

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

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Abstract

Optimal health policy depends on whether and how liquidity constraints affect health care utilization. We measure the impact of liquidity constraints in this paper by exploiting the randomized timing of the 2008 Economic Stimulus Payments. We find that the payments raised the probability of an adult emergency department visit by an average of 1.3 percent throughout a 23-week post-payment observation period. This effect, identified at the individual level in administrative hospital data, 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 the increase is driven by visits for urgent medical conditions, especially but not exclusively including drug- and alcohol-related visits. Complementary evidence implies consumers are not simply substituting from outpatient doctor visits to hospital care when liquidity constraints relax. Our results suggest liquidity constraints do not constitute a direct barrier to care, but rather that liquidity can increase health care utilization indirectly by increasing the need for care.

JEL Codes: H51, D14, I18, D91. *Keywords:* Liquidity constraints, Health care demand, Emergency care, Permanent Income Hypothesis

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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 distribution of 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). Those two digits are quasi-randomly assigned. In exploiting this variation we follow Evans and Moore (2009b); Gross et al. (2011); and Parker et al. (2010); among others. Furthermore, Evans and Moore (2009a) and Dobkin and Puller (2007) provide evidence on the relationship between government transfers and health that is closely related to this paper. For most of our analysis, we rely on an administrative dataset that captures a near-census of hospitalizations and ED visits in California during 2008. The dataset includes a variable, based on the last two digits of each patient's SSN, that indicates when s/he would have received the payment.² This particular context allows us to identify the impact of liquidity

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

²In these administrative data from California we do not observe actual receipt of stimulus payments: our main results reflect the impact of having an SSN that would have resulted in stimulus payment receipt in a particular week, and can be scaled up to be comparable to income elasticities of health care demand.

shocks averaging \$900 on hospital utilization.

One might expect stimulus payments to have no more than a small effect on health care utilization for several reasons. First, the Life-Cycle Hypothesis/Permanent Income Hypothesis (LCH/PIH) would predict little effect, because the stimulus payments had little effect on lifetime income.³ Second, the effect of the stimulus payments on health care consumption would be especially small if health care demand is state-dependent or highly inelastic. Third, less than 10 percent of US health care costs are paid out of pocket (Centers for Medicare and Medicaid Services, 2009), and we would not expect changes in liquidity to affect the demand for insured care.

On the other hand, the stimulus payments could have a nontrivial effect on health care consumption for liquidity-constrained consumers. This effect could be driven by direct and indirect mechanisms. First, liquidity-constrained consumers may use their stimulus payment to purchase health care they had already needed but could not afford.⁴ 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 affect health care utilization indirectly, by first affecting health. This could occur if households use the stimulus payments to increase their consumption, and that consumption in turn affects the need for 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.

We find that the stimulus payments caused ED visits to increase by an average

³Hundreds of papers examine the effect of predictable changes in income on changes in overall consumption. In the study closest to ours, Parker et al. (2010) find an overall marginal propensity to consume out of the 2008 stimulus payments of 52%.

⁴This direct effect could increase hospitalizations and ED visits if liquidity-constrained individuals could not afford such health care before the stimulus 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.

of 1.3 percent. This amounts to over 40,000 additional ED visits in California. This impact is difficult to reconcile with the LCH/PIH. In contrast, it suggests that a portion of the population responds to transitory changes in liquidity.

Furthermore, our results suggest the increase in hospital utilization occurred indirectly via an increase in risky consumption patterns, rather than directly by mitigating liquidity constraints. Specifically, the stimulus payments had similar effects for people who were publicly insured, privately insured, and uninsured. In addition, we find that the payments caused a large increase in alcohol- and drug-related hospital visits, but did not change the risk of an avoidable hospitalization or hospital visits associated with chronic conditions. All of these results suggest the indirect mechanism.

Our findings complement previous studies on how household finances affect health and health care.⁵ Income transfers have previously been shown to affect mortality (Evans and Moore, 2009b), and recessions have been shown to affect both health and health care consumption (Ruhm, 2000; Miller et al., 2009; Lusardi et al., 2010; Dehejia and Muney, 2004).⁶ Second, Nyman (2002) has argued that both income effects and liquidity constraints may be central in how insured consumers respond to the price of health care. Third, Dobkin and Puller (2007) find that transfers through cash welfare programs increase hospitalizations for drug- and alcohol-related medical conditions. Fourth, Bundorf and Pauly (2006) test for the affordability of health insurance by studying positive liquidity shocks.

Finally, Johnson et al. (2006) examine the 2001 tax rebates and estimate that

⁵Our findings also relate to previous studies on the 2008 stimulus payments (Bertrand and Morse, 2009; Shapiro and Slemrod, 2009; Sahm et al., 2010, forthcoming).

⁶This paper is also related to a small literature that studies a cohort of retirees who received a sustained increase in Social Security income. Snyder and Evans (2006) examine how the increase in lifetime income affected mortality. Moran and Simon (2006) study how the increase affected the consumption of pharmaceuticals. In contrast to this paper, however, these studies focus on a change in lifetime income.

consumers spent nearly twenty percent of their rebate checks on health care. We replicate this result and extend it by estimating what types of health care goods were purchased. Consistent with our other results, one-third of the increase in health care expenditure was spent at hospitals. We estimate that another third was spent on dental services (which are less often covered by insurance), and one-sixth was spent on prescription drugs. The remaining sixth was spread across 20 other categories, of which one (for which the point estimate is not statistically significant) represents visits to doctors' offices. 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 be dangerous, indirectly increasing health care utilization because of increases in risky forms of other 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 our replication and extension of the results of Johnson et al. (2006). Section 5 discusses the implications of our findings and concludes.

2 Background on the Stimulus Payments and Hospital Data

With macroeconomic conditions beginning to deteriorate, the Economic Stimulus Act of 2008 was passed by bipartisan majorities in both houses of Congress on February 7, 2008, and signed into law less than a week later. Two-thirds of the \$152 billion in 2008 net outlays provided for in the bill, and more than 90% of the net outlays from 2008-2018, consisted of direct payments to households. These economic stimulus payments were distributed by the Internal Revenue Service according to a staggered

schedule determined by the last two digits of the head-of-household’s SSN. These digit-pairs are effectively randomly assigned.⁷ Paper checks were sent out over three months, as reported in Table 1. The first SSN group (digits 00–09) was sent its checks on the 16th of May and the final group (digits 88–99) on the 11th of July.

Households in 2008 could elect to receive their stimulus payments via direct deposit instead of mail. The direct deposit transfers were made on only three dates, listed in the third column of Table 1. Roughly 40 percent of households elected to receive their rebates via direct deposit (Parker et al., 2010). We find few statistically significant effects of the direct deposit payments. This is not surprising, because direct deposit recipients may be less likely to be liquidity constrained, and the three-week span for disbursing direct deposit payments may provide too little variation to observe an impact. We focus solely on the variation induced by paper checks throughout the remainder of the paper.

Households received stimulus payments in 2008 if they paid taxes or had sufficient qualifying income in 2007.⁸ 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 percent 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 2008.⁹ For each visit, we

⁷Johnson et al. (2006) and Parker et al. (2010) describe with great care how the payments were distributed. Here we summarize the most relevant facts.

⁸Notably, the qualifying income included Social Security income.

⁹The dataset includes all visits at hospitals regulated by OSHPD. Only Veterans’ Hospitals, Prison

observe the patient’s zip code of residence, gender, and insurance status, in addition to the medical condition and the exact date when the visit occurred.¹⁰ The data also include a categorical variable for each patient corresponding to one of the 12 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 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. The Appendix Table presents our main results with and without this first SSN-group. The results are generally not sensitive to the exclusion of this group, although most estimates become less precise when the first group is excluded.

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

Hospitals, and State Hospitals are excluded from coverage.

¹⁰Emergency department patients who are admitted to the hospital appear only in the inpatient data.

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 stratify visits by characteristics of the patients and their medical conditions.

3.1 The Effect of the Stimulus Payments on Total Visits

We first use a standard difference-in-difference approach to measure the effect of the stimulus payments. We aggregate the data to counts of visits by SSN group and week, Y_{gt} , and estimate:

$$\log(Y_{gt}) = \beta \cdot I\{\text{Check Sent}\}_{gt} + \alpha_t + \alpha_g + \varepsilon_{gt}.$$

This regression includes a fixed effect for each week, α_t , and a fixed effect for each SSN group, α_g . The indicator function, $I\{\text{Check Sent}\}_{gt}$, indicates whether checks were mailed to group g by time t . We thus interpret β 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 when the logarithm of ED visits, inpatient visits, or all visits is the dependent variable. The first column demonstrates that after the stimulus payments are mailed, total ED visits increase by 1.3 percent (p -value of 0.005), over a baseline average of 116,143 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 1.1%, over a baseline of 172,461 visits per week, implying an increase of nearly 1,900 ED visits per week extending for 23 weeks after the checks were sent. The remaining columns of Table 3 present estimates of β separately for visits by adult men and adult women. Both genders experienced a 1.3 percent 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 0.47). 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 not be constant, for instance decaying as time passes. For that reason, we pursue a second empirical approach to investigate the dynamic 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 such a 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.¹¹

In all panels, the probability of an ED visit is positive and becomes statistically significant within the 3 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

¹¹The response we estimate would be diminished if households anticipate the payments and adjust consumption before the checks arrive.

budgets, and the surplus may only become salient at the end of the month. After week 3, we observe a statistically significant 2-percent increase in emergency visits until roughly week 6. The estimated increase in visits remains positive after week 6, but that increase is imprecisely estimated. The modest pre-trends discernable here are consistent with anticipation of stimulus payments by some households. These patterns are similar for male and female patients.

The estimates above rely on a proxy for when individuals would have been sent their stimulus checks if they received a stimulus payment in the week corresponding to their own SSN. If we also observed actual stimulus payment receipt, we would instrument for it with *CheckSent*. Relative to those instrumental variable (IV) estimates, the reduced-form coefficients we estimated above are scaled towards zero by the probability of actual check receipt. Several considerations determine the scaling factor.

First, Parker et al. (2010) 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 19% 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 $\frac{1}{0.85 \times 0.6 \times 0.81} = 2.42$ in order to obtain an IV-

¹²In 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.

type 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 would imply an IV-type effect of actually receiving a stimulus payment check of 3.15%.

This number continues to differ from the average treatment effect for several reasons. First, paper check recipients differ systematically from the general population. Parker et al. (2010) 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 and discussion 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 described above. 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 in-

¹³Primary 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.,

crease 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 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 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 1.3% overall increase in ED visits.

Column 2a shows that the 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

1983). Section 4, below, presents estimates of the effect of the 2001 tax rebates on outpatient visits to physicians. The results suggest little effect.

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

¹⁵We 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).

¹⁶Drug- and alcohol-related visits account for roughly 5% of all ED visits. Drug-related inpatient visits, in contrast, do not increase.

Parker et al. (2010) estimate only a 1.1% marginal propensity to consume alcohol out of the 2008 stimulus payments. At the same time (Column 2b), ED visits unrelated to drugs or alcohol increase by 1.1% in the ED. Drugs and alcohol thus contribute an economically important share of the total increase in ED visits, but not all of the increase in visits can be explained by such conditions.

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 1.7%.

To measure the relevant adjustment dynamics for the outcomes studied in Table 4, we again estimate distributed-lag models. Figure 3 presents the results of such models for emergency department visits. The figure generally demonstrates similar patterns as in Figure 2. For all outcomes except avoidable visits, we observe a statistically significant increase in visits around one month after the payments were sent. In nearly all cases, the effect of the stimulus payments 9 weeks after they are sent is statistically insignificant at the 5-percent level. The magnitude of the point estimates suggests that the risk of a visit did not return to baseline. This pattern is consistent with the finding of Parker et al. (2010) that some of the consumption impact of the stimulus is detectable at a one-quarter lag.

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 be drug-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 in magnitude across insurance status. Estimates for inpatient visits, in contrast, are more variable.

Figure 4 presents distributed-lag estimates for these outcomes in the emergency department. Two patterns in the figure are striking. First, the time-pattern in stimulus effects is remarkably similar across insurance groups. We observe the largest and most precise estimates for publicly insured households, who tend to face the lowest co-payments. Second, while the point estimates in Table 5 are similar across

income groups, the dynamics indicate that low-income households exhibited a short-lived, statistically significant increase in ED visits.

Taken as a whole, Table 5 and Figure 4 do not suggest a clear pattern between the change in visits and socioeconomic 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 across insurance status, 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 other visits. This further suggests that the overall treatment effect is not driven directly by the relaxation of liquidity constraints.

4 Evidence from the 2001 Tax Rebates

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 consumption. Consumers may have also changed the rate at which they consume 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.

Specifically, we perform this test by replicating and extending the analysis of Johnson et al. (2006) on the expenditure effects of the 2001 tax rebates. 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. Payments in 2001 were \$300 for single-headed households and \$600 for married couples. Compared to 2008, a much larger share of payment recipients received paper checks rather than direct deposits. Johnson

et al. (2006) measure the effects of the 2001 rebates using the Consumer Expenditure Survey (CEX), which included a special module with questions on the rebates.

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 2001 tax rebate received.¹⁷ Specifically, we regress changes in nine categories of health-related expenditures on the contemporaneous and once-lagged tax rebate amount. Exactly following Johnson et al. (2006), we use indicators for 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 exactly replicates the penultimate column of Table 6 presented by Johnson et al. (2006). It demonstrates that \$0.187 of each tax rebate dollar was spent on health in the two quarters after rebate receipt.¹⁸

The remainder of Table 6 decomposes this increase in health expenditures into subcategories.¹⁹ Columns 2 through 5 report the effects on the four health subcategories included in the CEX FMLY data files: health insurance, medical services, prescription drugs, and medical supplies. As Johnson et al. (2006) mention, most of the health spending induced by the tax rebates is spent on medical services, a statistically significant \$0.132 out of each rebate dollar. Prescription drug spending also

¹⁷Because the CEX includes questions about the rebates themselves, we can estimate not only the effect of rebate eligibility, but also the effect of rebate receipt.

¹⁸The dependent variable in Table 6 is a change in expenditure. Thus the cumulative effect from the contemporaneous and lagged rebates equals twice the contemporaneous coefficient plus the lagged coefficient.

¹⁹Note that all of this spending is out-of-pocket.

rises by \$0.033. These four categories compose the overall effect measured in Column 1.

Using the CEX MTAB data files, we further decompose the medical services expenditures into the outcomes listed in Columns 6 through 9. Columns 6 and 7 demonstrate that nearly half of the medical services spending caused by the 2001 rebates was allocated to hospitals in the form of hospital rooms and meals (\$0.027) and hospital services like medical care (\$0.035). These results support the findings based on the 2008 stimulus payments in Section 3.

In addition, Column 8 of Table 6 shows no effect of the 2001 tax rebates on consumption of outpatient physicians' services. The point estimate is negative but not statistically significant. This suggests that the stimulus payments induced health care expenditures in hospitals, but not the more routine, preventive, and non-urgent care typically provided in doctors' offices.

Finally, we analyze the effect of the 2001 rebates on one other category of medical services: dental services. Column 9 of Table 6 demonstrates that dental services account for over half of the increased medical services expenditures caused by the rebates. The immediate effect of the rebate on dental services is statistically significant at the five-percent level (p -value of 0.043), but the cumulative increase is not (p -value of 0.099).

Several other studies have documented that demand for dental care is especially sensitive to time-varying financial circumstances. The RAND Health Insurance Experiment (Newhouse, 1996) documented pent-up demand for dental care. Further, Cabral (2011) demonstrates how the consumption of dental care can be shifted over time. Thus it is not surprising that the 2001 tax rebates specifically increased expenditures on dental care. Such care is usually paid for out-of-pocket and is easy to postpone until liquidity arrives.

In all specifications using the CEX data, lagged tax rebates do not have a statistically significant additional impact on health-related expenditures. Instead, all of the effect is contemporaneous with receipt of the rebate check. This pattern is consistent with the findings of Section 3 that liquidity can cause short-term increases in hospital utilization for urgent conditions.

Table 6 leads to two conclusions. First, it confirms that positive liquidity shocks increase hospital utilization. Second, the results extend the estimates in Section 3 by focusing on other forms of health care consumption. The rebates may have led to an increase in dental care, but likely did not lead to an increase in other forms of preventive care.

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. Individual-level random variation identified our regressions in administrative hospital data. 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 appear to have provided liquidity that was dangerous to some recipients' health, leading to additional emergency hospital care.

These findings have several implications. First, optimal health policy depends in part on how health care utilization responds to household finances. Co-pays and deductibles are often viewed as useful for improving the efficiency of decisions to seek medical care. However, to the extent hospital utilization isn't affected through the direct channel of an expanded short-run budget set, co-pays, deductibles, and other deviations from full insurance for hospital visits 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 small but nonzero 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. In addition, 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 surprising 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: | Direct 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 - 59 | 20-Jun | 9-May |
| 60 - 63 | 20-Jun | 9-May |
| 64 - 69 | 27-Jun | 9-May |
| 70 - 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.

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

| | (1) All Adult Visits | (2) Men | (3) Women |
|---|-----------------------------|-----------------------------|-----------------------------|
| <u>A. Dependent Variable: logarithm of ED visits</u> | | | |
| I(After Check Receipt) | 0.013 (0.003) [0.005] | 0.013 (0.003) [0.002] | 0.013 (0.005) [0.024] |
| Avg. Visits / Week | 116,143 | 50,116 | 66,022 |
| <u>B. Dependent Variable: logarithm of inpatient visits</u> | | | |
| I(After Check Receipt) | 0.008 (0.004) [0.079] | 0.008 (0.009) [0.368] | 0.008 (0.005) [0.143] |
| Avg. Visits / Week | 56,319 | 21,878 | 34,439 |
| <u>C. Dependent Variable: logarithm of all visits</u> | | | |
| I(After Check Receipt) | 0.011 (0.003) [0.005] | 0.012 (0.003) [0.004] | 0.011 (0.004) [0.028] |
| Avg. Visits / Week | 172,461 | 71,994 | 100,461 |

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. $N = 9 \times (1+19+23) = 387$. 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) | (1b) | (2a) | (2b) | (3a) | (3b) |
|--|-------------------------------------|---|----------------------------------|-------------------------------------|-----------------------------|--------------------------------|
| | Related to Chronic Conditions | Not Related to Chronic Conditions | Drug- and Alcohol- Related | Not Drug- or Alcohol- Related | Classified as Avoidable | Not Classified as Avoidable |
| <u>A. Dependent Variable: Log(ED Visits)</u> | | | | | | |
| I(After Check Receipt) | 0.017 (0.008) [0.061] | 0.012 (0.003) [0.006] | 0.058 (0.018) [0.012] | 0.011 (0.003) [0.011] | 0.001 (0.005) [0.846] | 0.016 (0.005) [0.011] |
| Avg. Visits / Week | 18,085 | 98,058 | 4,515 | 111,628 | 27,222 | 88,921 |
| <u>B. Dependent Variable: Log(Inpatient Visits)</u> | | | | | | |
| I(After Check Receipt) | 0.004 (0.007) [0.593] | 0.011 (0.004) [0.038] | 0.010 (0.017) [0.571] | 0.008 (0.004) [0.063] | 0.011 (0.008) [0.199] | 0.006 (0.003) [0.096] |
| Avg. Visits / Week | 22,779 | 33,539 | 4,692 | 51,627 | 16,855 | 39,464 |
| <u>C. Dependent Variable: Log(ED + Inpatient Visits)</u> | | | | | | |
| I(After Check Receipt) | 0.010 (0.005) [0.082] | 0.012 (0.003) [0.004] | 0.034 (0.012) [0.026] | 0.010 (0.003) [0.008] | 0.005 (0.006) [0.386] | 0.013 (0.003) [0.004] |
| Avg. Visits / Week | 40,864 | 131,597 | 9,207 | 163,254 | 44,076 | 128,385 |

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 specification 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. $N = 9 \times (1+19+23) = 387$. 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 |
| <u>A. Dependent Variable: Log(ED Visits)</u> | | | | | | |
| I(After Check Receipt) | 0.014 (0.007) [0.093] | 0.013 (0.006) [0.064] | 0.015 (0.008) [0.084] | 0.012 (0.007) [0.109] | 0.012 (0.005) [0.027] | 0.015 (0.007) [0.059] |
| Avg. Visits / Week | 45,805 | 61,776 | 46,216 | 47,200 | 46,158 | 22,785 |
| <u>B. Dependent Variable: Log(Inpatient Visits)</u> | | | | | | |
| I(After Check Receipt) | 0.015 (0.005) [0.015] | 0.006 (0.008) [0.458] | 0.002 (0.008) [0.806] | 0.015 (0.009) [0.135] | 0.006 (0.003) [0.062] | - 0.012 (0.016) [0.470] |
| Avg. Visits / Week | 20,473 | 27,342 | 20,580 | 18,378 | 35,623 | 2,317 |
| <u>C. Dependent Variable: Log(ED + Inpatient Visits)</u> | | | | | | |
| I(After Check Receipt) | 0.014 (0.006) [0.044] | 0.011 (0.005) [0.050] | 0.011 (0.006) [0.113] | 0.013 (0.007) [0.093] | 0.010 (0.002) [0.002] | 0.012 (0.007) [0.100] |
| Avg. Visits / Week | 66,277 | 89,118 | 66,797 | 65,578 | 81,781 | 25,103 |

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 percentiles, respectively. In each specification 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 also included in the regressions. $N = 9 \times (1+19+23) = 387$. The standard errors in parentheses adjust for correlation between observations from the same SSN group. Associated p-values in brackets.

Table 6: The Effect of the 2001 Tax Rebates on Health Care Consumption as Measured in the Consumer Expenditure Survey

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|-------------------|------------------------------|------------------------------|------------------------------|-----------------------------|------------------------------|-----------------------------|-----------------------------|------------------------------|------------------------------|
| | All Health Expenditures | Health Insurance | Medical Services | Prescription Drugs | Medical Supplies | Hospital Room & Meals | Hospital Services | Physician Services | Dental Services |
| Rebate | 0.098 (0.040) [0.014] | 0.008 (0.020) [0.695] | 0.069 (0.031) [0.027] | 0.013 (0.009) [0.157] | 0.009 (0.009) [0.336] | 0.013 (0.010) [0.224] | 0.016 (0.010) [0.107] | -0.009 (0.009) [0.314] | 0.041 (0.020) [0.043] |
| Lagged Rebate | -0.009 (0.040) [0.826] | -0.004 (0.022) [0.863] | -0.005 (0.030) [0.874] | 0.007 (0.007) [0.333] | -0.007 (0.009) [0.410] | 0.002 (0.011) [0.887] | 0.002 (0.009) [0.799] | 0.009 (0.009) [0.342] | -0.011 (0.020) [0.565] |
| Cumulative Effect | 0.187 (0.082) [0.022] | 0.011 (0.038) [0.760] | 0.132 (0.065) [0.042] | 0.033 (0.019) [0.083] | 0.010 (0.018) [0.558] | 0.027 (0.022) [0.226] | 0.035 (0.021) [0.098] | -0.010 (0.019) [0.605] | 0.070 (0.043) [0.099] |
| R^2 | 0.002 | 0.001 | 0.002 | 0.002 | 0.002 | 0.001 | 0.002 | 0.001 | 0.002 |

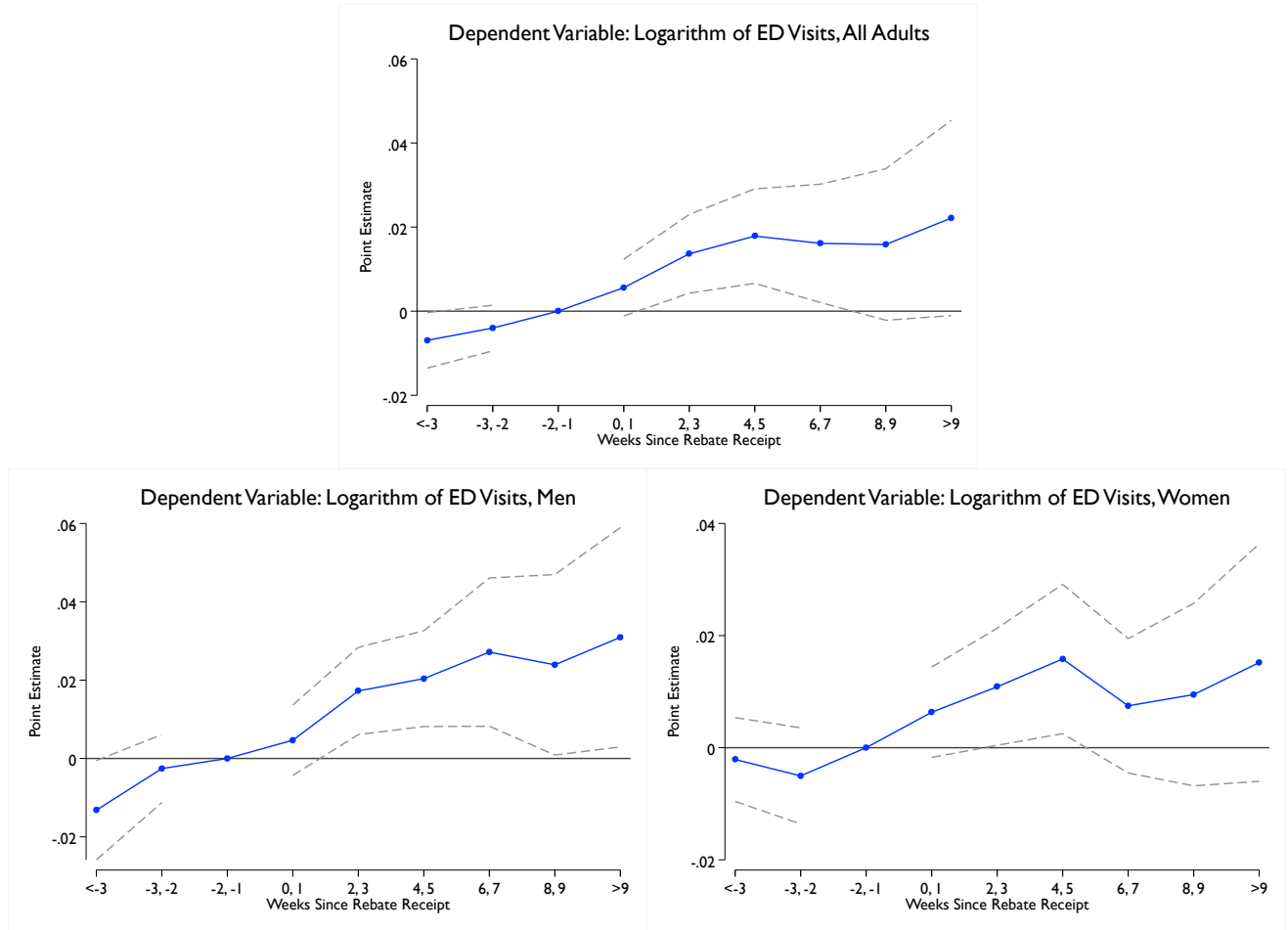
Note: This table presents estimates based on the Consumer Expenditure Survey. The estimates extend specifications reported by Johnson, Parker, and Souleles (2006) to subcategories of health care expenditure. Specifically, each column contains the results from a regression of a change in expenditure on the contemporaneous and once-lagged tax rebate amount, using indicators for receiving any tax rebate in the two periods as exogenous instruments. 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). $N = 12,370$. Standard errors are in parentheses and p-values in brackets.

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

| | (1) | (2) | (3) | (4) | (5) | (6) |
|------------------------|-----------------------------|-----------------------------|----------------------------------|-----------------------------|-----------------------------|-----------------------------|
| | | | <u>A. Replication of Table 3</u> | | | |
| | All Adult Visits | Men | Women | | | |
| I(After Check Receipt) | 0.011 (0.005) [0.049] | 0.013 (0.004) [0.020] | 0.010 (0.006) [0.120] | | | |
| Avg. Visits / Week | 95,153 | 40,855 | 54,294 | | | |
| | | | <u>B. Replication of Table 4</u> | | | |
| | Chronic Conditions | Not Chronic Conditions | Drug-Related | Not Drug-Related | Avoidable | Not Avoidable |
| I(After Check Receipt) | 0.020 (0.009) [0.069] | 0.010 (0.004) [0.051] | 0.058 (0.024) [0.046] | 0.009 (0.005) [0.079] | 0.000 (0.006) [0.950] | 0.015 (0.007) [0.073] |
| Avg. Visits / Week | 15,018 | 80,135 | 3,532 | 91,620 | 22,708 | 72,445 |
| | | | <u>C. Replication of Table 5</u> | | | |
| | Low Income | Middle Income | High Income | Privately Insured | Publicly Insured | Not Insured |
| I(After Check Receipt) | 0.008 (0.007) [0.303] | 0.008 (0.006) [0.185] | 0.011 (0.008) [0.228] | 0.007 (0.006) [0.318] | 0.016 (0.005) [0.011] | 0.011 (0.008) [0.175] |
| Avg. Visits / Week | 32,383 | 43,944 | 33,091 | 39,941 | 38,408 | 16,804 |

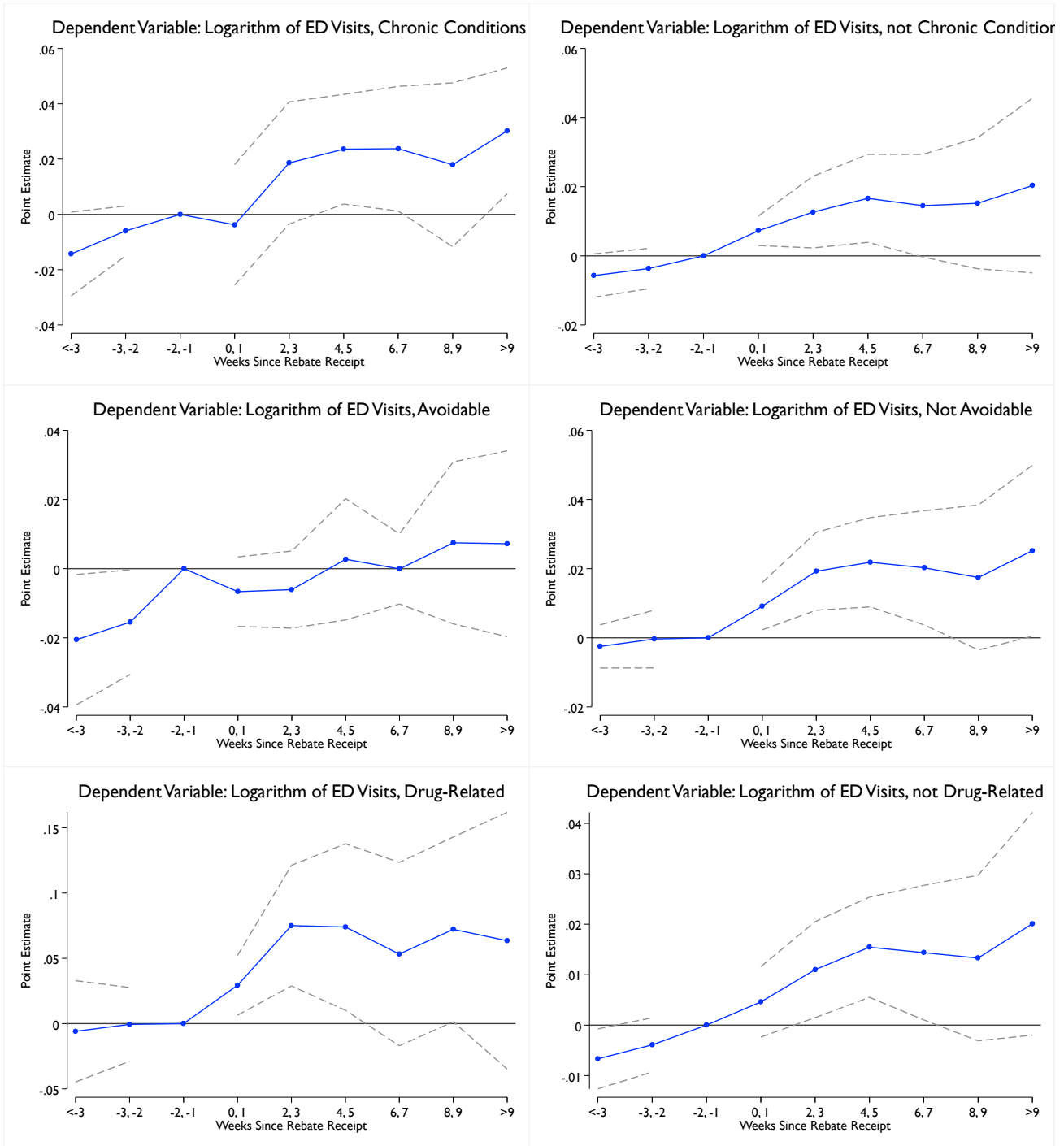
Note: Each cell presents a regression with the logarithm of ED visits from the given category as the outcome of interest. Panels A, B, and C replicate the ED results in Tables 3, 4, and 5, respectively, while excluding the first SSN-group with digits 00-09 from the analysis. Full sets of SSN-group fixed effects and week fixed effects are included in the regressions. $N = 8 \times (1+19+23) = 344$. The standard errors in parentheses adjust for correlation between observations from the same SSN group. Associated p-values in brackets.

Figure I: Distributed Lag Estimates by Gender



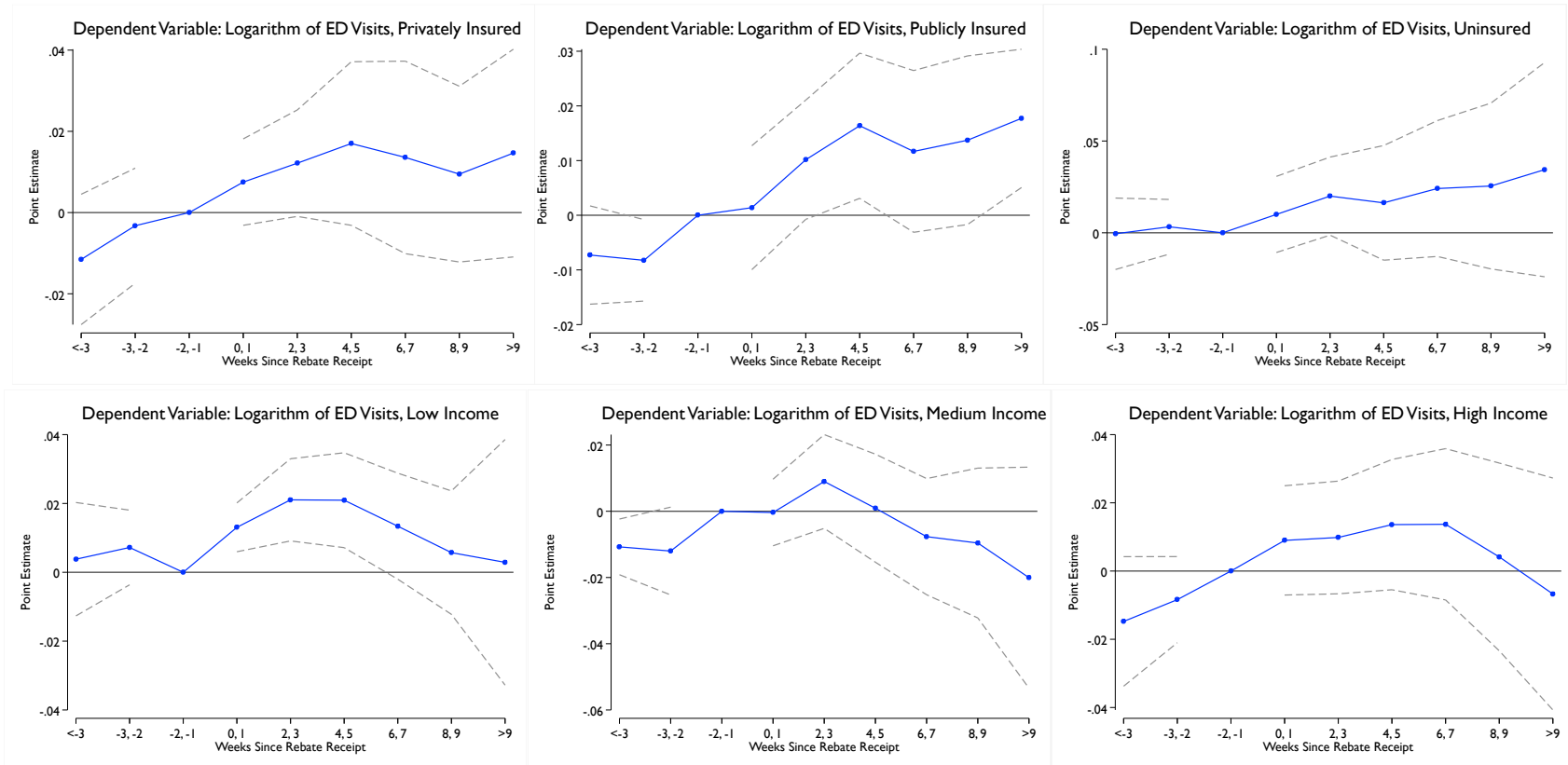
Note: Each figure plots point estimates from a regression of log counts of visits on a set of indicators for two-week intervals. The dotted lines plot 95% confidence intervals that are robust to autocorrelation between observations from the same SSN group. SSN-group fixed effects and week fixed effects 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 and week fixed effects 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 and week fixed effects 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.