Dangerous Liquidity and the Demand for Health Care: Evidence from the 2008 Stimulus Payments*

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Abstract

Household finances can affect health and health care through several channels. To explore these channels, we exploit the randomized timing of the arrival of the 2008 Economic Stimulus Payments. We find that the payments raised the probability of an adult emergency department visit over the following 23 weeks by an average of 1.1%. This effect is difficult to reconcile with the Permanent Income Hypothesis. We observe little impact on avoidable hospitalizations or emergency visits for non-urgent conditions and no difference in effects as a function of health insurance coverage. By contrast, we show that the increase is driven by visits for urgent medical conditions, like drug- and alcohol-related visits. Complementary evidence suggests that consumers are not simply substituting from outpatient doctor visits to hospital care. The results thus suggest that liquidity constraints may not constitute a direct barrier to care, but rather that liquidity can increase health care utilization indirectly by increasing the need for care.

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Keywords: Liquidity constraints, Health care demand, Permanent Income Hypothesis, Dangerous liquidity

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1 Introduction

Nearly ten percent of low-income respondents to the National Health Interview Survey report that in the past year they needed medical care but could not afford it.[1] Such households may not have been able to afford care because they were liquidity constrained. Health policy depends critically on whether such liquidity constraints affect the care that people receive. If consumers are liquidity constrained, then deductibles, co-payments, and other departures from full insurance may inefficiently discourage care. Similarly, liquidity constraints may cause consumers to forgo cost-effective preventive care and risk expensive hospitalizations.

In this paper, we test for liquidity constraints in health care utilization. To do so, we study how government payments to taxpayers affect inpatient hospitalizations and emergency department (ED) visits. We exploit exogenous variation in the timing of the 2008 Economic Stimulus Payments. The payments were sent to households on a date determined by the last two digits of the head-of-household's Social Security Number (SSN), and those two digits are quasi-randomly assigned. In exploiting this variation we follow Evans and Moore (2012); Gross et al. (2011); and Parker et al. (2011); among others.

The stimulus payments may have a direct effect on hospital visits by temporarily relaxing households' liquidity constraints. This direct effect could increase hospitalizations and ED visits if liquidity-constrained individuals could not afford such health care before the payments. Alternatively, the direct effect could be negative if consumers use the payments to purchase preventive care that reduces the probability of a hospital visit. One would expect such a direct effect to be strongest for the uninsured and for medical conditions in which the timing of care is discretionary.

Alternatively, the stimulus payments may impact health care consumption indi-

[1] Calculation by the authors based on the 1997–2007 National Health Interview Survey.

rectly, via changes in health. This could occur if households use the stimulus payments to increase certain types of consumption which affect the need for hospital care. For instance, if some consumers spend the payments on recreational drugs, then such a change in consumption patterns might indirectly affect health care utilization by leading to adverse events. Such a mechanism would be consistent with the findings of Evans and Moore (2011) and Dobkin and Puller (2007). Other consumption, representing a general increase in activity, could be dangerous as well (Ruhm, 2000; Miller et al., 2009; Lusardi et al., 2010; Dehejia and Muney, 2004).

We find that the stimulus payments caused ED visits to increase by an average of 1.1% over a 23 week follow-up observation period. This amounts to an annualized rate of over 50,000 additional ED visits in California. This impact alone is difficult to reconcile with the Life-Cycle Hypothesis/Permanent Income Hypothesis, because the stimulus payments were small relative to lifetime income. In contrast, it suggests that a portion of the population responds to transitory changes in income.

Moreover, our results suggest that the increase in hospital utilization occurred via the indirect channel rather than the direct channel. We document evidence that the stimulus payments increased risky consumption patterns. The payments caused a large percentage increase in alcohol- and drug-related hospital visits, but did not change the risk of an avoidable hospital visit or visits associated with chronic conditions. The stimulus payments had relatively similar effects for people who were publicly insured, privately insured, and uninsured.

Our findings relate to several studies of household finance, health, and health care. First, our findings complement previous studies that have demonstrated how income transfers can affect health. Evans and Moore (2011) demonstrate that short-term income transfers increase mortality, and Dobkin and Puller (2007) find that transfers through cash welfare programs increase hospitalizations for drug- and alcohol-related medical conditions. Our results support the conclusions of these two papers in that we demonstrate how income transfers to the general population affect the need for emergency care.

Second, this paper is related to a larger discussion regarding the affordability of health care. Moran and Simon (2006) estimate that an increase in lifetime income increases the consumption of pharmaceuticals.[2] This suggests that many individuals may not consume the health care they need because they cannot afford it. This paper studies a similar question, but instead focuses on hospital visits and the general population. Moreover, we focus on a temporary increase in income rather than a large, sustained increase in income. Doing so allows us to test whether short-term liquidity constraints affect the demand for health care.

Finally, Parker et al. (2011) and Shapiro and Slemrod (2009) examine the consumption response to the 2008 Economic Stimulus Payments tax rebates and estimate that consumers had a marginal propensity to consume of only 0.025 on health care. We replicate this finding and decompose it across types of health care goods. We find economically and statistically insignificant changes in all of the available subcategories of health expenditure. This suggests the liquidity shocks did not cause substitution from outpatient physicians' services into hospital care, and confirms our main results.

Together with this prior work, our findings support the view that liquidity constraints are not a primary, direct barrier to obtaining health care. Rather, temporary increases in liquidity can be dangerous by inducing risky forms of consumption.

The paper proceeds as follows. Section 2 describes the structure of the stimulus payments and the administrative hospital data on which we rely. Section 3 presents our analysis for hospital visits. Section 4 presents a replication and extension of the

[2] Similarly, Snyder and Evans (2006) examine how this increase in lifetime income (driven by the Social Security benefits "notch") affected mortality.

results of Parker et al. (2011). Section 5 discusses the implications of our findings and concludes.

2 Background on the Stimulus Payments and Hospital Data

On February 13, 2008, the Economic Stimulus Act of 2008 was enacted with bipartisan support. Two-thirds of the $152 billion bill, and more than 90% of the net outlays from 2008–2018, consisted of direct payments to 130 million households. The priority of most subsequent analysis has been to study the consequences of the stimulus for aggregate consumption and savings (e.g., Parker et al. (2011) and Shapiro and Slemrod (2009)). The randomized disbursement schedule also provides an opportunity to study impacts on additional outcome variables, and in this paper we take up the question of how the stimulus payments affected health and the utilization of health care.

The stimulus payments were distributed by the Internal Revenue Service according to the staggered schedule reported in Table 1. Heads-of-household with lower values of the last two digits of their SSN's received their stimulus payments earlier, and these SSN digit-pairs are effectively randomly assigned.[3] Paper checks were sent out over three months. The first SSN group (digits 00–09) was sent its checks on the 16$^{\text{th}}$ of May and the final group (digits 88–99) on the 11$^{\text{th}}$ of July.

Households in 2008 could elect to receive their stimulus payments via direct deposit instead of mail, and roughly 40% of households did so (Parker et al., 2011). The direct deposit transfers were made on only three dates, listed in the third column of Table 1. In our analysis below, we treat the direct deposit and paper check schedules symmetrically, though direct deposit recipients were less likely to be liquidity constrained, and the three-week span for disbursing direct deposit payments provides

[3] Parker et al. (2011) review additional details of the stimulus payment distribution. Here we summarize the most relevant facts.

little variation for detecting an impact.[4]

Households received stimulus payments in 2008 if they paid taxes or had sufficient qualifying income in 2007.[5] The base payments ranged from $300–$600 for single filers to $600–$1,200 for couples. The IRS also included a $300 supplement for each qualifying child. Stimulus payments were phased out linearly between income levels of $75,000 and $87,000 for childless, single-headed households and at twice those levels for childless households of married couples filing jointly. Roughly 85% of households received a stimulus payment, and the average payment was roughly $900.

In order to measure how the stimulus payments affected health care utilization, we obtained an extract of administrative hospital records from the California Office of Statewide Health Planning and Development (OSHPD). The data comprise a near-census of ED and inpatient hospital visits in California for 2008.[6] For each visit, we observe the patient's gender, age, insurance status, and ZIP code of residence, in addition to the medical condition and the exact date when the visit occurred.[7] The data also include a categorical variable for each patient corresponding to one of the 10 SSN groups in Table 1. We restrict the sample to visits that occurred at most 19 weeks before the stimulus payments were sent and at most 23 weeks after the stimulus payments were sent. That restriction is the widest interval possible given the data set.

Table 2 presents summary statistics for the entire sample and for the 9 groups sent paper checks. Observations in the first group, with digit pairs 00–09, are more numerous, more likely to be uninsured, and more likely to live in lower-income ZIP

[4]Online Appendix Table 1 indicates that ignoring the direct deposit schedule entirely would yield similar overall conclusions.

[5]The qualifying income included Social Security income.

[6]The dataset includes all visits at hospitals regulated by OSHPD. Only Veterans' Hospitals, Prison Hospitals, and State Hospitals are excluded from coverage.

[7]Emergency department patients who are admitted to the hospital appear only in the inpatient data.

codes. Those differences exist because the first group includes not only patients with SSN's ending in 00–09 but also patients with no SSN recorded. The first group thus contributes a disproportionate share of measurement error, since patients with missing SSN's are either not affected by the stimulus payments (for instance, if they are illegal immigrants) or are randomly assigned to other mailing dates. We thus drop that SSN group from the analysis.[8]

The remaining rows of Table 2 demonstrate that the other SSN groups have similar characteristics. That comparison is reassuring, because the SSN groups are randomly assigned.

3 The Effect of the Stimulus Payments on Hospital Utilization

This section presents our main empirical results. We first demonstrate the effect of the stimulus payments on ED visits and hospitalizations. We then investigate the mechanisms involved. To do so, we decompose visits by characteristics of the patients and their medical conditions.

3.1 The Effect of the Stimulus Payments on Total Visits

In principle, the impact of the stimulus payments could arise and fade at any delay after payment receipt. We seek to be fully agnostic about these dynamics, and we use several approaches to study the effects. First, we run a standard difference-in-difference specification to measure the average effect of the stimulus payments over the follow-up observation period. We aggregate the data to counts of visits by SSN group and week, Y_{gt}, and estimate

$$\log\left(Y_{gt}\right) = \beta_1 \cdot I\left\{\text{Check Sent}\right\}_{gt} + \beta_2 \cdot I\left\{\text{Direct Deposit Sent}\right\}_{gt} + \alpha_t + \alpha_g + \varepsilon_{gt}.$$

[8] Online Appendix Table 3 presents our main results with this first SSN-group added back in. The results are generally not sensitive to the exclusion of this group.

This regression includes a fixed effect for each week, α_t, and a fixed effect for each SSN group, α_g. The indicator functions, $I\left\{\text{Check Sent}\right\}_{gt}$ and $I\left\{\text{Direct Deposit Sent}\right\}_{gt}$, indicate, respectively, whether checks were mailed and whether the direct deposits were made to group g by time t. We thus interpret the point estimates as the percentage change in utilization for groups that have received their stimulus payments relative to groups that have not yet received their payments. The fixed effects control for seasonality in hospital utilization and variation driven by differences in the size of the groups.

Table 3 reports estimates of this specification. Each cell of the table presents an estimate of β_1 when the logarithm of ED visits, inpatient visits, or all visits is the dependent variable.[9] The first column demonstrates that after the stimulus payments are mailed, total ED visits increase by 1.1% (p-value of 0.036), over a baseline average of 95,076 visits per week. Inpatient visits increase by less than one percent, a change that is not statistically significant at the 5-percent level. ED and inpatient visits combined increase by 0.9%, over a baseline of 141,982 visits per week, implying an increase of nearly 1,200 visits per week on average for 23 weeks after the checks were sent. The remaining columns of Table 3 present estimates of β_1 separately for visits by adult men and adult women. Both genders experienced a roughly 1% increase in ED visits; both estimates are statistically significant at the 5-percent level.

Younger patients are not matched to the SSN group of their parents. Reassuringly, we find no statistically significant change in visits for children (p-value of 0.41). For all remaining estimates, we focus solely on visits by adults.

These difference-in-difference estimates assume that the stimulus payments have a constant, persistent effect on hospital visits. The treatment effect, however, may

[9] In all specifications throughout the paper, estimates of β_2 are statistically insignificant so we do not report them. There may be less statistical power to detect an effect of the direct deposit payments because the deposits were made over only three weeks and they may have been less salient to recipients.

not be constant, for instance decaying as time passes.

Our second empirical approach attempts to measure the dynamics of the response to the stimulus payments. We estimate distributed-lag specifications, by replacing $I\{\text{Check Sent}\}_{gt}$ in the regression equation above with a series of indicator functions that are equal to one if the hospital visit occurred 1–2 weeks before rebate receipt, the week of rebate receipt or 1 week after, 2–3 weeks after rebate receipt, and so on.

Figure 1 presents the point estimates from this regression, for all visits and separately by gender. In each panel, the solid line plots the point estimates, whereas the dashed lines plot 95-percent confidence intervals. The omitted lag in each regression is the period immediately prior to the week in which the stimulus payments were sent.

In all panels, the probability of an ED visit becomes positive and statistically significant within 5 weeks after the stimulus payments are sent. Some delay in the impact may be caused by the time required for households to receive and cash the stimulus checks. Alternatively, the payments may alter families' monthly budgets, and the surplus may only become salient at the end of the month. For all visits, we observe a statistically significant 2-percent increase in emergency visits in weeks 4 and 5 after the rebates. The modest pre-trends discernible here are consistent with anticipation of stimulus payments by some households. For men, the figures surprisingly suggest a permanent effect of the payments on ED visits. But the confidence intervals after the first month are wide. We view such long-term estimates as speculative, because we possess no true control group after all groups receive their checks.[10]

The estimates above rely on a proxy for when individuals would have been sent their stimulus checks if they received a payment based on their own SSN. But the

[10] Online Appendix Table 4 reports estimates of an alternative functional form, which allows for exponential decay of an initial effect. The point estimates in that table also show a statistically significant initial increase in ED visits of 1.1%. The decay rate takes the wrong sign in that specification, but is not statistically different from 0.

actual number of individuals treated (by being sent a check) differs from the number identified by our research design. The regressions above capture the intent-to-treat effect of the payments on health care consumption. If we observed actual payment receipt, we could scale the intent-to-treat effect by the share of individuals who received a payment, to estimate the treatment effect on the treated.[11] Relative to the treatment effect on the treated, the intent-to-treat effect is scaled towards zero by the probability of actual check receipt. Several considerations affect this scaling factor.

First, Parker et al. (2011) report that roughly 85% of households received a stimulus payment and 60% of households received the payments via paper check. Second, in married households receiving stimulus payments, the date the check was sent was determined by the first SSN listed on the joint income tax return (IRS, 2008). Either spouse could be listed first on a joint return, and in 2008, 38% of returns were joint. This implies that 16% of patients in the California data were matched to their own SSN group when the date they received a stimulus check would have been determined by their spouse's SSN group.[12]

If the causal effect of actual check receipt on hospital visits were identical across households, and stimulus payment amount, receipt by paper check, marital status, and ordering of spousal SSN's on the tax return are independent, then our reduced-form estimates could be scaled up by $\frac{1}{0.85\times0.6\times0.84} = 2.33$ in order to obtain a scaled estimate. This scale-up would apply both for the difference-in-difference and distributed-lag specifications. Multiplying, our estimate of a 1.3% effect from the difference-in-difference estimates implies that the effect of actually receiving a stimulus payment is 3.15%.

[11] In estimating effects of the stimulus payments using the Consumer Expenditure Survey, Parker et al. (2011) run instrumental variables regressions, instrumenting for payment amount with an indicator for (randomly assigned) payment receipt.

[12] In fact, men and women might not have been equally likely to be listed first on their joint income tax document, in which case the reduced-form coefficients could be scaled differently by gender.

This number differs from the average treatment effect for several reasons. First, paper check recipients differ systematically from the general population. Parker et al. (2011) indicate that direct deposit recipients had higher incomes than paper check recipients, similar family sizes, and slightly larger stimulus payment amounts. Households without sufficient qualifying income to receive a stimulus payment, and households with sufficient income to be above the phase-out would likely have had different responses as well. Second, the reduced-form regressions are biased toward zero because of measurement error and because some check recipients may have begun to change consumption behavior in anticipation of their checks. It is not clear which of these reasons for differences from the average treatment effect dominate, but they go in offsetting directions.

Taken together, our results provide evidence of an increase in hospital utilization caused by modest liquidity shocks. We next investigate the mechanisms for this effect by testing for variation in treatment effects by medical condition and patient characteristics.

3.2 The Effect of the Stimulus Payments by Medical Condition

Section 1 discussed two possible mechanisms for the increase in ED visits. First, the stimulus payments may have relaxed household liquidity constraints, in which case the increase in visits may be driven directly by an increase in demand for primary care or by the treatment of chronic conditions.[13] Alternatively, the increase in ED visits may be driven indirectly, by a change in non-health consumption patterns that may affect health care needs. For instance, if the stimulus payments increased general activity, then that consumption itself may lead to an increase in hospital utilization to

[13]Primary care is more likely to be consumed in a clinic or private doctor's office than in an ED. Nevertheless, emergency departments are the source for much primary care (Grumbach et al., 1983). We use the Consumer Expenditure Survey later to present estimates of the effect of the 2008 tax rebates on outpatient visits to physicians. The results suggest little effect.

treat new or newly aggravated health conditions. This section distinguishes between these direct and indirect channels by comparing the types of medical conditions that drive the increase in ED visits.

We classify visits in the data using three proxies for each visit's cause. First, we isolate visits that are related to a chronic condition.[14] Second, we categorize visits as alcohol- or drug-related using the same criteria as Dobkin and Puller (2007).[15] Finally, we classify some hospital visits as "avoidable" following Aizer (2007), Kolstad and Kowalski (2010), and Dafny and Gruber (2005), amongst others. Avoidable hospital visits are visits that could have been prevented with timely care outside of the hospital. For instance, an adult visit for asthma is classified as avoidable.

Table 4 presents estimates of β_1 when the sample is restricted to visits that do and do not fall into these three categories. Columns 1a and 1b present estimates for visits linked to chronic conditions and visits not linked to chronic conditions. The results suggest that chronic and non-chronic conditions are roughly equal contributors to the 1.1% overall increase in ED visits. But the increase in ED visits linked to chronic conditions is not statistically significant at conventional levels.

Column 2a shows that the percentage increase in visits is especially large for drug- and alcohol-related medical conditions. In the emergency department, such visits increase by nearly 6% after the stimulus payments.[16] This estimate is surprising, given that Parker et al. (2011) estimate only a 0.9% marginal propensity to consume on alcohol out of the 2008 stimulus payments. At the same time (Column 2b), visits unrelated to drugs or alcohol increase by nearly 1% in the ED. Drugs and alcohol

[14] We rely on computer code published by the Agency for Health, Research, and Quality that links International Classification of Diseases 9$^{\text{th}}$ Revision (ICD-9) codes to an indicator for whether the visit is likely related to an underlying chronic condition.

[15] We use the following ICD-9 CM codes: cocaine (304.2, 305.6), opioid (304.0, 304.7, 305.5), amphetamines (304.4, 305.7), alcohol (291, 303, 305.0), and drug dependence or psychosis (304, 292).

[16] Drug- and alcohol-related visits account for roughly 4% of all ED visits. Drug- and alcohol-related inpatient visits, in contrast, do not increase.

thus contribute a notable share of the total increase in ED visits, but because of the low baseline share of these conditions in ED visits, we estimate that they account for only one-fifth of the overall increase in visits.[17]

Finally, Columns 3a and 3b of Table 4 document that the overall effect on emergency visits is not driven by avoidable hospitalizations. All estimates for avoidable visits are close to zero. The confidence interval for all hospital visits (in the third panel of Table 4) rules out a change in avoidable hospitalizations greater than 1.3%.

To measure the relevant adjustment dynamics for the outcomes studied in Table 4, we again estimate distributed-lag models. Figure 2 presents the results of such models for emergency department visits. The figure generally demonstrates similar patterns as in Figure 1. For all outcomes except avoidable visits, we observe a statistically significant increase in visits around one month after the payments were sent. In all cases, the effect of the stimulus payments 9 weeks after they are sent is statistically insignificant at the 5-percent level. Still, the magnitude of the point estimates suggests that the risk of a visit did not return to baseline. For instance, the point estimates suggests that drug- and alcohol-visits increased by roughly 5% for weeks after the stimulus payments. This pattern is consistent with the finding of Parker et al. (2011) that some of the consumption impact of the stimulus is detectable at a one-quarter lag. That said, the confidence intervals in all of the figures widen in the weeks after the checks are distributed. More importantly, after 9 weeks, all groups have been sent their checks, and thus we possess no control group that has not yet received its check. We thus view the long-term estimates as speculative rather than conclusive.

In summary, Table 4 and the associated figures imply that the increase in ED visits overall is driven by visits that tend: (1) not to be related to chronic conditions, (2) to

[17] Online Appendix Table 2 shows that the point estimates are very similar when drug-related visits and alcohol-related visits are examined separately. Not surprisingly, statistical power falls, and only the alcohol effect is significant on its own.

be drug- and alcohol-related, and (3) not to be avoidable. All of these characteristics point to a health response that is characterized less by a direct response to liquidity as by an indirect response. The results suggest that the stimulus payments changed households' consumption, which in turn worsened health and increased hospital utilization.

3.3 The Effect of the Stimulus Payments by Patient Characteristics

This section tests which patients are responsible for the results above. We divide patients by proxies of their socio-economic status. Specifically, we match each patient to their ZIP code of residence, and each ZIP code to the median household income recorded in the 2000 census. We define a ZIP code as low-income if its median household income is between percentiles 0 through 30 of the ZIP code income distribution, middle-income if its median income is between percentiles 31 through 70, and high-income if its median income is between percentiles 71 through 100. We also separate patients by the insurance status recorded in the administrative data (privately insured, publicly insured, or uninsured). If liquidity constraints constitute barriers to care, they would be most consequential for the uninsured and those with low income.

Table 5 presents estimates when we separate the patients by income. The first three columns present the treatment effect for patients from low-income, middle-income, and high-income ZIP codes. The point estimates present no clear pattern in treatment effects. In the emergency department, the stimulus payments led to a roughly equal increase in visits across income categories. Any increase in inpatient visits, however, was driven solely by residents of low-income ZIP codes.

The remaining columns of Table 5 present estimates by insurance status of the patient. In the emergency department, the estimated treatment effects are nearly identical for publicly insured and uninsured, but the effect of the stimulus payments on privately insured visits is close to zero. Estimates for inpatient visits, in contrast, are more variable.

Figure 3 presents distributed-lag estimates for these outcomes in the emergency department. The figure makes clear that ED visits by publicly insured patients increased dramatically after the checks were sent. In contrast, the pattern of point estimates for privately insured and uninsured patients are much less precise. Second, while the point estimates in Table 5 are similar across income groups, the dynamics indicate that only low-income patients exhibited a short-lived, statistically significant increase in ED visits.

Taken as a whole, Table 5 and Figure 3 do not suggest a clear pattern based on socioeconomic status or insurance status. The figures suggest that the treatment effect was concentrated in lower-income groups, but the simple difference-in-difference results do not demonstrate such a contrast. Moreover, the pattern of estimates is similar between uninsured and publicly insured patients, even though financial barriers to care vary by insurance status. For instance, the publicly insured typically face the lowest co-payments and deductibles, and yet Table 5 suggests that publicly insured visits were affected in a similar manner as uninsured visits. This further suggests that the overall treatment effect is not driven directly by the relaxation of liquidity constraints.

4 Evidence from Expenditure Data

The previous section demonstrates that the 2008 stimulus payments affected the rate at which households visited the emergency department. The data, however, do not allow us to test how the stimulus payments affected other forms of health care utilization. Consumers may have also changed the rate at which they consumed preventive care, office visits, and medication. In particular, they may have substituted visits to the emergency department for other forms of less expensive care outside of the hospital. This section attempts to rule out such a possibility, and thereby reinforce the conclusion that worsening health was the reason for increased emergency care episodes.

We perform this test by replicating and extending the analysis of Parker et al. (2011) on the expenditure effects of the 2008 stimulus payments. Parker et al. (2011) use the Consumer Expenditure Survey (CEX), which included a special module with questions on the stimulus payments. A disadvantage of the CEX is its low (quarterly) time resolution. The CEX allows confident estimation of only the contemporaneous effect of the tax rebate and one, quarter-long lag. On the other hand, the CEX asks respondents about their expenditures in many categories of health care.

We estimate the change in health care consumption per dollar of stimulus payment.[18] Specifically, we regress changes in nine categories of health-related expenditures on the contemporaneous and once-lagged tax rebate amount. Following Parker et al. (2011), we use indicators for tax rebate receipt and lagged tax rebate receipt as instrumental variables. We control for age, the change in the number of adults in the household, the change in the number of children in the household, and a full set of indicator variables for the month of the survey interview.

Table 6 presents estimates of these regressions. The first column replicates the estimates of Parker et al. (2011). It demonstrates that \$0.023 of each stimulus payment dollar was spent on health in the two quarters after rebate receipt.[19] That change in spending is not statistically significant at conventional levels; the p-value is 0.397.

The remainder of Table 6 decomposes this increase in health expenditures into subcategories.[20] Columns 2 through 5 report the effects on the four health sub-

[18]Because the CEX includes questions about the payments themselves, we can estimate not only the effect of stimulus payment eligibility during a given week, but also the effect of the payments.

[19]The dependent variable in Table 6 is a change in expenditure.

[20]Note that all of this spending is out-of-pocket.

categories included in the CEX FMLY data files: health insurance, medical services, prescription drugs, and medical supplies. Using the CEX MTAB data files, we further decompose the medical services expenditures into the outcomes listed in Columns 6 through 9.

None of the estimates in Columns 1 through 9 are statistically significant at conventional levels, and the point estimates tend to be economically small. The results provide no evidence of a change (in either direction) in outpatient care as a result of the stimulus payments. Parker et al. (2011) do document large consumption responses in other areas, like car purchases, without an obvious connection to hospital utilization. Consistent with our evidence from the hospital utilization data of an increase in alcohol- and drug-related emergency visits, the final Column in Table 6 replicates Parker et al. (2011)'s finding that the stimulus payments did have a significant effect on the purchase of alcoholic beverages. We estimate that \$0.011 of every stimulus payment dollar was spent on alcohol.[21]

5 Discussion

We find that the 2008 stimulus payments increased emergency department visits by over one percent, a result that is difficult to reconcile with the LCH/PIH. The increase was driven by non-discretionary visits and did not differ by insurance status. This suggests that the stimulus payments did not affect health care consumption directly by expanding the short-run budget set. Rather, the effect was indirect: the payments provided liquidity that was dangerous to some recipients' health, leading to additional

[21] The 2001 tax rebates were distributed in a manner similar to the 2008 stimulus payments, based on the last two digits of filers' SSN's. Johnson et al. (2006) measure the effects of the 2001 rebates in a manner similar to Parker et al. (2011). We have analogously replicated and extended that analysis, and the results are included in Online Appendix Table 5 for comparison. The details differ, perhaps because the 2001 rebates were smaller and expected to be long-lasting, while the 2008 stimulus payments were a larger, temporary response to on-rushing recession. But the overall message is similar: there is no significant change in outpatient substitutes for emergency visits.

emergency care.

These findings have several implications. First, optimal health policy depends in part on how health care utilization responds to household finances. Co-payments and deductibles are often viewed as useful for improving the efficiency of decisions to seek medical care. However, to the extent that hospital utilization is not affected through the direct channel of an expanded short-run budget set, co-payments, deductibles, and other deviations from full insurance may be less useful.

Second, we contribute to the literature on the effects of fiscal policy. In 2008, the economic stimulus payments distributed by paper check appear to have caused a statistically significant, small increase in hospital utilization. Because hospital visits are relatively severe indicators of health, this result may have been counterbalanced by improvements in the rest of the health distribution that we do not observe. The effects we find are too small to speak decisively to debates about the overall effectiveness of fiscal policy. Instead, our findings are best viewed as an example of how the effects of fiscal policy can be detected beyond standard economic variables, for example on proxies for well-being.

Finally, the finding that liquidity can be dangerous has implications for the implementation of transfer programs. Payments made by direct deposit or electronic benefit transfer might be less salient than those made by paper check, and they also might receive mental accounting treatment that makes them less susceptible to use for risky consumption (Sahm et al., forthcoming) . Of course, the purpose of the Economic Stimulus Act of 2008 was to encourage consumption broadly. The question of how transfer design can engender the optimal mix of consumption is ripe for future research.

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Table 1: Dates When Economic Stimulus Payments were Sent in 2008

Last 2 digits of filer's SSN	Paper check sent on:	deposit made on:
00 - 09	16-May	2-May
10 - 18	23-May	2-May
19 - 20	30-May	2-May
21 - 25	30-May	9-May
26 - 38	6-Jun	9-May
39 - 51	13-Jun	9-May
52 - 63	20-Jun	9-May
64 - 75	27-Jun	9-May
76 - 87	4-Jul	16-May
88 - 99	11-Jul	16-May

Table 2: Hospital Data Summary Statistics

	Visits	Visits per week	SSN digit pairs in group	Visits per digit pair	Zip code median income	Share uninsured
All Visits	7,142,097	248,730			46,424	0.147
ED Visits	4,944,481	172,196			45,919	0.197
Inpatient Visits	2,197,616	76,534			47,558	0.042
Digits 00-09	2,152,198	74,952	10	215,220	46,015	0.218
Digits 10-18	498,862	17,373	9	55,429	46,574	0.130
Digits 19-25	388,579	13,533	7	55,511	46,624	0.132
Digits 26-38	720,986	25,109	13	55,460	46,633	0.132
Digits 39-51	718,275	25,015	13	55,252	46,611	0.130
Digits 52-63	666,758	23,220	12	55,563	46,567	0.131
Digits 64-75	664,519	23,142	12	55,377	46,609	0.128
Digits 76-87	663,665	23,113	12	55,305	46,634	0.131
Digits 88-99	662,211	23,062	12	55,184	46,548	0.130

Note: The data, from the California Office of Statewide Health Planning and Development, consist of a near-census of administrative records on California hospital visits in 2008. Income data are merged at the zip code level from the Census. The SSN group with Digits 00-09 is excluded from all analysis except in Online Appendix Table 3.

Table 3: The Effect of the Stimulus Payments on Hospital Visits

	(1) All Adult Visits	(2) Men	(3) Women
	A. Dependent variable: Logarithm of ED visits		
After Check Sent	0.011	0.011	0.010
	(0.004)	(0.005)	(0.005)
	[0.036]	[0.073]	[0.093]
Avg. Visits / Week	95,076	40,822	54,250
	B. Dependent variable: Logarithm of inpatient visits		
After Check Sent	0.006	0.005	0.006
	(0.004)	(0.009)	(0.006)
	[0.183]	[0.636]	[0.294]
Avg. Visits / Week	46,906	18,669	28,236
	C. Dependent variable: Logarithm of all visits		
After Check Sent	0.009	0.009	0.009
	(0.003)	(0.004)	(0.005)
	[0.024]	[0.072]	[0.119]
Avg. Visits / Week	141,982	59,491	82,486

Note: This table reports estimates from difference-in-difference regressions. In each case the sample consists of counts of California hospital visits by SSN-group and week, covering 19 weeks before and 23 weeks after the rebates were sent. Full sets of SSN-group fixed effects, week fixed effects, and an indicator for whether direct deposits have been made are also included in the regressions. N = 9 × (1+19+23) = 387. The standard errors in parentheses adjust for correlation between observations from the same SSN group. Associated *p*-values in brackets.

Table 4: The Effect of the Stimulus Payments by Medical Condition

	(1a) Related to Chronic Conditions	(1b) Not Related to Chronic Conditions	(2a) Drug- and Alcohol-Related	(2b) Not Drug- or Alcohol-Related	(3a) Avoidable	(3b) Not Avoidable
			A. Dependent variable: Logarithm of ED visits			
After Check Sent	0.013	0.010	0.062	0.009	0.001	0.014
	(0.013)	(0.004)	(0.021)	(0.004)	(0.006)	(0.006)
	[0.346]	[0.031]	[0.019]	[0.071]	[0.913]	[0.053]
Avg. Visits / Week	15,005	80,071	3,528	91,548	22,694	72,382
			B. Dependent variable: Logarithm of inpatient visits			
After Check Sent	0.001	0.009	- 0.002	0.006	0.004	0.006
	(0.009)	(0.005)	(0.013)	(0.004)	(0.012)	(0.004)
	[0.921]	[0.093]	[0.892]	[0.163]	[0.713]	[0.201]
Avg. Visits / Week	19,684	27,222	3,967	42,939	14,548	32,358
			C. Dependent variable: Logarithm of all visits			
After Check Sent	0.006	0.010	0.028	0.008	0.002	0.011
	(0.007)	(0.003)	(0.012)	(0.003)	(0.007)	(0.003)
	[0.431]	[0.016]	[0.046]	[0.045]	[0.765]	[0.011]
Avg. Visits / Week	34,689	107,293	7,495	134,487	37,242	104,740

Note: This table reports estimates from difference-in-difference regressions. Chronic, Drug-Related, and Avoidable conditions are defined in Section 3.2 of the text. In each case the sample consists of counts of California hospital visits by SSN-group and week, covering 19 weeks before and 23 weeks after the rebates were sent. Full sets of SSN-group fixed effects, week fixed effects, and an indicator for whether direct deposits have been made are also included in the regressions. N = 9 × (1+19+23) = 387. The standard errors in parentheses adjust for correlation between observations from the same SSN group. Associated p-values in brackets.

Table 5: The Effect of the Stimulus Payments by Patient Demographics

	(1)	(2)	(3)	(4)	(5)	(6)
	Low Income	Middle Income	High Income	Privately Insured	Publicly Insured	Uninsured
			A. Dependent variable: Logarithm of ED visits			
After Check Sent	0.006	0.009	0.010	0.004	0.017	0.011
	(0.005)	(0.005)	(0.008)	(0.007)	(0.005)	(0.008)
	[0.283]	[0.103]	[0.239]	[0.514]	[0.009]	[0.177]
Avg. Visits / Week	32,362	43,914	33,065	39,913	38,373	16,790
			B. Dependent variable: Logarithm of inpatient visits			
After Check Sent	0.013	0.005	- 0.008	0.007	0.006	- 0.018
	(0.006)	(0.008)	(0.007)	(0.011)	(0.004)	(0.018)
	[0.071]	[0.538]	[0.271]	[0.516]	[0.127]	[0.340]
Avg. Visits / Week	13,742	19,096	14,919	15,736	29,498	1,672
			C. Dependent variable: Logarithm of all visits			
After Check Sent	0.008	0.008	0.005	0.005	0.012	0.009
	(0.005)	(0.004)	(0.005)	(0.007)	(0.003)	(0.007)
	[0.132]	[0.068]	[0.413]	[0.477]	[0.002]	[0.250]
Avg. Visits / Week	46,104	63,010	47,984	55,649	67,871	18,461

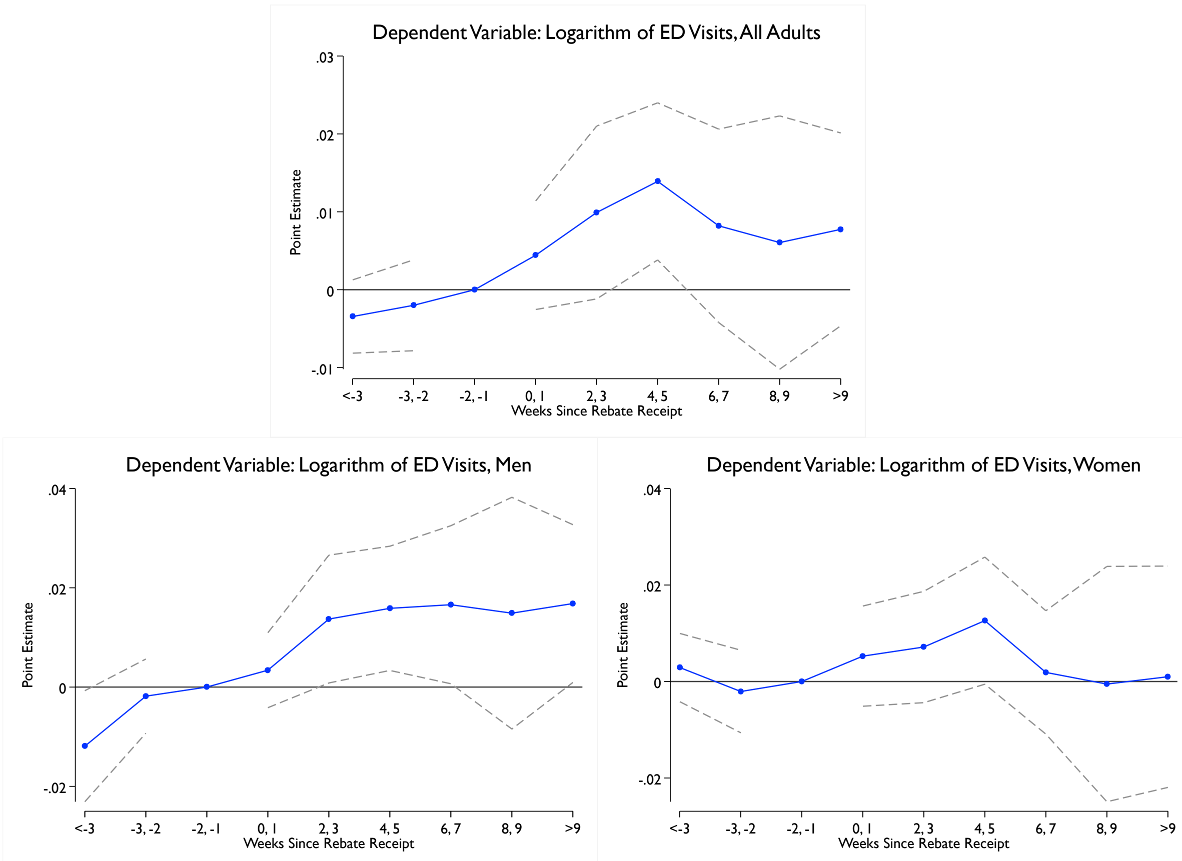
Note: This table reports estimates from difference-in-difference regressions. Low-, Middle-, and High-Income groups live in zip codes with median income between the 0-30th, 30-70th, and 70-99th precentiles, respectively. In each case the sample consists of counts of California hospital visits by SSN-group and week, covering 19 weeks before and 23 weeks after the rebates were sent. Full sets of SSN-group fixed effects, week fixed effects, and an indicator for whether direct deposits have been made are also included in the regressions. N = 9 × (1+19+23) = 387. The standard errors in parentheses adjust for correlation between observations from the same SSN

Table 6: The Effect of the 2008 Economic Stimulus Payments on Health Care Expenditure

	(1) All Health Expenditures	(2) Health Insurance	(3) Medical Services	(4) Prescription Drugs	(5) Medical Supplies	(6) Hospital Room & Meals	(7) Hospital Services	(8) Physician Services	(9) Dental Services	(10) Alcoholic Beverages
Rebate Effect	0.023	-0.003	0.006	0.010	0.010	0.002	-0.004	-0.005	0.005	0.011
	(0.028)	(0.012)	(0.022)	(0.007)	(0.007)	(0.010)	(0.005)	(0.006)	(0.013)	(0.005)
	[0.397]	[0.804]	[0.787]	[0.155]	[0.156]	[0.881]	[0.418]	[0.413]	[0.685]	[0.018]
Share of Nondurable Expenditure	0.129	0.081	0.027	0.017	0.003	0.003	0.000	0.008	0.009	0.015
Expenditure ($)	761.33	437.78	201.71	98.02	23.82	28.63	6.45	49.97	71.65	85.42

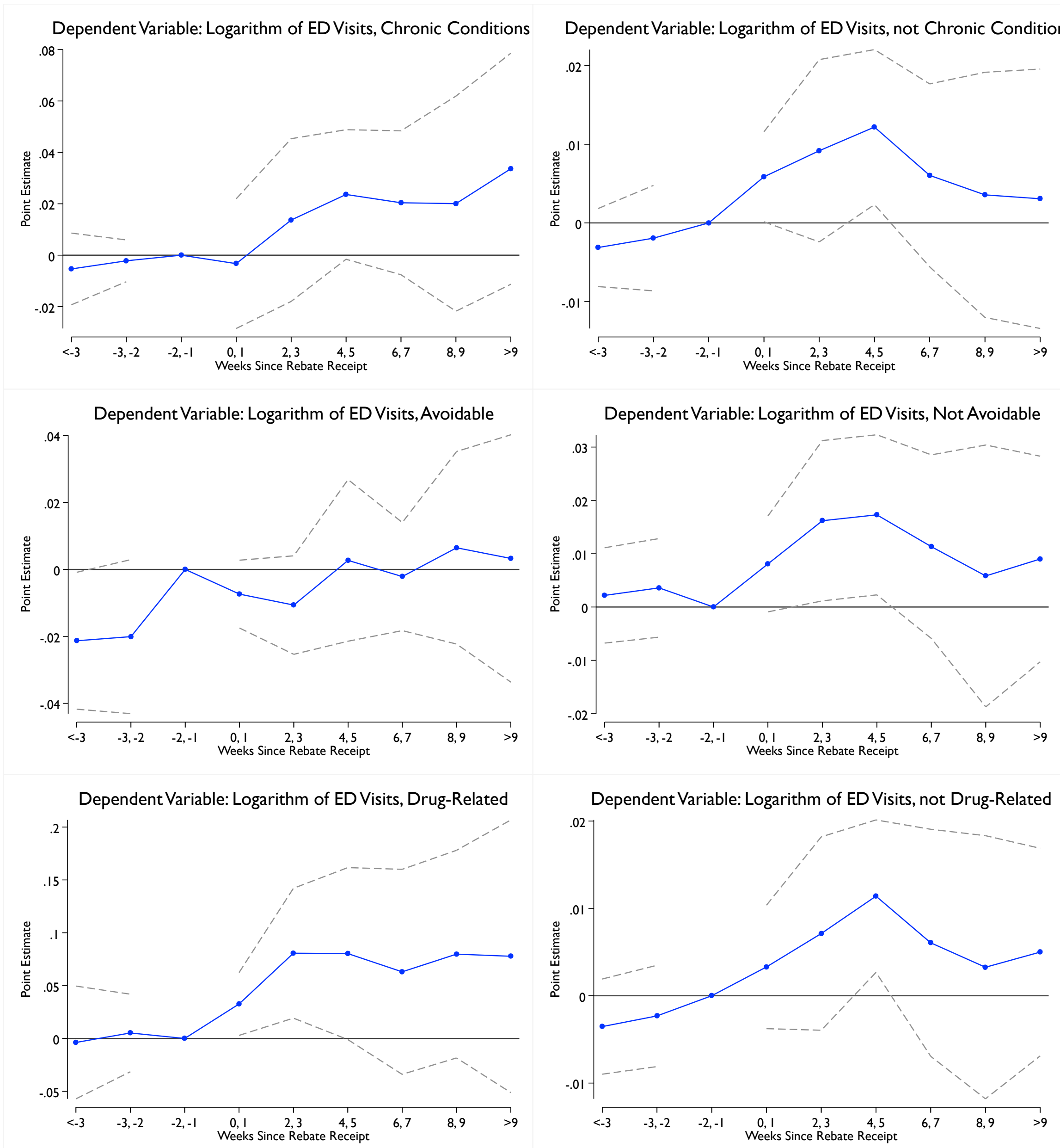
Note: This table presents estimates based on the Consumer Expenditure Survey. The estimates extend specifications reported by Parker, Johnson, Souleles, and McClelland (2011) to subcategories of health care expenditure. Each column contains the results from a regression of a change in expenditure on the contemporaneous tax rebate amount, using an indicator for receiving any tax rebate as an exogenous instrument. The regressions control for age, the change in the number of adults in the household, the change in the number of children in the household, and indicator variables for the month of the survey interview. Column 1 approximately replicates the penultimate column of Table 8 in Parker et al. (2011). Columns 2-5 partition Column 1, and Columns 6-9 are components of Column 3. N = 18,532 for all specifications. Standard errors are in parentheses and p-values are in brackets.

Figure 1: Distributed Lag Estimates by Gender



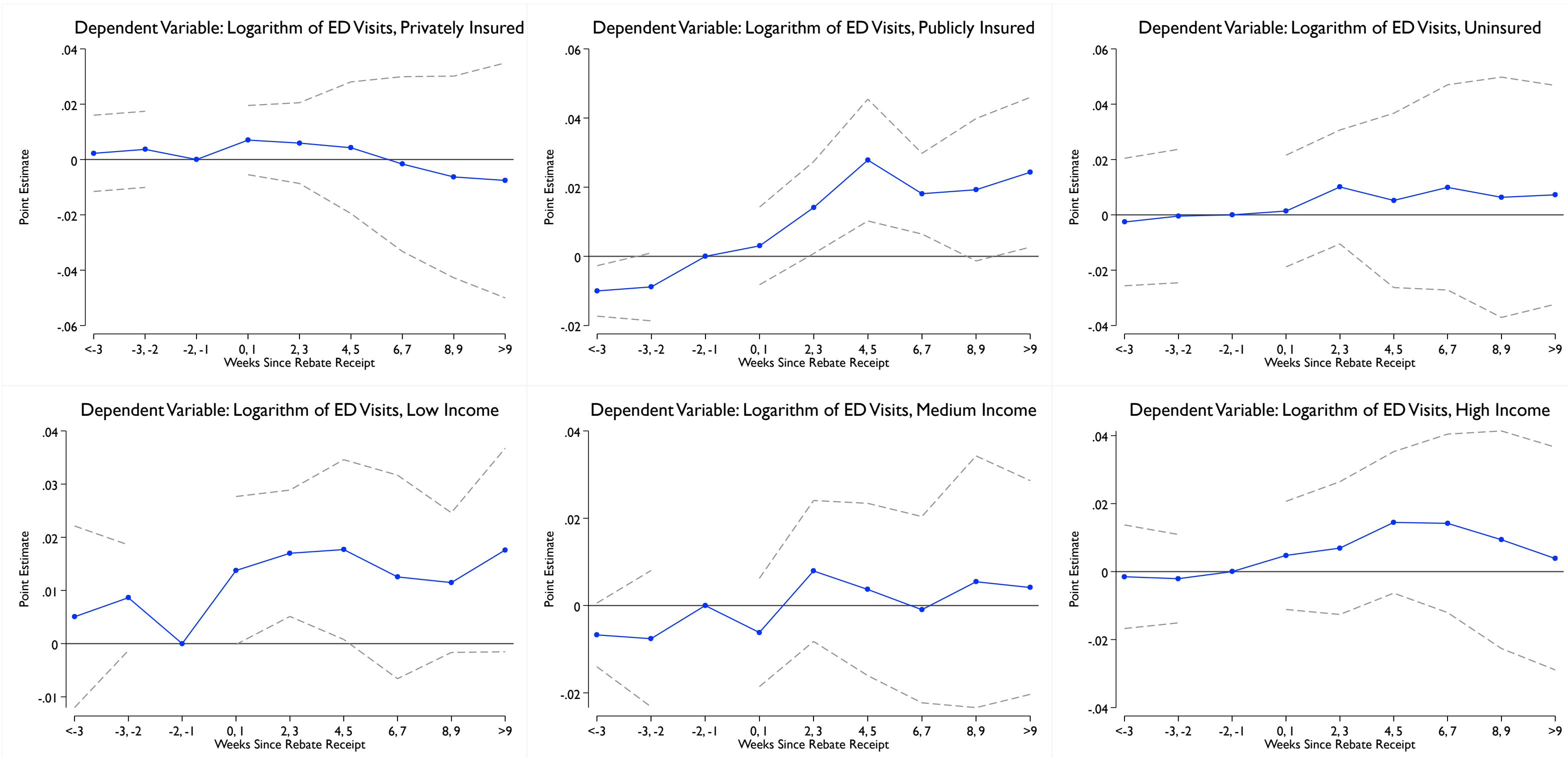
Note: Each figure plots point estimates from a regression of log counts of visits on a set of indicators for two-week intervals. The dotted lines plot 95-percent confidence intervals that are robust to autocorrelation between observations from the same SSN group. SSN-group fixed effects, week fixed effects, and an indicator for whether direct deposits had been made are also included in the regressions. The omitted time period is 1 and 2 weeks before rebate checks were sent.

Figure 2: Distributed Lag Estimates by Medical Condition



Note: Each figure plots point estimates from a regression of log counts of visits on a set of indicators for two-week intervals. The dotted lines plots 95% confidence intervals that are robust to autocorrelation between observations from the same SSN group. SSN-group fixed effects, week fixed effects, and an indicator for whether direct deposits had been made are also included in the regressions. The omitted time period is 1 and 2 weeks before rebate checks were sent. Chronic, avoidable, and drug-related conditions are described in Section 3.2 of the text.

Figure 3: Distributed Lag Estimates by Patient Demographics



Note: Each figure plots point estimates from a regression of log counts of visits on a set of indicators for two-week intervals. The dotted lines plot 95% confidence intervals that are robust to autocorrelation between observations from the same SSN group. SSN-group fixed effects, week fixed effects, and an indicator for whether direct deposits had been made are also included in the regressions. The omitted time period is 1 and 2 weeks before rebate checks were sent. Zip codes are defined as low, middle, and high-income if their median household income is between percentiles 0-30, 30-70, and 70-99 of the zip code income distribution, respectively.

Online Appendix Table 1: Diff-in-Diff Specification with and without Direct Deposit Control

Dependent variable: Logarithm of ED visits for all adults

	(1)	(2)
After Check Sent	0.013	0.011
	(0.003)	(0.004)
	[0.005]	[0.036]
After Direct Deposit		0.012
		(0.015)
		[0.429]

Note: This table reports estimates from difference-in-difference regressions. In each case the sample consists of counts of CA hospital visits by SSN-group and week, covering 19 weeks before and 23 weeks after the rebates were sent. Full sets of SSN-group fixed effects and week fixed effects are also included in the regressions. The first column includes the first SSN group, the second column does not. N = 9 × (1+19+23) = 387. The standard errors in parantheses adjust for correlation between observations from the same SSN group. Associated *p* -values in brackets.

Online Appendix Table 2: The Effect of the Stimulus Payments on Alcohol versus Drugs

Dependent variable: Logarithm of ED visits for the given cause

	(1) Drugs and Alcohol	(2) Drugs	(3) Alcohol
After Check Sent	0.0622 (0.0211) [0.0186]	0.0624 (0.0413) [0.1695]	0.0615 (0.0255) [0.0421]
R^2	0.982	0.897	0.979

Note: This table reports estimates from difference-in-difference regressions. In each case the sample consists of counts of CA hospital visits by SSN-group and week, covering 19 weeks before and 23 weeks after the rebates were sent. Full sets of SSN-group fixed effects and week fixed effects are also included in the regressions. N = 9 × (1+19+23) = 387. The standard errors in parantheses adjust for correlation between observations from the same SSN group.

Online Appendix Table 3: The Effect of the Stimulus Payments on ED Visits
Including Digit Pairs 00-09

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Replication of Table 3			
	All Adult Visits	Men	Women			
After Check Sent	0.012	0.011	0.013			
	(0.003)	(0.005)	(0.004)			
	[0.005]	[0.045]	[0.021]			
Avg. Visits / Week	116,237	50,156	66,077			
			B. Replication of Table 4			
	Chronic Conditions	Not Chronic Conditions	Drug-Related	Not Drug-Related	Avoidable	Not Avoidable
After Check Sent	0.012	0.012	0.060	0.010	0.001	0.015
	(0.010)	(0.003)	(0.017)	(0.003)	(0.006)	(0.005)
	[0.294]	[0.004]	[0.006]	[0.013]	[0.898]	[0.009]
Avg. Visits / Week	18,098	98,140	4,518	111,719	27,248	88,990
			C. Replication of Table 5			
	Low Income	Middle Income	High Income	Privately Insured	Publicly Insured	Not Insured
After Check Sent	0.011	0.014	0.015	0.009	0.013	0.014
	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)	(0.006)
	[0.108]	[0.039]	[0.092]	[0.213]	[0.022]	[0.060]
Avg. Visits / Week	45,883	61,877	46,286	47,235	46,190	22,812

Note: Each cell presents a regression with the logarithm of ED visits from the given category as the outcome of interest. Panels A, B, and C replicate the ED results in Tables 3, 4, and 5, respectively, while including the first SSN-group with digits 00-09 in the analysis. Full sets of SSN-group fixed effects and week fixed effects are included in the regressions. N = 10 × (1+19+23) = 430. The standard errors in parantheses adjust for correlation between observations from the same SSN group. Associated p-values in brackets.

Online Appendix Table 4: The Effect of the Stimulus Payments on Hospital Visits, Exponential Decay Model

Dependent variable: Logarithm of given type of visit

	(1) ED	(2) Inpatient
Paper Check		
Effect	0.011	-0.001
	(0.005)	(0.003)
Rate of Decay	0.109	-0.133
	(0.186)	(0.096)
Electronic Funds Transfer		
Effect	-0.002	0.000
	(0.005)	(0.001)
Rate of Decay	-0.121	-1.929
	(0.070)	(0.106)
Constant	9.183	8.562
	(0.013)	(0.016)

Note: This table reports estimates from nonlinear regressions that allow for responses to the stimulus payments by paper check and electronic funds transfer that decay exponentially. In each case the sample consists of counts of California hospital visits by SSN-group and week, covering 19 weeks before and 23 weeks after the rebates were sent. Full sets of SSN-group fixed effects and week fixed effects are included. N = 9 × (1+19+23) = 387.

Online Appendix Table 5: The Effect of the 2001 and 2008 Liquidity Shocks on Health Care Expenditure

	(1) All Health Expenditures	(2) Health Insurance	(3) Medical Services	(4) Prescription Drugs	(5) Medical Supplies	(6) Hospital Room & Meals	(7) Hospital Services	(8) Physician Services	(9) Dental Services	(10) Alcoholic Beverages
					A. The 2001 Tax Rebates					
Rebate Effect	0.097	0.009	0.067	0.012	0.009	0.012	0.016	-0.010	0.041	0.004
	(0.040)	(0.020)	(0.031)	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.020)	(0.011)
	[0.014]	[0.633]	[0.031]	[0.166]	[0.345]	[0.229]	[0.118]	[0.273]	[0.043]	[0.729]
Share of Nondurable Expenditure	0.137	0.078	0.029	0.025	0.004	0.001	0.002	0.008	0.012	0.019
Expenditure ($)	575.04	298.65	159.22	96.22	20.95	8.20	12.58	36.48	64.49	84.20

Note: This table presents estimates based on the Consumer Expenditure Survey. The estimates extend specifications reported by Johnson, Parker, and Souleles (2006) to subcategories of health care expenditure. Specifically, each column contains the results from a regression of a change in expenditure on the contemporaneous tax rebate amount, using an indicator for receiving any tax rebate as an exogenous instrument. The regressions control for age, the change in the number of adults in the household, the change in the number of children in the household, and indicator variables for the month of the survey interview. Column 1 replicates the penultimate column of Table 6 in Johnson, Parker, and Souleles (2006). *N* = 12,370 for the 2001 CEX. Standard errors are in parentheses and p-values in brackets.