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LIQUIDITY, ACTIVITY, MORTALITY

William N. Evans
Timothy J. Moore

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ABSTRACT

We document a within-month mortality cycle where deaths decline before the 1st day of the month and then spike after the 1st. This cycle is present across a wide variety of causes and demographic groups. A similar cycle exists for a range of activities, suggesting the mortality cycle may be due to short-term variation in levels of activity. We provide evidence that the within-month activity cycle is generated by liquidity. Our results suggest a causal pathway whereby liquidity problems reduce activity, which in turn reduces mortality. These relationships help explain the pro-cyclic nature of mortality.

William N. Evans
Keough-Hesburgh Professor of Economics
Department of Economics and Econometrics
447 Flanner Hall
University of Notre Dame
Notre Dame, IN 46556
and NBER
wevans1@nd.edu

Timothy J. Moore
Department of Economics
University of Maryland
Tydings Hall
College Park, MD 20742
moore@econ.umd.edu

I. Introduction

Daily mortality counts fluctuate over the course of a calendar month. As has been documented by Phillips et al. (1999), daily mortality decreases to about one percent below the average in the week prior to the 1st day of the month, and then increases to almost one percent above the average in the first few days of the month. This within-month mortality cycle is particularly pronounced for homicides, suicides, and accidents. Phillips et al. (p.97) speculate that this cycle may be driven in part by substance abuse, since "money for purchasing drugs or alcohol tends to be available at the beginning of the month and is relatively less available (for people with low incomes) at the end of the month." Subsequent work has focused almost exclusively on the role that substance abuse plays in explaining the within-month cycle.

In this paper, we show that the within-month mortality cycle is a more general phenomenon than is currently understood. Although the peak-to-trough of the within-month cycle is large in percentage terms for substance abuse deaths, these deaths represent a small fraction of the total, and account for a minority of the overall pattern. Updating and extending the earlier work of Phillips et al., we document within-month mortality cycles 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. These patterns remain after controlling for special days that occur at a particular time in a month, such as New Year's Day and Independence Day.

The broad-based nature of the within-month mortality cycle leads us to examine whether these cyclic patterns are present for different activities. To that end, we obtained daily data on 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 food and non-food retail purchases. These data all show the same pattern, namely, that activity declines toward the end of the month and rebounds after the 1st of the month.

With wages and transfers frequently paid around the 1st, this set of results is consistent with research on the 'excess sensitivity' of consumption to the arrival of expected income payments. Although the life-cycle/permanent income hypothesis implies that predictable changes in income should have no effect on consumption once the income is actually received, many authors have demonstrated that consumption increases after income arrives (e.g. Wilcox, 1989; Shea, 1995; Parker, 1999; Souleles, 1999; Johnson et al., 2006). Our work is most similar to Stephens (2003), who found seniors consume more after receiving Social Security checks on the 3rd day of the month.

The concordance between the mortality and activity cycles leads us to conclude that an increase in activity leads to an increase in mortality. For some causes of death, this link is obvious: one cannot die in a traffic accident unless one is in traffic. As activity falls before and then spikes after the 1st of the month, it is natural for such causes to demonstrate the same pattern. While the link between activity and other causes of death is not as obvious, it is well-documented in the medical literature that, for example, heavy meals and exercising are among the triggers for heart attacks and strokes. In the same way, our results suggest a rise in activity after the 1st of the month is responsible for the rise in mortality.

We provide suggestive evidence that the rise in mortality is linked to changing liquidity over the month. First, we document that the peak-to-trough in mortality is greatest for those with low levels of education, a group that has been found to have liquidity problems. Second, we link liquidity to movements in consumption by showing there are smaller movements in activity and consumption over the month for groups we would expect to have less liquidity issues, namely, those in higher-income groups and those with more education. Third, of all the goods and activities we examine, the largest swing in consumption is for lottery tickets: a good that can only be purchased with cash in many states. Finally, we provide direct evidence that mortality increases in the short term after the receipt of income.

Much of the direct evidence for this last result is provided in a companion paper (Evans and Moore, 2009). In that work we consider five different situations in which we can identify when a group of decedents received an income payment, and in each case find that mortality increases immediately after income receipt. First, 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 declines just before paycheck receipt, and is highest the day after checks are received. Second, for those who enrolled in Social Security after April 1997, benefits are paid on either the second, third or fourth Wednesday of the month, depending on beneficiaries' birth dates. Among this group, mortality is highest on the days checks arrive. Third, the Alaska Permanent Fund pays residents of Alaska an annual dividend, and during the week that direct deposits are made, mortality among urban Alaskans increases by 13 percent. Fourth, during the week the 2001 tax rebate checks arrived, mortality among 25-64 year olds increased by 2.5 percent. Finally, counties with a large percentage of their population in the active military experience relatively large spikes in mortality among 17-64 year olds immediately after the 1st and the 15th of the month, the dates on which military personnel are paid.

In this paper, we expand on the last two tests to show how these findings are linked to liquidity. We find that the impact of the 2001 rebate checks on mortality was larger when checks arrived at the end of the month, when liquidity issues are most acute. We also describe the liquidity problems experienced by military personnel and focus on mortality in groups most likely to be military personnel. Among 17-39 year olds, we find that mortality in counties with a large military presence increases by 10 percent during the four days after mid-month paychecks arrive, while there is no change in mortality in other counties over the same period.

This examination of the broader relationship between liquidity, activity and mortality has implications for a growing literature on mortality over the business cycle. A voluminous literature with contributions from a variety of disciplines has established that health outcomes are much better among individuals with higher socioeconomic statuses. In contrast to this work is a more recent strand of literature documenting that mortality is pro-cyclic (Ruhm, 2000). What has been missing from this literature is an explanation for the pro-cyclicality of mortality that reconciles it with the protective role of income. The relatively short cycle of a month enables a distinction to be made between transitory income changes and permanent income levels. To see whether a possible explanation for the pro-cyclicality of mortality is changing activity levels, in the final section of the paper we show that the death categories with the greatest peak-to-trough within-month mortality cycle are also those categories most strongly tied to the business cycle. This suggests that rising mortality in a boom is a function of greater activity generated by a robust and healthy economy.

II. The Within-Month Mortality Cycle

In a 1999 paper in the *New England Journal of Medicine,* Phillips et al. use data on all deaths in the United States between 1973 and 1988 to identify a within-month mortality cycle.[1] Looking at the 14 days prior to the 1st of the month and the 14 days after, the authors find daily deaths decline as the 1st approaches and spike to above-average levels on the 1st of the month. This within-month mortality cycle is particularly pronounced for homicides, suicides, traffic accidents, and other external causes. These death categories are also more likely to be associated with substance abuse. With government transfers generally paid at the beginning of each month, the authors speculate that this cycle is primarily due to an interaction of liquidity constraints and the consumption of alcohol and drugs. They identify deaths whose primary or secondary cause of death is related to the use of alcohol or drugs other than tobacco, and find that these deaths are 14 percent higher during the first week of the month compared to the week before.

[1] Exact dates of death are only available on the 1973-1988 public use Multiple Cause of Death data files, which is why Phillips et al. (1999) restrict their analysis to those years.

The link between the transfer program payments and substance abuse – a phenomenon sometimes referred to as the ‘full wallets' hypothesis – has been documented by Verhuel et al. (1997), Rosenheck et al. (2000), Maynard and Cox (2000), Halpern and Mechem (2001), Swartz et al. (2003), Riddell and Riddell (2006), and Li et al. (2007). In the most detailed study to date, Dobkin and Puller (2007) use administrative records from California to show a within-month hospital admission cycle among Supplemental Security Income and Social Security Disability Insurance recipients, with the peaks particularly pronounced for substance abuse admissions. Dobkin and Puller also find a large within-month mortality cycle for in-hospital deaths among these groups, but no in-hospital cycle for those not on federal assistance programs. In related work, Foley (2008) finds a different monthly cycle for crimes motivated by financial gain, such as burglary, robbery and motor vehicle theft. In states where government transfers are primarily paid at the start of the month, these crimes increase in the last few days prior to the 1st of the month, and then decline after the 1st a pattern he attributes to the same lack of liquidity towards the end of the month.

Although the Phillips et al. (1999) also documents a within-month cycle for deaths not classified as due to substance abuse, none of the existing studies have considered an explanation outside the transfer payment/substance abuse nexus. To do so here, we first update the Phillips et al. analysis using data from 1973 to 2005. The within-month cycle is evident and of a similar magnitude in this expanded sample. Then, using broader criteria for classifying deaths as related to substance abuse, we demonstrate that a focus on substance abuse deaths is too narrow, and that the within-month mortality cycle encompasses many causes of death.

III. Replicating and Expanding the Basic Findings

a. Pooling Samples from 1973 -2005.

The primary data for this analysis are the Multiple Cause of Death (MCOD) data files compiled by the National Center for Health Statistics (NCHS). They contain a unique record of each death occurring in the United States, which includes information about the decedent's age, race, gender, place of residence, and cause of death.[2] Exact dates of death were reported on public use data files starting in 1973, but with the redesign of the public use layout in 1989, this information is now only available on restricted-use versions of the data.[3] Permission to use the restricted data was obtained from the NCHS. Combining the 1973-1988 public use files with the 1989-2005 restricted-use data provides us with information on over 71.5 million deaths.

A graph of all deaths for the entire 1973-2005 period is shown in Figure 1. The horizontal axis shows days in relation to the 1st of the month: *Day 1* is the 1st. To provide symmetry, we report the 14 days prior to the 1st and the first 14 days of the month, a total of 336 (12*28) days per year. The height of the graph represents the relative risk of death on a particular day, computed as the average deaths on a given day divided by the average deaths across all days. Thus, a value of 1.1 represents a 10 percent increase in the daily risk of death. The relative risk is represented by the hollow circles, while the vertical lines from the circles are 95 percent confidence intervals.[4]

The shape of the graph is similar to that in Phillips et al.[5] Starting about 12 days before the 1st of the month, daily deaths decline slowly and fall to 0.8 percent below average deaths the

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[2].Detailed information about the Multiple Cause of Death data files is available at the NCHS web site, http://www.cdc.gov/nchs/products/elec_prods/subject/mortmcd.htm .

[3] Available at the NCHS Research Data Center (NCHS/RDC), http://www.cdc.gov/nchs/r&d/rdc.htm.

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

[5] Using data from 1973-1988 only, we are able to replicate the basic results in Phillips et al. (1999).

day before the 1st. Deaths then increase on the 1st of the month to 0.6 percent above average. The peak-to-trough represents about a 1.4 percent difference in daily mortality rates. In 2005, there were roughly 6,700 deaths per day. As such, the current increase in deaths from the last day of the month to the 1st represents 94 deaths per month, or about 1,100 deaths per year.

This within-month mortality cycle remains once we control for a set of covariates in a regression similar in structure to that in Stephens (2003). Let Y_{dmy} be counts of deaths for day d in month m and year y. Days are organized in relation to the 1st of the month, so d goes from -14 to 14. Months do not follow the calendar; instead, they are the 28 days surrounding the 1st of the month. *Month 1* contains data from December 18 through January 14 of the next year, *Month 2* from January 18 through February 14, and so on. Given this structure for the data, the econometric model we estimate is of the form:

$$(1) \qquad \ln(Y_{dmy}) = \alpha + \sum_{\substack{d=-14 \\ d\neq -1}}^{14} Day(d)\beta_d + \sum_{j=1}^{6} Weekday(j)_{dmy}\gamma_j + \sum_{j=1}^{M} Special(j)_{dmy}\varphi_j + \mu_m + v_y + \varepsilon_{dmy}$$

Where *Day(d)* is a dummy variable equal to one if it is day d and zero otherwise, *Weekday(j)* is one of six dummy variables for the different weekdays, and *Special(j)* is one of J dummy variables for special days throughout the year.[6] The variables μ_m and v_y capture synthetic month and year effects, and ε_{dmy} is an idiosyncratic error term. The reference day is the day prior to the start of the month (i.e. *Day(-1)*), and the reference weekday is Saturday. We estimate standard errors allowing for arbitrary correlation within the 28 days of the synthetic month.

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[6] We include unique dummies for a list of reoccurring special days, including January 1st and 2nd, the Friday through Monday associated with 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 to Sunday of the week of Thanksgiving, a dummy for the days from the day after Thanksgiving to New Year's Eve, plus single day dummies for December 24th through December 31st. We also reduce the number of homicides on September 11, 2001 by 2,902 deaths, which according to a Center for Disease Control report was the number of deaths on that date due to the terrorist attacks http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm. In models of fatality counts for specific demographic groups, such adjustments are not possible so we add a dummy variable for September 11, 2001.

In the first two columns in Table 1, we report estimates for the *Day(d)* coefficients from a regression of the natural log of the fatality counts on the 27 dummy variables for the day of the synthetic month (-14, -13, etc.), and no other covariates. This estimate is an empirical analog to the graphical presentation in Figure 1. On the right side of Table 1 we generate estimates using equation (1), and find results do not change much once we add these covariates. Deaths on the first fourteen days of the month are approximately 1 percent higher than the day prior to the 1st of the month (*Day(-1)*). The main difference between the models is that the regression-adjusted coefficient on *Day(1)* is 20 percent lower than the unadjusted raw number. This is because New Year's Day is a high mortality day, with 17 percent more deaths than the daily average, and the New Year's Day effect is eliminated from the analysis when we control for special days.

To better understand the magnitude of the results in Table 1, we alter the model in equation (1) and estimate a model similar to those in Stephens (2003). Instead of including dummies for particular days, we estimate models with three weekly dummy variables: *Week(-2)* includes *Day(-14)* to *Day(-8)*, *Week(1)* includes *Day(1)* to *Day(7)*, and *Week(2)* includes *Day(8)* to *Day(14)*. The reference group is the week before the 1st of the month (*Week(-1)*).

Results for this model are reported in the top row of Table 2. Daily mortality is about 0.9 percent higher in the first week of the month than in the preceding week, and this result has a z-score of about 5. With 5,938 deaths per day in our sample, over a year the first week of the month will have about 4,324 (5,986*0.0086*7*12) more deaths than the last week of the month.

This relatively parsimonious specification can also be used to show that the mortality cycle is present for the fatality counts of a wide variety of demographic subgroups. The remaining rows of Table 2 contain the *Week(-2)*, *Week(1)* and *Week(2)* coefficients for groups divided by sex (male, female), race (white, black, other race), marital status (single, married, widowed, divorced), and age (under 18 years, 18 to 39 years, 40 to 64 years, over 65 years).[7] The results indicate the breadth of the phenomenon: all groups have a coefficient on *Week(1)* that is positive and statistically significant at conventional levels, with deaths at least 0.5 percent higher in the first week of the month compared to the previous week. The pattern is most pronounced for the non-white (i.e. black, other race) and working-age (i.e. 18 to 39 years, 40 to 64 years) subpopulations, as well as those who are single and divorced.

b. Does the Within-Month Cycle Extend Past Substance-Abuse Related Deaths?

We now examine how much of the within-month cycle can be explained by substance abuse. Each observation in the MCOD data has up to 20 causes of death that are coded according to the International Classification of Disease (ICD) codes. During our time period of analysis, the MCOD used three different versions of the ICD codes: ICD-8 (1973-78), ICD-9 (1979-98), and ICD-10 (1999-2005). In this section, we focus on the 20-year period when the ICD-9 coding system was in place, as the specificity of the codes used to identify substance abuse varies substantially across the three versions.

Given that our primary concern is to examine the mortality cycle for deaths unrelated to substance abuse, we err on the side of including too many deaths in the substance abuse category rather than too few. Phillips et al. define a death as substance abuse-related if it has a primary or secondary cause related to alcohol or drug use.[8] We expand this definition in two ways. First, we use a broader set of ICD-9 codes to identify substance abuse. In addition to the codes in Phillips et al., we also use conditions attributable to alcohol or drugs contained in studies on the

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[7] In a later section of the paper, we generate results by education level.

[8] They use the following ICD-9 codes: 291 (drug psychoses), 292 (alcohol psychoses), 303 (drug dependence), 304 (alcohol dependence), 305.0 and 305.2-305.9 (non-dependent abuse of drugs except for 305.1, tobacco abuse), 357.5 (alcoholic polyneuropathy), 425.5 (alcoholic cardiomyopathy), 535.3 (alcoholic gastritis) 571.0-571.3, (chronic liver disease and cirrhosis with mention of alcohol), 790.3 (excessive blood alcohol level), E860 (accidental poisoning by alcohol not elsewhere classified), 947.3 and E977.3 (alcohol-use deterrents), and 980 (toxic effect of alcohol).

economic costs of substance abuse in the United States (Harwood et al., 1998), Australia (Collins and Lapsley, 2002), and Canada (Single et al., 1999).[9] Second, a death is classified as a substance abuse death if these codes are listed as any of the 20 causes, rather than only as one of the first two. Given our broader definition of substance abuse, it is no surprise that we define a far higher proportion of deaths as related to substance abuse (4.4 percent) compared to Phillips et al. (1.7 percent).

Figure 2 contains the relative daily mortality rates for deaths related to substance abuse (in Panel A) and deaths not related to substance abuse (Panel B). There is a large peak-to-trough for substance abuse deaths. For the four days prior to the 1st of the month, deaths are about two percent below the daily average, before spiking on *Day(1)* to four percent above the daily average. Panel B contains the results for deaths not related to substance abuse. The magnitude of the within-month cycle for this sample is nearly identical to the graph for all deaths in Figure 1. The trough occurs on *Day(-1)* and the peak occurs on *Day(1)*, with a difference of more than one percent between the two. The cycle present in Figure 1 is not caused by substance abuse.

These patterns persist once we estimate a model using the natural log of fatality counts regressed on weekly dummies and the various controls contained in equation (1). The first row of Table 3 contains the coefficients on the weekly dummies for all deaths occurring between 1979 and 1998, with the reference period being *Week(-1)*. The results for this limited sample are virtually identical to those for the full sample from the first row of Table 2.

The results for substance abuse and non-substance abuse related deaths appear in the second and third rows of Table 3. Substance abuse deaths are 3.0 percent higher the first week of the month compared to the previous week, while for non-substance abuse related deaths this number is 0.77 percent. Notice, however, that there is an average of only 257 substance abuse

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

deaths per day, so a three percent increase means 647 more deaths per year in the first week of the month compared to the previous week. By comparison, deaths not related to substance abuse average 5,622 per day, so there are 3,636 more of these deaths per year in the first week of the month compared to the last. Therefore, although substance abuse deaths are more cyclic than other causes, they represent only 15 percent of the excess deaths associated with the within-month mortality cycle.

c. Disaggregating Deaths into Detailed Causes

The breadth of this phenomenon can also be seen in the within-month mortality patterns for different causes of death. We create 15 subgroups based on primary cause of death that are consistently defined across Versions 8, 9 and 10 of the International Classification of Disease.[10] There are four groups based on external causes (motor vehicle accidents, suicide, homicide, and all other external causes) and four cancer-related groups (breast cancer, leukemia, lung cancer, and other cancers). The remaining categories are heart attacks; heart diseases other than heart attack; chronic pulmonary obstructive disease (COPD); stroke; alcohol-related cirrhosis; cirrhosis not related to alcohol; and a category composed of deaths not included in the previous groups.

The monthly patterns for all of these categories are shown in Figure 3. Panel A to Panel D includes the relative daily mortality rates for the four external cause categories: motor vehicle accidents, suicides, murders, and all other external causes, such as accidents and drowning. All have a dip before the 1st of the month and a spike on the 1st. The number of suicides peaks again on the 5th of the month, and the other three cause-of-death categories peak on the 4th. The

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[10] Each ICD version has several thousand individual codes, but the changes from version to version mean only large death categories can be consistent throughout the sample. The exact mapping of deaths is provided in an appendix that is available from the authors.

magnitude of the peak-to-trough is 7 percentage points for motor vehicle accidents and suicide, 13 percentage points for murder, and 8 percentage points for other external causes.

External cause-of-death categories are clearly connected to the role of substance abuse. More interesting is that the within-month mortality cycle is present in a number of the other cause-of-death categories. Panel E shows the pattern for deaths in which the primary cause was a heart attack. These deaths increase by more than two percent from the day before the 1st of the month to the day after the 1st. Other heart diseases, shown in Panel F, display a similar pattern, although the peak-to-trough is of a slightly smaller magnitude (around one percent). The same pattern is observed for COPD (Panel G) and stroke (Panel H), with average differences between deaths on the 1st and last days of the month of 1.8 percent for COPD and 1.0 percent for stroke. In all cases, the ranges of the 95 percent confidence intervals are below the average in the last few days of the month, and above the average in the first few days of the month. While these patterns are not as stark as for external causes, they are evidence of a phenomenon that requires a more general reason than alcohol and drug use.

The pattern is slightly different for cirrhosis. Alcohol cirrhosis deaths (Panel I) are above the average daily rate between the 4th and the 14th of the month, peaking at four percent above the average on the 9th of the month. Non-alcohol cirrhosis deaths (Panel J) exhibit a similar pattern, increasing above the average on the 4th of the month and then peak about three percent above the average on the 8th of the month. As short-term changes in cirrhosis are influenced by changes in liver toxicity, the lagged nature of this pattern is consistent with substance abuse and, more generally, higher consumption early in the month (Cook and Tauchen, 1982).

Finally, Panels K to N contain deaths for different types of cancers. Breast cancer (Panel K) and leukemia (Panel L) deaths exhibit no discernible pattern over the course of a month. There is a slight dip below the average prior to the 1st for lung cancer deaths (Panel M), but there is an equivalent dip in the first few days of the month, which differs from the general pattern. A similar pattern occurs for other cancers (Panel N). Unclassified deaths (Panel O) show the same pattern as aggregate mortality.

The regression-adjusted pattern for these specific causes of death is investigated using the same equation (1) model used throughout this section. The week-of-month coefficients are shown in Table 4. Focusing on the *Week(1)* dummy, there are statistically significant increases in mortality during the first week for all causes of death except three cancer groups: lung cancer, breast cancer, and leukemia. We find a small within-month cycle for other cancers. The largest within-month cycles are (in descending order): suicides, homicides, COPD, alcohol cirrhosis, other forms of cirrhosis, and motor vehicle accidents. The percentages of each group's deaths defined as related to substance abuse are also shown in Table 4. Heart attacks, other forms of heart disease, stroke, COPD, and non-alcohol cirrhosis all display a within-month mortality cycle – and even using a broad definition for substance abuse, only 0.5 percent or less of the deaths in each of these categories are potentially related to substance abuse.

d. Results for Motor Vehicle Fatalities

One of the largest peak-to-troughs in the within-month mortality cycle is for mortality caused by traffic accidents. In this section, we use the National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS) to look at this death category in detail. Local law enforcement agencies are required by federal law to provide detailed data about motor vehicle accidents in which a death occurs within 30 days of the accident and therefore, FARS provides a census of deaths associated with motor vehicle crashes. FARS records information about the time, date and location of crashed, plus data on all vehicles, drivers, passengers, pedestrians and cyclists involved in the accident. FARS is superior to the MCOD data because it records the date when the event that leads to death occurred

Investigators are required to collect blood alcohol concentrations (BAC) of drivers, pedestrians and cyclists involved in fatal crashes. In the absence of a BAC level, law enforcement officers provide an estimate of whether the drivers were drinking. Combining these two indicators, we can generate estimates of the fraction of fatal accidents in which alcohol is involved. Between 1982 and 2004, 44.9 percent of accidents in the FARS have a driver, pedestrian or cyclist involved in the accident who had been drinking.[11] This fraction varies over the day, with alcohol involvement peaking at 82 percent for accidents between 2:00am and 3:00am, declining monotonically to 11 percent between 9:00am and 10:00am, and then increasing monotonically throughout the rest of the day.[12] If the within-month mortality cycle is driven primarily by changes in substance abuse and alcohol consumption at the end of the month, we should see stark differences in the within-month mortality cycle for accidents that occur at different times of the day.

In Table 5, we report results for a model similar to that in equation (1) where we estimate the determinants of the natural log of daily counts of motor vehicle fatalities from FARS over the 1975 to 2004, first for all fatalities and then for those occurring at different times of the day. This model is identical to the one used in Table 2, and the reference week is again *Week(-1)*. We allow for arbitrary correlation in errors across observations within a synthetic month.

In the first row of the table, we report results for all motor vehicle accidents. There is a noticeable drop in fatalities before the 1st of the month, with large and statistically significant coefficients on *Week(-2), Week(1),* and *Week(2)*. Mortality counts are 3.4 percent higher in the first week of the month compared to the previous week. Disaggregated results by time of day,

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[11] FARS documentation cautions users about the quality of the data in the early years, and most official reports about alcohol involvement from the National Highway Traffic Safety Administration use data from 1982. For more information about FARS, see http://www.nhtsa.dot.gov/people/ncsa/fars.html. FARS data is available for download at ftp://ftp.nhtsa.dot.gov/fars/.
[12] The use of illicit drugs is not measured in FARS data and the prevalence of "drugged driving" is not particularly well understood. However, officer judgments as to driver impairment may partly take into account the effects of illicit drug use.

shown in the remaining rows of Table 2, all show a within-month cycle. Notice that while the percent of accidents with alcohol involvement is highest from 12:00am to 6:00am (73.1 percent) and lowest from 6:00am to 10:00am (15.8 percent), there is little difference in the *Week(1)* coefficients, which are 3.5 percent and 2.8 percent, respectively. In fact, the largest peak-to-trough is for motor vehicle accidents that occur between 10:00am and 4:00pm, when only 18 percent of accidents involve alcohol. These results suggest that within-month changes in mortality are due to more general phenomena than changes in alcohol and drug use alone.

IV. Linking Mortality to Activity

We require a more general explanation of the within-month mortality cycle than changing levels of substance abuse. The causes of death that demonstrate the most cyclicality suggest that activity spurs on mortality, which means a drop in activity before the 1st of the month and the rise in activity after the 1st can explain the basic pattern of results.

For some causes of death, the link between activity and mortality is obvious. We have already noted that traffic accidents are a function of economic activity; many other external causes also largely occur outside of the home. Extensive empirical evidence suggests that an increase in activity temporarily raises the risks of other causes of death. Nowhere is this more evident than in the voluminous literature on the triggers for heart attacks. Strenuous exercise (Mittleman et al., 1993), sexual activity (Moller et al., 2001), eating a heavy meal (Lipovetsky et al., 2004), returning to work on Monday mornings (Willich et al., 1994), shoveling snow (Heppell et al., 1991), the Christmas season (Phillips et al., 2004), and watching your favorite soccer team lose (Witte et al., 2000) are all found to increase the incidence of heart attacks and/or deaths from heart attacks.

To provide some intuitive evidence of the link between activity and mortality, in Table 6 we report the coefficients on the weekday dummy variables and some of the special day dummy variables for the external causes originally reported in Table 4. The results are consistent for traffic accidents, murders and other external causes: deaths spike on Saturdays and Sundays, when more discretionary activity is involved. Likewise, deaths across these three categories spike on holidays associated with activity, such as New Year's Eve, New Year's Day, Holy Thursday, Good Friday, Memorial Day, July 4th, Labor Day, Christmas Eve and Christmas Day. Suicides follow a different pattern, peaking on Mondays and falling considerably on most holidays except New Year's Day.

Given the structure of the MCOD data, we are unable to directly link increased activity to mortality. We can show, however, that many consumer purchases and activities have the same within-month cycle as mortality. In this section, we use a variety of data sets to document within-month consumption and purchasing cycles, which we then use as a proxy for activity.

These results are consistent with tests of the life cycle/permanent income hypothesis in which authors have found that predictable changes in income *do* affect consumption, and are similar in form to studies using regular income payments. Stephens (2003) finds an increase in the consumption of time-sensitive purchases, like perishable food and eating at restaurants, among seniors after the receipt of Social Security checks. Using data for the United Kingdom, Stephens (2006) finds an increase in consumption after the receipt of paychecks. Among Food Stamp recipients, Shapiro (2005) finds a drop in daily caloric consumption of 10-15 percent over the food stamp month, a result he finds consistent with hyperbolic discounting. Likewise, Mastrobuoni and Weinberg (forthcoming) find food consumption declines between Social Security payments among seniors with a high fraction of income coming from Social Security, although families with more non-Social Security income smooth consumption over the month.

a. The Consumer Expenditure Survey

Following much of the previous work in this area, we initially examine the within-month consumption and purchase cycle using data from the Diary Survey component of the Consumer Expenditure Survey (CEX). The CEX is produced by the Bureau of Labor Statistics. It contains a quarterly Interview Survey designed to capture purchases of items purchased infrequently, and a Diary Survey to record purchases of less expensive and more frequently purchased items such as food, personal care items, and gasoline. The sampled unit for the Diary Survey is a consumer unit (CU), which is a household containing related family members. Beginning at different points in the month, each CU provides detailed information about purchases for a 14-day period, at the end of which detailed demographic data is collected from each member of the CU.

We use three CEX data files containing information on people who began their two-week diaries in 1996 to 2004. The first is the Consumer Unit Characteristics and Income File, which contains data about the household and household head. The second is the Member Characteristics Income File, which records the income of each CU member. The third is the Detailed Expenditure File. This lists each item's purchase date, price, and Universal Classification Code, which then enables items to be grouped into detailed product categories. We have data from 57,972 CUs and roughly 715,000 daily observations, or about 12 daily observations per CU.

We generate three aggregate product categories. The first is all food purchases, both those for home and away from home. The second is called non-food items, which includes alcohol, cigarettes, apparel, gasoline, entertainment, personal products, personal services, and over-the-counter drugs. The third is the sum of these two categories. We aggregate data into the same synthetic month categories as before (December 18th through January 14th is *Month 1*, etc.), and convert all expenditures into real December 2008 values. [13]

In Table 7, we report regression estimates in which we examine the determinants of daily household purchases for all the CUs in our sample. The key explanatory variables are three dummy variables, representing *Week(-2)*, *Week(1)*, and *Week(2)* within the synthetic month, with the week prior to the 1st of the month serving as the reference period. Covariates include complete sets of dummies for the householder's age, sex, race, marital status, and education. We also add descriptive information about the CU, including a complete set of controls for the region of the country, size of the urban area, family size, and reported income.[14] We also add dummy variables for the days of the week, plus synthetic month and year effects; and we allow for arbitrary correlation in errors within each CU.

Table 7 contains results for food, non-food items, and all items. All three purchase categories show similar results, with purchases prior to the 1st of the month being lower than purchases after the 1st. Food purchases during the first week of the month are 27 cents higher than the preceding week, an amount that is 1.8 percent of the sample mean. Non-food items show a statistically significant increase of 16 cents a month. The purchase of all items is 49 cents (1.7 percent of the sample mean) higher in the first week of the month than in the previous week. The magnitudes of these results are similar to the size of the peak-to-trough in the within-month mortality cycle. In Section V, we will generate results by various population subgroups and demonstrate tremendous heterogeneity in the within-month purchase cycle.

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[13] For synthetic *Month 1*, we use the January CPI, for synthetic *Month 2* (January 18th through February 14th) we use the February CPI, etc.

[14] Income is reported in nine groups. However, roughly 27 percent of the sample did not respond to the income question, so we added a separate dummy for income not reported.

b. Evidence from Other Consumer Products and Activities

In this section, we consider whether there is a within-month consumption cycle for some specific activities and products consumed upon purchase. We use models and data similar to those estimated for equation (1).

The first product we consider is the purchase of lottery tickets. Most states run lotteries with "daily number" games, where contestants pay $1 to pick a three or four digit number and win $500 or $5000, respectively, if their number is selected. These daily games provide high-frequency outcomes, and we were able to obtain data on the daily tickets purchased for Pick 3 and Pick 4 games in two states: Maryland and Ohio. Lottery ticket purchases are an interesting product line to consider because many credit card issuers prohibit the purchase of tickets by credit cards. In some states, including Maryland, retailers are prohibited from accepting credit card payments for lottery ticket purchases. Therefore, for most lottery transactions, consumers must use cash. If liquidity is an issue for consumers near the 1st of the month, then the within-month cycle for lottery tickets should be particularly large.

Maryland and Ohio have twice-daily Pick 3 and Pick 4 games, although Ohio has no drawings on Sunday and Maryland only had a single Sunday drawing prior to May 23, 2004. We obtained daily ticket sales for the Pick 3 and Pick 4 games in Maryland from January 1, 2003 to the end of 2006, and for Ohio from June 20, 2005 through June 16, 2007.

We use both datasets to estimate models identical to those reported in Table 4. The outcome of interest is the natural log of daily sales, and we control for artificial month and year effects, weekday effects, and dummies for the list of special days contained in footnote 6. In the Maryland specification, we include a dummy that equals one for Sundays starting on May 23, 2004, to account for the extra draw on that day.[15] We allow for arbitrary correlation in the errors within each artificial month-year cell.

The results from these models are reported in the first two rows of Table 8. The Maryland and Ohio lotteries both have a pronounced within-month purchase cycle: ticket purchases in the first week of the month are 7.1 percent and 8.8 percent higher compared to the previous week, respectively. Both of these results are statistically significant.

A nationwide consulting firm for the retail trade sector that conducts a large daily survey of retail establishments and malls[16] provided us with data on average daily foot traffic through malls (from 1/1/2000 through 12/22/2007), all retail establishments (from 1/4/2004 through 12/22/2007) and apparel establishments (1/4/2004 through 12/22/2007). The outcome of interest is the natural log of foot traffic through the establishments, and the approach is the same except that we omit Christmas Day as traffic on that day is substantially smaller than for the rest of the year. The results are also reported in Table 8. For malls, all retail outlets and apparel stores, foot traffic is estimated to be 3.8, 5.7 and 5.8 percent higher during the first seven days of the month compared to the previous week. These data show a pronounced within-month activity cycle.

We obtained data on daily box office receipts for the top ten grossing movies within a one-week period (Friday through Thursday) from www.boxofficemojo.com for January 1, 1998 through June 7, 2007. With this data, we use the natural log of the box office receipts as the outcome of interest. The covariates in the model are identical to the ones used in the previous model with one exception. Because new movies are usually released on Fridays, and the top 10 movies can change dramatically from one week to the next, we define a week as a Friday to a Thursday period and add a dummy variable for each unique week in the data. The results for

[15] http://www.mdlottery.com/resources/retailersreport.pdf

[16] As per our user agreement, we cannot identify the producers of the data.

movies are reported in the sixth row of Table 8 and in this case, we see that the first week of the month generates 5.6 percent more in revenues than the previous week.[17]

We did not find a within-month cycle for two activities for which we obtained daily data. First, we used data on daily attendance at major league baseball games for the 1973-98 and 2000-04 seasons[18] from www.retrosheet.org/schedule/index.html. The unit of observation is a game at a particular stadium and the dependent variable is log attendance. We control for standard covariates including dummies for opening and closing day of the season, a dummy for whether it was before Memorial Day or after Labor Day, indicators for double headers, dummies for whether it was a day or night game interacted with weekday dummies, plus dummies for the team pair at a given stadium in a year.[19] We find no within-month cycle in baseball attendance.

Second, we obtained DC Metro subway ridership figures from January 1, 1997 to September 19, 2007. The outcome of interest is log ridership and the extra controls are dummies for Redskin home games, days the Cherry Blossom festival is in effect, and five dummies for exceptionally large crowds on the mall such as for the Million Man March, etc. The results for this model, presented in the last row of Table 8, show no within-month mortality cycle.

V. Is Liquidity Responsible for these Within-Month Cycles?

The previous two sections show within-month mortality and consumption cycles that are similar in nature. There is suggestive evidence that these cycles may be due to liquidity, such as the fact that the within-month cycle is greatest for those we would expect to have more liquidity issues (younger people, females, minorities, divorcees). The most striking evidence is that the

[17] The difference between unadjusted (i.e. raw data) and regression-adjusted results is largest for this outcome. The single biggest movie going week of the year is Christmas Eve to New Year's Eve. Over this period, average daily gross of the top 10 movies is more than twice the average during the rest of the year. Therefore, a plot of average daily gross by days in relation to the 1st of the month would show a tremendous spike in attendance before the 1st of the month. However, controlling for the special days throughout the year eliminates the Christmas effect on movies.

[18] There was no attendance data for the 1999 season on the web site.

[19] For example, there were separate season dummies for each Red Sox/Yankees game at Fenway.

one good that must be purchased with cash, lottery tickets, shows the largest peak to trough. In this section, we provide further evidence suggesting that liquidity problems at the end of the month are responsible for the within-month cycles.

The 1st of the month is a focal point of economic activities for many households. In the 1996-2004 CEX sample used above, about ten percent of respondents who receive a paycheck do so monthly, and we suspect a large fraction are paid on or near the 1st of the month. Furthermore, during much of the period of the analysis in this paper, most federal transfer programs distributed checks on or near the 1st of the month. Social Security recipients claiming benefits prior to April of 1997 receive checks on the 3rd of each month, while Supplemental Security Income benefits are paid on the 1st of the month.[20] In an email survey of state Temporary Assistance for Needy Family programs, we found that 30 of 41 states that responded distribute checks during the first week of the month.

Likewise, many families have periodic bills that are due on or near the 1st of the month. In our CEX samples, half of all households who made a mortgage or rent payment during their 14-day survey period did so somewhere between the day before the 1st of the month and the first week of the month, with 14 percent of the sample paying on the 1st of the month. Since most rent and mortgage payments must be paid by check or cash, uncertainty about whether there will be enough in the bank at the start of the month may force some to ratchet down spending until after these bills are paid.

In this section, we provide more direct evidence that liquidity issues play a role in these within-month cycles. First, we show that the groups we would expect to have liquidity issues at the turn of the month are precisely those groups with the greatest peak-to-trough in the within-month consumption and mortality cycles. We show the consumption cycle is large for a variety

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[20] If the 1st or the 3rd fall on a Saturday, Sunday, or public holiday, then the payment is made on the closest prior business day.

of liquidity-constrained CEX respondents: those with Social Security income; on federal disability insurance programs; with low income; and with low education. Similarly, we show that the mortality cycle is largest for those with lower levels of education. We also provide evidence that the receipt of income leads to an increase in mortality in the short run, by summarizing the results in Evans and Moore (2009) and expanding on two direct tests of the hypothesis. We first analyze changes in mortality after the receipt of the 2001 tax rebate checks, and then examine changes in mortality after the receipt of paychecks for a group of workers with demonstrated liquidity problems: the active-duty military.

a. Heterogeneity in the Within-Month Consumption Cycle

In this section, we show that those expected to face the greatest liquidity issues at the end of the month have the largest within-month purchase cycle. We do this by grouping CEX respondents based on, in turn, their educational attainment, whether they receive government transfers, and their income level. The data and approach is the same as was used previously.

First, the CEX sample is divided into three groups based on the education of the head of household: 1) those with less than a high school education; 2) those with a high school education and/or some college; and 3) those with a college degree or more. The results are presented in the first section of Table 9. Expenditure on food by the least-educated households falls considerably before the 1st of the month, while non-food expenditures do not change as dramatically. The coefficient on *Week(1)* for food items is a statistically significant 99 cents, or 8.0 percent of the sample mean. Among CUs with a high school educated head, there are statistically significant within-month purchase cycles in all three expenditure categories. In the all items category, the coefficient on the *Week(1)* dummy is $1.14, or about four percent of the sample mean for daily spending. Finally, for the most educated group, we find no evidence of a within-month cycle for food purchases and some evidence of a cycle for non-food items, although the estimates are statistically insignificant. The *Week(1)* coefficient for total purchases by this group is a statistically insignificant 28 cents, which is less than one percent of the sample mean.

The next group of results, also presented in Table 9, is for three groups based on their receipt of government income. The first group consists of households with any federal or state income assistance other than Social Security. The bulk of these families will be receiving income from either the Temporary Assistance for Needy Families (TANF) or the Supplemental Security Income (SSI) programs. SSI payments are made on the 1st of the month, and an email survey of 41 state TANF offices we conducted indicates that 30 states pay TANF payments in the first week of the month. There is a substantial within-month cycle for this group, with food purchases \$2.87 higher (21 percent of the sample mean) and total purchases \$3.76 (16 percent of the sample mean) during the 1st week of the month compared to the previous week. The *Week(1)* coefficient on non-food consumption is also positive, but not statistically significant.

The second group consists of households receiving Social Security but no other government income. This is similar to the sample used in Stephens (2003), although his 1986-96 sample are all paid on the 3rd of the month, while our 1996-2004 sample contains some Social Security recipients being paid at other times of the month.[21] As the results in Table 9 indicate, we find positive and statistically significant *Week(1)* coefficients for these households' purchases of food items (73 cents), non-food items (53 cents) and all items (123 cents), which represent about five percent of the daily mean in each category.

The third group in this block of results is a sample of households with neither Social Security income nor income from other federal or state transfer programs. This set of estimates provides no evidence of a within-month purchase cycle.

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[21] As discussed already, those claiming Social Security prior to May 1997 are paid on the 3rd of the month, while newer beneficiaries are paid on the second, third or fourth Wednesday of the month depending, respectively, on whether the birth date is on the 1st-10th, 11th -20th, or 21st-31st. http://www.socialsecurity.gov/pubs/calendar.htm.

Finally, we create sub-samples based on household income by dividing the CEX sample into households with incomes of less than $30,000, households with incomes of $30,000 and more, and households that do not report income. Among low income households, we find a statistically significant coefficient on the *Week(1)* dummy for all three spending categories. In the total purchases model, for example, the coefficient of 84 cents is about four percent of the sample mean. We find similar results for households not reporting income, which is not surprising as the average education of the reference person in these households is close to the education of the reference person in the low income group. Among families with an income of $30,000 or more, we actually find a negative and statistically significant coefficient on the *Week(1)* dummy variable for food and total purchases, perhaps because our covariates do not fully control for these households purchases around Thanksgiving and Christmas.

These results suggest liquidity drives the consumption cycle. Households receiving government transfers or with low income or education display such a cycle, while the purchases of high income and educated households do not. The results may be consistent with a hyperbolic discounting as suggested by Shapiro (2005) and Mastrobuoni and Weinberg (forthcoming).

b. Mortality Results by Education Levels

In this section, we examine the heterogeneity in the within-month mortality cycle based on the education of the deceased. Since 1989, the MCOD file has included the decedent's education, which is usually provided by the next of kin.[22] Educational attainment is strongly and positively correlated with households' wealth and financial savings (Juster et al., 1999), so education should provide a proxy for those with and without liquidity constraints.

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[22] In 1989, 21 states reported an education for at least 90 percent of decedents. This number rises to 42 states by 1995 and 48 states by 2005. Sorlie and Johnson (1996) assessed the accuracy of education listed on death certificates, and found that when certificates are matched to survey data obtained prior to death, the former match the latter in about 70 percent of cases. When they differ, the death certificate data overstates reported education.

We group decedents into three categories: those whose highest education is less than high school completion, those who completed high school but not college, and those who completed college.[23] The relative mortality risks for these three groups are shown in Figure 4. There is a large within-month cycle for decedents with less than high school education, shown in Panel A. Deaths move from 0.9 percent below the average on the day before the 1st to 0.5 percent above average on the 1st, before peaking at 0.8 percent above the average on the 5th of the month. The within-month mortality pattern for high school graduates (in Panel B) is similar: mortality is about 0.5 percent below the daily average in the last few days of the month, before increasing to about one percent above the average on the 1st of the month. This cycle in the daily relative mortality risk disappears for the college-educated decedents (in Panel C), and point estimates are neither consistently below the daily average in the last few days of the month, nor above the average in the first few days of the month.[24]

The results from regressions with week-of-month dummies for these three education-based groups are show in Table 10. Once special days and other controls are introduced in a regression framework, a within-month cycle is present for all three education groups. With *Week(-1)* again the reference week, the largest change from *Week(-1)* to *Week(1)* is for those who did not complete high school (1.0 percent), followed by high school completers (0.93 percent) and those with a college education (0.45 percent). The *Week(2)* coefficients display the same pattern; they are higher for high school non-completers (0.93 percent) than high school completers (0.72 percent) and college-educated decedents (0.23 percent). This last coefficient is

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[23] Between 1989 and 2002, the number of years of schooling rather than education outcomes is recorded in the MCOD file. Decedents were classed as having less than a high school education is they reported three or fewer years of high school; having a high school education if they completed four years of high school but fewer than four years of college; and having completed college if they had four or more years of college education.

[24] An adjustment is made for the September 11 terrorist attacks as there are nearly four times as many college-educated decedents (2,643) on September 11, 2001, as on the previous day (682), and this large change could distort the analysis. We remove this difference from the *Day 11* aggregate count. This event does not affect the other two education groups a lot, so they are not adjusted (high school completers have 26 percent higher mortality on September 11, 2001 than the previous day, while non-completers' fatality counts appear largely unaffected).

the only *Week(1)* or *Week(2)* coefficient that is not statistically significant at conventional levels, while none of the *Week(-2)* coefficients are. These mortality patterns are consistent with changing liquidity over the month affecting the relative mortality risks different groups face.

c. The Short-Term Mortality Consequences of Income Receipt

The evidence in the first two parts of this section is primarily circumstantial with regard to our liquidity/activity/mortality hypothesis. Here we provide evidence that income receipt results in a short-term increase in mortality, before connecting two types of income receipt to within-month movements in mortality in the remaining parts of Section IV.

A key implication of the life-cycle/permanent income hypothesis (LC/PIH) is that predictable and certain changes in income should have no effect on consumption once they occur. Over the past 15 years authors have used high-frequency survey data on consumption to test this prediction, with the bulk of this work finding that consumption behavior displays "excess sensitivity" to expected changes in income. Using various versions of the MCOD data, Evans and Moore (2009) return to many of the tests considered by the LC/PIH literature and examine whether there is a corresponding increase in mortality once income is received. In their work, they considered three existing tests of the LC/PIH and two additional quasi-experiments.

The first two tests exploit the fact that over 90 percent of people aged 65 or over receive Social Security income, and that age and dates of birth are provided in MCOD data files. First, Evans and Moore follow Stephens (2003) by examining seniors who enrolled in Social Security prior to May 1997. These recipients typically received their Social Security checks on the 3rd of the month. For this group, mortality declines just before Social Security receipt and is highest the day after payment. Second, in an extension of that test, they use date of birth information to identify when seniors enrolling after April 1997 are paid on the new schedule (already described in footnote 21). Among this group, too, mortality is highest on the days checks arrive.

The third test in Evans and Moore follows Hsieh's (2003) use of Alaska Permanent Fund dividend payments. They find that in the week that direct deposits of Permanent Fund dividends are made, mortality among urban Alaskans increases by 13 percent. Fourth, following Johnson et al. (2006), Evans and Moore demonstrate that during the week the 2001 tax rebate checks arrived, mortality among 25 to 64 year olds increased by 2.5 percent. Finally, Evans and Moore consider active duty military wage payments made on the 1st and 15th of the month. Among 17 to 64 year olds in counties with a large military presence, they find that mortality increases by nearly 12 percent the day after mid-month paychecks arrive, while over the same period, there is no change in mortality in counties with little military presence.

These five cases show direct evidence of a short-term increase in mortality after the receipt of income, providing a stronger link between liquidity and mortality. In the next two sections, we delve into two of these cases in more detail to further consider the role of liquidity. In part (d) we return to the case of the 2001 tax cuts and exploit the fact that stimulus checks were distributed at different times of the month. We show that the mortality impact of receiving income near the end of the month is substantially larger than at other times in the month. Finally, in part (e) we consider the special case of military personnel. These workers have a documented history of liquidity problems, and we show that there is an incredibly large increase in mortality after paycheck receipt for this group, especially among younger enlisted men.

d. The 2001 Tax Stimulus Checks

The *Economic Growth and Tax Relief Reconciliation Act* (PL107-16), signed into law on June 7, 2001, was a sweeping tax bill that lowered individual and capital gains tax rates, increased the child tax credit, and made changes to estate and gift taxes. The portion of the Act we consider is the reduction in the tax rate in the lowest income bracket from 15 percent to 10 percent. This tax change was applied retroactively to all income earned in 2001 and, as an advance payment on the tax cuts, households with taxable income in 2000 were sent rebate checks between June and September of 2001. The maximum rebates for single and married taxpayers were $300 and $600, respectively. Johnson et al. (2006) estimate households received about $500 on average, or about one percent of median annual family income. Approximately two-thirds of all households received a rebate check, and rebate payments totaled $38 billion.

Rebate checks were mailed on ten successive Mondays, and check distribution dates were based on the second-to-last digit of the Social Security number (SSN) of the person filing taxes.[25] The first checks were sent to taxpayers whose second-to-last SSN digit was a zero on Monday, July 23, and the last checks were sent on Monday, September 24 to taxpayers whose second-to-last digit was a nine.[26] The last three digits of the SSN are given sequentially at local Social Security offices and are therefore effectively randomly assigned. Johnson et al. (2006) exploit this fact using data from a special module in the CEX to show that consumption of nondurable goods increased in the months after the rebate was paid. Agarwal et al. (2007) perform similar tests using administrative data on credit card charges.

We use a similar approach to examine the short-run consequences of the rebates on mortality. This is possible because the NCHS merged the second-to-last digit of a decedent's SSN from the National Death Index (NDI)[27] to the 2001 MCOD data files at our request. We initially report the basic findings of Evans and Moore (2009), before showing that these rebates affect mortality in a manner consistent with an underlying connection between a within-month liquidity cycle and the within-month mortality cycle.

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[25] For married taxpayers filing jointly, the first Social Security number on the return determined mailing date.
[26] The other checks were sent on the following dates (second-to-last digit of SSN): July 30 (1), August 6 (2), August 13 (3), August 20 (4), August 27 (5), September 3 (6), September 10 (7), September 17 (8).
[27] 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.

Given that we have variation across groups in the timing of income payments from the 2001 rebates, the econometric model we use is a difference-in-differences specification. The outcome of interest is the natural log of mortality counts Y_{it}, where i indexes groups of people based on the second-to-last digit of their SSN (i=1 to 9), and t indexes one of 30 seven-day periods which begin ten weeks prior to the first check being distributed and end ten weeks after the last check was sent. The estimating equation is of the form:

$$(2) \qquad \ln(Y_{it}) = \alpha + REBATE_{it}\beta_1 + \eta_i + \nu_t + \varepsilon_{it}$$

where $REBATE_{it}$ is a dummy variable that equals one in the week that group i's rebate checks arrive. The parameter β therefore measures the percentage change in weekly mortality associated with rebate check receipt. The fixed effect η_i captures persistent differences in mortality across groups; however, no such differences are expected because of the random assignation of the second-to-last digit of a SSN. The fixed effect υ_t captures differences in weekly mortality counts that are common to all groups but vary across weeks. The September 11 terrorist attacks occurred during Week 18 in our analysis, and the deaths for that week are about twenty percent above the average.[28] The week effects will capture these changes so long as the deaths associated with September 11 are equally distributed across the 10 SSN groups. The remaining variable in the model is ε_{it}, which is a random error term.

A key to the analysis is to reduce the sample to people with taxable income in 2000, as they were the only ones to receive a tax rebate. The IPUMS-CPS project (King et al., 2004) has attached estimates of taxable income to March Current Population Survey (CPS) data. These data from the 2001 March CPS (2000 tax year) suggest that 52 percent of people aged 25 to 64 were in households that paid federal income taxes, while the comparable number for people aged 65 and older was 26 percent. Therefore, we restrict our attention to people aged 25 to 64.

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

Even with this restriction, the sample includes many non-taxpayers. It also includes couples who filed their taxes jointly but who were not listed first on the IRS 1040 form, as their household's check was mailed according to their spouse's SSN rather than their own. The IRS 1040 form does not record the sex of the taxpayers, so we cannot ascertain whether husbands or wives are more likely to be listed as the first taxpayer. As both non-taxpayers and the second person listed on joint tax returns should be randomly distributed across the different groups, our results should be systematically biased towards zero. The parameter β_1 does not measure the impact of check receipt, but rather the intention to treat with a check.

The results for equation (2) are reported in the first column of Table 11. The same values are also reported in Table 6 of Evans and Moore (2009). There is a statistically significant 2.7 percent increase in mortality for adults aged 25 to 64 the week rebate checks arrive. We cannot reject the null hypothesis that the group fixed effects are all zero, which provides support for the conjecture that the last two digits of the SSN are randomly assigned. Overall, the results suggest a large short-term increase in mortality immediately after income receipt.

We use the IPUMS-CPS data to vary the sample by the fraction of individuals likely to have been 'treated' by a tax rebate. It is not clear *a priori* how the estimates should change. A higher fraction of taxpayers means more treated people, but it also means a larger fraction of people with higher incomes, who would be expected to have fewer liquidity problems. Single males aged 25 to 64 are a subsample that does not include people who received rebates based on their spouse's SSN, and it contains a high fraction of people who paid taxes in the previous year (in excess of 75 percent). Results for this 'high income, high treatment' group are presented in column (2). There is a large and statistically significant short-run mortality effect of 4.7 percent. At the opposite end of the spectrum, we estimate the model using a sample of seniors aged 65 and older, a group with a low fraction of people who received a tax rebate (about one quarter).

Results for this group are reported in column (3); we find no impact of the rebate on mortality among seniors.

We postulate that a lack of liquidity at the end of the month leads to a decline in mortality, before liquidity and mortality increase on the 1st of the month. If so, rebate checks arriving towards the end of the month will relieve liquidity to a much greater degree than those arriving at other times, and should have a commensurately greater effect on mortality.

To see if this is the case, we compare how mortality changed on the three occasions that checks arrived in the last week of the calendar month to the other seven weeks in the rebate payment period.[29] In column (4) of Table 11 we estimate the same model as in column (1), except that we allow the coefficient on $REBATE_{it}$ to vary based on whether the check was received during the last week of the month or at some other time. The effect of receiving a check at the end of the month is large, with mortality increasing by a statistically significant 5.2 percent. This is in contrast to a 1.6 percent increase (t-statistic of 1.37) at other times of the month. There is a p-value of 0.11 on the null hypothesis that both coefficients are equal. The results fit with our prediction that households are liquidity-constrained towards the end of the month, and that this constraint affects their short-term mortality risks.

e. The Military Payment Schedule

The military is of interest here for several reasons. First, military personnel are paid on the 1st and the 15th of each month,[30] on a schedule that has been in place for decades.[31] Second, military personnel normally reside on or near the base to which they are attached, so we can

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[29] These weeks begin on the following Mondays: July 30, August 27, and September 24, 2001.
[30] When these dates fall on a weekend or a public holiday, wages are paid on the previous business day. The public holidays that alter payments here are New Year's Day, Presidents Day, Labor Day and Martin Luther King Day (since 1986).
[31] 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.

identify the effects of the military payments in the MCOD data by focusing on residents from counties with a high military presence. Third, the size of the military was relatively large from the mid 1970s to the early 1990s, so this analysis can be conducted with public-use MCOD data for the 1973-1988 period.[32] Fourth, military personnel have well-documented financial issues, making them a group for which income payments are likely to resolve immediate liquidity problems.

The financial plight of enlisted personnel has been the subject of many academic articles and popular press accounts. Buddin and Do (2002) surveyed enlisted military personnel in the mid-1990s and found 16.5 percent had pressure to pay bills, 9.5 percent had utilities shut off, 9.1 percent pawned or sold valuable items, 7.1 percent fell behind in rent or mortgage, 3 percent had bill collectors contact their unit, and over one-quarter reported at least one of these problems. While the active military are overwhelmingly male, younger and less educated than the average adult, Buddin and Do found that enlisted personnel had a much higher probability of financial problems than civilians even after controlling for these demographic differences. There are estimates that one-fifth to one-quarter of military personnel have used payday lending services (Stegman, 2007; Carrell and Zinman, 2008), a rate that is three times the national average (Stegman, 2007).[33]

We compare the mortality patterns in counties with a large number of residents who are active military personnel with non-military counties. While the widespread nature of the within-

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[32] Between 1973 and 1990 there were anywhere from 2.04 to 2.25 million military personnel in the US, before falling to 1.38 million in 2001 and then increasing slightly thereafter. Authors' calculations from various issues of the *Statistical Abstract of the United States*.

[33] The availability of payday lending is an interesting source of variation we do not exploit in this paper. Payday lending institutions are prevalent and current estimates suggest there are more payday lending outlets in America than McDonalds and Starbucks stores combined (Carrell and Zinman, 2008). Given the heavy concentration of military personnel on bases, supply considerations encourage locating outlets near military bases. Payday lending may be occasional useful for smoothing consumption over the month, but within the military, there are high serial borrowing rates. Combined with the high user fees associated with payday lending, such borrowing practices may accentuate the monthly cycle. However, payday lending was a much smaller industry in 1988, the year our analysis ends. Carrell and Zinman (2008) note that there were 'very few' payday lending outlets in the early 1990s.

month mortality cycle may mean all 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 other counties because of the small fraction of workers who are paid monthly (10 percent as we mentioned above) or twice monthly (5.5 percent).

We construct a sample for analysis by using the 1970, 1980, and 1990 Census Summary File 3 data set[34] to identify counties with more than 15 percent of their population aged 17 to 64[35] in the military during all three Censuses.[36] There are 21 counties that meet this criterion.[37] In 1990, there were 326,000 people aged 17 to 64 in these "military" counties, of which about one quarter were in the military. As military personnel have a large number of dependents, and bases typically employ civilians paid on the same schedule,[38] the fraction of the population in these areas affected by the military payment schedule will be much higher than 25 percent. We compare the mortality patterns for residents of these military counties with a sample of people from 2,772 "nonmilitary" counties, each with under one percent of their population aged 17 to 64 employed as military personnel. We then construct daily mortality counts amongst 17 to 39 year olds for these two groups for the 1973-1988 period. Those aged 17 to 39 represent 92.3 percent of military personnel in the 1980 5-percent Census PUMS (Ruggles et al., 2009).

To test whether military and nonmilitary counties exhibit different mortality patterns around the 1st and 15th of the month, we estimate a model similar to equation (1), but – because daily mortality counts in the military counties are small and occasionally zero – we use a

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[34] These data are from the National Historical Geographic Information System, Minnesota Population Center, 2004.
[35] Enlistment in the military can occur at age 17 with parental consent, and at age 18 without.
[36] Counties that changed boundaries between 1970 and 1990 were merged prior to this exercise (changes are at http://wonder.cdc.gov/WONDER/help/Census1970-2000.HTML). There were many changes to Alaska's county-equivalent geographic boundaries over this period, so we did not use Alaskan deaths in this analysis.
[37] 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), and WA (Island).
[38] 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.

negative binomial model and estimate it by maximum likelihood (Hausman, Hall and Griliches, 1984). Let Y_{idmy} be daily counts for group i (for military and nonmilitary counties) on day d, synthetic month m, and year y, where the synthetic months here contain the seven days before and after the two payments, and start a week before the first payment for the month.[39] Let X_{idmy} be a vector that captures the exogenous variables in equation (1). Within the negative binomial model, $E[Y_{idmy} \mid X_{idmy}] = \delta\exp(X_{idmy}\beta)$, where δ is a parameter that captures whether the data exhibits over-dispersion.[40] By definition, $\partial \ln E[Y_{idmy} \mid X_{idmy}]/\partial X_{idmy} = \beta$ so the parameters in this model are interpreted similarly to those in equation (1).

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

$$
\begin{aligned}
(3) \qquad X_{idmy}\beta = {} & \beta_0 + \sum_{j=1}^{6} Weekday(j)_{dmy}\gamma_j + \sum_{j=1}^{M} Special(j)_{dmy}\varphi_j + \\
& \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Military_{idmy} PP1_{idmy} Payday(d)\beta_{1md} + \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Military_{idmy} PP2_{idmy} Payday(d)\beta_{2md} + \\
& \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Nonmilitary_{idmy} PP1_{idmy} Payday(d)\beta_{1nd} + + \sum_{\substack{d=-7 \\ d\neq -1}}^{7} Nonmilitary_{idmy} PP2_{idmy} Payday(d)\beta_{2nd} + \\
& PP1_{idmy}\beta_p + Military_{idmy}\beta_m + (PP1_{idmy})(Military_{idmy})\beta_m + \mu_m + v_y
\end{aligned}
$$

where *Weekday, Special,* and the fixed month and year effects are defined as before. We control for differences across groups with a dummy for counts in military areas (*Military*); we control across pay periods with a dummy for the first pay period (*PP1*); and we also interact these two variables. The variables *Payday* are a series of 13 dummy variables defined for the seven days before and seven days after wage payments, except for *Payday(-1)*, which is the reference day and the day before checks are distributed. We add *Nonmilitary* and *PP2* dummies, and estimate four vectors of coefficients on the payday variables: two for military and nonmilitary counties

[39] 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 President's 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 happens in 1982 and 1988.

[40] It can be demonstrated that the variance to mean ratio in this model is δ +1. When δ>0, the variance grows faster than the mean and the data exhibit over-dispersion, and when δ=0 the negative binomial collapses to a Poisson model which by construction restricts the variance to equal the mean.

around the first pay period of the month (β_{1md} and β_{1nd}, respectively), and then similar values for the second pay period (β_{2md} and β_{2nd}). We examine whether the daily mortality patterns differ across the two groups by testing the null hypothesis H_o: $\beta_{jnd} = \beta_{jmd}$ for all *Payday(d)*. Standard errors allow for arbitrary correlation across observations within the same 28-day synthetic month.

The maximum likelihood results for the negative binomial model are reported in Table 11. Columns (1) and (2) present the coefficients on the first period payday dummies for military counties and nonmilitary counties. Column (3) reports the p-value on the -2 log-likelihood test statistic for the null hypothesis that military and non-military coefficients for a particular day are equal. Columns (4) through (6) show the same results for the payday near the 15th of the month.

The results in columns (1) through (3) of Table 11 indicate that deaths are lowest in both sets of counties the day before paychecks arrive, and highest the day after. Deaths are 10.6 percent higher in military counties the day after checks arrive than the day before, and are 3.9 percent higher on the same day in nonmilitary counties. Both results have a p-value less than 0.05. Despite the differences between the size of the payday coefficients in military and nonmilitary counties, in no case can we reject the null that the differences are zero.

The differences are clearer in the second pay period. Mortality is 10.6 percent higher in military counties the day checks arrive compared to the day before (p-value of 0.05). The corresponding numbers for *Payday(2)* and *Payday(3)* are 8.8 percent (p-value of 0.099) and 8.5 percent (p-value of 0.11), respectively. In contrast, in nonmilitary counties, the coefficients on these same three dummy variables are less than six-tenths of a percent in absolute value. For *Payday(1)*, we can reject the null that the coefficients are the same across military and nonmilitary counties at the 0.05 level, while the p-value for this test on *Payday(2)* and *Payday(3)* is 0.11 and 0.13, respectively.

For both pay periods, we replace the daily dummies with weeklong *Payweek* dummy variables that cover paydays and the six days that follow them, so that the coefficients represent the difference between the week after payment and the week before. For this model, we obtain the following estimates (standard errors): for the first paycheck, *Payweek(1)* is 0.0237 (0.0210) in military counties and 0.0145 (0.0038) in non-military counties; for the second paycheck, *Payweek(1)* is 0.0489 (0.0205) in military and 0.0049 (0.0032) in non-military counties. For the second paycheck, deaths per day are 4.4 percent higher the week after paycheck receipt in military compared to non-military counties, and we can easily reject the null that these two coefficients are equal (p-value of 0.033).

VII. Understanding Mortality Over the Business Cycle

This examination of the broader relationship between liquidity, activity, and mortality has implications for research on mortality over the business cycle. A voluminous literature, with contributions from a variety of disciplines, has established that health outcomes are better among individuals with higher socioeconomic status (Kitigawa and Hauser, 1973). This relationship has been documented for virtually all measures of health and health habits, including mortality (Backland et al., 1999), self-reported health status (House et al., 1990), child health measures (Case et al., 2002), smoking (Chaloupka and Werner, 2000), obesity (Chang and Lauderdale, 2005), the incidence of disease (Banks et al., 2006) and biomarkers (Muenning et al., 2007; Seeman et al., 2008).

In contrast to this work is a more recent group of papers that show mortality is pro-cyclical. The basic statistical relationship has been documented for the United States (Ruhm, 2000) and several OECD countries (Gerdtham and Johannesson, 2005; Neumayer, 2004; Tapia Granados, 2005), and for many outcomes including deaths from heart disease, certain cancers, murder (Ruhm, 2000), motor vehicle fatalities (Evans and Graham, 1988), plus infant health

(Dehejia and Lleras-Muney, 2004), and self reported health status (Ruhm, 2003). The one death category that shows a decidedly counter-cyclical pattern is suicides (Ruhm, 2000).[41]

What has been missing from this literature is an explanation for this phenomenon. While Ruhm (2005a; 2005b) provides evidence that smoking, severe obesity, heavy drinking, and a sedentary lifestyle decline during economic downturns, no decomposition has identified whether these habits explain the changes in mortality. Ruhm (2005b, p. 1210) concludes, "…research needs to better identify mechanisms for the procyclic variation in mortality."

Our results linking activity and income receipt to short-term changes in mortality suggest a possible explanation for its pro-cyclical nature. As the economy expands, people naturally engage in more economic activity. They drive more, and go out to dinner and the movies more often. These changes can, in turn, increase mortality. If changing activity levels explain both the within-month cycle and the pro-cyclical nature of mortality, then causes of death with the greatest within-month cycles should also be those most strongly tied to the business cycle.

To see if this is the case, we compare the pro-cyclicality of mortality to the within-month cycle for the 15 cause of death categories presented in Table 4, using MCOD data for the 1976-2004 period. The methodology for analyzing the pro-cyclicality of mortality dates to Evans and Graham (1988), and is typified in Ruhm (2000). Using pooled time-series/cross-sectional data at the state level, mortality rates are regressed on state and year effects, demographic covariates, and a measure of the business cycle, which is typically the unemployment rate.

Let M_{it} be the mortality rate for state i in year t, defined as deaths per 100,000 people. Following Evans and Graham (1988) and Ruhm (2000), the model we estimate is of the form:

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[41] The disparity between the older literature on socioeconomic status and health and the more recent work on mortality and the business cycle is not all that surprising. Typical measures of socioeconomic status include variables such as education, wealth, income, or occupational status, which can all be considered measures of permanent income. In contrast, the econometric models used to test the cyclicality of mortality all use within-group estimators that hold state characteristics constant and ask whether year to year fluctuations in the unemployment rate alter mortality. These later models are therefore measuring the impact of transitory changes in economic activities on mortality.

(4) $\ln(M_{it}) = X_{it}\beta + UNEMP_{it}\alpha + u_i + v_t + \varepsilon_{it}$

Where X_{it} is a vector of demographic characteristics, u_i and v_t are state and year effects and ε_{it} is an idiosyncratic error. The key covariate is the state i's unemployment rate in year t ($UNEMP_{it}$). In the model, we include in X_{it} the fraction of people who are under 18, the fraction who are 65 and over, and the fraction that are black. We allow for arbitrary correlation in the errors within a state and weight observations by population size.

Results from this regression are reported in Table 13. In the first row, we report estimates for all-cause mortality. These results show a large, negative and statistically significant impact of the unemployment rate on mortality. A one percentage point drop in the unemployment rate will increase mortality by about 0.4 percent, which is about 20 percent lower than Ruhm's (2002, Table II, column a) estimate based on a slightly different time period.

In the next 15 rows, we show estimates of the pro-cyclicality of mortality for specific causes. These results are consistent with previous estimates, with traffic accidents, murders, other external causes, heart attacks, COPD, and the 'all other causes' category showing a pro-cyclical relationship and a p-value of at least 0.1. There are statistically significant counter-cyclical results for suicides, lung cancers and other cancers, while diseases like breast cancers, leukemia, heart disease, and non-alcohol cirrhosis have a weak statistical relationship with the business cycle.

This pattern of results is similar to the within-month pattern. To demonstrate this point, in Figure 5 we plot the coefficients on the unemployment rate from Table 13 along the x-axis and the within-month peak-to-trough estimates (the coefficient on the *Week(1)* dummy variable) from Table 4 on the y-axis. The graph shows a pronounced negative relationship, and the correlation coefficient between the two series is -0.4. There is one obvious outlier: suicides, which have a large within-month cycle but are decidedly counter-cyclical. When we exclude suicides, the correlation between the remaining 14 causes rises to -0.8, and an OLS line through these 14 points shows a strong negative relationship between the two sets of values. Overall, if the within-month mortality cycle is indeed due to changes in activity, then the similarity in the results across death categories between this cycle and the pro-cyclicality of mortality provides suggestive evidence that activity is the underlying cause for both.

VIII. Conclusion

When daily counts of deaths in the United States are arranged around the 1st day of the calendar month, what emerges is a clear pattern of deaths decreasing during the final days of the month, and then spiking on the 1st. We show that this within-month mortality cycle is a broad-based phenomenon common to most subgroups and many causes of death. It cannot be satisfactorily explained by changes in drug and alcohol consumption alone.

We find that many activities, such as consumer purchases, mall visits and cinema attendance, exhibit similar within-month cycles. While we do not have activity and mortality information available in a single dataset, existing medical knowledge of the activity triggers for specific health conditions – combined with the similarity of the demonstrated mortality and activity patterns – suggests that short-term changes in activity may be the missing explanation for the within-month mortality cycle. Furthermore, the patterns in activity and mortality are consistent with changes in liquidity over the month affecting people's activity levels and, in turn, the number of deaths on a given day.

These results link medical literature on the within-month mortality cycle to the economics literature on consumption smoothing, with implications for both. First, for the medical literature, understanding substance abuse as only part of the within-month mortality cycle means liquidity and payments have broader medical effects than is commonly thought. 'Full wallets' do not just mean increases in drug-related attendances in emergency departments, but probably affect many more aspects of health and health services provision. Second, in terms of consumption smoothing, this result points to the potential breadth of the excess sensitivity of consumption to the timing of payments. There are over 70 million records in the mortality data we use, and we estimate only 15 percent of the within-month cycle may be accounted for by substance abuse. If the remaining part of the pattern is due to liquidity changes affecting activity, then excess sensitivity and its explanations – such as hyperbolic discounting – must not be limited to narrow subpopulations.

The magnitudes of the mortality patterns we describe are not small relative to other movements in aggregate mortality rates. In Table 2, we estimate that mortality is 0.86 percent higher in the first week of the month compared to the last week. In 1990, this would have resulted in 4,252 more deaths in the first week of the month than in the last.[42] On the basis of our business cycle calculations, this is equivalent to the additional deaths generated by a half percentage point decline in the unemployment rate.

An alternative comparison is with income. Using the same National Health Interview Survey sample as Snyder and Evans (2006) and regressing a dummy for whether someone died in the year following interview on demographics (age, race, ethnicity, sex, marital status, and education) and the natural log of family income, the family income coefficient (standard error) is -0.00077 (0.00021). Given a population of 248,709,873 in 1990, 4,252 more deaths raises annual mortality rates by 2.37E-5. If we treat the -0.00077 estimate as the causal effect of income on mortality – an estimate many believe to be overstated (Deaton, 2003) – then to generate a 2.37E-5 change in the mortality rate, average family income in the United States must rise by 3.1 percent (2.37E-5/7.7E-4). To put this into perspective, between 1970 and 2007, the average annual change in real mean family income has been 1.1 percent.[43]

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[42] There were 2,148,463 deaths in 1990, or 5,886 deaths per day. This results in 4,252 (7*12*0.0086*5,886) excess deaths per year.
[43] http://www.census.gov/hhes/www/income/histinc/f05.html.

Of course, in order to understand whether there are potential gains to smoothing liquidity we need to know whether these short-term variations in liquidity and activity are actually changing the *total* number of deaths, or merely changing the *timing* of deaths of susceptible people by several days (what epidemiologists refer to as "harvesting"). For some causes, such as motor vehicle accidents, it is logical that more activity leads to an increase in deaths; but for conditions like heart attacks, the answer is not so clear. Analysis of one-off payments by Evans and Moore (2009) suggests most, but not all, of the variation in mortality may be harvesting, although more work needs to be done to understand this issue properly.

There are some potential policy implications suggested by our results. For example, the within-month mortality cycle and 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.

Our results also suggest a complex relationship between income and mortality that may have implications for how and when people are paid. While we do not directly address whether the mortality consequences of improved liquidity early in the month can be reduced by spreading out income payments, our exploration of the relationship between military payments and mortality – where we demonstrate that deaths among 17-39 year olds in military counties increase by about 10 percent the four days after the *second* check of the month – suggest the problem is not simply that some people are paid near the start of each month.

Finally, the results have implications for our understanding of the pro-cyclicality of mortality. The causes of death with the largest within-month mortality cycle also exhibit the most pro-cyclical mortality, suggesting that whatever drives the within-month mortality cycle also causes mortality to be pro-cyclical. Short-term changes in liquidity are more easily separated from permanent levels of income over the course of a month that over a business cycle.

The similarity of the two mortality phenomena suggests that the apparent contradiction between the protective effect of income and the pro-cyclicality of mortality can be resolved by viewing business cycle movements as events that lead to medium-term changes in liquidity, which then affect the activity levels and mortality risks people face.

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Figure 1: Relative Daily Mortality Risk (95% Confidence Intervals) by Day in Relation to the 1st of the Month, 1973-2005 MCOD, All Deaths, All Ages

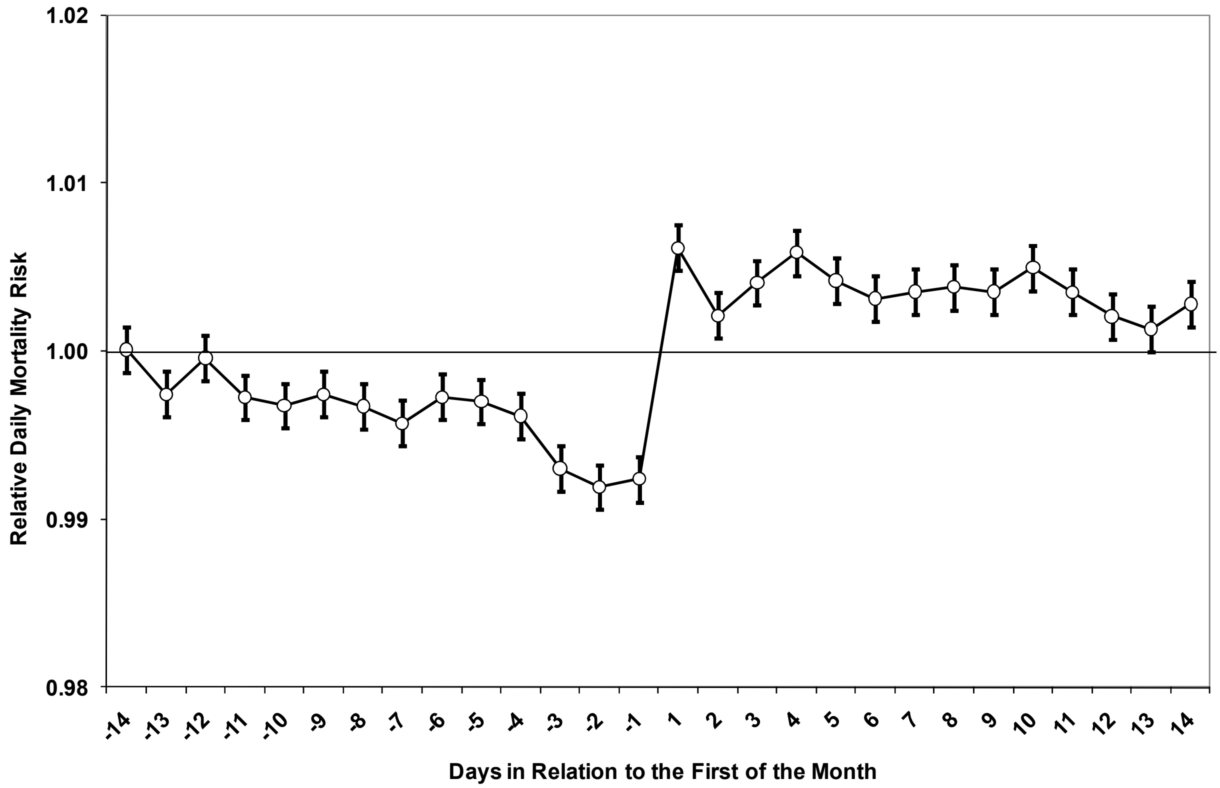


Figure 2: Relative Daily Mortality Rates (95% Confidence Intervals), With and Without Mention of Substance Abuse, 1978-1998 MCOD, All Ages

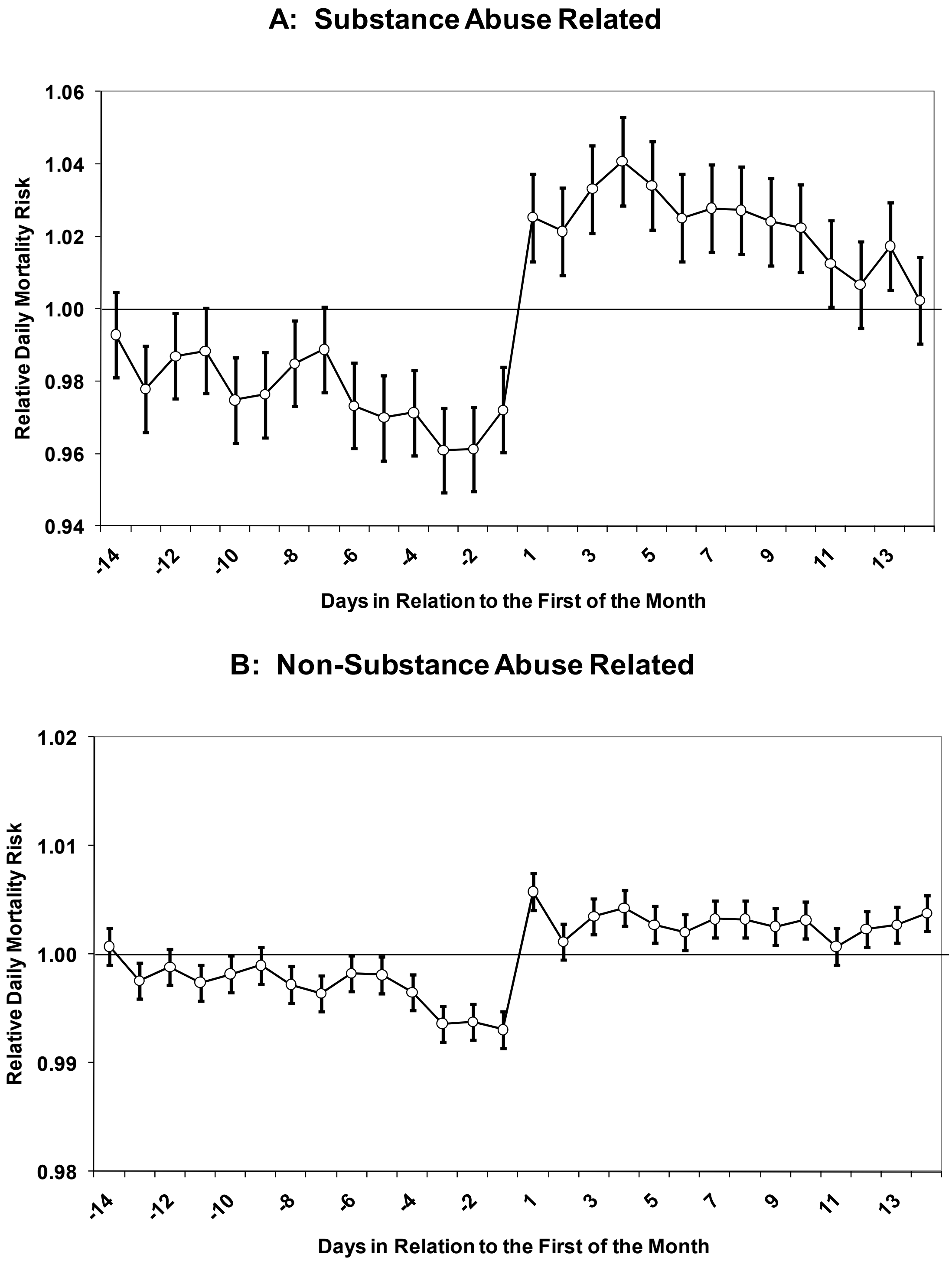
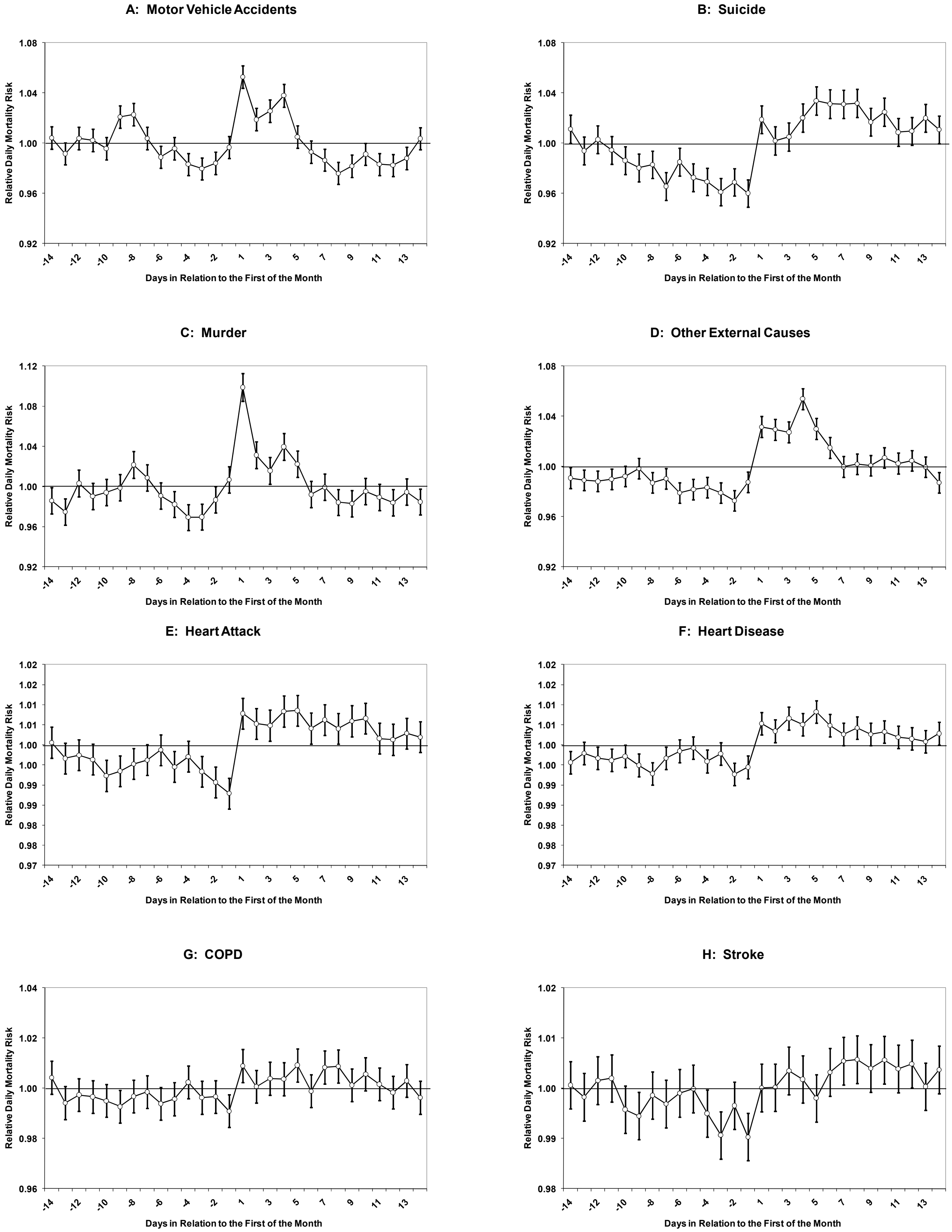
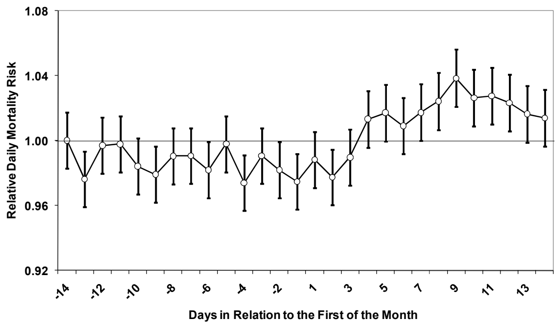


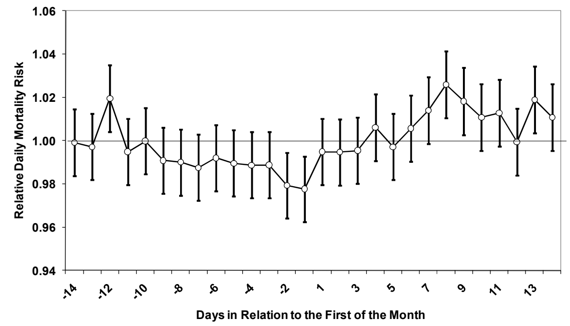
Figure 3: Relative Daily Mortality Rates (95% Confidence Intervals), By Specific Causes, 1973-2005 MCOD



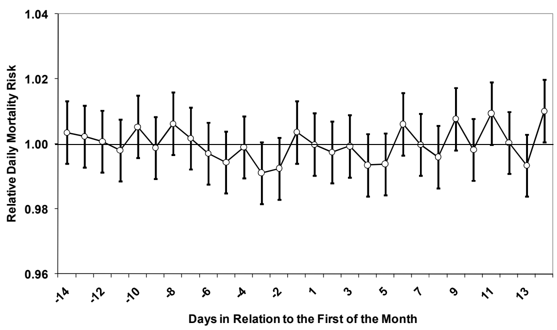
I: Alcohol Cirrhosis



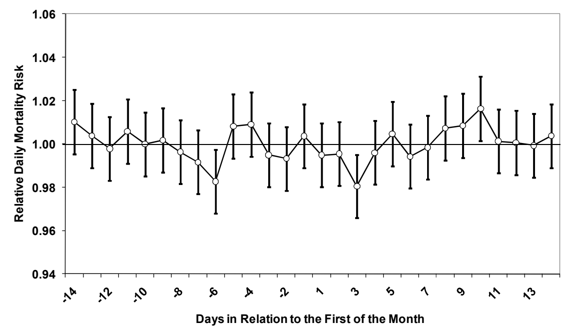
J: Non-Alcohol Cirrhosis



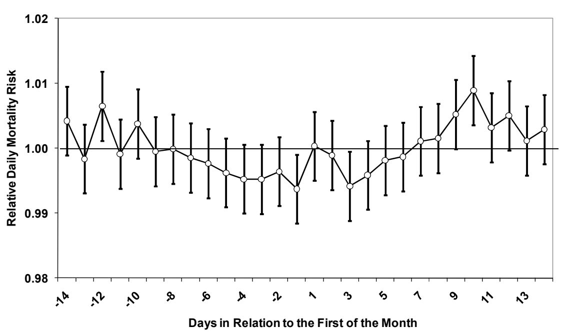
K: Breast Cancer



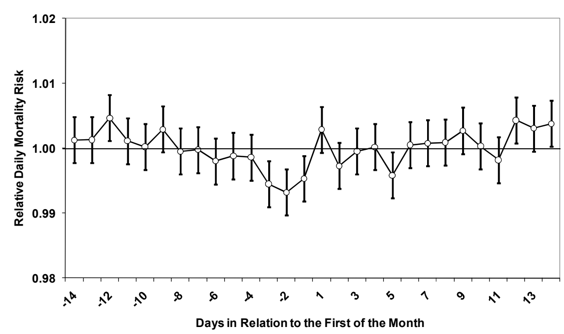
L: Leukemia



M: Lung Cancer



N: Other Cancers



O: Other causes

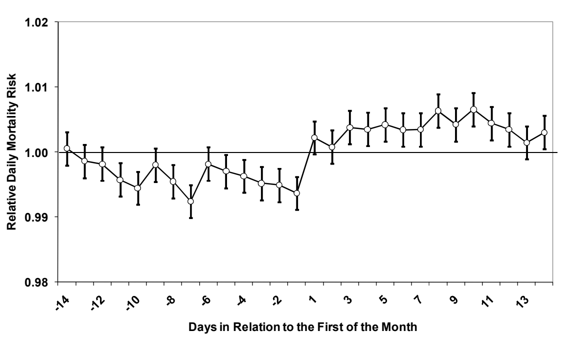


Figure 4: Relative Daily Mortality Rates (95% Confidence Interval), By Education, 1989-2005 MCOD

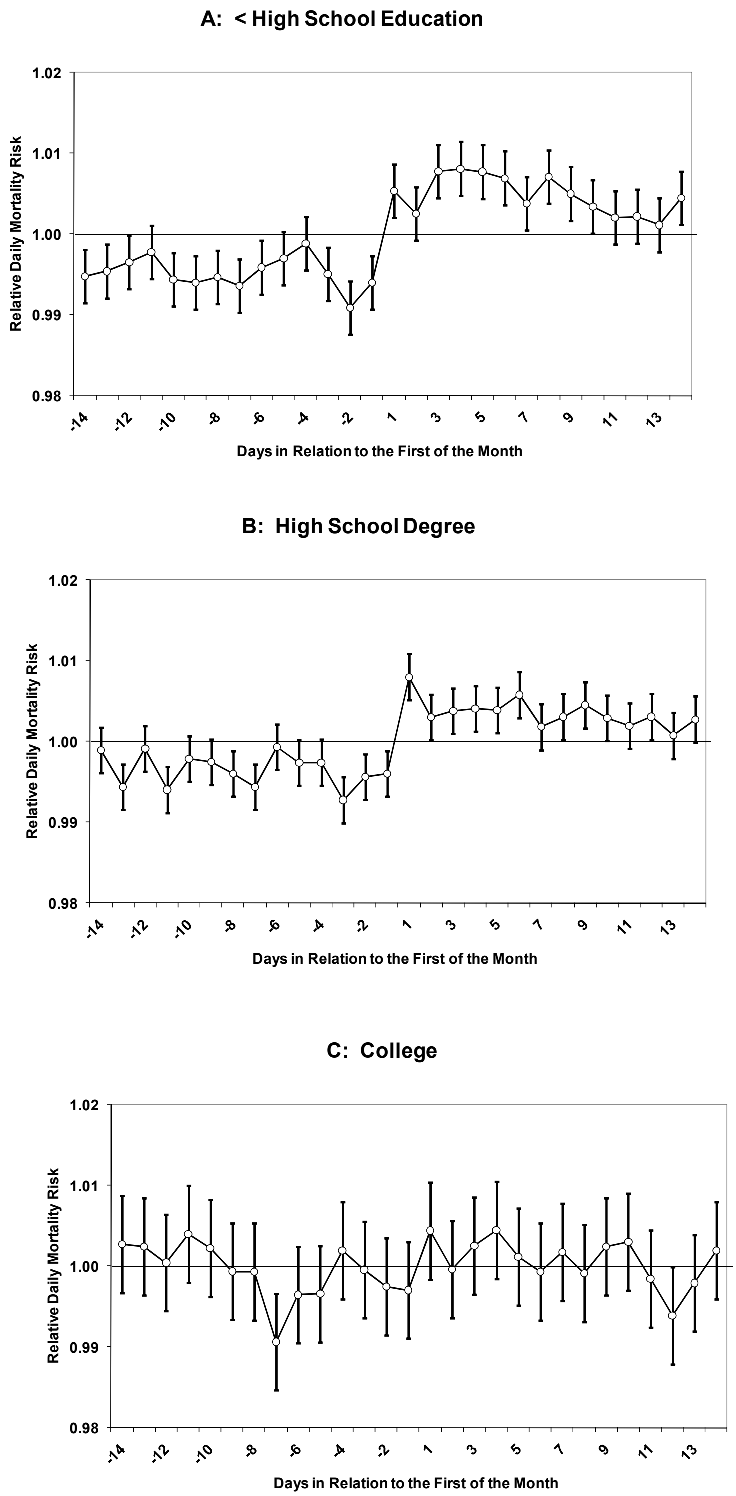


Figure 5: Scatter Plot, Mortality and the Business Cycle versus the Size of the Within-Month Mortality Cycle, By Cause of Death

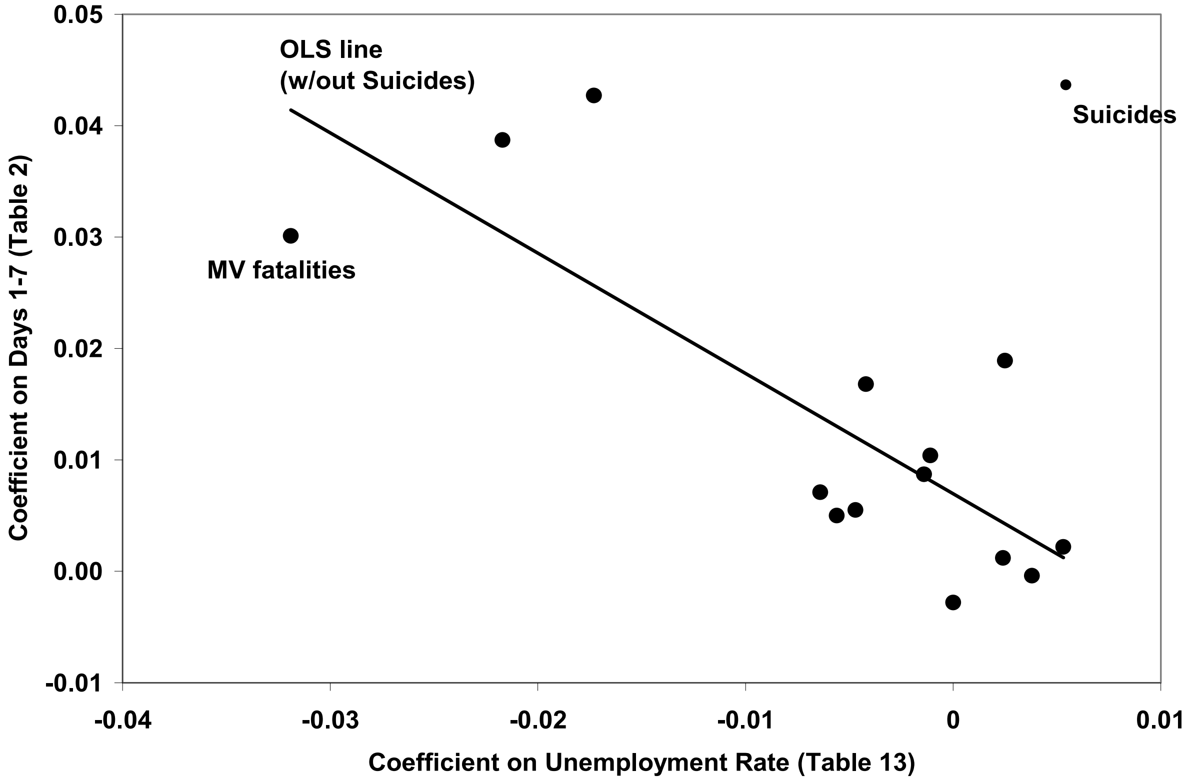


Table 1
OLS Estimates of ln(Daily Mortality Counts), MCOD 1973-2005

OLS estimates of ln(Daily Mortality Counts) R^2=0.0013 (With no other covariates)				OLS estimates of ln(Daily Mortality Counts) R^2=0.9083 (With all covariates)			
Day(-14)	0.0078	*Day(1)*	0.0133	*Day(-14)*	0.0079	*Day(1)*	0.0107
	(0.0021)		(0.0015)		(0.0020)		(0.0012)
Day(-13)	0.0056	*Day(2)*	0.0096	*Day(-13)*	0.0057	*Day(2)*	0.0096
	(0.0020)		(0.0016)		(0.0019)		(0.0014)
Day(-12)	0.0076	*Day(3)*	0.0114	*Day(-12)*	0.0081	*Day(3)*	0.0127
	(0.0020)		(0.0018)		(0.0019)		(0.0016)
Day(-11)	0.0051	*Day(4)*	0.0133	*Day(-11)*	0.0060	*Day(4)*	0.0143
	(0.0020)		(0.0018)		(0.0017)		(0.0015)
Day(-10)	0.0046	*Day(5)*	0.0121	*Day(-10)*	0.0079	*Day(5)*	0.0132
	(0.0019)		(0.0017)		(0.0017)		(0.0015)
Day(-9)	0.0049	*Day(6)*	0.0104	*Day(-9)*	0.0073	*Day(6)*	0.0116
	(0.0017)		(0.0018)		(0.0016)		(0.0016)
Day(-8)	0.0044	*Day(7)*	0.0110	*Day(-8)*	0.0061	*Day(7)*	0.0119
	(0.0015)		(0.0016)		(0.0015)		(0.0016)
Day(-7)	0.0038	*Day(8)*	0.0115	*Day(-7)*	0.0069	*Day(8)*	0.0120
	(0.0017)		(0.0018)		(0.0016)		(0.0016)
Day(-6)	0.0048	*Day(9)*	0.0110	*Day(-6)*	0.0061	*Day(9)*	0.0116
	(0.0017)		(0.0019)		(0.0015)		(0.0016)
Day(-5)	0.0045	*Day(10)*	0.0123	*Day(-5)*	0.0053	*Day(10)*	0.0129
	(0.0017)		(0.0019)		(0.0015)		(0.0017)
Day(-4)	0.0032	*Day(11)*	0.0107	*Day(-4)*	0.0040	*Day(11)*	0.0107
	(0.0016)		(0.0022)		(0.0014)		(0.0020)
Day(-3)	0.0010	*Day(12)*	0.0099	*Day(-3)*	0.0015	*Day(12)*	0.0103
	(0.0015)		(0.0019)		(0.0013)		(0.0017)
Day(-2)	-0.0003	*Day(13)*	0.0090	*Day(-2)*	0.0005	*Day(13)*	0.0097
	(0.0013)		(0.0019)		(0.0011)		(0.0017)
		Day(14)	0.0101			*Day(14)*	0.0107
			(0.0018)				(0.0017)
				Sun	-0.0229	*Wed.*	-0.0258
					(0.0007)		(0.0009)
				Mon.	-0.0109	*Thur.*	-0.0258
					(0.0008)		(0.0009)
				Tue.	-0.0213	*Fri.*	-0.0121
					(0.0008)		(0.0007)

The reference period is *Day(-1)*. There are 11,088 observations (336 observations per year for 33 years) and there is an average of 5,931 deaths per day. Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 6.

Table 2
OLS Estimates of ln(Daily Mortality Counts) Model
Demographic Subgroups, 1973-2005

Demographic subgroup	Mean daily Deaths	*Week(-2)* *[Day -14 to -7]*	*Week(1)* *[Day 1 to 7]*	*Week(2)* *[Day 8 to 14]*	R^2
All deaths	5,938	0.0035	0.0086	0.0077	0.9083
		(0.0011)	(0.0008)	(0.0013)	
Male	3,073	0.0048	0.0114	0.0091	0.8217
		(0.0009)	(0.0009)	(0.0010)	
Female	2,868	0.0030	0.0083	0.0069	0.9340
		(0.0010)	(0.0010)	(0.0010)	
White	5,137	0.0031	0.0064	0.0060	0.8954
		(0.0010)	(0.0010)	(0.0010)	
Black	706	0.0062	0.0235	0.0176	0.8433
		(0.0014)	(0.0015)	(0.0015)	
Other race	85	0.0025	0.0172	0.0150	0.9245
		(0.0037)	(0.0037)	(0.0037)	
Under 18 years	170	0.0048	0.0077	0.0028	0.8597
		(0.0027)	(0.0024)	(0.0028)	
18 to 39 years	310	0.0097	0.0204	0.0108	0.8003
		(0.0021)	(0.0021)	(0.0021)	
40 to 64 years	1,234	0.0062	0.0161	0.0141	0.7862
		(0.0010)	(0.0010)	(0.0010)	
Over 65 years	4,185	0.0028	0.0056	0.0057	0.9319
		(0.0013)	(0.0011)	(0.0015)	
Single, 1979-2005	753	0.0043	0.0150	0.0087	0.6748
		(0.0015)	(0.0015)	(0.0015)	
Married, 1979-2005	2,540	0.0041	0.0063	0.0067	0.7555
		(0.0010)	(0.0010)	(0.0010)	
Widowed, 1979-2005	2,214	0.0012	0.0063	0.0059	0.9055
		(0.0014)	(0.0014)	(0.0014)	
Divorced, 1979-2005	540	0.0069	0.0214	0.0173	0.9672
		(0.0017)	(0.0017)	(0.0017)	
Metropolitan county	4,311	0.0034	0.0085	0.0073	0.9508
		(0.0010)	(0.0010)	(0.0010)	
Non-metropolitan county	1,609	0.0037	0.0088	0.0083	0.8402
		(0.0012)	(0.0012)	(0.0012)	

The reference period is *Week(1)*. All have 11,088 observations, except for the groups defined by marital status. This information was not included in MCOD data before 1979; these models have 9,408 observations. Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 6.

Table 3
OLS Estimates of ln(Daily Mortality Counts) Model by Substance Abuse, 1979-1998

Cause of death	Years	Mean daily deaths	*Week(-2)*	*Week(1)*	*Week(2)*	R^2
All deaths	1979-98	5,879	0.0037 (0.0013)	0.0087 (0.0012)	0.0078 (0.0015)	0.8763
Deaths with a substance abuse multiple cause	1979-98	257	0.0108 (0.0028)	0.0295 (0.0026)	0.0141 (0.0029)	0.5989
Deaths without a substance abuse multiple cause	1979-98	5,622	0.0034 (0.0014)	0.0077 (0.0012)	0.0076 (0.0016)	0.8824

The reference period is *Week(1)*. All models have 6,720 observations. Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 6.

Table 4
OLS Estimates of Log Daily Mortality Counts, 1973-2005

Cause of death	Mean daily deaths	Percent substance abuse	*Week(-2)*	*Week(1)*	*Week(2)*	R^2
All deaths	5,938	4.37%	0.0035 (0.0011)	0.0086 (0.0008)	0.0077 (0.0013)	0.908
				By Cause of Death		
Motor vehicle	127.6	43.02%	0.0152 (0.0037)	0.0301 (0.0023)	0.0106 (0.0039)	0.753
Suicides	81.1	14.44%	0.0205 (0.0035)	0.0436 (0.0038)	0.0397 (0.0037)	0.381
Homicides	58.0	79.80%	0.0105 (0.0046)	0.0387 (0.0047)	0.0107 (0.0049)	0.591
Other external causes	147.0	22.26%	0.0125 (0.0035)	0.0427 (0.0036)	0.0238 (0.0041)	0.655
Heart disease	1268.6	0.52%	0.0013 (0.0016)	0.0087 (0.0014)	0.0060 (0.0017)	0.866
Heart attack	678.0	0.19%	0.0031 (0.0016)	0.0104 (0.0016)	0.0067 (0.0018)	0.956
COPD	231.8	0.44%	0.0020 (0.0028)	0.0055 (0.0026)	0.0033 (0.0032)	0.937
Cirrhosis	42.3	0.42%	0.0135 (0.0048)	0.0168 (0.0049)	0.0269 (0.0046)	0.418
Alcohol Cirrosis	33.3	100%	0.0076 (0.0051)	0.0189 (0.0052)	0.0387 (0.0052)	0.128
Stroke	445.0	0.37%	0.0039 (0.0017)	0.0050 (0.0017)	0.0062 (0.0020)	0.832
Lung cancer	353.9	0.12%	0.0036 (0.0019)	0.0022 (0.0018)	0.0075 (0.0018)	0.938
Breast cancer	109.4	0.06%	0.0034 (0.0028)	-0.0004 (0.0030)	0.0019 (0.0028)	0.521
Leukemia	50.3	0.14%	0.0032 (0.0045)	-0.0028 (0.0043)	-0.0061 (0.0042)	0.446
Other cancers	794.5	0.19%	0.0033 (0.0012)	0.0012 (0.0013)	0.0042 (0.0012)	0.913
Other conditions	1517.5	4.49%	0.0025 (0.0016)	0.0071 (0.0014)	0.0078 (0.0019)	0.953

The reference period is *Week(1)*. All models have 11,088 observations. Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 6. The percentage of substance abuse deaths is calculated using deaths between 1979 and 1998.

Table 5
OLS Estimates of Log Daily Motor Vehicle Fatality Count Model,
Fatal Accident Reporting System, 1975-2004

Time of day	Mean Daily Deaths	Percent Alcohol Involvement	*Week(-2)*	*Week(1)*	*Week(2)*	R^2
All accidents	120.4	44.9%	0.0164	0.0342	0.0139	0.753
			(0.0042)	(0.0039)	(0.0044)	
12:00am-6:00am	27.4	73.2%	0.0348	0.0346	0.0190	0.793
			(0.0094)	(0.0087)	(0.0092)	
6:00am-10:00am	12.9	15.8%	0.0093	0.0281	0.0220	0.232
			(0.0111)	(0.0105)	(0.0108)	
10:00am-4:00pm	26.3	18.1%	0.0120	0.0460	0.0163	0.301
			(0.0072)	(0.0073)	(0.0073)	
4:00pm-8:00pm	27.6	42.8%	0.0094	0.0413	0.0103	0.369
			(0.0075)	(0.0075)	(0.0079)	
8:00pm-12:00am	25.3	66.7%	0.0205	0.0321	0.0112	0.640
			(0.0085)	(0.0085)	(0.0086)	

The reference period is *Week(1)*. All models have 10,008 observations (28 observations per month x 12 months x 30 years). Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 6.

Table 6
OLS Estimates of Log Daily Mortality Counts, 1973-2005 MCOD

Covariate	Motor vehicle fatalities	Murders	Other external causes	Suicides
Sunday	0.315	0.251	0.078	-0.120
	(0.006)	(0.006)	(0.004)	(0.005)
Tuesday	-0.033	-0.032	-0.009	-0.066
	(0.004)	(0.006)	(0.004)	(0.004)
Wednesday	-0.013	-0.048	-0.009	-0.098
	(0.005)	(0.006)	(0.004)	(0.004)
Thursday	0.031	-0.032	-0.005	-0.118
	(0.005)	(0.006)	(0.004)	(0.004)
Friday	0.238	0.080	0.034	-0.127
	(0.005)	(0.006)	(0.004)	(0.004)
Saturday	0.452	0.329	0.143	-0.157
	(0.007)	(0.008)	(0.004)	(0.004)
New Year's Day	0.372	0.551	0.203	0.213
	(0.040)	(0.034)	(0.021)	(0.024)
Holy Thursday	0.135	0.088	0.050	-0.019
	(0.021)	(0.026)	(0.017)	(0.021)
Good Friday	0.077	0.055	0.036	-0.028
	(0.016)	(0.025)	(0.019)	(0.018)
Memorial Day	0.151	0.063	0.119	-0.131
	(0.022)	(0.025)	(0.025)	(0.018)
July 4th	0.222	0.214	0.219	-0.096
	(0.022)	(0.040)	(0.021)	(0.019)
Labor Day	0.165	0.151	0.084	-0.171
	(0.016)	(0.025)	(0.018)	(0.019)
Thanksgiving	0.206	0.165	0.028	-0.162
	(0.026)	(0.027)	(0.019)	(0.019)
Christmas Eve	0.374	0.215	0.027	-0.168
	(0.048)	(0.038)	(0.027)	(0.028)
Christmas Day	0.071	0.188	0.009	-0.123
	(0.039)	(0.035)	(0.025)	(0.031)
New Year's Eve	0.371	0.179	0.001	-0.020
	(0.040)	(0.032)	(0.003)	(0.027)

There are 11,088 observations (336 observations per year for 33 years). Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects, other dummies for special days of the year that are not reported in this table, plus the other coefficients from the model reported in the final two columns of Table 1. A complete list of days is included in footnote 6.

Table 7
OLS Estimates of Daily Consumption Equations,
1996-2004 Consumer Expenditure Survey Diary Data File

	By Types of Items Purchased		
Variable	Food	Non-food	All items
Week(-2)	-0.059	0.021	-0.101
	(0.107)	(0.135)	(0.193)
Week(1)	0.273	0.162	0.486
	(0.107)	(0.137)	(0.191)
Week(2)	0.183	0.212	0.328
	(0.120)	(0.147_	(0.214)
Mean of dep. var.	15.39	12.61	27.79

The reference period is *Week(1)*. There are 715,213 observations in the models. Standard errors are in parentheses and allow for within-person correlation in errors. The numbers are in real December 2008 dollar values. Other covariates include a complete set of dummy variables for age, sex, race, and the education of reference person, a complete set of dummies for region, urban area and income of the family, dummies for the weekday, month, and year, plus dummies for special days during the year. A complete list of special days is included in footnote 6.

Table 8
OLS Estimates of the Within-Month Purchase Cycle, Various Sources

Outcome	Time Period	Obs.	Mean daily counts	*Week(-2)*	*Week(1)*	*Week(2)*	R^2
Ticket sales, MD pick 3 and pick 4	1/1/2003 – 12/31/2006	1,344	0.81 million	0.0065 (0.0055)	0.0705 (0.0047)	0.0319 (0.0041)	0.924
Ticket sales, OH daily number + pick 4	6/20/2005-6/16/2007	573	1.76 million	0.0121 (0.0071)	0.0875 (0.0061)	0.0388 (0.0061)	0.840
Visits to malls	1/1/2000-12/22/2007	2,657	25.4 million	0.0375 (0.0087)	0.0207 (0.0079)	0.0314 (0.0079)	0.895
Visits to retail establishments	1/4/2004-12/22/2007	1,328	94.1 Million	0.0573 (0.0205)	0.0307 (0.0144)	0.0193 (0.0162)	0.851
Visits to apparel retailers	1/4/2004-12/22/2007	1,325	60.4 million	0.0578 (0.0175)	0.0328 (0.0148)	0.0225 (0.0152)	0.850
Ticket sales top 10 grossing movies	1/1/1998-6/7/2007	3,171	19.3 million	-0.0057 (0.0237)	0.0558 (0.0192)	-0.0057 (0.0237)	0.928
Attendance at baseball games	1973-1998 2000-2004	54,939	24,238	0.0036 (0.0049)	0.0013 (0.0052)	0.0337 (0.0059)	0.872
DC Metro ridership	1/1/1997 – 9/19/2007	3,573	480,898	0.0015 (0.0070)	0.0009 (0.0069)	0.0078 (0.0069)	0.941

Numbers in parentheses are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 6. Please see the text for any other characteristics of specific models.

Table 9
OLS Estimates of Daily Consumption Equations,
1996-2004 Consumer Expenditure Survey Diary Data File

	Week(-2)	*Week(1)*	*Week(2)*	Mean ($)	*Week(-2)*	*Week(1)*	*Week(2)*	Mean ($)	*Week(-2)*	*Week(1)*	*Week(2)*	Mean ($)
	Head has < high school education (N=109,069)				Head completed high school but not college (N=349,915)				Head completed college (N=256,229)			
Food	0.027	0.994	0.383	12.36	0.492	0.643	0.519	14.46	0.271	-0.069	0.362	17.89
	(0.233)	(0.238)	(0.256)		(0.132)	(0.137)	(0.149)		(0.183)	(0.183)	(0.204)	
Non-food	0.202	0.177	0.210	8.39	0433	0.500	0.540	11.77	0.555	0.252	0.513	15.54
	(0.267)	(0.252)	(0.284)		(0.168)	(0.170)	(0.185)		(0.250)	(0.246)	(0.269)	
Total	-0.078	1.188	0.505	20.96	0.877	1.144	1.009	26.24	0.845	0.284	0.759	33.43
	(0.404)	(0.393)	(0.448)		(0.242)	(0.246)	(0.275)		(0.345)	(0.351)	(0.383)	
	Household has federal support other than Social Security (N=34,372)				Household has Social Security but no other federal support (N=130,239)				Household has no federal support income (N=550,602)			
Food	-0.227	2.868	1.173	13.50	0.206	0.732	0.259	13.14	-0.103	0.004	0.109	16.02
	(0.454)	(0.496)	(0.517)		(0.208)	(0.219)	(0.238)		(0.126)	(0.124)	(0.141)	
Non-food	-0.082	0.599	-0.563	9.29	-0.021	0.534	0.326	9.46	0.046	0.053	0.244	13.56
	(0.528)	(0.538)	(0.561)		(0.252)	(0.252)	(0.280)		(0.162)	(0.164)	(0.177)	
Total	-0.061	3.755	1.131	22.73	-0.061	1.230	0.507	22.52	-0.093	0.114	0.250	29.53
	(0.362)	(0.843)	(0.904)		(0.359)	(0.378)	(0.423)		(0.229)	(0.229)	(0.256)	
	Family income < $30,000 (n=338,890)				Family income ≥$30,000 (n=182,263)				Family income not reported (n=194,060)			
Food	0.015	0.561	0.172	12.66	-0.572	-0.507	0.174	22.45	0.250	0.505	0.507	13.50
	(0.130)	(0.135)	(0.145)		(0.263)	(0.254)	(0.286)		(0.204)	(0.210)	(0.235)	
Non-food	0.036	0.252	0.135	10.01	-0.448	0.032	0.099	20.08	0.393	0.101	0.393	10.15
	(0.162)	(0.160)	(0.172)		(0.353)	(0.364)	(0.387)		(0.244)	(0.244)	(0.265)	
Total	-0.082	0.839	0.320	22.58	-1.087	-0.431	-0.235	42.09	0.690	0.761	0.734	23.36
	(0.233)	(0.235)	(0.261)_		(0.486)	(0.488)	(0.544)		(0.357)	(0.362)	(0.308)	

The reference period is *Week(1)*. Standard errors are in parenthesis and allow for within-person correlation in errors. Covariates include a complete set of dummy variables for age, sex, race and education of reference person; region; urban area; family income; weekday; month; year; and special days during the year, which are listed in footnote 6. Numbers are in real December 2008 dollars.

Table 10
Negative Binomial Estimates of Daily Mortality Counts, 1988-2005

Group	Mean daily Deaths	*Week(-2)*	*Week(1)*	*Week(2)*	R^2
All deaths	6,360	0.0015	0.0091	0.0074	0.9344
		(0.0015)	(0.0015)	(0.0015)	
		By level of education			
< High school	1,916	0.0021	0.0102	0.0093	0.7981
		(0.0018)	(0.0018)	(0.0018)	
High school	2,908	0.0008	0.0093	0.0072	0.9610
		(0.0015)	(0.0019)	(0.0015)	
College degree	664	0.0031	0.0045	0.0023	0.9417
		(0.0020)	(0.0020)	(0.0021)	

The reference period is *Week(1)*. All models have 5,712 observations. Numbers in parenthesis are standard errors that allow for arbitrary correlation in the within-month (-14 to 14) errors. Other covariates include a complete set of day of the week, monthly and annual dummy variables, plus a complete set of dummies for special days specified in footnote 6.

Table 11
Estimates of Log of Weekly Mortality Counts Equation, 30-Week Period, Summer and Fall 2001

Sample	Ages 25-64 (1)	Unmarried Males, 25-64 (2)	Ages 65+ (3)	Ages 25-64 (4)
Rebate	0.0269	0.0469	-0.0009	
	(0.0097)	(0.0197)	(0.0056)	
Rebate x				0.0515
LastWeekInMonth				(0.0183)
Rebate x				0.0163
NotLastWeekInMonth				(0.0119)
% in sample w/out Federal Taxes	51.5%	75.2%	25.2%	51.5%
p-value: Group effects=0	0.813	0.334	0.127	0.851
p-value: rows (2)=(3)				0.113
R^2	0.715	0.340	0.8411	0.718
Mean deaths per obs.	1,014	304	3,285	1,014

Standard errors are in parentheses. Other covariates in the model include week fixed effects and Social Security number group fixed effects. The percent in sample that paid federal taxes in 2000 is estimated from the IPUMS-CPS for March 2001.

Table 12
Maximum Likelihood Estimates of Daily Mortality Negative Binomial Equation
Counties With and Without a High Military Presence, Aged 17 to 39, 1973 to 1988

	Payday near the 1st of the Month			Payday near the 15th of the Month		
	Military Counties (1)	Non-Military Counties (2)	P-value on Test: Coefficients (1) = (2)	Military Counties (4)	Non-Military Counties (5)	P-value on Test: Coefficients (5) = (6)
Payday(-7)	0.0057	0.0051	0.989	0.0291	0.0066	0.688
	(0.0564)	(0.0089)		(0.0555)	(0.0085)	
Payday(-6)	0.0147	0.0078	0.546	0.0323	-0.0044	0.501
	(0.0551)	(0.0088)		(0.0541)	(0.0084)	
Payday(-5)	-0.0228	0.0112	0.545	-0.0469	0.0013	0.386
	(0.0556)	(0.0087)		(0.0550)	(0.0084)	
Payday(-4)	-0.0224	0.0146	0.515	-0.0194	-0.0088	0.851
	(0.0563)	(0.0087)		(0.0554)	(0.0085)	
Payday(-3)	0.0590	0.0085	0.367	0.0222	-0.0001	0.688
	(0.0555)	(0.0087)		(0.0550)	(0.0085)	
Payday(-2)	0.0680	0.0017	0.237	0.0113	-0.0082	0.321
	(0.0556)	(0.0087)		(0.0553)	(0.0085)	
Payday(1)	0.0405	0.0230	0.757	0.1063	-0.0064	0.040
	(0.0562)	(0.0088)		(0.0544)	(0.0086)	
Payday(2)	0.1055	0.0394	0.223	0.0881	0.0021	0.111
	(0.0538)	(0.0087)		(0.0534)	(0.0085)	
Payday(3)	0.0599	0.0290	0.575	0.0849	0.0040	0.134
	(0.0545)	(0.0085)		(0.0534)	(0.0084)	
Payday(4)	0.0651	0.0181	0.396	0.1046	0.0070	0.072
	(0.0549)	(0.0087)		(0.0538)	(0.0085)	
Payday(5)	0.0794	0.0295	0.370	-0.0095	-0.0011	0.880
	(0.0551)	(0.0086)		(0.0554)	(0.0085)	
Payday(6)	-0.0991	0.0166	0.047	0.0235	0.0139	0.863
	(0.0578)	(0.0086)		(0.0552)	(0.0085)	
Payday(7)	0.0473	0.0235	0.675	0.0123	-0.0009	0.816
	(0.0563)	(0.0085)		(0.0558)	(0.0084)	

There are 10,584 observations. Military counties had over 15 percent of 17 to 64 year old residents enlisted as active military personnel in the 1970, 1980, and 1990 Censuses, while non-military counties had less than one percent of their 17 to 64 year old residents in the military in 1970, 1980, and 1990. Average daily deaths in military and non-military counties are 3.7 and 244.2, respectively. Numbers in parentheses are standard errors that allow for an arbitrary correlation across observations within a synthetic month/year group based on military payments. Other covariates include a complete set of synthetic month and year effects, weekday effects, dummies for special days described in footnote 6, a dummy for observations from counties with a high military presence, an indicator for the first pay period, and an interaction between the military county and pay period indicators.

Table 13
OLS Estimates of State-Level ln(Cause-Specific Death Rate) Equation,
50 States and the District of Columbia, 1976-2004.

Cause of death	Deaths per 100,000 people	Coefficient (Standard error) on state-level unemployment	R2
All deaths	869.1	-0.0039	0.968
		(0.0013)	
		By Causes of Death	
Motor vehicle accidents	21.3	-0.0319	0.930
		(0.0043)	
Suicides	12.9	0.0146	0.886
		(0.0059)	
Homicides	7.9	-0.0217	0.907
		(0.0080)	
Other external causes	23.9	-0.0175	0.803
		(0.0049)	
Non-AMI heart disease	177.3	-0.0014	0.919
		(0.0026)	
AMI	102.9	-0.0113	0.963
		(0.0038)	
COPD	33.8	-0.0046	0.963
		(0.0024)	
Cirrhosis, non-alcohol related	5.9	-0.0042	0.819
		(0.0079)	
Cirrhosis, alcohol related	4.9	0.0026	0.826
		(0.0092)	
Stroke	66.7	-0.0056	0.948
		(0.0032)	
Lung cancer	50.3	0.0054	0.958
		(0.0019)	
Breast cancer	15.6	0.0039	0.910
		(0.0018)	
Leukemia	7.3	-0.0000	0.845
		(0.0018)	
Other cancers	115.4	0.0024	0.968
		(0.0012)	
All other causes	223.0	-0.0064	0.941
		(0.0020)	

All models have data from 50 states and the District of Columbia over the 29 year period 1976-2004. The dependent variable is the log death rate (deaths per 100,000 people). All models control for state and year effects, plus the fraction black, fraction under five years of age, and the fraction over 64 years of age. Observations are weighted by population. The standard errors are calculated allowing for arbitrary correlation in errors within a state.