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NBER Working Paper No. 15311
September 2009
JEL No. D91,H31,H55,I10,I12,I38

<u>ABSTRACT</u>

Many studies find that households increase their consumption after the receipt of expected income
payments, a result inconsistent with the life-cycle/permanent income hypothesis. Consumption can
increase adverse health events, such as traffic accidents, heart attacks and strokes. In this paper, we
examine the short-term mortality consequences of 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.

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The life cycle-permanent income hypothesis (LC/PIH) is widely used in modern macroeconomic theory to model how households allocate consumption across time. 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 to test this prediction. Among the income changes that have been exploited in this context are increases in union wages (Shea, 1995); a change in federal tax withholding (Shapiro and Slemrod, 1995); changes in Social Security tax payments (Parker, 1999); income tax refunds (Souleles, 1999); the arrival of Social Security payments (Stephens, 2003); the receipt of tax stimulus checks (Johnson, Parker and Souleles, 2006); the arrival of paychecks (Stephens, 2006); and Alaska Permanent Fund dividends (Hsieh, 2003). All but one of these studies (Hsieh, 2003) find consumption behavior displays "excess sensitivity" to expected changes in income, a result inconsistent with the LC/PIH.

In this paper, we consider a related but largely unexplored question: if income receipt increases consumption, does it affect mortality? While the potential relationship between consumption and mortality is 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, many triggers for heart attacks and strokes are activity-related, and hence if an income payment increases economic activity, one may expect a higher incidence of heart attacks to follow.[1] Likewise, Ruhm (2000) shows that mortality is pro-cyclical, suggesting a deadly aspect to increased economic activity.

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 for three cases already considered within the LC/PIH literature, as well as two new tests. 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) 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.

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[1] Triggers for heart attacks include getting out of bed (Elliott, 2001), returning to work on Mondays (Witte et al., 2005), shoveling snow (Heppell et al. 1991), the Christmas season (Phillips et al., 2004) and physical exertion (Albert et al. 2000). Similar triggers have been observed for strokes.

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 2nd, 3rd or 4th 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.

Our work helps illuminate and broaden three disparate literatures. The first is the literature on the LC/PIH. Most tests of this hypothesis rely on consumption data such as that found in the Consumer Expenditures Survey (CEX). While these datasets do a good job of measuring recurring monthly expenditures such as housing and car payments, they do less well in measuring goods that are the focus of LC/PIH tests, like alcohol and food away from home (Meyer and Sullivan, 2009). In contrast, mortality is exceptionally well-measured, even at the daily level, and our dataset includes all deaths in the United States. If mortality is viewed as an ex post measure of market activity, our results provide further evidence of widespread increases in economic activity after predictable changes in income.

Second, our analysis is similar in structure and content to a related group of papers found in the medical literature that argues there is an increase in substance abuse-related mortality following payments to welfare recipients. Sometimes called the 'full wallets' hypothesis, work by Verhuel, Singer and Christenson (1997), Maynard and Cox (2000), Riddell and Riddell (2006), Li et al. (2007), and especially Dobkin and Puller (2007) shows convincingly that problems associated with substance abuse increase after federal transfer program payments. Our work demonstrates that the effect of income receipt on mortality is not limited to recipients of federal transfer programs and to deaths involving substance abuse.

Finally, our work also has important implications for the large literature on income and health (Kitigawa and Hauser, 1973; Deaton, 2003). While this research has established robust correlations, 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,[2] suggesting that the direction of causation may run from health to income. Given this possibility of reverse causation and the lack of an obvious causal pathway from income to health, Deaton (2003, p. 118) notes, "…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[3] and income.[4] While the results exploiting exogenous variation in schooling have consistently found that education improves health, there are conflicting results among studies using variation in 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 phenomena 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.[5] A number of scholars who have attempted to empirically measure the link between socioeconomic status and health have expressed similar sentiments.[6] Our results suggest that the negative short-run consequences of these transfers must be considered in any such evaluation.

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[2] For example, see Bound, 1989, Haveman et al., 1995, and especially Smith, 1999.

[3] For example, authors have examined whether health outcomes are altered by increases in education generated by policies such as compulsory schooling (Lleras-Muney, 2003), an increase in access to colleges (Currie and Moretti, 2003) and the Vietnam Draft (de Walque, 2007; Grimand and Parent, 2007).

[4] 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).

[5] http://www.archive.official-documents.co.uk/document/doh/ih/ih.htm

[6] 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

In the next section, we examine how regular payments to Social Security beneficiaries and military personnel affect short-term mortality. In both cases we find large increases in mortality immediately after the receipt of income, and that these increases are not just among deaths involving substance abuse.

Given the recurring nature of these two events, we are unable to examine whether increases in mortality merely reflect short-term mortality displacement: mortality has been hastened for people who would have died soon anyway. In section 2, we examine this issue by considering the one-time receipt of 2001 tax stimulus checks and the annual receipt of Alaska Permanent Fund dividends. We find in both cases that a short-term increase in mortality is offset by a subsequent decrease in deaths, suggesting that much of the immediate effect we estimate is actually short-term mortality displacement. In section 3, we discuss the implications of our work for both the LC/PIH and the income/health literature.

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

Existing research on the 'full wallets' hypothesis focuses on mortality among welfare recipients who are likely to have drug and alcohol problems. In this section, we consider broader populations by examining the mortality consequences of Social Security payments and military wage payments. We also separate substance abuse from other causes of death.

1a.   Monthly Social Security Payments

Before May 1997, all Social Security recipients received checks on the 3rd of each month or on the previous work day when the 3rd fell on a weekend or on Labor Day. Stephens (2003) used the structure of payments and data from the CEX to test the LC/PIH and finds Social Security recipients did not smooth consumption over the month, but instead spent more in the week after the receipt of checks compared to the week before its arrival.

In the same way, we investigate whether Social Security recipients' mortality increases immediately after they are paid. We initially restrict our attention to before 1997, when all beneficiaries were on the "3rd of the month" schedule. Later in this section, we examine more recent periods using the new pay schedule. Both tests require data on each

narrowing the gap in relative standards is now much more important to the quality of life in the developed world than further economic growth."

decedent's age and exact date of death. We constructed such a data set using various versions of the MCOD data file.[7] 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 versions of the MCOD files containing exact dates of death from 1989 through 2006 at their Research Data Center, and we pooled these data with the 1973 to 1988 public-use files.[8]

For the "3rd of the month" analysis, we constructed a data file of all deaths from 1973 to 1996 among those aged 65 or over.[9] The Social Security Administration reports that benefits were paid to 32.7 million adults aged 65 and older in 2000,[10] which is 93.5 percent of the population in this age group in the 2000 Census.

Similar to Stephens (2003), we look at changes in mortality beginning 14 days prior to the day of Social Security payment. As checks not paid on the 3rd are almost always paid on Fridays,[11] day-of-the-week effects may obscure patterns in raw counts. We can uncover these patterns, however, by regressing the natural log of daily mortality counts on weekday, month and year dummy variables, and averaging the residuals for the 14 days prior and the 14 days after checks arrive. Figure 1 shows the results from this exercise. Movements in mortality among seniors are similar to the consumption patterns in Figures 1a-1d of Stephens (2003), in which he shows that spending in the seven days after checks arrive is higher than spending in the preceding seven days. There is a large drop in mortality three days before checks arrive. In most months, the checks were distributed on the 3rd, so *Day(-3)* is usually the last day of the previous calendar month. Phillips, Christenfeld, and Ryan (1999) and

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[7] Information about the MCOD is at http://www.cdc.gov/nchs/products/elec_prods/subject/mortmcd.htm, and information about the NDI is at http://www.cdc.gov/nchs/r&d/ndi/what_is_ndi.htm.
[8] http://www.cdc.gov/nchs/r&d/rdc.htm.
[9] 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.
[10] Social Security Administration Office of Research, Evaluation and Statistics, *Annual Statistical Supplement of the Social Security Bulletin, 2001*, Washington, DC: SSA, December 2001.
[11] The lone exception is that when January 3rd is a Sunday, checks are distributed on Thursday, December 31.

Evans and Moore (2009) document a within-month mortality cycle where deaths decline before the 1st of the month and rise on the 1st, which suggests that this mortality spike is partly driven by a within-month cycle.[12] Separate to this, however, is an increase in mortality of half a percent from the day before checks arrive to the day checks arrive.

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.[13] With this spacing, there will be an uneven number of days in our synthetic months, since the length of the month depends on the day on which the 3rd falls and the number of days in the month. These synthetic months can be anywhere from 28 to 34 days in length.[14] 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.

To isolate the mortality impact of receiving a Social Security check from other factors, we estimate an econometric model that controls for the day-of-the-week and other effects. 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 extends from -14 to 20.[15]

Given this structure for the data, the econometric model we estimate is of the form:

$$(1) \quad \ln(Y_{dmy}) = \alpha + \sum_{\substack{w=-2 \\ w \neq -1}}^{3} Week(w)_{dmy} \delta_w + \sum_{\substack{w=-2 \\ w \neq -1}}^{3} Payweek(w)_{dmy} \beta_w + \sum_{j=1}^{6} Weekday(j)_{dmy} \gamma_j$$

$$+ \sum_{j=1}^{M} Special(j)_{dmy} \varphi_j + \mu_m + \nu_y + \varepsilon_{dmy}$$

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[12] Possibly due to the occurrence of payments from transfer programs like Supplemental Security Income and food stamps, and many bills falling due on or near the 1st of the month.
[13] 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.
[14] 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.
[15] 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.

where *Payweek(w)* is defined as above, *Weekday(j)* is one of six dummy variables for the different days of the week, *Special(j)* is one of *J* dummy variables that capture special days throughout the year such as New Year's Day and Christmas,[16] and *Weeks(w)* are weekly dummy variables created in reference to the 1st of the calendar month. Therefore, *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. The variables μ_m and v_y capture synthetic month and year effects[17] and ε_{dmy} is an idiosyncratic error term. In this equation, the reference period for the *Payweek* dummies is *PayWeek(-1)* and for *Week* dummies is *Week(-1)*, while the reference weekday is Saturday. We estimate standard errors allowing for arbitrary correlation within the days of the synthetic month.

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. The results demonstrate 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 similar magnitude. Deaths are about five-tenths of a percent higher in the seven days after check receipt compared to the preceding seven days.[18]

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

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[16] We include unique dummies for a list of reoccurring special days: January 1st and 2nd, the Friday through Monday associated with 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.
[17] We have estimated all models with synthetic month-year effects, μ_m, instead of separate synthetic month and year effects. Results with this alterative specification are virtually identical to results from the more parsimonious specification.
[18] 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.

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.

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.[19] 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 2. 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. Song and Manchester (2007) find that nearly 100 percent of the 1937 cohort enrolled by age 65, so 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 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.

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

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)*.

Next, we consider whether people receiving Social Security checks under the new (post-May 1997) system display a spike in mortality after they are paid. Based on Figure 2, we again using data for 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 1st to the 10th of the month (paid on the second Wednesday of the month); birth dates from the 11th to the 20th (paid on the third Wednesday); and from the 21st to the 31st (fourth Wednesday). For this sample, we allow the dependent variable to vary across days, months, years and groups (k), and estimate an equation of the form:

$$(2) \quad \ln(Y_{kdmy}) = \alpha + \sum_{\substack{w=-2 \\ w \neq -1}}^{3} Week(w)_{kdmy}\delta_w + \sum_{\substack{w=-2 \\ w \neq -1}}^{3} Payweek(w)_{kdmy}\beta_w + \sum_{j=1}^{6} Weekday(j)_{kdmy}\gamma_j$$

$$+ \sum_{j=1}^{M} Special(j)_{kdmy}\varphi_j + \lambda_k + \mu_m + \nu_y + \varepsilon_{dmy}$$

The variables *Week(w), Special(j), Weekday, μ, ν,* and *ε* are defined as before. In this model, we add effects for the birthday-based groups, and the variable *Payweek(w)* is 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 the synthetic months. 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 – some people in each group are not being treated on the same schedule. Nevertheless, people who never married should be claiming benefits under their own birth date, so we report results for never-married seniors aged 65 to 69 in the 2005 and 2006 MCOD files in column (2) of Table 2. 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 substance abuse related deaths. 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 is 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 is the same list used by Phillips et al. (1999), and the causes used by studies on the economic costs of substance abuse in the United States (Harwood, Fountain, and Livermore, 1998), Australia

(Collins and Lapsley, 2002), and Canada (Single et al., 1999).[20] 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 only 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 excess substance-abuse related deaths each year compared to 1,236 excess 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.[21] 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 pay week effect (coefficient and standard error on *Payweek(1)* is 0.0410 (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 a placebo test, because cancer deaths are far less affected by activity levels 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.

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[20] A complete list of these codes is provided in an appendix that is available from the authors.
[21] 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).

1b.   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.[22] In this section, we examine whether mortality spikes on or immediately after these dates. Parker (1999), Stephens (2006), and Browning and Collado (2001) use the receipt of earnings to test the LC/PIH, with the first two studies finding consumption was excessively sensitive to income receipt. In this section, we compare mortality patterns in counties with and without a high proportion of their population on active military duty.

Military personnel are predominantly male (currently 85 percent), young (approximately one half are under 25 years of age) and healthy (Segal and Segal, 2004). 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.[23] 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 the public-use MCOD files contain exact dates of death during this time, we focus on that time period in this section.

We generate a military working-age sample of 17 to 64 year olds.[24] Using information from the 1970, 1980, and 1990 Census Summary File 3 data sets,[25] we identified counties with more than 15 percent of their population aged 17 to 64 who were military personnel in all three Censuses.[26] There are 21 counties that meet this criterion[27] and in 1990 and there were roughly 326,000 people aged 17 to 64 in these "military" counties, of which about one quarter were in the military. Military personnel have a large number of dependents

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[22] 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.
[23] Authors' calculations from various issues of the *Statistical Abstract of the United States*.
[24] Enlistment in the military can occur at age 17 years with parental consent, and at age 18 years without.
[25] These data are taken from the National Historical Geographic Information System.
[26] 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.
[27] 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).

and bases typically employ many civilians paid on the same schedule,[28] so the proportion of the population who would have been affected by the military payment schedule in these areas will be 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 that have less than one percent military among adults aged 17 to 64 in the same 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.[29]

In Figure 3, we use data from the 1973-1988 MCOD to construct the relative daily mortality risk 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 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

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[28] 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.
[29] 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.

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 on 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 we use a negative binomial model that allows for integer values and estimate it by maximum likelihood (Hausman, Hall and Griliches, 1984), because daily mortality counts in the military counties are small and occasionally zero. 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.[30] 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 seven days before the first payment each month.[31] 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.[32]

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

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[30] 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.

[31] 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.

[32] 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).

$$(3) \quad X_{idmy}\beta = \beta_0 + \sum_{j=1}^{6} Weekday(j)_{dmy}\gamma_j + \sum_{j=1}^{M} Special(j)_{dmy}\varphi_j +$$

$$\sum_{\substack{d=-7 \\ d\neq-1}}^{7} Military_{idmy}Period1_{idmy}Payday_d\beta_{1md} + \sum_{\substack{d=-7 \\ d\neq-1}}^{7} Military_{idmy}Period2_{idmy}Payday_d\beta_{2md} +$$

$$\sum_{\substack{d=-7 \\ d\neq-1}}^{7} Nonmilitary_{idmy}Period1_{idmy}Payday_d\beta_{1nd} + + \sum_{\substack{d=-7 \\ d\neq-1}}^{7} Nonmilitary_{idmy}Period2_{idmy}Payday_d\beta_{2nd} +$$

$$Period1_{idmy}\beta_p + Military_{idmy}\beta_m + (Period1_{idmy})(Military_{idmy})\beta_m + \mu_m + v_y$$

where *Weekday, Special,* and the fixed 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 for military and nonmilitary counties around the first pay period of the month (β_{1md} and β_{1nd}, respectively) then 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)*.

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. 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 second payday near the 15th of the month. Standard errors allow for arbitrary correlation across observations within the same 28-day synthetic month.

The results in Table 4 correspond with the visual evidence in Figure 3. 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 difference between the two sets of counties is not statistically significant.

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

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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.[33]

As in the previous section, we identify deaths related and unrelated to substance abuse using the same ICD-9 codes. 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 t-ratios 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 non-substance abuse deaths, the accuracy of each estimate has decreased and the test identifying differences across groups are imprecise.[34]

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[33] 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 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].
[34] 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.

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

In the previous section, we demonstrated that mortality increases immediately after income receipt. The periodic nature of Social Security and military payments did not allow us to determine whether the increases merely 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). In this section, we use two new events to examine how much of the short-term mortality increase represents displacement.

2a. The 2001 Tax Rebates

The *Economic Growth and Tax Relief Reconciliation Act*[35] 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 rebate based on their 2000 tax returns 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.[36] The first checks were sent on Monday, July 23, to taxpayers whose second-to-last SSN digit was a zero.[37] Table 6 shows the exact distribution dates of checks by SSN. The Treasury Department sent letters to taxpayers a few weeks before checks arrived informing 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 LC/PIH, as the second-to-last digit of the SSN is effectively randomly assigned.[38] Johnson, Parker and Souleles

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[35] http://www.gpo.gov/fdsys/pkg/PLAW-107publ16/pdf/PLAW-107publ16.pdf.

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

[37] Households who filed their year 2000 tax return late may have been sent their rebates after the ten-week period shown in Table 7. 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 7.

[38] Geographic areas determine the first three digits of Social Security Number, a group determines the middle two digits, and the last four digits are assigned sequentially, so are effectively random. The second-to-last digit

(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. Using data on SSNs of credit card holders, Agarwal, Chiu and Souleles (2007) found that households initially used rebate money to reduce credit card debt, but soon afterward increased their credit card spending by amounts comparable to the initial payments. In contrast to these results, Shapiro and Slemrod (2003) found a minority of households planned to spend their rebate.

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)[39] 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 the spring through fall of 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 ν_i are fixed week effects, η_j 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.[40] 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

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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).

[39] 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.

[40] http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm.

effects will capture these cyclic changes in mortality so long as the deaths associated with 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 groups such as seniors.[41] 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. Since 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 consider models where we reduce the sample to unmarried taxpayers, a group where we should better 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.

While we anticipate there is some autocorrelation in mortality rates, Monte Carlo estimates suggest that Huber/White-type procedures allowing for arbitrary correlation in errors perform poorly when the number of groups is small (Wooldridge, 2003). While we could employ an AR(1), the residuals from column (1) of Table 6 regressed on a one-period lag (deleting the first observation in each group) generate an estimate of the AR(1)

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[41] 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.

coefficient (standard error) of 0.0085 (0.0584), which suggests that autocorrelation is not a problem in this relatively young group of decedents.

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), we get an estimated change (standard error) in mortality of -0.0151 (0.0194). In this instance, 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.[42] 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, 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.[43] 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 the coefficient (standard error) on *REBATE1* of 0.0280 (0.0134).

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[42] 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).
[43] 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.

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.[44] Importantly, we find no impact of the rebates on single-cause cancer deaths[45] (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.

2b.   Dividend Payments from the Alaska Permanent Fund

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.[46] Since 1982, an annual dividend has been paid to Alaskans from the average 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.[47] 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

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[44] 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).

[45] The cancer category was created using the same underlying cause of death recode used in Section 2. There was an increase in all cancer deaths in the week checks arrived, but once this category was limited to deaths where cancer was the only cause then this effect disappeared.

[46] From the 2008 Annual Report of the Alaska Permanent Fund Corporation. Available at: http://www.apfc.org/home/Content/reportspublications/reportArchive.cfm.

[47] Residency requirements have been the same since 1990. Minor changes occurred in earlier years. Historical information is at: https://www.pfd.state.ak.us/historical/index.aspx

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).

The recent years of the Permanent Fund payments provide an opportunity to explore the short-term relationship between income payments and mortality. 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 have been 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.[48]

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.[49]

Residuals from a simple regression using these data show an increase in Alaskan deaths at the point of direct deposit payments. In Figure 4 is the result of creating weekly counts of deaths in each state of residence for annual ten-week periods that begin fifteen days after Labor Day, regressing these counts against week-year dummies and dummies for each state in a negative binomial model of the type used in section 1, and then calculating the mean residuals in terms of when Permanent Fund payments arrive. Deaths in Alaska deviate

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[48] 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).

[49] 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, in both the Alaskan and non-Alaskan groups we remove deaths with an Underlying Cause-of-Death 358 Recode of 400 (Water transport accidents) or 401 (Air and space transport accidents).

most from trends in other states in a positive direction in the week dividends are paid, and in a negative direction two weeks later. This is consistent with an increase in deaths when income is paid which largely displaces mortality which would have occurred soon after.

The econometric model here is a simple difference-in-difference specification, with treatments occurring at particular times of the year in Alaska. The data for the rest of the U.S. provides 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,[50] with the first period each year beginning fifteen days after Labor Day, the first Monday in September.[51] 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:

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

where v_{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 results for the 2001 tax rebate. In column (1), we see a large immediate 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

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[50] All direct deposits during 2000 to 2006 were made on Tuesdays, Wednesdays or Thursdays.
[51] 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.

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.[52] 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 with more than 10,000 residents.[53] 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 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 does not appear to be entirely 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 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).[54] 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, which is again statistically insignificant.

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[52] Data from the 2000 Census indicates 16.5 percent live in areas with fewer than 1,000 people or in no defined place.
[53] 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.
[54] There are too few substance abuse-related deaths in Alaska to estimate the impact of dividend payments.

3.   Discussion

Many authors have demonstrated that consumption increases after individuals receive an expected infusion of cash. In this paper, we returned to three tests of the LC/PIH and developed two others to document the mortality consequences of this excess sensitivity. We find that mortality increases after the receipt of income for a wide variety of payments: transfer payments, paychecks, one-time cash bonuses, and annual residency-based dividends.

Changing levels of consumption/activity is the most plausible mechanism through which income receipt affects mortality. The findings for particular causes of death are consistent with this, for both when we observe a relationship – like we do for heart attacks and traffic accidents – and when we do not, as with the tests using cancer deaths.

Two 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 month after death,[55] a deceased applicant's Permanent Fund dividends go to their estate, military wages are already earned and the tax rebates were based on tax returns from the previous year.

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 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. At this point it is hard to judge the value of shifting to smaller, more frequent income payments.

It is also difficult to assess the role of income levels and liquidity constraints, as our tests use partially-treated populations. What is striking, however, is that similar results are found across a wide range of demographic groups, with our tests covering seniors, working-age taxpayers, predominantly younger military personnel, and all of the residents in one state.

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

The percentage changes may seem small: mortality for 65 to 69 year olds increases by 1.1 percent the week after Social Security checks arrived in 2005 and 2006, while mortality increased by 2.7 percent for those aged 18 to 64 the week the 2001 stimulus checks arrived. Relative to general movements in mortality, however, these results are substantial.

Consider a simple analysis for 65 to 69 year olds in 2005 and 2006. There are 471 deaths per day among this group, so paycheck receipt increases mortality by 36.3 deaths per week. In 2005 there were 5,532,900 people aged 65 to 69, so the death rate increased by 7.86E-5 (36.3/5,532,900) the week after paycheck receipt. 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.[56] 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 indictors 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, then these results suggest that in order to produce an decrease in the mortality rate by 7.86E-5, incomes in this group would have to increase by 2.65 percent, which is roughly equal to the annual cost-of-living adjustments to Social Security payments over the past decade.

Estimates for annual payments in Alaska produce a similar story. In 2000, there was an average of 52.4 deaths per week among Alaskans aged over 18 years. A 12 percent increase in mortality in one week would result in an extra 6.3 deaths for this group. In the 2001 American Community Survey, reported median household income was $67,090, and the average household had 2.77 members. Each applicant received a $1,964 dividend in 2000, which would have increased average family income by 9.7 percent. Using similar data from the NHIS/MCOD, the coefficient (standard error) on the log of family income in a one-year mortality regression is -0.00077 (0.00021). Assuming again that this is represents the causal impact of income on one-year mortality, a 9.7 percent increase in income would mortality rates by 7.45E-5. Multiplying this by the 418,815 adults in Alaska in 2000, this

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[56] 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).

increase in income is estimated to reduce deaths by 31.2. Therefore, in this best case scenario of the impact of income on mortality, the short-term increase in mortality of 6.3 deaths eliminates 20 percent of the estimated benefits from Permanent Fund income.

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 distinguish the time period of analysis because the short-term consequences may be very different from the long-term consequences. 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 income rises over the business cycle, then the short-term mortality effects of income receipt may provide just such an explanation.

There are potential policy consequences flowing from these results. First, there is evidence of worse hospital patient outcomes when there are fewer medical professionals per patient (Kostis et al., 2007). 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 the benefits of such a policy regime shift are far from certain. There is little evidence to date that cash transfers increase health. In contrast, the results in this paper show that, in the short run, there is a pronounced negative consequence to cash infusions for a wide variety of groups.

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Figure 1:
Residuals From ln(Daily Mortality Count) Model,
Controlling for Weekdays, Months and Years,
Decedents Aged 65 and Over, 1973 to 1996 MCOD Data

Days in Relation to Social Security Payment
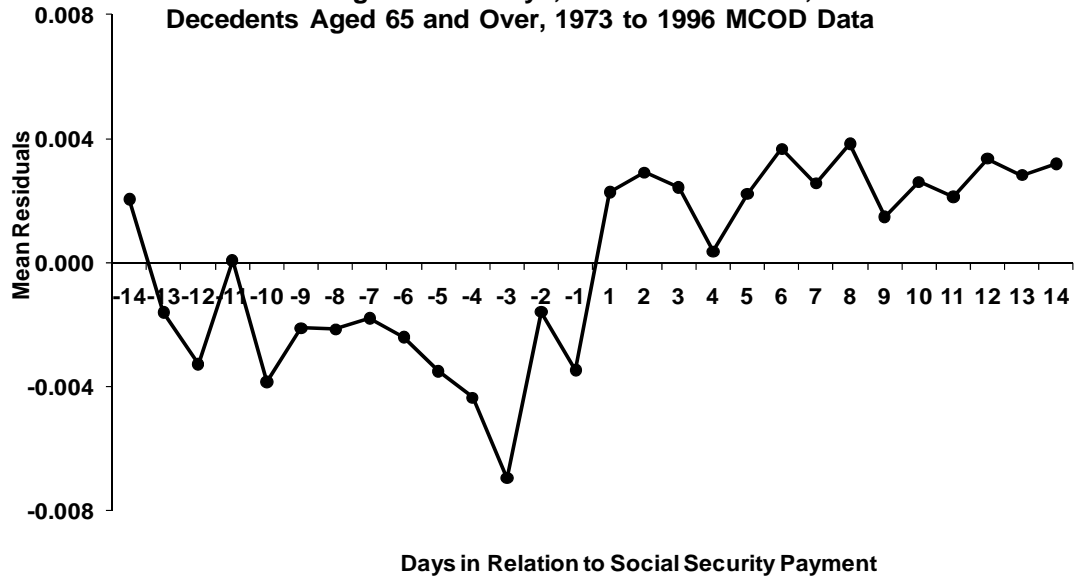
Figure 2: Period/Cohort Diagram

						Year						
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

All enrollees entered SS under post-1997 rules

Enrollees could have entered SS under either pre or post 1997 Rules

All enrollees entered SS under pre-1997 rules

Figure 3:
Daily Mortality Counts in Military and Non-Military Counties,
Those Aged 17-64, 1973-1988 MCOD
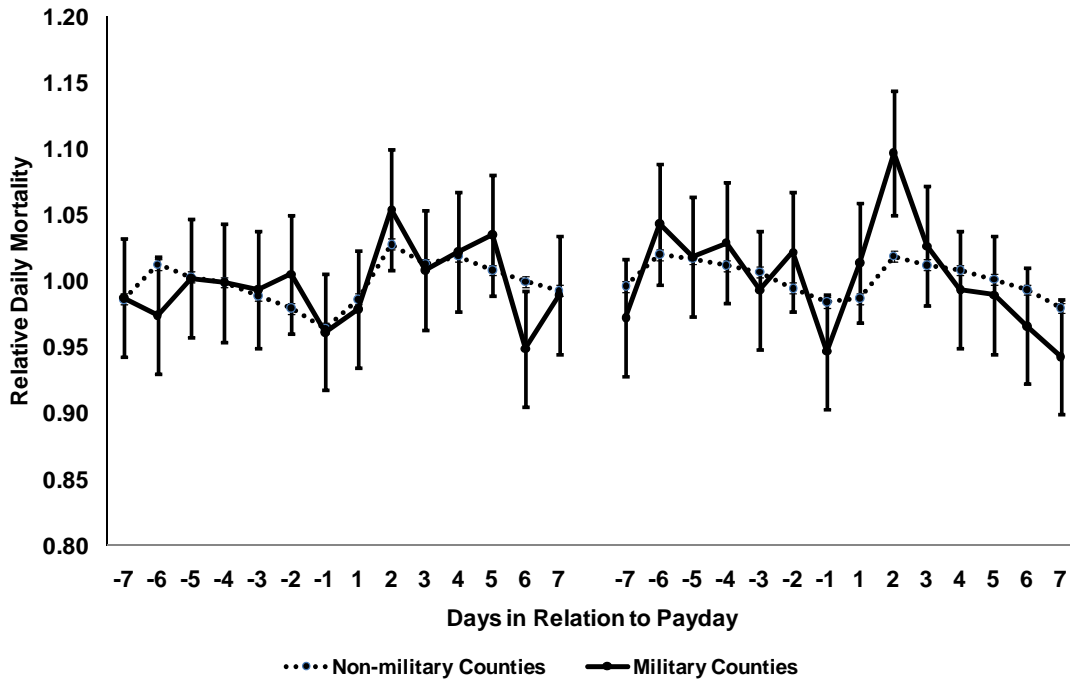
Figure 4:
Average Residuals from ln(Weekly Mortality Counts) Model,
Controlling for Week and Year Effects
Alaska and the Rest of the US, 2000-2006 MCOD

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 first 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 16.
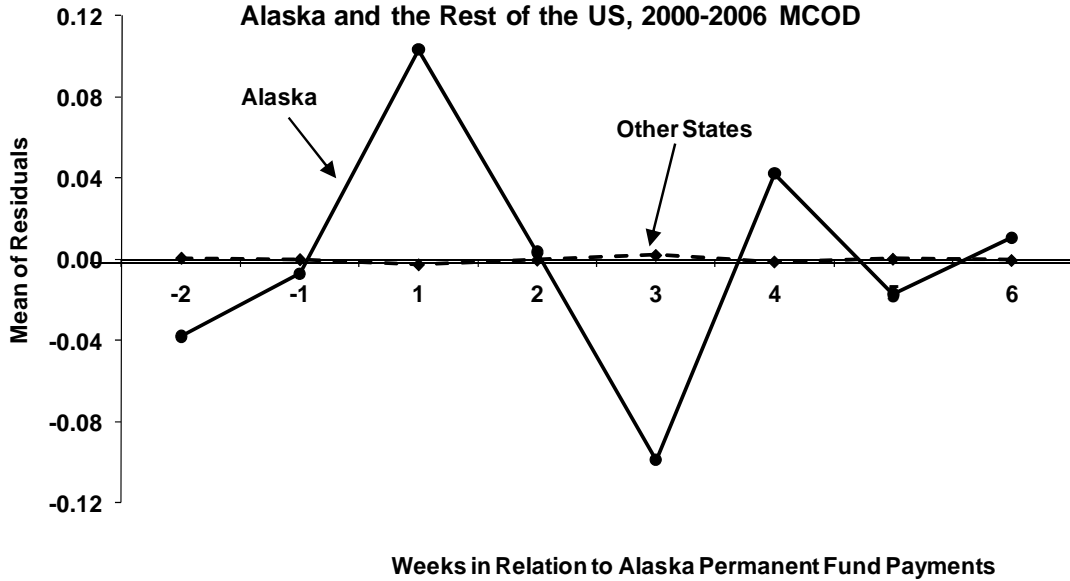
Table 2
Estimates of Log of Daily Mortality Counts Equation
In Relation to the Post-1997 Social Security Payment Schedule and the
1st of the Calendar Month

	Aged 65-69 All Decedents 2005-06	Aged 65-69 Singles 2005-06	Aged 65-69 All Decedents 1995-96	Aged 50-59 All Decedents 2005-2006
	(1)	(2)	(3)	(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 first of the month and days 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 16, 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 first of the month and days 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 16, 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	Non-Military Counties	P-value on Test: Coefficients	Military Counties	Non-Military Counties	P-value on Test: Coefficients
	(1)	(2)	(1) = (2)	(4)	(5)	(5) = (6)
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 military and 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 16, 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. Other covariates in the model include 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		Urban Areas		Urban Areas, Without Substance Abuse	
	(1)	(2)	(3)	(4)	(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 non-substance abuse deaths per week in the rest of the United States is 44,606. Other covariates in the model include fixed week-year effects and a dummy for Alaska.