Spending Responses to High-Frequency Shifts in Payment Timing: Evidence from the Earned Income Tax Credit

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Abstract

This study explores how shifts in the timing of large lump-sum payments to households affect spending. Using a novel data set combining daily, state-level measures of retail sales with IRS administrative data on tax refund issuance, we exploit plausibly exogenous, high-frequency variation across states in the timing of tax refunds to households claiming the Earned Income Tax Credit, particularly variation resulting from the 2017 PATH Act. Retail spending increases by 27 cents per refund dollar, implying an extra $1,150 of spending associated with the average refund within just two weeks of issuance. Results show non-durable and services expenditures increase along with durables, suggesting a considerable consumption response to the two-week shift in the timing of a large, predictable payment. Our results, which provide a lower bound on spending out of lump-sum payments, are informative for the efficacy of lump-sum transfers, including stimulus payments made during the recent recession.

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1. Introduction

The Earned Income Tax Credit (EITC) is a means-tested transfer program directed towards low-income working households in the United States, and it is distributed annually as part of a tax filer's federal refund. These federal tax refunds to EITC recipients (hereafter, "EITC refunds") represent a large, predictable, regular, and salient payment to millions of households each tax filing season and provide a large lump-sum transfer equivalent to a couple months of pay for the typical recipient. The implementation of the Protecting Americans from Tax Hikes (PATH) Act in 2017 permanently shifted the typical timing of refunds by roughly two weeks for early-filers claiming EITC. In this study, we investigate how these high-frequency shifts in the timing of EITC refund receipt affect spending, and how these responses inform the overall spending response to tax refunds or other lump-sum payments such as fiscal stimulus.

Our ability to conduct the analysis hinges on a novel daily, state-level dataset that we build, combining administrative Internal Revenue Service (IRS) data on magnitudes of tax refunds issued to federal tax filers receiving the EITC over the 2014 to 2018 tax filing seasons with spending at retail stores and restaurants. The daily frequency of the data allow us to exploit plausibly exogenous, high-frequency variation in the timing of EITC refund disbursements, most notably the roughly two-week shift in refund issuance to EITC claimants resulting from the implementation of the PATH Act in 2017. Such short-lived variation would be unobservable, or at least attenuated, in more typical time-aggregated data. In addition, cross-state differences in EITC claiming behavior generate differences in how the delays impact spending across states, allowing us to use a rich fixed effects model to understand how spending responds to the timing of refund issuance.

Our baseline specification controls for daily national shocks as well as arbitrary state-specific seasonality, slow-moving trends, and weather. In doing so, we can isolate shifts in spending that occur concurrently with the shift in refund timing, independent of aggregate shocks, seasonality, or state-specific fluctuations absorbed by our controls. Our estimates provide a view into how low-income households cope with changes to large lump-sum payments and how spending responds to the timing of cash flows more generally.

Under our baseline specification, we find that each EITC refund dollar shifted leads to 27 cents of additional retail spending within two weeks of refund issuance, providing clear evidence that spending out of EITC refunds is not smoothed evenly throughout the year. We interpret this effect as sizeable for a few reasons. First, our estimate implies an extra $1,150 of retail spending associated with the typical EITC refund in the month of receipt – a 74 percent increase relative to average monthly retail spending for EITC households. Second, our spending measures focus only on retail goods and food services as measured by card transactions—covering about half of discretionary spending. Notably, our measure excludes components like automobile purchases, which have previously been shown to respond to EITC receipt (Goodman-Bacon and McGranahan, 2008). Even without including spending in these categories, the implied spending response out of the refunds is quite large, as the estimated swing in retail and restaurant spending relative to a typical week is comparable to the extra spending seen around Christmas each year.

The spending response is also rapid. About two-thirds of the estimated spending response occurs during the contemporaneous week of refund issuance and the week after, before petering out over the next couple of weeks. We also estimate a statistically significant spending response in the week prior to EITC refund issuance, which, to our knowledge, has not been documented by previous researchers.[2] This response could reflect temporarily running down account balances, the use of short-term credit, or spending enabled by refund anticipation loans (RALs). Tools from the IRS, such as the 'Where's My Refund' website, provide a projected deposit date for the refund and may allow for some very short-term smoothing of spending. RALs, which are offered by third-party tax preparers such as H&R Block for a fee, typically provide access to a portion of a household's projected refund within hours of filing.

Finally, we find that the spending out of EITC refunds is *not* limited to durable goods. Indeed, nearly one quarter of the spending response occurs at grocery stores and restaurants, with additional responses in other industries likely selling non-durables and services. While our data do not allow us to rule out the possibility that spending shifts to higher-cost, higher-quality purchases or storable goods, the result suggests that consumption—and not only expenditures—varies significantly with refund receipt timing.

Sharp spending responses to shifts in the timing of refund issuance are consistent with survey evidence that many EITC claimants have tenuous financial situations prior to refund receipt (e.g. Maag et al., 2016; Jones and Michelmore, 2018). Combining the FRBNY/Equifax Consumer Credit Panel and public-use IRS Statistics of Income data, we find that the average

[2] Baugh et al (2021) find no anticipatory effect in their sample of relatively high-income, high-liquidity EITC recipients. Notably, their sample does not include households using refund anticipation loans (RALs).

revolving credit utilization rate – a household's outstanding credit balance as a share of its borrowing limit – is more than twice as high in zip codes that belong to the top quintile of the EITC share distribution than those in the bottom quintile.

Our paper makes several contributions. First, our work is the first to show how spending responds to a roughly two-week shift in a large lump-sum payment and complements existing studies examining spending responses following other types of transitory shocks. In particular, our approach recovers the local average treatment effect of a roughly two-week shift in refund timing on spending by early-filing EITC claimants. Despite the timing shift being only a couple of weeks, we find spending shifts considerably. The tools that households have to insure against various shocks may differ and responses would likely be different if the delay were longer or the payment size were smaller. As such, our work complements the handful of existing papers that use timing variation in cash flows to households to study spending, though ours is the first to focus on this type of high-frequency shock to such a large lump-sum transfer (Parker et al, 2013; Baker and Yannelis, 2017; Gelman, et al, 2019).

Second, our work complements existing studies on how spending rises around receipt of income or lump-sum payments (i.e. excess sensitivity). Several papers have studied how receipt of tax refunds impacts spending, independent of shifts in refund timing such as those we exploit (Barrow and McGranahan, 2000; Ferrell et al, 2019), and others have studied related settings such as spending responses to the timing of regular income such as Social Security (Stephens, 2003) and pensions (Stephens and Unayama, 2011). Our results show that households exhibit a considerable spike in spending around refund receipt, and that this spike shifts in response to a shift in the timing of refunds.

Finally, our estimates are informative about the efficacy of the EITC. Nearly one-fifth of U.S households rely on the program, and for the vast majority of these households, their tax refunds represent a substantial income flow. While many papers have addressed the implications of the EITC for labor supply, our paper examines its effect on consumer spending (for review of literature, see Nichols and Rothstein, 2016). Household survey data, such as that collected by Maag, Roll, and Oliphant (2016), show households receiving EITC may be unprepared to weather shocks to their cashflows, suggesting fluctuations in refunds may impact spending. Existing studies estimating spending out of the EITC have focused on longer time horizons than our paper due to data limitations (Barrow and McGranahan, 2000; Goodman-Bacon and

McGranahan, 2008). As a result, confounding factors in the months surrounding receipt may impede their ability to isolate the effect of the EITC refunds on spending. The high-frequency nature of our novel data set allows us to exploit short-lived timing shifts in EITC refund issuance. In turn, we can isolate spending changes driven by refund receipt in a short time window while controlling for a rich set of location-specific seasonal factors and trends.

Our empirical approach is designed to capture responses to timing shifts in refunds, abstracting from potential permanent income effects that an increase in EITC would entail. Even so, our result provides a lower bound on the overall effects of an additional dollar of refunds to low-income households, and likely a tight bound on the short-run effect (i.e. the amount spent within a month of receipt). Specifically, because we exploit a high-frequency shock to the *timing* of refunds, our estimate only recovers responses among households that are highly sensitive to refund receipt timing. Households that smooth the refund over longer time horizons may still respond to changes in the *magnitude* of refunds, boosting the overall effect of refunds relative to our estimate. Still, because those smoothers will spend only a small portion of the refunds within the short period we examine, our response likely captures well the short-run effect of refunds to EITC claimants.

Furthermore, because we use state-level spending measures, our estimates recover a local aggregate response in spending that includes not only the average EITC household-level marginal propensity to consume (MPC), but also any indirect effects on spending by non-EITC households and price adjustments by retailers in response to EITC refund issuance. Such an "all-in" reduced form estimate is advantageous for projecting the likely overall spending effects of stimulus payments or other large transfers to low- to moderate-income households, as well as expansions of the EITC itself. As such, our results complement research utilizing household-level survey data which recovers direct effects of payments, absent local spillovers.[3]

Because we estimate a local aggregate spending response, our results may be especially informative for states considering the likely spending and sales tax revenue generating effects of expanded transfers to these households (such as state-level EITC expansions). For example, Bastian and Jones (2021) utilize estimates from an earlier version of our paper (Aladangady et

[3] Spillovers in our context include any effects that play out within the few weeks over which we estimate responses. For example, our effects may capture pricing responses at establishments where EITC households shop (see Hastings and Washington, 2010) and may include impacts on spending of non-EITC households that, say, share a meal out with an EITC household.

al., 2018) to study the net costs of the federal EITC – that is, EITC outlays less additional taxes and the reduction in other government transfers to EITC recipients. Specifically, the authors utilize our estimates to calculate the additional sales tax revenues that would be collected from a federal EITC expansion.

Finally, our work contributes to the broader empirical literature on the impacts of large, direct payments to households in a recession (Parker, Souleles, Johnson, and McClelland, 2013); Sahm, Shapiro, and Slemrod, 2012) and is particularly informative about the relative efficacy of targeting stimulus payments to low-income, low-liquidity households such as those receiving EITC. Our estimates suggest such households spend a considerable fraction of lump-sum transfers very quickly after receipt, consistent with the findings of recent papers studying the latest batches of stimulus during the COVID pandemic (Baker et al, 2020; Karger and Rajan, 2020; Chetty et al, 2020; Cox et al, 2021). Other findings in the literature suggest lower-income households may suffer larger swings in income during recessions (Guvenen et al, 2017; Patterson, 2021) and that the response to transfer payments is potentially state-dependent (Gross et al, 2020). These two facts suggest our estimated responses during the later stages of a long expansion may be a lower bound for effects at the trough of a recession. Even so, our estimates suggest a considerable spending response.

2. EITC refunds and the PATH Act

The EITC is a means-tested, refundable tax credit claimed by a large share of low-income working households. During the 2018 tax filing season (tax year 2017), 27 million households received the credit, accounting for roughly one-fifth of all tax returns processed.[4,5] Of these households, receipt is concentrated among families that, in the absence of the credit, would have incomes after other taxes and transfers between 75 percent and 150 percent of the poverty line, which was about $25,000 in 2018 (Hoynes and Patel, 2018).

Because it is a refundable credit, any portion of the EITC not used to cover positive income tax liability is disbursed lump sum as part of the recipient's federal tax refund. Among

[4] Source: Internal Revenue Service (June 2020). SOI Tax Stats – Individual Income Tax Returns Publication 1304. Retrieved from: https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-returns-publication-1304-complete-report.

[5] Bhargava and Manoli (2015) report that the EITC take-up rate is 75 percent, which the authors note is high relative to other social programs such as food stamps, Social Security, and health insurance.

households claiming the EITC, the overwhelming majority receive a federal tax refund, which, during the 2018 tax season, averaged $4,272.[6] Maag et al. (2016) note that EITC refunds are equal to roughly two months of take-home pay for a typical EITC claimant and are often the largest income flow for these households in a given year.

In addition to being a large payout, EITC refund amounts are known prior to receipt. At the time a tax return is filed, EITC claimants learn the expected amount of their refund. But even prior to filing, Caldwell et al. (2018) document that low-income households have correct mean expectations about their refund. One potential explanation for low-income taxpayers' accurate refund expectations is that EITC eligibility is highly persistent over time. Stevens et al. (2018) find that for all households beginning a spell of EITC eligibility, over one half are eligible for more than five years in the next decade.

EITC claimants tend to file early in the tax season, and prior to 2017, the earliest issued federal tax refunds accrued primarily to this group. Maag et al. (2016) document that during the 2015 and 2016 tax seasons, 56 percent of EITC claimants filed their taxes before February 15. As a substantial majority of EITC claimants use third-party tax preparers – who by and large are required to e-file – claimants could expect their refund to be issued by the IRS less than three weeks after the filing date. Indeed, we estimate that during the 2016 tax season, refunds were issued to over 60 percent of EITC claimants by early March.[7]

Beginning in 2017, the PATH Act prohibited the IRS from issuing any federal tax refund to a household claiming the EITC until February 15 in an effort to reduce tax fraud.[8] As a result, EITC claimants waited longer to receive their tax refunds than in prior years. And because there is significant geographic variation in the share of tax filers receiving the EITC, some locales

[6] The magnitude of the credit varies with earned income, marital status, and number of children, but averaged about $2,500 in 2018. (Total refunds were larger, averaging $4,272 per household.) Details on the full EITC schedule can be found at https://taxfoundation.org/earned-income-tax-credit-eitc/. Jones (2012) documents that average tax refunds to EITC recipients exceed the average size of the credit, as many recipients over-withhold taxes despite having limited, or even zero, tax liability

[7] Authors' calculations using administrative data on EITC and non-EITC refund issuance counts during the 2016 tax season, which we define to be late January through May. We consider early tax filers to be those who received their EITC refund before March 5, 2016. Source: Internal Revenue Service: Research, Applied Analytics, & Statistics.

[8] The legislation stipulated that the entire refund to tax filers claiming the EITC or Additional Child Tax Credit (ACTC) must be held for processing. The change was implemented to provide IRS additional time to detect fraud amongst early-filing EITC and ACTC claims, such as instances where multiple filing units declared the same dependent child—a common type of fraud given the marginal increase in the credit for dependent children. There was no corresponding shift in refund issuance timing for filers who did not claim the EITC or ACTC. Source: Internal Revenue Service, Refund Timing for Earned Income Tax Credit and Additional Child Tax Credit Filers. Retrieved from https://www.irs.gov/individuals/refund-timing.

were disproportionately affected by the refund timing shift than others. It is both the time-series and geographical variation in EITC refund receipt that we utilize to estimate spending out of EITC refunds.

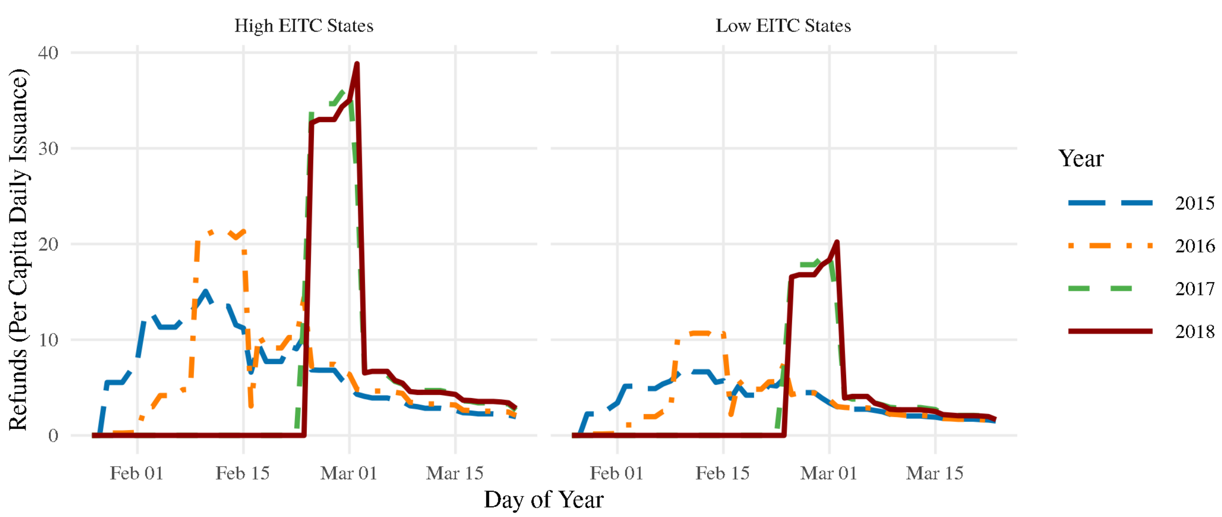
3. Data

3.1. Daily, state-level EITC refunds

A central component to this analysis is daily, state-level EITC refund issuance dollars that we obtained from the IRS's Research, Applied Analytics, and Statistics group (see Appendix A for additional details). These data allow us to measure the short-lived shift in refunds driven by the PATH Act, as well as other high-frequency variation in refund timing. Figure 1 plots the 7-day trailing moving average of per-capita EITC refund issuance dollars during the early months of the 2015-2018 tax seasons. We also separate EITC refund issuance by "high" (left panel) and "low" (right panel) EITC refund states, where we define high (low) EITC states as the ten states with the highest (lowest) shares of tax filing units receiving the federal EITC.[9]

Figure 1. Per-capita daily EITC refund issuance ($) in high- and low-EITC states

Early months of 2015-2018 tax seasons



Source: Internal Revenue Service: Research, Applied Analytics, & Statistics. Note: For comparability across years, we align years by week of year, defining the first week as the first one which has both a Monday and Friday within January. Date labels on x-axis correspond to the date in 2017. We calculate daily per-capita EITC refund issuance in high- (low-) EITC

[9] See Appendix Figure A.5 for each state's federal EITC share. In order of descending EITC recipient shares, the "high-EITC states" category includes Mississippi, Louisiana, Georgia, Alabama, Arkansas, South Carolina, New Mexico, Florida, Tennessee, and Texas. In order of ascending EITC recipient shares, the "low-EITC states" category includes North Dakota, New Hampshire, Alaska, Minnesota, Connecticut, Washington, Wyoming, Wisconsin, and Vermont.

states as the sum of EITC refund dollars issued on a given day in the high (low) states divided by the total population in the state. We then calculate the 7-day trailing moving average of per-capita EITC refund issuance over dates t to $t - 6$. Refund issuance in 2014 (not plotted, but in our baseline samples) is nearly identical to 2016.

For both high- and low-EITC states, we observe considerable variation in the timing of EITC refund issuance across tax seasons. This variation is primarily a result of the PATH Act legislation. During the 2015 and 2016 tax seasons, EITC refund issuance peaked in the second week of February and gradually declined thereafter (dashed blue and orange lines). In contrast, EITC refund issuance was nonexistent during early February in 2017 and 2018. Instead, the figure reveals that there was a spike in EITC refund issuance in 2017 and 2018 about two weeks later (solid green and red lines), and issuance thereafter tended to be a bit higher than in prior years. Beyond the large timing shift observed in EITC refund issuance between the pre- and post-PATH Act years, we also observe a more gradual ramping up and down of EITC refund issuance in early February 2015 compared to the same period in 2016. This variation appears to be the product of the vagaries of IRS tax return processing from one year to the next, as we observe a similar pattern for both high- and low-EITC states, as well as for non-EITC refunds (Appendix Figure A.4).

Figure 1 also highlights notable differences in per-capita EITC refund magnitudes between high- and low-EITC refund states. Throughout the 2015-2018 tax seasons, daily per-capita EITC refund issuance was roughly twice as large in high-EITC states. For example, during the 2018 tax season daily per-capita EITC refund issuance peaked at about $40 in high-EITC states at the beginning of March (red line in left panel), whereas it was just $20 in low-EITC states (red line in right panel).

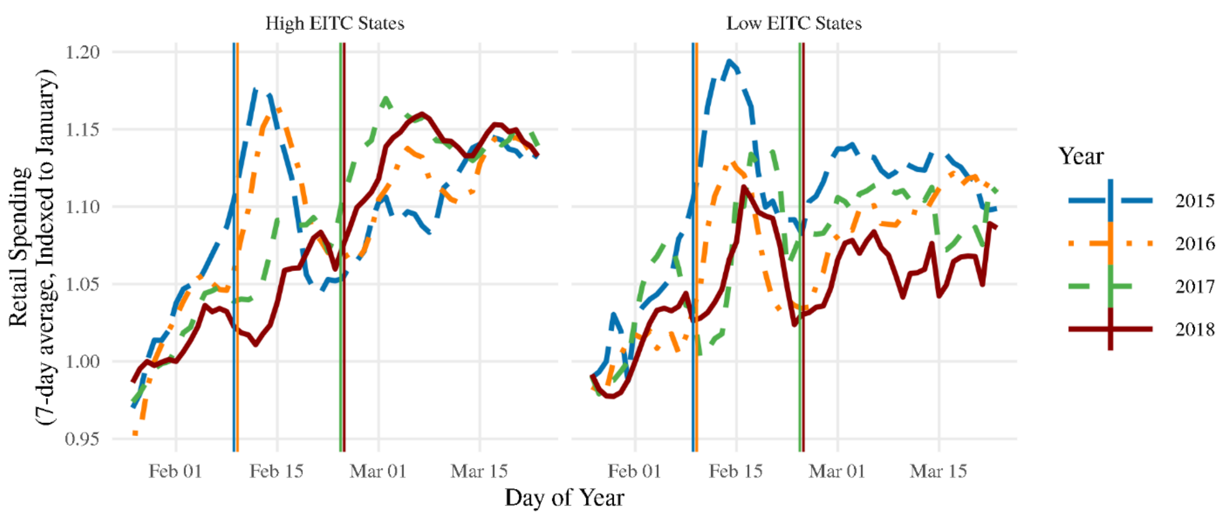
3.2.Daily, state-level spending indexes

In addition to our IRS refund data, we utilize novel daily, state-level indexes of spending at retail stores and restaurants, as developed by Aladangady et al. (2019). The sample used in our main regressions runs from 2014 to 2018, covering 3-digit NAICS codes in retail trade and food services—about 30 percent of personal consumption expenditures (PCE), and about half of discretionary spending (PCE, excluding housing and healthcare services). The underlying data reflect a considerable fraction of total sales in the U.S.—about 7.4 percent, on average, over our sample window—and provide an accurate measure of spending that closely tracks national aggregate measures such as the Census Monthly Retail Trade Survey. We provide additional details about these measures in Appendix A and Aladangady et al. (2019).

In Figure 2, we plot the 7-day trailing moving average of our national retail sales spending index in high- and low-EITC states, categorized as we did in Figure 1. We use a 7-day trailing moving average of spending to smooth through large and regular day-of-week effects, align weeks of the year in 2015-2016 and 2018 to match with their corresponding week in 2017, and index spending to the average from January 4 to January 31 in each year. In these plots, we otherwise do not control for any other factors that might affect spending during the time periods shown.[10]

Figure 2. Index of national daily spending at retail stores and restaurants

Early months of 2015-2018 tax seasons



Source: Fiserv. Note: For comparability across years, we align years by week of year, defining the first week as the first one which has both a Monday and Friday within January. Date labels on x-axis correspond to date in 2017. We index spending to of the average over January 4 - 31 in each year to compare within-year variation in timing. Vertical lines correspond to the end of each week of peak EITC refund issuance. The peak is the same from 2014 to 2016 (2014 not shown), and for 2017 and 2018.

Retail spending in early to mid-February of 2017 and 2018 – when EITC refunds were not disbursed due to the PATH Act – deviated considerably from previous years and did so particularly in high-EITC states. Whereas spending in 2015-2016 picked up in the week of peak EITC refund issuance in those years (orange and blue vertical lines) and peaked the week after

[10] Notably, the figure does not control for winter storms in low-EITC states that shifted spending patterns around tax season in certain northern states. We do include these controls in our baseline regressions, along with a rich set of state-specific seasonality effects, helping to reduce noise and improve the precision of our results.

(roughly the second week of February), spending in 2017 and 2018 remained relatively flat, only overtaking pre-PATH Act spending in March. This shift— while still evident—is muted in states with lower shares of EITC households.

Figures 1 and 2 lay out the general approach to identification in our paper. The PATH Act induced a roughly two-week shift in both refunds and spending, and the magnitude of this shift varied across states. The difference in the shift in spending and refunds across states allows us to separate the impact of refunds from other state-specific or national trends and seasonality that may exist in the data.

4. Regression methodology and results

4.1 State-Time Fixed-Effects Regression

Our primary approach formalizes the intuition behind Figures 1 and 2 through a rich set of fixed effects and controls to account for fully-flexible national time trends, as well as state-specific seasonality and longer-run growth. These controls absorb variation that may drive spurious correlations between spending and refunds, allowing us to isolate plausibly exogenous state-level differences in how the PATH Act shifted tax refunds. In particular, the residual variation in $EITCrefunds_{st}$, conditional on our controls, provides a measure of variation in the *timing* of refunds. Like Figures 1 and 2, the model allows us to compare year-to-year changes in the timing of spending and refunds across states to identify spending responses to EITC refunds.

Specifically, we allow per-capita retail spending C_{st} in state s on day t to be:

$$C_{st} = \sum_{\ell=-2}^{4} \theta_\ell EITCrefunds_{s,t-7\ell} +$$

$$\delta_t + \sum_{y\in[2014,2018]} \delta^{s,y} + \sum_{w\in wk.of.yr} \delta^{s,w} + \beta^s snow_{st} + u_{st}$$

(1)

Our main variable of interest is average daily per-capita EITC refunds issued to households in state s over the prior 7 days, $EITCrefunds_{s,t}$.[11] The distributed lag in the first row of Equation (1) allows spending in a given week to respond to refunds received two weeks in the future to four weeks in the past, allowing us to capture the effect of refunds on household spending over a window before and after the actual receipt of funds.

The second row of terms in Equation (1) includes time dummies, δ_t, at the daily frequency to capture a very flexible national time trend. In particular, the variable captures all aggregate variation occurring at the same time as EITC refund issuance, including average cross-state issuance of refunds. As such, the spending coefficients are identified using state-level differences from average EITC issuance, comparing high-EITC states and low-EITC states in a manner similar to Figure 1.

In addition to national trends, we include state-specific year and week-of-year fixed effects ($\delta^{s,y}$ and $\delta^{s,w}$) to account for low-frequency trends and seasonality in spending that may be correlated with refund issuance at a state level. We allow these variables to vary flexibly across states to account for the potential that states with differing shares of EITC recipients may have different economic trends or seasonality. Importantly, the state-by-year dummy $\delta^{s,y}$ also captures variation in spending and refunds from year-to-year, ensuring that the residual variation in refunds is driven by shifts in timing rather than magnitudes.[12] We also include controls for daily snowfall and 7-day cumulated snowfall (a proxy for accumulation) to capture potential differences in weather across high- and low-EITC states.[13]

Our estimator nests a simpler two-way fixed effects model with time and state dummies and can be interpreted in a similar manner. The regression estimates how a state's spending changes around refund issuance relative to national averages, and relative to average spending at the same time of year in the given state. As with a standard two-way fixed effects setup, a key

[11] On day t, for the contemporaneous week of issuance $EITCrefunds_{s,t}$ is the average of EITC refund issuance over days $t-6$ to t. Thus, the one-week-lagged term $EITCrefunds_{s,t-7}$ corresponding to the coefficient θ_{-1} is average EITC refund issuance between 7 and 13 days prior to the date of interest (i.e. issuance on dates $t-13$ to $t-7$). By taking 7-day moving averages, we smooth through the lumpy timing of refund disbursements. Furthermore, a household may be more likely to spend on the subsequent weekend regardless of whether the refund was received on a Tuesday or Friday. Our specification recovers the average effect on spending of refunds received in the prior 7 days, allowing for this possibility.

[12] Changes in magnitudes over years are primarily driven by changes in eligibility and take up in each state over time. EITC credit amounts were little changed over the sample period.

[13] We thank Brigitte Roth Tran for sharing county weather data used to construct population-weighted state and MSA snowfall relative to normal (ie, average in past 30 years) in that state.

identifying assumption is that spending in states with high- or low-EITC shares has parallel trends around the disbursement of EITC refunds, conditional on the controls. In other words, spending in these states is shifted because of the shift in refund timing and not because underlying trends or other factors are simultaneously driving spending and refunds. Later in the paper, we show responses at longer leads and lags are insignificant and centered at zero, supporting this claim. In addition, Appendix D provides a series of randomized placebo tests further supporting the identifying assumption. Specifically, we find that our specification leads to no measured effect on spending when refunds are artificially shifted outside of tax season, suggesting that, conditional on our controls, state spending does not differ systematically with the variation in EITC refunds during other parts of the year. Results are similar when EITC refunds are randomized across states, and in both sets of placebos our baseline results are in the tail of the randomized placebo distribution.

Note that we can include such a rich set of controls because of the high-frequency spending data available to us, and the idiosyncratic residual variation in EITC refunds induced by the passage of the PATH Act described in Sections 2 and 3. Our model includes fixed effects to absorb confounding variation from daily-frequency aggregate shocks through a fully-flexible time trend and state-specific trends or seasonality. Doing so leaves very little variation in the data outside of the PATH Act. Time aggregation to a monthly or quarterly frequency—as is typical in most spending data—would leave little residual variation, because the variation in issuance timing between years is only one or two weeks. Our ability to identify spending responses to high-frequency liquidity shocks, therefore, depends on the high-frequency nature of both our data and the exogenous variation we exploit.

4.2 Baseline results

4.2.1 Baseline estimates of spending out of EITC refunds

Estimates of spending responses to timing shifts in EITC refunds are illustrated in Figure 3 (dots), as are the 95 percent confidence intervals (bars) which cluster standard errors by state.[14]

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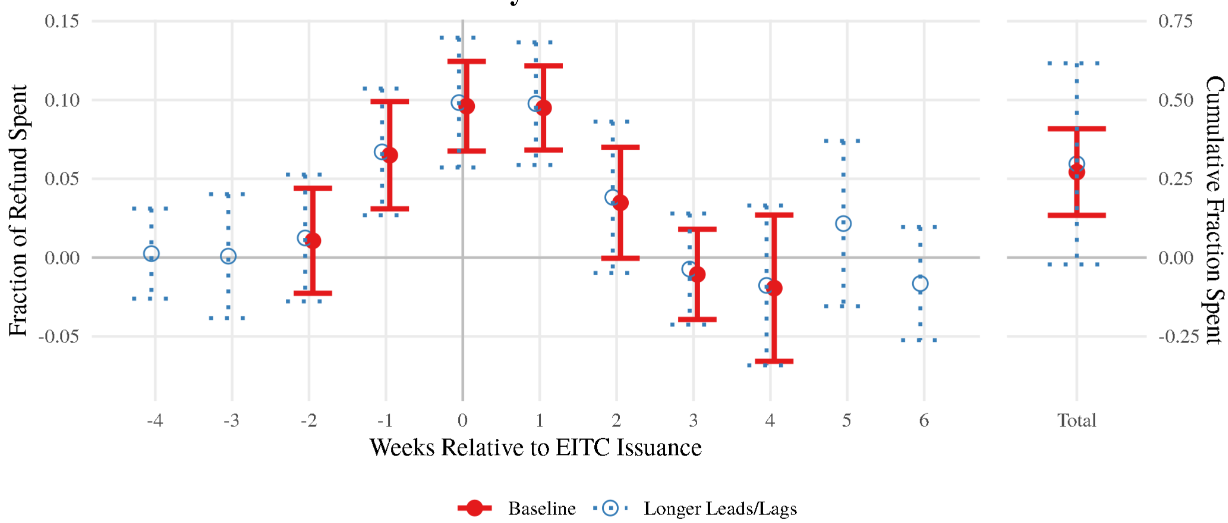
[14] To account for potential time correlation that may impact inference in state-level regressions (Bertrand, et al; 2004), all state-level specifications cluster standard errors by state. Specifications using MSA-level spending data in Appendix E are clustered by MSA. While our baseline specifications should control for arbitrary correlations within state, we also provide placebo tests (Fisher exact tests) in Appendix D that show our results are robust to randomization of refund timing within state (treatment timing) and randomization of state EITC receipt (treatment intensity).

In addition to our baseline model (solid dots and bars), we include an extended model allowing for responses at longer leads and lags (hollow dots and dashed bars). Notably, responses long before and long after refund receipt show no significant spending response, supporting our identifying assumption of parallel trends. However, allowing for additional leads and lags costs statistical power, and we utilize the more efficient restricted model as our baseline.

Figure 3. Fraction of EITC refund spent at retail stores and restaurants by week relative to refund issuance



Note: Figure 3 displays point estimates (dots) and 95 percent confidence intervals (bars) for the θ_ℓ coefficents in Equation (1) along with their sum $\sum\theta_\ell$ (dot and bars, right axis). Standard errors are clustered by state. The solid dots and bars denote our baseline model, which restricts responses outside of $\ell \in \{-2, \ldots, 4\}$ to be zero. The hollow dots and dashed bars reflect estimates of an extended model where we allow additional leads and lags. We interpret the coefficients as yielding the fraction of each EITC refund dollar spent at retail stores and restaurants ℓ weeks from refund issuance. Weekly spending begins to rise in the week prior to issuance, peaking in week of issuance. Spending remains elevated in two weeks following refund issuance, cumulating to 27 cents per dollar. Responses outside a few weeks are zero, supporting our assumption of parallel trends.

The cumulative response over the 7 weeks around issuance—displayed on the right axis—is 27 cents per dollar of refund. The largest impact of EITC refunds on spending at retail stores and restaurants is in the week of issuance and the week following issuance, which account for about 2/3 of the overall response. In addition, each refund dollar is associated with 6.5 cents of spending in the week before issuance.

We interpret the magnitude of the spending response to be sizeable for a couple of reasons. First, recall that during our sample period, the average EITC refund was about $4,250. Ignoring for the moment that our estimate may capture effects beyond the household-level MPC (see Section 4.3.1), a spending response of 27 cents on the refund dollar implies that the typical EITC recipient spends roughly an additional $1,150 within a few weeks of refund receipt. Given that EITC-eligible households in the Consumer Expenditure Survey (CEX) report average monthly expenditures in retail categories of about $1,550, our results indicate that spending is about 74 percent greater than normal in the month of refund receipt for EITC recipients. Another way to place our result into context is to note that the additional spending in response to EITC refund receipt is similar to the increase in most categories of retail spending observed in the Fiserv index at Christmas time.[15]

Second, the already sizeable observed spend out is likely a lower bound on overall spending out of EITC refunds. For one, retail and restaurant spending make up only about one-third of total household spending in the national accounts and a little more than one-half of discretionary household spending. Notably, our indexes do not include spending on motor vehicles (6 percent of discretionary spending), transportation services (4 percent), or recreation (4 percent), all of which have been found to respond to EITC refunds (Goodman-Bacon and McGranahan, 2008). If spending in these three categories responded to EITC refund issuance in the same manner as our measure of retail spending, then the cumulative spending response within a few weeks of issuance would be roughly 40 cents per EITC refund dollar.[16]

Our data allow us to estimate the baseline specification shown in Equation (1) by retail establishment type to understand the composition of the spending response. Table 1 displays the cumulative spending response per EITC refund dollar at a few types of establishments, all of which significantly differ from zero (see Appendix Table A.1 for weekly responses). Combined spending at furniture and electronic stores responds by about 3.5 cents per EITC refund dollar (Column 2), with the composition of spending shifted towards these categories relative to normal (Columns 3 and 4). This result corroborates previous findings of durable spending out of EITC receipt (e.g. Barrow and McGranahan, 2000). We also find considerable responses in non-durables and services categories. Roughly one quarter of the spending response – approximately 6.5 cents out of each EITC refund dollar – occurs at grocery stores and restaurants. And sales at

[15] During each year of our sample period, national retail spending in the Fiserv index in the week leading up to Christmas is about 140 percent of the level observed in the first half of the year, and grocery spending is about 150 percent of the level observed in the first half. These increases are comparable to our estimated spending response in both magnitude and composition.

[16] The fact that our measures are based on card transactions (credit, debit, and EBT) and not cash or check introduce some complications, though not clearly a downward bias. As we discuss in Appendix A, our state-by-NAICS measures are benchmarked to the total spending in the 2012 Economic Census, inclusive of cash or check spending. If responses to refund receipt are disproportionately in cash or check *relative to the 2012 cash/check share*, then our estimate would understate the overall effect. However, if spending medium is roughly equal to the typical medium of spending, our estimates are not biased.

general merchandise stores—which are typically 56 percent non-durables—account for another 8.2 cents per EITC refund dollar.[17]

Table 1. Cumulative Spending Response for Selected Establishment Types

Category	Cumulative Spending Response	Share of Spending Response	Overall Share of Spending
Retail Sales (total)	0.271*** (0.0701)	100.0	100.0
Furniture	0.013*** (0.0047)	4.9	2.5
Electronics	0.022*** (0.0076)	7.9	2.9
Groceries	0.039* (0.0204)	14.2	17.4
Restaurants	0.026*** (0.0087)	9.7	14.4
General Merchandise	0.082* (0.0462)	30.3	18.0

Note: Table 1 displays the cumulative spending response per EITC refund dollar by establishment type, computed as the sum over the θ_ℓ coefficient estimates from Equation (1). See Appendix Table A.1 for weekly responses. Standard errors in parentheses are clustered by state. Share of spending response shows ratio of each category to the total. Overall spending shares are from 2012 Economic Census. Not all establishment types are displayed, and totals may not sum as estimates are from independent regressions.

The composition of the response across merchant types suggests that while the marginal spending response is skewed towards durable goods, non-durables also respond to refund receipt sharply. The result suggests considerable excess sensitivity of consumption to the liquidity shock, as opposed to simply a shift in expenditure timing. Of course, our merchant-based data do not allow us to rule out the possibility that responses are in storable goods or the possibility that households shifted to higher-quality or higher-priced goods rather than increasing real consumption.

Even so, the shift in non-durables appears sizeable. For example, we find that 3.9 cents of each refund dollar are spent at grocery stores. While that may appear to be small, it implies that

[17] As of the 2012 Economic Census (Table EC1244SLLS1), 56.4 percent of sales at general merchandise stores were food, alcohol, tobacco, non-durable household supplies (paper products, cleaning products, pet food, etc), pharmaceuticals, cosmetics, and fuel.

the typical EITC recipient spends an additional $166 at grocery stores within a few weeks of refund receipt. Given that EITC claimants in the CEX report average monthly spending on "Food at home" of $434, our results indicate that spending at grocery stores is nearly 40 percent higher than normal in the month of refund receipt for this group.

4.2.2 What do we know about liquidity and credit constraints faced by EITC recipients?

While our primary data sources do not allow us to identify a causal mechanism for the apparent excess sensitivity of consumption to EITC refund receipt, there is ample evidence from previous survey work and our own auxiliary analysis of credit and loan utilization in high- and low-EITC locales, that EITC recipients are, in general, liquidity and credit constrained. The survey evidence on EITC recipients in Maag et al. (2016) finds that at tax filing time, the median household reports liquid assets of only $400 and credit card debt of $2,000. More generally, the authors document that nearly four in five claimants of the EITC with children report having faced financial hardship, such as skipping a rent payment, at some point in the six months prior to being surveyed. In addition, 4 out of 10 of these families report use of an alternative financial service, such as a payday loan, in the six months prior to filing their return.

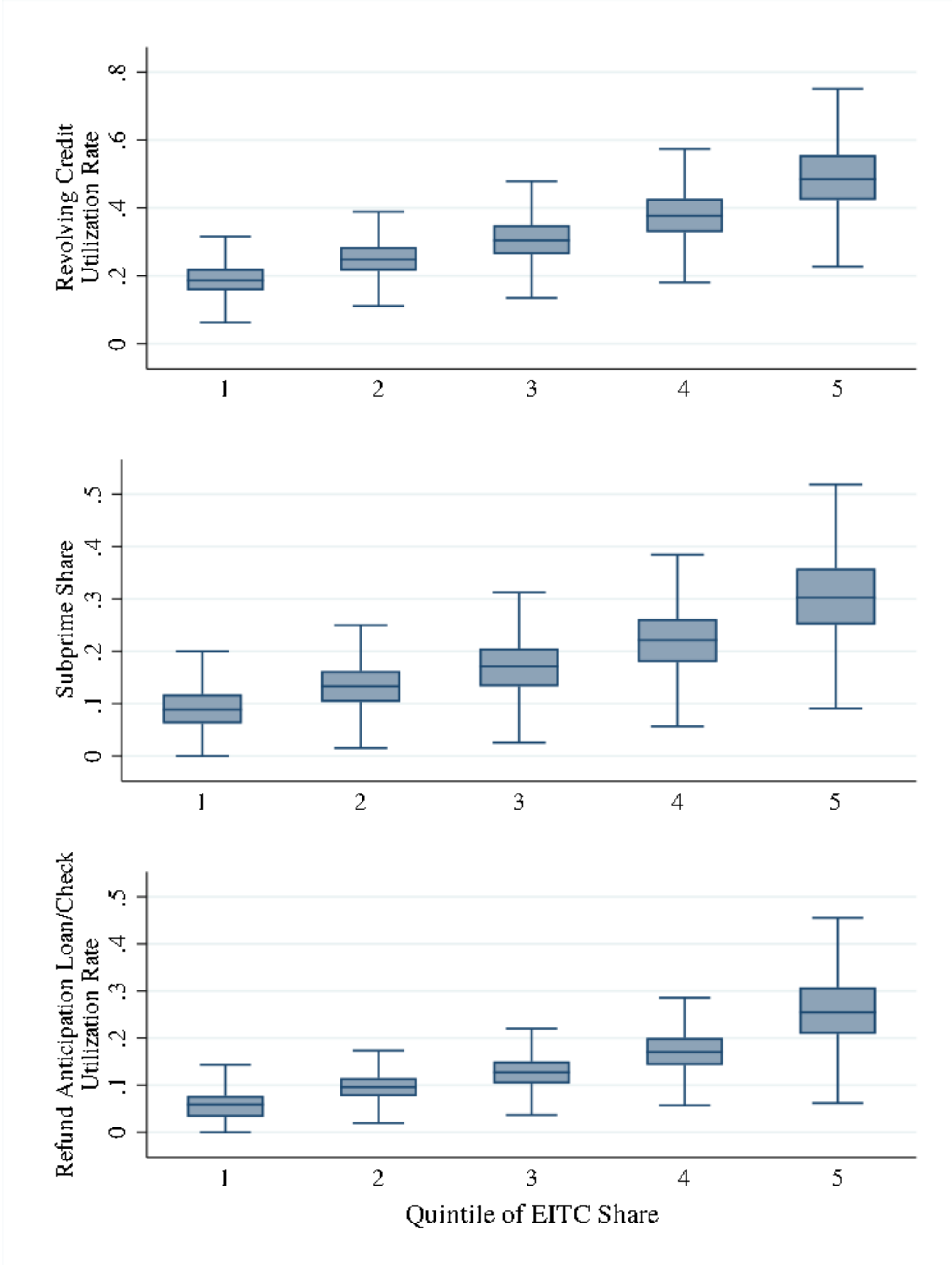
Beyond the existing survey evidence, we use the FRBNY/Equifax Consumer Credit Panel (CCP) in conjunction with publicly available IRS Statistics of Income data to document that residents of locales with higher shares of EITC recipients are, on average, much closer to their credit limits, and much likelier to be subprime borrowers. In Figure 4, we bin zip codes across the U.S. according to their EITC shares – that is, the proportion of tax filers within the zip code who claim the EITC. In particular, we bin by EITC share quintiles weighted by number of filers in each zip code, where the highest quintile contains zip codes with the highest shares of EITC recipients. Within each EITC share quintile, we then plot the range of various measures of credit and loan utilization across zip codes within that quintile. The top panel plots the range (min, 25th percentile, median, 75th percentile, and max) of each zip code's average revolving credit utilization rate – the amount of revolving credit outstanding for a household divided by their credit limit. A higher average credit utilization rate within a zip code indicates that the typical household within that locale is closer to its borrowing limit, and consequently at a greater risk of being credit constrained. We find that for zip codes in the top EITC share quintile, the credit utilization rate is roughly 0.5, more than double the average credit utilization rate for zip codes in the bottom EITC share quintile. Moreover, at the start of tax season, we find that nearly

75 percent of households in the top EITC share quintile have a lower available credit line than the size of the average EITC refund, and many of these households have nearly no credit line at all – further indicating the importance of the refunds to EITC households' liquidity.

In the middle panel of Figure 4, we plot the fraction of households within a zip code that have Equifax Risk Scores under 620 and would be considered "subprime" borrowers. These borrowers are seen as a higher risk to banks due to their incomes or past credit history and are less likely to qualify for loans or pay higher rates when they do qualify. As with credit utilization rates, we find that median subprime shares increase monotonically with EITC share quintile, rising from 10 percent in the bottom quintile to 30 percent of households in the top EITC share quintile.

In Appendix E, we combine the CCP data with our spending and EITC refund data to provide suggestive evidence that credit constraints are at least partly responsible for the sensitivity of spending to EITC refund receipt. Specifically, we document larger MPCs out of EITC refunds in geographic locales (MSAs) with higher average credit utilization rates and higher shares of subprime borrowers. That said, the differences we observe are not statistically significant, perhaps reflecting that most EITC recipients are constrained or that there is significant heterogeneity in credit outcomes within geographic locales that our measures of central tendency do not appropriately capture.

Figure 4. Zip code-level credit and loan utilization measures binned by EITC shares



Sources: FRBNY/Equifax Consumer Credit Panel (CCP) and IRS Statistics of Income (SOI)
Figure 4 plots the range of various zip code-level credit outcomes from the first quarter of 2016, which are binned by zip code-level EITC share quintiles. Zip codes with higher shares of households claiming the EITC (where "1" corresponds to the bottom-20% quintile of EITC share (weighted by number of tax filers) and "5" to the highest-20% quintile zip codes) have much higher shares of subprime borrowers (defined as those with Equifax Risk Scores under 620); higher utilization of refund anticipation loans (RALs) or checks (RACs) that, for a fee, provide advances on refunds; and higher revolving credit utilization rates (defined as the ratio of revolving balances to revolving credit available at the snapshot date). The data exhibit considerable within-zip code heterogeneity for credit scores and revolving credit (not shown), but patterns remain evident. RAL/RAC usage is not available at a within-zip code level of disaggregation.

Finally, in the bottom panel of Figure 4, we report the share of filers for each zip code that use RALs or refund anticipation checks (RACs) by EITC share quintile.[18] We find that, in the vast majority of zip codes in the highest EITC share quintile, use of RALs and/or RACs is much higher than is the case for zip codes in lower EITC quintiles. For example, in the top EITC share quintile, about 25 percent of households in the median zip code use a RAL or RAC, whereas just over 5 percent do so in the median zip code in the bottom EITC share quintile.

The use of short-term loan vehicles like RALs and RACs and limited available credit could indicate binding liquidity or credit constraints, though in our data we cannot determine whether these constraints arise from credit supply or demand factors.[19] That said, previous research on RAL and RAC use points out that an important motivator for their use, despite the relatively high fees for such a short-term loan, is an inability to pay tax preparation fees without a refund in hand (e.g. Hayashi, 2016; Theodos et al., 2010).

It also seems likely that RALs are at least partly responsible for the anticipatory spending effect that we observe in Figure 3, given the results of Cole et al. (2008) who find households that spend quickly and early often utilize high-cost credit products, revealing either high degrees of impatience or credit constraints. Moreover, Baugh et al. (2021), who study responses to income tax refunds in a sample that explicitly excludes RAL users, find no evidence of an anticipatory spending response to refund receipt.

4.3 Discussion and Contributions to Literature

4.3.1 What do the results tell us about responses to liquidity shocks and lump-sum payments?

In order to interpret the spending responses we recover, it is useful to consider the source of variation that drives the spending response. Identification comes from the relationship between per-capita spending and the *residual* variation in refunds conditional on our controls. As suggested in the motivating figures, the primary source of this variation is the shift in refund timing driven by the PATH Act, and nearly all variation occurs early in the filing season.

[18] RALs allow filers to take an advance on a portion of the tax refund, providing households with funds a couple weeks prior to issuance. RACs allow filers to delay the price charged for tax preparation services until their refund is issued, but do not provide additional liquidity. Both RALs and RACs typically charge a fee that is later deducted from the refund once it is issued by the IRS. According to a note published by the Consumer Financial Protection Bureau, as of 2018 RAC fees typically ranged from $30 to $50. RAL fees tend to be somewhat higher, depending on the size of the advance.

[19] For example, we cannot rule out demand factors such as impatience or financial literacy driving households past decisions which leading them to have low liquidity or be credit constrained at tax time.

Therefore, the response we recover captures the local average treatment effect (LATE) of the shift in refunds for early-filing EITC claimants impacted by the legislation.[20] Importantly, this residual variation in refunds reflects within-year timing shifts in liquidity which cumulate to zero over the year, and therefore, a shift in the timing of liquidity without a permanent change in income.

How do such liquidity shocks pass through to spending? Consumption optimizing behavior in the absence of constraints would suggest no pass-through of transitory shocks, as households use various insurance mechanisms like credit or prior savings to insulate consumption from the timing of cashflows. Of course, reality deviates from this theoretical benchmark and household spending may track the timing of cash flows for several reasons. For example, credit constraints may cause spending to respond to cashflows, even when permanent income is unchanged. Indeed, evidence from Section 4.2.2 suggests credit constraints may be a factor driving responses among early-filing EITC households. Households may also utilize large, lump-sum cashflows to finance the timing of large-ticket purchases, consistent with the fact that spending increased sharply at places like furniture and electronics stores.

Consider a simplified world where early-filing EITC claimants come from one of two sub-groups: "smoothers" whose spending is not impacted by the observed variation in the timing of refunds, and "non-smoothers" whose spending responds to the shift in cash flows.[21] The delay has an impact on the timing of refunds for both smoothers and non-smoothers so long as they file early and claim EITC, but only the spending of non-smoothers is shifted. As we show in Appendix B, our estimate can be thought of as the relation between the shift in spending and the

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[20] In the terminology of the treatment effects literature, the PATH Act and other idiosyncratic variation in issuance induce some variation in $EITCrefunds_{st}$ for "compliers" and the estimator recovers the LATE among these compliers (Imbens and Angrist, 1994). Much of the literature on LATEs focuses on IV, though the intuition generalizes to models including ours. Figures 1 and 2 suggest there is little systematic variation in refunds outside of the PATH Act that is not absorbed by our controls. In Appendix C, we show the residual variation—conditional on our baseline controls—is very small outside of the PATH Act delay. We also show that results from an IV exercise isolating a portion of variation from the delay are similar to our baseline, suggesting the controls absorb endogenous variation in refunds.

[21] Note that we use these terms to describe how households respond to the *timing shift* in refunds rather than the refunds themselves. For example, some "smoothers" may increase spending considerably in March or April because they expect to receive refunds, but there is no systematic shift in the timing of this spike when refunds are moved by a couple weeks. In our setting, this scenario seems unlikely because the PATH Act generates a permanent shift in refund timing. Households that respond sharply to refund receipt most likely also shift spending to coincide with refunds after the PATH Act, and a lack of such a shift is absorbed as seasonality in our state-week fixed effects. As such, our estimate of responses to the liquidity shock likely coincides exactly with the excess sensitivity to the refund itself.

shift in refunds and reflects both the fraction of households affected by the delay who are non-smoothers and the average spending response these households have to the shift in tax refunds captured by our regression.

Both the fraction of non-smoothers and their average spending response likely differ under alternative counterfactuals and settings. For example, many households may be able to smooth through a shift to a smaller cashflow, but not a lump sum as large as the EITC. And their response to a short delay may differ from a longer one. Relative to existing studies in the literature, our paper is the first to study the impact of delaying a very large lump-sum payment by a short interval and is informative for how well households may be able to insure against this type of shock.

Beyond capturing the direct response among households affected by the timing shifts in refunds, our state-level spending response also captures any indirect effects these timing shifts have on spending by non-EITC households and price adjustments by retailers. For example, if the recipient of an EITC refund dines out with a non-EITC individual who would not have dined out otherwise, then our estimate will also capture the additional spending by the individual who did not receive an EITC refund.[22] The estimated coefficients will also theoretically capture any differential price adjustments by retailers in high- and low-EITC refund states, so long as these price adjustments shift with the timing of EITC refund issuance.[23] For these reasons, we interpret our estimate of θ as capturing a local aggregate spending response per EITC refund dollar over weeks following disbursement rather than an EITC household-level MPC.[24]

Our estimated responses to shifts in the timing of refund issuance are closely linked to the impact of EITC refunds and lump-sum payments, more generally. First, our estimates directly map to the spike in spending around refund receipt, or the excess sensitivity of spending.[25] In

[22] Given our state-level spending measures and research design exploiting cross-state EITC refund issuance, the estimate of θ would also pick up a local Keynesian multiplier effect if it appears quickly upon refund issuance.
[23] To our knowledge, whether retailers adjust prices in response to EITC refund issuance is an open empirical question. In the context of the Supplemental Nutrition Assistance Program (SNAP), Goldin et al. (2020) find that retailers do not adjust prices based on predictable patterns of demand resulting from SNAP benefit issuance. Moreover, aggregate responses in prices and those that occur independently of delays in refunds will be absorbed by our controls discussed in Section 4.1.
[24] Similarly, using variation in minimum wages across cities and city-level spending measures, Cooper et al. (2020) estimate "local aggregate" spending effects from minimum wage increases, though their time frame for measuring the effects – 1 to 2 years – is longer than ours.
[25] We interpret the response to the timing shift in refunds as mapping directly to excess sensitivity to refund receipt. Appendix B lays out conditions under which these two concepts coincide in further detail—namely, that households

particular, it appears households exhibit a spike in spending around refunds early in the tax season, and the timing of this spike shifts when refund timing is altered.

Second, our estimate provides a lower bound for the impact of a change in the magnitude of refunds. To understand why, it is useful to consider how our outcome relates to an alternative counterfactual where the PATH Act cut the size of EITC refunds rather than delaying them. Such a shock impacts permanent income and would likely result in *all* households—including smoothers, as defined above—responding to the change. Our estimate can be thought of as a lower bound for the response that would have occurred in this counterfactual. The tightness of this bound depends on the fraction of early-filing EITC households that smooth through the two-week shock. Because this group is likely to smooth consumption over a long horizon such as a year or longer, most of the response to a lump-sum payment within a few weeks of receipt reflects the spending behavior of non-smoothers and our estimate likely captures the overall effect of refunds within a short horizon of refund receipt fairly well. As such, our results provide a reasonable estimate for short-run (i.e. within about one month) responses to lump-sum transfers like fiscal stimulus, and a lower-bound for the overall longer run effects.

4.3.2 Contributions to the literature and policy implications

As we discuss in the introduction, our results contribute to the existing literature in a number of ways. First, our paper adds to the literature examining how transitory shocks to cashflows impact household spending. A large literature, stemming from work by Blundell et al (2008) and even earlier work by Campbell and Mankiw (1990) attempts to understand the pass-through of various income shocks into spending.[26] This class of papers utilizes the covariances of income and consumption viewed through the lens of a parametric statistical process governing earnings to decompose variation in earnings into different types of shocks to understand pass-through. Rather than relying on parametrization of the earnings process, our paper follows an alternate branch of the literature in utilizing a natural experiment to provide evidence of pass-through of a shock to refund issuance timing driven by a policy change.

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exhibiting a spike in spending also alter the timing of that spike in response to changes in the timing of cashflows. This appears consistent with our results. As such, throughout the paper, we will use the term "response per EITC refund dollar", which is equivalent to "response per EITC refund dollar shifted" based on the argument laid out here.

[26] Japelli and Pistaferri (2010) describe the various approaches to recovering spending responses to income shocks, including parametric statistical decompositions and exploiting natural experiments. See their review article for a summary of the literature.

In a closely related paper to ours, Baker and Yannelis (2017) use data from a personal finance website to study the spending effects from the 2013 federal government shutdown, which shifted the timing of paycheck receipt for many federal government employees by a few weeks but kept permanent income unchanged. Gelman et al (2019) exploit similar variation in a related paper. These sets of authors estimate MPCs between 0.3 to 0.6 and find that the spending response is driven by households with negative or zero net savings, though households do tap into various forms of credit to smooth through the shock. Parker et al (2013) exploit differences in the timing of \$300-\$600 stimulus check disbursements over a three-month period to identify an MPC of 0.5 to 0.9 when including durables. Differences between these papers and ours likely reflect the fact that insurance mechanisms available to households differ based on the magnitude and duration of shocks (Kaplan et al, 2016). While these papers establish the pass through of delays in smaller (regular or irregular) payments to households, ours builds on these existing results by showing how households respond to a short, roughly two-week delay in a much larger lump-sum payment, equal to a couple months of typical paychecks for the average EITC-eligible household. Despite the very short duration of the delay, we find considerable pass-through to spending, even in merchant categories predominantly selling nondurables and services.

Our estimates also provide information about the impact of large lump-sum payments on spending. As we discussed in Section 4.3.1, the responses we recover can be thought of as capturing the excess sensitivity to EITC refund receipt and provide a lower bound on the *overall* effects of refunds—including the impact on households that are able to smooth spending over longer horizons. Moreover, the fact that we recover local aggregate effects using state-level data provides value to policy makers that may be interested in the "all in" effect of a policy—inclusive of spillovers—when considering its costs and benefits.

We view our estimates as informative not only about how spending responds to delays in large lump sums, but also about how household consumption responds to the timing of payments more generally. In particular, households that display a shift in the timing of their spending in response to a shift in cash-flows must also exhibit some spike in spending around refund issuance. As such, our work complements several papers that have shown spending responds when households receive tax refunds, even though refund amounts are often known in advance (Barrow and McGranahan, 2000; Ferrell et al, 2019), and those that have studied excess sensitivity in other settings (Stephens, 2003, for example).

Our work also contributes to the body of papers that have attempted to estimate responses to tax refunds over a more general population. Farrell et al. (2019) track daily financial flows for half a million families with Chase checking accounts that received a tax refund and find that for relatively low-liquidity households whose tax refund exceeds their account balance, about 60 percent of the refund is spent within one month of refund receipt. More recently, Baugh et al. (2021) use account-level data to estimate spending out of income tax refunds and payments. Most relevant to this study, the authors estimate a cumulative MPC of about 0.2 thirty days after refund receipt for households in the bottom quintile of observed account balances. And Gelman et al. (2019) estimate large MPCs out of large tax refunds for households with relatively little cash on hand, though it is important to note that the authors specifically focus on higher income households that are likely ineligible for the EITC.[27] Our paper builds on this literature by focusing on the spending response to refunds issued to EITC claimants, an important behavioral response given that these refunds affect nearly one-fifth of U.S households and represent a substantial income flow for the vast majority of them. Moreover, the EITC is among the largest transfer programs in the US, and the population receiving EITC is of particular interest given overlaps with eligibility for other discretionary policies such as stimulus and the expanded Child Tax Credit currently under consideration.

The existing literature on EITC has largely focused on implications for labor supply, and understanding spending responses may provide additional insight into the efficacy of the program for boosting demand and improving welfare.[28] In fact, survey data collected by Maag, Roll, and Oliphant (2016), show households receiving EITC may be unprepared to weather shocks to their cashflows, and Jones and Michelmore (2018, 2019) show household finances

[27] Variation in results across these papers is consistent with differences in the underlying data and samples. For example, Farrell et al (2019) use a broader measure of spending than our study, including bill payments and cash withdrawals, and find larger effects. Baugh et al (2021) use a sample that excludes users of RALs and others with low liquidity and find smaller effects with no spending prior to receipt. Baugh et al (2021) and Gelman et al (2019) both use household-level data from personal finance apps, which may lead to concerns about selection into the sample on unobservable characteristics such as financial literacy. While our data do not allow us to study household-level heterogeneity, it is based on far more representative source data covering a broader set of overall spending gathered at the merchant level. Our results, therefore, also provide some confirmation that selection into personal finance apps is not significantly biasing results in these papers.

[28] As we mention in the introduction, Nichols and Rothstein (2016) provide a thorough review of the academic literature on the EITC. More recent papers by Lim and Michelmore (2018), Bastian (2020), and Michelmore and Pilkauskas (2020) explore implications for labor supply, particularly among mothers. Manoli and Turner (2018) show tax receipts for EITC households may increase college enrollment, particularly for cash-strapped households. Ramnath and Tong (2017) show that an exogenous shock to filing (eligibility for 2008 stimulus payments) led to persistent changes in filing behavior and EITC claiming, which reduced poverty rates.

improve for EITC households following refund receipt. The handful of prior studies estimating spending out of the EITC have focused on longer time horizons than our paper due to data limitations (Barrow and McGranahan, 2000; Goodman-Bacon and McGranahan, 2008). As a result, confounding factors in the months surrounding receipt may impede their ability to isolate the effect of the EITC from other factors influencing spending in the months following receipt. Our daily data allow us to exploit high-frequency timing shifts in EITC refund issuance to isolate variation driven by refund receipt from these sorts of confounding factors. Moreover, our state-level data allow us to recover local aggregate effects from the policy, which may be informative for policymakers considering the aggregate impacts of changes to EITC. For example, our estimates can be used to assess the net costs of the EITC – that is, EITC outlays less additional taxes and the reduction in other government transfers to EITC recipients. In a recent paper on the topic, Bastian and Jones (2021) utilize our preliminary EITC refund spend out estimates (Aladangady et al., 2018) to calculate additional sales tax paid for each dollar of federal EITC expansion.

In addition to tax refunds, our estimates are informative about the impact of large, lump-sum payments on local spending more generally, and particularly those that are targeted at lower-income households similar to the EITC-eligible population. Our results add to the large existing literature estimating spending responses to lump-sums such as fiscal stimulus payments.[29] The fact that we recover local aggregate effects using state- and MSA-level data builds on these existing studies that utilize household spending directly. Moreover, our results suggest spending responses to lump-sum payments—particularly among lower-income households—may be *very* quick. This fact appears to be consistent with several recent working papers studying the impact of various rounds of stimulus during the COVID pandemic (Baker et al, 2020; Karger and Rajan, 2020; Chetty et al, 2020; Farrell et al, 2019).

[29] Several papers study the impact of lump-sum transfers to lower-liquidity or lower-income households using household-level data. Kueng (2018) exploits variation in annual dividend payments from the Alaska Permanent Fund, and estimates an MPC of roughly 0.3 for low-liquidity households, but just 0.1 for households in the lowest income quintile. Misra and Surico (2014) use the CEX coupled with a heterogeneous response model to study the spending response to the 2001 and 2008 tax rebates, finding that renters with relatively low income spent between 10 and 40 cents for each dollar of rebate. Broda and Parker (2014) use Nielsen scanner data to estimate the spending response to the 2008 tax rebates. The authors estimate a cumulative MPC of roughly 0.2 three months after receipt for low-income households and those reporting insufficient liquidity. And finally, Parker et al. (2013) use the CEX to estimate an MPC out of the 2008 tax rebates for relatively low-income households. The authors obtain estimates of 0.25 for spending on non-durables and a considerably larger 1.3 for total spending. All of these papers utilize household-level data, and our work builds on these by estimating a local aggregate response.

Additionally, our study utilizes data primarily from the late stages of a long expansion period, and household responses may be sharper during recessions. Notably, Gross et al. (2020) find MPCs may be state dependent and rise during bad times as household credit constraints are more binding. Other results in the literature suggest lower-income households may suffer larger swings in income during recessions, potentially leading them to be more hand-to-mouth in recessions (Guvenen, et al, 2017; Patterson, 2021). Even considering these reasons, our estimates suggest a local "relative" transfer multiplier of 0.4 or a government spending multiplier of 1.4 during expansions.

Furthermore, to our knowledge, the statistically significant anticipatory spending effect that we observe in the week prior to EITC refund issuance has not been documented in previous research on spending responses to predictable income changes. In part, this reflects the fact that our data and empirical strategy allow us to recover responses week-by-week. Notably, aggregation to lower frequencies may, in fact, attenuate measured responses. In fact, data aggregated to calendar months may not be able to identify the effect from our experiments, as peak refund issuance is shifted by a couple weeks, largely within February. Even considering some refunds shifted from February to March, our weekly estimates suggest responses estimated from monthly data would be biased downwards.[30]

The anticipatory response suggests some households are willing to run down account balances or use short-term credit once their projected refund magnitude and issuance date are known. This behavior is facilitated in recent years by tools such as the IRS' 'Where's my refund' website and the reemergence of RALs offered by third-party tax preparers like H&R Block.[31] Despite observing a spending response prior to receipt of an anticipated income flow, it is important to note that this response is not consistent with the predictions of a benchmark rational expectations/permanent income consumption model; if it were, then we would expect to observe a spending response much earlier than one week prior to EITC refund issuance, given

[30] Peak refund issuance shifted from about February 10 to February 23 (see Figure 1), providing no identification in monthly data. Some refunds were shifted from February into March, providing identification in monthly data. However, refunds shifted into early March would continue to support some spending within February. As an example, $1 of refunds shifted into the first week of March would only shift about $0.20 of spending into March compared to our overall spend-out estimate of $0.27.

[31] The availability and utilization of RALs picked up following changes in IRS policy on disclosure of tax liens in roughly 2013.

that even prior to filing their taxes, most low-income households have correct mean expectations about their refund (Caldwell et al., 2018).

4.4 Robustness checks and alternative specifications

In our baseline estimates, we find that spending rises by about $0.27 per EITC refund dollar cumulatively over the few weeks around issuance. Our estimates are based on a rich fixed effects model that accounts for a number of potential confounding factors. In addition, we show in Appendix Table A.2 that results are very similar in several alternative specifications, such as exclusion of states subject to a winter blizzard in 2016, inclusion of additional leads and lags, and alternate choices of sample years around the PATH Act. In all cases we study, point estimates range from 0.24 to 0.30, and remain close to our preferred baseline.

We highlight a couple of interesting specifications here. First, we note that the shift in spending appears to be persistent, consistent with the shift in refund timing. In particular, results are little changed when we exclude 2017—the first year following the PATH Act—and remain similar when including 2019. Consequently, any surprise from the shift in refund timing in 2017 does not appear to have materially affected our results, and households that exhibit excess sensitivity appear to have shifted their spending in line with the shift in refunds each year following the PATH Act.[32] As we discussed in Section 4.3, this fact also supports our interpretation that our results reflect both the impact of the shift in refund timing on spending, and the excess sensitivity of spending to refunds.

Second, as described in Appendix C, we use an instrumental variables (IV) strategy that identifies spending resulting from EITC refund receipt based solely on the exogenous variation in EITC refund timing and magnitudes generated by the PATH Act. Under this specification, we estimate a cumulative spend out of 0.27, very close to our baseline estimate. The similarity of the IV result suggests that the variation in EITC refund timing unrelated to the PATH Act is either small or exogenous with respect to consumption. Indeed, discussions with the IRS suggest that much of this variation – such as the difference in early EITC refund issuance patterns between 2015 and 2016 shown in Figure 1 – is the result of idiosyncrasies in processing and

[32] As of the 2016 tax filing season, Maag et al. (2016) find that over 90 percent of EITC claimants were unaware that their refunds would be delayed during the 2017 tax filing season, despite the fact that the PATH Act legislation was passed in late 2015.

disbursing refunds at the IRS and Treasury. As such, we prefer the fixed-effects baseline specification, which has more power and provides tighter estimates of the weekly response.

Third, we provide a specification that utilizes MSA-level spending data, which provides a cumulative spending estimate of 0.25, close to our state-level baseline. While we can create spending measures more disaggregated than the state level, disaggregating further poses tradeoffs.[33] Importantly, aggregation to the state level does not bias our results, and because our state-level results provide sufficient statistical power to identify the spending response robustly, we prefer these as our baseline estimates. However, disaggregation does allow for better comparisons across MSAs that may differ in local credit or macroeconomic conditions. In Appendix E, we provide further details on this alternative specification. Though estimates are somewhat noisy, they are suggestive that responses may be dependent on local or household-level economic conditions, as found by Gross et al (2020).

5. Conclusion

We estimate the magnitude, speed, and composition of the spending response to high-frequency timing shifts in a very large, predictable, and regular payment to low-income households. Our ability to do so rests on a combination of a novel data set and plausibly exogenous variation induced by the EITC legislation. Specifically, we are able to exploit a roughly two-week shift in EITC refund issuance timing driven by the PATH Act by using a new daily, geographically-disaggregated data set combining retail spending measures derived from card transactions and IRS refund issuance data. Moreover, our result provides evidence that local spending responds to the timing of refund receipt itself and provides a bound on the overall impact of lump-sum transfers to low-income working households.

Our results show a sizeable and speedy spending response to the shock, with the average EITC refund yielding an additional $1,150 of retail spending within just a few weeks of issuance. We also find that, upon refund receipt, spending at grocery stores increased by about 40 percent relative to the typical monthly grocery spending of an EITC recipient, which strongly suggests

[33] Notably, our daily IRS refund issuance data is at the state level, but we must impute MSA-by-day issuance using daily state issuance and annual shares of refunds in various counties within a state. This means variation in timing predominantly arises from the state level, and disaggregation offers little benefit in terms of power. In fact, additional noise in MSA-level spending series likely reduces precision of these estimates somewhat.

considerable excess sensitivity of consumption to income receipt for a group that makes up roughly one-fifth of U.S households.

As policymakers continue to discuss how best to allocate fiscal relief during the recovery from the COVID recession, our results suggest that, beyond providing much-needed income support, stimulus payments and tax credits targeted towards low-income working households are likely to provide a sharp, quick boost to aggregate demand.

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