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The Impact of Government Transfer Payment Frequency on Consumption: Evidence from Delayed UI

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Abstract:
We study how the frequency of government transfer payments affects spending behavior. Our empirical approach uses transaction-level data on income and spending and exploits quasi-random delays in the receipt of unemployment insurance (UI) benefits. Spending drops by about half of the loss in income that occurs while individuals wait for UI benefits, revealing the value of periodic payments for liquidity-constrained individuals. Once delayed payments are received as lump sums, individuals reallocate spending toward less commonly purchased big-ticket categories that are dominated by durables. Our findings suggest that transfer programs with mixed frequencies, such as advance disbursements of lump-sum tax credits, can be beneficial to recipients.

JEL Classifications: E21, H53, H31
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1 Introduction

Government cash transfers play an important role in the income security of U.S. households. In 2019, when the unemployment rate was low, these transfers accounted for $1.6 trillion, or 7.5 percent of GDP (see, for example, U.S. Bureau of Economic Analysis (2024)). Cash transfers are disbursed in two ways. The first mode involves high-frequency (that is, monthly or weekly) payments and includes programs such as Social Security, Supplemental Nutrition Assistance Program (SNAP) benefits (formerly known as food stamps), unemployment insurance (UI), etc. The second mode involves lump-sum refundable tax credits disbursed once per year and includes programs such as the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC). Payment frequency can be influential for households with liquidity constraints. For example, constrained households waiting for lump-sum payments may rely on high-interest debt or postpone necessary spending. They might also choose to spend out of lump sums on categories that are different from the ones they would spend on out of frequent payments. Despite the economic significance of transfer programs, little is known about how payment frequency affects spending behavior. In this paper, we exploit a shock to the timeliness of UI payments to provide some of the first evidence from the United States concerning this question.

Our study considers UI claims at the start of the COVID-19 pandemic, when they surged from fewer than 1 million in February 2020 to nearly 19 million in April 2020. State UI agencies, understaffed and still dependent on legacy computer systems at the time, were unable to effectively process this large influx of claims, which resulted in some claimants receiving their weekly UI payments on time and others having to wait many weeks before receiving their benefits. Those who experienced delays ultimately received backdated payments in the form of lump sums when their claims were processed. As we show in the paper, this shock created quasi-random variation in the arrival of UI payments without any change in the total amount received. A key feature of the delay is that it changed the frequency of UI payment disbursement from smooth to lumpy. That is, individuals unaffected by the delays received weekly benefits of fixed dollar amounts over the course of their unemployment spell, whereas those affected by the delay initially received large lump sums followed by the conventional smooth allocation of payments thereafter.

Using transaction-level data on income and spending for a low-income sample of individuals, we first isolate UI claimants who experienced payment delays and compare them with UI claimants who obtained their benefits in a timely manner. We then implement an event-study-based approach to estimate the impact of the UI payment delays on spending behavior. Using this framework, we find that spending dropped by 46 percent of the income decline over the roughly five-week period that UI payments were delayed and rebounded completely once the backdated UI payments were received. These results suggest that the low-income population we analyze did not have sufficient liquidity to smooth such a temporary shock. We find evidence that welfare losses were mitigated as delay-affected individuals temporarily reduced discretionary spending on, for example, purchases from big box and clothing stores, home maintenance, and debt service payments.

Once the backdated UI payments were finally received as lump sums, spending on vehicles, hardware items, home maintenance, and auto maintenance increased substantially. Although there was essentially no change in the total dollar amount of income and spending once the backdated UI payments were received, we find evidence that delay-affected individuals used the lump sums to reallocate spending toward infrequently purchased and high-value categories, which are dominated by durables. To complement this result, we analyze data from the Consumer Expenditure Survey and find that households in states with greater exposure to UI payment delays (and hence more exposure to lump-sum transfers) increased their expenditure on durables relative to nondurables.

Following models that distinguish between nondurable and durable consumption, such as Luengo-Prado (2006) and Laibson et al. (2022), we note that periodic payments and lump sums, respectively, favor different margins of consumption. Periodic payments help individuals smooth nondurable consumption and spending, which coincide. By contrast, with durables, for which spending adjustments are larger and less frequent, consumption and spending do not coincide. That is, the utility flow derived from the stock of durables is smooth, but the spending decision itself is lumpy. For these categories, lump-sum transfers act as a form of forced saving that facilitates big-ticket purchases by enabling individuals to surmount borrowing constraints or down-payment requirements.

Our empirical results highlight the importance of each of these margins. In low-liquidity states, where individuals do not have access to periodic UI payments, we find significant reductions in spending. By contrast, when individuals receive backdated UI payments as lump sums, we find sharp increases in spending with reallocation toward big-ticket purchases. These findings are relevant for assessing the efficacy of recent and prospective reforms to major income support programs. For instance, the expanded CTC in 2021 was disbursed with mixed frequency: Half of the eligible amount was paid in advance in the form of six equal monthly disbursements, while the remainder was reconciled as a lump-sum refund against tax liabilities. Similarly, several reform proposals call for changing the EITC so that some fraction of the credit is advanced in the form of payments spread across the year and the remainder is refunded as a lump sum (see, for example, Maag et al. (2021)).

Our paper makes two main contributions to the literature. First, we provide novel evidence from the United States about changes in the timing and composition of spending arising from quasi-random variation in transfer payment frequency.[1] Existing evidence about this question comes from lower-income countries. Haushofer and Shapiro (2016) compare lump-sum versus monthly unconditional cash transfers in the context of a randomized controlled trial in Kenya. They find that monthly transfers improve food security while lump sums are more likely to be spent on durables. Aguila et al. (2017) study the introduction of social security programs in Yucatan, Mexico, that, respectively, pay benefits on monthly and bimonthly schedules. Using survey data on spending decisions, they find that monthly payments enable individuals to smooth consumption while bimonthly payments enable individuals to purchase more durable goods. Our results are consistent with these findings, even though our paper focuses on the United States, where, compared with

[1] Kramer et al. (2019) analyze a 2014 pilot that provided 504 Chicago public housing recipients with half of their anticipated EITC in advance in four quarterly payments. While participants reported an improved sense of financial stability, the treatment was not randomized and no information on expenditures was collected.

Kenya or Mexico, individuals have greater access to credit markets and public insurance.

For our second contribution, we study the impact of liquidity shocks on spending, holding permanent income fixed. Analyzing such a shock with disaggregated spending data sheds light on how individuals navigate low-liquidity states. Existing evidence on pure liquidity shocks comes from Baker and Yannelis (2017) and Gelman et al. (2020), who study the temporary halting of government paychecks and ensuing furloughs during the 2013 U.S. government shutdown. Because this pair of studies compares the spending of government workers with the spending of non-government workers, the estimated effects are attributable not only to a temporal reallocation of income, but also to the effect of furloughs on work-related expenditures, time use, and home production (see, for example, Aguiar and Hurst (2005)). By contrast, our paper studies a group of individuals who all experience job loss (and receive UI) but differ only in terms of whether the UI was received with a delay or not. Compared with studies of the government shutdown, our analysis isolates the impact of liquidity shocks from confounding effects arising from work-related expenses or time budget-induced changes in home production.

Our paper is also related to the literature on UI expansions that occurred during the pandemic. Ganong et al. (2024) investigate the impact of these policies using large-scale administrative data on JP Morgan Chase customers. Their empirical approach exploits a variety of quasi-experimental changes, including UI payment delays, to study the impact of increased UI generosity on spending and labor supply behavior. By contrast, our paper studies high-frequency spending effects before and after the delay and analyzes how the shock generates reallocation across different categories of consumption. Navarette (2024) exploits state-level variation in computer systems used to process UI claims to study the aggregate effects of UI payment delays. He finds that the general-equilibrium result of the delays was a reduction in aggregate expenditures, which implies a deterioration in the automatic-stabilizer effect of UI.

2 UI Payment Delays Were Widespread during the Pandemic

The COVID-19 pandemic lockdowns caused an unprecedented increase in UI claims. From roughly mid-March to mid-May of 2020, UI claims exceeded 2 million per week, with a peak of roughly 6 million weekly claims in early April (see Figure 1). As claims spiked, state-level UI programs were enhanced through the Coronavirus Aid, Relief, and Economic Security (CARES) Act, which became law in March 2020. These enhancements added to weekly benefit amounts and benefit duration, expanded coverage to traditionally excluded members of the workforce such as self-employed workers, waived mandatory waiting periods, and eliminated job-search requirements.

Faced with this unexpected spike in UI claims, state UI programs struggled to process claims in a timely manner because they were understaffed and still relied on legacy computer systems (see, for example, Government Accountability Office (2022)). Consequently, as shown in Figure 1, the share of claims nationwide that were paid within 21 days fell from about 97 percent in March 2020 to about 52 percent in June 2020.

Because the UI payment delays resulted from technical limitations and resource constraints, some unemployed workers obtained their benefits relatively easily while others experienced periods without any benefits for reasons outside their control. As we show in the following section, this argument for quasi-random UI payment delays is borne out in our data.

3 Data and Identification Strategy

In this section, we provide an overview of the account-level data that we use for our main analysis. We explain how we identify payment-delay-affected individuals in the data and then explain the econometric approach that we use to estimate the impact of the delays.

3.1 High-frequency Account-level Data from Facteus

Our analysis relies on Facteus' U.S. Consumer Payments data panel, which is a transaction-level data set. Facteus' data cover unbanked individuals who use payroll debit cards as a substitute for conventional bank accounts and are directly sourced from financial institutions. Broadly speaking, the accounts in Facteus' data function like conventional bank accounts; that is, they enable holders to receive payroll income and spend on goods and services. Importantly, these accounts can be used to receive government transfers such as UI payments, tax refunds, and stimulus payments. Facteus' data have been used to study spending responses to stimulus payments in Karger and Rajan (2020) and Cooper and Olivei (2021).

Each transaction in the data is associated with a time stamp, a dollar value, a transaction class that identifies whether it is income or spending, and a label that provides additional information about the transaction. Labels about spending could include the name of the merchant, while labels about income include the identity of the payer, such as an employer-specific payroll deposit or the U.S. Treasury in the case of a stimulus payment. As we describe later, these labels are critical in identifying UI income. Spending transactions also include merchant category codes (MCCs), which are reported by merchants to payment card networks. Using MCCs, we can allocate spending into categories such as grocery stores, ATMs, gas stations, etc.

While we can obtain the account holder's date of birth and home Zip code from Facteus, it is important to note that the Facteus data uniquely identify debit card accounts but not individuals. Consequently, if an individual holds multiple accounts, we cannot consolidate information about that individual's spending transactions across those accounts. Based on correspondence with Facteus, Cooper and Olivei (2021) indicate that multiple account holding is unlikely in Facteus' data, especially since the sample skews toward low-income consumers.

3.2 Sample Construction and Summary Statistics

We begin by identifying accounts that received UI payments between March 1, 2020, and June 1, 2020. We focus on this time period because it captures the initial spike in unemployment generated from the onset of the pandemic, as shown in Figure 1. We identify UI deposits through transaction labels that match the deposit labels used by state UI agencies. An exhaustive list of UI-specific deposit labels is presented in Appendix Table D1. The list was constructed from information provided by payment systems industry associations and supplemented with public information including Twitter posts.

We turn next to identifying the treatment group, that is, isolating accounts that received delayed UI payments. We do so using two related methods (additional details are provided in Appendix A):

1. Our first method identifies accounts that received more than one UI payment deposit on the day that the UI spell began but received a smooth weekly allocation of benefits thereafter. The key idea is that a burst of deposits received at the start of the UI spell indicates delayed benefit disbursement associated with preceding weeks of insured unemployment.

2. Our second method relies on the extent to which UI payment disbursements are unusually lumpy at the start of the spell. The main idea here is that a single large initial payment represents a lump-sum delayed disbursement for preceding weeks of insured unemployment.

The control group thus comprises accounts that do not meet either of the conditions described above.[2] We finally restrict our sample to accounts for which we observe 24 weeks of activity before the first UI payment deposit and eight weeks of activity after the first UI payment deposit. Activity is defined as no more than one consecutive week without spending or deposits.

Table 1 shows average characteristics of the treatment and control groups constructed for the period from 24 weeks before the start of the UI spell through six weeks before the start of the spell. As we show in the next section, this period defines the window before job loss and UI payment delays. For each variable, we report the control mean in the first column. In the second column, we report the treatment group mean as the sum of the control mean and the treatment effect, after residualizing state and start-of-UI calendar week fixed effects. The p-values shown in the third column test for equality in the treatment and control group means, conditional on state and start-of-UI calendar week fixed effects. We compare our sample with survey data on unbanked households from the 2019 wave of the Survey of Consumer Finances (SCF) in the fourth column. Households in the SCF are considered unbanked if none of the occupants has a checking account—a sub-population that represents about 6 percent of households nationally. Notably, these households have median incomes that are only 30 percent of the SCF median, underscoring the financial fragility of this low-income population.

The first four rows of Table 1 show variables that can be measured both in the Facteus samples and the SCF. In the Facteus data, individuals are in their late 30s, which is marginally younger than the unbanked households in the SCF, where the average age is about 42. The share of individuals making credit card payments is about 11 percent, while the equivalent share among unbanked households in the SCF is 24 percent. Debt servicing beyond credit cards (that is, auto loans, pay-day loans, mortgages, and student loans) is about 15 percent in the Facteus data, which is

[2] In Appendix B, we use data from Connecticut, where claim dates are recorded in the deposit labels, to validate our data-driven procedure.

substantially lower than the 50 percent incidence in the SCF. Because it is at the individual level and net of taxes, the $450 average weekly income in the Facteus data is lower than weekly pre-tax income at the household level in the SCF, which is about $605.

The next set of rows shows average weekly spending, which we disaggregate into categories that are constructed using MCCs. Average weekly spending, which ranges from $320 to $330, represents a little less than 75 percent of average weekly income in both the treatment and the control groups. Given that low-income individuals are likely to be hand-to-mouth consumers, the relatively high saving rates from these data suggest that some spending transactions could be missing. Nevertheless, we do not find any statistically significant difference in income or spending between the control and treatment groups, which suggests that unobserved spending (if any) is likely missing at random.

Moving next to the different categories, we see that cash transactions are the biggest source of expenditure, followed by food and grocery stores. Billables, which cover utilities, phone, and internet, are next. The shopping category covers transactions at retailers such as Amazon, clothing stores, etc. Spending at restaurants is about one-third of the spending at grocery stores. Gasoline, non-MCC transactions (which include person-to-person payments such as Venmo and Paypal), auto maintenance, and debt servicing round out the top-10 named categories. The remaining categories represent less common transactions.[3] On the whole, the statistics in Table 1 suggest that the Facteus sample is broadly similar to the low-income unbanked household population as described in the SCF. In addition, individuals who experience UI payment delays appear very similar to those who do not experience delays, not only in terms of income and overall spending, but also in the way that they allocate expenditure across a variety of categories.

3.3 Identification Strategy

Let i index individuals, and t index calendar days. Let k index seven-day intervals relative to the receipt of UI, with $k = 0$ being the seven-day interval in which an individual receives their first UI payment. Define T_{ik} as an indicator variable that is equal to 1 if individual i experiences a UI delay and is k seven-day bins away from their first UI payment. We estimate the effect of delays on income and spending using a two-way fixed effect event study design. Our regression specification is written as

$$y_{it} = \alpha_i + \theta_{st} + \sum_{k=-24}^{7} \beta_k \times T_{ik} + \varepsilon_{it}. \tag{1}$$

In Equation (1), the outcome variable y_{it} represents income or spending, α_i and θ_{st} are individual and state-by-calendar day fixed effects, and ε_{it} is the error term. The β_k coefficients are the main parameters of interest, which capture the evolution of differences in the outcome variable between the treatment and control groups in seven-day intervals. The inclusion of state-by-calendar day fixed effects allows us to flexibly control for state-level trends in COVID-19–induced lockdowns and

[3]Rent is classified through MCCs only in instances where landlords take card payments.

their effect on consumer spending.

To address potential bias stemming from variation in treatment timing across individuals, we estimate Equation (1) using the interaction-weighted (IW) estimator proposed by Sun and Abraham (2021). Consistent estimation of the β_k parameters relies on the assumption that unobserved determinants of income or spending are not systematically related to whether an individual experiences a UI payment delay, conditional on individual and state-by-calendar day fixed effects. This assumption hinges on the institutional features of pandemic-era UI disbursement whereby benefit processing delays driven by technical issues randomly affected some claimants but did not affect others.

4 Spending Responses to Delay-induced Liquidity Shocks

In this section, we provide estimates of the impact of UI payment delays on income and spending. We then study how different components of spending change when individuals are waiting for delayed UI benefits, after they receive those benefits, and the net effect of the delays.

4.1 Delay-induced Trajectory of Income and Spending

Figure 2 plots the β_k coefficients from Equation (1) for income and spending. The horizontal axis measures event time in seven-day intervals relative to the start of the UI spell for the treated group. The vertical axis measures the gap between the treatment and control groups. From week –24 through week –6, we observe no statistically significant difference either in income or in spending. We infer that this approximately three-month pre-period represents the evolution of income and spending before job loss, which allows us to evaluate our identification approach. The absence of pre-trends over this period provides evidence for the validity of our identifying assumption.

Total income for delay-affected individuals starts to drop five weeks before the receipt of UI benefits. Thus, variation in income in weeks –5 through –1 is driven by an unexpected waiting period during which individuals have applied for UI benefits but have yet to obtain them. In week 0, we observe a large spike in income for the treatment group. This one-time influx of liquidity represents retroactive UI benefits that are delivered after processing delays are resolved. After the receipt of the lump sum, the income trajectory for the treated group immediately reverts to the control group trend.

In the waiting period, spending follow a trajectory similar to income's, but the fluctuations are less extreme. From weeks –5 through –1, we find that income drops by an average of about \$408 per week, while spending during the same period drops by an average of about \$188 per week, or about 46 percent of the decline in income. Restricting spending to just nondurables, we find a drop of about \$107 per week or about 26 percent of the decline in income.[4] This pattern is consistent

[4] We isolate nondurables using the MCC-to-durables crosswalk in Ganong and Noel (2019). Their categorization splits MCCs into durables, strict nondurables, and nondurables. We exclude cash, debt servicing, digital goods (such as downloaded songs and games), and transactions without MCCs from the categorization. Our nondurable MPC estimate combines spending on strict nondurables and nondurables.

with some consumption smoothing as the life cycle permanent income hypothesis predicts. After the start of the UI spell, spending remains elevated not only in the week of the lump-sum receipt but also for the next two weeks, which is again indicative of smoothing. In the symmetric five-week period from weeks 0 through 4, we find that individuals increase spending by about 46 percent of the increase in income, which mirrors the smoothing propensity measured over the waiting period. The same estimate computed using nondurable spending is 28 percent.

In terms of overall spending, our MPC of 0.46 is most comparable to the results of Ganong et al. (2024), who also exploit variation in income from pandemic-era UI payment delays and find an MPC of 0.42. Our estimate is somewhat lower than that of Ganong and Noel (2019), who study consumption and UI more generally and report MPCs in the 0.77 to 0.83 range, and that of Gelman et al. (2020), who study the impact of the government shutdown and find an MPC of 0.58. In terms of nondurables, our MPC of 0.26 is very close to the estimates of 0.27 and 0.3, respectively, reported by Ganong and Noel (2019) and Baker and Yannelis (2017).

4.2 Changes in Spending Behavior across Different Categories

We next investigate how individuals changed their spending behavior within different categories before and after the delay and present our results in Table 2. Panel A shows total income and spending, Panel B shows expenditures split across MCC-based categories, Panel C shows expenditure split across durables and nondurables, and Panel D shows expenditure split based on relative transaction frequency and value.

We show the impact during the waiting period, the period after lump sums are received, and the net effect of the shock in columns (1) through (3), respectively. We obtain these estimates by estimating a version of Equation (1) in which we respectively replace the T_{ik} variables for $k \in [-5, -1]$ and the T_{ik} variables for $k \in [0, 4]$ with two indicator variables. The coefficient on the first indicator variable captures the daily average response over the five-week period when individuals are waiting for UI payments to arrive. Similarly, the coefficient on the second indicator variable captures the daily average response over the symmetric five-week period when individuals have received backdated UI payments. We compute the net impact of the shock by averaging the waiting effect and the backdated UI effect coefficients. The net effect measures how average daily responses are affected over the 10-week period when individuals are first waiting for UI payments to arrive and then spending out of their lump-sum payments. Finally, we divide each of the estimated coefficients by the control group's daily average for the same variable over the entire sample period. The reported estimates therefore represent proportional changes relative to the average counterfactual. We report delta-method standard errors in parentheses. Within each panel, we sort categories by the magnitude of the net effect shown in column (3).

From Panel A, we see that income drops by 68 percent while spending drops by about 43 percent during the waiting period. These reductions are approximately reversed in magnitude once UI payments are received. The net effect of the shock shown in column (3) is economically small and statistically insignificant. These point estimates reveal that the UI payment delays, while significantly shifting the temporal allocation of income, have no impact on permanent income. The UI payment delays therefore function as pure liquidity shocks whereby spending drops sharply in the low-liquidity state and bounces back completely in the high-liquidity state, yielding a net effect that is statistically indistinguishable from zero.

In Panel B, we study the impact of the shock on different MCC-based categories. During the waiting period, we see prominent drops in discretionary and deferrable categories such as home maintenance, shopping, and debt servicing. These results indicate that some consumption smoothing comes from short-term liquidity management through deferral of debt servicing and postponement of discretionary expenses, which is similar to the mechanisms found by Baker and Yannelis (2017) and Gelman et al. (2020) in their respective studies of the 2013 government shutdown. After the receipt of lump sums, we see large increases in big-ticket expenditure categories such as commercial (largely hardware equipment), vehicles, auto maintenance, home maintenance, and shopping.[5] Looking finally at column (3), we see statistically significant net increases in commercial, vehicles, and food and grocery spending, alongside net reductions in restaurant spending. Taken together, these estimates suggest that lump sums tend to induce more spending on higher-value durables relative to other categories.

In Panel C, we group MCCs into durables, strict nondurables, and nondurables using the crosswalk from Ganong and Noel (2019). Cash, digital goods such as downloaded songs and games, debt servicing, and transactions without MCCs are excluded from the categorization, so we do not show them in this panel. In the waiting period, we find reductions across all three groups followed by sharp rebounds after the receipt of lump-sum UI payments. On net, the shock appears to generate larger spending increases for durables relative to nondurables, although the net effects are noisy.[6]

In Panel D, we group MCCs into four mutually exclusive categories based on transaction frequency and transaction value: infrequent (or lumpy) and expensive, infrequent and cheap, frequent and expensive, and frequent and cheap. We partition the data this way because it highlights the role of lump-sum payments in supporting higher-value purchases, which are not commonly transacted. Combining all transaction activity in the baseline period between reference weeks −24 and −6, we define low-frequency MCCs as those with below-average log weekly transaction probability, and high-frequency MCCs are those with above-average log weekly transaction probabilities. Similarly, expensive MCCs are those with above-average transaction values, while cheap MCCs are those with below-average transactions values. Appendix C provides additional details about our classification. The key takeaway from Panel D is seen in column (3), which reveals a rotation in spending across categories. We see that the net impact of the shock is a statistically significant increase in the proportion of spending allocated to lumpy and expensive categories. Notably, more than half of the spending in this group is classified as durables. By contrast, we see a reduction in the proportion of net spending allocated to frequent and expensive categories, of which nearly 80 percent are cash

[5] The commercial category covers spending on electrical equipment, plumbing supplies, hardware equipment, and some other MCCs.

[6] Cash declines on net due to the lump-sum payments, but it is excluded from the durables/nondurables categorization. Consequently, the point estimates for the net effects shown in column (3) are all positive.

withdrawals. On this point we note that although cash withdrawals are themselves relatively large, the Federal Reserve's Survey and Diary of Consumer Payment Choice shows that cash tends to be spent on low-value transactions (see Foster et al. (2024)). Consequently, we see these results as indicative of a lump-sum-driven increase in expenditure on big-ticket items alongside a likely reduction in spending on small-value items.

5 Supplementary Evidence from the Consumer Expenditure Survey

In this section, we use data from the Consumer Expenditure Survey (CEX) to provide additional evidence about the effect of UI payment delays on the composition of household spending. We find that households with greater exposure to UI payment delays tilt their spending toward durables.

5.1 Constructing the Analysis Sample and Measuring UI Payment Timeliness

The CEX collects information on households' expenditures, income, and demographics. The survey is conducted through two independent samples: the Interview Survey and the Diary Survey. The Interview Survey is a panel survey that solicits information on major and/or reoccurring purchases once per quarter. Households in the Interview Survey are contacted quarterly and are asked to report their monthly expenditures over the three-month period leading up to their interview. By contrast, the Diary Survey is a cross-sectional sample of households that captures high-frequency snapshots of expenditures in the form of two self-administered one-week diaries. The structure of the Diary Survey allows for smaller and more frequent purchases to be recorded. In the CEX, expenditures on goods and services are reported at the level of universal category codes (UCCs). We employ the concordance used by Coibion et al. (2021) to map UCCs into durable goods, nondurable goods, services, and nonconsumption. We analyze data from 2019–2021 survey waves and restrict our sample to households with positive income whose heads are aged 25 to 64.[7]

The CEX does not capture information on labor force status or UI benefits receipt, which precludes measurement of UI payment delays at the household level. We rely instead on U.S. Department of Labor data to compute the share of initial UI claims in each state and month that are paid within 21 days, which we then studentize. This studentized variable becomes our measure of UI payment timeliness that we attach to each CEX survey respondent by state of residence and month. Appendix Figure D1 illustrates the variation that we exploit in this exercise by showing the sizable between-state dispersion in UI payment timeliness induced by the pandemic.

5.2 Association between UI Payment Timeliness and Expenditure on Durables

Indexing households by h and time at the monthly frequency by t, our specification takes the form of

$$y_{ht} = \lambda_{s(h)} + \kappa_t + \delta \text{UI Timeliness}_{s(h)t} + \gamma\prime \mathbf{X}_{ht} + \nu_{ht}, \tag{2}$$

[7]The analysis period of 2019 through 2021 allows us to compare spending behavior before and after the acute phase of the pandemic, during which UI claim rates were extremely high.

where the dependent variable, y_{ht}, is the ratio of durable goods expenditures to total expenditures. λ_s are state fixed effects, and κ_t are month fixed effects, which respectively control for time invariant state-level heterogeneity and seasonal factors. The parameter of interest is δ, which measures the impact of a one-standard-deviation improvement in UI payment timeliness on the share of total expenditure allocated to durables. We account for other determinants of the durables share of total expenditure through a vector of household-level controls, $\mathbf{X}_{ht}$. The control vector includes the state-level unemployment rate to account for the fact that UI payment receipt is unobserved at the household level in the CEX. It also includes log family income; the head of household's age and indicators for their race, sex, education level; and indicators for the household's family size, number of members under age 18, number of members over age 64, urban status, number of vehicles, and receipt of SNAP benefits. Additionally, for analysis using the Interview Survey, we observe indicators for the household's residence building type and number of bedrooms, which we include as controls.

Table 3 shows the estimates of δ from the Interview Survey and Diary Survey samples in columns (1) and (2), respectively. A one-standard-deviation improvement in UI payment timeliness is associated with a 0.3 percentage point reduction in the share of spending allocated to durables in the Interview Survey and a 0.4 percentage point reduction in the Diary Survey. Compared with the dependent variable means, which are reported in the bottom row of the table, these estimates represent a modest 5.5 to 5.8 percent reduction in the share of spending allocated to durables. A qualifier on the magnitude of these effects is that our UI payment timeliness variable is constructed at the state level rather than household level, leading to measurement error that attenuates the point estimates.

On the whole, our CEX-based analysis is consistent with our findings from the Facteus data, that more timely UI payment disbursement, which accords with the conventional smooth weekly payout of benefits, reduces households' spending on durables relative to nondurables and vice versa.

6 Conclusion

Although cash transfer payments are a critical form of income support for many households in the United States, little is known about the liquidity effect associated with the frequency of these payments. Our paper leverages transaction-level data on income and spending to identify individuals who experienced unexpected delays in receiving UI payments and compares them with individuals who receive UI payments in a timely way. The shock we isolate represents a change to the timing of benefit receipt without any change in permanent income, as the backdated benefits are eventually received as lump sums. We find that spending drops by 46 percent of the decline in income while individuals wait for their benefits to arrive but fully recovers once lump-sum payments are received. Although the net impact of the shock on income and spending is zero, we find evidence that lump-sum payments induce a rotation toward less commonly purchased, higher-value items dominated by durables and away from cash withdrawals. Less sharply identified estimates from the

CEX also suggest that UI payment delays increase households' expenditures on durables relative to nondurables.

Our findings highlight that transfer payment frequency is influential for individuals who are liquidity constrained, which likely describes the budget situation of many cash transfer recipients. For these individuals, periodic payments can support nondurable consumption, which is coincident with spending. By contrast, infrequent lump-sum payments can act like forced saving that supports expenditure on big-ticket durables. These results suggest that recent reforms considering mixed-frequency disbursements of lump-sum transfers such as the the EITC and CTC could improve recipients' welfare.

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Table 1: Selected Summary Statistics

	(1)	(2)	(3)	(4)
	Facteus Analysis Sample			2019 SCF
	Control	Treated	p-value: (1)-(2) = 0	Unbanked Households
Age	37.73	39.03	0.004	41.80
Share with Credit Cards	0.112	0.107	0.689	0.243
Share with Any Debt Payment	0.148	0.146	0.904	0.504
Weekly Income	450.06	439.75	0.331	605.06
Weekly Spending	328.26	319.49	0.234	
Cash	84.20	78.40	0.112	
Food and Grocery Stores	72.20	68.21	0.189	
Billables	39.95	39.52	0.754	
Shopping	28.21	28.46	0.828	
Restaurants	24.15	24.87	0.409	
Gas	18.56	17.97	0.471	
MCC Unavailable	15.20	15.95	0.737	
Auto Maintenance	7.22	6.66	0.479	
Debt Servicing	5.38	5.22	0.841	
Miscellaneous Services	5.31	6.44	0.024	
Rent	3.76	3.50	0.747	
Unclassified Travel	3.45	3.25	0.601	
Entertainment	3.44	4.08	0.025	
Other Spending	2.98	2.88	0.808	
Home Maintenance	2.64	3.20	0.171	
Fees and Payments	2.20	2.05	0.371	
Hotels	1.83	1.68	0.696	
Medical	1.79	1.74	0.845	
Subscriptions	1.46	1.48	0.804	
Contractors	0.86	0.92	0.731	
Airlines	0.75	0.46	0.121	
Commerical	0.54	0.40	0.392	
Organizations	0.51	0.44	0.679	
Education	0.45	0.58	0.377	
Travel Agency	0.44	0.51	0.574	
Car Rental	0.29	0.21	0.506	
Vehicles	0.26	0.30	0.717	
Lottery and Gambling	0.20	0.06	0.206	
Shipping	0.04	0.06	0.448	
N	3,384	1,638		1,000

Notes: All outcomes in the Facteus sample represent averages over the period starting from weeks –24 through –6 relative to the start of each individual's UI spell. The second column reports the treatment group mean as the sum of the control mean and the treatment effect, after residualizing state and start-of-UI calendar week fixed effects. The p-values shown in the third column test for equality in the treatment and control group means, conditional on state and start-of-UI calendar week fixed effects. We classify individuals as having credit cards if we observe a payment toward a credit card account at any time in our sample window. We classify individuals as servicing debt if we observe payments toward credit cards, auto loans, student loans, mortgages, or payday loans. SCF statistics are computed at the household level for survey respondents who have positive labor earnings but do not have checking accounts. Weekly income in the SCF is computed as total annual income divided by 52.

Table 2: Proportional Changes in Spending by Category over Different Time Intervals

	(1) Waiting Effect		(2) After Receiving UI		(3) Net Effect	
Category	Point estimate	SE	Point estimate	SE	Point estimate	SE
Panel A: Income and Spending						
Income	-0.683***	0.059	0.765***	0.064	0.041	0.06
Spending	-0.429***	0.037	0.484***	0.043	0.028	0.037
Panel B: Spending by Broad MCC Groups						
Car Rental	0.958	0.721	1.854	1.236	1.406	0.892
Commercial	0.41	0.334	1.673***	0.642	1.041**	0.41
Vehicles	-0.127	0.294	1.990**	0.911	0.932*	0.518
Travel Agency	-0.051	0.321	1.108**	0.496	0.528	0.359
Airlines	0.392	0.474	0.583	0.532	0.488	0.473
Other Spending	-0.227	0.253	0.820***	0.306	0.297	0.255
Fees and Payments	0.055	0.185	0.506**	0.214	0.28	0.185
Auto Maintenance	-0.321*	0.178	0.873***	0.252	0.276	0.194
Food and Grocery Stores	-0.415***	0.072	0.706***	0.087	0.146*	0.075
Home Maintenance	-0.804***	0.268	0.982***	0.352	0.089	0.284
Shopping	-0.528***	0.054	0.645***	0.086	0.058	0.062
MCC Unavailable	-0.368**	0.179	0.471**	0.233	0.051	0.192
Rent	-0.291	0.261	0.392	0.309	0.051	0.265
Medical	-0.350*	0.187	0.407	0.29	0.029	0.206
Debt Servicing	-0.573**	0.258	0.623	0.39	0.025	0.292
Billables	-0.409***	0.062	0.362***	0.072	-0.024	0.063
Miscellaneous Services	-0.490***	0.176	0.423**	0.214	-0.034	0.179
Cash	-0.472***	0.063	0.397***	0.076	-0.037	0.064
Hotels	-0.323	0.435	0.234	0.497	-0.044	0.449
Gas	-0.264***	0.046	0.174***	0.061	-0.045	0.05
Restaurants	-0.361***	0.042	0.205***	0.054	-0.078*	0.044
Subscription	-0.462***	0.142	0.143	0.2	-0.159	0.156
Entertainment	-0.479*	0.279	0.019	0.292	-0.23	0.276
Contractors	-0.542*	0.321	-0.165	0.391	-0.354	0.32
Unclassified Travel	-0.705	0.441	-0.344	0.453	-0.524	0.444
Organizations	-1.106**	0.484	-0.053	0.561	-0.579	0.49
Shipping	-0.438	0.685	-1.354*	0.762	-0.896	0.658
Education	-1.511	1.514	-1.406	1.56	-1.458	1.52
Lottery or Gambling	-1.259	1.058	-1.717	1.518	-1.488	1.283
Panel C: Spending by Durability						
Durables	-0.399***	0.088	0.622***	0.104	0.112	0.088
Non-durables	-0.565***	0.061	0.704***	0.084	0.07	0.066
Strict Non-durables	-0.371***	0.044	0.427***	0.053	0.028	0.045
Panel D: Spending by Transaction Frequency and Value						
Lumpy & Expensive	-0.231**	0.117	0.881***	0.148	0.325***	0.123
Frequent & Cheap	-0.413***	0.041	0.481***	0.051	0.034	0.043
Lumpy & Cheap	-0.582***	0.099	0.637***	0.122	0.028	0.101
Frequent & Expensive	-0.486***	0.051	0.339***	0.062	-0.074	0.052

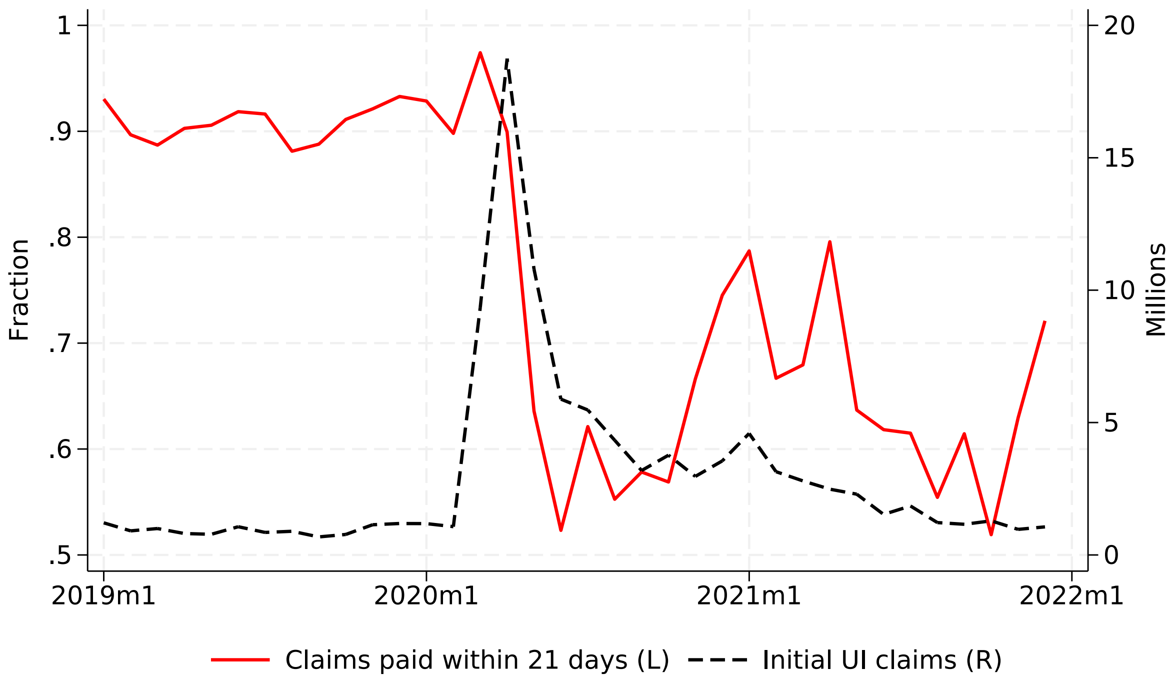
Notes: This table shows the proportional change in a given category (either income, spending, or a disaggregated measure of spending) for individuals who experience UI payment delays. Estimates represent proportional changes for a given category relative to the control group mean for that category computed over the entire sample period. See text for details. Standard errors are computed using the delta method. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effect of UI Payment Timeliness on the Share of Durable Goods Expenditure in the CEX

	(1) Interview Survey	(2) Diary Survey
UI Timeliness (standardized)	-0.00315***	-0.00415**
	(0.000802)	(0.00205)
Controls	Y	Y
State fixed effects	Y	Y
Month fixed effects	Y	Y
Mean of dependent variable	0.0574	0.0714
N	89,143	9,543

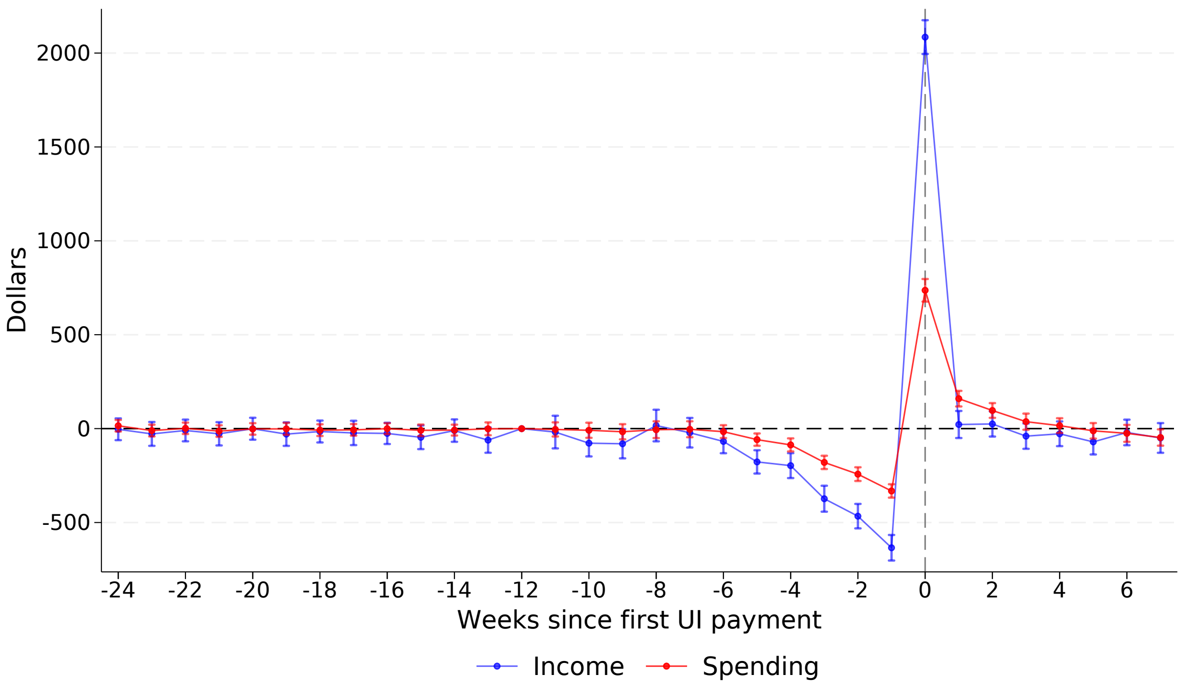
Notes: This table shows estimates of δ from Equation (2) obtained using 2019–2021 CEX data. The dependent variable is the ratio of durable goods expenditure to total expenditure. Standard errors clustered at the state level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Initial UI Claims and Processing Delays



Notes: Initial claims are from FRED via series ICNSA. U.S. Employment and Training Administration (2024). Fraction of claims paid within 21 days is from Department of Labor table ETA 9050.

Figure 2: Impact of UI Payment Delay on Weekly Income and Spending



Notes: This figure plots the $\hat{\beta}_k$ coefficients estimated from Equation (1) using daily income and spending as the respective dependent variables on a panel of 5,022 account holders (3,384 control and 1,638 treated). The estimated coefficients are scaled by a factor of 7 to convert them from daily averages (as implied by the regression equation) to weekly averages. Capped spikes show 95 percent confidence intervals, which are based on standard errors clustered at the individual level.

Appendix for online publication

Appendix A Additional Details on Sample Construction

A.1 Filtering Steps

We exclude individuals that received benefits under the Pandemic Unemployment Assistance (PUA) program. In our data, the large majority of identified PUA deposits (based on transaction labels) are observed in Ohio. PUA provided benefits to workers who were not eligible for conventional unemployment insurance either because they had insufficient previous earnings or limited work histories or were self-employed before the onset of the COVID-19 pandemic. Compared with individuals receiving traditional UI benefits, we find that PUA recipients had substantially different earnings trajectories before the start of a benefit spell.

As an alternative to direct deposit, some states disburse payments through standalone UI-specific debit cards that they issue. In California, all UI payments are made through standalone UI-specific debit cards. Because our analysis is restricted to individuals who we can observe receiving UI payments via direct deposit, our sample excludes UI recipients who receive benefits via standalone debit cards.

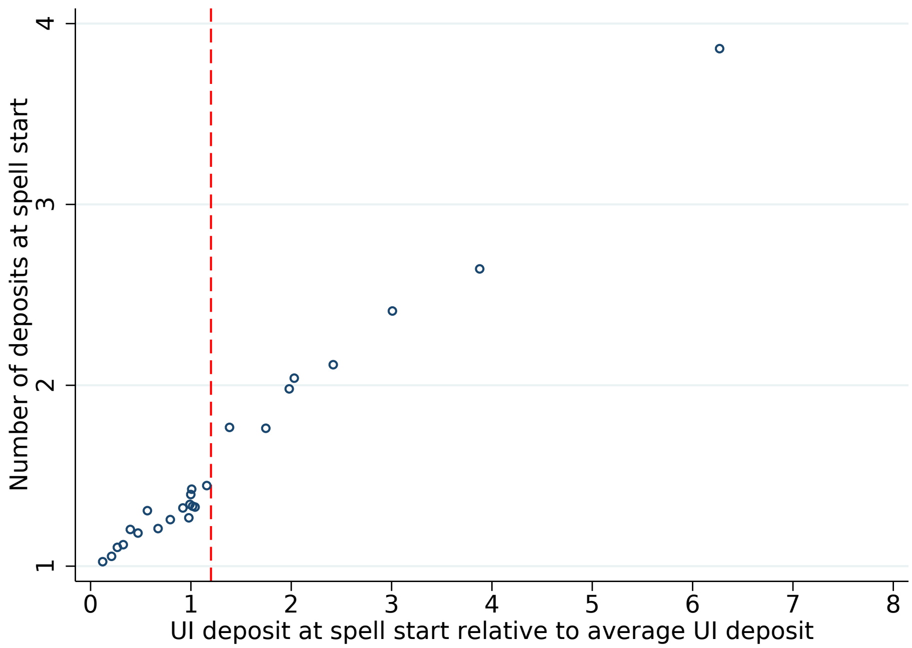
A.2 Identifying Lump-sum UI Payments

1. Our first method identifies accounts that receive more than one UI deposit on the day that the UI spell begins but receive a smooth allocation of benefits thereafter. Smooth allocations mean that individuals do not receive more than two payments in any week after the start of the UI spell. In some instances, $600 supplementary weekly benefits paid through Federal Pandemic Unemployment Compensation (FPUC) were disbursed after individuals began their UI spells. We identify initial FPUC disbursements using state-level notices on FPUC payment start dates and treat any mid-spell FPUC payments as conventional (that is, smooth) allocations of benefits. In most cases, individuals receive only one UI payment in a given week. In a small share of cases, individuals receive two payments mid-spell. The key idea is that a burst of deposits received at the start of the UI spell indicates delayed benefit disbursement associated with preceding weeks of insured unemployment.

2. Our second method relies on the extent to which UI benefit disbursements are unusually lumpy at the start of the spell. The main idea here is that a single large initial payment represents a lump-sum delayed disbursement for preceding weeks of insured unemployment. In Figure A1, we show the relationship between the implied size of the first UI payment deposit relative to subsequent UI payment deposits during a UI spell in the form of a binned scatterplot. The x-axis measures the ratio of the UI payment deposit at the start of the spell relative to subsequent UI payment deposits, and the y-axis shows the number of UI payment deposits made at the start of the spell. As is evident in the figure, observations with low ratios are

clustered together and less likely to receive multiple deposits at the start of the UI spell, while the converse is true for accounts with ratios greater than 1.2 (which is marked as a vertical dashed red line). We use the cutoff of 1.2 to further classify accounts that receive an initial UI payment deposit that is more than 120 percent of the usual weekly benefit that is subject to a delay.

Figure A1: UI Payment Deposit Patterns at Spell Start



Notes: Each dot in the figure divides the sample into 25 equally sized bins.

Appendix B Validating Delayed UI Payment Classifications

Our classification of treated accounts is indirect because information on the date of UI claims is typically unobserved in transaction-level data. However, we are able to validate our classification approach using data from Connecticut, where UI payment deposit labels include claim-week-ending (CWE) dates thereby providing information on the date when benefits are claimed. Comparing the first CWE date with the first benefit payment date for each account allows us to directly measure delay duration, thereby validating our treatment assignment procedure. Table B1 shows, in blue, an example of several CWE dates starting April 18, 2020, that precede the first deposit date.

For the Connecticut control sub-sample, we find that a median of days days elapse between the first CWE date and payment. For the Connecticut treated sub-sample, a median of 24 days elapse between the first CWE date and payment. These statistics are based on 144 control accounts and 97 treated accounts from Connecticut, where CWE dates are observed in the deposit labels. This validation exercise indicates that our data-driven assignment of treatment and control groups does well in classifying individuals who were waiting for UI benefits relative to those who received benefits quickly.

Table B1: Examples of Connecticut UI Payment Deposits

Transaction Label	Date	Amount ($)
CTDOL UNEMP COMP CWE 041820AHHFC	2020-05-14	247.59
CTDOL UNEMP COMP CWE 042520YAYQQ	2020-05-14	285.40
CTDOL UNEMP COMP CWE 050220ZTSS1	2020-05-14	289.49
CTDOL UNEMP COMP CWE 0509204KN15	2020-05-14	288.63

Notes: This table shows UI payments from Connecticut deposited on May 14, 2020. Claim-week-ending (CWE) dates, which are seven days apart starting April 18, 2020, are embedded in the transaction labels in blue.

Appendix C Data-driven Categorization of Spending

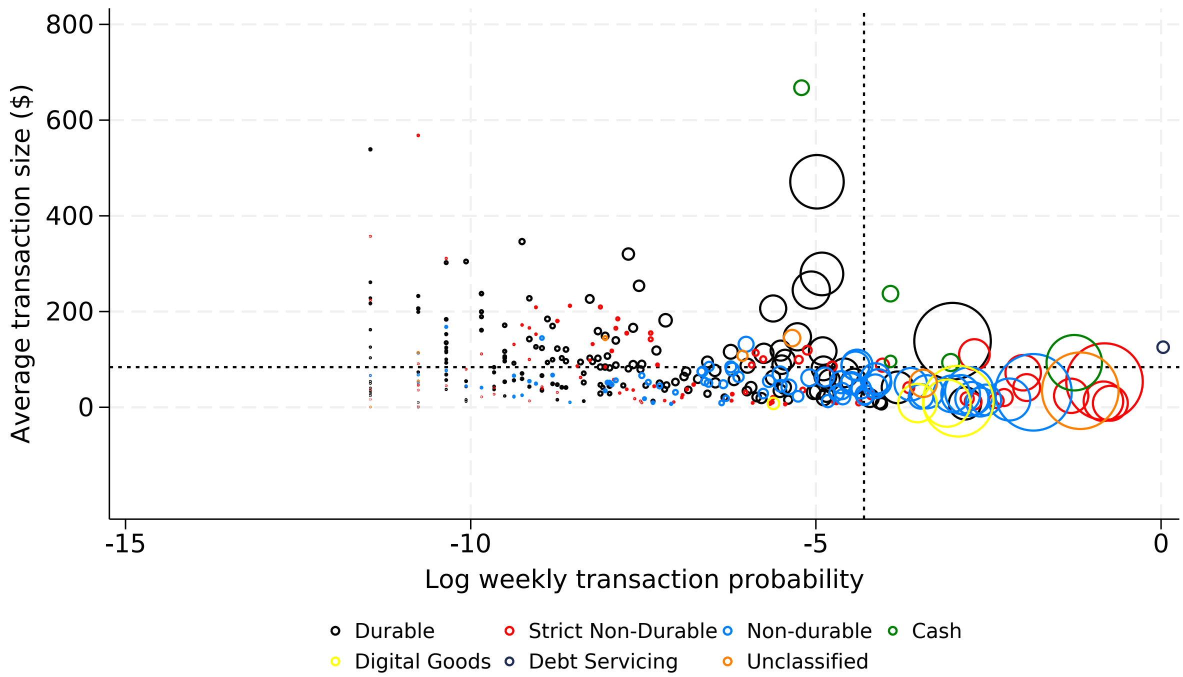
Figure C1 shows how we partition spending into four categories: The x-axis measures the log of the weekly probability that individuals spend in a given MCC, while the y-axis measures the average transaction amount for a given MCC. Each point in the figure represents an MCC weighted by its share of total expenditure. The transaction data underlying the figure represent a baseline period from reference weeks –24 and –6 relative to the start of the UI spell, that is, well before any job-loss-induced change in spending behavior. As is evident from the figure, there is a negative relationship between transaction amounts and the relative frequency with which they occur: High-value transactions are rare, while low-value transactions are more common. To provide more intuition about the data, we color the underlying MCCs based on whether they represent durables, strict nondurables, nondurables, cash withdrawals, digital goods, or unclassified MCCs.[8] In general, we see a pattern in which durables tend to be lumpy and of higher value, strict nondurables tend to be frequent and of lower value, and nondurables are generally of low value but vary considerably in their lumpiness. We consider MCCs with above-average transaction values as expensive and MCCs with below-average transaction probabilities as lumpy. The dotted lines in Figure C1 plot the relevant boundaries for lumpy versus frequent (horizontal axis) and expensive versus cheap (vertical axis), thereby defining the four categories.

Table C1 provides additional information on the four-way categorization of spending types during the baseline period. Types are shown in rows; average transaction amounts, average transaction probabilities, and extent to which each type is made up of durables, strict nondurables, nondurables, cash, digital goods, debt servicing, or MCC unavailable are shown in columns. Lumpy-expensive spending is dominated by durable MCCs, which together account for about 52 percent of spending in this class. Lumpy-cheap spending is made up of a roughly equal mix of durables and nondurables,

[8] Our classification of MCCs into durables and nondurables is based on the crosswalk for MCC-labeled transactions used by Ganong and Noel (2019). We exclude cash-related MCCs from the durable/nondurable distinction. Digital goods and non-MCC transactions are not classified in the MCC-to-durable crosswalk.

with each contributing about 45 percent of spending. Transaction amounts are about one-quarter as large in the lumpy-cheap class as those for lumpy-expensive spending. Almost 80 percent of frequent-expensive spending is at cash-related MCCs. Because cash cannot be tied to spending at particular stores or on particular goods or services, we do not seek to classify whether this spending is durable or nondurable. Finally, we see that almost 70 percent of frequent-cheap spending is on strict nondurables, and another 18 percent is on nondurables.

Figure C1: Spending Amounts and Frequencies by Transaction Type



Notes: Statistics in this table are based on control and treatment group account activity starting from reference weeks –24 through –6 relative to the start of the UI spell. Each point in the figure is weighted by total spending. The dashed horizontal line represents the average transaction size across all MCCs. The vertical line represents the log of the average weekly transaction probability computed across all MCCs.

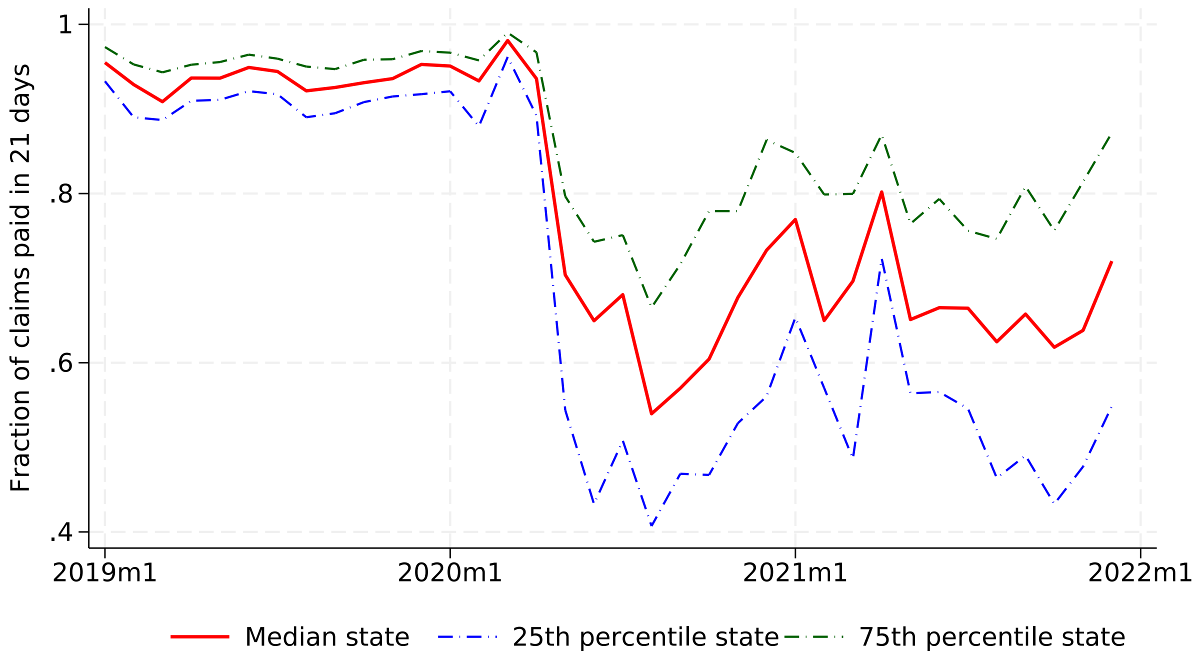
Table C1: Characteristics of Spending Types during Baseline Period

Spending type	Spending Characteristics		MCC-based classification shares						
	Average Transaction Size ($)	Weekly Probability	Durable	Strict Non-Durable	Non-Durable	Cash	Digital Goods	Debt Servicing	MCC Unavailable
Lumpy & Expensive	163.26	0.001	0.519	0.152	0.121	0.168	0.000	0.000	0.039
Lumpy & Cheap	40.35	0.002	0.434	0.100	0.464	0.000	0.002	0.000	0.000
Frequent & Expensive	122.73	0.067	0.069	0.104	0.000	0.777	0.000	0.049	0.000
Frequent & Cheap	27.11	0.097	0.018	0.692	0.177	0.000	0.012	0.000	0.101

Notes: Statistics in this table are based on control and treatment group account activity starting from reference weeks –24 through –6 relative to the start of the UI spell. Digital goods typically represent downloadable music, games, or apps.

Appendix D Supplementary Tables and Figures

Figure D1: Dispersion in UI Payment Timeliness across States



Notes: Fraction of claims paid within 21 days by state and month is from U.S. Department of Labor table ETA 9050.

Table D1: Unemployment Insurance Deposit Labels

State	Transaction Label
AK	AK DEPT OF LABOR UI PAYMENT
AL	DEPT OF LABOR UNEMP
AR	ADWS FS BENEFIT
AR	ADWS UI BENEFIT
AZ	STATE OF ARIZONA
AZ	STATE OF ARIZONA BENEFITPAY
CO	CDLE UI BENEFITS
CT	CTDOL UNEMP COMP
DC	D.C. EMPL. SRVCS UI COMP
DE	DELABOR UNEMPINS
GA	GA DEPT OF LABOR REG UI
HI	DEPT OF LABOR UI PAYMENT
IA	ST OF IA-UI PAY
ID	ID DEPT OF LABOR UI PAYMENT
IL	IDES PAYMENTS
IN	STATE OF INDIANA UI PAYMENT

Unemployment Insurance Deposit Labels (continued)

State	Transaction Label
KS	KS DEPT OF LABOR UNEMPL BEN
KY	UNEMPLOYMENT INS
LA	LOUISIANA WORKFO LDOL
LA	LOUISIANA WORKFO LDOLUIBEN
MA	MA DUA CARES ACT
MA	MA DUA MA UI TAX
ME	MAINE DEPT LABOR UNEMP COMP
MI	UIA PRE-PAID CAR
MI	UIA PRE-PAID CAR UI BE
MI	UIA PRE-PAID CAR UI BENEFIT
MN	MN DEPT OF DEED UI
MN	MN DEPT OF DEED UI BENEFIT
MN	MN UI FUND UI BENEFIT
MO	MODES UI BENEFIT
ND	JOB SERVICE ND UI BEN PMT
NE	NEB WORKFORCE UIPAYMENT
NH	NHUS NHUC BEN
NM	NEW MEXICO DWS UI BENEFIT
NY	NYS DOL UI DD
OH	ODJFS
OH	ODJFS FAC
OH	ODJFS OHIO
OR	EMPLOYMT BENEFIT UI BENEFIT
PA	COMM OF PA UCD
RI	RIDLT-UI
SC	SCESC-UIBENEFITS
SD	SD UIBP FUND SD UI PYMT
TX	TWC-BENEFITS
UT	UI BEN EFT
VA	VEC - VIRGINIA
WA	WA ST EMPLOY SEC UI BENEFIT
WI	WISCONSIN-DWD-UI
WV	WORKFORCE WV UI BENEFIT
WY	WYOMING DWS UI BENE