



# The short-term mortality consequences of income receipt

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## ABSTRACT

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

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## 1. Introduction

A large literature spanning many disciplines has established that individuals from higher income groups tend to have lower mortality and morbidity rates, and better health habits (Kitigawa and Hauser, 1973; Backlund et al., 1999). Although there is some question as to whether these observed correlations represent a causal relationship (Smith, 1999; Deaton, 2003), the evidence is at least suggestive that higher income is protective of health.

In contrast to this work, there are some persistent patterns in mortality data that run counter to the standard income/health gradient. Two examples are the within-month mortality cycle and the pro-cyclic nature of mortality. Mortality steadily declines as the end of the calendar month approaches, then increases by almost one percent on the first day of the month, and remains above the daily average in the first few days of the month (Phillips et al., 1999; Evans and Moore, forthcoming). A large fraction of the population receives cash infusions at the beginning of the month, either from transfer programs or employment, and there is evidence that these payments

increase economic activity and raise mortality rates. Similarly, mortality tends to move negatively with the business cycle, increasing during booms and declining during recessions (Ruhm, 2000). Interestingly, the death categories that have the greatest peak-to-trough within the month are the same categories that are the most responsive to changes in the business cycle (Evans and Moore, forthcoming).

Both the within-month mortality cycle and the pro-cyclic nature of mortality indicate the possibility of a short-term increase in mortality following income receipt. Such a relationship has been investigated among recipients of transfer payments, whose morbidity and mortality increase following income payments as a result of elevated levels of substance abuse (e.g., Dobkin and Puller, 2007).<sup>1</sup> The within-month mortality cycle and the pro-cyclicality of mortality is present for many demographic groups and causes of death, however, which suggests that this phenomenon may be more general than previously considered.

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<sup>1</sup> Papers by Verhuel et al. (1997), Rosenheck et al., 2000; Maynard and Cox (2000), Halpern and Mechem (2001), Riddell and Riddell (2006), and Li et al. (2007) have also found such a relationship.

In this paper, we use various versions of the Multiple Cause of Death (MCO) data, a census of all deaths in the United States, to examine the short-run mortality consequences of income receipt. Taking our cue from research that tests predictions about the life-cycle/permanent income hypothesis (LC/PI) using known dates of income receipt, we examine three cases of income receipt from that literature, as well as two new tests.<sup>2</sup> We examine the mortality consequences of (1) the receipt of Social Security payments around the 3rd of each month (2) changes in the Social Security payment schedule to one based on beneficiaries' dates of birth, (3) the receipt of military wages on the 1st and 15th days of each month, (4) the 2001 federal tax rebates, and (5) the annual Alaska Permanent Fund dividend payments.

In all cases, we find that mortality increases after the receipt of income. Seniors who enrolled in Social Security prior to May 1997 typically received their Social Security checks on the 3rd of the month. For this group, daily mortality is half a percentage point higher the week after checks arrive compared to the week before. For those who enrolled in Social Security after April 1997, benefits are paid on either the second, third or fourth Wednesday of the month, depending on beneficiaries' birth dates. Among this group, daily mortality is one percent higher the week after checks arrive compared to the previous week. In counties with a large military presence, daily mortality among 17–29 year olds increases by around 10% the week after mid-month paychecks arrive, while over the same period there is little change in mortality in counties with a small military presence. During the week the 2001 tax rebate checks arrived, mortality among 25–64 year olds increased by 2.5%. During the week that direct deposits of Permanent Fund dividends are made, mortality among urban Alaskans increases by 13%.

Previous work suggests consumers tend to reduce spending before income receipt and increase purchases immediately afterwards. Stephens (2003) found that seniors increase their consumption of time-sensitive purchases, like perishable food and eating at restaurants, after the receipt of Social Security checks. Stephens (2006) found a similar increase in consumption after the receipt of paychecks in the United Kingdom. This bunching effect is particularly pronounced for those on federal income transfer programs and those with lower incomes. Among Food Stamp recipients, Shapiro (2005) found a drop in daily caloric consumption of 10–15% from when food stamps are paid to just before they are next due. Likewise, Mastrobuoni and Weinberg (2009) found food consumption declined between Social Security payments for seniors with a high fraction of income coming from Social Security.

Results from existing medical literature suggest that short-term health risks may be heightened by increases in consumption or activity. While the link is most obvious in cases like traffic fatalities – since increased travel increases the likelihood of an accident – other causes of death also have well-documented links to consumption and economic activity. For example, many triggers for heart attacks are activity-related.<sup>3</sup> If income payments increase economic activity, one may expect a higher incidence of heart attacks to occur after the receipt of income. This is consistent with the cause-of-death patterns

we find in the Social Security analysis. We also find larger mortality responses to income payments among younger groups, which may reflect their having more variable levels of consumption and activity (and a higher fraction of deaths resulting from external cause injuries and acute health problems).

Our work broadens the literature on the short-term relationship between income and mortality that has been largely limited to a single group (those receiving transfer payments) and a narrow group of causes of death (substance abuse).<sup>4</sup> It also provides a possible explanation for the patterns in mortality within the month and across the business cycle, and may explain why it is difficult to estimate the long-term relationship between income and health.

The welfare and policy implications of these short-term increases in mortality are uncertain. They depend on how much of the increase in deaths immediately following payments is mortality displacement, and whether alternative disbursement schemes would lessen the change in mortality. On the first issue, increases in aggregate mortality in the first week following the payment of 2001 tax rebates and the Alaska Permanent Fund dividends are offset by declines in mortality in subsequent weeks.<sup>5</sup> In some of the subgroups, however, an initial increase in mortality is not offset by subsequent declines. Age and cause of death are probably important for understanding this issue. We suspect external cause deaths and deaths among younger people are unlikely to be displacement, but our estimates are not precise enough to make any definitive claims on this point.

The second issue depends on how the size of the mortality effect varies with payment size and frequency. It is not clear from our results that greater pay frequency would decrease the size of the mortality response, as evidenced by large mortality effects from the second military wage payment each month. We do not have enough variation in payment size within particular groups to know whether this variable affects short-term mortality.

The results in this paper complement our work on the within-month mortality cycle (Evans and Moore, forthcoming). In that paper, the within-month mortality cycle is documented for many causes of death, including external causes, heart disease, heart attack, and stroke, but not cancer. The within-month cycle is also evident for both sexes and for all age groups, races, marital status groups, and education groups. Similar within-month cycles are shown to present in a number of different activities and purchases, including going to the mall, visiting retail establishments, purchasing lottery tickets, going to the movies, and the amounts spent on retail purchases. Suggestive evidence that the rises in mortality and activity are linked to changing liquidity over the month comes from the peak-to-trough in mortality and consumption being largest for people expected to have the greatest liquidity issues, such as those with low levels of education and income, and those on federal transfer programs. In this current paper, we try to establish a definitive causal link between income payments and mortality in the short-run, which is not done in the other paper.

In Section 2, we demonstrate that mortality is higher immediately after the receipt of Social Security checks and military paydays. To examine whether mortality also increases following less regular income payments, in Section 3 we consider the mortality effects of the

<sup>2</sup> The LC/PI hypothesis is the standard model for inter-temporal choice in modern macroeconomics. A key implication of the model is that predictable and certain changes in income should have no effect on consumption once they occur. Over the past 15 years, authors have used high-frequency survey data on consumption and exact dates of income receipt to test this prediction. Three of our tests have been used in this way: Stephens (2003) examined the receipt of Social Security checks in the pre-1997 period; Johnson et al. (2006) examined the 2001 tax rebates; and Hsieh (2003) considered consumption after the receipt of Alaska Permanent Fund dividend payments.

<sup>3</sup> The activities that increase the short-term risk of a heart attack include exercise (Mittleman et al., 1993; Albert et al., 2000), sexual activity (Moller et al., 2001), eating a heavy meal (Lipovetsky et al., 2004), the busy Christmas holiday season (Phillips et al., 2004), returning to work on Mondays (Willich et al., 1994; Witte et al., 2005), and shoveling snow (Heppell et al., 1991; Franklin et al., 1996).

<sup>4</sup> Dobkin and Puller (2007), using administrative records from California, find elevated drug-related hospital admissions and within-hospital mortality in the first few days of the month for recipients on federal disability insurance programs paid on the first of the month. They do not find such a similar pattern for people not enrolled in transfer programs. It is likely that we find a broader income-mortality relationship because we exploit exact dates for the arrival of non-transfer income payments, and our sample includes non-hospital mortality. For all age groups, a minority of deaths occur in hospital. Data from the 1986 MCO indicate that the fractions of deaths occurring in hospitals by age group are: 24% (ages 19–39), 37% (ages 40–54), 42% (ages 55–64), 43% (ages 65–74) and 37% (ages 75 and over).

<sup>5</sup> Recurring payments like the Social Security and military wage payments do not shed light on this issue.

one-time receipt of 2001 tax stimulus checks and the annual receipt of Alaska Permanent Fund dividends. The populations in these examples broaden the phenomenon beyond the elderly and military personnel. In both cases, there is a short-term increase in mortality that is partially offset by a subsequent decrease in deaths, suggesting that some of the effect reflects short-term mortality displacement. In Section 4, we discuss the implications of our work.

## 2. The short-term mortality consequences of regular income payments

### 2.1. Monthly Social Security payments

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

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

The mortality data we use are various versions of the National Center for Health Statistics' (NCHS) Multiple Cause of Death (MCO) data file.<sup>6</sup> The MCO contains a unique record for each death in the United States. Records have information about a decedent's age, gender, race, place of residence, place of death, and cause of death. Exact date of death is reported on public-use files from 1973 to 1988, but is removed from later public-use files. We obtained permission from the NCHS to use restricted-use MCO files containing exact dates of death from 1989 to 2006 at their Research Data Center.

We used the information on decedents' age and exact date of death in the 1973 to 1996 MCO files to construct daily counts of decedents aged 65 and over, a group consisting almost entirely of Social Security recipients.<sup>7</sup> The Social Security Administration reports that benefits were paid to 32.7 million adults aged 65 and older in 2000,<sup>8</sup> which is 93.5% of the population in this age group in the 2000 Census.

The basic relationship between mortality and social security payments can be seen in the residuals plotted in Fig. 1, which come from a regression of the natural log of daily mortality counts on weekday, month and year effects, plus dummies for special days (e.g., Christmas, Thanksgiving, etc.). The solid line is a plot of the averaged residuals over the 14 days prior and the 14 days after checks arrive. From five days before checks arrive, the average daily residuals steadily decrease and mortality is 0.8% below the daily average the day before checks arrive. Mortality increases sharply on the day checks arrive, and then the average residuals are generally positive in the days following paycheck receipt. This pattern is very similar to the pattern of results in Fig. 1a–d in Stephens (2003).

Evans and Moore (forthcoming) highlight how the concentration of economic activity and other income payments at the start of the month affect mortality. It is important to take that into account here,

<sup>6</sup> Information about the MCO is at [http://www.cdc.gov/nchs/products/elect\\_prods/subject/mortmcd.htm](http://www.cdc.gov/nchs/products/elect_prods/subject/mortmcd.htm).

<sup>7</sup> 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.

<sup>8</sup> Social Security Administration Office of Research, Evaluation and Statistics, *Annual Statistical Supplement of the Social Security Bulletin, 2001*, Washington, DC: SSA, December 2001.

as Social Security is only one source of income for seniors.<sup>9</sup> To get some idea of whether there is a separate within-month cycle in mortality among those 65 and older, the residuals from the regression described in the previous paragraph are also arranged in relation to the 1st of the calendar month and plotted as the dashed line in Fig. 1. There is a reduction in mortality leading into the 1st of the month, and then an increase in the first couple of days of the calendar month.

To further analyze the relationship between Social Security payments and daily mortality, we follow Stephens (2003) and construct ‘synthetic’ months that begin 14 days prior to the day of Social Security payment and last until 14 days before the next payment.<sup>10</sup> Synthetic months are anywhere from 28 to 34 days in length, as they depend on the day when the checks are distributed and the number of days in the month. We divide each month into five periods: *Payweek*(−2) is the seven days from 14 days before payday to 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 payday; and *Payweek*(3) is the extraneous days before the next synthetic month starts.

We control for the within-month mortality cycle by creating weekly dummy variables in reference to the 1st of the calendar month, where *Week*(−2) equals one if the day is eight to 14 days before the start of the calendar month; *Week*(−1) equals one if the day is one to seven days before the start of the month; *Week*(1) and *Week*(2) equal one for the 1st to 7th and 8th to 14th days in the calendar month, respectively; and *Week*(5) is all the extra days before the 14th day prior to the start of the next calendar month. As checks not paid on the 3rd are almost always paid on Fridays,<sup>11</sup> we also need to control for day-of-the-week effects.

To isolate the short-term mortality impact of receiving a Social Security check from other factors, we estimate the following econometric model. Let  $Y_{dmy}$  be counts of deaths for day  $d$  in synthetic month  $m$  and synthetic year  $y$ .<sup>12</sup> Days are organized in relation to Social Security payments, so  $d = -1$  is the day before payday,  $d = 1$  is payday, and so on;  $d$  ranges from −14 to 20. The econometric model is of the form:

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

where *Payweek*( $w$ ) and *Week*( $w$ ) are the dummy variables defined as above, *Weekday*( $j$ ) is one of six dummy variables for the different days of the week, and *Special*( $j$ ) is one of  $j$  dummy variables that capture special days throughout the year.<sup>13</sup> The variables  $\mu_m$  and  $\nu_y$  capture

<sup>9</sup> For families with someone age 65 years and over, 32 percent of income comes from Social Security. Authors' calculations based on data from the 1974–1997 March Current Population Survey.

<sup>10</sup> 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 through February 20th, and so on.

<sup>11</sup> The lone exception is that when January 3rd is a Sunday, checks are distributed on Thursday, December 31.

<sup>12</sup> Synthetic years follow a similar 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 go through to December 16th of the following year.

<sup>13</sup> We include unique dummies for a long list of reoccurring special days, including for January 1st and 2nd, the Friday through Monday associated with the all federal holidays occurring on Mondays (Presidents' Day, Martin Luther King Jr. Day since 1986, Memorial Day, Labor Day, Columbus Day), Super Bowl Sunday and the Monday afterwards, Holy Thursday through Easter Sunday, July 4th, Veteran's Day, the Monday through Sunday of Thanksgiving, a dummy for all days from the day after Thanksgiving through New Year's Eve, plus single day dummies for December 24th through December 31st.

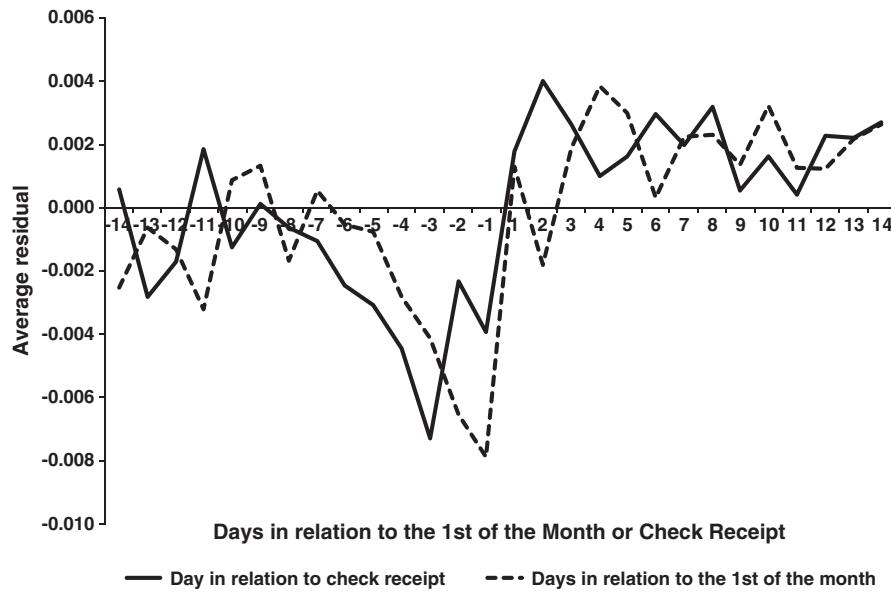


Fig. 1. Mean residuals from log daily mortality counts regression, by “3rd of the Month” Social Security payment schedule and the 1st of the calendar month, 1973–1996, Ages 65+.

synthetic month and year effects and  $\varepsilon_{dmy}$  is an idiosyncratic error term. In this equation, the reference period for the *Payweek* dummies is *Payweek(-1)* and the reference period for *Week* dummies is *Week(-1)*. The reference weekday is Saturday. We estimate standard errors allowing for arbitrary correlation within each unique synthetic month, e.g., we allow for correlation in errors for month 1 of 1995, month 2 of 1995, etc.

The results for Eq. (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 which show that deaths are about one half of a percent higher in the seven days after check receipt compared to the preceding seven days.<sup>14</sup> Relative to the week before payment, deaths are one half a percent higher two weeks before payment (*Payweek(-2)*) and two weeks after payment (*Payweek(2)*).<sup>15</sup> The results suggest a fall in mortality in the last few days before seniors are paid; the increase when they are paid is a return to ‘normal’ mortality. This suggests seniors decrease their level of activity as they run of out money, rather than ‘splurge’ when they get paid. It is consistent with the consumption behavior among seniors reported in Stephens (2003), and the food intake patterns found in Mastrobuoni and Weinberg (2009).

In the next four rows, we present results for the calendar weeks in relation to the 1st of the month. There is a within-month mortality cycle, with deaths declining the week before the 1st and then rising afterwards. Daily death rates are about three-tenths of a percent higher in the first week of the month compared to the previous seven days, with a p-value for the test that the null hypothesis is zero of less than 0.05.

In columns (2) to (4) of Table 1, we consider results for age-based subgroups because Evans and Moore (forthcoming) document that the within-month mortality cycle is less pronounced for older groups. Similar mortality patterns are present across the 65–74 years, 75–84 years and 85 years and over groups. The *Payweek* coefficients are generally smaller for the group aged 85 years and over than the other

two groups, although the differences between coefficients are not statistically significant.

In column (5), we consider a set of decedents who should NOT be impacted by the “3rd of the month” schedule, which allows us to see whether our results are driven by some other effect at the 3rd of the

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+	Age-based subgroups, 1973–1996			Aged 65–69	Aged 65–69
	1973–96	Aged 65–74	Aged 75–84	Aged 85+	2005–06	1995–96
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Payweek(-2)</i>	0.0041 (0.0016)	0.0036 (0.0017)	0.0049 (0.0019)	0.0039 (0.0025)	-0.0122 (0.0083)	0.0105 (0.0078)
<i>Payweek(1)</i>	0.0046 (0.0015)	0.0063 (0.0017)	0.0050 (0.0018)	0.0022 (0.0021)	-0.0109 (0.0091)	0.0207 (0.0071)
<i>Payweek(2)</i>	0.0051 (0.0020)	0.0056 (0.0021)	0.0057 (0.0024)	0.0042 (0.0029)	-0.0209 (0.0127)	0.0041 (0.0092)
<i>Payweek(3)</i>	0.0050 (0.0029)	0.0050 (0.0027)	0.0064 (0.0032)	0.0037 (0.0043)	-0.0109 (0.0115)	-0.0002 (0.0083)
<i>Week(-2)</i>	-0.0003 (0.0017)	0.0008 (0.0018)	-0.0006 (0.0020)	-0.0011 (0.0027)	0.0154 (0.0070)	0.0028 (0.0068)
<i>Week(1)</i>	0.0027 (0.0014)	0.0045 (0.0016)	0.0015 (0.0018)	0.0020 (0.0020)	0.0155 (0.0085)	0.0044 (0.0055)
<i>Week(2)</i>	0.0020 (0.0018)	0.0027 (0.0020)	0.0013 (0.0023)	0.0018 (0.0026)	0.0219 (0.0095)	0.0134 (0.0103)
<i>Week(3)</i>	0.0005 (0.0021)	0.0011 (0.0022)	-0.0006 (0.0025)	0.0012 (0.0033)	0.0262 (0.0093)	0.0094 (0.0091)
R <sup>2</sup>	0.921	0.731	0.890	0.947	0.577	0.664
Mean daily deaths	3946	1288	1538	1122	472	553
Observations	8766	8766	8766	8766	730	731

The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks, as they represent the days outside the 28-day periods centered, respectively, on the 1st of the calendar month and each day Social Security is paid. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, and a complete set of dummies for special days throughout the year described in footnote 13.

<sup>14</sup> 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.

<sup>15</sup> It is difficult to interpret the *Week(3)* and *Payweek(3)* coefficients in any regressions, because the length of these dummy variables varies across months and creates strong seasonal components that are not necessarily controlled for with other covariates.

month. 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.<sup>16</sup> As a falsification test, we estimate the “3rd of the month” model on decedents who should be enrolled via the new payment schedule. The sample we construct for this test uses deaths among 65 to 69 year olds as recorded in the MCODE files for 2005 and 2006, the most recent year data is available. The only cohorts that we can be sure enrolled in Social Security after the change-over in rules are beneficiaries who turned 62 after May of 1997. As before, we restrict our attention to people over 65 because nearly all beneficiaries claim Social Security by age 65. Someone 62 years of age in 1998 is 69 years old in 2005, and therefore anyone aged 65 to 69 years in 2005 and 2006 who was receiving Social Security benefits would have been enrolled on the new schedule.<sup>17</sup>

In column (5) of Table 1 we show the results for this group. The coefficient on *Payweek(1)* is negative and statistically insignificant. The lack of precision for this result is not due to small sample sizes. In column (4) we report results for the old payment system using only two years of data (1995–1996) for the same 65 to 69 age range and find a statistically significant two percent increase in daily mortality during *Payweek(1)*.

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

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

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

The variables *Week(w)*, *Special(j)*, *Weekday*,  $\mu$ ,  $\nu$ , and  $\varepsilon$  are defined as before. In this model, we add effects for the birthday-based groups ( $\lambda$ ), and *Payweek(w)* variables are now centered on the second, third, or fourth Wednesday of the month, depending on the group. Synthetic

<sup>16</sup> <http://www.ssa.gov/pubs/2007calendar.htm>.

<sup>17</sup> The exceptions are seniors also receiving Supplemental Security Income (SSI) and former Social Security Disability Insurance (SSDI) recipients in that age range. According to the Social Security Administration’s *Annual Statistical Supplement, 2010*, 3.3% of recipients of Retirement and Survivors Insurance also received SSI in 2005 and 2006. Tables in the same publication suggest that there should be no more than 500,000 former SSDI beneficiaries per year who are aged 65–69 years, which is about 1.5% of Retirement and Survivors Insurance recipients.

**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	Aged 65–69	Aged 65–69	Aged 50–59
	All decedents	Singles	All decedents	All decedents
	2005–06	2005–06	1995–96	2005–2006
	(1)	(2)	(3)	(4)
Payweek(–2)	0.0071 (0.0041)	–0.0013 (0.0231)	0.0010 (0.0054)	–0.0056 (0.0042)
Payweek(1)	0.0111 (0.0035)	0.0275 (0.0176)	0.0001 (0.0042)	–0.0033 (0.0028)
Payweek(2)	0.0023 (0.0057)	0.0033 (0.0232)	–0.0043 (0.0050)	–0.0053 (0.0065)
Payweek(3)	–0.0188 (0.0110)	–0.0605 (0.0296)	–0.0147 (0.0100)	–0.0029 (0.0060)
Week(–2)	0.0052 (0.0061)	–0.0130 (0.0219)	0.0077 (0.0055)	–0.0058 (0.0058)
Week(1)	0.0138 (0.0061)	0.0187 (0.0190)	0.0201 (0.0047)	0.0172 (0.0048)
Week(2)	0.0086 (0.0057)	0.0241 (0.0180)	0.0194 (0.0068)	0.0081 (0.0058)
Week(3)	0.0149 (0.0066)	0.0233 (0.0286)	0.0088 (0.0082)	–0.0097 (0.0057)
Born 1st to 10th	–0.0239 (0.0058)	–0.0190 (0.0116)	–0.0220 (0.0056)	–0.0254 (0.0039)
Born 11th to 20th	–0.0308 (0.0049)	–0.0480 (0.0148)	–0.0356 (0.0048)	–0.0271 (0.0031)
R <sup>2</sup>	0.303	0.080	0.394	0.242
Mean daily deaths	157	12.0	185	215
Observations	2190	2190	2193	2190

The reference periods are *Week(–1)* and *Payweek(–1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks as they represent the days outside the 28-day periods centered, respectively, on the 1st of the calendar month and each day Social Security is paid. Decedents are divided into three groups: those born on the 1st to 10th, 11th to 20th, and 21st to 31st of the month. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, a complete set of dummies for special days throughout the year described in footnote 13, and dummies for observations for decedents born in the first two periods in the month.

months are uniquely defined for each birth date group (*k*). Because pay dates are now fixed on Wednesdays, there are either 28 or 35 days in each synthetic month. If the receipt of income alters short-term mortality, then the payday/mortality cycle should have shifted to different parts of the month for Social Security beneficiaries enrolling after May 1997.

Results from Eq. (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% value on the *Week(1)* variable. There is also a large pay effect: the coefficient on *Payweek(1)* is a statistically significant 1.1%. We also report results for the group effects; the reference group is those born from the 21st to the 31st.<sup>18</sup> A shortcoming of this test is that not all recipients are paid based on their own birth date. A person who claims Social Security benefits under their spouse’s earnings would actually receive the check based on their spouse’s birth date. Consequently, there is some measurement error across the three birth date groups. People who never married should be claiming benefits under their own birth date, so in column (2) of Table 2 we report results for never-married seniors aged 65 to 69 in the 2005 and 2006 MCODE files. There is a much larger increase in the payday effect on mortality. The coefficient on *Payweek(1)* is now 2.75%, although it is a smaller group and so the t-statistic is

<sup>18</sup> In a non-leap year, there are 125 birth days that would put a person into this group, while there are only 120 such days for the other two groups. This is why the dummy variables for these coefficients are negative.

**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	Substance abuse	Non-substance abuse	External causes	Heart attacks	All cancers	All other causes
	1979–96	1979–96	1979–96	1973–96	1973–96	1973–96	1973–96
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Payweek(−2)	0.0039 (0.0018)	0.0086 (0.0109)	0.0039 (0.0018)	0.0268 (0.0061)	0.0042 (0.0023)	0.0026 (0.0023)	0.0035 (0.0020)
Payweek(1)	0.0038 (0.0016)	0.0367 (0.0112)	0.0036 (0.0016)	0.0410 (0.0057)	0.0048 (0.0023)	0.0009 (0.0022)	0.0043 (0.0018)
Payweek(2)	0.0045 (0.0022)	0.0099 (0.0137)	0.0044 (0.0022)	0.0322 (0.0070)	0.0063 (0.0028)	0.0004 (0.0028)	0.0051 (0.0025)
Payweek(3)	0.0038 (0.0034)	0.0119 (0.0131)	0.0037 (0.0034)	0.0275 (0.0074)	0.0052 (0.0038)	0.0044 (0.0030)	0.0041 (0.0035)
Week(−2)	0.0001 (0.0018)	0.0111 (0.0111)	−0.0002 (0.0019)	0.0077 (0.0061)	−0.0020 (0.0024)	0.0015 (0.0024)	−0.0003 (0.0020)
Week(1)	0.0043 (0.0015)	0.0190 (0.0111)	0.0041 (0.0015)	0.0257 (0.0059)	0.0030 (0.0022)	0.0006 (0.0023)	0.0023 (0.0015)
Week(2)	0.0034 (0.0018)	0.0164 (0.0129)	0.0033 (0.0018)	0.0128 (0.0072)	0.0002 (0.0026)	0.0052 (0.0027)	0.0013 (0.0021)
Week(3)	0.0016 (0.0023)	0.0068 (0.0143)	0.0016 (0.0023)	0.0041 (0.0077)	−0.0017 (0.0031)	0.0051 (0.0030)	−0.0002 (0.0024)
R <sup>2</sup>	0.901	0.370	0.900	0.395	0.847	0.961	0.883
Mean daily deaths	4124	36	4088	89	1008	802	2047
Observations	6575	6575	6575	8766	8766	8766	8766

The reference periods are *Week(−1)* and *Payweek(−1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks as they represent the days outside the 28-day periods centered, respectively, on the 1st of the calendar month and each day Social Security is paid. Decedents are divided into three groups: those born on the 1st to 10th, 11th to 20th, and 21st to 31st of the month. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, a complete set of dummies for special days throughout the year described in footnote 13, and dummies for observations for decedents born in the first two periods in the month.

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 Eq. (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.

The work linking mortality to income payments has to date primarily focused on the impact on deaths related to substance abuse. In this section, we estimate models for causes both related and unrelated to substance abuse. Causes of death in the MCODE files are defined using the International Classification of Disease (ICD) codes. Three different ICD versions are used during the period we consider: ICD-8 (1973–78), 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 aim of this analysis is to see whether paycheck/mortality relationship can be explained solely 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 comes from Phillips et al. (1999) and studies of the economic costs of substance abuse in the United States (Harwood et al., 1998), Australia (Collins and Lapsley, 2002), and Canada (Single et al., 1999).<sup>19</sup> 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 Eq. (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 large coefficient (standard error) on the *Payweek(1)* variable of 0.0367 (0.0112). There is also pronounced within-month mortality cycle – the *Week(1)* coefficient is 1.90%, with a p-value of 0.11. In column (3) we re-estimate the model using non-substance abuse deaths. These deaths represent 99% 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), together with the mean daily deaths in each category, indicate that substance abuse deaths account for around eight percent of the rise in mortality in the week after checks arrive. Even with some under-reporting of substance abuse deaths, these results suggest that the effect of income on mortality extends well beyond substance abuse.

In the final four columns of Table 3, we use causes of death codes in the ICD-8 and ICD-9 to create a few broad underlying cause-of-death categories. For each cause, we estimate Eq. (1) for decedents 65 and older for the entire 1973–1996 period.<sup>20</sup> In column (4), we present results for external causes of death (e.g., accidents, murders, suicides, motor vehicle crashes), and find both a large pay week effect (coefficient and standard error on *Payweek(1)* is 0.0410 (0.0057)) and a large within-month effect (coefficient and standard error on *Week(1)* is 0.0257 (0.0059)). In column (5), we present results for heart attacks, a cause often associated with a short time from onset to

<sup>19</sup> A complete list of these codes is provided in an appendix that is available from the authors.

<sup>20</sup> 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](http://seer.cancer.gov/codrecode/1969+_d09172004/index.html)).

death. The *Payweek* coefficients are slightly larger for heart attacks than for all deaths (as reported in column (1) of Table 1). In column (6), we report results for cancer—a cause of death we can view as something of a placebo test because we suspect cancer deaths are less affected by activity than most other causes. We do not find either a pay week or within-month cycle for cancer, as the results for *Payweek* (1) and *Week*(1) demonstrate. Finally, the results for all other causes are presented in column (7), and are similar to the aggregate patterns. Heart attacks account for 26% of the size of the *Payweek*(1) coefficient presented in column (1) of Table 1. Even though the size of the *Payweek*(1) coefficient for external causes is much larger than for heart attacks, external causes explain less of the aggregate pattern (around 20%).

## 2.2. 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.<sup>21</sup> In this section, we examine whether mortality spikes after these pay dates. Active duty military are predominantly male (currently 85%), young (approximately one half are under 25 years of age) and healthy (Segal and Segal, 2004). Newspaper accounts suggest that many military personnel spend more than average on and immediately after payday. The phenomenon appears to be widespread, with payday-generated spending increases reported at bars, restaurants, cinemas, malls and hairdressers near bases in Connecticut,<sup>22</sup> Hawaii,<sup>23</sup> North Carolina<sup>24</sup> and Virginia.<sup>25</sup>

In this section, we compare mortality patterns in counties with and without a high fraction of their population on active military duty. Soldiers normally reside on or near the base to which they are attached, and these bases are unevenly distributed throughout the country. Since both the size of the military and base locations were fairly uniform over the 1973 to 1988 period, and since the public-use MCOB files contain exact dates of death during this time, we focus on these years.<sup>26</sup> The size of the military changed considerably in the early 1990s following military downsizing and a number of base closings.

We identified counties with more than 15% of their population aged 17 to 64 who were military personnel<sup>27</sup> in the 1970, 1980 and 1990 Censuses using Census Summary File 3 data sets.<sup>28</sup> There are 21 counties that meet this criterion.<sup>29</sup> In 1990 there were roughly 326,000 people aged 17 to 64 in these “military” counties, of which about one quarter were in the military. The proportion of the population affected by the military payment schedule is higher than this fraction because civilian employees on military bases are paid

<sup>21</sup> 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.

<sup>22</sup> Carbone, Gerald M. “Dive! Dive! Groton Fears Loss of Base,” *Providence Journal*, Providence RI, Feb 28, 1995, p.A01.

<sup>23</sup> Song, Jaymes. “Many Businesses that Rely on Sales to the Military Struggle to Survive with Recent Mass Deployments,” *Honolulu Star-Bulletin*, Honolulu HI, June 21, 2004.

<sup>24</sup> Foster, J. Kyle. “Retail Boost,” *Fayetteville Observer*, Fayetteville NC, May 7, 2001, Local & State Section. Mullen, Rodger. “It Must be Payday,” *Fayetteville Observer*, Fayetteville NC, Mar 11, 1990, Lifestyle Section.

<sup>25</sup> Snead Fulk, Sande. “Lifeblood of a Local Economy; Advisory Committee Studies Base’s Impact,” *The Richmond Times-Dispatch*, Richmond VA, May 22, 2002, p.4.

<sup>26</sup> Various issues of the *Statistical Abstract of the United States* indicate that the active duty military was anywhere from 2.04 to 2.25 million from 1973 to 1988, dropping to 1.38 million in 2001 as a number of bases closed across the country.

<sup>27</sup> Enlistment in the military can occur at age 17 years with parental consent, and at age 18 years without.

<sup>28</sup> These data are taken from the National Historical Geographic Information System (Minnesota Population Center, 2004).

<sup>29</sup> 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).

on the same schedule,<sup>30</sup> and both they and military personnel have dependents.

We compare mortality in these counties with deaths in 2772 “non-military” counties that have less than 1% military among adults aged 17–64 in the 1970, 1980 and 1990 Censuses. We present results for two groups: those aged 17–29 years and 17–39 years. We choose these age breakdowns because, during this period, 69% of all active duty military were aged 17–29 years and 91% were aged 17–39 years.<sup>31</sup>

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.<sup>32</sup>

In Fig. 2, we use data from the 1973–1988 MCOB to construct relative daily mortality rates for those aged 17–29 years in military and non-military counties. We construct rates for a 28 day period that represents the seven days before and after the two military paychecks are distributed each month—the first check being near the 1st of the month and the second being near the 15th of the month. *Day*(1) is the day checks are distributed and *Day*(–1) is the day before checks arrive. The solid line in the graph represents the daily mortality risk for military counties, the dotted line is for non-military counties and the vertical lines are 95% confidence intervals for the daily mortality risk.

The two groups show similar patterns around the first payday of the month. There is a within-month mortality cycle for both military and non-military counties, with deaths declining before checks arrive and rebounding afterwards. The spike in deaths around the 1st of the month may be due to the within-month mortality cycle, and also the fact that three-sevenths of all payments are distributed on a Friday and there is a spike in deaths for all demographic groups on the weekend. Both groups show increases in mortality right after the second checks arrive, which may again be due by the fact that three-sevenths of these checks are paid on Fridays. A key difference, however, is that the pattern is more pronounced for military counties. Daily mortality rates on *Day*(1) to *Day*(4) are 10 to 18% higher than average, a noticeable increase over non-military counties. These raw numbers suggest mortality is higher in military counties right after the second check arrives.

To formally test whether military and non-military counties exhibit different mortality patterns around the 1st and 15th of the month, we estimate a model similar to Eq. (1). A key difference is that, because daily mortality counts in the military counties are small and occasionally zero, we use a negative binomial model that allows for integer values and estimate it by maximum likelihood (Hausman et al., 1984). Let  $Y_{idmy}$  be daily mortality counts for group  $i$  (for military and nonmilitary counties) on day  $d$ , month  $m$  and year  $y$ . Let  $X_{idmy}$  be vector that captures the exogenous variables in Eq. (1). Within the negative binomial model,  $E[Y_{idmy} | X_{idmy}] = \delta \exp(X_{idmy} \beta)$ , where  $\delta$  is a parameter that captures whether the data exhibits over-dispersion.<sup>33</sup>

<sup>30</sup> 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.

<sup>31</sup> Authors’ calculations using data from the 1980 Census 5% Public Use Micro Samples (Ruggles et al., 2010).

<sup>32</sup> Data from the 1996–2004 Diary Survey record of the CEX indicate that 9.6% of workers report their last pay check as being paid monthly, while 5.5% report being paid twice-monthly. Most respondents are paid weekly (31.4%) or every two weeks (50.6%), with 2.9% paid some other frequency.

<sup>33</sup> It can be demonstrated that the variance of counts in the negative binomial model is  $\text{Var}[Y_{idmy} | X_{idmy}] = \delta^2 [1 + (1/\delta)] \exp(X_{idmy} \beta)$ , so the variance to mean ratio in this model is  $\delta + 1$ . When  $\delta = 0$  the negative binomial collapses to a Poisson model which, by construction, restricts the variance to equal the mean.

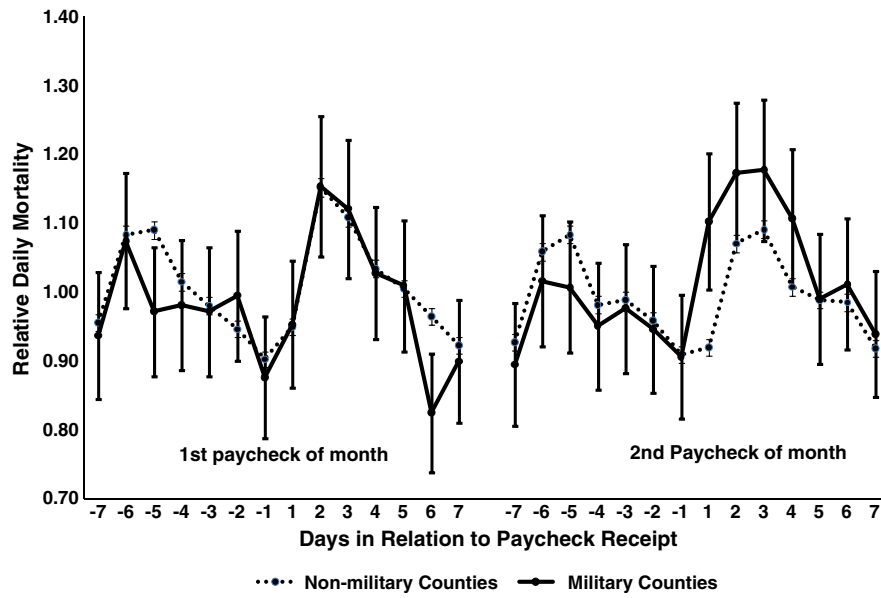


Fig. 2. Relative daily mortality rates, military and non-military counties, ages 17–29, 1973–1988 MCOD.

By definition,  $\partial \ln E[Y_{idmy} | X_{idmy}] / \partial X_{idmy} = \beta$  so the parameters in this model are interpreted similarly to those in Eq. (1).

In constructing the dataset, the “synthetic” months are 28-day periods that begin seven days before the first payment each month and end seven days after the second payment each month.<sup>34</sup> 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.<sup>35</sup>

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

$$X_{idmy}\beta = \beta_0 + \sum_{j=1}^6 Weekday(j)_{dmy}\gamma_j + \sum_{j=1}^M Special(j)_{dmy}\varphi_j + Period1_{dmy}\beta_p + Military_i\beta_m + (Period1_{dmy})(Military_i)\beta_{pm} + Military_iPeriod1_{dmy}Week(1)_{dmy}\alpha_{1m} + NonMilitary_iPeriod1_{dmy}Week(1)_{dmy}\alpha_{1n} + Military_iPeriod2_{dmy}Week(1)_{dmy}\alpha_{2m} + NonMilitary_iPeriod2_{dmy}Week(1)_{dmy}\alpha_{2n} + \mu_m + v_y \quad (3)$$

where *Weekday*, *Special*, and the synthetic month and year effects are defined as before and we capture the month and year effects through a series of dummy variables. We control for differences across groups with a dummy for counts in military areas (*Military*), across pay periods (*Period1*), and their interaction. Around each payday are two weekly periods, the week before (*Week(-1)*) and the week after checks arrive (*Week(1)*). The key covariates are interactions that measure whether military and non-military counties experience a spike in deaths the week after checks arrive compared to the week before. We examine whether the daily mortality patterns differ across military and non-military counties by testing the null hypothesis  $H_0: \alpha_{jm} = \alpha_{jn}$  for the two pay periods,  $j = 1$  and  $2$ .

The maximum likelihood results for the negative binomial model are reported in Table 4. In column (1), we report the results for those aged 17–29 years. The first two rows contain the coefficients on *Week(1)* for non-military and military counties for the first pay period of the month. The next two rows contain the same set of coefficients for the

second pay period. For each group of coefficients, we also report the p-value on the null hypothesis that the military and non-military coefficients are equal. 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 Fig. 2. Among 17–29 year olds, in the week after the first pay check arrived, there is a spike up in mortality of about 1.8% for both county types. The coefficient for the non-military counties is statistically significant, but the coefficient for the military counties is not. The p-value on the test that the coefficients are equal is very high. In the week after the second paycheck of the month arrives, we find a statistically

Table 4  
Maximum likelihood estimates of daily mortality negative binomial equation, counties with and without a high military presence, 1973–1988.

Covariates	Treatment counties have 15% military presence		Treatment counties have 20% military presence	
	Deaths 17–29 year olds	Deaths 17–39 year olds	Deaths 17–29 year olds	Deaths 17–39 year olds
(1) Non-military × pay period 1 × Week 1	0.0177 (0.0045)	0.0191 (0.0039)	0.0175 (0.0051)	0.0191 (0.0039)
(2) Military × pay period 1 × Week 1	0.0188 (0.0309)	0.0049 (0.0233)	0.0013 (0.0390)	0.0154 (0.0309)
p-value: test on test: (1) = (2)	0.972	0.661	0.667	0.901
(3) Non-military × pay period 2 × Week 1	0.0097 (0.0043)	0.0041 (0.0033)	0.0098 (0.0042)	0.0042 (0.0033)
(4) Military × pay period 2 × Week 2	0.1028 (0.0305)	0.0462 (0.0252)	0.1305 (0.0368)	0.0680 (0.0311)
p-value: test on test: (3) = (4)	0.002	0.033	0.001	0.028
<i>Mean of dependent variable:</i>				
Non-military counties	132.3	242.8	132.3	242.8
Military counties	1.62	2.62	1.09	1.78

There are 10,584 observations in each model. Military counties have over 15 or 20% 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. 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 13, 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.

<sup>34</sup> 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) are removed when this happened in 1982 and 1988.

<sup>35</sup> 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).



significant one percent increase in mortality in non-military counties and a statistically significant coefficient that is ten times larger in military counties. The p-value of 0.002 means we can reject the null that these results are the same.

In the next column, we include deaths for people aged 17–39 years. Focusing on the second paycheck of the month, we find that in military counties, mortality is a statistically significant 4.6% higher the week after the second paycheck arrives, an effect that is 10 times larger than the first-week effect in non-military counties. The p-value on the test of equality of the coefficients means we can reject the null.

In the third and fourth columns of the table, we re-estimate the basic models by restricting the definition of military counties to those with 20% or greater adults aged 17–64 years on active duty military. The number of counties falls to 15 and average deaths in the treatment group fall considerably as well, meaning we should witness a decline in the precision of the military coefficients. However, the fraction of treated people in a county should increase meaning the coefficient on *Military\*Period2\*Week(1)* should rise. Both of these conjectures are borne out in the data. Among 17–29 year olds in military counties, mortality is 13% higher the first week after the second check arrives, a number that is 30% larger than the effect in column (1) but with a standard error that is 21% larger as well. Among 17–39 year olds in this more restrictive sample, the coefficient on *Military\*Period2\*Week(1)* is now almost seven percent, which is statistically significant and 16 times larger than the similar coefficient for the non-military counties.

All of the results for the second pay period indicate that, in military counties, daily mortality rates are substantially higher the week after military checks are normally distributed. There is no comparable effect in non-military counties. We suspect the large difference in results between the first and second payday of the month for military personnel to be due to a combination of factors. Many households have large re-occurring bills near the 1st of the month (Evans and Moore, forthcoming). We suspect a large portion of the paycheck paid near the 1st of the month will go towards these items. This means the second paycheck of the month might have a larger discretionary component. Non-military counties will not display this pattern around the 15th of the month since so few outside the military are paid on a twice-monthly basis.

### 2.3. Providing a metric to scale the estimates

It is helpful to have a common metric in order to be able to compare the mortality responses across the different payment amounts used in these tests. We do this by constructing an elasticity that measures the percentage change in aggregate annual mortality given the change in annual income generated by one additional paycheck.

This calculation is most easily made for the military example. Among those aged 17–29 years in military counties, there are 2.62 deaths per day. The second paycheck raises mortality by about 10% for one week, so one of these paydays increases annual mortality by about 0.2% (10% divided by 52 weeks). Data from the 1980 Census 5% PUMS (Ruggles et al., 2010) indicates that, for families in the “military” counties with someone aged 17–29 years, military pay

represents roughly 24% of family income for the year. Therefore, each payday represents about one percent of aggregate annual income. Dividing 0.002 by 0.01, the elasticity of mortality with respect to the change in income is 0.20. This is a steep gradient in mortality with respect to income (Snyder and Evans, 2006).

For both Social Security pay schedules, each payment is a larger share of annual income because checks arrive once a month. The mortality responses are also smaller than in the military case. Using results from both the pre- and post-1997 Social Security payment schedules, we estimate annual mortality elasticities with respect to income of about 0.01 in the Social Security beneficiary population, which is substantially smaller than in the military wage analysis.

### 3. The short-term mortality consequences of one-time and infrequent income receipt

In this section, we consider the short-term mortality impact of one-time and infrequent income receipt. Specifically, we consider two cases: the 2001 Tax Rebates and the annual Alaska Permanent Fund payments. Both of these cases have been considered by authors in the literature on excess sensitivity. These two situations broaden the empirical work in this paper along three dimensions. First, these income changes can be considered exogenous increases in income (wealth), unlike the two cases in the previous section. The mortality impact of these payments could generate very different patterns. Second, these groups extend the phenomenon beyond the elderly and military personnel. Third, the infrequent nature of the payments will allow us to determine whether increases represent “short-term mortality displacement” where the deaths of the frail were hastened by a few days, a phenomenon routinely referred to as “harvesting” (Zeger et al., 1979).

#### 3.1. The 2001 tax rebates

The *Economic Growth and Tax Relief Reconciliation Act*<sup>36</sup> 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%. This tax change was applied retroactively for income earned in 2001 and, as an advance payment on the tax cut, households were sent rebates based on their 2000 tax returns in the summer and fall of 2001. The maximum rebates for single and married taxpayers were \$300 and \$600, respectively, and approximately two-thirds of all people lived in households that received a rebate check. Johnson et al. (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.<sup>37</sup> The first checks were sent on Monday, July 23, to taxpayers whose second-to-last SSN digit was a zero.<sup>38</sup> Table 5 shows the exact distribution dates of checks by SSN. The Treasury Department sent letters to taxpayers a few weeks before checks arrived to inform them of the size and date of their check (Johnson et al., 2006).

This tax rebate is a useful setting for testing the mortality consequences of income receipt, as the second-to-last digit of the SSN is effectively randomly assigned.<sup>39</sup> Johnson et al. (2006) use this

**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

<sup>36</sup> <http://www.gpo.gov/fdsys/pkg/PLAW-107publ16/pdf/PLAW-107publ16.pdf>.

<sup>37</sup> For married taxpayers filing jointly, the first Social Security number on the return determined mailing date.

<sup>38</sup> Households who filed their year 2000 tax return late may have been sent their rebates after the ten-week period shown in Table 5. According to Slemrod et al. (1997) 92 percent of taxpayers typically file on or before the normal April 15 deadline, so the vast majority of households would have received their checks according to the schedule outlined in Table 5.

<sup>39</sup> The last four digits of the SSN are assigned sequentially within a geographic area, so are effectively random. The second-to-last digit mailing system was in fact chosen because it was felt the random assignment made it a fair way to allocate the checks (Johnson et al., 2006).

**Table 6**

Estimates of log of weekly mortality counts equation aged 25 to 64 years, 30-week period, summer and fall 2001.

Independent variable	All deaths (1)	All deaths (2)	Substance abuse (3)	Non-substance abuse (4)	Aged 25–44 yrs (5)	Aged 45–54 yrs (6)	Aged 55–64 yrs (7)
Rebate1	0.0269 (0.0100)	0.0227 (0.0100)	0.0075 (0.0370)	0.0243 (0.0109)	−0.0089 (0.0198)	0.0530 (0.0179)	0.0151 (0.0150)
Rebate2		−0.0157 (0.0101)	−0.0134 (0.0371)	−0.0161 (0.0109)	−0.0222 (0.0199)	−0.0101 (0.0179)	−0.0160 (0.0150)
Rebate3		−0.0221 (0.0101)	−0.0182 (0.0371)	−0.0233 (0.0109)	−0.0119 (0.0199)	−0.0043 (0.0179)	−0.0414 (0.0150)
Rebate4		−0.0085 (0.0100)	−0.0693 (0.0370)	−0.0029 (0.0109)	−0.0081 (0.0198)	−0.0048 (0.0179)	−0.0100 (0.0150)
Total effect (Rebate1–4)		−0.0237 (0.0233)	−0.0934 (0.0859)	−0.0183 (0.0252)	−0.0511 (0.0460)	0.0338 (0.0415)	−0.0523 (0.0347)
P-value on test, group effects = 0	0.813	0.806	0.937	0.829	0.024	0.459	0.581
R <sup>2</sup>	0.715	0.723	0.157	0.724	0.791	0.410	0.256
Mean weekly deaths per group	1014	1014	85	929	249	314	451

Standard errors are in parentheses. The other covariates in the model are week fixed effects and Social Security number group fixed effects. Each regression has 300 observations.

fact and data from a special module in the Consumer Expenditure Survey to show that consumption of nondurable goods increased in the months after the arrival of checks, with food away from home being the main component that was affected.

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)<sup>40</sup> to the 2000–2002 MCOD data files.

The econometric model for this event is straightforward. Let  $i = 0$  to 9 index groups of people based on the second-to-last digit of their SSN. Let  $t$  index one of 30 7-day periods during 2001, with the first period beginning on Monday May 14th and the last beginning on Monday 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

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

where  $\nu_t$  are fixed week effects,  $\eta_i$  are fixed group effects, and  $\varepsilon_{it}$  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 September 11 terrorist attacks occurred during Week 18 in our analysis. The Centers for Disease Control estimates that there were 2902 deaths associated with September 11th, which is roughly twenty percent of weekly deaths during this period.<sup>41</sup> There also appears to be a drop in mortality in the weeks just after September 11th as individuals stayed home and reduced their travel. The week effects will capture these cyclic changes in mortality so long as the deaths associated with September 11 are equally distributed across the ten SSN groups. The coefficient on  $\beta_1$  is the key variable of interest and it identifies the short-run impact of the rebates on mortality.

There are two caveats to Eq. (4). First, only taxpaying units with taxable income in 2000 received a tax rebate in 2001. The coefficient on  $\beta_1$  represents a reduced-form effect and not the impact of actually receiving a check. Therefore, a key to the analysis is to reduce the sample to people likely to have received a tax rebate. We do this by

restricting the sample to those aged 25 to 64, who are much more likely to have paid taxes than other age groups.<sup>42</sup> Second, for married couples filing jointly, the rebate check was sent according to the SSN of the first name on the IRS 1040 form. This form does not record the sex of the taxpayers so we have no idea whether husband or wives are more likely to be listed as the first taxpayer. Although both partners in a marriage are presumably treated by the additional income, the mailing of the check was based on the SSN of only one of them. Because people not sent a check but treated with a rebate through their spouse should be randomly distributed across the different groups, this should systematically bias our results towards zero.

The results for Eq. (4) are reported in Table 6. The SSN groups experience a statistically significant 2.7% 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. This effect is also present among the non-married and when we use only the period prior to the September 11 terrorist attacks.<sup>43</sup>

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

In column (2) of Table 6, we add  $REBATE2$ ,  $REBATE3$ , and  $REBATE4$ , which are dummies for the second, third and fourth week after the checks arrive, respectively, to examine whether the increase in mortality in the first week represents mortality displacement. 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  $REBATE4$  coefficients in column (2) produces an estimated change (standard error) in mortality of  $-0.0237$  (0.0233). We cannot reject the null of no aggregate change in mortality over the first four weeks after checks arrive.

<sup>42</sup> 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% 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%.

<sup>43</sup> Restricting the sample to the unmarried produces a coefficient (standard error) on  $REBATE1$  of 0.0280 (0.0134). When we re-estimate the original model eliminating all data after week 17, which are observations after the September 11th attacks, the coefficient (standard error) on  $REBATE1$  is 0.0241 (0.0111).

<sup>40</sup> 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](http://www.cdc.gov/nchs/ndi.htm).

<sup>41</sup> <http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51Spa6.htm>.

**Table 7**

Timing and size of Alaska Permanent Fund dividend payments.

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

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%	\$1963.86	64%	10/4,W	10/5,Th	92.2%
2001	632,241	93%	\$1850.28	66%	10/10,W	10/17,W	93.6%
2002	640,544	92%	\$1540.76	70%	10/9,W	10/16,W	93.3%
2003	647,747	92%	\$1107.56	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%	\$1106.96	76%	10/4,W and 10/19,Th	11/14,Tu	97.8%

We define substance abuse-related deaths using the ICD-10 codes in a similar way as in the previous two sections. We estimate that 8% of deaths in this sample are due to substance abuse, or 85 deaths per group per week. 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.

We also show the results for three age-based subgroups in Table 6: deaths among those aged 25–44 years in column (5), 45–54 years in column (6); and 55–64 years in column (7). For the youngest sample, none of *REBATE1* to *REBATE4* coefficients are statistically significant. The p-value on the test that the group effects are zero is 0.02; given the persistently high values in the other regression, this may be chance. For 45–54 year olds, deaths increase by a statistically significant 5.3% in the first week after the checks arrive. The coefficients on *REBATE2* to *REBATE4* are less than one percent and statistically insignificant. Among 55–64 year olds, the coefficient on *REBATE1* is 1.5% and the coefficient on *REBATE2* is –1.5%, with neither statistically significant. There is a statistically significant negative coefficient on *REBATE3* of –4.1%, while the *REBATE4* coefficient is a statistically insignificant –1.0%. The total effects in the three age groups are all statistically insignificant.

Reducing the sample to specific causes of death produces few statistically significant coefficients due to the increased variance associated with disaggregated causes of death. We also estimate two placebo regressions using the same periods and group definitions as 2001, but re-estimated using 2000 and 2002 MCODE data. The coefficients (standard error) on *REBATE1* in these two models are 0.0094 (0.0107) and –0.0174 (0.0107), respectively.

### 3.2. 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.<sup>44</sup> Since 1982, an annual dividend has been paid to Alaskans from the income generated by fund investments during the previous five years. The amount paid has been between \$331 in 1984 and \$2069 in 2008 (when a one-off additional payment of \$1200 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.<sup>45</sup> Individuals

<sup>44</sup> From the 2008 Annual Report of the Alaska Permanent Fund Corporation. Available at: <http://www.apfc.org/home/Content/reportspublications/reportArchive.cfm>.

<sup>45</sup> Residency requirements have been the same since 1990. Minor changes occurred in earlier years. Historical information is available at: <https://www.pfd.state.ak.us/historical/index.aspx>

must apply each year to receive the dividend, and at least 88% of Alaskans have received the dividend each year. Table 7 contains the dividend amounts and the percentage of the population receiving them in recent years.

Hsieh (2003) uses variation in the size of dividends by family size and over time to test whether nondurable consumption changes in response to dividend payments. Using the CEX from the 1984 to 2001, he finds no evidence households react to these payments – even though household consumption is sensitive to income tax refunds – which leads him to conclude that households adhere to the LC/PIH for large and predictable payments (like the Alaska dividend), but not for small and less predictable payments (like income tax refunds). In recent years, however, the dividend payments have been concentrated in early October and anecdotal evidence of increased spending after dividends arrive suggests activity-induced changes in mortality are possible as a result of the dividend.<sup>46</sup>

We explore the short-term relationship between income payments and mortality for recent years. Payments were initially made entirely by check, mailed at a rate of 50,000 per week. Payment by direct deposit was introduced in 1993. Approximately 30% 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% of paper checks were processed and mailed in a single batch shortly after the payment of direct deposits. The exact dates that direct deposits were paid, as well as the dates checks were issued, are shown in Table 7 for the years 2000 to 2006. We use the timing of direct deposits from 2000 through 2006 to investigate whether dividend payments change mortality patterns among Alaskans. We focus on this period because of the popularity of direct deposit and the close proximity between the receipt of direct deposits and paper checks.

The primary data for this analysis are from the MCODE 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. The econometric model here is a simple difference-in-difference specification, with the data for the rest of the U.S. providing an estimate of the time path that would occur in the absence of the dividend intervention. Let  $w$  denote 12 seven-day periods that begin on Tuesdays,<sup>47</sup> with the first period each year beginning 15 days after Labor Day (the first Monday in September).<sup>48</sup> Let  $\ln(y_{swy})$  be the natural log of the deaths for state  $s$

<sup>46</sup> See for example: Chambers, Mike. "Alaska Permanent Fund dividend is \$1,850.28," *The Associated Press*, State & Local News, September 19, 2001; Egan, Timothy. "Fringe Benefits from Oil Give Alaska a Big Payday," *The New York Times*, October 9, 1996, p. A1; Pemberton, Mary. "Alaskans prepare to spend annual windfall," *The Associated Press*, State & Regional News, October 3, 2003.

<sup>47</sup> All direct deposits during 2000 to 2006 were made on Tuesdays, Wednesdays or Thursdays.

<sup>48</sup> 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.

**Table 8**  
Estimates of log of weekly mortality counts equation Alaskans compared to residents in the rest of USA, 2000 to 2006.

Independent variable	Compared to rest-of-USA				Comparison to similar states			
	All deaths		Urban areas		All deaths		Urban areas	
	All	All	All	W/o sub. abuse	Based on temp.	Based on income	Based on temp.	Based on income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Alaska*dividend(1)	0.0907 (0.0551)	0.0799 (0.0562)	0.1329 (0.0742)	0.1414 (0.0817)	0.0836 (0.0370)	0.0771 (0.0313)	0.1366 (0.0398)	0.1301 (0.0344)
Alaska*dividend(2)		-0.0368 (0.0562)	0.0272 (0.0742)	0.0518 (0.0817)	-0.0479 (0.0370)	-0.0325 (0.0313)	0.0162 (0.0398)	0.0315 (0.0344)
Alaska*dividend(3)		-0.0975 (0.0562)	-0.0809 (0.0742)	-0.0507 (0.0817)	-0.1033 (0.0370)	-0.1055 (0.0313)	-0.0867 (0.0398)	-0.0889 (0.0344)
Alaska*dividend(4)		0.0132 (0.0562)	0.0790 (0.0742)	0.0946 (0.0817)	0.0059 (0.0370)	0.0125 (0.0313)	0.0717 (0.0398)	0.0783 (0.0344)
Total effect [Alaska*dividend(1)–(4)]		-0.0412 (0.1333)	0.1582 (0.1761)	0.2370 (0.1938)	-0.0617 (0.0878)	-0.0484 (0.0743)	0.1378 (0.0944)	0.1510 (0.0816)
R <sup>2</sup>	0.9996	0.9996	0.9994	0.9994	0.9941	0.9942	0.9971	0.9970
Mean weekly deaths in Alaska	60.4	60.4	33.0	30.4	60.4	60.4	33.0	33.0
Observations	168	168	168	168	770	770	770	770

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

(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:

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

where  $v_{wy}$  is a fixed effect that varies by week  $w$  and year  $y$ , and  $\epsilon_{swy}$  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  $\beta_1$  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 Eq. (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 an increase in deaths of 9.1% for the week checks are received, and a p-value of 0.12. The results in column (2) suggest substantial harvesting, with the coefficients on *Alaska\*Dividend(2)* and (3) being -3.7% and -9.8%, respectively. This final number has a t-statistic of 1.73, which is statistically significant at the 10% 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. 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 column (3) of Table 8, we restrict our attention to residents in the boroughs that contain Anchorage (260,283 residents in 2000 Census), Fairbanks (30,224) and Juneau (30,711), the only cities in Alaska with more than 10,000 residents.<sup>49</sup> In this model, we keep the same comparison

<sup>49</sup> 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 MCOB data identifies the FIPS code of residence for all decedents over this time period.

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 13% increase in mortality – an extra four deaths – the week direct deposit occurs. The p-value on this statistic is less than 0.10. As in both column (2) and the case of the 2001 tax rebates, we see a drop in mortality the third week after dividends are paid. The sum of the coefficients over the first four weeks after checks arrive is 0.148, although it is not statistically significant. As with the previous tests, the results are not entirely due to substance abuse. Using the same ICD-10 coding as in the tax rebate section, we attribute 8% of deaths among Alaskans to substance abuse. The impact of the Permanent Fund payments on non-substance abuse deaths, reported in column (4), is similar to the corresponding values for deaths in columns (3).<sup>50</sup> The coefficient on *Dividend(1)* is 0.1414 and it is statistically significant at the 10% level.

To check the robustness of these results, the rest-of-USA counts are replaced with state-level weekly mortality counts for states that have similarities to Alaska. One comparison uses the ten states in the continental United States with the closest mean annual temperature to Juneau, Alaska, of which three have a lower average temperature and seven have higher.<sup>51</sup> Another uses ten states with similar per-capita income in 2007. Alaska is ranked 15th, and we use the five states ranked just lower and the five states ranked just higher than this level.<sup>52</sup> In both cases, the estimated model remains the same, except that there is a dummy variable for each state to capture underlying differences in mortality counts.

All Alaskan deaths are compared to similar temperature states in column (5) and similar income states in column (6) of Table 8. The coefficients are similar in direction and size to the results already discussed; the standard errors shrink, and in both regressions the positive coefficient on *Dividend(1)* and the negative coefficient on

<sup>50</sup> There are too few substance abuse-related deaths in Alaska to separately estimate the effect for these deaths.

<sup>51</sup> The National Oceanic and Atmospheric Administration have average temperature from 1971–2000 for 48 states here: <http://www.esrl.noaa.gov/psd/data/usclimate/tmp.state.19712000.climo>. They do not provide a figure for Alaska, although similar data is available for Juneau, Alaska for the same period, here: [http://www.census.gov/compendia/statab/cats/geography\\_environment/weather\\_events\\_and\\_climate.html](http://www.census.gov/compendia/statab/cats/geography_environment/weather_events_and_climate.html). The mean temperature in Juneau is 41.5 degrees. There are three states with colder average temperatures than Juneau (ND = 40.43, ME = 40.97, MN = 41.16) and seven states with annual temperatures under 45 degrees (WY, MT, VT, WI, NH, ID, MI).

<sup>52</sup> Per-capita income in 2007 is from: <http://www.census.gov/statab/ranks/rank29.html>. The five ranked lower are IL, RI, HI, PA and FL, and the five higher are CO, MN, DE, NV, WA.

*Dividend*(3) are now statistically significant at conventional levels. The urban Alaskan results are re-run using these comparison states and presented in columns (7) and (8) of Table 8. In both cases, the coefficients remain qualitatively the same while the standard errors shrink. In the income-based sample, the net effect of the four coefficients is 15.1%, which is statistically significant at the 10% level.

### 3.3. Providing a metric to scale the estimates

For the 2001 tax cuts and the Alaska Permanent fund examples, we can create a metric to compare these estimates to each other and other estimates in the literature. The metric in this case is an elasticity that represents the percent increase in excess deaths associated with the percent change in annual income generated by the one-off payment.

Using data from the March Current Population Survey from 2001 to 2007, we estimate that the Alaska Permanent Fund payments increased annual per capita income over the 2000–2006 period by about 5.5%. Data from Column (8) in Table 8 suggests that this transfer increases mortality by 15.1% over the first four weeks after checks arrive, which is an increase in annual mortality of 0.29%. The mortality/income elasticity is therefore 0.053 (0.0029/0.055).

For the 2001 tax cut, we illustrate how the calculation is made for the 45–54 age group and simply report the results for the other age groups. Using estimates from the 2001 March Current Population Survey of who paid taxes in 2000, and assuming that married couples receive a \$600 rebate and individuals receive a \$300 rebate, we estimate that the tax rebate increased the annual family income of 45–54 year olds by 0.35%. The results from Table 6 suggest this increased mortality by 3.4% for a four week period, which is an increase in annual mortality of 0.065%. The mortality/income elasticity is therefore  $0.00065/0.0035 = 0.19$ . For the other two age groups, the sum of the Rebate (1)–(4) coefficients is negative and as a result, we estimate a mortality/income elasticity of  $-0.28$  for adults aged 25–44 and a value of  $-0.29$  for adults aged 55–64 years, which are similar to elasticities in Snyder and Evans (2006) for similar age groups.

## 4. Discussion

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

The age variation across the Social Security and 2001 tax rebate analyses suggest that mortality in younger populations is more responsive to income receipt than in older groups. If the Social Security and military results are compared by looking at how much mortality increases relative to the percentage of annual income being received, then the effects are much larger in the military context.

Changing levels of consumption/activity is the most plausible mechanism through which income receipt affects mortality. The findings for particular causes of death in the Social Security analysis are consistent with this: we observe such relationships for causes of death connected to short-term consumption – like heart attacks and traffic accidents – but not for cancer deaths, where no such connection exists.

Three alternative reasons for such a relationship are improbable. First, the change to the Social Security payment schedule and the structure of the 2001 tax rebates allow us to rule out within-month or seasonal factors that coincide with income receipt. Second, the criteria for receiving these payments should not encourage people to improperly record dates of death for financial gain. For example, military paychecks are paid for income that has already been earned, so misreporting death dates cannot change that value. Likewise, a deceased applicant's Permanent Fund dividends go to their estate, and the tax rebates were based on tax returns from the previous year.<sup>53</sup> Third, there is a literature suggesting that some patients tend to die right after milestone dates are reached (e.g., birthdates, anniversaries, holidays, etc.). While it is possible that income recipients wanted to hang on for one more check, the large spike in mortality for external causes and heart attacks and the lack of any effect for cancers run counter to this argument.

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, and external causes of death are largely unconnected to short-term variation in a person's health.

When it comes to understanding the implications of these findings, the most important question is how much of the increased fatality is mortality displacement. While the 2001 tax rebates and the Alaska Permanent Fund payments have the potential to shed light on this, the results are not definitive on this point. In the tax rebate analysis using 25 to 64 year olds, a 2.3% increase in mortality in the first week after income receipt is offset by a 2.2% decrease in mortality in the third week. Among 45 to 54 year olds, however, there is a 5.3% increase in the first week that is not offset by decreases in the next three weeks. Similarly, while the analysis using all deaths in Alaska suggests there is not a net increase in deaths in the four weeks after income receipt, in urban areas there is a large increase deaths in the first week that is not fully offset in later weeks.

Age and cause of death are probably important for understanding this displacement issue. It is fairly easy to see how heart attack deaths are displacing mortality by a few days, as someone prone to a heart attack today is probably prone to one in a few days as well. In contrast, it is less likely that accidental deaths today would have occurred in the future. This is particularly true for younger people, who face few competing mortality risks. The Social Security analysis suggests both heart attacks and external causes are responsive to income, which may mean that some deaths are displacement while others are not.<sup>54</sup> Identifying the amount of mortality displacement will clarify the impact of income receipt on life expectancy.

Another interesting question is whether greater pay frequency mitigates some of the damage associated with payday mortality. It is not clear from our results that this is the case. The experience in the military, in particular, gives us pause as to the effectiveness of higher frequency payments. In that case, we found a large increase in mortality associated with the paycheck distributed near the middle of the month. Our conjecture is that since large bills such as rent/mortgage and car payments are bunched near the first of the month, less money from that paycheck is left over for discretionary items. In contrast, the midmonth check has less competition for resources and hence the larger mortality effect. If mortality is linked to having a full

<sup>53</sup> Payments to Social Security beneficiaries cease the calendar month after death. Funeral homes and government agencies report deaths so there are limited opportunities for delaying reporting.

<sup>54</sup> We tried to estimate the 2001 tax rebate and Alaska results by cause of death, but the sample sizes are too small to generate precise estimates.

wallet, then increasing the number of days with money in the pocket may increase aggregate mortality. This is a subject for further research. The variation in the size of the mortality effect in response to payment size is also a subject for future research, as the structure of the data and the nature of our quasi-experiments do not allow us to examine this.

In recent years, authors have tested whether socioeconomic status causally affects health by using exogenous variation in education<sup>55</sup> and income.<sup>56</sup> There are conflicting results among studies examining the role of income, and our results below may be instructive for this literature. First, authors must measure the impact of income from the time of receipt, because there are immediate consequences which may be different from those in the long-term. Second, the short-term mortality effect of income receipt makes it more difficult to use exogenous variation in income to identify a causal link between income and health. This increases the size of the sample or of the income shock required to find a statistically precise income/health relationship. Third, these short-run effects may impact the efficacy of cash transfers, although more research is required to determine whether the negative mortality effect is a fixed cost of income receipt or changing in the amount of income received.

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

One potential policy consequence flowing from these results is that 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.

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<sup>55</sup> For example, authors have examined whether health outcomes are altered by increases in education generated by policies such as compulsory schooling (Lleres-Muney, 2005), an increase in access to colleges (Currie and Moretti, 2003) and the Vietnam Draft (de Walque, 2007; Grimard and Parent, 2007).

<sup>56</sup> Such work exploits variation in income produced by such factors as winning the lottery (Lindahl, 2005), German reunification (Frijters et al., 2005), receiving an inheritance (Meer et al., 2003), South African pensions (Case, 2004) and changes in Social Security (Snyder and Evans, 2006).

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