures that may occur on dates not entirely of a household's choosing (e.g., renters may have a lease specifying that rent be paid on the 1st of the month). As can be seen in Figure 2b, expenditures during most of the last week of the calendar month are below the daily average, before there is a large increase in spending which peaks on the 1st of the calendar month. Changes in mortality are not as sharp and generally lag these changes in consumption, but there is enough similarity in the patterns to suggest there might be a connection between short-term consumption and aggregate mortality.

[19] Stephens (2003) uses these data, and provides more details about how they are collected and cleaned by the Bureau of Labor Statistics. Like him, we remove households that only have purchases recorded on their first day of a diary week, as if all dates of purchase are missing the first day of a diary week is assigned as the date of purchase for every item in that week.

[20] For example, the synthetic month around May 1st, 2005 begins 14 days before this date (April 17th) and ends 14 days before June 1st (May 17th). Purchases on these days are all deflated by the May 2005 CPI-U figure. This is again to avoid mechanical shifts in residuals at the 1st of the calendar month.

The primary challenge in moving beyond these correlations is that the mortality data contains no direct information about decedents' income or consumption behavior. There are some demographic variables, however, and our identification strategies throughout the paper use these variables to identify groups of decedents for whom we have some information as to when they were likely to have been paid. We are missing consumption information for these decedents but, as we detail before each test, a combination of previous studies and anecdotal evidence suggest that such groups do consume more after they are paid. This, in addition to separating the role of substance abuse from other causes of death, allows us to begin to explore the short-term link between income, consumption, and mortality.

III. The Short-Term Mortality Consequences of Regular Income Payments

a. Monthly Social Security Payments

Prior to May 1997, all Social Security recipients received checks on the 3rd of each month, or the previous work day when the 3rd fell on a weekend or on Labor Day. Stephens (2003) used the structure of these payments and data from the CEX to demonstrate that Social Security recipients spend more on a variety of goods immediately after their check arrived, including on food at home, food away from home, and "instantaneous consumption," which consisted of food away from home, sporting fees, admissions to entertainment and sporting events, and video rentals.

Given the connection between these types of spending and the mortality risks and triggers discussed in the previous section, it is possible that the mortality of Social Security recipients is higher immediately after they are paid than beforehand. We initially use the "3rd of the month" schedule and mortality data from prior to 1997 to investigate this possibility.

The mortality data we use in this and subsequent tests are various versions of the Multiple Cause of Death (MCOD) data file.[21] The MCOD contains a unique record for each death in the United States. Data are compiled by states and reported to the National Center for Health Statistics (NCHS), which disseminates the data. Each file contains information about the decedent, including age, gender, race, place of residence, place of death, and cause of death. Exact date of death was reported on public-use files from 1973 to 1988, but was removed from later public-use files. We obtained permission from the NCHS to use restricted-use MCOD files containing exact dates of death from 1989 to 2006 at their Research Data Center.

We used the information on decedents' age and exact date of death in the 1973 to 1996 MCOD files to construct daily counts of decedents aged 65 and over, a group consisting almost entirely of Social Security recipients.[22] The Social Security Administration reports that benefits were paid to 32.7 million adults aged 65 and older in 2000,[23] which is 93.5 percent of the population in this age group in the 2000 Census.

To comprehensively analyze the relationship between Social Security payments and daily mortality, we construct 'synthetic' months that begin 14 days prior to the day of Social Security payment and last until 15 days before the next payment.[24] These synthetic months can be anywhere from 28 to 34 days in length, as they depend on the day when the checks are

[21] Information about the MCOD is at http://www.cdc.gov/nchs/products/elec_prods/subject/mortmcd.htm.
[22] Workers can claim reduced retirement benefits at 62 and receive full benefits at between 65 and 66 years of age, depending on their cohort. Song and Manchester (2007) report that from 1998 to 2005, half of Social Security beneficiaries enrolled at age 62 and almost all enrolled by age 65. Therefore, we restrict our attention to decedents aged 65 years or more.
[23] Social Security Administration Office of Research, Evaluation and Statistics, *Annual Statistical Supplement of the Social Security Bulletin, 2001*, Washington, DC: SSA, December 2001.
[24] For example, January 3, 1995 is a Tuesday, so the first synthetic month of the year is December 20th of the previous year through to January 19, 1995; month two is then January 20th though February 20th, and so on.

distributed and the number of days in the month.[25] Thus we divide each month into five groups: *Payweek(-2)* is the seven days beginning 14 days before payday and ending on the eighth day before payday; *Payweek(-1)* is the seven days prior to payday; *Payweek(1)* is the seven days after payday (including payday); *Payweek(2)* is the period from eight to 14 days after the paycheck arrives; and *Payweek(3)* is the extraneous days before the next synthetic month starts.

The largest movement in the within-month mortality cycle occurs just before Social Security payments are made, so it is necessary to control explicitly for the within-month cycle. Therefore, we create weekly dummy variables in reference to the 1st of the calendar month, where *Week(-2)* equals one if the day is eight to 14 days before the start of the calendar month; *Week(-1)* equals one if the day is one to seven days before the start of the month; *Week(1)* and *Week(2)* equal one for the 1st to 7th and 8th to 14th days in the calendar month, respectively; and *Week(5)* is all the extra days before the 14th day prior to the start of the next calendar month. As checks not paid on the 3rd are almost always paid on Fridays,[26] we also need to control for day-of-the-week effects.

To isolate the mortality impact of receiving a Social Security check from other factors, we estimate the following econometric model. Let Y_{dmy} be counts of deaths for day d in synthetic month m and synthetic year y. Days are organized in relation to Social Security payments, so d=-1 is the day before payday, d=1 is payday, and so on; d ranges from -14 to 20.[27] The econometric model is of the form:

---

[25] When February 3rd falls on a weekday, the second synthetic month of the year will only contain 28 days. When the 3rd of the month falls on a Sunday in a month with 31 days, as it does in July 1994, the checks are distributed on July 1st and the month spans from June 17th to July 19th, making the synthetic month 33 days.

[26] The lone exception is that when January 3rd is a Sunday, checks are distributed on Thursday, December 31.

[27] Years also follow this structure, so when both the January and December payments are made on the 3rd of the month, the year will begin on December 20th and will go through until December 16th of the following year.

$$(1) \qquad \ln(Y_{dmy}) = \alpha + \sum_{\substack{w=-2 \\ w\neq -1}}^{3} Week(w)_{dmy}\delta_w + \sum_{\substack{w=-2 \\ w\neq -1}}^{3} Payweek(w)_{dmy}\beta_w + \sum_{j=1}^{6} Weekday(j)_{dmy}\gamma_j + \sum_{j=1}^{M} Special(j)_{dmy}\varphi_j + \mu_m + v_y + \varepsilon_{dmy}$$

where *Payweek(w)* and *Week(w)* are the dummy variables defined as above, *Weekday(j)* is one of six dummy variables for the different days of the week, and *Special(j)* is one of *J* dummy variables that capture special days throughout the year, which are already detailed in footnote 18. The variables μ_m and v_y capture synthetic month and year effects[28] and ε_{dmy} is an idiosyncratic error term. In this equation, the reference period for the *Payweek* dummies is *PayWeek(-1)* and the reference period for *Week* dummies is *Week(-1)*. The reference weekday is Saturday. We estimate standard errors allowing for arbitrary correlation within each unique synthetic month, e.g., we allow for correlation in errors for month 1 of 1995, month 2 of 1995, etc.

The results for equation (1) for decedents 65 and older from 1973 to 1996 are reported in the first column of Table 1. In the first four rows of the table, we report results for the calendar weeks in relation to the 1st of the month. There is a within-month mortality cycle, with deaths declining the week before the 1st and then rising afterwards. Daily death rates are about three-tenths of a percent higher in the first week of the month compared to the previous seven days, with a p-value for the test that the null hypothesis is zero of less than 0.05. In the next four rows, we show that Social Security payments have an effect of a

[28] We have estimated all models with synthetic month-year effects, μ_{my}, instead of separate synthetic month and year effects. Results with this alterative specification are virtually identical to results from the more parsimonious specification.

similar magnitude to the within-month cycle. Deaths are about one half of a percent higher in the seven days after check receipt compared to the preceding seven days.[29]

Deaths are also one half a percent higher two weeks before payment (*Payweek(-2)*) and two weeks after payment (*Payweek(2)*).[30] The results suggest a fall in mortality in the last few days before seniors are paid; the increase when they are paid is a return to 'normal' mortality. That is consistent with seniors decreasing their level of activity as they run of out money, rather than 'splurging' when they get paid. It fits with some of the consumption behavior among seniors reported in Stephens (2003), as well as in Mastrobuoni and Weinberg (2009) with respect to caloric intake. In column (2), we consider results for seniors aged 65 to 69. We focus on this group for two reasons. First, as we outline below, the sample used to examine the new Social Security payment schedule will only include those aged 65 to 69, so this will be a comparable group. Second, Evans and Moore (2009) demonstrate that the within-month mortality cycle – similar in scope to the effect we analyze here – is more pronounced for younger groups, so we will benefit from focusing on a younger group of Social Security recipients here. In line with this, we find income receipt has a greater absolute impact on mortality on this younger group than on seniors as whole, with the coefficient on *Payweek(1)* increasing to three-quarters of a percent. It is also worth noting that the coefficient on *Payweek(1)* is higher than the other *Payweek* coefficients, suggesting that in this group income receipt may be leading to a spike in mortality above 'normal' levels, and may reflect more 'splurging' behavior among this group than seniors as a whole.

[29] To provide a frame of reference, Stephens (2003) shows that the probability of any spending among all seniors is 1.6 percent higher in the first week after checks arrive compared to the previous seven days.
[30] While this is also true for *Payweek(3)*, it is difficult to interpret the *Week(3)* and *Payweek(3)* coefficients in any regressions. Because the length of these dummy variables varies across months, they have a strong seasonal component that is not necessarily controlled for with other covariates.

There is also a set of decedents in this age group who should NOT be impacted by the "3rd of the month" schedule, which allows us to see whether our results are spuriously correlated with some other effect. Starting in May of 1997, the timing of monthly payments for new recipients depended on their birth dates. Those with a birth date from the 1st to the 10th are now paid on the second Wednesday of each month; those with a birth date from the 11th to the 20th are paid on the third Wednesday; and those with a birth date from the 21st to the 31st are paid on the fourth Wednesday. Those already receiving payments on the 3rd of the month continued to receive checks as they had before.[31] As a falsification exercise, we estimate the "3rd of the month" model on decedents who are on the new payment schedule.

The sample we construct for this test uses deaths among 65 to 69 year olds as recorded in the MCOD files for 2005 and 2006, the most recent year data is available. We identified decedents on the new payment schedule using the period-cohort diagram shown as Figure 3. The vertical axis represents year-of-birth cohorts and the horizontal axis identifies the calendar year, so data elements represent a cohort's age in a particular year. Eligible beneficiaries can begin claiming benefits at age 62, and are represented by the shaded boxes in the table. Because nearly all beneficiaries claim Social Security by age 65, everyone below the solid line is most likely claiming benefits. Age groups in the darkest grey all turned 65 prior to May of 1997, so this group is claiming under the old system. The medium gray color represents people who could have enrolled in Social Security under either system. The lightest gray group all turned 62 after 1997, and therefore are all claiming under the new system. To ensure we have a sample of decedents paid under the new system, we use those

[31] http://www.ssa.gov/pubs/2007calendar.htm.

aged 65 to 69 who died in the 2005 and 2006 calendar year, which are the groups outlined by the dotted connected lines on the right side of the graph.

In column (3) of Table 1 we show the results for this group. The coefficient on *Payweek(1)* is statistically insignificant and negative. The lack of precision for this result is not due to small sample sizes, for in column (4) we report results for the old payment system using only two years worth of data (1995-1996) for the same 65 to 69 age range and find a statistically significant two percent increase in daily mortality during *Payweek(1).*

It is no surprise that the payweek and week effects are somewhat muted in this sample, given that the *Payweek* and *Week* variables overlap in similar ways each month. *Payweek(1)* most commonly covers the 3^{rd} to the 9^{th} of the month, and the *Week(1)* variable always covers the 1^{st} to the 7^{th} of the month, so the *Payweek(1)* coefficient is strongly influenced by differences between the 1^{st} and 2^{nd} compared to the 8^{th} and 9^{th} of the month. We are better able to isolate the within-month effect from the payweek effect for Social Security recipients on the new schedule, a group we consider next.

We examine the payday/mortality relationship in the post-May 1997 system using data on 65 to 69 year olds in 2005 and 2006. The restricted-use MCOD data identifies the decedent's exact date of birth, which allows us to place them into three groups: birth dates from the 1^{st} to the 10^{th} of the month (paid on the second Wednesday of the month); birth dates from the 11^{th} to the 20^{th} (paid on the third Wednesday); and from the 21^{st} to the 31^{st} (paid on the fourth Wednesday). For this sample, we allow the dependent variable to vary across days, months, years and birthday groups (*k*), and estimate an equation of the form:

$$\ln(Y_{kdmy}) = \alpha + \sum_{\substack{w=-2 \\ w\neq -1}}^{3} Week(w)_{kdmy}\delta_w + \sum_{\substack{w=-2 \\ w\neq -1}}^{3} Payweek(w)_{kdmy}\beta_w + \sum_{j=1}^{6} Weekday(j)_{kdmy}\gamma_j + \sum_{j=1}^{M} Special(j)_{kdmy}\varphi_j + \lambda_k + \mu_m + v_y + \varepsilon_{dmy} \quad (2)$$

The variables *Week(w), Special(j), Weekday*, μ, v, and ε are defined as before. In this model, we add effects for the birthday-based groups (λ), and *Payweek(w)* variables are now centered on the second, third, or fourth Wednesday of the month, depending on the group. Synthetic months are uniquely defined for each birth date group (*k*). Because pay dates are now fixed on Wednesdays, there are either 28 or 35 days in each synthetic month. If the receipt of income alters short-term mortality, then the mortality cycle patterns should have shifted to different parts of the month for Social Security beneficiaries enrolling after May 1997.

Results from equation (2) for 65 to 69 year olds in 2005 and 2006 are reported in the first column of Table 2. There is a pronounced within-month mortality cycle, with a statistically significant 1.4 percent value on the *Week(1)* variable. There is also a large pay effect: the coefficient on *Payweek(1)* is a statistically significant 1.1 percent.

A shortcoming of this test is that not all recipients are paid based on their own birth date. A person who claims Social Security benefits under their spouse's earnings would actually receive the check based on their spouse's birth date. Consequently, there is some measurement error across the three birth date groups. People who never married should be claiming benefits under their own birth date, so in column (2) of Table 2 we report results for never-married seniors aged 65 to 69 in the 2005 and 2006 MCOD files. There is a much larger increase in the payday effect on mortality. The coefficient on *Payweek(1)* is now 2.75 percent, although it is a much smaller group and so the z-score is only 1.56, meaning the results are statistically significant at a p-value of about 0.12.

The final two columns of the table contain the results of two placebo tests. First, we re-estimate the model from equation (2) by imposing the new payment schedule on decedents aged 65 to 69 in 1995 and 1996, who would have been on the old payment system. The *Payweek(1)* variable should be small and statistically insignificant in this case, and it is. Second, we estimate the same model for decedents aged 50 to 59 in 2005 and 2006, a group not enrolled in Social Security. As expected, we find no impact on *Payweek(1)*. In both columns (3) and (4), we document large and statistically significant within-month cycles.

As we noted above, the work linking mortality to income payments has to date primarily focused on the impact on deaths related to substance abuse. In this section, we estimate models for causes both related and unrelated to substance abuse. Causes of death in the MCOD files are defined using the International Classification of Disease (ICD) codes. Three different ICD versions are used during the period we consider: ICD-8 (1973-8), ICD-9 (1979-98), and ICD-10 (1999-2006). The codes used to identify substance abuse vary across versions, so for the "3rd of the month" analysis we use ICD-9 data from 1979 to 1996. The primary aim of this analysis is to see whether the increase in deaths following income receipt can be solely explained by substance abuse, so we err on the side of defining too many deaths as substance abuse-related, rather than too few. Each death has an underlying cause as well as up to 19 other causes, and we define a substance abuse death as one in which any of the causes has an ICD-9 code associated with substance abuse. The list of causes defined as substance abuse come from Phillips et al. (1999) and studies of the economic costs of substance abuse in the United States (Harwood et al., 1998), Australia (Collins and Lapsley, 2002), and Canada (Single et al., 1999).[32] We classify approximately one percent of deaths among seniors in 1979 to 1996 as substance abuse deaths.

Column (1) of Table 3 contains estimates for equation (1) for all causes of death among seniors during the ICD-9 reporting period of 1979-1996. These results are similar to those in Table 1. We report results for substance abuse in column (2), and find a pronounced within-month mortality cycle – the *Week(1)* coefficient is 1.90 percent, with a p-value of 0.11. There is also a large coefficient (standard error) on the *Payweek(1)* variable of 0.0367 (0.0112). In column (3) we re-estimate the model using non-substance abuse deaths. These deaths represent 99 percent of all deaths from column (1), so it is no surprise that the results in columns (1) and (3) are virtually identical. The results in columns (2) and (3) indicate that, compared to the week prior to payday, there are about 117 extra substance-abuse related deaths each year compared to 1,236 extra deaths from non-substance abuse causes. Even with some under-reporting of substance abuse causes, these results suggest that the effect of income on mortality extends well beyond substance abuse, and in fact that substance abuse deaths are responsible for a minority of the aggregate pattern.

In the final three columns of Table 3, we use both ICD-8 and ICD-9 to create a few broad underlying cause-of-death categories. For each cause, we estimate equation (1) for decedents 65 and older for the entire 1973-1996 period.[33] In column (4), we present results for external causes of death (e.g., accidents, murders, suicides, motor vehicle crashes), and find both a large within-month effect (coefficient and standard error on *Week(1)* is 0.0257 (0.0059)) and a large payweek effect (coefficient and standard error on *Payweek(1)* is 0.0410

[32] A complete list of these codes is provided in an appendix that is available from the authors.
[33] The NCHS recoded ICD-8 and ICD-9 deaths into 34 underlying causes. Our external causes group consists of deaths with codes 33 to 36. Heart attacks (acute myocardial infarctions) have an underlying cause of death code of 410 in both ICD-8 and ICD-9. The cancer category was created using a cause of death recode produced by the National Cancer Institute (available at http://seer.cancer.gov/codrecode/1969+_d09172004/index.html).

(0.0057)). In column (5), we present results for heart attacks, a cause often associated with a short time from onset to death. The pay week coefficients are slightly larger for heart attacks than for all deaths (as reported in column (1) of Table 1). Finally, in column (6), we report results for cancer – a cause of death we can view as something of a placebo test, because we suspect cancer deaths are less affected by activity than most other causes. We do not find either a pay week or within-month cycle for cancer, as the results for *Payweek(1)* and *Week(1)* demonstrate.

b. The Military Payment Schedule

Military personnel are paid on the 1st and the 15th of each month, or on the previous business day when these dates fall on a weekend or a public holiday.[34] In this section, we examine whether mortality spikes on or immediately after these dates.

Between 1973 and 1990 there were anywhere from 2.04 to 2.25 million military personnel in the US, before falling to 1.38 million in 2001 and then increasing slightly thereafter.[35] Active duty military are predominantly male (currently 85 percent), young (approximately one half are under 25 years of age) and healthy (Segal and Segal, 2004). Newspaper accounts suggest that many military personnel spend more than average on and immediately after payday. The phenomenon appears to be widespread, with large payday-generated increases spending at bars, restaurants, cinemas, malls and hairdressers reported near bases in Connecticut,[36] Hawaii,[37] North Carolina,[38] South Carolina[39] and Virginia.[40]

[34] We can date this policy as early as 1971, https://www.usna.com/SSLPage.aspx?pid=6121 but no older veteran or military expert we spoke with could remember a time when wages were not paid on these two dates.

[35] Authors' calculations from various issues of the *Statistical Abstract of the United States*.

[36] Carbone, Gerald M. "Dive! Dive! Groton Fears Loss of Base," *The Providence Journal*, Providence RI, Feb 28, 1995, p.A01.

In this section, we compare mortality patterns in counties with and without a high proportion of their population on active military duty. Soldiers normally reside on or near the base to which they are attached, and these bases are unevenly distributed throughout the country. Since both the size of the military and base locations were fairly uniform over the 1973 to 1988 period, and since the public-use MCOD files contain exact dates of death during this time, we focus on these years.

We identified counties with more than 15 percent of their population aged 17 to 64 who were military personnel in the 1970, 1980 and 1990 Censuses using Census Summary File 3 data sets.[41,42,43] There are 21 counties that meet this criterion.[44] In 1990 there were roughly 326,000 people aged 17 to 64 in these "military" counties, of which about one quarter were in the military. Given that military personnel have a large number of dependents and bases typically employ many civilians paid on the same schedule,[45] the proportion of the population affected by the military payment schedule in these areas would have been much higher than 25 percent. We compare the mortality patterns for people from this group of counties with a comparison sample of people from 2,772 "nonmilitary" counties

---

[37] Song, Jaymes. "Many Businesses that Rely on Sales to the Military Struggle to Survive with Recent Mass Deployments," *Honolulu Star-Bulletin*, Honolulu HI, June 21, 2004.

[38] Foster, J. Kyle. "Retail Boost," *The Fayetteville Observer*, Fayetteville NC, May 7, 2001, Local & State Section. Mullen, Rodger. "It Must be Payday," *The Fayetteville Observer*, Fayetteville NC, March 11, 1990, Lifestyle Section.

[39] Crombo, Chuck. "Base Echoes with Sounds of Silence," *The State*, Columbia SC, March 16, 2003, p.A1.

[40] Snead Fulk, Sande. "Lifeblood of a Local Economy; Advisory Committee Studies Base's Impact," *The Richmond Times-Dispatch*, Richmond VA, May 22, 2002, p.4.

[41] Enlistment in the military can occur at age 17 years with parental consent, and at age 18 years without.

[42] These data are taken from the National Historical Geographic Information System.

[43] Counties that changed boundaries between 1970 and 1990 were merged prior to this exercise (changes are at http://wonder.cdc.gov/WONDER/help/Census1970-2000.HTML). There were many changes to Alaska's county-equivalent geographic boundaries over this period, so we did not use Alaskan deaths in this analysis.

[44] The States (Counties) in our sample are: AL (Dale), GA(Chattahoochee, Liberty), ID (Elmore), KS (Geary, Riley), KY (Christian, Hardin), LA (Vernon), MO (Pulaski), NE (Sarpy), NC (Cumberland, Onslow), OK (Comanche, Jackson), SC (Beaufort), TN (Montgomery), TX (Bell, Coryell, VA (Norfolk City), WA (Island).

[45] Data from various issues of the *Statistical Abstract of the United States* indicate that during our analysis period, about one million civilians were employed annually by the military.

that have less than one percent military among adults aged 17 to 64 in the 1970, 1980 and 1990 Censuses.

While the widespread nature of the within-month mortality cycle may mean military and non-military counties exhibit a similar time series in mortality counts around the 1st of the month, we expect a much greater frequency of paycheck distributions around the 15th in military counties compared to non-military counties because the predominant payment frequency outside the military is weekly or biweekly.[46]

In Figure 4, we use data from the 1973-1988 MCOD to construct daily mortality counts for our sample for the seven days before and after military paychecks are distributed. The solid line in the graph represents the daily mortality risk for military counties and the dotted line is for non-military counties. The vertical lines from each point represent the 95 percent confidence interval for the daily mortality risk.

The two groups show similar pattern around the first payday of the month. There is a within-month mortality cycle for both military and nonmilitary counties, with deaths declining before checks arrive and rebounding afterwards (perhaps accentuated by weekend days disproportionately coming after payments). The day after military paychecks arrive is the peak mortality day for both groups in this two-week cycle. Compared to the day before payment (*Payday -1*), deaths the day after payment (*Payday 2*) are 9.3 percent higher in military counties and 6.4 percent higher in nonmilitary counties. For all days throughout this two-week period, we cannot reject the null that both groups have the same mortality risks.

The pattern is more pronounced for military counties around the arrival of the second paycheck. The day prior to the second wage payment, there is a drop in daily mortality of

[46] Data from the 1996-2004 Diary Survey Record of the CEX indicate that only 9.6 percent of workers report their last pay check as being paid monthly, while only 5.5 percent report being paid twice-monthly.

5.6 percent in the military counties compared with 2 percent in nonmilitary counties. Likewise, mortality is 9.6 percent higher in military counties on the day after the second paycheck of the month arrives, while the comparison counties show a 1.8 percent excess mortality on this day. For the day after the second paycheck is distributed, we can reject the null hypothesis that the mortality rates are the same in the military and nonmilitary counties.

To formally test whether military and nonmilitary counties exhibit different mortality patterns around the 1st and 15th of the month, we estimate a model similar to equation (1). A key difference is that, because daily mortality counts in the military counties are small and occasionally zero, we use a negative binomial model that allows for integer values and estimate it by maximum likelihood (Hausman, Hall and Griliches, 1984). Let Y_{idmy} be daily mortality counts for group *i* (for military and nonmilitary counties) on day *d*, month *m* and year *y*. Let X_{idmy} be vector that captures the exogenous variables in equation (1). Within the negative binomial model, $E[Y_{idmy} \mid X_{idmy}] = \delta \exp(X_{idmy}\beta)$, where δ is a parameter that captures whether the data exhibits over-dispersion.[47] By definition, $\partial \ln E[Y_{idmy} \mid X_{idmy}]/\partial X_{idmy} = \beta$ so the parameters in this model are interpreted similarly to those in equation (1).

In constructing the data set, the "synthetic" months are 28-day periods that include the seven days before and after the two military checks are distributed each month, and begin

[47] It can be demonstrated that the variance of counts in the negative binomial model is $Var[Y_{idmy} \mid X_{idmy}] = \delta^2 [1+(1/\delta)]\exp(X_{idmy}\beta)$, so the variance to mean ratio in this model is $\delta + 1$. When $\delta>0$ the variance grows faster than the mean and the data exhibit over-dispersion and when $\delta=0$ the negative binomial collapses to a Poisson model which, by construction, restricts the variance to equal the mean.

seven days before the first payment each month.[48] When the 1st or the 15th of the month are on a weekend or a public holiday, wages are paid on the closest prior working day.[49]

The exact specification for equation $X_{idmy}\beta$ is of the form:

$$(3) \quad X_{idmy}\beta = \beta_0 + \sum_{j=1}^{6} Weekday(j)_{dmy}\gamma_j + \sum_{j=1}^{M} Special(j)_{dmy}\varphi_j + \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Military_{idmy} Period1_{idmy} Payday_d \beta_{1md} + \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Military_{idmy} Period2_{idmy} Payday_d \beta_{2md} + \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Nonmilitary_{idmy} Period1_{idmy} Payday_d \beta_{1nd} + + \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Nonmilitary_{idmy} Period2_{idmy} Payday_d \beta_{2nd} + Period1_{idmy}\beta_p + Military_{idmy}\beta_m + (Period1_{idmy})(Military_{idmy})\beta_m + \mu_m + v_y$$

where *Weekday, Special,* and the synthetic month and year effects are defined as before. We control for differences across groups with a dummy for counts in military areas (*Military*), across pay periods with a dummy for the first pay period (*Period1*), and also interact these two variables. The variables *Payday* are a series of 13 dummy variables defined for the seven days before and seven days after wage payments except for *Payday(-1)*, which is the day before checks are distributed. We add *Nonmilitary* and *Period2* dummies, and estimate four vectors of coefficients on the payday variables: one each for military and nonmilitary counties around the first pay period of the month (β_{1md} and β_{1nd}, respectively), and similar values for the second pay period (β_{2md} and β_{2nd}). We examine whether the daily mortality patterns differ across the two groups by testing the null hypothesis H_o: $\beta_{jnd} = \beta_{jmd}$ for all *Payday(d)*.

[48] Days outside of the 28-day pay periods are dropped from the analysis. The two pay periods in each month do not overlap, except when Presidents Day falls on the 15th of February and the seven days after the previous wage payment overlaps with the seven days before this payment. The 28 days around these two payments (25th January–18th February) is removed when this happens in 1982 and 1988.
[49] The relevant public holidays that alter payments in this section are New Year's Day, Presidents Day, Labor Day and Martin Luther King Day (since 1986).

The maximum likelihood results for the negative binomial model are reported in Table 4. Columns (1) and (2) present the coefficients on the payday dummies for the first pay period, for military counties and non-military counties respectively. Standard errors allow for arbitrary correlation across observations within the same 28-day synthetic month. Column (3) reports the p-value on the -2 log-likelihood test statistic for the null hypothesis that military and non-military coefficients for a particular day are equal. The final three columns repeat the same set of results for the payday near the 15th of the month.

The results in Table 4 correspond with the visual evidence in Figure 4. In the first pay period, deaths are lowest in both sets of counties the day before paychecks arrive and highest the day after paychecks arrive, with deaths increasing by a statistically insignificant 4.7 percent in military counties and a statistically significant 2.1 percent in nonmilitary counties.

The differences are clearer in the second pay period. There is a large decline in mortality the day before the mid-month check arrives in military counties, as evidenced by the large positive coefficients before and after *Payday(-1)*. Mortality is 6.3 percent higher the day checks arrive compared to the day before (p-value of 0.085). The corresponding numbers for *Payday(2)* and *Payday(3)* are 11.8 percent (p-value < 0.001) and 5.6 percent (p-value of 0.125), respectively. In contrast, in nonmilitary counties, the coefficients on these same three dummy variables are smaller than four-tenths of a percent. For *Payday(1)* and *Payday(2)*, we can reject the null at the 0.05 level that the coefficients are the same across military and nonmilitary counties, while the p-value for this test on *Payday(3)* is 0.11.[50]

[50] The results move in the expected direction as we change the criteria for what constitutes a military county. If we only include as treated counties as those where the fraction of adults aged 17 to 64 must exceed 20 percent, average daily mortality falls to about 7 which should increase standard errors (because we increase the

We suspect the large difference in results between the first and second payday of the month for military personnel to be due to a combination of factors. As we noted above, most households have large re-occurring bills due at the 1st of the month, so much of the paycheck paid near the 1st of the month will go towards these items. This means the second paycheck of the month might have a larger discretionary component. Non-military counties will not display this pattern around the 15th of the month since so few outside the military are paid on a twice-monthly basis.

As in the previous section, we identify deaths related and unrelated to substance abuse using the same procedure. Between 1979 and 1988, approximately 10 percent of deaths among those aged 17 to 64 are defined as substance abuse deaths. There were 9.9 deaths per day in military counties during this period, with 8.8 deaths per day unrelated to substance abuse. In a negative binomial model of the non-substance abuse deaths, the coefficients (standard errors) on *Payday(1)* through *Payday(3)* for the paycheck near the 15th of the month for military counties are 0.0537 (0.0441), 0.0818 (0.0437) and 0.0675 (0.0433), respectively. The z-scores for *Payday(2)* and *(3)* are 1.87 and 1.54 respectively. The same set of coefficients for non-military counties are -0.0055 (0.0044), 0.0045 (0.0044), and 0.0013 (00047), and the p-values on the tests that the daily effects are the same across the two groups for the three days are 0.18, 0.08, and 0.13. While we still see large increases in

variability of daily deaths) but the coefficients should increase (as the counties have a higher fraction of treated people). This is close to what we find. The coefficients (standard errors) [p values on test of equality] for Payday 1, 2 and 3 in the second payday among military counties in this new sample are: 0.0840 (0.0439) [0.025], 0.1104 (0.0394) [0.006], and 0.0587 (0.0422) [0.160]. If we reduce the required fraction of adults in the military to 10 percent, the number of counties rise, the average daily deaths are now 16.2, meaning standard errors should fall as the day to day variance in death rates declines but coefficients also decrease as the impacted fraction of the population falls. This is exactly what we find. The coefficients (standard errors) [p values on test of equality] for Payday 1, 2 and 3 in the second payday among military counties in this new sample are: 0.0638 (0.0288) [0.010], 0.0672 (0.0262) [0.015], and 0.0559 (0.0287) [0.041].

non-substance abuse deaths, the accuracy of each estimate has decreased and the tests identifying differences across groups are imprecise.[51]

IV. The Mortality Consequences of One-time and Infrequent Income Receipt

In this section, we consider the short-term mortality impact of one-time and infrequent income receipt. Specifically, we consider two cases: the 2001 Tax Rebates and the annual Alaska Permanent Fund payments. Both of these cases have been considered by authors in the literature on excess sensitivity. These two situations broaden the empirical work in this paper along three dimensions. First, these income changes can be considered exogenous increases in income (wealth), unlike the two cases in the previous section. The mortality impact of these payments could generate very different patterns. Second, these groups extend the phenomenon beyond the elderly and military personnel. Third, the infrequent nature of the payments will allow us to determine whether increases represent "short-term mortality displacement" where the deaths of the frail were hastened by a few days, a phenomenon routinely referred to as "harvesting" (Zeger et al., 1999).

a. The 2001 Tax Rebates

The *Economic Growth and Tax Relief Reconciliation Act*[52] was signed into law on June 7, 2001 and included a reduction in the tax rate on the lowest income bracket from 15 to 10 percent. This tax change was applied retroactively for income earned in 2001 and, as an advance payment on the tax cut, households were sent rebates based on their 2000 tax returns

[51] Given the smaller sample size and the small number of deaths per day for substance abuse deaths, none of the coefficients on the *Payday(d)* variables were statistically significant.
[52] http://www.gpo.gov/fdsys/pkg/PLAW-107publ16/pdf/PLAW-107publ16.pdf.

in the summer and fall of 2001. Approximately two-thirds of all households in the United States received a rebate check. The maximum rebates for single and married taxpayers were $300 and $600, respectively. Johnson, Parker, and Souleles (2006) estimate households received about $500 on average, or about one percent of median annual family income.

Rebate checks were mailed over a ten-week period and check distribution dates were based on the second-to-last digit of the Social Security number (SSN) of the person filing the taxes.[53] The first checks were sent on Monday, July 23, to taxpayers whose second-to-last SSN digit was a zero.[54] Table 5 shows the exact distribution dates of checks by SSN. The Treasury Department sent letters to taxpayers a few weeks before checks arrived to inform them of the size and date of their check (Johnson, Parker and Souleles, 2006).

This tax rebate is a powerful quasi-experiment for testing the mortality consequences of income receipt, as the second-to-last digit of the SSN is effectively randomly assigned.[55] Johnson, Parker and Souleles (2006) use this fact and data from a special module in the CEX to show that consumption of nondurable goods increased in the months after the arrival of checks, with food away from home being the main component that was affected. In contrast to these results, Shapiro and Slemrod (2003) found a minority of households planned to spend their rebate.

[53] For married taxpayers filing jointly, the first Social Security number on the return determined mailing date.

[54] Households who filed their year 2000 tax return late may have been sent their rebates after the ten-week period shown in Table 5. According to Slemrod et al. (1997) 92 percent of taxpayers typically file on or before the normal April 15 deadline, so the vast majority of households would have received their checks according to the schedule outlined in Table 5.

[55] The last four digits of the SSN are assigned sequentially within a geographic area, so are effectively random. The second-to-last digit mailing system was in fact chosen because it was felt the random assignation made it a fair way to allocate the checks (Johnson, Parker and Souleles, 2006).

We use the check distribution schedule to examine the short-run consequences of the rebates on mortality. For this project, the NCHS merged the second-to-last digit of a decedent's SSN from the National Death Index (NDI)[56] to the 2000-2002 MCOD data files.

The econometric model for this event is straightforward. Let i =0 to 9 index groups of people based on the second-to-last digit of their SSN. Let t index one of 30 7-day periods during 2001, with the first period beginning on Monday May 14th and the last beginning on December 3rd. This 30-week period starts ten weeks prior to the first check being distributed and ends ten weeks after the last check was sent. Let y_{it} be the deaths for group i in week t and let $REBATE1_{it}$ be a dummy variable that equals one for the week group i received a check. The estimating equation is then

$$(4) \qquad \ln(Y_{it}) = \alpha + REBATE1_{it}\beta_1 + \eta_i + \nu_t + \varepsilon_{it}$$

where υ_t are fixed week effects, η_i are fixed group effects, and ε_{ij} is a random error term. The group effects identify persistent differences in weekly mortality counts that vary across groups, but since the second-to-last digit of a SSN is randomly assigned there should be little difference in mortality rates across groups. The week effects capture the differences that are common to all groups but vary across weeks. For example, the 9/11 terrorist attacks occurred during Week 18 in our analysis. The Centers for Disease Control estimates that there were 2,902 deaths associated with September 11th, which is roughly twenty percent of weekly deaths during this period.[57] There also appears to be a drop in mortality in the weeks just after September 11th as individuals stayed home and reduced their travel. The week effects will capture these cyclic changes in mortality so long as the deaths associated with

[56] The NDI is an index of death record information designed to assist medical and health researchers who want to ascertain whether subjects in their studies have died, and includes each decedent's SSN. More information about the NDI can be found at www.cdc.gov/nchs/ndi.htm.
[57] http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm.

September 11 are equally distributed across the 10 SSN groups. The coefficient on β_1 is the key variable of interest and it identifies the short-run impact of the rebates on mortality.

There are two caveats to equation (4). First, only taxpaying units with taxable income in 2000 received a tax rebate in 2001. The coefficient on β_1 represents a reduced-form effect and not the impact of actually receiving a check. Therefore, a key to the analysis is to reduce the sample to people likely to have received a tax rebate. We do this by restricting the sample to those aged 25 to 64, who are much more likely to have paid taxes than other age groups.[58] Second, for married couples filing jointly, the rebate check was sent according to the SSN of the first name on the IRS 1040 form. This form does not record the sex of the taxpayers so we have no idea whether husband or wives are more likely to be listed as the first taxpayer. Although both partners in a marriage are presumably treated by the additional income, the mailing of the check was based on the SSN of only one of them. Because people not sent a check but treated with a rebate through their spouse should be randomly distributed across the different groups, this should systematically bias our results towards zero. Later, we reduce the sample to unmarried taxpayers, a group where we should be better able to identify rebate recipients.

The results for equation (4) are reported in Table 6. The SSN groups experience a statistically significant 2.7 percent increase in mortality in the week the checks arrive. There is a large p-value on the test that all the group fixed effects are zero, adding empirical support to the assumption that the second-to-last digit of the SSN is randomly assigned. Overall, the results suggest a large short-term increase in mortality immediately after income receipt.

---

[58] The IPUMS-CPS project (King et al., 2004) has attached estimates of taxable income to March Current Population Survey (CPS) data. Using data from the 2001 March CPS (2000 tax year), their estimates suggest that 52 percent of people aged 25-64 were in households that paid federal income taxes but this same number for people aged 65 and older was 26 percent.

Although we would prefer to estimate standard errors from equation (4) that allow for correlation in residuals within each group, Monte Carlo estimates suggest that these Huber/White-type procedures perform poorly when the number of groups is small (Wooldridge, 2003). The residuals from column (1) of Table 6 regressed on a one-period lag generate an estimate of the AR(1) coefficient (standard error) of 0.0085 (0.0584), suggesting that autocorrelation is not a problem in this case.

In column (2) of Table 6, we add *REBATE2*, *REBATE3*, and *REBATE4*, which are dummies for the second, third and fourth week after the checks arrive, respectively, to examine whether the increase in mortality in the first week represents mortality displacement. If there is significant short-term displacement, then we should find that the sum of the coefficients in subsequent weeks should be negative and close in magnitude to the estimate for *REBATE1*. Notice that in the third week after the checks arrive there is a large drop in mortality that is similar in magnitude to the coefficient on *REBATE1*. Adding the *REBATE1* through *REBATE3* coefficients in column (2) produces an estimated change (standard error) in mortality of -0.0151 (0.0194). We cannot reject the null of no aggregate change in mortality over the first three weeks after checks arrive.

We define substance abuse-related deaths using the ICD-10 codes in a similar way as in the previous two sections, and allocate eight percent of deaths in this sample to substance abuse, which represents 85 deaths per group per week.[59] Column (3) of Table 6 contains the results for substance abuse deaths, and only the negative coefficient on *REBATE4* approaches statistical significance. Column (4) contains results for deaths not related to substance abuse,

[59] The list of ICD-10 codes comes from the Australian study (Collins and Lapsley, 2002) and updates of the United States (available at http://www.ncjrs.gov/ondcppubs/publications/pdf/economic_costs.pdf) and Canadian studies (available at http://www.ccsa.ca/Eng/Priorities/Research/CostStudy/Pages/default.aspx) used already.

and the results are nearly identical to the results for all deaths in column (2), showing once again a relatively minor role for substance abuse in the aggregate relationship.

In the final two columns of Table 3, we re-estimate the model eliminating all data after week 17, which are observations after the September 11th attacks. The results are qualitatively similar to those obtained in the first two columns.

As noted above, we can more accurately identify who receives the check by restricting the sample to never-married, widowed, divorced and separated taxpayers.[60] Among non-married adults aged 25 to 64, the IPUMS March CPS data estimates that 67 percent paid taxes in 2000. Restricting the sample to the unmarried generates similar results, with a coefficient (standard error) on *REBATE1* of 0.0280 (0.0134).

While reducing the sample to specific causes of death produces few statistically significant coefficients due to the increased variance associated with disaggregated causes of death, results suggest causes related to activity and consumption levels drive the aggregate pattern.[61] Importantly, we find no impact of the rebates on single-cause cancer deaths (coefficient and standard error on *REBATE1* of 0.0010 (0.0268)) and no effect when we estimate two placebo regressions using the same periods and group definitions as 2001, but re-estimated using 2000 and 2002 MCOD data. The coefficients (standard error) on *REBATE1* in these two models are 0.0094 (0.0102) and -0.0174 (0.0102), respectively.

b. Dividend Payments from the Alaska Permanent Fund

[60] The exception would be people who became divorced, separated or widowed since filing their year 2000 tax return, which should be a small number of people.

[61] The coefficients (standard errors) on *REBATE1* and *REBATE2* for regressions using weekly counts for particular causes (ICD-10 codes) are as follows: Liver disease and cirrhosis (K70, K73-4), 0.0714 (0.0405) and -0.0675 (0.0633); heart attacks (I21), 0.0356 (0.0270) and -0.0376 (0.0269); and traffic accidents (code 38 in the NCHS 39-cause recode), 0.0399 (0.0411), and 0.006 (0.030).

The Alaska Permanent Fund was established in 1976 to invest income received by the State of Alaska from the sale of oil, gas, and other minerals for the long-term benefit of current and future Alaskans. The fund has grown significantly over time, and had assets worth approximately $35.9 billion at the end of the 2008 financial year.[62] Since 1982, an annual dividend has been paid to Alaskans from the income generated by fund investments during the previous five years. The amount paid has been between $331 in 1984 and $2,069 in 2008 (when a one-off additional payment of $1,200 was also made).

Alaska residents who have lived in the state for at least one year are eligible for the dividend, and the same amount is paid to everyone, regardless of their length of residency, age, or income.[63] Individuals must apply each year to receive the dividend, and at least 88 percent of Alaskans have received the dividend each year. Table 7 contains the dividend amounts and the percentage of the population receiving them in recent years.

Hsieh (2003) uses variation in the size of dividends by family size and over time to test whether nondurable consumption changes in response to dividend payments. Using the CEX from the 1984 to 2001, he finds no evidence households react to these payments – even though household consumption is sensitive to income tax refunds – which leads him to conclude that households adhere to the LC/PIH for large and predictable payments (like the Alaska dividend), but not for small and less predictable payments (like income tax refunds). In recent years, however, the dividend payments have been concentrated in early October and

[62] From the 2008 Annual Report of the Alaska Permanent Fund Corporation. Available at: http://www.apfc.org/home/Content/reportspublications/reportArchive.cfm.
[63] Residency requirements have been the same since 1990. Minor changes occurred in earlier years. Historical information is available at: https://www.pfd.state.ak.us/historical/index.aspx

anecdotal evidence of increased spending after dividends arrive suggests activity-induced changes in mortality are possible as a result of the dividend.[64]

We explore the short-term relationship between income payments and mortality for recent years. Payments were initially made entirely by check, mailed at a rate of 50,000 per week. Payment by direct deposit was introduced in 1993. Approximately 30 percent of recipients initially received their dividend this way, which grew to two-thirds of recipients by 2001 and three-quarters by 2006. Direct deposits are made on only one or two dates, and since at least 2000, over 90 percent of paper checks were processed and mailed in a single batch shortly after the payment of direct deposits. The exact dates that direct deposits were paid, as well as the dates checks were issued, are shown in Table 7 for the years 2000 to 2006. We use the timing of direct deposits from 2000 through 2006 to investigate whether dividend payments change mortality patterns among Alaskans. We focus on this period because of the popularity of direct deposit and the close proximity between the receipt of direct deposits and paper checks.[65]

The primary data for this analysis are from the MCOD restricted-use files from 2000 through 2006, which include decedents' state of residence. We create separate weekly counts of deaths for Alaskans and residents of the rest of the United States for periods that include the direct dividend payments and several weeks afterwards.[66] The econometric

---

[64] See for example: Chambers, Mike. "Alaska Permanent Fund dividend is $1,850.28," *The Associated Press*, State & Local News, September 19, 2001; Egan, Timothy. "Fringe Benefits from Oil Give Alaska a Big Payday," *The New York Times*, October 9, 1996, p. A1; Pemberton, Mary. "Alaskans prepare to spend annual windfall," *The Associated Press*, State & Regional News, October 3, 2003.

[65] Since 1998, the estates of Alaskans who applied for the dividend in March but died prior to its payment around October have received the full amount. Using this time period therefore also allows us to rule out any bequest-related "death elasticity" of the sort suggested by Kopczuk and Slemrod (2003).

[66] Alaska has a disproportionate number of aircraft and fishing accidents (Baker et al., 1992). Fatalities from these events can be significant relative to the number of deaths in Alaska in any single week. To decrease the variation in weekly deaths from disasters and work-related deaths, in both the Alaskan and non-Alaskan groups

model here is a simple difference-in-difference specification, with the data for the rest of the U.S. providing an estimate of the time path that would occur in the absence of the dividend intervention. Let *w* denote twelve seven-day periods that begin on Tuesdays,[67] with the first period each year beginning fifteen days after Labor Day (the first Monday in September).[68] Let $\ln(y_{swy})$ be the natural log of the deaths for state *s* (with *s*=1 for Alaska or *s*=0 for all other states) in week *w* and year *y*. *Dividend(1)* is a dummy that equals one the first week after dividend payments are made and zero otherwise, and *Alaska* is a dummy variable for the state of interest. The model we estimate is:

$$(5) \qquad \ln(Y_{swy}) = \alpha + Dividend(1)_{wy}\, Alaska_s \beta_1 + Alaska_s \beta_3 + \nu_{wy} + \varepsilon_{swy}$$

where ν_{wy} is a fixed effect that varies by week *w* and year *y*, and ε_{swy} is a random error. The *Alaska* dummy variable controls for persistent differences in mortality counts between Alaska and the rest of the United States. The fixed week/year effects capture differences common to both groups, but which vary over time. The parameter β_1 captures the short-run impact of the dividend payments on mortality. As in the previous section, we examine whether estimated mortality effects for the week after payments are made are the result of harvesting by including *Alaska**Dividend(2)* to *Alaska*Dividend(4)* in subsequent models.

The results for equation (5) are reported in Table 8. In the first two columns, we report results for models using all Alaskan deaths. In column (1), we only include *Alaska**Dividend(1)*; in column (2), we include *Alaska*Dividend(2)* to *Alaska*Dividend(4)* as well. The results for the Alaska Permanent Fund tell a story similar to the one told by the

---

we remove deaths with an Underlying Cause-of-Death 358 Recode of 400 (Water transport accidents) or 401 (Air and space transport accidents).

[67] All direct deposits during 2000 to 2006 were made on Tuesdays, Wednesdays or Thursdays.

[68] We select the post-Labor day period for this analysis because daily mortality counts in the end of August and the first two weeks of September were incredibly volatile and did not match the trends in mortality counts for residents from other states.

results for the 2001 tax rebate. In column (1), we see an increase in deaths of 6.7 percent for the week checks are received, but the result is not statistically significant. The results in column (2) suggest substantial harvesting, with the coefficients on *Alaska*Dividend(2)* and *(3)* being -2.6 percent and -9.5 percent, respectively. This final number has a t-statistic of 1.77, which is statistically significant at the 10 percent level.

With about one-fifth of the land mass as the continental United States but only 670,000 residents, Alaska is the most sparsely populated state. A large fraction of residents live in remote areas and have limited access to the Internet, banking services, the postal service, etc.[69] In conversations with representatives of the Alaska Permanent Fund, they indicated that a much larger fraction of the direct deposit recipients live in the urban areas of Alaska. In columns (3) and (4) of Table 8, we restrict our attention to residents in the boroughs that contain Anchorage (260,283 residents in 2000 Census), Fairbanks (30,224) and Juneau (30,711), the only cities in Alaska with more than 10,000 residents.[70] In this model, we keep the same comparison group of non-Alaskan residents, as nearly everyone in the United States lives in a county with a town of more than 10,000 people.

In this urban sample, there is a 12 percent increase in mortality – an extra four deaths – the week direct deposit occurs. The p-value on this statistic is less than 0.10. As in both column (2) and the case of the 2001 tax rebates, we see a drop in mortality the third week after dividends are paid, suggesting a large fraction of these deaths represent short-term mortality displacement. In this instance, however, the increase in mortality may not entirely

---

[69] Data from the 2000 Census indicates 16.5 percent live in areas with fewer than 1,000 people or in no defined place.

[70] Alaska is organized into boroughs, which are equivalent to counties and form the basis for the Federal Information Processing System (FIPS) codes in the state. The restricted-use MCOD data identifies the FIPS code of residence for all decedents over this time period.

be harvesting. The sum of the coefficients over the first three weeks after checks arrive is 0.068, and over the first four weeks is 0.149, although neither sum is statistically significant.

As with the previous tests, the results are not entirely due to substance abuse. Using the same ICD-10 coding as in the tax rebate section, we attribute 8 percent of deaths among Alaskans to substance abuse. The impact of the Permanent Fund payments on non-substance abuse deaths, reported in columns (5) and (6), is similar to the corresponding values for deaths in columns (3) and (4).[71] The coefficient on *Dividend(1)* is 0.1304 and its t-statistic is 1.62, so the p-value for the test that this coefficient is zero is 0.11. In this case, the sum of the coefficients on *Dividends(1)* through *(3)* is 0.116,.

V. Discussion

As we outline above, a number of authors have documented a paycheck cycle where consumption increases after the receipt of income. These results have been interpreted as being consistent with liquidity problems and hyperbolic discounting, and at odds with the life-cycle/permanent income hypothesis. In this paper, we document a similar phenomenon in health: mortality increases immediately after the receipt of income. The effect is broad-based, occurring for a wide variety of payments methods (transfer payments, paychecks, one-time cash bonuses, and annual residency-based dividends), a range of causes of death (substance abuse deaths, external causes, and heart attacks), and a range of populations (the elderly, tax payers, residents of Alaska, and people living near military bases).

Changing levels of consumption/activity is the most plausible mechanism through which income receipt affects mortality. The findings for particular causes of death are

[71] There are too few substance abuse-related deaths in Alaska to estimate the impact of dividend payments on these deaths.

consistent with this: we observe such relationships for causes of death connected to short-term consumption – like heart attacks and traffic accidents – but not for cancer deaths, where no such connection exists.

Three alternative reasons for such a relationship are improbable. First, the change to the Social Security payment schedule and the structure of the 2001 tax rebates allow us to rule out within-month or seasonal factors that coincide with income receipt. Second, the criteria for receiving these payments should not encourage people to improperly record dates of death for financial gain. Payments to Social Security beneficiaries cease the calendar month after death, so a person who dies in July is not expected to get a check in August. Therefore, the only incentive is to change the timing of checks at the end of the month, not in the middle of the month when checks actually arrive.[72] Military paychecks are paid for income that has already been earned so misreporting death dates cannot change that value. Likewise, a deceased applicant's Permanent Fund dividends go to their estate and the tax rebates were based on tax returns from the previous year. Third, there is a literature suggesting that some patients tend to die right after milestone dates are reached (e.g., birthdates, anniversaries, holidays, etc.). While it is possible that income recipients wanted to hang on for one more check, the large spike in mortality for external causes and heart attacks and the lack of any effect for cancers runs counter to this argument.

Before discussing some implications, it is important to stress that we cannot say anything about whether people are maximizing their own welfare. Non-smoothing consumption behavior is consistent with a number of utility maximization models, including hyperbolic discounting (Shapiro, 2005). Moreover, increased mortality does not necessarily

---

[72] www.ssa.gov/pubs/10008.html

reflect contemporaneous poor health: those whose deaths have been hastened by a few days may have been in poor health already, and external causes of death are largely unconnected to short-term variation in a person's health.

Although the percentage change in daily mortality from any particular event above may seem small, relative to general movements in mortality, however, these results are substantial. For example, mortality for 65 to 69 year olds increases by 1.1 percent the week after Social Security checks arrived in 2005 and 2006. There are 471 deaths per day among this group, so paycheck receipt increases mortality by 36.3 deaths per week or a total of 435 additional deaths per year. In 2005 there were 5,532,900 people aged 65 to 69, so the death rate increased by 7.86E-5 (435/5,532,900) the week after paycheck receipt throughout an entire year. To demonstrate the significance of this increase, we select a sample of 15,774 adults aged 65 to 69 using data from the 1987-1990 National Health Interview Surveys Multiple Cause of Death (NHIS/MCOD) data file.[73] We regress a dummy variable that equals one if a person died within 365 days of the initial interview on the natural log of family income, a dummy for gender, a set of race/ethnicity indicators, three indicators for education, six indicators for marital status, and a complete set of age and year-of-survey effects. The coefficient (standard error) on log of family income in this regression is -0.00297 (0.00151). Assuming that this represents a causal relationship, these results suggest that in order to produce an increase in the mortality rate by 7.86E-5, income in this group would have to decrease by 2.65 percent, which is roughly equal to removing the average annual cost-of-living adjustment made to Social Security payments over the past decade.

[73] This file provides mortality information for National Health Interview Survey respondents by matching surveys to the National Death Index. A more detailed description of this data set and the sample can be found in Snyder and Evans (2006).

The results are even more dramatic in the case of military pay. Among those aged 17-64, there are 10.1 deaths per day in military counties during our period of analysis. In the three days following the second payday, deaths increase by 6.3, 11.8 and 5.6 percent respectively. This represents an increase of 2.4 deaths after the second pay period of each month, or about 28.7 additional deaths per year. Given a population of 326,000 people in those counties in 1990, this is an aggregate increase in the one-year mortality rate of 8.8E-5. Using the same NHIS/MCOD data above but restricting the attention to people aged 17-64, a linear probability model with a dummy for one-year mortality as the outcome of interest produces a coefficient (standard error) on the ln(family income) variable of -0.00084 (0.00014). Assuming this estimate represents the causal estimate of income on mortality, this value indicates that to raise one-year mortality rates by 8.8E-5 would require a 10.4 percent drop in income (8.8E-5/-8.4E-4).

These results have implications for research on the socioeconomic determinants of health. As we noted in the introduction, the authors who have attempted to determine whether there is a causal impact of income on health have generated inconsistent results. The short-term mortality impact of income receipt suggests two things about this literature. First, authors must measure the impact of income from the time of receipt, because there are immediate consequences which may be very different from those in the long-term. Second, the short-term mortality effect of income receipt makes it more difficult to use exogenous variation in income to identify a causal link between income and health. This increases the size of the sample or of the income shock required to find a statistically precise income/health relationship.

The results outlined above also suggest a potential mechanism for the pro-cyclic nature of mortality that is outlined in Ruhm (2000). The estimates in Ruhm and subsequent papers isolate a contemporaneous correlation between mortality and measures of the business cycle; yet to date, little has been offered to explain the pathways producing this result. However, if activity rises over the business cycle, then the short-term mortality effects of income receipt may provide just such an explanation. It may also account for much of the within-month mortality cycle.

There are potential policy consequences flowing from these results. First, the heightened mortality associated with income receipt might suggest that emergency rooms, hospitals, police, and fire departments should adjust staffing levels in accordance with predictable high- and low-mortality days. Our search of the Internet has so far not provided any anecdotal evidence that such adjustments already exist. Finally, we noted in the introduction that some health researchers have suggested that a way to reduce inequality in health outcomes across socioeconomic groups is to simply increase income transfers to low income groups. The results in this paper indicate that any long-term health benefits of such a policy are mitigated by these short-term mortality fluctuations.

Although our work may answer some questions, we also create many interesting questions as well. First, although it is tempting to conclude that greater pay frequency may mitigate some of the damage associated with payday mortality, it is not clear from our results that this is the case. The fact that the spikes in seniors' mortality moved when paycheck payment dates were altered suggests that the payday itself is the cause. However, the experience in the military gives us pause as to the effectiveness of higher frequency payments. In that case, we found a large increase in mortality associated with the paycheck distributed near midmonth. Our conjecture is that since large bills such as rent/mortgage and car payments are bunched near the first of the month, less money from that paycheck is left over for discretionary items. In contrast, the midmonth check has less competition for resources and hence the larger mortality effect. If mortality is linked to having a full wallet, then increasing the number of days with money in the pocket may increase aggregate mortality. This is a subject for further research. The shift in some states to twice-monthly TANF payments may provide a source of variation to identify this effect.

Second, given the structure of the multiple cause of death data and the nature of our quasi-experiments, we have not been able to examine the variation in the size of the paycheck cycle response in response to payment size.

Third, it is not clear how much of the increased fatality is mortality displacement. In the case of the 2001 tax cuts, we found a large amount of displacement but in the case of the Alaska permanent fund payments, there was much less. This could be a function of the type of deaths in these two groups. The 2001 samples had many heart attacks so it is easy to see how this could generate harvesting: a person prone to a heart attack in a few days is probably prone to a heart attack today as well. In contrast, Alaska has a high number of external causes and it is less likely that an accident that occurs today would have occurred in the future. Identifying the amount of mortality displacement will clarify the impact of income receipt on life expectancy.

References

Albert, Christine M., Murray A. Mittleman, Claudia U. Chae, I-Min Lee, Charles H. Hennekens, and JoAnn E. Manson. 2000. "Triggers of Sudden Death from Cardiac Causes by Vigorous Exertion." *New England Journal of Medicine*, 343(19): 1355-61.

Baker Susan P., Marvin J. Ginsburg, Brian O'Neill, and Guohua Li. 1992. *The Injury Fact Book*, 2nd ed. New York, NY: Oxford University Press.

Bound, John. 1989. "The Health and Earnings of Rejected Disability Insurance Applicants." *American Economic Review*, 79(3): 482-503.

Browning, Martin, and M. Dolores Collado. 2001. "The Response of Expenditures to Anticipated Income Changes: Panel Data Estimates." *American Economic Review*, 91(3): 681–92.

Case, Anne. 2004. "Does Money Protect Health Status? Evidence from South African Pensions." In *Perspectives on the Economics of Aging*, ed. David Wise, Chicago: University of Chicago Press, 287-311

Collins, David J. and Helen M. Lapsley. 2002. "Counting the Cost: Estimates of the Social Costs of Drug Abuse in Australia in 1998-9." *National Drug Strategy Monograph Series No. 49*, Canberra: Commonwealth Department of Health and Ageing.

Currie, Janet and Enrico Moretti. 2003. "Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings." *Quarterly Journal of Economics*, 118(4): 1495-532.

Deaton, Angus. 2003. "Health, Inequality, and Economic Development." *Journal of Economic Literature*, 41(1): 113-58.

Dehejia, Rajeev, and Adriana Lleras-Muney. 2004. "Booms, Busts, and Babies' Health." *Quarterly Journal of Economics*, 119(3): 1091-130.

De Walque, Damien. 2007. "Does Education Affect Smoking Behaviors? Evidence Using the Vietnam Draft as an Instrument for College Education." *Journal of Health Economics*, 26(5): 877-95.

Dobkin, Carlos and Steven L. Puller. 2007. "The Effects of Government Transfers on Monthly Cycles in Drug Abuse, Hospitalization and Mortality." *Journal of Public Economics*, 91(11-12): 2137-57.

Elliott, William J. 2001. "Cyclic and Circadian Variations in Cardiovascular Events." *American Journal of Hypertension*, 14(9): S291-5.

Evans, William N., and John D. Graham. 1988. "Traffic Safety and the Business Cycle." *Alcohol, Drugs, and Driving*, 4(1): 31-8.

Evans, William N., and Timothy J. Moore. 2009. "Liquidity, Activity, Mortality." Working Paper, Department of Economics and Econometrics, University of Notre Dame.

Frijters, Paul, John P. Haisken-DeNew, and Michael A. Shields. 2005. "The Causal Effect of Income on Health: Evidence from German Reunification." *Journal of Health Economics*, 24(5): 997-1017.

Franklin, BA, Bonzheim K, Gordon S, Timmins GC, 1996. "Snow Shoveling: A trigger for acute myocardial infarction and sudden coronary death," American Journal of Cardiology, 77(10), 855-8.

Gerdtham, Ulf-G., and Magnus Johannesson. 2005. "Business Cycles and Mortality: Results from Swedish Microdata." *Social Science and Medicine*, 60(1): 205-18.

Halpern, Scott D., and C. Crawford Mechem. 2001. "Declining Rate of Substance Abuse Throughout the Month." *American Journal of Medicine*, 110(5): 347-51.

Grimard, Franque and Daniel Parent. 2007. "Education and Smoking: Were Vietnam War Draft Avoiders Also More Likely to Avoid Smoking?" *Journal of Health Economics*, 26(5): 896-926.

Harwood, Henrick, Douglas Fountain, and Gina Livermore. 1998. *The Economic Costs of Alcohol and Drug Abuse in the United States 1992 – Appendix A: Health Disorder Codes*, Rockville, MD: National Institutes on Drug Abuse.

Hausman, Jerry, Bronwyn H. Hall, and Zvi Griliches. 1984. "Econometric Models for Count Data with an Application to the Patents-R&D Relationship." *Econometrica*, 52(4): 909-38.

Haveman, Robert C., Barbara L. Wolfe, Lawrence Buron, and Steven C. Hill. 1995. "The Loss of Earnings Capability from Disability/Health Limitations: Towards a New Social Indicator." *Review of Income and Wealth*, 41(3): 289-308.

Heppell, R., S.K. Hawley, and K.S. Channer. 1991. "Snow Shoveller's Infarction." *British Medical Journal*, 302(6774): 469-70.

Hsieh, Chang-Tai. 2003. "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund." *American Economic Review*, 93(1): 397-405.

Johnson, David, Jonathan Parker, and Nicholas S. Souleles. 2006. "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review*, 96(5): 1589-610.

King, Miriam, Steven Ruggles, Trent Alexander, Donna Leicach, and Matthew Sobek. 2004. *Integrated Public Use Microdata Series, Current Population Survey: Version 2.0* [Machine-readable database]. Minneapolis, MN: Minnesota Population Center.

Kitigawa, Evelyn, and Philip Hauser. 1973. *Differential Mortality in the United States: A Study of Socioeconomic Epidemiology*. Cambridge, MA: Harvard University Press.

Kopczuk, Wojciech, and Joel Slemrod. 2003. "Dying to Save Taxes: Evidence from Estate-tax Returns on the Death Elasticity," *Review of Economics and Statistics*, 85(2), 256–265.

Li, Xin, Huiying Sun, David C. Marsh, and Aslam H. Anis. 2007. "Impact of Welfare Cheque Issue Days on a Service for Those Intoxicated in Public." *Harm Reduction Journal*, 12(4).

Lindahl, Mikael. 2005. "Estimating the Effect of Income on Health and Mortality Using Lottery Prizes as an Exogenous Source of Variation in Income." *Journal of Human Resources*, 40(1): 144-68.

Lipovetsky, Nestor, Hanoh Hod, Arie Roth, Yehezkiel Kishon, Shmuel Sclarovsky, and Mansfield Green. 2004. "Heavy Meals as a Trigger for a 1st Event of the Acute Coronary Syndrome: A Case-Crossover Study." *Israeli Medical Association Journal* 6: 728-31.

Lleres-Muney, Adriana. 2005. "The Relationship Between Education and Adult Mortality in the United States." *Review of Economic Studies*, 72(1): 189-221.

Marmot, Michael. 2002. "The Influence of Income on Health: Views of an Epidemiologist." *Health Affairs*, 21(2): 31-46.

Mastrobuoni, Giovanni, and Matthew Weinberg. 2009. "Heterogeneity in Intra-Monthly Consumption Patterns, Self-Control, and Savings at Retirement." *American Economic Journal: Economic Policy*, 1(2): 163-89.

Maynard, Charles and Gary B. Cox. 2000. "Association Between Week of the Month and Hospitalizations for Substance Abuse." *Psychiatric Services,* 51(1): 31.

Meer, Jonathan, Douglass L. Miller, and Harvey S. Rosen. 2003. "Exploring the Health-Wealth Nexus." *Journal of Health Economics,* 22(5): 713-30.

Meyer, Bruce D. and James X. Sullivan. 2009. "Five Decades of Consumption and Income Poverty." *NBER Working Paper No. 14827.*

Mittleman, Murray A., Malcolm Maclure, Geoffrey Tofler, Jane B. Sherwood, Robert J. Goldberg, and James E. Muller. 1993. "Triggering of Acute Myocardial Infarction by Heavy Physical Exertion – Protection Against Triggering by Regular Exertion." *New England Journal of Medicine*, 329(23): 1677-83.

Moller, J., A. Ahlbom, J. Hulting, F. Diderichsen, U. de Faire, C. Rueterwall, and J. Hallqvist. 2001. "Sexual Activity as a Trigger of Myocardial Infarction: A Case-Crossover Analysis in the Stockholm Heart Epidemiology Programme (SHEEP)." *Heart*, 86(4): 387-90.

Neumayer, Eric. 2004. "Recessions Lower (Some) Mortality Rates: Evidence from Germany." *Social Science and Medicine,* 58(6): 1037-47.

Parker, Jonathan A. 1999. "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes." *American Economic Review*, 89(4): 959-973.

Phillips, David P., Nicholas Christenfeld, and Natalie M. Ryan. 1999. "An Increase in the Number of Deaths in the United States in the First Week of the Month: An Association with Substance Abuse and Other Causes of Death." *New England Journal of Medicine*, 341(2): 93-98.

Phillips, David P., Jason R. Jarvinen, Ian S. Abramson, and Rosalie R. Phillips. 2004. "Cardiac Mortality is Higher Around Christmas and New Year's Than at Any Other Time – The Holidays as a Risk Factor for Death." *Circulation*, 110(25): 3781-8.

Riddell, Chris and Rosemarie Riddell. 2006. "Welfare Checks, Drug Consumption, and Health: Evidence from Vancouver Injection Drug Users." *Journal of Human Resources,* 41(1)**:** 138-61.

Rosenheck RA, Dausey DJ, Frisman L, Kasprow W, 2000. "Outcomes After initial Receipt of Social Security benefits Among Homeless Veterans with Mental Illness," *Psychiatric Services*, 51(12): 1549-54.

Ruhm, Christopher J. 2000. "Are Recessions Good for your Health?" *Quarterly Journal of Economics,* 115(2): 617-50.

Ruhm, Christopher J. 2003. "Good Times Make You Sick." *Journal of Health Economics* 22(4), 637-58.

Ruhm, Christopher J. 2005. "Healthy Living in Hard Times." *Journal of Health Economics*, 24(2), 341-63.

Segal, David R. and Mady Wechsler Segal. 2004. "America's Military Population." *Population Bulletin*, 59(4), Washington, D.C.: Population Reference Bureau.

Shapiro, Jesse. 2005. "Is there a daily discount rate? Evidence from the food stamp nutrition cycle." *Journal of Public Economics*, 89(2-3): 303-25.

Shapiro, Matthew D. and Joel B. Slemrod. 1995. "Consumer Response to the Timing of Income: Evidence from a Change in Tax Withholding." *American Economic Review,* 85(1): 274-83.

Shapiro, Matthew D. and Joel B. Slemrod. 2003. "Consumer Response to Tax Rebates," *American Economic Review*, 93(1): 381-96.

Shea, John. 1995. “Union Contracts and the Life Cycle/Permanent Income Hypothesis.” *American Economic Review*, 85(1): 186-200.
Single, Eric, Lynda Robson, Jurgen Rehm, and Xiaodi Xi. 1999. “Morbidity and Mortality Attributable to Alcohol, Tobacco, and Illicit Drug Use in Canada.” *American Journal of Public Health*, 89(3): 385-90.
Slemrod, Joel B., Charles W. Christian, Rebecca London, and Jonathan A. Parker. 1997. “April 15 Syndrome.” *Economic Inquiry*, 35(4): 695-709.
Slemrod, Joel B. and Wojciech Kopczuk. 2003. “Dying to Save Taxes: Evidence from Estate Tax Returns on the Death Elasticity,” *Review of Economics and Statistics*, 85(2): 256-65.
Smith, James P. 1999. “Healthy Bodies and Thick Wallets: The Dual Relation Between Health and Economic Status.” *Journal of Economic Perspectives*, 13(2): 145-166.
Snyder, Stephen E. and William N. Evans. 2006. “The Impact of Income on Mortality: Evidence from the Social Security Notch.” *Review of Economics and Statistics*, 88(3): 482-95.
Song, Jae and Joyce Manchester. 2007. “Have People Delayed Claiming Retirement Benefits? Responses to Changes in Social Security Rules.” *Social Security Bulletin*, 67(2): 1-23.
Souleles, Nicholas S. 1999. “The Response of Household Consumption to Income Tax Refunds.” *American Economic Review*, 89(4): 947-58.
Stephens Jr., Melvin. 2003. “’3rd of tha Month’: Do Social Security Recipients Smooth Consumption Between Checks?” *American Economic Review*, 93(1): 406-22.
Stephens Jr., Melvin. 2006. “Paycheck Receipt and the Timing of Consumption.” *The Economic Journal*, 116(513): 680-701.
Tapia Granados, Jose A. 2005. “Increasing Mortality During the Expansions of the US Economy, 1900-1996.” *International Journal of Epidemiology*, 34(6): 1194-202.
Verhuel, G., S.M. Singer, and J.M. Christenson. 1997. “Mortality and Morbidity Associated with the Distribution of Monthly Welfare Payments.” *Academy of Emergency Medicine*, 4(2): 91-2.
Wilkinson, Richard G. 1994. “Health, Redistribution and Growth.” In Andrew Glyn and David Miliband, eds. *Paying for Inequality: The Economic Cost of Social Injustice.* London: Rivers Oram Press, 24-43.
Willich S.N., Lowel H., Lewis M., Hormann A., Arntz H.R., Keil U., 2000. “Weekly Variation of Acute Myocardial Infarction. Increased Monday Risk in the Working Population.” *Circulation*, 90(1): 87-93.
Witte, Daniel R., Diederick E. Grobbe, Michiel L. Bots, and Arno W. Hoes. 2005. “A Meta-Analysis of Excess Cardiac Mortality on Monday.” *European Journal of Epidemiology*, 20(5): 401-6.
Wooldridge, Jeffrey M. 2003. “Cluster-Sample Methods in Econometrics.” *American Economic Review*, 93(2): 133-8.
Zeger, Scott L., Francesca Dominici, and Jonathan Samet. 1979. “Harvesting-Resistant Estimates of Air Pollution Effects on Mortality.” *Epidemiology*, 10(2): 171-5.

Figure 1: Time Series of De-trended and Standardized Residuals, Unemployment Rates, ln(Mortality Rates) and ln(Real per Capita Expenditures)

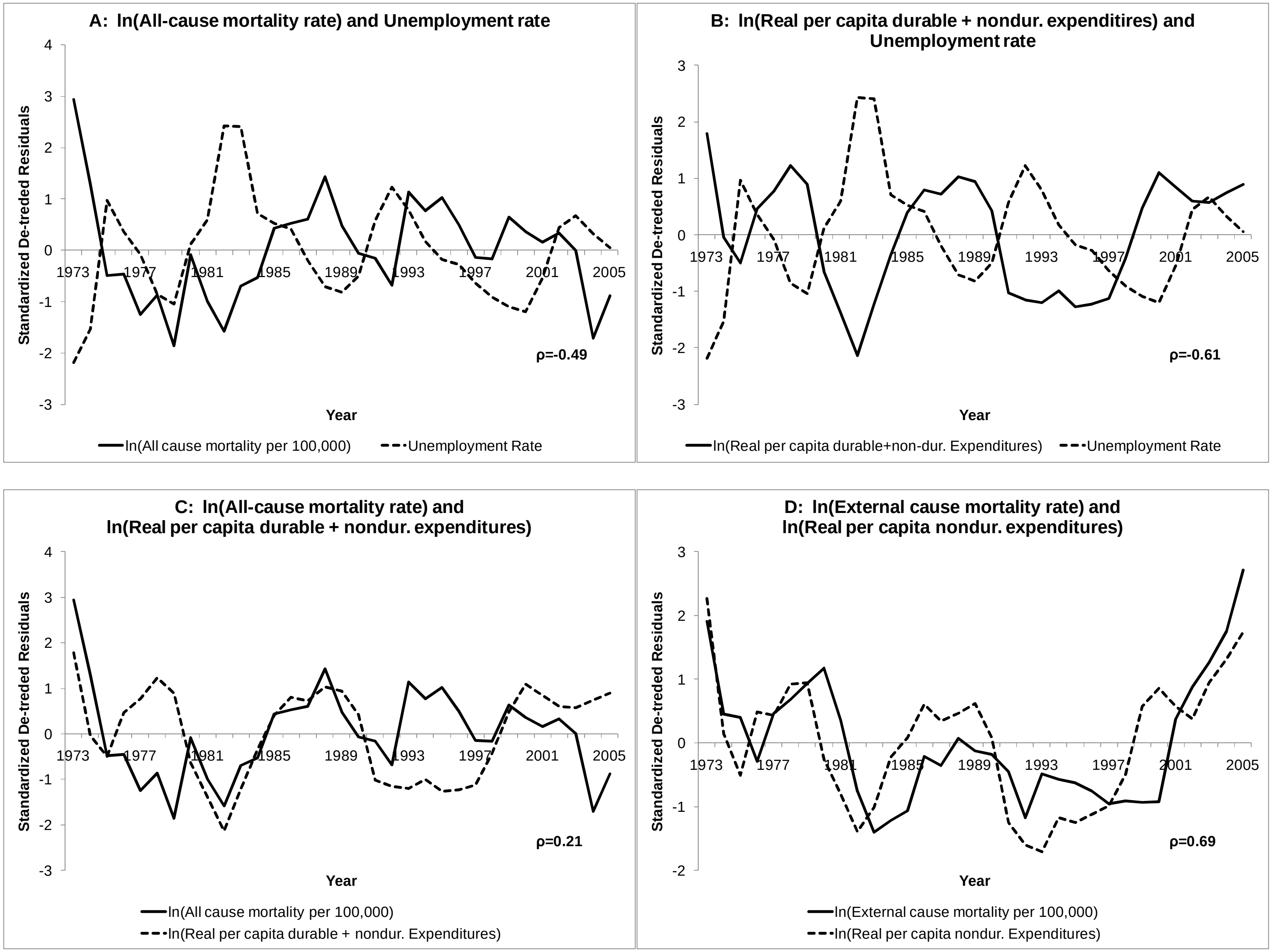

Figure 2a: Relative Daily Mortality Risk in Relation to the 1st of the Month, 1973-2005

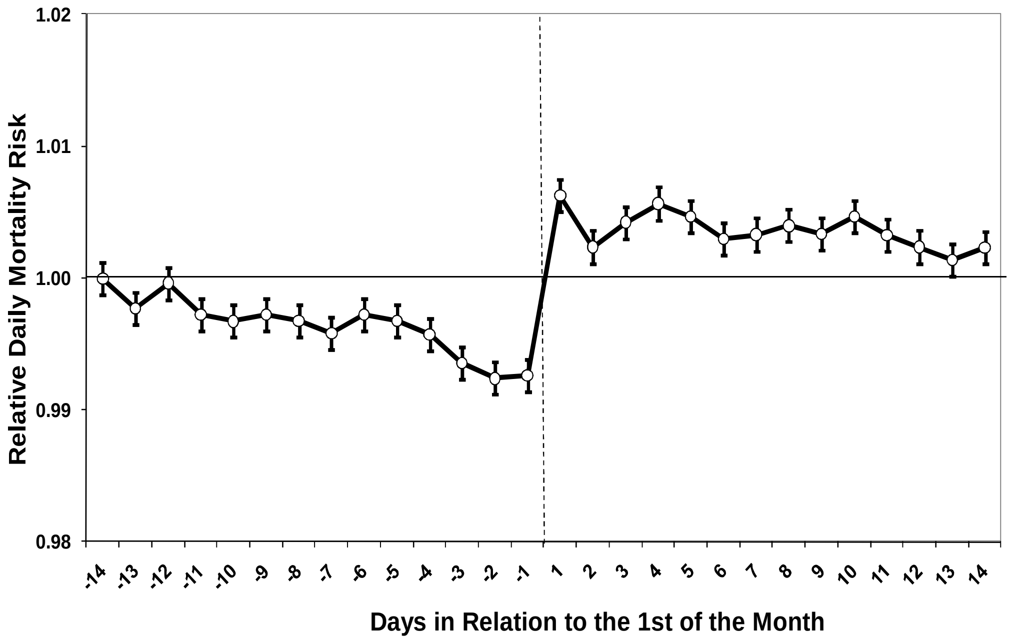

Figure 2b: Normalized Residuals in Relation to the 1st of the Month for ln(All-Cause Mortality) and ln(Ave. Daily Spending) 1986 and 1988-2005

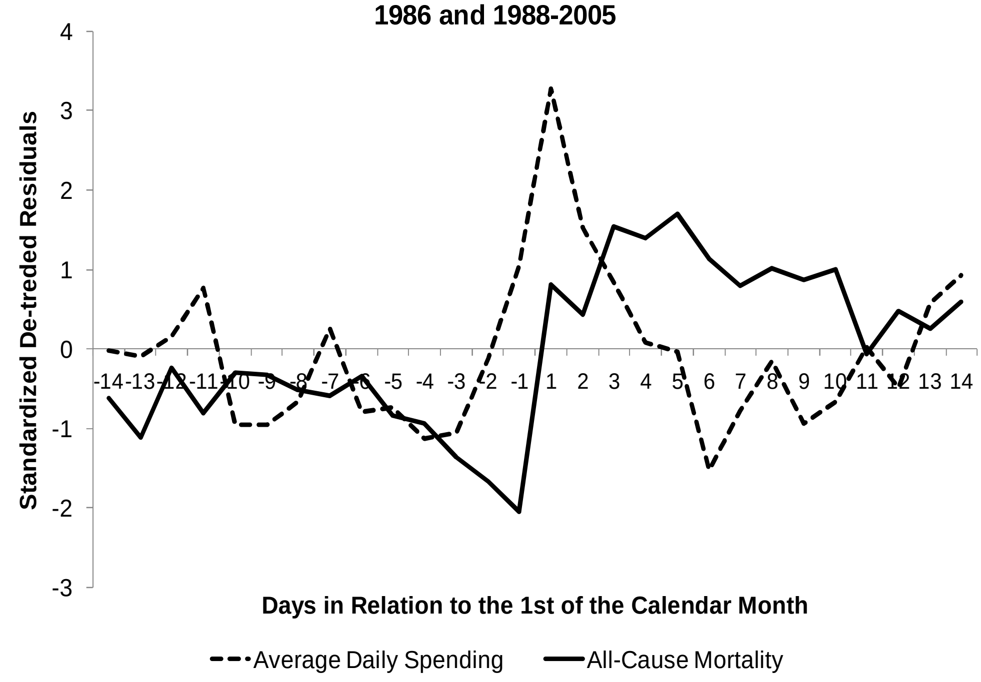

Figure 3: Period/Cohort Diagram

Cohort	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
1947	48	49	50	51	52	53	54	55	56	57	58	59
1946	49	50	51	52	53	54	55	56	57	58	59	60
1945	50	51	52	53	54	55	56	57	58	59	60	61
1944	51	52	53	54	55	56	57	58	59	60	61	62
1943	52	53	54	55	56	57	58	59	60	61	62	63
1942	53	54	55	56	57	58	59	60	61	62	63	64
1941	54	55	56	57	58	59	60	61	62	63	64	65
1940	55	56	57	58	59	60	61	62	63	64	65	66
1939	56	57	58	59	60	61	62	63	64	65	66	67
1938	57	58	59	60	61	62	63	64	65	66	67	68
1937	58	59	60	61	62	63	64	65	66	67	68	69
1936	59	60	61	62	63	64	65	66	67	68	69	70
1935	60	61	62	63	64	65	66	67	68	69	70	71
1934	61	62	63	64	65	66	67	68	69	70	71	72
1933	62	63	64	65	66	67	68	69	70	71	72	73
1932	63	64	65	66	67	68	69	70	71	72	73	74
1931	64	65	66	67	68	69	70	71	72	73	74	75
1930	65	66	67	68	69	70	71	72	73	74	75	76
1929	66	67	68	69	70	71	72	73	74	75	76	77

Year

All enrollees entered SS under post 1997 rules
Enrollees could have entered SS under either system
All enrollees entered SS under pre-1997 rules

Figure 4:
Daily Mortality Counts in Military and Non-Military Counties, Those Aged 17-64, 1973-1988 MCOD

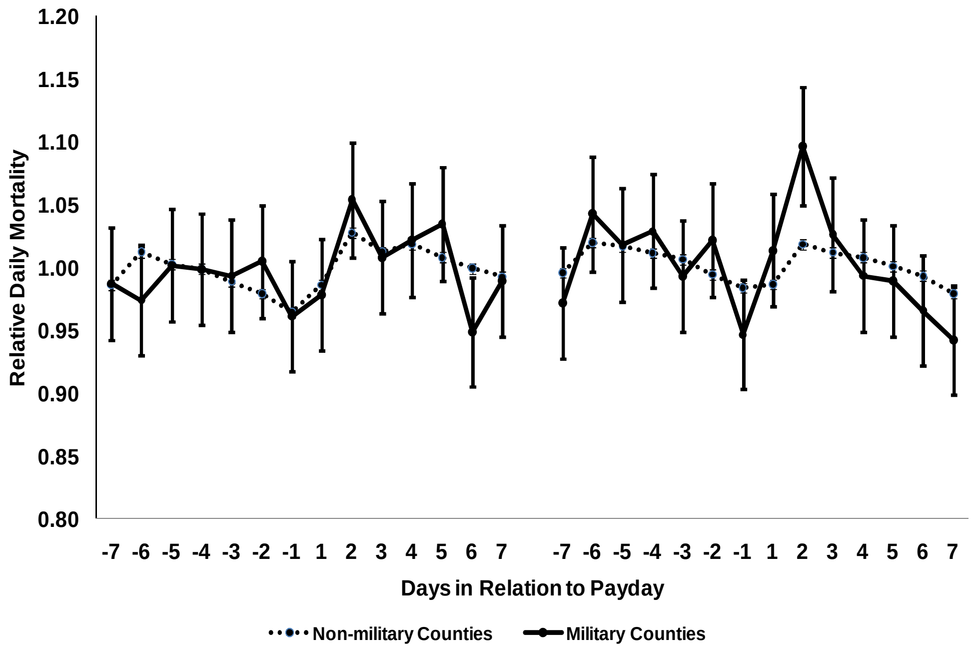

Table 1
Estimates of Log of Daily Mortality Counts Equation
In Relation to "3rd of the Month" Social Security Payment Schedule and the
1st of the Calendar Month

	Aged 65+ 1973-96 (1)	Aged 65-69 1973-96 (2)	Aged 65-69 2005-06 (3)	Aged 65-69 1995-96 (4)
Week(-2)	-0.0003	0.0017	0.0154	0.0028
	(0.0017)	(0.0023)	(0.0070)	(0.0068)
Week (1)	0.0027	0.0048	0.0155	0.0044
	(0.0014)	(0.0021)	(0.0085)	(0.0055)
Week (2)	0.0020	0.0053	0.0219	0.0134
	(0.0018)	(0.0026)	(0.0095)	(0.0103)
Week (3)	0.0005	0.0022	0.0262	0.0094
	(0.0021)	(0.0031)	(0.0093)	(0.0091)
Payweek(-2)	0.0041	0.0033	-0.0122	0.0105
	(0.0016)	(0.0022)	(0.0083)	(0.0078)
Payweek (1)	0.0046	0.0074	-0.0109	0.0207
	(0.0015)	(0.0023)	(0.0091)	(0.0071)
Payweek (2)	0.0051	0.0049	-0.0209	0.0041
	(0.0020)	(0.0029)	(0.0127)	(0.0092)
Payweek (3)	0.0050	0.0040	-0.0109	-0.0002
	(0.0029)	(0.0034)	(0.0115)	(0.0083)
R^2	0.921	0.570	0.577	0.664
Mean Daily Deaths	3,946	584	472	553
Observations	8,766	8,766	730	731

The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks, as they represent the days outside the 28-day periods centered, respectively, on the 1st of the calendar month and each day Social Security is paid. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, and a complete set of dummies for special days throughout the year described in footnote 12.

Table 2
Estimates of Log of Daily Mortality Counts Equation
In Relation to the Post-1997 Social Security Payment Schedule and the
1st of the Calendar Month

	Aged 65-69 All Decedents 2005-06 (1)	Aged 65-69 Singles 2005-06 (2)	Aged 65-69 All Decedents 1995-96 (3)	Aged 50-59 All Decedents 2005-2006 (4)
Week(-2)	0.0052	-0.0130	0.0077	-0.0058
	(0.0061)	(0.0219)	(0.0055)	(0.0058)
Week (1)	0.0138	0.0187	0.0201	0.0172
	(0.0061)	(0.0190)	(0.0047)	(0.0048)
Week (2)	0.0086	0.0241	0.0194	0.0081
	(0.0057)	(0.0180)	(0.0068)	(0.0058)
Week (3)	0.0149	0.0233	0.0088	-0.0097
	(0.0066)	(0.0286)	(0.0082)	(0.0057)
Payweek(-2)	0.0071	-0.0013	0.0010	-0.0056
	(0.0041)	(0.0231)	(0.0054)	(0.0042)
Payweek (1)	0.0111	0.0275	0.0001	-0.0033
	(0.0035)	(0.0176)	(0.0042)	(0.0028)
Payweek (2)	0.0023	0.0033	-0.0043	-0.0053
	(0.0057)	(0.0232)	(0.0050)	(0.0065)
Payweek (3)	-0.0188	-0.0605	-0.0147	-0.0029
	(0.0110)	(0.0296)	(0.0100)	(0.0060)
Born 1st to 10th	-0.0239	-0.0190	-0.0220	-0.0254
	(0.0058)	(0.0116)	(0.0056)	(0.0039)
Born 11th to 20th	-0.0308	-0.0480	-0.0356	-0.0271
	(0.0049)	(0.0148)	(0.0048)	(0.0031)
R^2	0.303	0.080	0.394	0.242
Mean Daily Deaths	157	12.0	185	215
Observations	2,190	2,190	2,193	2,190

The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks as they represent the days outside the 28-day periods centered, respectively, on the 1st of the calendar month and each day Social Security is paid. Decedents are divided into three groups: those born on the 1st to 10th, 11th to 20th, and 21st to 31st of the month. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, a complete set of dummies for special days throughout the year described in footnote 12, and dummies for observations for decedents born in the first two periods in the month.

Table 3
Estimates of Log of Daily Mortality Counts Equation
In Relation to "3rd of the Month" Social Security Payments and the 1st of the Calendar Month
By Involvement of Substance Abuse and Cause of Death, Aged 65 Years and Over

	All Deaths 1979-96 (1)	Substance Abuse 1979-96 (2)	Non-Substance Abuse 1979-96 (3)	External Causes 1973-96 (4)	Heart Cond. 1973-96 (5)	All Cancers 1973-96 (6)
Week(-2)	0.0001	0.0111	-0.0002	0.0077	-0.0020	0.0015
	(0.0018)	(0.0111)	(0.0019)	(0.0061)	(0.0024)	(0.0024)
Week (1)	0.0043	0.0190	0.0041	0.0257	0.0030	0.0006
	(0.0015)	(0.0111)	(0.0015)	(0.0059)	(0.0022)	(0.0023)
Week (2)	0.0034	0.0164	0.0033	0.0128	0.0002	0.0052
	(0.0018)	(0.0129)	(0.0018)	(0.0072)	(0.0026)	(0.0027)
Week (3)	0.0016	0.0068	0.0016	0.0041	-0.0017	0.0051
	(0.0023)	(0.0143)	(0.0023)	(0.0077)	(0.0031)	(0.0030)
Payweek(-2)	0.0039	0.0086	0.0039	0.0268	0.0042	0.0026
	(0.0018)	(0.0109)	(0.0018)	(0.0061)	(0.0023)	(0.0023)
Payweek (1)	0.0038	0.0367	0.0036	0.0410	0.0048	0.0009
	(0.0016)	(0.0112)	(0.0016)	(0.0057)	(0.0023)	(0.0022)
Payweek (2)	0.0045	0.0099	0.0044	0.0322	0.0063	0.0004
	(0.0022)	(0.0137)	(0.0022)	(0.0070)	(0.0028)	(0.0028)
Payweek (3)	0.0038	0.0119	0.0037	0.0275	0.0052	0.0044
	(0.0034)	(0.0131)	(0.0034)	(0.0074)	(0.0038)	(0.0030)
R^2	0.901	0.370	0.900	0.395	0.847	0.961
Mean Daily Deaths	4,124	36	4,088	89	1,008	802
Observations	6,575	6,575	6,575	8,766	8,766	8,766

The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks as they represent the days outside the 28-day periods centered, respectively, on the 1st of the calendar month and each day Social Security is paid. Decedents are divided into three groups: those born on the 1st to 10th, 11th to 20th, and 21st to 31st of the month. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, a complete set of dummies for special days throughout the year described in footnote 12, and dummies for observations for decedents born in the first two periods in the month.

Table 4
Maximum Likelihood Estimates of Daily Mortality Negative Binomial Equation
Counties With and Without a High Military Presence, Aged 17 to 64, 1973 to 1988

	Payday near the 1st of the Month			Payday near the 15th of the Month		
	Military Counties (1)	Non-Military Counties (2)	P-value on Test: Coefficients (1) = (2)	Military Counties (4)	Non-Military Counties (5)	P-value on Test: Coefficients (4) = (5)
Payday -7	0.0111	0.0092	0.950	0.0251	0.0057	0.562
	(0.0326)	(0.0038)		(0.0388)	(0.0031)	
Payday -6	-0.0275	0.0113	0.0113	0.0664	0.0036	0.055
	(0.0344)	(0.0037)		(0.0352)	(0.0035)	
Payday -5	0.0099	0.0100	0.854	0.0481	0.0060	0.202
	(0.0316)	(0.0037)		(0.0375)	(0.0035)	
Payday -4	0.0074	0.0080	0.999	0.0597	0.0027	0.083
	(0.0345)	(0.0034)		(0.0365)	(0.0037)	
Payday -3	0.0123	0.0067	0.862	0.0288	0.0041	0.458
	(0.0322)	(0.0036)		(0.0342)	(0.0037)	
Payday -2	0.0332	0.0067	0.419	0.0675	0.0000	0.040
	(0.0328)	(0.0036)		(0.0377)	(0.0035)	
Payday 1	0.0081	0.0141	0.854	0.0630	-0.0039	0.043
	(0.0315)	(0.0030)		(0.0367)	(0.0033)	
Payday 2	0.0467	0.0214	0.436	0.1178	0.0037	<0.001
	(0.0314)	(0.0038)		(0.0342)	(0.0035)	
Payday 3	0.0205	0.0243	0.906	0.0556	0.0029	0.110
	(0.0338)	(0.0038)		(0.0363)	(0.0035)	
Payday 4	0.0313	0.0240	0.823	0.0247	0.0012	0.478
	(0.0314)	(0.0037)		(0.0367)	(0.0035)	
Payday 5	0.0473	0.0241	0.477	0.0273	-0.0006	0.400
	(0.0334)	(0.0038)		(0.0367)	(0.0036)	
Payday 6	-0.0263	0.0233	0.137	0.0091	0.0001	0.786
	(0.0358)	(0.0037)		(0.0379)	(0.0036)	
Payday 7	0.0267	0.0274	0.999	-0.0008	-0.0048	0.906
	(0.0347)	(0.0035)		(0.0357)	(0.0036)	

There are 10,584 observations. Military counties had over 15 percent of 17 to 64 year old residents who were active military personnel in the 1970, 1980, and 1990 Censuses while non-military counties had less than one percent of the 17 to 64 year old residents in the military in 1970, 1980 and 1990. Average daily deaths in all military and in all non-military counties are 10.1 and 1235.7, respectively. Numbers in parentheses are standard errors that allow for an arbitrary correlation across observations within a synthetic month/year group based on military payments. Other covariates include a complete set of synthetic month and year effects, weekday effects, dummies for special days described in footnote 12, a dummy for observations from counties with a high military presence, an indicator for the first pay period, and an interaction between the military county and pay period indicators.

Table 5
When 2001 Tax Rebates Were Distributed

Last 2 digits of SS #	Checks distributed during the week of	Last 2 digits of SS #	Checks distributed during the week of
00-09	July 23	50-59	August 27
10-19	July 30	60-69	September 3
20-29	August 6	70-79	September 10
30-39	August 13	80-89	September 17
40-49	August 20	90-99	September 24

Table 6
Estimates of Log of Weekly Mortality Counts Equation
Aged 25 to 64 Years, 30-Week Period, Summer and Fall 2001

	All 30 Weeks of Data				Without Data After Week 17	
	All Deaths	All deaths	Substance abuse	Non-substance Abuse	All deaths	All deaths
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Rebate1	0.0269	0.0227	0.0057	0.0243	0.0241	0.0180
	(0.0097)	(0.0098)	(0.0387)	(0.0105)	(0.0111)	(0.0109)
Rebate2		-0.0157	-0.0135	-0.0161		-0.0360
		(0.0098)	(0.0392)	(0.0105)		(0.0119)
Rebate3		-0.0221	-0.0182	-0.0233		-0.0281
		(0.0098)	(0.0392)	(0.0105)		(0.0131)
Rebate4		-0.0085	-0.0678	-0.0029		0.0165
		(0.0098)	(0.0387)	(0.0105)		(0.0147)
P-value on Test, Group Effects =0	0.813	0.806	0.937	0.829	0.752	0.581
R^2	0.715	0.723	0.157	0.724	0.183	0.256
Mean Weekly Deaths per Group	1,014	1,014	85	929	993	993
Observations	300	300	300	300	170	170

Standard errors are in parenthesis. The other covariates in the model are week fixed effects and Social Security number group fixed effects.

Table 7
Timing and Size of Alaska Permanent Fund Dividend Payments

Year	Pop. of Alaska	% Pop. Receiving Payment	Amount of Payment	% Paid by Direct Deposit	Date/Day of Direct Deposit	Date/Day 1st Batch of Checks Issued	% Checks Issued in 1st Batch
2000	627,533	93%	$1,963.86	64%	10/4,W	10/5,Th	92.2%
2001	632,241	93%	$1,850.28	66%	10/10,W	10/17,W	93.6%
2002	640,544	92%	$1,540.76	70%	10/9,W	10/16,W	93.3%
2003	647,747	92%	$1,107.56	72%	10/8,W	10/15,W	93.5%
2004	656,834	91%	$919.84	72%	10/12,Tu	10/19,Tu	92.1%
2005	663,253	90%	$845.76	73%	10/12,W	10/21,F	90.9%
2006	670,053	88%	$1,106.96	76%	10/4,W & 10/19,Th	11/14,Tu	97.8%

Source: Annual Reports of the Alaska Permanent Fund Dividend Division, 2000 to 2008

Table 8
Estimates of Log of Weekly Mortality Counts Equation
Alaskans Compared to Residents in the Rest of USA, 2000 to 2006

Independent Variable	All Deaths (1)	(2)	Urban Areas (3)	(4)	Urban Areas, Without Substance Abuse (5)	(6)
Alaska*Dividend(1)	0.0671	0.0608	0.1220	0.1273	0.1206	0.1304
	(0.0534)	(0.0545)	(0.0722)	(0.0732)	(0.0789)	(0.0803)
Alaska*Dividend(2)		-0.0264		0.0250		0.0445
		(0.0545)		(0.0732)		(0.0803)
Alaska*Dividend(3)		-0.0949		-0.0843		-0.0589
		(0.0545)		(0.0732)		(0.0803)
Alaska*Dividend(4)		0.0212		0.0810		0.0921
		(0.0545)		(0.0732)		(0.0803)
R^2	0.9996	0.9996	0.9994	0.9994	0.9993	0.9994
Mean Weekly Deaths in Alaska	59.8	59.8	32.6	32.6	30.0	30.0

Standard errors are in parenthesis. There are 168 observations in each regression. The average deaths per week in the rest of the United States is 45,866. The average number of non-substance abuse deaths per week in the rest of the United States is 44,606. The other covariates in the model are fixed week-year effects and a dummy variable for weekly mortality counts in Alaska.