The Macroeconomic Effect of Stimulus Checks: Evidence from Postwar Veterans' Payments*

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Abstract

Do stimulus checks—one-off, deficit-financed lump-sum payments to households—boost the economy? We study a natural experiment: payments to U.S. veterans after World War II that mimic stimulus checks but were plausibly exogenous to the economy. These payments led to a temporary increase in transfers and a persistent, hump-shaped increase in consumption. The transfer multiplier—the cumulative response of consumption to transfers—reaches 1 after six months and exceeds 2 after a year. The standard heterogeneous-agent New Keynesian model cannot match the hump-shaped consumption response. A version with imperfect expectations—with households underreacting on impact but overreacting at longer horizons—can match our estimates.

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1. Introduction

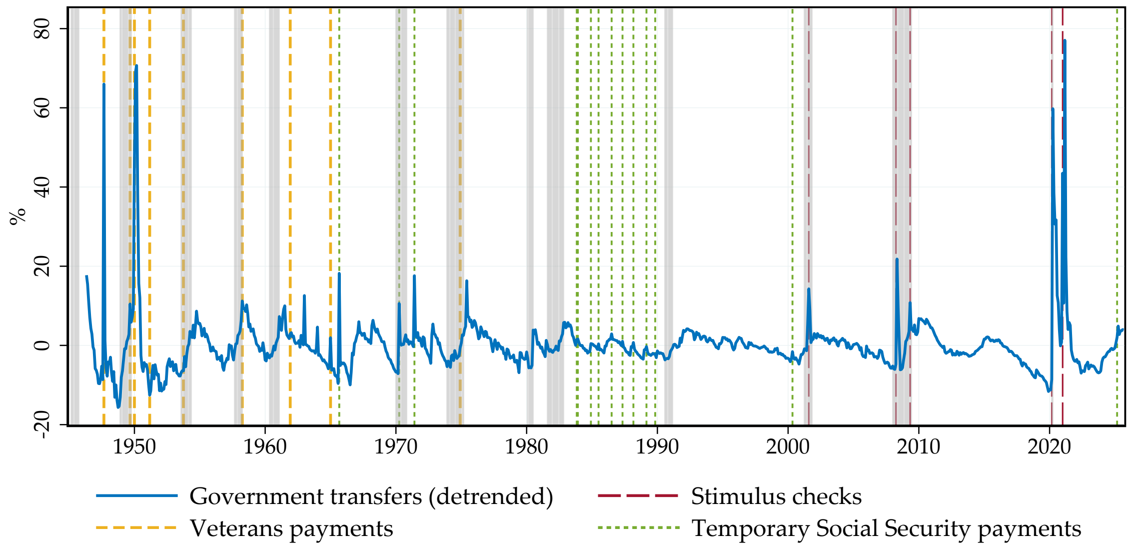
What is the effect of stimulus checks on the economy? In other words, does consumption go up when the government makes a one-off, deficit-financed, lump-sum payment to households? The answer matters because in recessions, the US government often pays out stimulus checks, and policymakers need to know whether they work. Stimulus checks also adjudicate between models. In models with Ricardian equivalence, households save the transfer to pay future taxes (Barro, 1974). In heterogeneous-agent models, many households spend the transfer instead (e.g. Kaplan and Violante, 2014).

There is surprisingly little direct evidence on the macroeconomic effect of stimulus checks. Many papers study how individual households respond to stimulus checks and related shocks, often finding sizable spending responses (e.g. Parker, Souleles, Johnson, and McClelland, 2013). However these studies hold aggregate conditions fixed, and cannot identify the macroeconomic impact of the policy. Aggregate time-series evidence is also hard to interpret. Stimulus checks are normally a response to an oncoming recession, and separating the stimulus from the effect of the recession is difficult. There is more evidence on the aggregate effects of other kinds of fiscal policy, such as government spending and marginal tax rate shocks (e.g. Romer and Romer, 2010; Auerbach and Gorodnichenko, 2012; Mertens and Ravn, 2013; Ramey and Zubairy, 2018). But stimulus checks may operate through different channels, so this evidence provides only limited guidance.

This paper studies a natural experiment: large transfer payments that resemble stimulus checks but are unrelated to the state of the economy. Beginning in the late 1940s, the Truman administration and its successors made a series of one-off, lump-sum payments to World War II veterans. These payments were made not to fight recessions, but instead due to unexpected surpluses in a government-managed life-insurance fund. During the war, service members purchased government-provided life insurance, and the premia were invested in special Treasury bonds. Mortality turned out to be unexpectedly low, and the fund accumulated a large surplus. The government returned this excess to veterans through lump-sum dividend payments.

The largest payment, in January 1950, amounted to 4.3% of quarterly GDP—roughly twice the size of the 2008 stimulus checks relative to output. About 35% of adult males were eligible, and the average payment was approximately $2,500 in 2025 dollars. The payment date had been fixed by years-long administrative timelines, making its timing unrelated to prevailing macroeconomic conditions. Additional payments followed over the subsequent decades. There was also an earlier lump-sum payment in September 1947 compensating veterans for unused leave. Together, these episodes generate plausibly exogenous and economically large shocks to aggregate transfer payments. To increase statistical power, we augment these episodes with a

Figure 1: Transfers and Veterans' Payments



Notes: Solid line: HP-filtered deviation of log real transfers (λ = 129,600). Markers indicate veterans' payments, temporary Social Security shocks, and stimulus checks; shaded bars are NBER recessions. Sample: 1945–2025.

second, less powerful series of transfer shocks: the temporary and plausibly exogenous Social Security payments identified by Romer and Romer (2016), extended to include two additional recent episodes.[1]

To study the macroeconomic effects of these transfer shocks, we assemble a new monthly dataset on government transfers and consumption spanning the postwar period. We construct a monthly series of aggregate transfers going back to 1945 by digitizing historical Treasury tables. We also compile monthly retail sales data—separately for durables and non-durables—which we use to impute monthly consumption. We combine these new series with standard aggregate macroeconomic data.

Figure 1 shows the monthly deviation of log real transfer payments from their trend (measured with a Hodrick-Prescott filter). We also indicate the months with veterans' payments, temporary Social Security payments and, for comparison, stimulus checks. NBER recession dates are shaded. The figure illustrates the appeal of the natural experiment: the postwar payments generate sharp, temporary spikes in transfers that resemble stimulus checks. Unlike stimulus checks, however, these payments were not enacted in response to recessions.

We then estimate the macroeconomic effects of transfers. Our shock series consists of 24 dates on which postwar veterans' payments and temporary Social Security transfers were paid out. The identification assumption is that the motivation and, in particular, the timing of these postwar payments were unrelated to macroeconomic developments. We support this argument

[1] As we shall see, the Social Security payments—which are different from the tax shocks of Romer and Romer (2010)—are not by themselves powerful enough to precisely detect the effect of temporary transfers.

with archival evidence: based on documents from the Veterans' Administration, the Congressional Record, presidential speeches, and contemporary press accounts, the payments of the late 1940s and 1950s were not timed to stabilize the business cycle but instead reflected congressional pressure to compensate veterans and administrative delays. Some payments in the 1960s and 1970s were explicitly motivated by macroeconomic stabilization; we exclude these episodes from our shock series. We use local projections to estimate the dynamic causal effects of transfer payments on the macroeconomy.

The postwar payments led to a significant but transitory increase in transfers, yet were followed by large, persistent and hump-shaped effects on economic activity. On average, transfers increase by about 15% on impact and return to baseline within three months. Consumption, however, rises gradually and remains elevated: it increases by roughly 0.75% after six months and peaks at 1% after twelve months. The price level rises more modestly, and the short-term real interest rate remains essentially unchanged. We summarize these magnitudes with a "postwar payments multiplier"—the cumulative response of consumption to the cumulative increase in transfers after the shock. The multiplier is approximately 1 after six months and exceeds 2 after one year.

We probe the identifying assumptions and robustness of the results in several ways. First, we find no evidence of pre-trends, and the timing of payments is not predictable using a broad set of macroeconomic variables. Second, the estimates are essentially unchanged after controlling for observable shocks—including monetary policy, government spending, permanent government transfers, and oil price shocks. Third, the results are robust to varying the sample period, incorporating information on shock size, and using alternative selections of transfer shock episodes. Indeed, a jackknife exercise that sequentially excludes one shock at a time leaves the estimates virtually unaffected. We also show that the veterans' payments are central to the empirical design: using only the temporary Social Security shocks from Romer and Romer (2016) yields noisy estimates.

We also present micro-evidence that the postwar payments increased consumption. We use microdata that is available around the largest postwar payment in January 1950. We first verify that the behavior of macroeconomic aggregates around this episode is typical. Transfers jump sharply in January and return to their previous level by June. At the same time, retail sales rise rapidly and remain elevated over the subsequent months. We then make use of a one-off survey question about the transfer that was added to a special module of the 1951 *Survey of Consumer Finances*. Veterans said that they spent 42–50% of the transfer in the subsequent year.

Our results chime with the important critique of heterogeneous-agent New Keynesian (HANK) models made by Orchard, Ramey, and Wieland (2025). Orchard et al. (2025) point out that if standard HANK models are calibrated to deliver a large transfer multiplier, then the result is a temporary spike in consumption. The authors argue this prediction is a flaw in the standard HANK model, because had stimulus checks not been paid in 2008, there would have been an implausibly sharp and "V-shaped" recession.[2] Consistent with this logic, we show that a well-calibrated model should produce a hump-shaped response of aggregate consumption after a transfer (as well as large cumulative multipliers). According to such a model, failing to pass stimulus checks leads to shallower but longer recessions than the standard HANK prediction, which is perhaps more plausible.

We close the paper by asking what model can match our finding of a large hump-shaped consumption response to a temporary shock. We start with a standard HANK model (Auclert, Rognlie, and Straub, 2024, 2025). We enrich the fiscal block with a veterans' life-insurance fund that distributes dividends to a subset of households ("veterans"), financed through an exchange of bonds for cash with the remainder of the government (the "Treasury"). We add another ingredient that will be essential for matching the hump shape. Households form forecasts of aggregate income using a diagnostic-expectations framework with noisy information (Coibion and Gorodnichenko, 2015; Bordalo, Gennaioli, Ma, and Shleifer, 2020). Bardóczy and Guerreiro (2023) show that this specification can capture a central feature of observed expectations: following a shock, expectations initially underreact, but subsequently overshoot realized outcomes (see also Angeletos, Huo, and Sastry, 2021; Auclert, Monnery, Rognlie, and Straub, 2026).

One implication of the model is that the multipliers for postwar payments and for stimulus checks are the same. The result follows from the government's consolidated balance sheet. The life-insurance fund pays veterans by exchanging its bond holdings for cash from the Treasury. The Treasury finances the cash outlay by issuing additional debt. From the perspective of the consolidated government, the operation is equivalent to a one-time, deficit-financed stimulus check. This equivalence requires that the marginal propensity to consume is the same for veterans as for others, and that monetary policy and the debt issuance rule are the same. The implication is that a suitably calibrated model—which matches the postwar payments evidence—can be used to study stimulus checks.

The main insight from the model is that in order to match the impulse response of consumption, aggregate expectations must underreact on impact and overreact in the longer run. If we impose full information rational expectations, then the model can match the initial rise in consumption but predicts that consumption peaks on impact and subsequently declines. In the data, by contrast, consumption continues to increase and reaches its maximum roughly 12 months after the shock. With rational expectations, the consumption response is dominated by a partial-equilibrium force. Since households have the highest marginal propensity to spend immediately after receiving the transfer, consumption responds most strongly on impact.

[2] Ramey (2025) and Orchard et al. (2026) apply a similar critique to other stimulus check episodes.

Imperfect expectations create a general-equilibrium force that leads consumption to gradually rise, leading to hump shapes. Initially, households underestimate the income gains generated by the increase in aggregate spending, muting the general-equilibrium amplification of the shock. Over time, however, households become overly optimistic about future income gains, creating additional amplification and causing consumption to continue rising after the initial transfer. The mechanism is empirically grounded: the response of expectations in the model is quantitatively consistent with estimates of how the transfer shock affects output expectations.

Related literature and contribution. This paper joins the half-century-old quest to understand whether consumption responds to temporary income shocks. The early papers shared our time series approach. Hall (1978), building on Friedman (1957), found support for the permanent income hypothesis—consumption being almost unchanged after temporary income shocks. Flavin (1981), Hayashi (1982) and Campbell and Mankiw (1989) found instead a partial response of consumption to temporary income shocks. One challenge was identification: these papers instrumented for temporary shocks to income with lagged macroeconomic variables; these instruments are not exogenous without strong assumptions. We instead use plausibly exogenous variation in transfers as an instrument.

Modern work moved from the time series to the cross section, and from generic income shocks to one-time payments such as stimulus checks. The typical cross-sectional design estimates how consumption responds for households who receive the payment compared to others who do not; time fixed effects absorb all factors common to households (e.g. Parker et al., 2013; Hausman, 2016; Boehm, Fize, and Jaravel, 2025). By construction, this approach does not identify the aggregate response to the payment—the so-called "missing intercept" problem. This paper takes a different approach: studying macro-level shocks that mimic stimulus checks.[3]

Ramey (2019) observes that there "has been very little work on the aggregate effects of transfers". Besides the papers that we have already discussed—specifically, Orchard, Ramey, and Wieland (2025), Ramey (2025) and Orchard et al. (2026)—we now turn to the few other papers on this topic. A landmark study is Romer and Romer (2016), who identify both temporary and permanent shocks to income, from reforms to Social Security that were not taken in response to the business cycle. Their series of temporary shocks by itself yields somewhat noisy and imprecise estimates (though permanent shocks are more powerful).

A second important paper is Hausman (2016), who also studies payments to veterans—the Veterans' Bonus of 1936. The primary focus is an estimate of the marginal propensity to consume, using novel historical microdata. The paper examines the aggregate effects of the stimu-

[3]A related approach uses cross-sectional variation at the regional level to estimate the response of the economy to transfers, which again faces the missing intercept problem (Corbi, Papaioannou, and Surico, 2019; Pennings, 2021; Brandao-Roll, De Ridder, Hannon, and Pfajfar, 2023).

lus only tentatively, because there is a single aggregate observation.

Third, Wolf (2023) provides conditions under which private spending and government spending propagate similarly in general equilibrium. Under these conditions, termed "demand equivalence", the aggregate effect of transfers can be recovered by combining aggregate government spending multipliers with cross-sectional evidence on how transfers affect household spending—providing a test of whether Ricardian Equivalence fails. We instead directly estimate the effects of stimulus checks, and departures from Ricardian Equivalence, using plausibly exogenous transfer episodes in the time series.[4]

Methodologically, our paper relates to the fiscal multiplier literature, which studies narrative measures of government spending shocks, such as news about defense spending (Ramey, 2011; Barro and Redlick, 2011; Ramey and Zubairy, 2018). Instead, we apply the narrative approach to lump-sum transfers. As such, our paper relates more broadly to the literature on narrative identification in macroeconomics (e.g., Romer and Romer, 1989, 2010; Cloyne, 2013; Cloyne, Dimsdale, and Postel-Vinay, 2024; Fieldhouse and Mertens, 2024; Den Besten, Barnichon, Känzig, and Singh, 2026).

Our findings offer an empirical benchmark for heterogeneous-agent models—as in these models, temporary transfers can generate sizable aggregate effects (Kaplan, Moll, and Violante, 2018; Auclert, Rognlie, and Straub, 2024). We argue that to match the transfer multiplier, HANK models must incorporate expectations that underreact on impact to shocks, and overreact in the medium term, as in Angeletos, Huo, and Sastry (2021) and Auclert et al. (2026). In doing so, we also contribute to a growing literature studying behavioral HANK models.[5] Our focus is closest to Eichenbaum, Guerreiro, and Obradovic (2025), who also study how deviations from rational expectations affect the transfer multiplier. The Ricardian non-equivalence effect they emphasize is also present in our model; we emphasize an additional source of persistence from the delayed overreaction of expectations. The model of expectations we use was developed in Bardóczy and Guerreiro (2023), who show that the same pattern of delayed overreaction in expectations implies that the stimulative effects of unemployment benefits can be substantially delayed relative to the date the policy is enacted.

Outline. The paper proceeds as follows. Section 2 introduces the institutional setting, the new monthly dataset, and the narrative discussion of the postwar payments. Section 3 outlines the empirical approach and reports the main estimates. Section 4 develops a HANK model to interpret the estimates. Section 5 concludes.

[4] In related work, Adams and Matthes (2026) use a semi-structural approach, organized around the consumption function, to estimate whether aggregate behavior is Ricardian.

[5] See for instance Farhi and Werning (2019), Auclert, Rognlie, and Straub (2020), Farhi, Petri, and Werning (2020), Pfäuti and Seyrich (2022), Guerreiro (2023) and Angeletos, Guerreiro, and Zhang (2025).

2. Setting and Data

This section introduces new monthly data on U.S. government transfers dating back to 1945 and provides historical background on the large payments made to veterans in the late 1940s and subsequent decades. We then identify which of these postwar payments can plausibly be viewed as exogenous to business cycle conditions. Finally, we describe the temporary Social Security payments that complement the veterans' payments and increase statistical power.

2.1. New Data on Transfers

The data requirements for this project are demanding. To reliably estimate the effects of transfers on the postwar economy, we need monthly data extending back to 1945. The payments we study are discrete and short-lived, so lower-frequency data would obscure their timing and make it difficult to trace the dynamic response of the economy. In some specifications, we study payments to veterans before World War II, requiring data back to the 1930s. However, publicly available monthly transfer data begin only in 1959. Several other important series, such as durable and non-durable retail spending, are also not readily available over our entire sample at the monthly frequency.

The first step in our data construction is to build a monthly transfer series. We outline the steps here; for the full details, see Appendix A.1. While the necessary transfer data are not available in digital form, historical publications from the United States Treasury report most components of transfers at monthly frequency. Moreover, the National Income and Product Accounts provide a quarterly transfer series ("personal current transfer receipts: government social benefits to persons") back to 1947, and an annual transfer series back to 1935.

We begin by digitizing historical monthly data from the *Annual Report of the Secretary of the Treasury: Statement of the Finances*. These tables provide monthly information on the main components of aggregate transfers, including Social Security, Unemployment Insurance, Railroad Retirement, and Government Life Insurance.[6] We additionally digitize monthly payments reported in the *Monthly Treasury Bulletin* for the Armed Forces Leave Bond program.

From these series, we construct both a narrow and a broad measure of transfers. The narrow measure includes Social Security, Railroad Retirement, and government life insurance-related disbursements, which correspond closely to the transfer payments we exploit. The broad measure additionally includes unemployment insurance.

To ensure consistency, we take two steps. First, we rescale the monthly series so that, within each quarter, the sum of the monthly observations equals the quarterly aggregate series. Sec-

[6] Health-related transfers, such as Medicare/Medicaid, are small or non-existent in the early part of our sample.

ond, for components of aggregate transfers that are not reported in the historical monthly tables, we assume that payments were spread evenly across the three months of the quarter.

Next, we assemble monthly data on retail spending. We start from the total retail spending series used by Romer and Romer (2016), from 1947–1991, sourced from the *Survey of Current Business Statistics*. We extend this series backward to 1935 using earlier vintages of the same publication, and forward to 2025 using data from the Census Bureau's *Advance Monthly Retail Trade Survey*. These data also allow us to classify retail spending into durable and non-durable goods (see Appendix A.2 for more information).

Monthly personal consumption expenditures are only observed from January 1959. To obtain a longer monthly series, we extend consumption backward using a Chow–Lin procedure that combines quarterly consumption with monthly retail spending; Appendix A.3 describes the procedure.

We combine our constructed monthly series with standard monthly macroeconomic variables: industrial production, non-farm employment, and consumer prices. Our final dataset spans the period 1935–2025. All series are seasonally adjusted, either as reported by the source or, when necessary, using the X-11 algorithm. We complement the aggregate dataset with microdata from the 1951 *Survey of Consumer Finances*; see Appendix A.4 for details.

2.2. Background on the Postwar Veterans' Payments

During the Second World War (WWII), U.S. service members were offered government-provided life insurance under the National Service Life Insurance (NSLI) program. Enrollment was nearly universal: approximately 97% of service members participated (The Camp Lejeune Globe, 1948). Under the program, service members paid regular premia in exchange for a payout to beneficiaries in the event of death.

The origins of this policy date back to the First World War, when private insurers had refused to cover soldiers against death in combat. In response, the federal government introduced United States Government Life Insurance to fill the gap. Similar arrangements were later implemented during the Korean War. The program encouraged many veterans to retain their policies after the war.

The financing of the life insurance scheme was mostly standard. Policy holders would pay a regular monthly premium, averaging around 65 cents per 1,000 face dollars of coverage, into the life insurance trust fund (U.S. House of Representatives, 1951). On death, the beneficiary would receive the face value of the coverage. The face value was on average 9,100 dollars, or around 165,000 dollars as of 2025 (U.S. Senate Committee on Finance, 1958). The fund was then invested in "special issue" United States government bonds. These Treasuries were interest-bearing, but unlike typical debt, could be redeemed at any time. Whenever the fund needed cash to pay a beneficiary, it could redeem these bonds with the Treasury, in return receiving the face value in cash. The rest of the government would have to raise more debt, increase taxes, or cut spending, to fund the payout.

By 1949, the trust fund had accumulated substantial surpluses. When it was established under the National Service Life Insurance Act of 1940, actuaries had projected mortality rates and expected rates of return in order to set premia at actuarially fair levels. These projections—particularly concerning mortality—proved pessimistic (Board of Governors of the Federal Reserve System, 1949; U.S. Department of Veterans Affairs, 2021). Therefore, the fund had a surplus, which according to legislation, had to be paid back as dividends. The Truman administration paid the first dividend out of the life insurance fund in January 1950 (U.S. Department of Veterans Affairs, 2021).

The scale of the payments was enormous. The first payment was 4.3% of quarterly GDP (U.S. Department of Veterans Affairs, 2021). For comparison, the 2008 stimulus checks were 2.2% of quarterly GDP (Parker et al., 2013). The payment was large partly because 35% of adult males had served in WWII (Vespa, 2020). However, individual payments were also large, with the typical veteran receiving 2,500 dollars as of 2025 (U.S. General Accounting Office, 1951).

There were several more large, one-off "special dividends". In March 1951 there was a second special dividend out of the WWII fund, followed by further payments in July 1961 and January 1963. There were also similar, but smaller, special dividends paid out of the WWI life insurance fund, in September 1949, October 1953, and April 1958. Finally, there was a special dividend paid out of the life insurance fund associated with the Korean War in December 1961.

The special dividends became less common and smaller because of a mechanism that the administration introduced in 1952. Veterans would receive "regular dividends" on the anniversary of their enrollment date. These regular dividends were disbursed continuously throughout the year. Nevertheless, special dividends were still occasionally paid, when actuaries periodically revalued the fund's liabilities as mortality projections changed. Moreover, from the 1960s onwards, governments would occasionally "accelerate" the regular dividend payments, by making all of them at the start of the year. From the Vietnam War onwards, the life insurance fund used any excess to lower premia instead of paying dividends (U.S. Department of Veterans Affairs, 2025).

There were two other large payments made to veterans after World War II. The first was the September 1947 Armed Forces Leave Bond (U.S. Department of the Treasury, 1949). After the war, the government owed veterans money for vacation that had not been taken. In 1946, the government paid for this leave not with cash, but with bonds. Initially, these bonds were not allowed to be redeemed for five years. However, in September 1947, the Truman administration

Table 1: Postwar Veterans' Payments

Program	**Date**	**Size** nominal	**Size** % qtr. GDP	**Sources**
Armed Forces Terminal Leave	Sep. 1947	$2.1 billion	3.36%	Date and size: U.S. Department of the Treasury (1949)
World War I insurance dividend	Sep. 1949	$40 million	0.06%	Date: Sullivan Daily Times (1949); Size: U.S. Department of Veterans Affairs (2021)
World War II insurance dividend	Jan. 1950	$2.9 billion	4.3%	Date and size: U.S. Department of Veterans Affairs (2021)
World War II insurance dividend	Mar. 1951	$685 million	0.81%	Date and size: U.S. Department of Veterans Affairs (2021)
World War I insurance dividend	Oct. 1953	$64 million	0.07%	Date and size: The Fayette County Record (1953)
World War I insurance dividend	Apr. 1958	$32 million	0.03%	Date: The Brantley Enterprise (1958); Size: U.S. Department of Veterans Affairs (2021)
Korean War insurance dividend	Dec. 1961	$60 million	0.04%	Date: Atlanta Daily World (1962); Size: U.S. Senate Committee on Finance (1961)
World War II insurance dividend acceleration	Jan. 1965	$200 million	0.11%	Date and size: U.S. Department of Veterans Affairs (2021)
Veterans Compensation Act	Dec. 1974	$217 million	0.05%	Date and size: United States Congress (1974)

Notes: This table provides an overview of the identified, plausibly exogenous postwar veterans' payments. Reported is the first month of the program, the size (in nominal terms), the size (relative to quarterly GDP), and the sources used to identify the date and the size of the program.

allowed veterans to convert the bonds to cash. The scale of these payments was also enormous, at 3.4% of quarterly GDP (U.S. Department of the Treasury, 1949). A second, smaller, one-off payment was made to veterans in December 1974. That month, Vietnam War veterans received a permanent increase in training and education benefits, in order to help them reintegrate into society after the end of the war. However, the law raised benefits effective in September 1974. As a result, veterans received a temporary, retroactive payment for benefits in the previous three months, worth around $200 million (0.05% of quarterly GDP) in aggregate (United States Congress, 1974).

Table 1 contains the complete set of payments. The first column describes the payment and the second column provides the first month of payments. The third and fourth columns present the size of the shock, either in nominal terms or scaled to quarterly GDP. The fifth column provides the sources for the payment date and size. These include official documents, such as the Veterans Benefits Insurance Manual, or newspaper articles. These dates form the chronology of our shock series. We will take these shocks to be exogenous to the business cycle; the next subsection explains the rationale for this classification.

2.3. Which Veterans' Payments Are Exogenous?

We now ask which veterans' payments are likely exogenous to current and future macroeconomic conditions. We adopt a narrative approach in the spirit of Romer and Romer (2023). We scrutinize archival documents from the Veterans' Administration, principally the Department of Veterans Affairs' M-29 Insurance User Manual, along with the Congressional Record, presidential speeches, and contemporary press accounts. The goal is to determine whether each payment reflected macroeconomic stabilization motives or instead arose from administrative, institutional, or legal features of the veterans' insurance programs.

As we show below, the historical record provides strong evidence that many veterans' payments were plausibly exogenous. The payments were motivated primarily by the need to disburse accumulated surpluses in the veterans' insurance funds, not by a desire to stabilize economic activity or respond to macroeconomic shocks. The case is even stronger for the timing of the payments, which were often shaped by administrative delays and institutional constraints rather than by the business cycle.

January 1950 special dividend. We start by discussing the largest shock, the dividend to Second World War veterans paid in January 1950. As discussed, the reason for this payment was to disburse money from the life insurance fund. The precise timing was determined by administrative lags. Given payments to much of the nation, and the lack of modern bookkeeping or payments systems, the scale of the task was enormous. The first mention of paying the dividend was in President Truman's Annual Budget Message, at the start of 1948 (Truman, 1948). However, even by the middle of 1949, the date at which dividends could be paid was still uncertain. On June 9th 1949, the St. Louis Star and Times reported Assistant Administrator for Insurance Harold Breining saying that the payments could take as long as July 1950 to arrive (St. Louis Star and Times, 1949). In an October 9th interview Breining stated that the Veterans' Administration was "*even working overtime in an effort to get these dividend checks to the veterans entitled to them according to our present schedule.*" (Atlanta Daily World, 1949).

March 1951 special dividend. The next dividend from the World War II life insurance fund was again motivated by the fund's surplus. The first dividend paid out the excess in the fund as of January 1948, when Truman had made the original Budget speech to Congress. By the time that dividend had been processed and paid, a further excess had built up and had to be disbursed.[7] The timing of the payment was again determined by administrative lags. The process to finalize the paperwork began in December 1950, before dividends were mailed the following March.

Armed Forces Leave Bond. This payment compensated enlisted veterans for unused leave accrued during the Second World War. The underlying obligation reflected fairness concerns rather than a discretionary attempt to stimulate the economy: officers had received terminal leave pay in cash at the end of the war, while enlisted men had been paid in bonds that were initially not redeemable until 1951. Congress responded to this unequal treatment by moving redemption forward to September 1947, amid political pressure to pay veterans what they were owed. The payment was made over the objections of a Truman administration concerned about macroeconomic stimulus. In the House debate on the bill changing the timing, Republican Congressman Blackney from Michigan stated that "*we have been informed by the Treasury Department that to pass such a bill would increase the hazards of inflation ... [but it] was manifestly unfair to the enlisted man to require him to wait 5 years to receive his pay for terminal leave, while officers were entitled to receive their terminal leave in cash*" (U.S. House of Representatives, 1947). When President Truman signed the bill in July 1947, he made a final effort to discourage spending, saying: "*I wish to emphasize strongly that it is to the veterans' best interest that they keep their bonds if they do not absolutely need to cash them now*" (Truman, 1947).

September 1949 special dividend. This payment, made to World War I veterans, was motivated by accumulated surpluses in the insurance fund. In his Annual Budget Message of 1948, President Truman referred to the "*substantial accumulations*" in the fund as the reason for making the payment (Truman, 1948).[8] The timing of the payment appears to have been shaped by administrative considerations: once the decision to disburse the surplus had been made, the Veterans' Administration still had to process the payments before checks could be sent out.

December 1961 special dividend. This payment, made to veterans of the Korean War, reflected again the gradual buildup of excess balances in the insurance fund. Its precise timing was likely driven by political delays. Unlike the World War II insurance fund, there was no straightforward mechanism to pay out dividends from the Korean War insurance fund, so legislation had to be passed to authorize the payment (U.S. Senate Committee on Finance, 1961).

[7] The Oak Leaf (the in-house newspaper of a U.S. Navy hospital) wrote in February 1951 that "*[t]he first special dividend of nearly three billion dollars, payment of which is now virtually completed, covered the period each policy was in force up to its anniversary date in 1948. The second dividend will be for the number of months in force from that date to the corresponding date in 1951*" (The Oak Leaf, 1951).

[8] Likewise, contemporary accounts discussing the reason for the disbursement emphasized that the government was "*shar[ing] insurance money with veterans ... because premiums paid were higher than necessary to offset actual deaths*" (Kent Stater, 1949).

This process took almost a year. The bill, HR 4539, "special dividends for certain NSLI policy holders," was first introduced in the House of Representatives on February 17, 1961. It passed the House on March 21, 1961, passed the Senate on September 1, 1961, and was signed into law as Public Law 87-223 on September 13, 1961 (U.S. Congress, 1961). With the usual administrative delays, the payments were made the following December.

December 1974 veterans' compensation. The main motivation for these payments was the ongoing difficulty that Vietnam War veterans faced reintegrating into society.[9] Their timing was again shaped by legislative delays. For instance, a Senate statement on October 11, 1974 complained that the payments had not coincided with the start of the academic year (U.S. Congress, 1974). In particular, following President Ford's veto, the payments were only sent out in December (United States Congress, 1974).

October 1953 and April 1958 special dividends. To our knowledge, there is no contemporary discussion of these dividends—perhaps because they were small compared to the others. In particular, there is no discussion that these dividends were a form of fiscal stimulus. In the baseline, we include these events in our shock series. As we will discuss, the results are robust to excluding these events.[10]

January 1965 regular dividend acceleration. In January 1965, the administration paid in a single month all of the regular dividend payments from the life insurance fund, due in that year. Otherwise, the payments would have been paid smoothly throughout the year. There is again no discussion, to our knowledge, of the motivation for this payment. For instance, when discussing the payment, the 1965 *Survey of Current Business* does not disclose a motive (Bureau of Economic Analysis, 1965). However, circumstantial evidence does not suggest a stimulus motive. The acceleration is not mentioned in speeches by Congress or the president. The acceleration did not coincide with a recession and there is no other discretionary fiscal stimulus that coincides with this payment. Therefore, we cautiously include this payment in our exogenous shock series. Again, however, results are robust to dropping this shock.

June 1936 veterans' bonus. In June 1936, Congress paid a large bonus to World War I veterans, in the middle of the Great Depression. This episode is the focus of Hausman (2016), who provides a detailed account of its motives. The bonus had been granted in 1924 as compensation for wartime service and was originally scheduled to be paid in 1945. During the Great Depres-

[9]For instance, in October, Democratic Senator Alan Cranston said that it was "*urgent today that the Congress act to approve a bill to improve and liberalize veterans education and readjustment assistance for the millions of veterans who served during the Vietnam war*" (U.S. Congress, 1974).

[10]Both shocks occurred during recessions. However, there were three recessions over 1949–59, and so we would expect some veterans' payments over this time to coincide with recessions simply by chance.

sion, however, veterans pressed for early payment. After several years of lobbying and political negotiations, Congress authorized the payout over a presidential veto. The timing therefore appears to have reflected the political resolution of a long-running dispute, rather than contemporaneous business-cycle conditions. We do not include this payment in our baseline event set, in order to focus on the less volatile postwar macroeconomic sample, but we examine it in supplementary exercises.

We now discuss an additional set of veterans' payments that we deem endogenous. These veterans' payments are summarized in Appendix Table A.7.

July 1961 and January 1963 special dividends. These two special dividends appear to have been endogenous, taken in response to the business cycle. In reference to the July 1961 dividend, Kennedy stated that the dividend had been "*speeded up in order to assist the economy*" (Kennedy, 1961). Likewise, the January 1963 dividend was intended to "*provide a needed boost to the economy*" (Kennedy, 1962).

Other dividend accelerations. With the exception of the January 1965 dividend acceleration, discussed above, the other dividend accelerations from the 1960s onwards appear to have been taken in response to recessions. For instance, the dividend acceleration of March 1961 was "*stepped up as an antirecessionary measure*" (U.S. Department of Commerce, Office of Business Economics, 1961). The dividend acceleration of January 1963 was bundled with the special dividend of January 1963, discussed above. The dividend acceleration of January 1964 was described as "*additional stimulus*" by the *Report of the Joint Economic Committee of the United States* (Joint Economic Committee, 1964). Each of the subsequent dividend accelerations (in February 1967, February 1972, February 1975, February 1976, April 1977, and February 1992) either coincided with a recession or with other fiscal stimulus.

2.4. Social Security Payments

Our narrative analysis identifies 10 plausibly exogenous veterans' payments. To increase statistical power, we augment these episodes with a series of temporary Social Security transfer shocks identified in Romer and Romer (2016). That paper provides a narrative account of changes in Social Security benefits over the period 1952–1991. They use detailed historical evidence to isolate plausibly exogenous variation in Social Security transfers. These payments tend to be smaller than the veterans' dividends, as shown in Figure 1.

Romer and Romer (2016) focus on legislated changes in benefits to existing recipients, excluding changes driven by demographic shifts or expansions in program coverage. They draw on Congressional reports, presidential statements, and administrative records to identify the timing and size of each policy change and to classify its motivation.

Romer and Romer (2016) further distinguish between permanent and temporary changes in transfers. The temporary changes—which are the focus of our analysis—primarily consist of one-off payments arising from retroactive benefit adjustments, as well as short-lived amendments to benefit formulas. We incorporate the subset of Romer and Romer (2016) events corresponding to temporary payments that were not tied to contemporaneous economic conditions. Like the veterans' payments we identify, these payments closely resemble stimulus checks: they are one-off, lump-sum payments that are largely deficit-financed. As we discuss in Appendix A.5.1, we extend Romer and Romer (2016) by adding two more recent episodes of temporary Social Security payments. Both episodes reflect legislative or administrative changes that generated discrete, transitory payments to existing beneficiaries and thus fit naturally within our definition of exogenous transfer shocks.

Overall, this yields a set of 14 additional transfer shocks that complement the veterans' payments by providing more frequent, though smaller-scale, variation. Appendix Table A.6 provides an overview of all 24 identified events, comprising both veterans' and temporary Social Security payments. As the table makes clear, two events—the Veterans' Bonus of 1936 and the Social Security payments in 2025—fall outside our baseline sample; we consider them in alternative specifications.

3. Estimates: the Macroeconomic Effect of Postwar Payments

In this section, we estimate the macroeconomic effects of temporary transfer payments. We first present our identification strategy and main specification, before turning to the baseline results.

3.1. Identification Strategy and Main Specification

We now estimate the dynamic effects of the postwar payments. Figure 1 summarizes the key variation: postwar veterans' payments and temporary Social Security payments appear as large, discrete spikes in transfers that reverse quickly, closely resembling stimulus checks. Unlike modern stimulus checks, however, they were not designed as countercyclical policy and often occurred outside NBER recessions, making them well suited for identifying the causal effects of temporary transfers.

Empirical framework. We assume that the economy admits a structural vector moving-average representation: $\mathbf{y}_t = \sum_{l=0}^{\infty} \boldsymbol{\theta}_l \boldsymbol{\varepsilon}_{t-l}$, where $\mathbf{y}_t$ is a vector of macroeconomic variables, $\boldsymbol{\varepsilon}_t$

is a vector of mutually uncorrelated structural shocks driving the economy, and $\{\boldsymbol{\theta}_l\}_l$ capture the dynamic causal effects. Let $\varepsilon_{T,t}$ denote the structural innovation to transfers and $\boldsymbol{\varepsilon}_{-T,t}$ collect all other structural shocks. Let $z_{T,t}$ denote a narrative proxy for the transfer shocks. For $z_{T,t}$ to recover the dynamic causal effects of interest, it must satisfy (i) $\mathbb{E}[z_{T,t}\varepsilon_{T,t}] = \zeta \neq 0$; (ii) $\mathbb{E}[z_{T,t}\boldsymbol{\varepsilon}_{-T,t}] = \mathbf{0}$; and (iii) $\mathbb{E}[z_{T,t}\boldsymbol{\varepsilon}_{t+j}] = \mathbf{0},$ for $j \neq 0$. The first condition requires relevance: the narrative shock series must be correlated with the true structural transfer shock. The second condition imposes contemporaneous exogeneity: the narrative shock must be orthogonal to all other structural disturbances. The third condition is a dynamic restriction, ruling out correlation with past or future structural shocks and thus excluding anticipation or systematic policy responses to evolving macroeconomic conditions.

Provided that $z_{T,t}$ satisfies these three conditions, we can estimate the dynamic causal effects of transfers using local projections (Jordà, 2005). Specifically, for each horizon $h = 0, \ldots H$, we estimate

$$y_{t+h} = \alpha_h + \theta_h z_{T,t} + \boldsymbol{\beta}_h' \mathbf{x}_{t-1} + \nu_{t+h}, \tag{1}$$

where y_{t+h} is the outcome variable h periods ahead, $z_{T,t}$ denotes the narrative transfer shock, $\mathbf{x}_{t-1}$ is a vector of controls, and ν_t is a potentially serially correlated error term. The coefficients θ_h trace out the dynamic response of the outcome variable y to an exogenous, transitory transfer shock. For inference, we use heteroskedasticity-robust standard errors, following Montiel Olea and Plagborg-Møller (2020).

Identification. Our baseline identification strategy focuses on the timing of transfer episodes rather than their size. Thus, $z_{T,t}$ is an indicator equal to one in months with plausibly exogenous temporary transfer payments and zero otherwise. Because the timing is correlated with the transfer shock of interest, this is a relevant instrument (see Boer and Lütkepohl, 2021 and Plagborg-Møller, 2022).

As explained in Section 2.3, the narrative evidence supports a stronger case for exogeneity of payment timing than for payment magnitudes. Even when the decision to make a payment was not motivated by macroeconomic stabilization, the amount paid may partly reflect past economic conditions. For example, the size of veterans' dividends depends on the accumulated performance of the underlying insurance fund, which in turn may reflect past economic conditions. Focusing on timing also improves robustness: using information on payment size makes the regression more sensitive to a small number of large events. Nevertheless, we also present results that incorporate information on payment magnitudes.

Our approach pools veterans' payments and Social Security payments. Appendix B.1 shows that, when the effects of the two payment types may differ, the coefficient in equation (1) recovers a weighted average of their effects. The weights depend on the number of payments of each type and on the average increase in transfers associated with them. In our baseline specification, veterans' payments receive a weight of 55%, despite being less frequent than Social Security payments, because transfers increase much more after veterans' payment dates. If we instead use information on payment size for identification, the weight on veterans' payments rises further and loads almost entirely on the two largest payments, in 1947 and 1950.

Shock diagnostics. The transfer shock dates satisfy a series of standard diagnostic checks that support the identifying assumptions. The corresponding figures and tables are reported in Appendix B.2. Most importantly, the dates are not predictable from lagged macroeconomic conditions. This is the key requirement for our design: temporary transfer payments should not systematically occur in response to the business cycle. In the data, standard macroeconomic indicators contain little information about the timing of the shocks. The shock series also does not appear to capture other well-known macroeconomic disturbances—including military spending shocks, tax shocks, monetary policy shocks, and oil supply shocks—consistent with it providing an independent source of variation in disposable income. The dates exhibit some serial correlation, reflecting the small number of events and their concentration in particular historical periods. In our empirical specifications, we therefore include lags of $z_{T,t}$ to control for any serial dependence.

Controls and outcomes. Our specification includes a rich set of controls designed to absorb predictable macroeconomic dynamics and policy responses. Specifically, we include 12 lags of the outcome variable, log industrial production, log producer prices, the 3-month Treasury bill rate to proxy for the monetary policy stance, the log level of real government transfers, measured using the broad transfer series, and the transfer shock dates. Conditional on this information set, the transfer shock dates are more likely to satisfy the exogeneity requirement. At the same time, if the shock dates capture plausibly exogenous variation in transfers, the estimates should not be sensitive to the precise set of controls included. Indeed, we find that the results are very robust across alternative control sets.

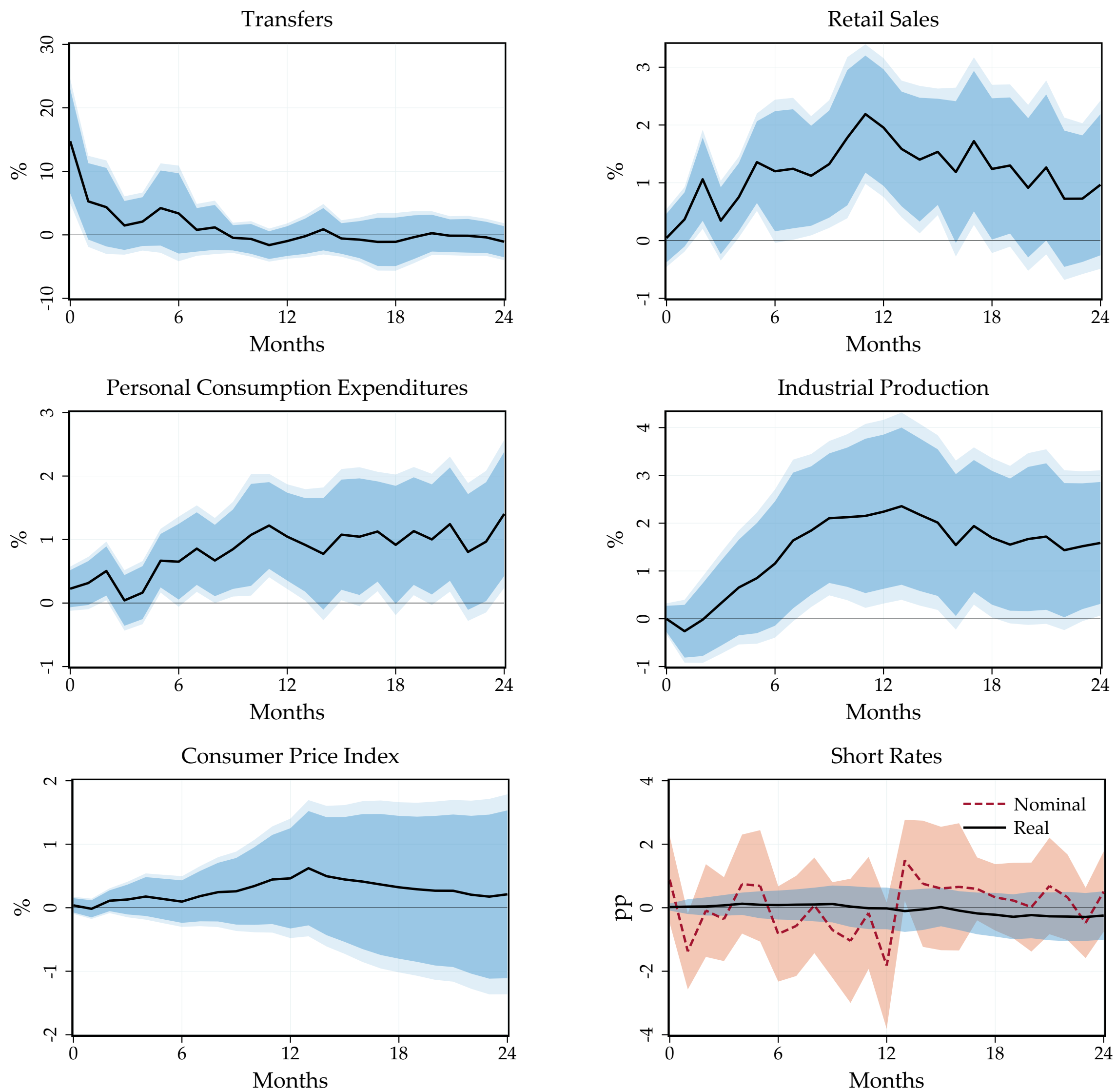
We focus on standard macroeconomic outcomes to trace out the dynamic effects of exogenous transfer innovations on the broader economy. For transfers, we focus on the narrow series, which aligns most closely with the shocks we study. Nominal variables are deflated using the Consumer Price Index (CPI). Our main sample spans October 1945 to December 2019.[11]

[11] This sample period drops two of our shocks: the World War I veterans' bonus of 1936, and the Social Security Fairness Act payments of 2025 (see Appendix Table A.6). We extend the sample to include these shocks in alternative specifications.

3.2. Main Estimates: the Macroeconomic Effect of Postwar Payments

We now turn to our main estimates. Figure 2 reports the impulse responses to a temporary increase in transfers. The top-left panel shows that transfers rise sharply on impact, by around 15%, and then unwind over subsequent months, confirming the transitory nature of the shock.[12]

Figure 2: The Macroeconomic Effects of Transfer Payments



Notes: Impulse responses to a temporary transfer shock estimated based on the local projections specification (1) using the exogenous transfer shock dates. Panels show narrow transfers, retail sales, PCE, industrial production, CPI, and nominal/ex-post real 3-month Treasury bill rates. Lines are point estimates; shaded areas are 90 and 95% confidence bands, except the rate panel, which shows 95% bands. Sample: 1945m10–2019m12.

[12]Appendix Figure B.3 shows that this increase corresponds to roughly a 10% rise in the broader transfer series.

Real activity responds persistently and with a hump shape. Retail sales increase by around 2% (Figure 2, top-right panel), real personal consumption expenditures rise by more than 1% (middle-left panel), and industrial production expands by roughly 2% (middle-right panel). Each series peaks at around 12 months. These effects are not only economically meaningful but also statistically significant at the 95% level. Appendix Figure B.4 further documents meaningful labor market impacts with a significant increase in non-farm employment, and a fall in the unemployment rate.

The response of prices is relatively small. The bottom-left panel of Figure 2 shows that consumer prices rise by around 0.5%, but the effect is not statistically significant.[13] We also examine the response of monetary policy to transfer shocks. The bottom-right panel shows the responses of the nominal short-term interest rate and the ex-post real rate. We find little evidence of a systematic monetary policy reaction. The responses for both the nominal and real rates are small and statistically indistinguishable from zero at all horizons. Thus, there is little evidence of a monetary tightening that would offset the expansionary effects of the transfer shock.

Overall, these results point to large, persistent and hump-shaped real effects of temporary transfer shocks, with responses peaking at 12 months, while prices and short-term rates respond little. The existing literature often estimates hump-shaped responses to persistent macroeconomic shocks such as monetary policy (e.g. Christiano, Eichenbaum, and Evans, 2005). However, we estimate a hump-shaped response to a *transitory* shock.

Which components of spending drive the aggregate response? Figure 3, left panels, decomposes the consumption response into durable and non-durable expenditures. Durable spending responds strongly and immediately following a transfer shock, rising by around 4% at its peak. Non-durable spending also increases substantially, though more gradually, reaching a peak of roughly 1%. This finding is consistent with micro evidence, which also finds that durables respond more to transfer shocks (e.g. Parker et al., 2013; Boehm, Fize, and Jaravel, 2025). At the same time, the relative price of durables is relatively constant, as the bottom-left panel shows. This finding contrasts with recent evidence suggesting that stimulus checks increase the relative price of durables (Orchard, Ramey, and Wieland, 2025). In our setting, higher demand for durables affects quantities more than prices.

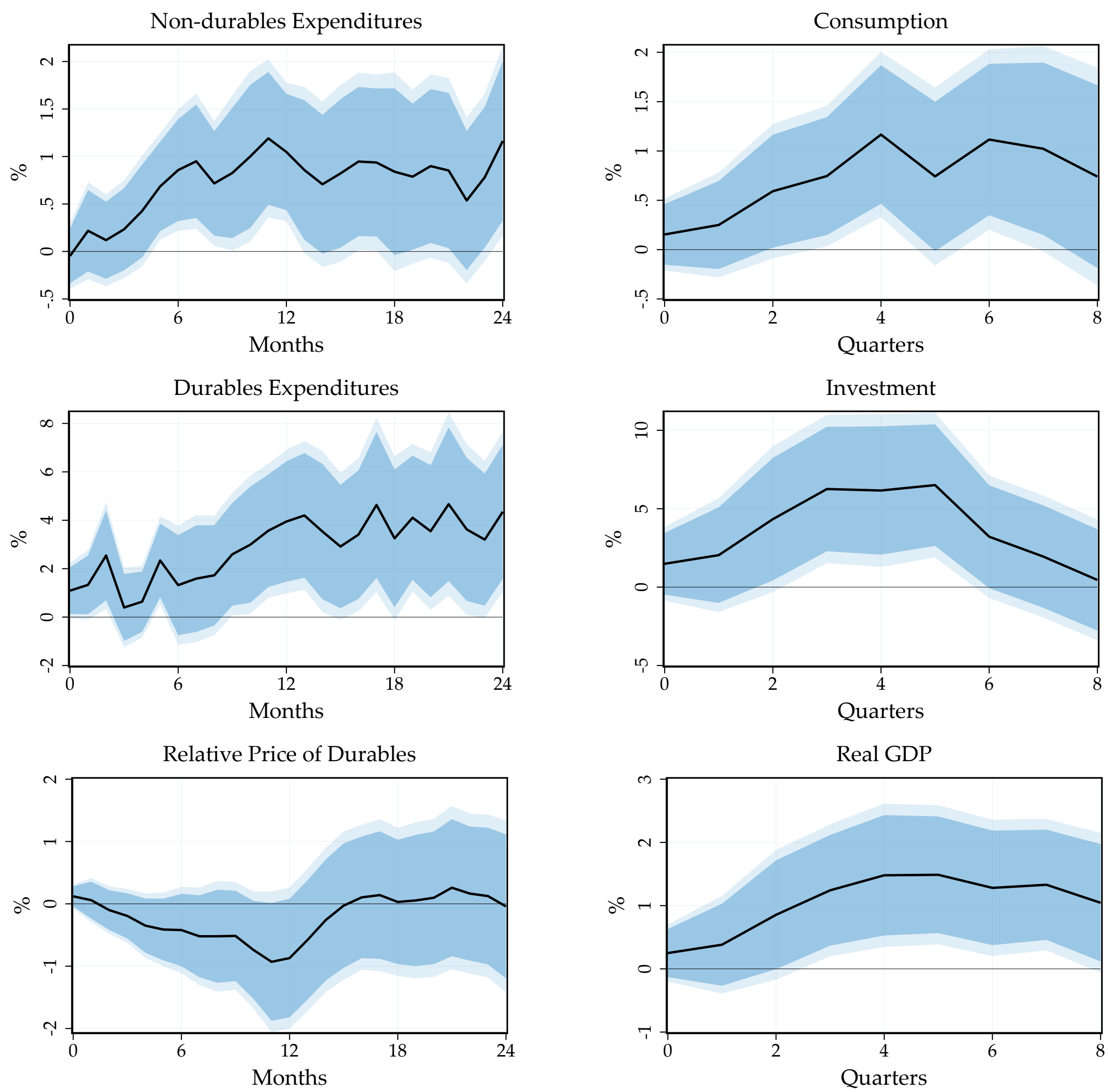
We next ask whether the spending response translates into broader aggregate effects. To do so, we aggregate the shock series to the quarterly frequency and estimate its effects on con-

[13] However, the point estimate does imply that large enough stimulus checks do still lead to substantial inflation. Consider the following back of the envelope exercise: combining the late-2020 and early-2021 stimulus checks, which amounted to around 2.5% of annual GDP, with our estimate of how transfers affect the price level. This extrapolation implies roughly 4 percentage points of additional cumulative inflation after two years, similar to the estimates of Hazell and Hobler (2025), who use a high frequency narrative approach.

Figure 3: Consumption Components, Relative Prices, and Broader Impacts

(a) Non-durable/Durable Responses

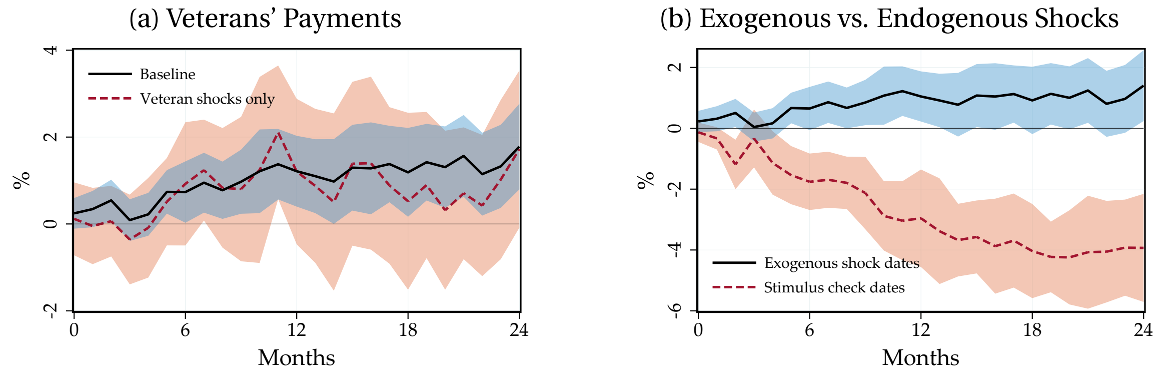
(b) GDP and Components



Notes: Impulse responses to a temporary transfer shock estimated based on the local projections specification (1) using the exogenous transfer shock dates. Left panels: non-durables, durables, and durable-goods PPI relative to aggregate PPI. Right panels: quarterly consumption, investment, and real GDP. Lines are point estimates; shaded areas are 90 and 95% confidence bands. Monthly samples are 1947m1–2019m12 for spending and 1945m10–2019m12 for prices; quarterly sample is 1945q4–2019q4.

sumption, investment, and real GDP. Figure 3, right panels, shows that the increase in household spending is accompanied by a broader expansion in economic activity. Aggregate consumption rises persistently and with a hump shape, reaching about 1% after four quarters, while investment responds even more strongly, increasing by roughly 6% at its peak. These increases are reflected in real GDP, which rises persistently and again with a hump shape, peak-

Figure 4: The Role of Veterans' Payments and Accounting for Endogeneity



Notes: Personal consumption expenditure responses under alternative shock specifications. Panel a restricts shocks to veterans' payment episodes; Panel b compares exogenous and endogenous transfer episodes (stimulus checks). Dashed red lines show the alternative estimates; solid black lines show the baseline. The responses here are normalized to match the peak increase in transfers in the baseline. Lines are point estimates; shaded areas are 95% confidence bands. Sample: 1945m10–2019m12.

ing at around 1.5%. Overall, the results suggest that temporary transfer payments have effects beyond the direct household spending response, generating a broader expansion in aggregate demand and output.

Appendix Section B.5.5 investigates the financing of the postwar payments. As we have seen, transfers rise on impact. However, taxes do not rise for at least four quarters. Therefore, in the short run, the postwar payments appear to be deficit financed.

The role of veterans' payments and additional episodes. How important is our new chronology of exogenous veterans' payments for the results? To answer this question, we first restrict the shock series to the nine veterans' payment dates only. Figure 4a shows that the responses are similar to the baseline in terms of both magnitude and persistence. However, the estimates are less precise, given the smaller number of events (see Appendix Figure B.14 for the full set of responses). We then perform the complementary exercise of restricting attention to the Romer and Romer (2016) shocks alone. The estimated responses remain positive, but again are less precise (Appendix Figure B.15). These exercises suggest that the variation provided by veterans' payments is crucial for statistical power.

We also examine whether the results are sensitive to including two episodes that fall outside the main sample: the June 1936 veterans' bonus payment, which occurs during the Great Depression, and the March 2025 Social Security Fairness Act payment, which falls in the COVID-era window. Appendix Figure B.13 shows that including either episode leaves the responses of transfers and personal consumption expenditures broadly unchanged.

Exogenous versus endogenous shocks. How important is the endogeneity of stimulus checks? To examine this question, we compare our baseline estimates to a set of transfer episodes that are clearly endogenous to the economy. This exercise provides a useful benchmark: if our narrative shocks isolate plausibly exogenous transfers, then endogenous transfers should lead to different dynamics. We estimate impulse responses based on dates of stimulus checks, including the rebate payments in 2001, 2008 and 2020.[14]

Appendix Figure B.5 shows that stimulus-check dates are also associated with a transitory increase in transfers. The consumption response, however, is very different. As shown in Figure 4b, plausibly exogenous transfer shocks are followed by a persistent increase in personal consumption expenditures, whereas consumption declines after stimulus-check dates. This contrast underscores the importance of identification: because stimulus checks were enacted during downturns, their estimated effects are likely confounded by the recessions that triggered the payments.

Regional evidence. As an additional sense check, we examine whether the effects of temporary transfer shocks are stronger in states that were more exposed to the payments. Since state-level consumption data are not available with the frequency and historical coverage needed for our analysis, we focus on employment responses. We measure state exposure by allocating the real size of each national transfer shock across states in proportion to the relevant recipient shares: veterans' shares for veterans' payments and Social Security recipient shares for Social Security payments. Appendix B.5.4 shows that the average state-level employment response closely tracks the aggregate response, and that states with greater exposure experience stronger employment gains after transfer shocks. This pattern is robust to including time fixed effects.

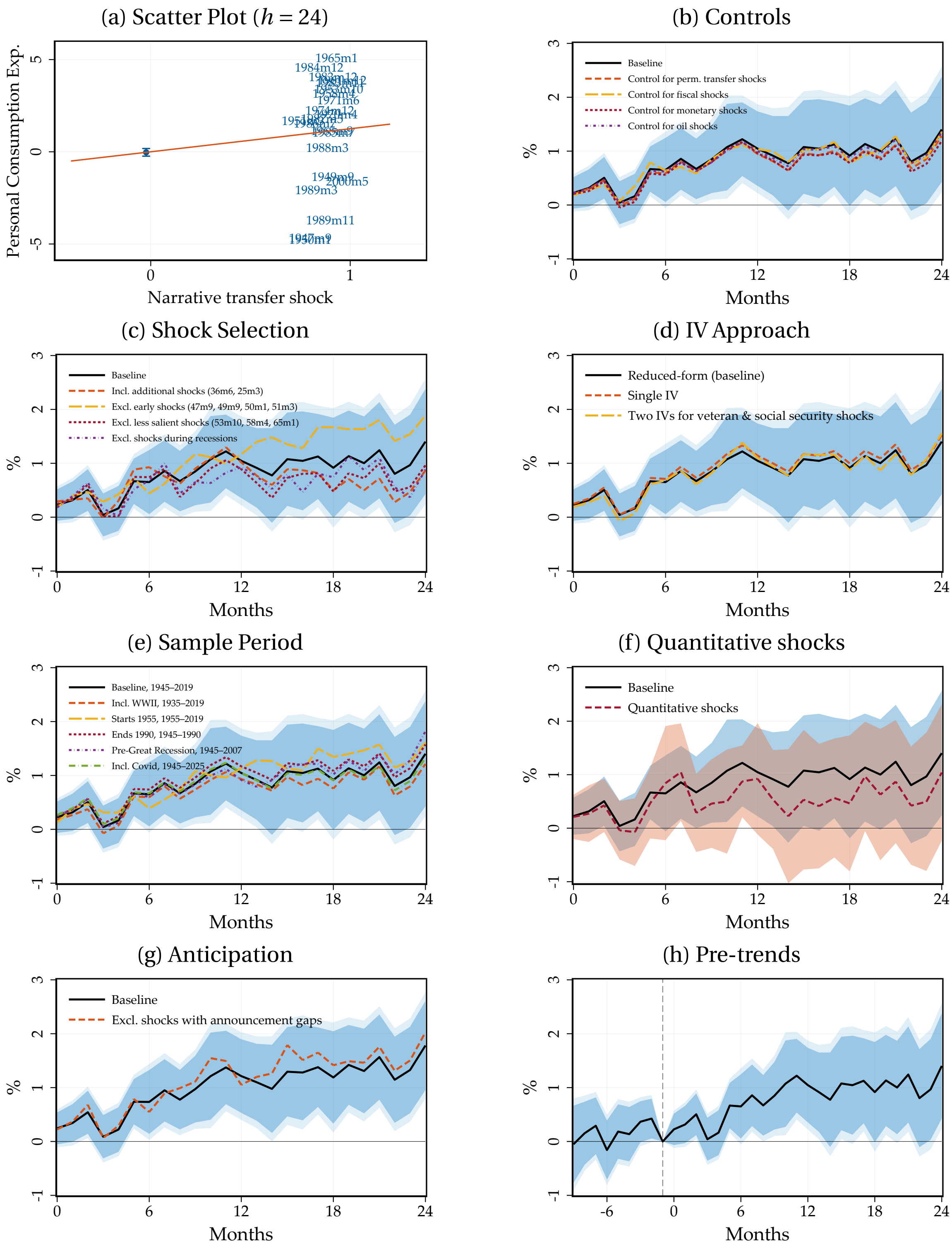
Robustness. We perform an extensive set of sensitivity checks to assess the robustness of our baseline results. We focus here on the personal consumption expenditure responses; full results for all our main outcome variables are reported in Appendix B.6.

Figure 5 summarizes our key robustness checks. We first visualize the identifying variation underlying the estimates. Panel a presents a scatter plot of residualized consumption against the narrative transfer shock, after partialling out the baseline controls. For non-shock dates, we report the mean and corresponding 95% confidence bands. The positive relationship between transfer events and subsequent consumption growth is evident, and the fitted line is not driven by a single influential observation.

Second, Panel b shows that the results are largely unchanged when we control for permanent transfer shocks identified by Romer and Romer (2016), as well as for other prominent

[14] In Appendix B.5.2, we alternatively consider all endogenous transfer payments, including the endogenous veteran and Social Security payments.

Figure 5: Sensitivity Analyses



Notes: Robustness checks for the personal consumption expenditures response to a temporary transfer shock. Panels report the residual scatter plot, additional controls, alternative shock selections, IV specifications, alternative samples, quantitative shock specification, anticipation checks, and pre-trends. Lines are point estimates; blue shaded areas are baseline 90 and 95% confidence bands. Sample: 1945m10–2019m12 unless stated otherwise.

macroeconomic disturbances, including military spending shocks from Ramey (2011), monetary policy shocks from Romer and Romer (2004), tax shocks from Romer and Romer (2010), and oil price shocks from Känzig (2021).[15] The consumption response remains quantitatively similar across specifications, suggesting that our transfer shocks are not proxying for broader fiscal, monetary, or energy-related shocks.[16]

Third, Panel c examines robustness with respect to shock selection. Our results remain largely unchanged when we exclude the early episodes in the immediate aftermath of World War II, drop less salient shocks with limited historical coverage, or exclude shocks that occurred during recession periods. In addition, Appendix Figure B.16 reports a systematic leave-one-out exercise in which we re-estimate the responses dropping one shock at a time. The resulting impulse responses lie well within the 90% confidence bands of the baseline estimates, indicating that our results are not driven by any single, extreme episode. Placebo event dates produce no systematic responses, while the baseline estimates lie near the upper end of the placebo distribution (Appendix Figure B.17). The stability of the estimates, when we drop shocks from 1951 and earlier, is of independent interest. It suggests that the particular features of the post war economy—notably, significant financial repression (Lustig, 2026)—do not drive the results.

Fourth, Panel d shows that the consumption response is similar when we use an instrumental-variable local-projection (LP-IV) specification rather than the reduced-form local projection, and in particular when we use the Social Security and the veterans' payments as separate instruments. Therefore, the results are not due to pooling the two instruments together. Appendix B.6.3 contains more information about the specification, and Appendix Figure B.18 shows the same for other outcomes.

Fifth, Panel e shows that the results are robust across different sample periods. Starting the sample in 1935 or 1955, or stopping before 1990 or 2008, yields consumption responses that are very similar in magnitude and persistence. Similarly, extending the sample to include the COVID-19 period produces similar results.

Sixth, Panel f shows that using a quantitative shock measure based on the logarithm of the real amount disbursed produces similar, though less precise, responses. Appendix Figure B.19 shows the same for other outcomes, and Appendix B.6.3 contains more information about the specification.

Finally, we discuss anticipation, a pervasive concern in the fiscal shocks literature. If households anticipate a transfer before it is disbursed, then our estimates around the disbursement month could miss part of the response that occurred in advance. In our setting, how-

[15]The military spending shocks from Ramey (2011) are only available at quarterly frequency. We convert the series to monthly by assigning quarterly shocks evenly over all months in the quarter.

[16]We also show in Appendix Figure B.11 that a specification with no controls leads to similar results, while the controls help improve precision.

ever, announcement-to-disbursement gaps tend to be short, as we discuss in detail in Appendix A.5.2. As an additional check, we exclude shocks for which the gap between announcement and disbursement exceeds three months. Panel g shows that the estimates remain similar, suggesting that anticipation effects are not quantitatively important for our results. As a complementary timing check, shifting the transfer-shock series backward yields no significant pre-disbursement responses (Appendix Figure B.22).

We also examine pre-trends, to assess the strength of anticipation effects. As discussed above, macroeconomic variables do not predict transfer-shock disbursements, which already makes pre-trends unlikely. Panel h provides visual confirmation by plotting responses in the months before disbursement.[17] The estimates are flat before the shock and show no evidence of systematic pre-trends. This supports the interpretation that the post-disbursement responses are not driven by confounding shocks or anticipatory dynamics.[18]

The responses are also insensitive to adding a linear trend or estimating the local projections in cumulative differences (Appendix Figure B.25). Applying a small-sample local-projection bias correction leaves the estimates virtually unchanged (Appendix Figure B.26).

3.3. The Transfer Multiplier

To quantify the aggregate impact of temporary transfer shocks, we estimate the *transfer multiplier*: the cumulative dollar response of spending per dollar of additional transfers. We follow the approach of Ramey and Zubairy (2018). Specifically, for each horizon h, we regress the cumulative dollar change in consumption on the cumulative dollar change in government transfers over the same horizon, with both variables expressed relative to the lagged level of consumption. Formally, we estimate:

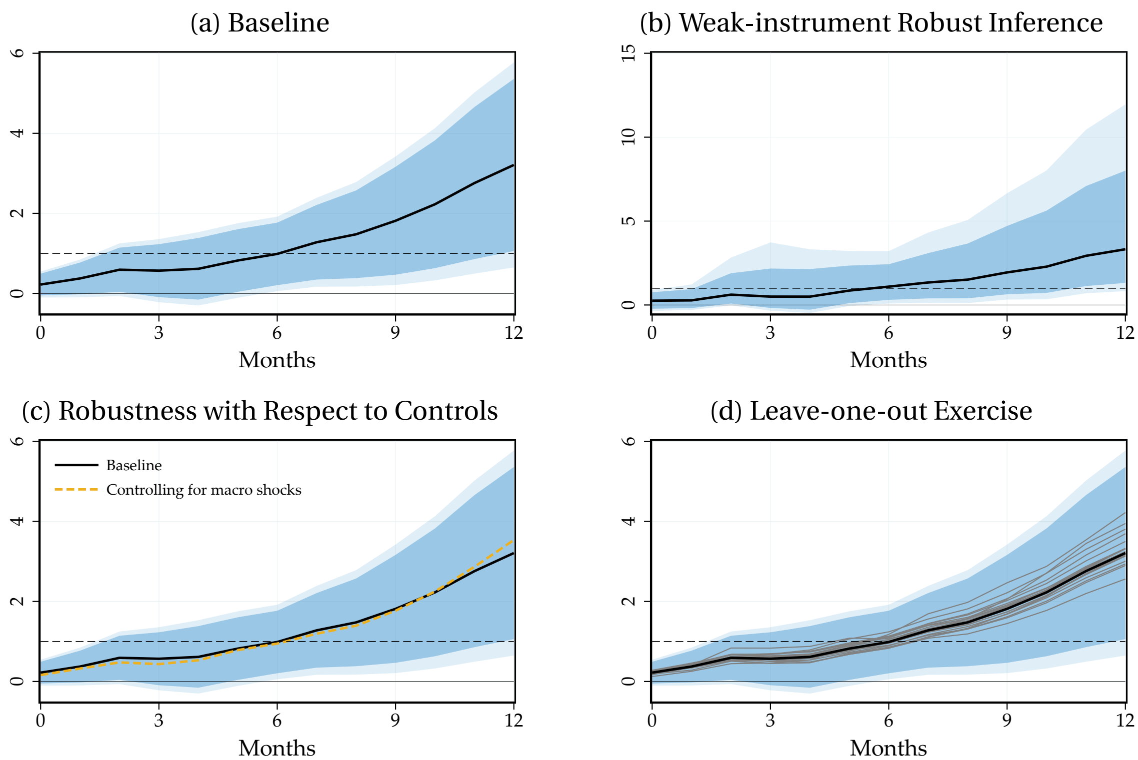
$$\sum_{j=0}^{h} \frac{\Delta_j C_{t+j}}{\tilde{C}_{t-1}} = \beta_h \sum_{j=0}^{h} \frac{\Delta_j T_{t+j}}{\tilde{C}_{t-1}} + \mathbf{\Gamma}'_h \mathbf{W}_{t-1} + \varepsilon_{t+h}, \tag{2}$$

where C denotes personal consumption expenditures in dollars and T denotes government transfers, and $\Delta_j X_{t+j} = X_{t+j} - X_{t-1}$ is the long difference of variable $X \in \{Y, T\}$; we instrument for the right-hand side with our shock series. By construction, β_h directly measures the cumulative spending multiplier through horizon h, i.e., the cumulative dollar response of spending per dollar of transfers. To account for trending behavior, we normalize both variables by $\tilde{C}_{t-1}$,

[17] Appendix B.6.5 reports the pre-trends for other outcomes. When plotting pre-trends, we omit the corresponding lagged outcome controls so that the pre-shock coefficients are not mechanically pinned down by the controls. Appendix B.6.5 also reports pre-trends at earlier horizons, keeping the same control set as in the baseline.

[18] Appendix Section B.5.3 estimates size dependence, by asking whether larger and smaller transfers have different marginal effects per dollar transferred. We find that larger transfers have smaller marginal effects. However, the differences are imprecisely estimated, particularly for smaller transfers.

Figure 6: Transfer Multipliers



Notes: Cumulative transfer multipliers for personal consumption expenditures, estimated using (2). Panels report standard inference, Anderson–Rubin weak-IV bands, additional controls, and leave-one-out estimates. Lines are point estimates; blue shaded areas are baseline 90 and 95% confidence bands. Sample: 1945m10–2019m12.

defined as a 12-month moving average of the outcome variable. This smooths high-frequency variation and mitigates the influence of changes in the denominator. Our results are robust to alternative definitions of the trend. We include as controls 12 lags of changes in consumption and transfers (relative to $\tilde{C}$), CPI inflation, and lags of the transfer shock.

Figure 6 presents the results. From the top-left panel, we can see that the multiplier reaches roughly 1 after six months and exceeds 2 after twelve months. The confidence bands indicate that the medium-run responses are statistically significant at conventional levels. Our narrative transfer shock series is a strong instrument at short horizons. The robust first-stage F-statistic on impact is 10.8. Instrument relevance declines at longer horizons, with F-statistics falling to around 6 after six months (see Appendix B.7). This decline is not surprising given the transitory nature of the transfer shock: the shock generates a sharp increase in transfers on impact, but contains less information about transfers at longer horizons. To alleviate concerns about weak instrument problems, we also report weak-instrument robust Anderson–Rubin confidence bands in the top-right panel. These bands are initially similar to those obtained under standard inference, but widen at longer horizons; eventually, the bands become too wide to deliver informative inference. For this reason, we focus on the multiplier over the first year; at this horizon, estimated multipliers remain statistically different from zero with weak-instrument robust inference.

In the bottom panels of Figure 6, we assess the robustness of the multiplier estimates using two complementary exercises. First, we show that controlling for a rich set of macro shocks (military spending, tax, monetary policy, and oil price shocks) produces similar multiplier paths as in the baseline (bottom-left panel). Second, we perform a leave-one-out exercise, re-estimating the cumulative multiplier after dropping each transfer episode in turn. The bottom-right panel shows that the multiplier paths remain stable, indicating that the results are not driven by any single episode. Overall, the results imply that temporary lump-sum transfers generate a large, persistent and hump-shaped increase in aggregate spending, even though the transfer shock itself is short-lived.

3.4. Corroborating Micro-Evidence from the January 1950 Payment

We close this section with corroborating micro-evidence, from the individual-level spending response to the largest payment in January 1950. We start by establishing that the behavior of the aggregate economy around this episode was typical, compared to the full set of shocks that make up our regressions; before going to the micro evidence.

We study the response of the aggregate economy over the six months following the payment. This window balances two considerations: it is long enough to capture the initial effects of the transfer shock, but short enough to avoid confounding the response with other large, atypical shocks affecting the economy. Narrative accounts suggest that the dividend payments were the main shock hitting the economy over this period.[19] We end the window in June 1950 to avoid contamination from the outbreak of the Korean War on June 25.[20,21]

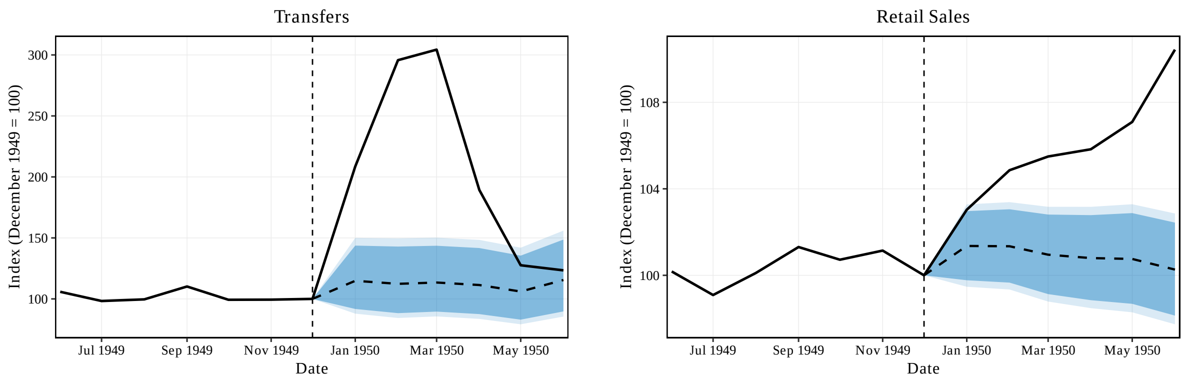
To study aggregate dynamics around the single episode, we compare the observed series to counterfactual paths based on their behavior before the event. Specifically, we construct forecasts using a vector autoregression (VAR) model, estimated on data from 1945–1949. The VAR is estimated with six monthly lags and includes our narrow transfer series, retail sales, industrial production, and the consumer price index, all expressed in logs. These forecasts provide an es-

[19] For instance, the Department of Commerce's *Survey of Current Business* notes in March 1950 that: "*[e]conomic activity during the first 2 months of 1950 tended slightly upward from the rate prevailing at the close of last year ... consumer purchasing in particular was firm as personal incomes were boosted by the substantial payments to veterans*" (U.S. Department of Commerce, 1950). The survey does not mention comparably large shocks driving consumption. Other major shocks, such as the easing of consumer credit regulations (Regulation W), took place in the first half of 1949.

[20] In our regression analysis, we account for this potential confounder using defense spending news as a control.

[21] See Binder and Brunet (2022) for a microdata-based analysis of this episode.

Figure 7: The Economy around the January 1950 Veterans' Payment



Notes: Transfers and real retail sales around the January 1950 veterans' payment. The vertical dashed line marks the payment; dashed lines are four-variable VAR forecasts estimated on 1945–1949 data. Shaded areas are 90 and 95% bootstrap forecast intervals.

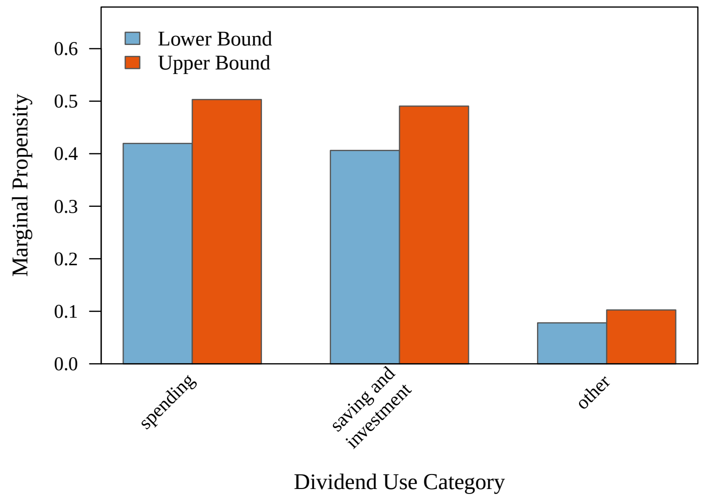
timate of the path the economy would have followed, absent the stimulus—accounting for the evolution of the economy after it emerged from a shallow recession in October 1949. They also provide a benchmark for the size of typical fluctuations in the data relative to the movements observed after the payment date. Appendix B.3 shows that the results are robust to alternative VAR specifications.

The case study shows that the January 1950 payment was associated with a large but temporary increase in transfers and a sizable, persistent increase in retail sales—just as in our regression estimates. From Figure 7, we can see that monthly transfers increased by as much as 200%, before returning to their initial level by June 1950. Over the same period, retail sales rose steadily, reaching an increase of around 12% by June. There is an inflection point in the retail sales series, just as transfers increase. Notably, neither series displays a clear trend prior to the payment. The VAR forecasts for the period after January 1950 are flat. The observed dynamics thus differ sharply from the predicted paths and lie well outside the 95% forecast intervals, suggesting the transfer shock had a large effect on sales. That is, movements in retail sales after the event are much more extreme than even the 95th percentile of typical retail sales movements.[22]

Having confirmed with aggregate data that the January 1950 episode is typical, we turn to the individual-level response. We make use of a remarkable one-off question about the transfers, from the 1951 *Survey of Consumer Finances*. The question asked: "Did you (or anyone in your spending unit) receive a dividend on G.I. insurance in 1950?"; "How much did you get?"; and "What did you do with it?" For the final question, respondents could select up to two op-

[22]An aggregate shock that the contemporary *Survey of Current Business* did not mention, but could still be important was a strike at Chrysler during January-May 1950; perhaps the end of this strike could have contributed to some of the growth in May and June, though likely not earlier.

Figure 8: Marginal Propensity to Spend out of 1950 Dividend



Notes: Self-reported uses of the 1950 G.I. insurance dividend from the 1951 Survey of Consumer Finances, weighted by sample weight times dividend amount. Lower bounds count single-category responses; upper bounds also include responses listing that category alongside one other use.

tions; we map these observations to spending, saving and investment, or other categories. As we discuss in Appendix B.4, we use this information to bound the fraction of the transfer spent. Figure 8 presents the results. Veterans say that they spent 42–50% of the transfer, consistent with modern estimates of the marginal propensity to spend (e.g. Colarieti, Mei, and Stantcheva, 2024).[23,24]

4. Matching Estimates to a HANK Model

We develop a heterogeneous-agent New Keynesian model, for two purposes. First, we use the model to characterize conditions under which the multipliers for stimulus checks and veterans' payments are the same. Second, we use the model to ask what mechanisms are required to match the hump-shaped response of consumption to transfer shocks. The HANK model is standard in most respects. However, we allow imperfect macroeconomic expectations, which underreact to shocks on impact and overreact to shocks later on. This mechanism will be crucial for matching the hump-shaped response of consumption to the transitory shock.

[23]Some previous papers estimate the marginal propensity to spend out of the dividend using micro-data (Bodkin, 1959; Bird and Bodkin, 1965). However, these papers used a single cross section of data, which makes identification challenging (as Souleles, 1999 discusses).

[24]We also make a caveat: we interpret the question as asking veterans how they would spend the dividend *holding fixed* other factors such as income and taxes. While this interpretation is reasonable, others are possible.

4.1. Environment

Time is discrete and infinite, $t = 0, 1, 2, \ldots$. The economy is initially in steady state, and the object of interest is the response to an unanticipated transfer shock. A unit mass of households is indexed by $i \in [0,1]$. A perfectly competitive final-goods producer operates the linear technology $Y_t = N_t$, where Y_t is output and N_t is aggregate labor in efficiency units. Nominal wages are sticky as in Erceg, Henderson, and Levin (2000). Monetary policy directly controls the real interest rate r, which we hold constant throughout the analysis.[25] There are two ex ante household groups. Veterans ($g = v$) constitute a share $\mu_v \in (0,1)$ of the population, and non-veterans ($g = nv$) constitute the remaining share $\mu_{nv} = 1 - \mu_v$.

Idiosyncratic productivity. Each household draws an idiosyncratic productivity shock $e_{i,t}$ from a finite-state Markov chain with transition matrix $\mathbf{\Pi}$ and ergodic distribution π. The Markov process is common across groups, so veterans and non-veterans face the same transitory income risk. Groups may differ, however, in permanent productivity. Let $\overline{e}_g > 0$ denote the permanent productivity of group g. The effective productivity of household i in group g at date t is $\varepsilon_{i,t} = \overline{e}_g e_{i,t}$. The permanent component $\overline{e}_g$ allows veterans and non-veterans to differ in average earnings, while the common Markov process keeps idiosyncratic risk comparable across groups. We normalize $\mathbb{E}_\pi[e] = 1$, so $\overline{e}_g$ equals mean effective productivity for group g. Permanent productivity is normalized so that $\mu_v(\overline{e}_v)^{1-\gamma} + \mu_{nv}(\overline{e}_{nv})^{1-\gamma} = 1$.

Veterans' insurance fund. The veterans' insurance fund holds special-issue Treasury securities F_t and makes stochastic disbursements T_t^v to veterans. The fund's flow budget constraint is

$$T_t^v = [F_{t-1} - F_t] + rF_{t-1} + \mu_v P, \tag{3}$$

where P is the per-veteran insurance premium. In the deterministic steady state, $T^v = 0$ and F is constant. To make payments T_t^v, the veterans' fund finances itself through premia P, via interest payments on its savings, or by redeeming its special-issue government bonds.

Households. Households in group g have preferences over consumption and labor,

$$E_{i,0} \sum_{t=0}^{\infty} \beta_g^t \left[u(c_{i,t}) - v(n_{i,t}) \right],$$

where $\beta_g \in (0,1)$ is the group-specific discount factor, u is strictly increasing and strictly concave, v is strictly increasing and strictly convex, and $E_{i,0}$ is the household's subjective expecta-

[25] A constant-real-interest-rate policy can be viewed as a Taylor rule with a unit coefficient on expected inflation; see e.g., Bilbiie (2020) and Auclert, Rognlie, and Straub (2024).

tions operator. Under the union-based wage-setting protocol of Erceg, Henderson, and Levin (2000), all households supply identical hours, $n_{i,t} = N_t$.

After-tax labor income follows the progressive retention function of Heathcote, Storesletten, and Violante (2017):

$$y_{i,t} = (1-\tau_t)\big(w_t \bar{e}_g e_{i,t} N_t\big)^{1-\gamma}, \tag{4}$$

where τ_t is a time-varying intercept governing the average tax burden, $\gamma \in [0,1)$ governs the degree of progressivity, and w_t is the wage per efficiency unit.

The flow budget constraint of a veteran household is

$$c_{i,t} + a_{i,t} + P = y_{i,t} + (1+r)a_{i,t-1} + \omega^v_{i,t} T^v_t + \omega_{i,t} T_t, \tag{5}$$

where $a_{i,t}$ denotes financial assets at the end of period t. The variables T^v_t and T_t denote, respectively, transfer shocks from the veterans' fund and from the government. The premium P is paid by each veteran into the insurance fund. Non-veterans are not eligible for veterans' fund disbursements, and their flow budget constraint is

$$c_{i,t} + a_{i,t} = y_{i,t} + (1+r)a_{i,t-1} + \omega_{i,t} T_t. \tag{6}$$

The targeting coefficients $\omega^v_{i,t} \geq 0$ and $\omega_{i,t} \geq 0$ determine the cross-sectional allocation of veteran payments and government transfers. They satisfy $\int \omega^v_{i,t}\, di = 1$ and $\int \omega_{i,t}\, di = 1$, with $\omega^v_{i,t} = 0$ for non-veterans. Households also face the group-specific borrowing constraint $a_{i,t} \geq -\underline{a}_g(\bar{e}_g)^{1-\gamma}$. Scaling the borrowing limit by $(\bar{e}_g)^{1-\gamma}$ keeps the tightness of the constraint invariant to permanent productivity.

Government. Total tax revenue under the progressive tax system is $\mathcal{T}_t = Y_t - \int y_{i,t}\, di = Y_t - (1-\tau_t)(w_t N_t)^{1-\gamma}\bar{\varepsilon}^{1-\gamma}$, where $\bar{\varepsilon}^{1-\gamma} \equiv \mathbb{E}[\int(\bar{e}_g e_i)^{1-\gamma}]$ is constant. Individual after-tax labor income can therefore be written as $y_{i,t} = (\bar{e}_g)^{1-\gamma}\theta_{i,t}(Y_t - \mathcal{T}_t)$, where $\theta_{i,t} \equiv \frac{e^{1-\gamma}_{i,t}}{\bar{\varepsilon}^{1-\gamma}}$ is household i's idiosyncratic share of aggregate disposable labor income and $\int \theta_{i,t}\, di = 1$.

The government issues marketable public debt B_t and backstops the special-issue securities F_t held by the veterans' fund. Its flow budget constraint is

$$B_t + F_t + \mathcal{T}_t = (1+r)(B_{t-1} + F_{t-1}) + T_t. \tag{7}$$

Taxes adjust gradually so that public debt held by the public converges back to its steady-state level:

$$\mathcal{T}_t = \mathcal{T} + \tau_d(B_{t-1} - B), \tag{8}$$

where $\mathscr{T}$ is steady-state tax revenue and $\tau_d > 0$ governs the speed of fiscal adjustment.

Expectations. We depart from the standard HANK model by allowing a form of imperfect expectations, which will turn out to be crucial for matching the data. Households observe the variables that enter their contemporaneous budget constraint but do not perfectly observe the future path of aggregate income and taxes. To isolate the role of aggregate expectations, we assume households forecast their idiosyncratic productivity process correctly. Furthermore, we assume that veterans know the path of transfers from the veterans' fund.

As in Bardóczy and Guerreiro (2023), households form beliefs according to a model that combines noisy information with long-memory diagnostic expectations, building on Bordalo et al. (2020) and Bianchi, Ilut, and Saijo (2024). The implied cross-sectional average expectation of the change in an aggregate variable X_{t+h} is

$$\overline{E}_t[dX_{t+h}] = \left[(1+\theta)\frac{\tau(t+1)}{1+\tau(t+1)} - \theta\sum_{j=1}^{t}\alpha_j\frac{\tau(t+1-j)}{1+\tau(t+1-j)}\right]\mathbb{E}_t[dX_{t+h}], \tag{9}$$

where $\mathbb{E}_t[\cdot]$ denotes model-consistent expectations conditional on date-t information, τ is the relative precision of signals, θ governs diagnostic over-extrapolation, and the memory weights satisfy $\alpha_j \geq 0$ and $\sum_{j=1}^{\infty}\alpha_j = 1$.

The specification nests several benchmarks. If $\tau \to \infty$ and $\theta = 0$, it converges to full-information rational expectations. If $\theta = 0$ and $\tau < \infty$, it becomes a noisy-information rational-expectations model in the tradition of Lucas Jr (1972), Woodford (2003), or Angeletos and Huo (2021); sticky information yields a similar form of underreaction (Mankiw and Reis, 2002; Auclert, Rognlie, and Straub, 2020). If $\theta > 0$ and $\tau \to \infty$, the specification reduces to full-information diagnostic expectations. With one-period memory ($\alpha_1 = 1$, $\alpha_j = 0$ for $j > 1$), it nests Bordalo, Gennaioli, and Shleifer (2018); with multi-lag decaying weights, it nests the long-memory generalization of Bianchi, Ilut, and Saijo (2024).

This specification allows for expectations that underreact to shocks on impact, but overreact later on, which is a salient feature of the data (Angeletos, Huo, and Sastry, 2021). The first term in brackets is a cumulative Kalman gain—the optimal Bayesian weight that households place on their accumulated signals. Suppose that τ is finite. Then the gain is below one and household expectations underreact to shocks on impact. The second term is a diagnostic correction relative to a memory-weighted reference. Rearranging, the bracket equals $K_{t+1} + \theta(K_{t+1} - \bar{K}_{t+1})$, where $\bar{K}_{t+1} = \sum_j \alpha_j K_{t+1-j}$ is a memory-weighted average of past Kalman gains. Because the gain rises as signals accumulate, K_{t+1} pulls ahead of $\bar{K}_{t+1}$ over time. The diagnostic correction amplifies this gap—pushing the response of expectations above the pure noisy-information case and, for large enough θ, above one (Bordalo, Gennaioli, and Shleifer, 2018). The memory weights α_j control how far back the comparison reaches; with the slowly decaying weights of Bianchi, Ilut, and Saijo (2024), the lag in $\bar{K}_{t+1}$ is larger and the resulting overreaction is more persistent.

Wages and labor supply. Wages and labor supply are chosen by monopolistically competitive unions (Erceg, Henderson, and Levin, 2000; Schmitt-Grohé and Uribe, 2005). As in Auclert, Rognlie, and Straub (2024), unions require every worker to supply the same number of hours, $n_{i,t} = N_t$. The union problem implies the wage New Keynesian Phillips curve

$$\pi_t^w = \kappa_w \left[\psi \frac{dN_t}{N} + \sigma \frac{dC_t}{C} - \left\{ \frac{dZ_t}{Z} - \frac{dN_t}{N} \right\} \right] + \beta \mathbb{E}_t[\pi_{t+1}^w],$$

where ψ is the inverse Frisch elasticity, σ is the inverse intertemporal elasticity of substitution, and

$$Z_t \equiv (1-\tau_t)(w_t N_t)^{1-\gamma}.$$

Market clearing and linear solution. Goods market clearing requires $Y_t = C_t \equiv \int c_{i,t}\, di$. The bond market clears when $B_t = A_t \equiv \int a_{i,t}\, di$. We solve the linearized economy using sequence-space Jacobian methods (Auclert, Bardóczy, Rognlie, and Straub, 2021; Auclert, Rognlie, and Straub, 2024), extended to allow deviations from full-information rational expectations as in Auclert, Rognlie, and Straub (2020). Appendix C.4 defines the fixed-point representation and the aggregate Jacobians used below.

4.2. Veteran Payments and Stimulus Checks: An Equivalence

We now provide equivalence results: under certain conditions, the multiplier on veterans' payments is the same as for stimulus checks. Therefore a suitably calibrated model—matching the response of the economy to veterans' payments—can be used to study the effect of stimulus checks.

The logic works through the consolidated balance sheet of the government. Combining (3) and (7) yields

$$B_t + \mathcal{T}_t + \mu_v P = (1+r)B_{t-1} + T_t + T_t^v. \tag{10}$$

The consolidated budget constraint (10) is the key object for mapping veterans' payments into standard fiscal-transfer experiments. Once the veterans' insurance fund is consolidated with the Treasury, the internal position F_t nets out. The fiscal authority must finance the total cash outflow, $T_t + T_t^v$, regardless of whether that outflow is recorded as a government transfer or as a veterans' fund disbursement.

Lemma 1 (Taxes Depend Only on Consolidated Payouts)**.** *Fix the initial level of government debt B_{-1}. Then the sequences $\{B_t\}_{t\geq 0}$ and $\{\mathcal{T}_t\}_{t\geq 0}$ implied by (10) and the tax rule (8) depend on transfers only through the total transfer outlays inclusive of veteran payments, $\{T_t + T_t^v\}_{t\geq 0}$.*

The lemma is an accounting result. Since taxes respond to debt through the fiscal rule, the timing and magnitude of tax adjustment inherit the same consolidated-budget property: the government budget depends on the overall cash outflow, not on whether that outflow is labeled a regular fiscal transfer T_t or a veterans' fund payment T_t^v.

Proposition 1 (Veteran Payments Are Equivalent to Transfers Targeting Veterans)**.** *Consider two one-time policies with the same path of total transfer expenditure, so that $\{T_t^v\}_{t\geq 0} = \{T_t\}_{t\geq 0}$:*

1. *a veterans' fund payment of size $\{T_t^v\}_{t\geq 0}$;*
2. *a government transfer of size $\{T_t\}_{t\geq 0}$ targeted to the same recipients, with targeting coefficients $\omega_{i,t} = \omega_{i,t}^v$.*

The two policies generate the same equilibrium effects on every aggregate variable. In particular, they induce the same aggregate consumption response and the same output path.

This proposition is an equivalence result showing that with the same targeting rule, stimulus checks and veterans' payments have the same effect. When the insurance fund pays veterans, the fund redeems special-issue Treasury securities for cash, and the Treasury finances the redemption by issuing marketable debt. From the standpoint of the consolidated government, the transaction is a debt-financed cash transfer to a subset of households. Under the same targeting rule, a veterans' fund payment and a government transfer to veterans are equivalent. Implicitly, we are also assuming that the response of monetary policy, and the fiscal rule of the government—whether to raise taxes or issue debt to finance the cash transfer—are the same for stimulus checks and veterans' payments.

The veterans' fund operates like a forced-saving device that pays back at a fixed date. The claim on the fund is illiquid—neither pledgeable nor tradable. Therefore constrained households cannot smooth consumption in anticipation of the dividend, which is a liquidity shock with the same effect as a stimulus check.[26] The fund saves by holding claims to future government tax revenue, i.e., the redeemable government bonds. When paying the dividend, the government has to finance these claims by issuing more debt (or changing taxes or spending); similar to deficit-financed stimulus checks.

The result formalizes the sense in which a veterans' payment is a special case of a fiscal

[26] In practice, future dividend payments were neither tradable nor pledgeable (U.S. Senate Committee on Finance, 1946).

transfer. Any difference between the macroeconomic effects of veterans' payments and those of broader stimulus checks must arise from differences in recipient composition and targeting. In Appendix C.7, we discuss what happens when the targeting of veterans' payments and stimulus checks is different. In that case, veterans' payments and a stimulus check generate the same aggregate output path if and only if the marginal propensities to consume (MPCs) of the two recipient groups coincide at all horizons. Therefore a suitably calibrated model—which matches the MPCs of veterans and normal households as well as the time-series evidence on the postwar payments multiplier—can be used to study stimulus checks.[27]

4.3. Estimating the HANK model

We now suitably calibrate our HANK model. Following Christiano, Eichenbaum, and Evans (2005) and Auclert, Rognlie, and Straub (2020), we proceed in two steps. First, we calibrate the steady state so that the household block reproduces the relevant cross-sectional balance-sheet and spending moments for veterans and non-veterans. Second, conditional on this steady state, we estimate the transfer process and the expectation process that best match the monthly responses of transfers and personal consumption expenditure.

Calibration. Appendix Tables C.1 and C.2 summarize the calibrated parameters. Households have constant-elasticity preferences, $u(c) = c^{1-\sigma}/(1-\sigma)$ and $v(n) = \zeta n^{1+\psi}/(1+\psi)$. We set the intertemporal elasticity to $\sigma^{-1} = 0.50$, and set the Frisch elasticity to $\psi^{-1} = 0.75$. The parameter ζ normalizes steady-state hours to $N = 1$. Idiosyncratic shocks follow a discretized AR(1) process with persistence $\rho_e = 0.95^{1/3}$ and pre-tax dispersion 0.92, which implies the after-tax dispersion reported in the table.

The group-specific steady-state heterogeneity is disciplined by the *Survey of Consumer Finances*. Veterans account for 22.6% of the population ($\mu_v = 0.226$) and 29% of income. Table C.1 reports the resulting group-level moments. Together with the permanent-productivity normalization above, these moments pin down $\bar{e}_v = 1.29$ and $\bar{e}_{nv} = 0.92$. We calibrate the veteran insurance premium to equal the average monthly NSLI premium as a share of average monthly veteran household income in 1950. Using an average monthly premium of \$5.91 and an average monthly veteran household income of \$4,183/12, this gives $P = 0.0219$.

The progressivity parameter is $\gamma = 0.18$, as in the Heathcote, Storesletten, and Violante (2017) tax schedule. The debt-feedback coefficient is $\tau_d = 0.77\%$, so yearly debt persistence

[27] By a similar logic, there is an equivalence between temporary social security payments and deficit financed stimulus checks, given the same targeting rule. The reason is that the social security fund is invested in US government debt. Therefore the same consolidated balance sheet logic applies. When the social security fund disburses temporary payments, it redeems government debt, forcing the US government to issue debt to the public (or adjust taxes and spending).

is 0.93, as in Auclert, Rognlie, and Straub (2024). To match the empirical evidence on the distribution of dividend payments across the income distribution, we impose the veteran-payment targeting rule $\omega^{v}_{i,t} = 1_{\{g=v\}} e^{\phi}_{i,t} / (\mu_v \mathbb{E}_{\pi}[e^{\phi}])$. The empirical distribution of GI-dividend payments across the income distribution implies $\phi = 0.158$. We calibrate the slope of the monthly Phillips curve to match the quarterly estimates in Hazell et al. (2022), $\kappa_w = 0.0062/9$.

The remaining steady-state parameters are calibrated internally. We jointly choose the discount factors and the level of government debt to match a real interest rate of 2% annually ($r = 0.17\%$ monthly), an annual MPC out of the GI-dividend transfer of 0.45, and the veteran liquid asset share of 18.2%. The solution is $\beta_v = 0.976$, $\beta_{nv} = 0.983$, and $B = 9.88$. The lower discount factor for veterans helps reconcile their relatively small asset share with their higher permanent income.

Impulse-response matching. We next estimate the dynamic response to the transfer shock. The empirical targets are the full monthly impulse responses of transfers (relative to steady state consumption),[28] personal consumption expenditure and the cumulative transfer multiplier, covering horizons $h = 0, \ldots, 24$. The transfer response is assumed to follow the geometric-decay process $T^{v}_{t} = \epsilon \rho^{t}$. We estimate the transfer-shock parameters jointly with the admissible parameters of the expectation process.

The full diagnostic-expectations specification is indexed by $\Theta^{DE} = (\epsilon, \rho, \theta, \tau, a, b)$. As in Bianchi, Ilut, and Saijo (2024) and Bardóczy and Guerreiro (2023), the memory weights are approximated by a beta-binomial distribution over $n = 24$ months, with $\alpha_j = \Pr(J = j \mid n = 24, a, b)$ for $j \in \{1, \ldots, 24\}$. For each candidate parameter vector, we transform the full-information household Jacobians into Jacobians under diagnostic expectations, solve the general-equilibrium sequence-space system, and compare the model-implied transfer and consumption paths to the empirical IRFs. The criterion weights deviations by the estimated standard errors of the empirical impulse responses:

$$\mathscr{L}(\Theta) = \sum_{X \in \mathscr{X}} \sum_{h=0}^{T} \left(\frac{X^{model}_{h}(\Theta) - X^{data}_{h}}{\mathrm{SE}_{X,h}} \right)^2,$$

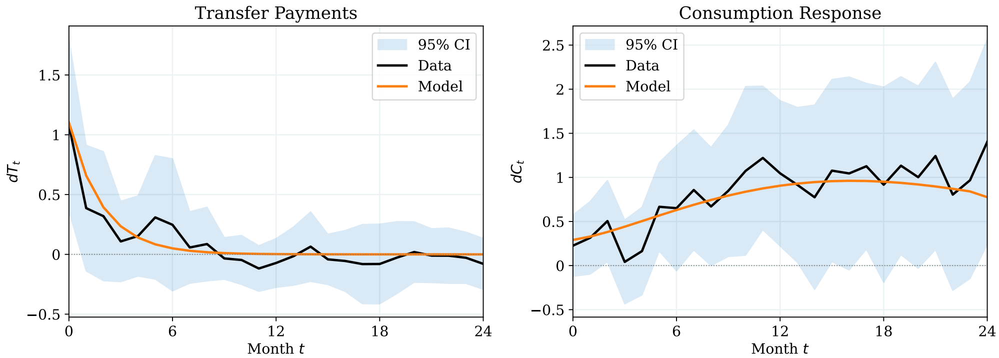
where $\mathscr{X} \equiv \{T, C, \text{Transfer Multiplier}\}$.

Does matching the spending response require imperfect expectations? To answer this question, we compare the full specification with the full-information rational expectations (FIRE) model, which imposes $\theta = 0$ and $\tau = \infty$. In the FIRE economy, we reestimate the admissible

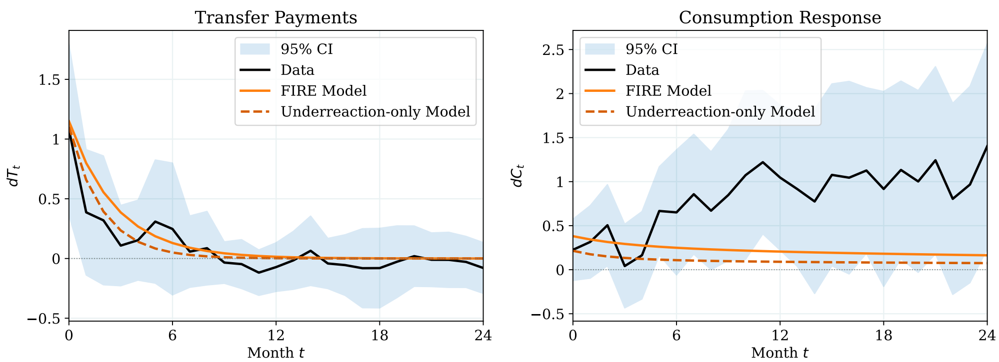
[28] To obtain the empirical path of transfers, we scale the transfer shock series $d\log\text{Transfers}_t$ by a coefficient T/C such that $\sum_{h=0}^{12} d\log\text{Consumption}_h / \sum_{t=0}^{12} \left[\frac{T}{C} d\log\text{Transfers}_t\right]$ matches the estimated cumulative multiplier. The coefficient is $T/C \approx 0.06$ which is approximately the historical ratio of transfers to personal consumption expenditure.

Figure 9: Model Fit to Transfer and Consumption Impulse Responses

(a) Full Model



(b) Counterfactuals: FIRE and Underreaction-only Models



Notes: Transfer and consumption impulse responses under the estimated full model, the FIRE model, and the underreaction-only model, which imposes $\theta = 0$.

parameters ϵ and ρ to give that model its best chance to match the transfer and spending IRFs.

4.4. Imperfect Expectations and the Response of Consumption

This subsection shows that imperfect expectations—which underreact on impact and overreact later on—are critical for matching the hump-shaped response of consumption. Figure 9a displays the fit of the full model. The model tracks the sharp transfer payment on impact and its rapid decay thereafter. More importantly, it reproduces the delayed response of spending: consumption does not simply jump on receipt and then fade, but remains elevated after the transfer response has largely passed through.

The mechanism is the interaction between high MPCs and distorted aggregate expectations. Under our calibration, the response of consumption to transfer shocks is large—recall that we match an annual marginal propensity to consume of 0.45. However, noisy information dampens the initial response of consumption. Households are slow to realize that—as others spend transfers—income will rise in general equilibrium. Later on, households' expectations about general equilibrium movements in income overreact. Diagnostic expectations lead households to believe that income will be larger than what ends up happening. The combination generates a spending response that is initially muted but becomes stronger, as in the data.

To understand these forces, Figure 9b plots the response of consumption with full information rational expectations. The initial response of consumption is large, but peaks on impact. Households immediately understand the future path of aggregate income and taxes. They start consuming immediately, and gradually reduce their consumption level as the shock fades into the past. As such, the rational expectations model cannot match the delayed response of consumption, which peaks after a year. The failure of rational expectations is not that consumption responds too little to transfers—but rather, that the consumption response is too early relative to the data.[29]

Figure 9b also shows that underreaction alone cannot match the data; later overreaction is also necessary. The figure displays the impulse response function of consumption in the estimated economy, but shutting down the delayed overreaction mechanism by imposing $\theta = 0$. The underreaction-only economy can match the initial transfer response, but it fails to sustain spending after the transfer shock itself has faded.

Appendix Figure C.2 shows that expectations data are consistent with our mechanism. We estimate the response of 12-month-ahead industrial production expectations from the Livingston Survey to the transfer shock and compare this response to what our model predicts. Survey expectations are not targeted in the model estimation; however, beliefs in the model and the data closely align (though the impulse response of expectations is noisy): in the data, expectations move little on impact, increase only gradually, and eventually overshoot the realized industrial production response. We conclude that imperfect expectations are a promising way of explaining the delayed response of consumption.

Other explanations for the hump-shaped consumption response. Are there other appealing mechanisms that could also deliver the hump-shaped response of consumption to a transitory shock? One common technique to introduce a hump-shaped response of consumption is habit formation (e.g. Christiano, Eichenbaum, and Evans, 2005). However, habit formation is inconsistent with well-known micro facts, in particular that consumption tends to jump on impact after households receive cash transfers (Auclert, Rognlie, and Straub, 2020). Perhaps a model of durable consumption, in which households face costs of adjusting the stock of durable goods, could lead to a hump shape. However, we have seen that the response of non-durables is also

[29] The root mean squared error (RMSE) of the full model is 0.52, while the RMSE of the FIRE model is 1.16. The full model therefore fits the data twice as well as the rational expectations specification.

hump shaped, meaning durability alone is unlikely to be sufficient (see Figure 3a). One potentially promising explanation is investment adjustment costs: perhaps investment adjusts slowly to shocks, meaning consumption also takes a while to rise. Appendix Section C.11 evaluates this possibility and finds that investment adjustment costs cannot quantitatively account for the hump-shaped response of consumption in the data.

The stimulus checks multiplier. We estimated the model using the impulse responses of consumption to veterans' dividends. As we have discussed, combining this information with a well calibrated model—which matches the MPCs of veterans and other households—allows us to study the stimulus checks multiplier. Our stimulus check experiment differs from the veterans' payment experiments for two reasons: first, we assume that the stimulus checks are distributed to the entire population, rather than just to veterans, and, second, we assume that each person receives the same stimulus check.

The model-implied multipliers are very close to the directly estimated multipliers in Section 3.3 (see Appendix Table C.3): they are close to one after six months and exceed two after one year. The stimulus-check multipliers are slightly lower than the veterans' payment multipliers, because the broader recipient population includes households with lower MPCs than veterans. However, the difference between the two transfer types is small, suggesting that the estimated veterans' payment multipliers are informative about the likely effects of stimulus checks. Importantly, the multipliers under FIRE are much lower and generally remain below one.

5. Conclusion

This paper studies a classic question in macroeconomics: how do stimulus checks affect the economy? We provide a narrative account of postwar U.S. transfer policy and identify a series of large, transitory, and plausibly exogenous changes in government transfers, driven by veterans' payments and temporary Social Security payments. Using newly assembled monthly data, we use these episodes to estimate the dynamic causal effects of temporary transfers. We find that a temporary increase in transfers generates a large, persistent and hump-shaped rise in personal consumption expenditures. The resulting transfer multiplier reaches roughly one after six months and exceeds two after twelve months, with little response from monetary policy or the price level.

To interpret these results, we develop a HANK model that links the historical transfer episodes to modern stimulus checks. Standard versions of the model struggle to explain the persistence of the consumption response. We show that imperfect expectations are crucial for matching the data: households initially underreact to the transfer shock, but later overreact, generating the hump-shaped response we estimate empirically. More broadly, our results suggest that the macroeconomic effects of stimulus checks depend not only on who receives the transfers, but also on how households perceive and learn about their aggregate consequences.

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Online Appendix

The Macroeconomic Effect of Stimulus Checks

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Contents

A. Data Appendix

A.1. Monthly Government Transfer Series

This appendix describes the construction of the monthly government transfer series used in the empirical analysis. We build a monthly series beginning in 1935 using digitized program-level flows from Treasury publications, and align aggregate levels to NIPA transfer totals when available. We construct (i) Treasury-program aggregates and (ii) benchmark-consistent aggregates.

A.1.1. Sources and Treasury-program aggregates

Treasury program flows. The primary input is program-level monthly nominal flows digitized from historical U.S. Treasury publications. The main source is the *Annual Report of the Secretary of the Treasury: Statement of the Finances* (henceforth, *Statement of the Finances*), using the table reporting expenditures of trust accounts. The layout varies across vintages, but the relevant program entries are reported at monthly frequency. Figure A.1 shows a representative example.

Figure A.1: Raw Treasury monthly program series (representative vintage)

TABLE 5.—*Trust account and other transactions, monthly for the fiscal year 1956 and totals for 1955 and 1956*—Continued

[In thousands of dollars]

Trust accounts, etc. Expenditures (except investments)	Fiscal year 1956						
	July 1955	August 1955	September 1955	October 1955	November 1955	December 1955	January 1956
Federal old-age and survivors insurance trust fund—Continued							
Payments to general fund:							
Administrative expenses	2,443	3,543	2,455	2,531	2,531	2,531	2,470
Refunds of taxes				66,000			
Benefit payments	424,102	428,390	427,851	434,162	436,644	437,443	438,481
Other		(*)	1	(*)	(*)	(*)	1
Railroad Retirement Board:							
Railroad retirement account:							
Administrative expenses	632	532	519	718	504	471	527
Benefit payments, etc	47,453	47,708	47,499	49,391	49,891	49,867	49,795
Payment to Federal old-age and survivors insurance trust fund	7,439						
Unemployment insurance administration fund	481	844	412	689	491	453	627
Unemployment trust fund:							
Railroad unemployment insurance account	5,391	8,814	8,573	8,139	9,074	10,776	12,020
State accounts—withdrawals by States	89,090	90,825	77,739	67,034	67,333	92,712	140,517
Veterans' life insurance funds:							
Government life insurance fund—benefits, refunds, etc	9,741	5,774	5,427	6,057	5,705	5,773	6,842
National service life insurance fund—benefits, refunds, and dividends	38,964	43,116	44,020	41,741	39,912	37,911	41,663

Notes: Screenshot from the *Statement of the Finances* (1956 vintage) illustrating monthly program flows used in the construction. Disability is incorporated into Social Security beginning in August 1957 when separately reported.

Armed Forces Leave Bonds (AFLB) are digitized from the *Monthly Treasury Bulletin*, from the table titled *"Comparison of Total Budget Expenditures with Cash Budget Expenditures."* Figure A.2 shows a representative example.

Figure A.2: Raw monthly Armed Forces Leave Bonds (AFLB) series

Table 3.- Derivation of Cash Budget Expenditures
(In millions of dollars)

Fiscal year or month	Total budget expenditures 1/ 2/	Less: Noncash budget expenditures											Equals: Cash budget expenditures
		Total	Interest payments by Treasury			Transfers to trust accounts 2/	Payroll deduction for Government employees' retirement	Budget expenditures involving issuance of Federal securities 5/			Payments to Treasury by Government enterprises		
			On savings bonds and Treasury bills 3/	To Government corporations (partially owned) 4/	To trust accounts			Armed forces leave bonds	Adjusted service bonds	Notes issued to International Bank and Fund	Interest	Investments in Federal securities	
1941........	13,387	600	58	-	167	333	56	-	-20	-	1	5	12,787
1942........	34,187	754	81	-	207	384	88	-	-12	-	6	-	33,433
1943........	79,622	1,198	130	2	254	440	227	-	-7	-	37	115	78,424
1944........	95,315	1,470	213	3	325	559	269	-	-5	-	67	39	93,845
1945........	98,703	2,750	342	4	429	1,659	290	-	-108	-	108	25	95,952
1946........	60,703	3,281	435	22	567	1,927	281	-	-86	-	118	18	57,422
1947........	39,289	6,099	467	25	645	1,361	259	1,846	-8	1,366	105	31	33,190
1948........	36,791	4,304	559	24	746	4,178	236	-1,221	-4	-350	112r	30	32,482
1949 (Est.).	37,180	2,942	575	27	846	1,298	336	-149	-	-35	31	13	34,239
1950 (Est.).	41,858	2,867	450	29	926	1,124	359	-59	-	-50	44	44	38,990
1948-Jan....	2,800	282	62	-	133	66	20	-47	*	-	47r	3	2,518
Feb....	2,224	-35	36	*	*	11	19	-34	*	-50	*	-16	2,259
Mar....	3,086	28	37	1	14	17	19	-38	*	-50	*	28	3,058
Apr....	2,541	87	28	1	2	71	19	-34	*	-	*	1	2,454
May....	2,222	45	42	*	1	1	20	-26	*	-	*	8	2,176
June...	7,018	3,648	51	9	564	3,077	25	-25	*	-50	9	-10	3,370
July...	3,558	676	66	*	*	611	17	-21	*	-	2 r	1	2,882
Aug....	2,143	57	37	*	*	13	24	-19	*	-	1	1	2,086
Sept...	2,869	83	42	1	15	6	27	-15	*	-	*	8	2,786
Oct....	2,685	129	36	*	1	80	26	-15	*	-	*	1	2,556
Nov....	2,815	68	45	-	*	6	29	-14	*	-	*	1	2,747
Dec....	3,603	296	77	10	187	1	28	-14	*	-	4	3	3,308

Notes: Screenshot from the *Monthly Treasury Bulletin* (February 1949) illustrating the AFLB series used in the construction.

Adjusted Compensation Payments under the Bonus Act of 1935 are digitized from the *Statement of the Finances*, from Table 4 ("Public debt receipts and expenditures"), using the "Adjusted service bonds" line.

Benchmarks. We benchmark aggregate levels to NIPA transfer totals (via FRED) at the highest available frequency. The broad benchmark is NIPA *personal current transfer receipts: government social benefits to persons.* We also use NIPA components for Social Security and veterans' benefits to form a narrow benchmark concept.

Treasury-program aggregates. Let the digitized monthly Treasury program flows be: Social Security (SS; Old-Age and Survivors Insurance, and Disability Insurance when separately reported beginning in August 1957), unemployment insurance (UI), railroad retirement (RR), and government life insurance-related disbursements (GLI). We define

$$T_t^{GLI} \equiv T_t^{NSLI} + T_t^{USGLI} + T_t^{AFLB} + T_t^{ACP}, \tag{1}$$

where $NSLI$ denotes National Service Life Insurance, $USGLI$ the U.S. Government Life Insurance, and ACP the Adjusted Compensation Payments under the Bonus Act. The Treasury-program aggregates are

$$T_t^{\text{narrow}} = T_t^{SS} + T_t^{RR} + T_t^{GLI}, \tag{2}$$

$$T_t^{\text{broad}} = T_t^{\text{narrow}} + T_t^{UI}. \tag{3}$$

A.1.2. Benchmarking and splicing

Seasonal adjustment. We seasonally adjust T_t^{narrow} and T_t^{broad} prior to benchmarking. Seasonal adjustment is implemented using X-13ARIMA-SEATS (X-11 filter) and performed separately for the narrow and broad aggregates.

Residual redistribution. Let $\widetilde{T}_t$ denote a seasonally adjusted Treasury-program aggregate (narrow or broad). Let B_P denote the corresponding NIPA benchmark total for period P (year or quarter), and let $\widetilde{T}_P = \sum_{t \in P} \widetilde{T}_t$. Define the residual

$$R_P = B_P - \widetilde{T}_P. \tag{4}$$

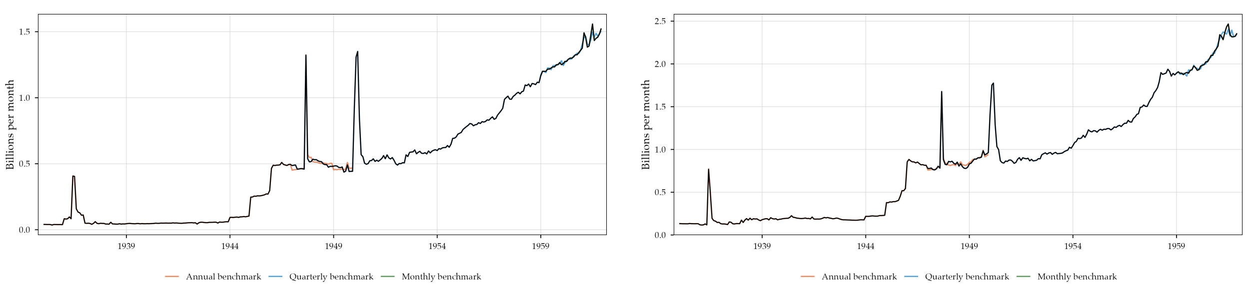
We allocate this residual uniformly within the period:

$$T_t^{\text{bench}} = \widetilde{T}_t + \frac{R_P}{N_P}, \qquad t \in P, \tag{5}$$

where $N_P = 12$ for annual benchmarking and $N_P = 3$ for quarterly benchmarking. When a monthly NIPA benchmark exists, we use it directly.

Figure A.3: Benchmark candidates and stitched series

(a) Narrow transfers (b) Broad transfers



Notes: Benchmark-consistent candidates constructed using annual, quarterly, and monthly benchmarks where available, and the stitched preferred series used in the paper.

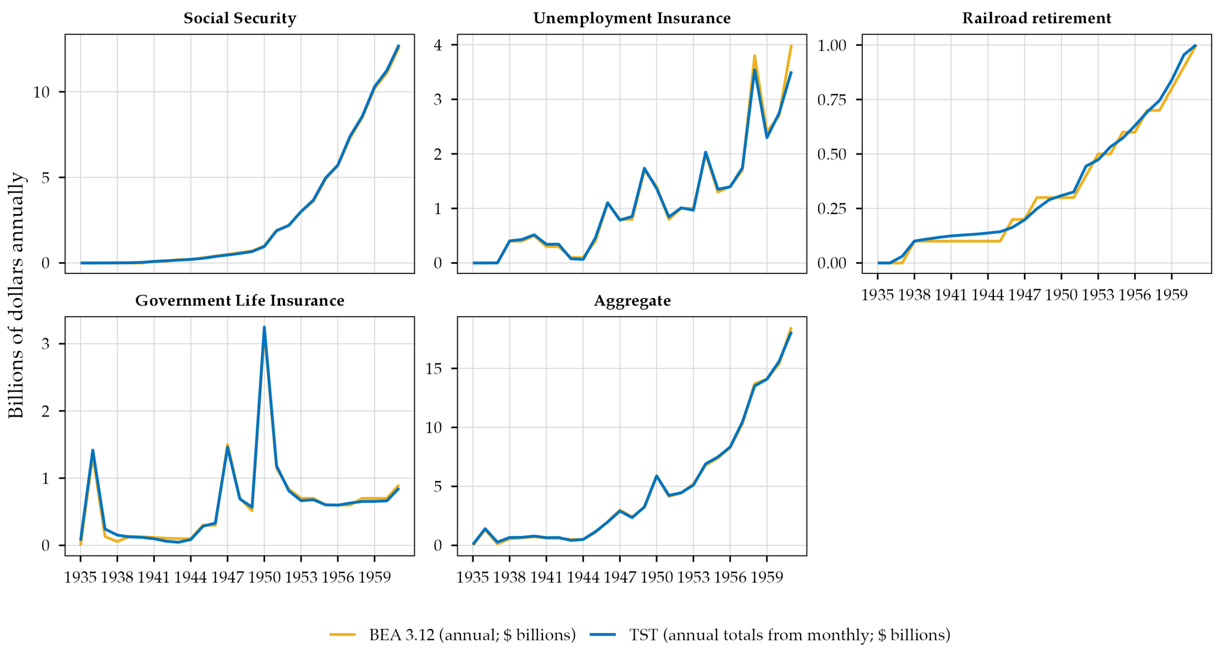
Splice. For each aggregate (narrow and broad), the preferred splice is: 1935–1946 annual benchmarking; 1947–1958 quarterly benchmarking; 1959 onward monthly benchmarking. Figure A.3

shows the benchmark-consistent candidates and the stitched preferred series, separately for the narrow aggregate in Panel a and the broad aggregate in Panel b.

A.1.3. Validation and coverage

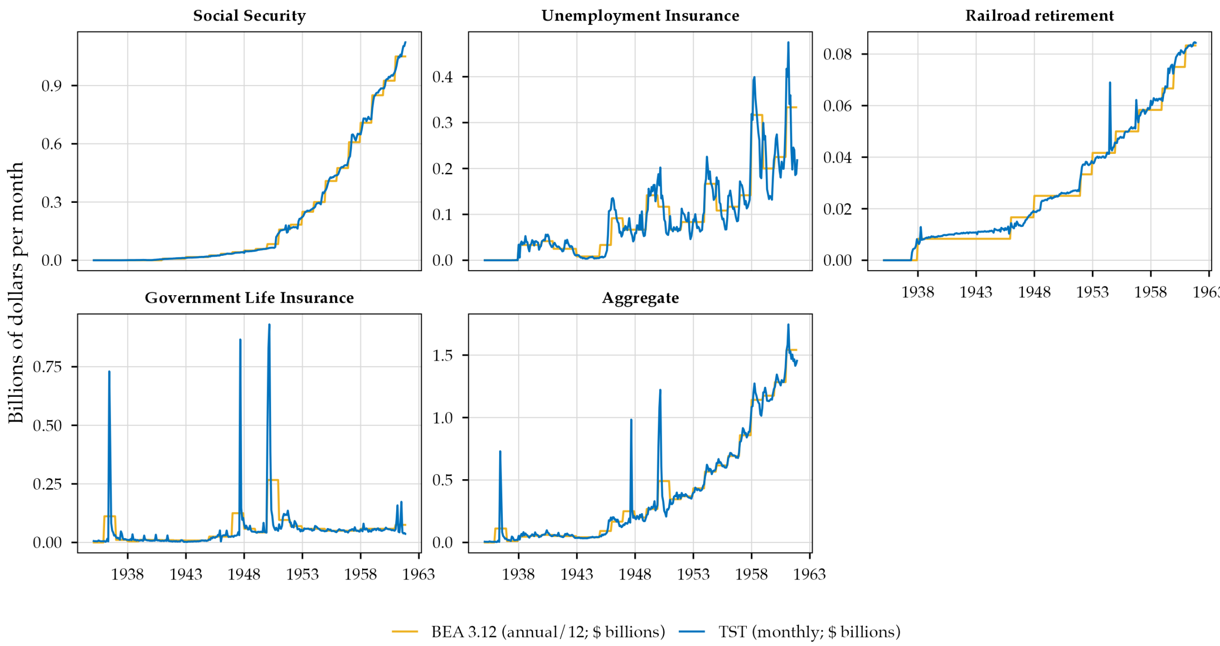
BEA Table 3.12 validation. We compare annual totals implied by summing Treasury monthly flows within each year to annual line items from BEA Table 3.12 (*Government social benefits*) for Social Security, unemployment insurance, railroad retirement, and veterans' insurance-related payments. Figure A.4 shows annual comparisons. Figure A.5 compares Treasury monthly flows to BEA annual totals divided by twelve.

Figure A.4: Validation: BEA Table 3.12 vs Treasury-implied annual totals



Notes: BEA Table 3.12 line items compared to annual totals constructed by summing digitized Treasury monthly flows.

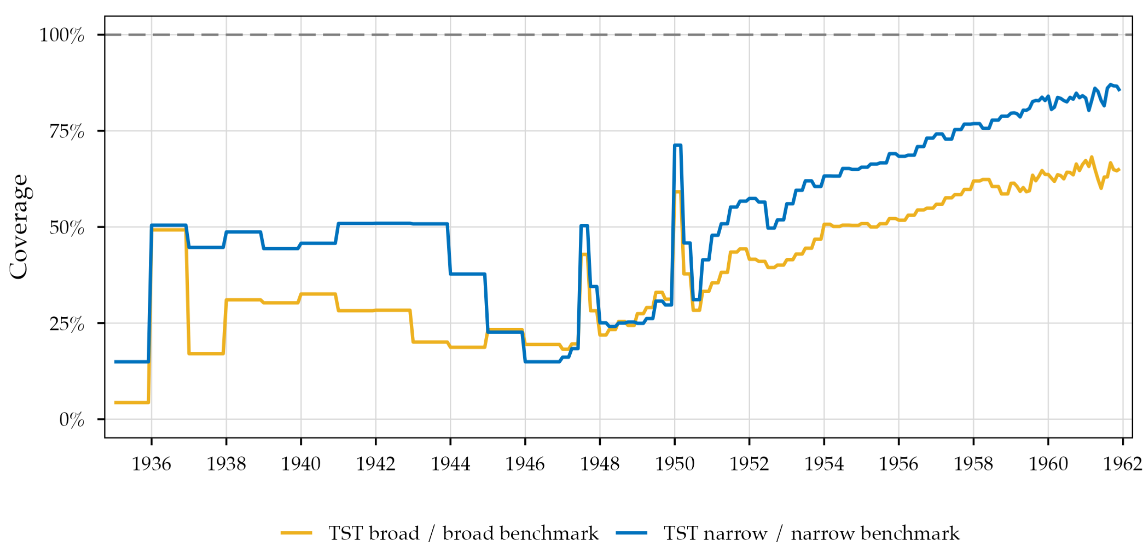
Figure A.5: Validation: monthly Treasury flows vs BEA annual totals divided by twelve



Notes: Treasury monthly flows compared to BEA annual totals divided by twelve.

Coverage. We present coverage ratios, defined as the ratio of seasonally adjusted Treasury-program aggregates to NIPA benchmark totals, in Figure A.6.

Figure A.6: Coverage ratios: Treasury program aggregates relative to NIPA benchmarks



Notes: Ratio of seasonally adjusted Treasury-program aggregates to NIPA benchmark totals (annual benchmarks in 1935–1946, quarterly benchmarks in 1947–1958, monthly benchmarks thereafter). The dashed line at one denotes full coverage.

Specifically, the ratios are defined as:

$$\text{Coverage}_P^{\text{narrow}} = \frac{\widetilde{T}_P^{\text{narrow}}}{B_P^{\text{narrow}}}, \qquad \text{Coverage}_P^{\text{broad}} = \frac{\widetilde{T}_P^{\text{broad}}}{B_P^{\text{broad}}}, \tag{6}$$

where P denotes the benchmark period (annual for 1935–1946, quarterly for 1947–1958, monthly thereafter).

A.2. Retail

This subsection describes the construction of a monthly, seasonally adjusted U.S. retail sales dataset split into *durable* and *non-durable* goods over 1935m1–2025m12. The central challenge is to obtain long-run series that are consistent in levels and growth rates despite changes in survey design, industry classifications (SIC/NAICS), and published aggregates. Accordingly, we combine historical benchmarks from *Survey of Current Business* (SCB) tables and the Romer–Romer historical series with modern Advance Monthly Retail Trade Survey (MARTS) components. Table A.1 summarizes the coverage of each input.

Table A.1: Data sources and coverage for long-run retail sales durables and non-durables

Source block	Coverage used	Role in construction
MARTS NAICS components	1992m1–2025m9	Modern reconstruction of durables/non-durables (sum of components)
MARTS SIC releases	1991m12 and 1992m1	One-month bridge to align regimes (growth-rate link)
Romer–Romer (SCB vintages)	1947m1–1991m12	Postwar benchmark series (vintage-spliced)
Digitised SCB biennial tables	1935m1–1948m1	Early-period backbone (levels), rescaled at overlaps

A second challenge is that, in the modern MARTS era, the historical headline split into "durables" versus "non-durables" is not mechanically available as a single published aggregate. We therefore reconstruct durables and non-durables from detailed NAICS components, following the definition provided in archived MARTS documentation and used also by Breach and Gupta (2024). Table A.2 reports the exact component mapping and the FRED series used.

Table A.2: Aggregation of NAICS retail components into durables and non-durables

Group	NAICS	FRED series	Component label
Durables	441	`rsmvpd`	Motor Vehicle and Parts Dealers
Durables	442	`mrtssm442uss`	Furniture and Home Furnishings Stores
Durables	443	`mrtssm443uss`	Electronics and Appliance Stores
Durables	444	`mrtssm444uss`	Building Material and Garden Equipment and Supplies Dealers
Non-durables	445	`rsdbs`	Food and Beverage Stores
Non-durables	446	`rshpcs`	Health and Personal Care Stores
Non-durables	447	`mrtssm447uss`	Gasoline Stations
Non-durables	448	`mrtssm448uss`	Clothing and Clothing Accessories Stores
Non-durables	451	`rssghbms`	Sporting Goods, Hobby, Musical Instrument, and Book Stores
Non-durables	452	`rsgms`	General Merchandise Stores
Non-durables	453	`rsmsr`	Miscellaneous Store Retailers
Non-durables	454	`rsnsr`	Nonstore Retailers

Notes: Durable-goods retail sales are defined as the sum of NAICS 441–444 components. Non-durable-goods retail sales are defined as the sum of NAICS 445–454 components.

Finally, the end of the Romer–Romer benchmark (1991m12) does not overlap with the start of the modern NAICS-based reconstruction (1992m1) except through a short bridge constructed from digitized MARTS releases. We use the growth rate between 1991m12 and 1992m1 implied by the digitized SIC MARTS values and apply it to the Romer–Romer levels to obtain a consistent 1992m1 benchmark. More generally, we splice the long-run series multiplicatively at a small number of anchor dates to ensure continuity in levels at splice points; Table A.3 lists these anchors and splicing moments.

Table A.3: Splice anchors and bridging operations

Step	Anchor date(s)	Series blocks linked
Regime splice (post-1992)	1992m1	Pre-1992 benchmark ↔ NAICS reconstruction
Vintage splice (Romer–Romer)	1967m1	1984/1991 vintages
Vintage splice (Romer–Romer)	1961m1	1979/1984 vintages
SCB splice	1945m1	Biennial 1947 → biennial 1949
SCB splice	1941m1	Biennial 1947 → Biennial 1942

Notes: At each splice, we preserve within-block growth rates and enforce level continuity at the anchor date by multiplying the earlier block by a constant factor. The bridge month 1991m12–1992m1 uses digitized MARTS releases to carry the benchmark one period forward.

We then rescale the durables and non-durables series so that, at each date, their sum matches the headline retail sales aggregate.

Figure A.7 displays the resulting retail sales series and its durable/non-durable decomposition over 1935–2025.

Retail Sales: Durables and Non-Durables

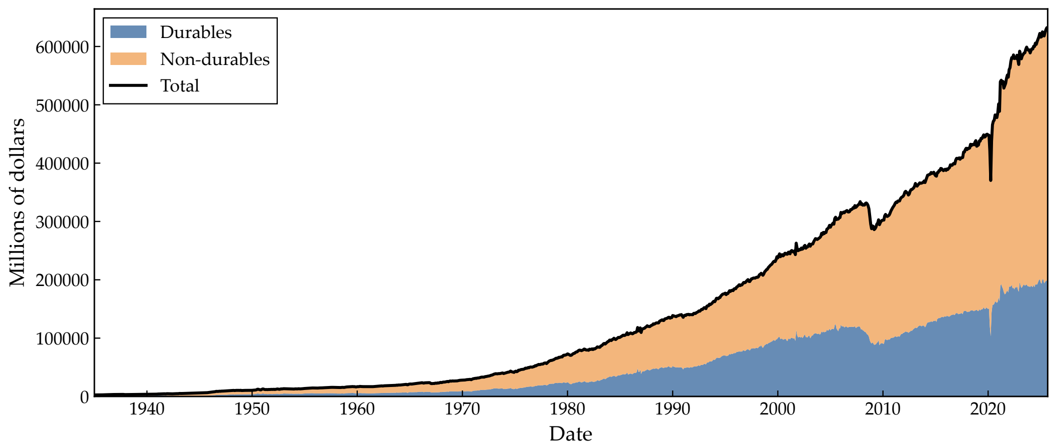


Figure A.7: U.S. retail sales and durable/non-durable decomposition (1935m1–2025m9).

A.3. Other Data Construction

We supplement our data on transfers and retail sales with various monthly indicators. The starting point is various series from FRED. Where monthly data were unavailable in FRED over our full sample, we constructed our own monthly series, supplementing with historical data from various sources. We describe our methods for doing so below, and provide a full list of sources in Tables A.4 and A.5.

Consumption. We use the Personal Consumption Expenditures series from FRED, available at monthly frequency since January 1959. We use our monthly retail sales data to extend this back by interpolating quarterly consumption using the method in Chow and Lin (1971). To do so, we first extend back the quarterly Personal Consumption Expenditures series from FRED, available from 1947, using the consumption series in Antolin-Diaz and Surico (2025). The dataset in Antolin-Diaz and Surico (2025) contains data on (log) real personal consumption per capita from 1890 – we convert this to nominal personal consumption using data on the GDP deflator and population from Ramey and Zubairy (2018).[1] We interpolate the resulting quarterly series over the period 1935-2019, and then use this interpolated series to extend back the monthly series from FRED. The resulting series covers the period 1935M1-2025M12.

We also construct consumption series for durables and non-durables following a similar procedure. We use data from FRED – Personal Consumption Expenditures: Durable Goods and Personal Consumption Expenditures: Nondurable Goods – available at monthly frequency

[1] We use this quarterly consumption series directly in our quarterly local projections from Figure 3b. We describe below the construction of the additional quarterly series in that Figure.

since January 1959. We then use our monthly retail sales series on durable and non-durable spending to extend this back by interpolating the respective quarterly series – available from FRED since 1947 – using the method in Chow and Lin (1971). The resulting series cover the period 1947m1-2025m12.

Consumer prices. We use the monthly consumer price index (CPI) from FRED, available since 1947. We extend this back to 1935 using historical CPI data from Finaeon's Global Financial Database (series: CPUSAM).

We also use data on consumer durables prices from FRED (series: Consumer Price Index for All Urban Consumers: Durables in U.S. City Average), available at monthly frequency since 1955m12 and quarterly frequency from 1935q1. We seasonally adjust the monthly and quarterly series using the X-13 package. We then use the procedure in Chow and Lin (1971) to interpolate the quarterly series over 1938q3-1955q4 using our monthly CPI series alongside data on producer prices for furniture and household durables, available from FRED at monthly frequency since 1926m1 (series: Producer Price Index by Commodity: Furniture and Household Durables). To do so, we first seasonally adjust the producer price series using the X-13 package. We extend back the monthly durables prices series prior to 1955 using our interpolated series.

Interest rates. We use the Federal Funds Effective Rate series from FRED (available since 1954m7) and extend this back using the 3-Month Treasury Bill Secondary Market Rate from FRED (available since 1934m1). We construct real rates by subtracting realized 1-year ahead inflation from the short-term nominal rate.

Additional quarterly variables. We also construct quarterly real GDP by extending back the Real Gross Domestic Product series on FRED, available since 1947, with the real GDP series from Ramey and Zubairy (2018), available since 1890. We construct quarterly investment by extending back the Gross Private Domestic Investment series on FRED, available since 1947, with the investment series from the NBER Macrohistory Data (series names: q10026a and q10026b), available since 1939. We seasonally adjust the latter series using the X-13 package. We construct an estimate of real investment by dividing our resulting series by the GDP-deflator series in Ramey and Zubairy (2018).

Other macroeconomic shocks. We also use data on various other macroeconomic shocks from the previous literature. We use the monthly oil supply news series from Känzig (2021), updated to 2025m06. We use the updated monthly monetary policy shock series from Romer and Romer (2004) provided in the replication materials of Ramey (2016). We use the military news and tax shocks series from Ramey (2011) and Romer and Romer (2010) respectively, both provided by Ramey (2016). Since these two series are only available at quarterly frequency, we construct monthly series by assigning quarterly shocks evenly over each month within a quarter. We use data on temporary and permanent Social Security shocks from the replication package of Romer and Romer (2016).

Table A.4: Monthly macroeconomic series imported from FRED

FRED code	Description	Availability
PAYEMS	All Employees, Total Nonfarm	1939M01–2025M12
FEDFUNDS	Federal Funds Effective Rate	1954M07–2025M12
CPIAUCSL	Consumer Price Index for All Urban Consumers: All Items in U.S. City Average	1947M01–2025M12
INDPRO	Industrial Production: Total Index	1919M01–2025M12
PCE	Personal Consumption Expenditures	1959M01–2025M11
PCEDG	Personal Consumption Expenditures: Durable Goods	1959M01–2025M11
PCEND	Personal Consumption Expenditures: Non-Durable Goods	1959M01–2025M11
CUUR0000SAD	Consumer Price Index for All Urban Consumers: Durables in U.S. City Average	1956M01–2025M12 (1935Q1–1955Q4)
WPU12	Producer Price Index by Commodity: Furniture and Household Durables	1926M01–2025M11
TB3MS	3-Month Treasury Bill Secondary Market Rate, Discount Basis	1934M01–2025M12

Table A.5: Additional Series

Variable name	Source	Description	Availability
CPUSAM	Finaeon Global Financial Database	United States BLS Consumer Price Index Inflation Rate NSA (with GFD Extension)	1900M01-2025M10
pop	Ramey and Zubairy (2018)	Population	1889Q1-2015Q4
rgdp	Ramey and Zubairy (2018)	Real GDP	1889Q1-2015Q4
pgdp	Ramey and Zubairy (2018)	GDP implicit price deflator	1889Q1-2015Q4

Table A.5: Additional Series *(continued)*

Variable name	Source	Description	Availability
rconspc	Antolin-Diaz and Surico (2025)	Log real consumption per capita	1890Q1-2015Q4
PCEC	FRED	Personal Consumption Expenditures	1947Q1–2025Q3
PCDG	FRED	Personal Consumption Expenditures: Durable Goods	1947Q1–2025Q3
PCND	FRED	Personal Consumption Expenditures: Non-Durable Goods	1947Q1–2025Q3
Real_GDP	FRED	Real Gross Domestic Product (series: GDPC1)	1947Q1–2025Q3
GDPI	FRED	Gross Private Domestic Investment	1947Q1–2025Q3
GDPI_nber	NBER Macrohistory Database	U.S. Gross Private Domestic Investment (series: q10026a and q10026b)	1939Q1–1967Q4

A.4. Survey of Consumer Finances (SCF)

This appendix describes the construction of the Survey of Consumer Finances microdata used in the paper. We use the 1951 wave, which reports reference-year 1950 outcomes and includes the special questions on the 1950 G.I. insurance dividend. The unit of observation is the spending unit (roughly equivalent to a household).

The raw input files from the *Survey* are fixed-width card-image files. We parse the card-image records directly and merge cards by spending-unit identifier. The fields used in the paper come from three cards: the sampling weight and World War II veteran status of the spending-unit head; total income, wages and salaries, G.I. dividend receipt, G.I. dividend amount, and the two dividend-use response slots; and liquid-asset components. Liquid assets are the sum of U.S. savings bonds, interest-bearing government bonds, savings accounts, and checking accounts. The survey contains 3,415 spending units. We restrict to households with positive total income, positive non-missing sample weight, and non-missing veteran status. This gives a base sample of 3,373 spending units.

A.5. Narrative Transfer Shock Episodes

In this appendix, we provide more detail on the narrative underlying our transfer shock series. For the Social Security transfer shocks, we largely rely on the narrative analysis in Romer and Romer (2016). That paper provides a detailed account of the legislative changes, motivations, and classification of benefit adjustments, including an extensive narrative appendix. We therefore refer the reader to Romer and Romer (2016) for a comprehensive discussion of these episodes. Table A.6 lists our full set of shocks used in the analysis.

Table A.6: Veterans' and Social Security Payments

Program	First month	Nominal size	Size / GDP	Baseline
World War I bonus payment	Jun. 1936	\$1.8 billion	8.41%	
Armed Forces Terminal Leave	Sep. 1947	\$2.1 billion	3.36%	✓
World War I insurance dividend	Sep. 1949	\$40 million	0.06%	✓
World War II insurance dividend	Jan. 1950	\$2.9 billion	4.3%	✓
World War II insurance dividend	Mar. 1951	\$685 million	0.81%	✓
World War I insurance dividend	Oct. 1953	\$64 million	0.07%	✓
World War I insurance dividend	Apr. 1958	\$32 million	0.03%	✓
Korean War insurance dividend	Dec. 1961	\$60 million	0.04%	✓
World War II insurance accelerated dividend	Jan. 1965	\$200 million	0.11%	✓
Social Security Amendments	Sep. 1965	\$885 million	0.47%	✓
Tax Reform Act	Apr. 1970	\$686 million	0.26%	✓
Social Security Benefit Increase	Jun. 1971	\$1.1 billion	0.40%	✓
Retroactive Veterans' Benefit Payments	Dec. 1974	\$217 million	0.05%	✓
Retroactive Social Security Payments	Nov. 1983	\$442 million	0.05%	✓
Retroactive Social Security Payments	Dec. 1983	\$533 million	0.06%	✓
Retroactive Social Security Payments	Dec. 1984	\$717 million	0.07%	✓
Retroactive Social Security Payments	Jul. 1985	\$475 million	0.04%	✓
Retroactive Social Security Payments	Jul. 1986	\$525 million	0.05%	✓
Retroactive Social Security Payments	May. 1987	\$533 million	0.04%	✓
Retroactive Social Security Payments	Mar. 1988	\$408 million	0.03%	✓
Retroactive Social Security Payments	Mar. 1989	\$517 million	0.04%	✓
Retroactive Social Security Payments	Nov. 1989	\$308 million	0.02%	✓
Senior Citizens' Freedom to Work Act	May. 2000	\$1.6 billion	0.06%	✓
Social Security Fairness Act	Mar. 2025	\$17 billion	0.23%	

Note: This table contains the set of shocks. Nominal sizes are quoted as the total size of the payment, and the first month refers to the first month in which those payments began to be paid out. The fourth column is the total size of the payment as a fraction of quarterly (nominal) GDP for the quarter in which the first payment was made. We use nominal GDP data from FRED, extended back to 1936 using the series in Ramey and Zubairy (2018).

A.5.1. Additional Temporary Social Security Payments

The first additional payment is the Senior Citizens' Freedom to Work Act of 2000, which induced a one-off payment of approximately $1.6 billion (0.06% of GDP).[2] The aim was to repeal the Social Security retirement earnings test for beneficiaries at or above full retirement age. Policymakers claimed that this test penalized older Americans for earning labor income after retirement. For instance, a House Committee stated that the legislation would "*remove work disincentives*" for older Americans (U.S. House of Representatives, Committee on Ways and Means, 2000). The one-off payment arose because the legislation was passed in April, but the repeal was made retroactive to January of the same year. Therefore beneficiaries received reimbursement for benefits that had been withheld earlier in the year.

The second additional payment is the Social Security Fairness Act of 2025. The Act was signed in January, with payments beginning in March.[3] This legislation aimed to undo policies that penalized public servants (e.g. firefighters or teachers) on retirement. For instance, Republican Senator Susan Collins said that the act repealed provisions that "*unfairly penalize*" public servants (United States Congress, 2024). The March 2025 payments arose because the repeal of the policy required retroactive payments, for benefits withheld since January 2024.

A.5.2. Announcement vs Disbursement

To supplement the discussion in the main text, we provide more information on the potential implementation lags between the announcement and disbursement of various payments. Following previous literature on fiscal policy changes – see e.g. Mertens and Ravn (2013) – for legislated changes, we consider the announcement date as the date on which the legislation was enacted. For non-legislated changes, we assess the effective announcement date by consulting various historical documents (e.g. news reports and presidential speeches).

Armed Forces Leave Bond. As discussed in the main text, the Armed Forces Leave Bond payment was initially scheduled to be made in 1951. The timing was changed to September 1947

[2] The size and timing of the payment is discussed in the BEA Personal Income News Release for May 2000: "*The May increase included benefit payments of $19.2 billion at an annual rate, resulting from new legislation the Senior Citizens' Freedom to Work Act of 2000 that eliminated the "retirement earnings test" and allowed social security beneficiaries aged 65 and older to receive full benefits regardless of their earnings*" (Bureau of Economic Analysis, U.S. Department of Commerce, 2000).

[3] There is some uncertainty around the exact timing and size of these payments. The Social Security Administration (SSA) announced on 25th February that it was "beginning to pay retroactive benefits" as part of the Social Security Fairness Act and that "most people...will receive their one-time retroactive payment by the end of March" (Social Security Administration, 2025b). We set the date of first payment as March, which is when the bulk of initial payments began. The SSA's FY 2025 Agency Financial Report reports sending out $17 billion in retroactive payments to eligible beneficiaries as part of the Social Security Fairness Act as of July 2025 (Social Security Administration, 2025a).

following political pressure, with President Truman signing the amendment in July 1947.

January 1950 special dividend. The first mention that a dividend was due appears in President Truman's Annual Budget Message, at the start of 1948 (Truman, 1948). However, as discussed in the main text, the timing and size of the payment remained uncertain for a significant period of time. Clarity around the payment seems to have arrived about six months prior to the start of the disbursements. Following comments from the Veterans Administration, the New York Times reported on 19 June 1949 that the dividend was "set for January," with an expected average payment of $140 (Whitney, 1949).

Korean War insurance dividend. As discussed in the main text, legislation for these payments was signed into law on 13 September 1961. Payments then began the following December.

December 1974 veterans' compensation. These payments were authorized under the Vietnam Era Veterans' Readjustment Assistance Act of 1974, signed by President Ford on 3 December 1974. Payments began in the same month that the bill was enacted (retroactive to September of that year).

Other veterans' payments. There is greater uncertainty around the effective announcement dates of the remaining veterans' payments, although there do not appear to have been any major announcements significantly prior to their disbursements. As discussed in the main text, some discussion of the March 1951 payment appears in *The Oak Leaf* (the in-house newspaper of a U.S. Navy hospital) one month prior to the payment (in February 1951). For the 1953 payment, *The Fayette County Record* reported on Friday 16th October that the Veterans Administration announced the payment on the previous Wednesday with payments beginning on the Thursday (The Fayette County Record, 1953). Similarly, reporting of the 1958 payment appears in *The Brantley Enterprise* on Thursday, April 3, 1958, the same month as payments began (The Brantley Enterprise, 1958). And there does not appear to be any discussion of the January 1965 payment until after it had been disbursed.

Social Security payments. The announcements of the various Social Security payments employed in our main analysis are discussed in Romer and Romer (2016). The three largest Social Security shocks – in September 1965, April 1970 and June 1971 – were all legislated a few months in advance. Legislation was enacted on 30 July 1965 for the September payments, on 30 December 1969 for the April payments, and on 17 March 1971 for the June payments (Romer and Romer, 2016). The various other retroactive Social Security payments were not legislated and so lack an obvious prior announcement date. Where there is mention of these payments in news outlets prior to their disbursement, the implementation lags appear relatively short. For example, Romer and Romer (2016) report a *Miami News* article from 19 October 1983 which discussed the incoming retroactive payments for November and December. The May 2000 payments – which we add to the original shock series from Romer and Romer (2016) – were authorized via the Senior Citizens' Freedom to Work Act, enacted on 7 April 2000.

A.5.3. Endogenous Transfer Series

Additionally, Table A.7 lists the endogenous veterans' payments that we do not include in our baseline series. We describe each of these shocks in more detail below.

Table A.7: Endogenous Temporary Postwar Veterans Payments

Programme	First month	Nominal size	Size / GDP
World War II insurance dividend acceleration	Mar. 1961	$150 million	0.11%
World War II insurance special dividend	Jul. 1961	$218 million	0.15%
World War II insurance special dividend and acceleration	Jan. 1963	$327 million	0.21%
World War II insurance dividend acceleration	Jan. 1964	$234 million	0.14%
World War II insurance dividend acceleration	Feb. 1967	$196 million	0.09%
World War II insurance dividend acceleration	Feb. 1972	$125 million	0.04%
World War II insurance dividend acceleration	Feb. 1975	$167 million	0.04%
World War II insurance dividend acceleration	Feb. 1976	$217 million	0.05%
World War II insurance dividend acceleration	Apr. 1977	$158 million	0.03%
World War II insurance dividend acceleration	Feb. 1992	$375 million	0.02%

Note: This table contains the set of temporary veterans payments that we deem to be endogenous and so remove from our baseline analysis.

March 1961 dividend acceleration. The acceleration of the regular NSLI dividend in March 1961 was explicitly counter-cyclical in motivation. The *Survey of Current Business* records that the payment "*was stepped up this year as an antirecessionary measure and was virtually completed by the end of March,*" with total disbursements of $150 million (U.S. Department of Commerce, Office of Business Economics, 1961). The payment coincided with other stimulative legislation in early 1961, including the Area Redevelopment Act of May 1961.

July 1961 special dividend. The special NSLI dividend paid in July 1961 was explicitly identified by the Kennedy administration as a discretionary stimulus measure. President Kennedy stated that the dividends had been "*speeded up in order to assist the economy*" (Kennedy, 1961). The timing and size of the payment are discussed in the August 1961 BEA Personal Income News Release, which records that the special dividends paid in July totalled $218 million (Bureau of Economic Analysis, U.S. Department of Commerce, 1961). These payments coincided with the Temporary Extended Unemployment Compensation Act of March 1961, as part of the administration's broader response to the 1960–61 recession.

January 1963 special dividend and acceleration. In January 1963, an acceleration of the regular dividend of $237 million and a $90 million special dividend were bundled together as a deliberate stimulus package. A Presidential statement announced that "*all dividends will be paid during the month of January rather than being spread out during the entire year of 1963*" (Kennedy, 1962). Kennedy explicitly stated that the package would "*provide a needed boost to the economy.*" Both payments therefore fail our criterion for exogeneity, having been timed in direct response to economic conditions.

January 1964 dividend acceleration. The *Survey of Current Business* records that an "*accelerated dividend payment to holders of GI life insurance—$234 million—was also scheduled to start in January*" (U.S. Department of Commerce, Office of Business Economics, 1964). This acceleration was similarly framed as deliberate fiscal stimulus. The *Report of the Joint Economic Committee* described it as "*additional stimulus*" (Joint Economic Committee, 1964). The acceleration coincided directly with the Revenue Act of 1964—the Kennedy–Johnson tax cut—signed the following month.

February 1967 dividend acceleration. The February 1967 acceleration coincided with a cyclical slowdown and a number of other fiscal stimulus measures. The *Survey of Current Business* records that "*large initial payments*" were made in February and March, and that "*during March and early April, the administration and the monetary authorities took a number of actions of a stimulative nature*" (U.S. Department of Commerce, Office of Business Economics, 1967). The size of the payment is reported by the *Herald Progress*, which records that "*about $196 million will be paid to some 4.2 million holders of NSLI policies*" (Herald Progress, 1967).

Other dividend accelerations. The timing and size of the remaining NSLI dividend accelerations are discussed in the BEA Personal Income News Releases around the time of each transfer (Bureau of Economic Analysis, U.S. Department of Commerce, 1972, 1975, 1976, 1977, 1992). Each of these dividend accelerations coincided with recessionary periods and other expansionary fiscal measures. The February 1972 acceleration closely followed the Revenue Act of 1971, signed by President Nixon in December 1971, which provided significant stimulus in the form of tax cuts. The February 1975 acceleration coincided with the Tax Reduction Act of 1975 which provided tax rebates explicitly targeted at countering the 1974–75 recession. The February 1976 acceleration coincided with ongoing Congressional pressure for countercyclical public works spending, which culminated in the Public Works Employment Act of 1976, enacted in July 1976. The April 1977 acceleration coincided with the passage of President Carter's Economic Stimulus

Appropriations Act of 1977 in response to lingering high unemployment following the 1974–75 recession. Finally, the February 1992 acceleration occurred during the "jobless recovery" that followed the 1990–91 recession, coinciding with the ongoing extension of emergency unemployment compensation under the Emergency Unemployment Compensation Act of 1991 and its 1992 amendments. In each case, the coincidence with recessionary conditions and other fiscal stimulus leads us to treat these accelerations as endogenous.

B. Additional Empirical Results and Sensitivity

This appendix reports additional econometric and empirical results not presented in the main text and provides an extensive set of robustness checks for our baseline empirical findings.

B.1. Identification With Binary Instruments

We start by providing more detail on our baseline local-projection specification (1) with binary instruments. Specifically, we characterize the estimand of the baseline regression when Social Security and veterans' payments may have heterogeneous treatment effects.

Environment. As in the baseline, we assume that the economy admits a structural vector moving-average representation: $\mathbf{y}_t = \sum_{l=0}^{\infty} \boldsymbol{\theta}_l \boldsymbol{\varepsilon}_{t-l}$, where $\mathbf{y}_t$ is a vector of macroeconomic variables, ε_t is a vector of mean-zero mutually uncorrelated structural shocks driving the economy, and θ_l capture the dynamic causal effects. We set $\mathbf{y}_t = [y_t, T_t, w_t]$ where y_t is a scalar outcome of interest (e.g. consumption), T_t denotes transfers to households, and w_t is a vector collecting all other variables. We now suppose there are two underlying shocks of interest ε_t^s and ε_t^v–a transfer shock targeted to Social Security beneficiaries and veterans respectively. We normalize both shocks to imply a one-unit on-impact increase in transfers: $\theta_0^{T,s} = \theta_0^{T,v} = 1$.

Identification with binary instruments. We assume we have two binary instruments z_t^s and z_t^v, each a valid proxy for ε_t^s and ε_t^v respectively. Specifically, we assume:

- Relevance: $\mathrm{Cov}(z_t^s, \varepsilon_t^s) \neq 0$ and $\mathrm{Cov}(z_t^v, \varepsilon_t^v) \neq 0$
- Contemp. Exogeneity: $\mathrm{Cov}(z_t^s, \boldsymbol{\varepsilon}_t^{-s}) = \mathrm{Cov}(z_t^v, \boldsymbol{\varepsilon}_t^{-v}) = 0$
- Lead/lag exogeneity: $\forall j \neq 0 : \mathrm{Cov}(z_t^s, \boldsymbol{\varepsilon}_{t-j}) = \mathrm{Cov}(z_t^v, \boldsymbol{\varepsilon}_{t-j}) = \mathbf{0}$

where $\boldsymbol{\varepsilon}_t^{-s}$ and $\boldsymbol{\varepsilon}_t^{-v}$ collect all other structural shocks, other than the Social Security and veterans transfer shocks respectively. We sum the two instruments to form: $z_t = z_t^s + z_t^v$, and consider pairs of local projections of the form:

$$y_{t+h} = \alpha_{y,h} + \beta_{y,h} z_t + u_{y,t,h}$$

$$T_{t+h} = \alpha_{T,h} + \beta_{T,h} z_t + u_{T,t,h}$$

It is straightforward to show that under these assumptions we have:

$$\beta_{y,h} = \left(\omega_s \theta_h^{y,s} + \omega_v \theta_h^{y,v}\right) \beta_{T,0} \tag{7}$$

where $\omega_s + \omega_v = 1$. In this case, our local projection for the outcome variable of interest recovers a weighted average of the impulse responses to a transfer shock to Social Security beneficiaries and a transfer shock to veterans, scaled by the impact effect of our instrument on transfers. Further, for $g \in \{s, v\}$, the weights satisfy:

$$\omega_g \propto V_g \Delta_g$$

where $V_g = \text{Var}(z_t^g)$ and $\Delta_g = E[T_t \mid z_t^g = 1] - E[T_t \mid z_t^g = 0]$. The variance term is increasing in the number of times that the binary event takes place, provided that this number is small. Specifically, if $P_g = P(z_t^g = 1)$, the variance is $P_g(1 - P_g)$. Therefore the weights depend on two forces: the frequency of the event type, and the average increase in transfers associated with the events. These weights are straightforward to estimate. Since we have relatively more Social Security events in our sample, $\hat{V}_s > \hat{V}_v$. However, since our veterans series is generally associated with a much larger response of transfers, $\hat{\Delta}_v > \hat{\Delta}_s$. As we discuss below, on net, we find the second effect dominates, with relatively larger weight assigned to the veterans shocks overall.

Identification with size-scaled instruments. We now consider instead using an instrument that captures the size of the shock. Specifically, for $g \in \{s, v\}$, we consider the alternate instruments: $q_t^g = z_t^g \cdot d_t^g$, where d_t^g is the size of the increase in transfers associated with each event. This generally requires stronger assumptions: for the proxies to be valid we require that the size d_t^g, rather than just the timing z_t^g, is exogenous. This assumption could be violated for a number of reasons. For example, the exact size of the surplus built up by the Veterans Fund at each point likely reflected at least in part underlying macroeconomic conditions over the previous years.

As well as requiring stronger identifying assumptions, this approach would also further upweight events with larger effects on transfers. In this case, it is straightforward to show that, for local projections on $q_t = q_t^s + q_t^v$ instead of z_t, the decomposition (1) remains valid, with weights proportional to: $\text{cov}(T_t, q_t^g)$. To provide some intuition for how these weights depend on the size of each event, suppose the size-scaling d_t^g captures exactly the contemporaneous rise in transfers associated with the transfer shock at time t: $d_t^g = \theta_0^{T,g} \varepsilon_t^g$. In this case mean-independence of the instrument with other shocks implies: $\text{cov}(T_t, q_t^g) = E[(q_t^g)^2]$. And so the weights satisfy:

$$\omega_g \propto P_g D_g^2$$

where $P_g = P(z_t^g = 1)$ and $D_g^2 = E[(d_t^g)^2 \mid z_t^g = 1]$. In this case, the weights depend on the relative probability of each event as well as the squared size of the increase in transfers associated with each event.

Estimating the weights. The ω_g-weights for various specifications are straightforward to estimate in our data. When the regression includes additional controls $\boldsymbol{x}_t$, the implicit weighting on each impulse response is given by the covariance between T_t and the instrument orthogonalised with respect to $\boldsymbol{x}_t$. We thus estimate weights using the instrument orthogonalised with respect to our baseline set of controls. For the binary instrument, we find the weights are slightly tilted towards the veterans shocks: $\omega_v \approx 0.55$. With the size-scaled instrument we instead find $\omega_v \approx 0.65$. Further decomposing this, we find this 65% loading weighs almost entirely on just two events: the largest veterans payments in 1947 and 1950.

B.2. Instrument Diagnostics

We perform a series of validity checks on our narrative transfer shock series. Specifically, we examine its serial correlation properties and its predictability based on past macroeconomic conditions.

A key requirement for our identification strategy is that transfer shock dates are not systematically related to prior economic conditions. To assess this, we test whether the occurrence of transfer shocks can be forecast using lagged macroeconomic variables. Table B.1 presents a series of Granger causality tests using a wide range of macroeconomic and financial predictors. We find no evidence that past macroeconomic and financial indicators predict the timing of transfer shocks, consistent with the view that these events are not driven by the business cycle.

We also examine whether the narrative transfer shocks are correlated with other sources of macroeconomic fluctuations. In particular, we consider military spending shocks, tax shocks, monetary policy shocks, and oil supply shocks. Table B.2 shows the transfer shock series is uncorrelated with these alternative shock measures, supporting the interpretation that it captures an independent source of variation in disposable income.

Finally, we analyze the time-series properties of the shock series itself. Figure B.1 shows the autocorrelation function of the narrative transfer shock series. The narrative transfer shocks

Table B.1: Granger causality test

Variable	p-value
Transfer shock	0.783
Transfers	0.376
Industrial production	0.198
Retail sales	0.860
Employees nonfarm	0.510
Unemployment rate	0.274
Personal consumption expenditure	0.564
Consumer price index	0.542
Commodity price index	0.966
WTI crude	0.861
Short rate	0.724
Overall	1.000

Notes: p-values of a series of Granger causality tests of plausibly exogenous transfer shock dates using a selection of macroeconomic and financial variables.

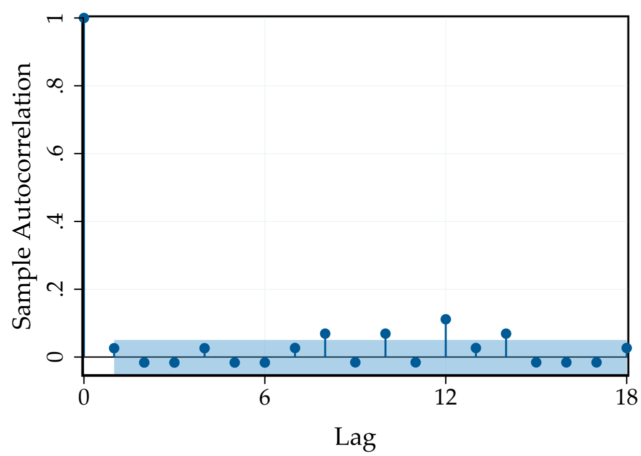
Table B.2: Correlation with Other Shock Measures

Shock	Description	Source	ρ	p-value	n	Sample
Government spending	Military news	Ramey, 2011	-0.03	0.32	825	1947m1– 2015m9
Tax	Narrative shock	Romer and Romer, 2010	0.01	0.75	756	1945m1–2007m12
Monetary policy	Narrative shock	Romer and Romer, 2004	0.03	0.52	468	1969m1–2007m12
Oil supply	Oil supply news	Känzig, 2021	-0.04	0.32	606	1975m1– 2025m6

Notes: Correlation coefficients of the narrative transfer shock series with a wide range of different shock measures from the literature, including fiscal policy, monetary policy, and oil shocks. ρ is the Pearson correlation coefficient, the p-value corresponds to the test whether the correlation is different from zero and n is the sample size.

exhibit some degree of serial correlation, reflecting the fact that several episodes involve disbursements that occur over consecutive months. However, this feature is largely mechanical given the dummy nature of the shock variable. In our empirical specifications, we account for this by including lags of the narrative transfer shock, ensuring that the estimated responses are not driven by serial dependence.

Figure B.1: Autocorrelation function



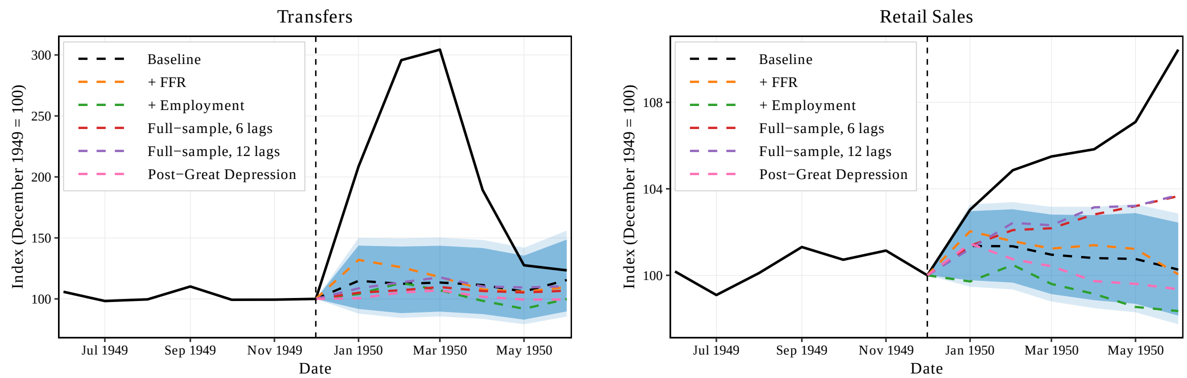
Notes: Autocorrelation function of narrative transfer shock series with 95% confidence bands.

B.3. Alternative Forecasts from the 1950 Case Study

This appendix reports forecasts for the period Jan. 1950-June 1950 from alternative VAR-specifications. The baseline VAR in Figure 7 is estimated over 1945-49, and includes six lags each of: our narrow transfer series, retail sales, industrial production, and the consumer price index (all in logs).

In Figure B.2, we consider various perturbations to this baseline. First, we consider a five-variable VAR which additionally includes the Fed Funds Rate (yellow line), as well as a five-variable VAR which additionally includes (log) employment (green line). We also vary the sample over which the baseline four-variable VAR is estimated. We present results from the baseline VAR specification estimated on a longer sample, starting after the Great Depression (1940), shown in the pink line. We also consider pseudo-out-of-sample forecasts from a VAR estimated over the same sample as the local projection (1945-2019) – both with six lags (red line), as well as 12 lags (purple line). In all cases we find that the observed data for both transfers and retail sales lie well above the estimated forecast.

Figure B.2: The Economy around the January 1950 Veterans' Payment



Notes: The figure shows the evolution of transfers and real retail sales in a six-month window around the January 1950 veterans' payment. The vertical dashed line indicates the timing of the payment. The black dashed line shows the forecast from a four-variable VAR estimated on pre-event data from 1945–1949, and the shaded areas represent the 90 and 95% bootstrap forecast intervals. Other dashed lines represent forecasts from alternative VAR specifications discussed in this subsection.

B.4. MPCs from the 1950 Case Study

This appendix studies how we estimate MPCs from the 1950 case study, as in Figure 8 from the main text. The dataset is the 1951 *Survey of Consumer Finances*, which asks about the 1950 G.I. insurance dividend. The unit of observation is the spending unit. The relevant variables record whether the spending unit received the payment, how much it received, and how the respondent said the money was used. Appendix Section A.4 describes the data construction.

The G.I. dividend questions are: "Did you (or anyone in your spending unit) receive a dividend on G.I. insurance in 1950?"; "How much did you get?"; and "What did you do with it? (2 responses)". The use question records up to two coded answers. We use 289 positive-dividend recipients with positive weights and positive dividend amounts. Of these, 277 have at least one substantive use response. The remaining 12 recipients have only a missing or not-ascertained use response and are excluded from the use calculation.

The use responses give categories, not dollar amounts. We therefore bound the fraction of the payment allocated to each broad use. Let i index recipient spending units in the use sample, let p_i be the survey weight times the dividend amount, and let S_i be the set of broad use categories reported by the spending unit. For broad category C, we compute

$$\underline{m}_C = \frac{\sum_i p_i \mathbf{1}\{S_i = \{C\}\}}{\sum_i p_i}, \qquad \overline{m}_C = \frac{\sum_i p_i \mathbf{1}\{C \in S_i\}}{\sum_i p_i}. \tag{8}$$

The lower bound assigns none of the payments with multiple broad uses to category C; the upper bound assigns all such payments to category C. If both raw responses map into the same broad category, the category is counted only once. The denominator, $\sum_i p_i$, is 1,511,040 in survey-weighted dividend dollars.

Table B.3 gives the mapping from the original use codes to the three broad categories. We classify general living expenses, medical expenses, luxuries, education, moving, gifts, consumer durables, repairs of consumer durables, and unspecified spending as spending. We classify bill repayment, mortgage and business debt repayment, investment, savings bonds, bank deposits, and farm operating expenses as saving and investment. Taxes, house repairs, unknown responses, the errata-listed code 53, and other uses are classified as other.

Table B.3: Mapping of G.I. dividend-use responses

Code	Survey category	Broad category
11	To pay back bills	Saving and investment
12	General living expenses	Spending
13	Hospital, medical expenses, illness, death	Spending
21	Repairs, additions to house	Other
22	Luxuries, travel, amusement	Spending
23	Education	Spending
24	Moving, storage	Spending
25	Taxes	Other
26	Gifts, charity, donations	Spending
31	Consumer durables	Spending
32	Repair of consumer durables	Spending
41	Payment of mortgage or business debts	Saving and investment
42	Investment in real estate	Saving and investment
43	Investment in business	Saving and investment
44	Investment in securities or other investment	Saving and investment
51	Bought savings bonds, Series E	Saving and investment
52	Put money in bank/building-and-loan/credit union/postal savings	Saving and investment
53	Errata-listed code 53, not in main printed list	Other
54	Spent it, NA for what or how	Spending
61	Farm operating expense, farm machinery	Saving and investment
99	Don't know	Other
YY	Other uses	Other

Table B.4 reports the resulting bounds. The spending share is between 0.420 and 0.503. Saving and investment account for 0.406–0.491, and other uses account for 0.078–0.103. The main-text Figure 8 plots the same bounds.

Table B.4: Bounds on G.I. dividend uses

Broad category	Lower bound	Upper bound
Spending	0.420	0.503
Saving and investment	0.406	0.491
Other	0.078	0.103

Notes: Bounds are computed from 277 positive-dividend recipient spending units with at least one substantive use response. Counts are weighted by survey weight times dividend amount. Twelve positive-dividend recipients whose only observed use response is `XX/-` are treated as missing and excluded.

As a separate check on the relationship between the dividend and household resources, we estimate a weighted log-log regression of the dividend amount on pre-dividend total income among positive-dividend recipients with positive pre-dividend income below the weighted 99th percentile of the 1950 base sample. The estimated elasticity is 0.088, with a standard error of 0.063 and 280 observations. Thus, in this sample, dividend amounts are only weakly related to pre-dividend income.

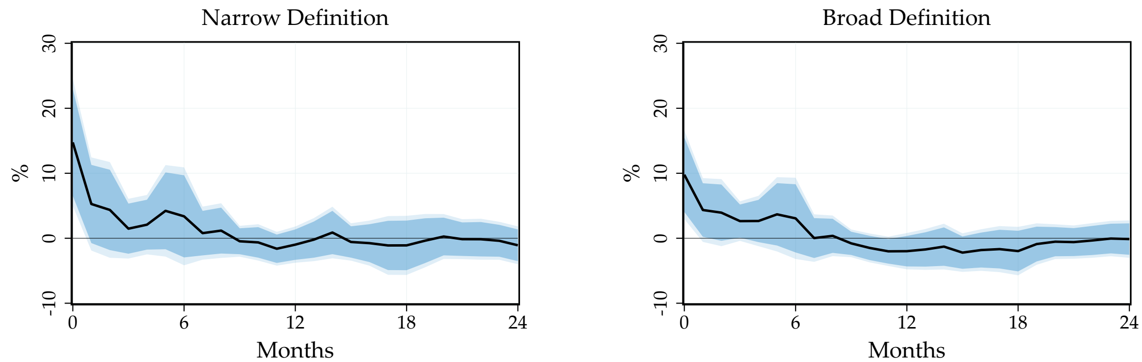
B.5. Additional Evidence

This appendix reports additional results that complement the baseline findings in the main text.

B.5.1. Additional Results From the Time Series

Figure B.3 examines the behavior of transfers under alternative definitions. The response of the narrow transfer measure displays a sharp and transitory increase following the shock. The broad measure exhibits a similar pattern, albeit somewhat attenuated, reflecting the inclusion of components that are less directly affected by the identified transfer episodes. Expressing transfers relative to trend consumption yields a comparable dynamic profile, with an increase of around one percentage point on impact that gradually unwinds. These results confirm that our characterization of the transfer shock is robust to alternative definitions of the transfer variable.

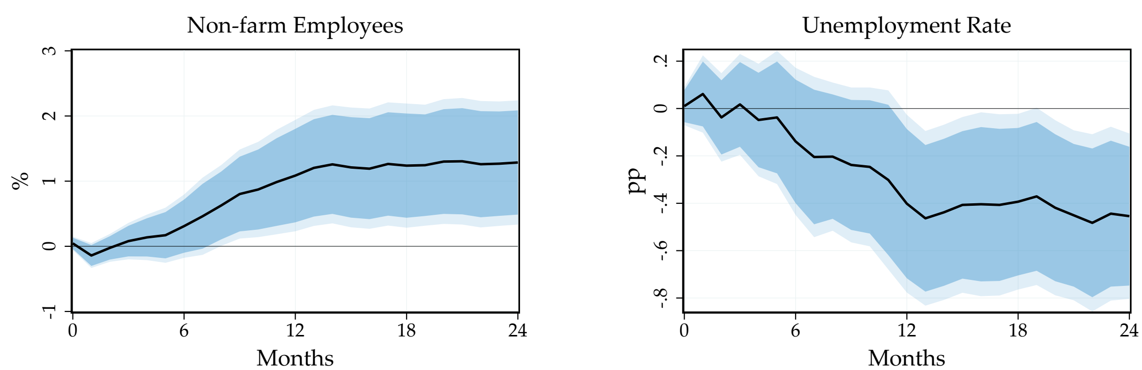
Figure B.3: Transfer Responses



Notes: Impulse responses of the narrow and broad government transfer series to a temporary transfer shock, estimated using the local projections specification (1) using the exogenous transfer shock dates. The narrow measure includes Social Security, railroad retirement, and government life insurance-related disbursements; the broad measure additionally includes unemployment insurance. Lines are point estimates; blue shaded areas are 90% and 95% confidence bands. Sample: 1945m10–2019m12.

We next consider labor-market responses to temporary transfer shocks. Figure B.4 reports impulse responses of non-farm employment and the unemployment rate. Non-farm employment rises following the shock, while the unemployment rate declines steadily over the same horizon.

Figure B.4: Employment Responses



Notes: Impulse responses of labor market variables (number of non-farm employees, unemployment rate) to a temporary transfer shock, estimated using the local projections specification (1) using the exogenous transfer shock dates. Lines are point estimates; blue shaded areas are 90% and 95% confidence bands. Sample: 1945m10–2019m12.

B.5.2. Endogenous Versus Exogenous Shocks

As discussed in the main text, we benchmark our baseline results against responses obtained from plausibly endogenous transfer episodes. We first focus on stimulus checks, which provide the starkest comparison: they are large, highly visible transfer payments, but their timing is closely tied to prevailing macroeconomic conditions; see Figure B.5a.

We then broaden the comparison by adding other endogenous or likely endogenous transfer episodes. Specifically, we construct an alternative transfer shock series consisting of (i) a temporary Social Security payment identified as endogenous by Romer and Romer (2016), (ii) veteran-related transfer episodes that we classify as endogenous or likely endogenous, as well as (iii) major stimulus check programs, including the 2001 and 2008 rebates as well as the COVID-era payments.

Figure B.5b compares the responses obtained from our baseline exogenous transfer shock dates to those generated by the broader endogenous shock dates. While the endogenous shocks still generate a short-run increase in transfers, the macroeconomic responses differ markedly from those implied by the exogenous episodes. Industrial production shows essentially no response, and retail sales, if anything, tend to fall after the payment. Personal consumption expenditures also show little movement on impact and only rise modestly at longer horizons; however, this increase appears to reflect a pre-trend rather than a clear response to the transfer shock. Overall, the endogenous episodes provide little evidence of a meaningful expansion in aggregate activity. This stands in sharp contrast to the positive and persistent effects obtained using the exogenous transfer shocks. The muted responses likely reflect the fact that many of the endogenous episodes occur in response to weak macroeconomic conditions or are relatively small in magnitude, and therefore contain limited signal about aggregate demand effects.

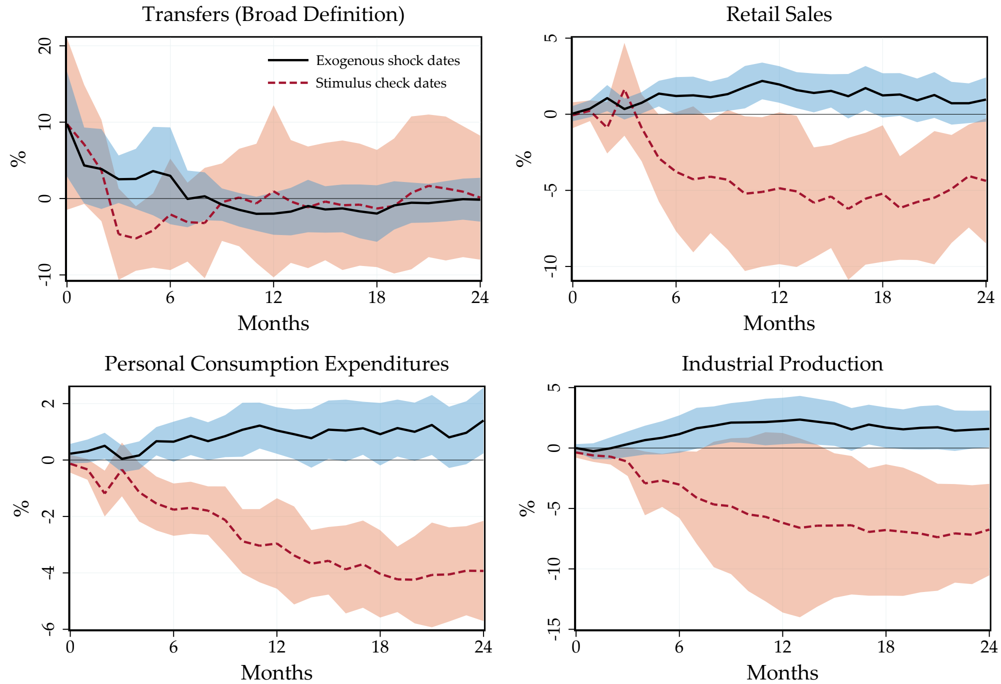
B.5.3. Size Dependence

We next ask whether the macroeconomic effects differ between smaller and larger transfer shocks. This exercise is necessarily demanding: the baseline shock series contains only a limited number of event months, so splitting the sample by size substantially reduces the identifying variation available for each response. We therefore interpret the estimates as suggestive evidence on size dependence rather than as a tightly powered test of heterogeneity. To implement the exercise, we split the narrative shock series at the median real size of the transfer payment among event months. Let q_t denote the real size of the payment associated with a transfer episode, and let $\bar{q}$ denote the median of q_t across the event months in the baseline sample. We define two mutually exclusive shock indicators:

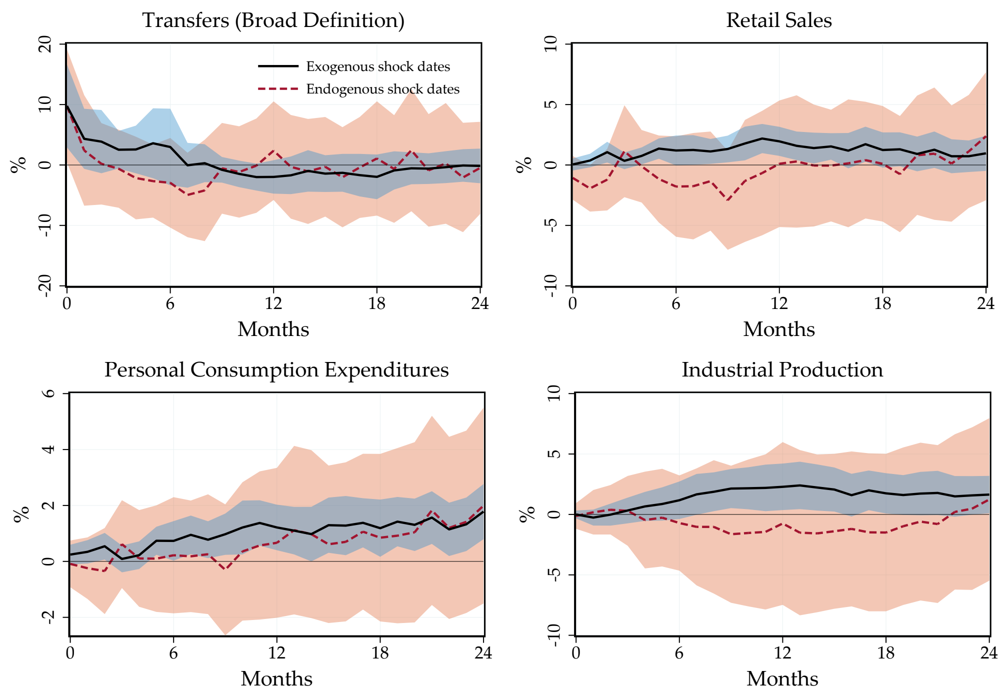
$$z^S_{T,t} = z_{T,t}\mathbf{1}\{q_t < \bar{q}\}, \qquad z^L_{T,t} = z_{T,t}\mathbf{1}\{q_t \geq \bar{q}\},$$

Figure B.5: Endogenous Versus Exogenous Transfer Shocks

(a) Stimulus checks



(b) All endogenous shocks



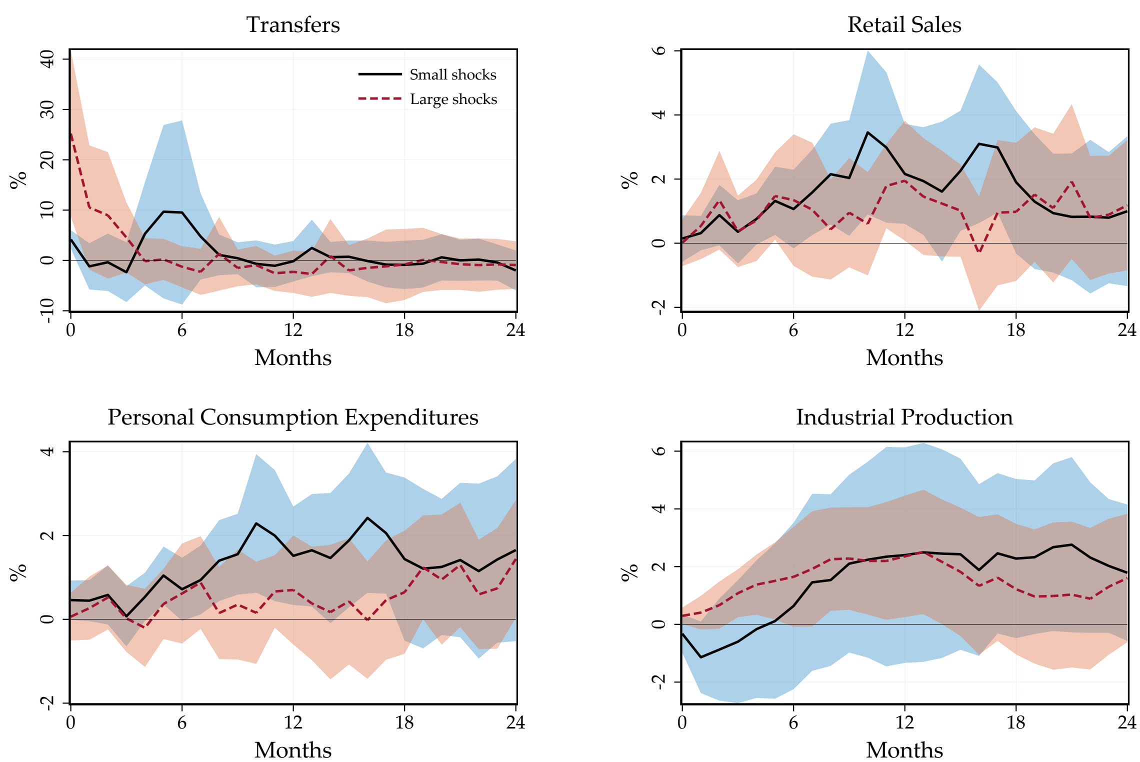
Notes: Impulse responses to exogenous and endogenous transfer shocks. We compare the responses when using endogenous transfer shock episodes to estimate (1) to our baseline responses using the exogenous transfer shock dates. Panel a focuses on stimulus checks; Panel b considers all endogenous transfer shock episodes. Endogenous shocks are scaled such that the impact response of transfers (broad definition) matches that from the exogenous shock responses. Lines are point estimates; shaded areas are 95% confidence bands. Sample: 1945m10–2019m12.

where $z^S_{T,t}$ captures smaller shocks and $z^L_{T,t}$ captures larger shocks. We chose $\bar{q}$ as the median transfer payment across our shock episodes. For each horizon $h = 0, \ldots, H$, we then estimate

$$y_{t+h} = \alpha_h + \theta^S_h z^S_{T,t} + \theta^L_h z^L_{T,t} + \boldsymbol{\beta}'_h \mathbf{x}_{t-1} + \nu_{t+h}, \tag{9}$$

where $\mathbf{x}_{t-1}$ contains the same lagged macroeconomic controls as in the baseline specification (1): 12 lags of the outcome variable, industrial production, producer prices, the 3-month Treasury bill rate, and broad real transfers as well as 12 lags of the small and large transfer shocks. The coefficients θ^S_h and θ^L_h trace out the dynamic responses to smaller and larger transfer shocks, respectively.

Figure B.6: Size Dependence



Notes: Impulse responses to small and large temporary transfer shocks, estimated using the local-projection specification (9). Smaller shocks are episodes with a real payment size below the median among baseline transfer-shock months; larger shocks are episodes at or above the median. Responses are scaled so that the cumulative transfer response over the plotted horizon matches the baseline cumulative transfer response. Lines are point estimates; shaded areas are 95% confidence bands. Sample: 1945m10–2019m12.

Figure B.6 reports the resulting impulse responses. Since larger transfer episodes mechanically generate larger increases in transfers, the figure scales the smaller- and larger-shock responses so that the cumulative transfer response over the plotted horizon matches the baseline cumulative transfer response. This normalization makes the responses comparable per unit of transfer impulse. The estimates are noisy, as splitting the shock series leaves limited identifying variation in each group, so we interpret the comparison as suggestive. Qualitatively, the responses are similar across the two groups. Quantitatively, smaller shocks are associated with somewhat larger responses, particularly for retail sales and personal consumption expenditures. This pattern is broadly consistent with Bianchi and Kaplan (2026), who show in calibrated HANK models that the aggregate consumption response to fiscal transfers is concave in the size of the transfer.

B.5.4. Regional Evidence

A complementary way to assess the economic effects of temporary transfer shocks is to exploit cross-state variation in exposure to the payments. In particular, veterans' payments should generate larger income shocks in states with a higher share of veterans, while temporary Social Security payments had larger effects in states with a greater share of Social Security recipients. This regional variation provides an additional source of identification and allows us to examine whether areas more exposed to the transfer shocks experienced stronger employment responses.

We begin with a simple panel local projections specification that mirrors our aggregate time-series analysis. Specifically, for each horizon $h = 0, \ldots, H$, we estimate

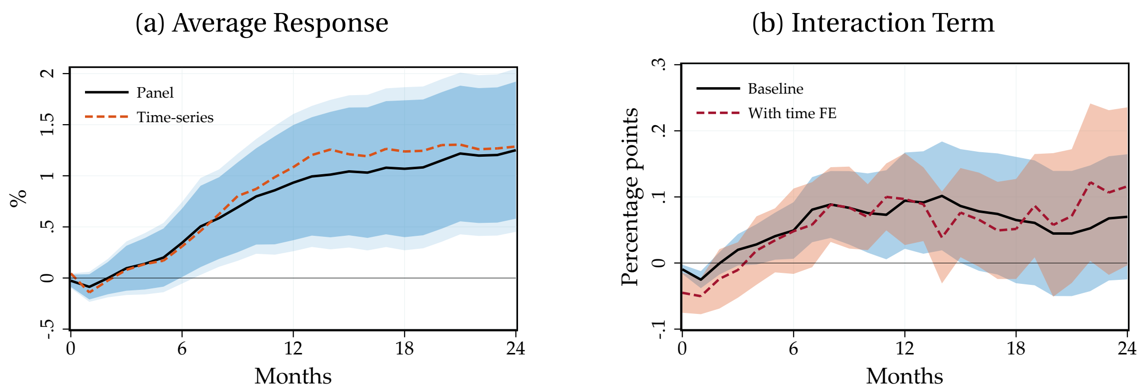
$$y_{i,t+h} = \alpha_{i,h} + \theta_h z_{T,t} + \mathbf{x}'_{i,t-1}\boldsymbol{\gamma}_h + \mathbf{x}'_{t-1}\boldsymbol{\beta}_h + v_{i,t+h}, \tag{10}$$

where $y_{i,t+h}$ denotes log employment in state i at horizon h, $z_{T,t}$ is the narrative transfer shock indicator, and $\mathbf{x}_{t-1}$ and $\mathbf{x}_{i,t-1}$ are aggregate and state-level controls, respectively. The specification includes state fixed effects, the sum of the state-level veterans' and Social Security recipient shares, 12 lags of state-level employment, and the same set of aggregate macroeconomic controls used in the aggregate specification in equation (1): 12 lags of industrial production, prices, interest rates, transfers, and the transfer shock. Standard errors are clustered by the time dimension.

Figure B.7a reports the resulting impulse responses and compares them to the aggregate time-series estimates from the main text. Reassuringly, the two approaches deliver comparable responses. In both specifications, employment initially responds little on impact, but gradually rises over subsequent months, reaching an increase of roughly 1 percent after one year. These results imply that the average effect across states is comparable to the aggregate U.S. effect. Moreover, the fact that the state-level employment responses add up closely to the aggregate response illustrates the consistency between the regional and aggregate employment data.

Next, we exploit heterogeneity in regional exposure to the transfer shocks. We proxy for the

Figure B.7: Regional Employment Responses



Notes: Impulse responses of state-level employment to a temporary transfer shock, estimated using panel local projections (10). Panel a compares the average response from the panel to the aggregate response from the time series. Panel b shows the interaction effects from the heterogeneous-exposure specification (13). Exposure allocates the real size of each national transfer shock across states in proportion to the relevant recipient shares. Lines are point estimates; shaded areas are 90 and 95 percent confidence bands. Sample: 1948m6–2019m12.

exposure as follows. Let q_t denote the real size of the transfer shock, obtained by deflating the nominal transfer-size shock by the CPI. For a veterans episode, we define

$$s_{i,t} = \frac{\omega^v_{i,t}}{\sum_j \omega^v_{j,t}} q_t, \tag{11}$$

where $\omega^v_{i,t}$ is the smoothed veterans share in state i. For a Social Security episode, we analogously use the smoothed Social Security recipient share $\omega^s_{i,t}$. The exposure measure is set to zero outside transfer-shock months. It can be interpreted as a proxy for the real amount of the aggregate transfer shock allocated to state i. This regional variation provides an additional source of identification and allows us to examine whether areas more exposed to the transfer shocks experienced stronger employment responses.

To facilitate interpretation, we center exposure at its average during transfer episodes and express exposure differences in standard-deviation units. Specifically, in transfer-shock months we define

$$\widetilde{s}_{i,t} = \frac{s_{i,t} - \bar{s}}{\sigma_s}, \tag{12}$$

where $\bar{s}$ is the mean exposure across observed state-months during transfer episodes, and σ_s is the pooled within-month cross-state standard deviation of exposure.

With this exposure measure at hand, we then estimate the following specification

$$y_{i,t+h} = \alpha_{i,h} + \theta_h z_{T,t} + \varphi_h \left(z_{T,t} \times \widetilde{s}_{i,t} \right) + \mathbf{x}'_{i,t-1} \boldsymbol{\gamma}_h + \mathbf{x}'_{t-1} \boldsymbol{\beta}_h + v_{i,t+h}. \tag{13}$$

The coefficient φ_h thus captures whether states with greater exposure to the transfer shocks experienced systematically stronger employment responses. Figure B.8 illustrates the underlying cross-state variation in exposure. Panel a plots the average veterans exposure across states, while Panel b reports the average Social Security recipient exposure. There is substantial geographic heterogeneity in both measures, providing meaningful variation for identification. States in the West and parts of the South generally exhibit higher exposure to veterans' payments, while Social Security exposure is particularly high in Florida, the Northeast and the West. In addition to the cross-sectional heterogeneity, both measures also display meaningful time variation over the sample period.

Because identification now comes from differential exposure across states, we can additionally include time fixed effects $\lambda_{t,h}$, thereby absorbing all aggregate shocks and common macroeconomic dynamics:

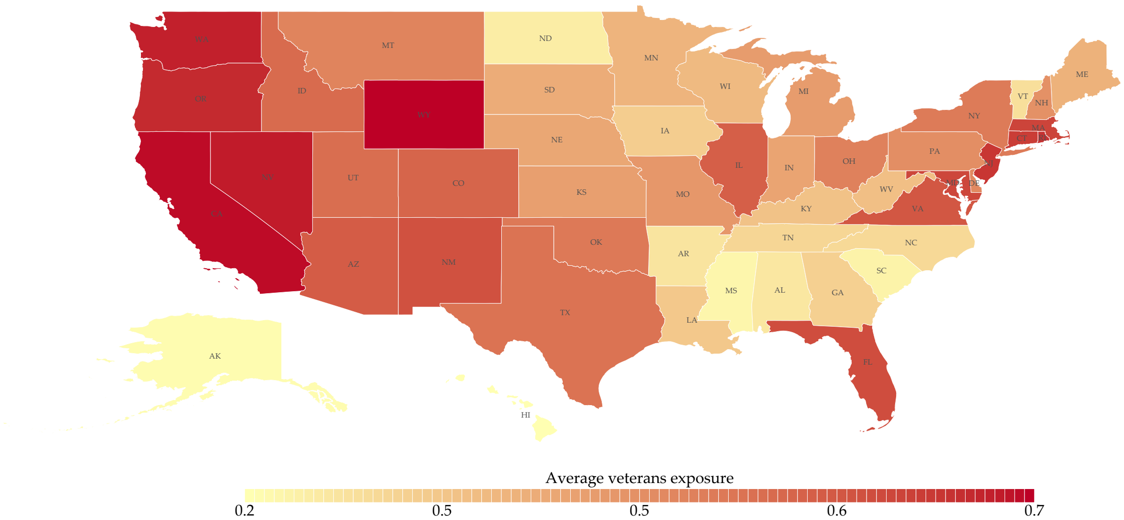
$$y_{i,t+h} = \alpha_{i,h} + \lambda_{t,h} + \varphi_h \left(z_{T,t} \times \widetilde{s}_{i,t} \right) + \mathbf{x}'_{i,t-1} \boldsymbol{\gamma}_h + v_{i,t+h}. \tag{14}$$

Figure B.7b presents the estimated response. The estimates point to economically meaningful heterogeneity across states. The shape of the interaction response closely mirrors the baseline effect, with employment gradually increasing over time and peaking after roughly one year. Quantitatively, a one standard deviation increase in a state's exposure to transfers raises the employment response by approximately 0.1 percent at peak. Importantly, these results remain very similar even after including time fixed effects, which absorb all aggregate macroeconomic fluctuations and common national shocks. This suggests that the aggregate controls included in the baseline specification do a good job capturing these common macroeconomic factors.

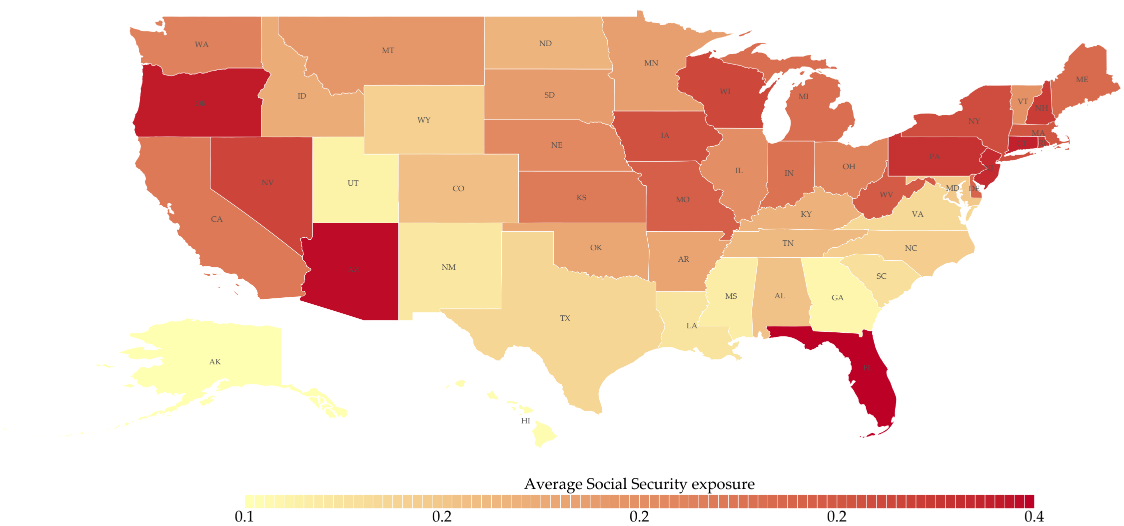
Overall, the regional evidence strongly corroborates the aggregate findings. States more exposed to the temporary transfer shocks experienced systematically larger increases in employment, while the aggregate panel responses closely track the time-series estimates.

Figure B.8: State-level Exposure to Transfer Shocks

(a) Veterans Exposure



(b) Social Security Exposure



Notes: The figure displays cross-state variation in exposure to temporary transfer shocks. Panel (a) reports the average veterans share in the state population, while panel (b) reports the average Social Security recipient share. These exposure measures are used to construct the interaction terms in the heterogeneous panel local projection specifications. Darker shading indicates greater exposure to the transfer shocks.

B.5.5. Financing

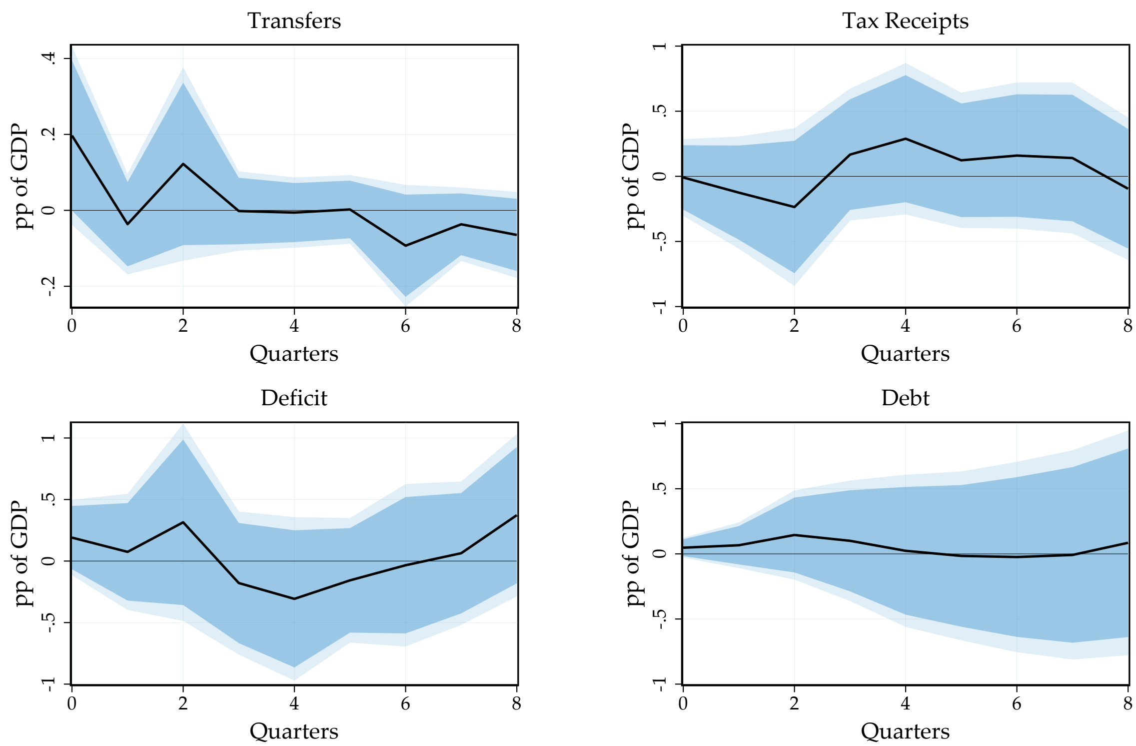
To unpick the financing of the transfer shocks, we additionally estimate the response of taxes and the government deficit to the shock, alongside the response of transfers. We use quarterly data from FRED. We measure taxes using FRED series `W006RC1Q027SBEA`, the federal deficit using FRED series `FGDEF`, and transfers using our narrow transfer series.[4]

For comparability, we scale all outcomes relative to GDP, and then estimate their response to the transfer shock via the regression

$$\frac{y_{t+h} - y_{t-1}}{\text{GDP}_{t-1}} = \alpha_{y,h} + \theta_{y,h} z_{T,t} + \boldsymbol{\beta}'_{y,h} \mathbf{x}_{t-1} + \nu_{y,t+h}, \tag{15}$$

where y_t denotes the outcome variable, and $\mathbf{x}_{t-1}$ includes four lags of the same set of variables as in the baseline quarterly regressions in the main text, alongside four lags of taxes, the government deficit and the narrow transfer series, each scaled by GDP. We also estimate the response of government debt by cumulating the deficit response across horizons—re-running (15) with the left-hand side variable defined as: $\frac{1}{4}\sum_{j=0}^{h}\left(\text{deficits}_{t+j} - \text{deficits}_{t-1}\right)/\text{GDP}_{t-1}$.

Figure B.9: Financing of Transfer Shocks



Notes: Impulse responses to a temporary transfer shock estimated based on the local projections specification (15) using the exogenous transfer shock dates. Lines are point estimates; shaded areas are 90 and 95% confidence bands. Sample: 1947q1–2019q4.

[4] All series are nominal, seasonally adjusted at annual rates (NIPA basis), 1947Q1–2019Q4.

Figure B.9 displays our results. Transfers rise on impact by around 0.20 percentage points of GDP (top-left panel). Taxes do not respond on impact (top-right panel). Hence, we find the increase in transfers flows through directly to an *almost exactly commensurate* increase in the deficit within the quarter of 0.19p.p. of GDP (bottom-left panel). The bottom-right panel converts this response into debt-space: we find debt initially rises before eventually coming back down after a year. While the responses are somewhat noisily estimated—with coefficients generally insignificant across all horizons—our best estimate is that the transfers were entirely deficit-financed within the quarter, with tax receipts then rising and so debt falling after around a year.

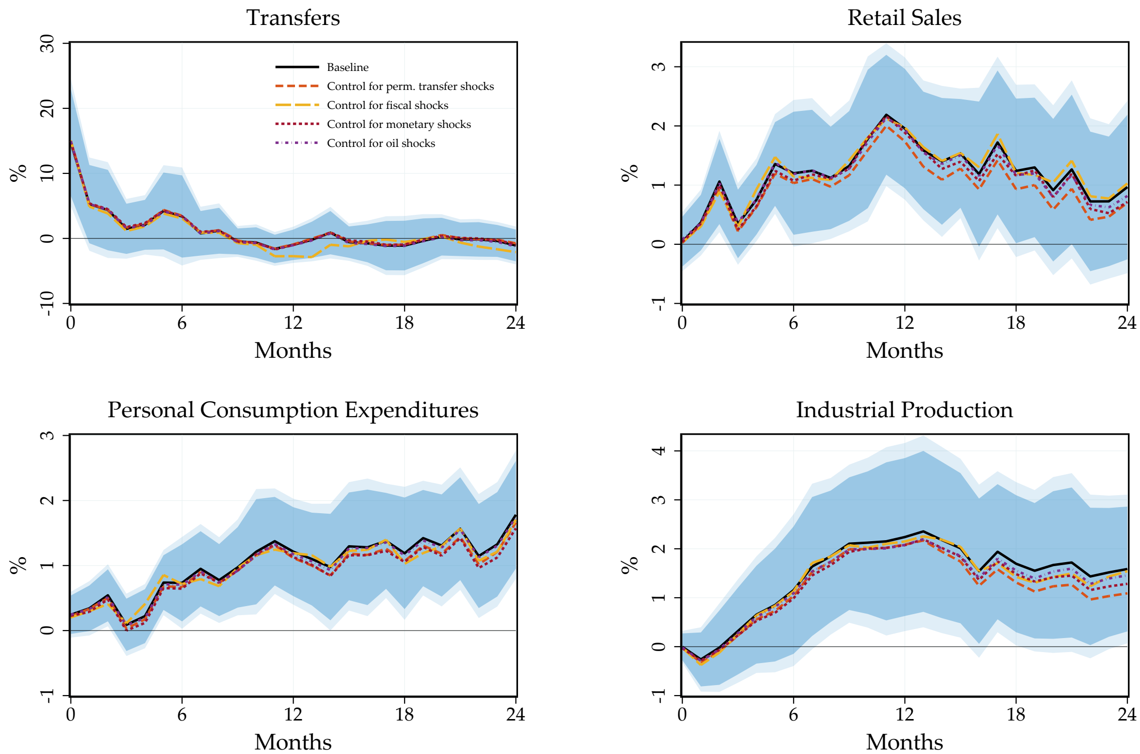
B.6. Additional Sensitivity

This appendix reports additional robustness results. We present the figures referenced in the robustness section of the main text, along with further sensitivity analyses.

B.6.1. Control Variables

In this appendix, we examine the robustness of our results to alternative control variables. Figure B.10 reports the responses of our main variables of interest under alternative control specifications, extending the analysis in the main text beyond consumption expenditures. The results remain very similar across specifications, indicating that our findings are not sensitive to the inclusion of additional controls.

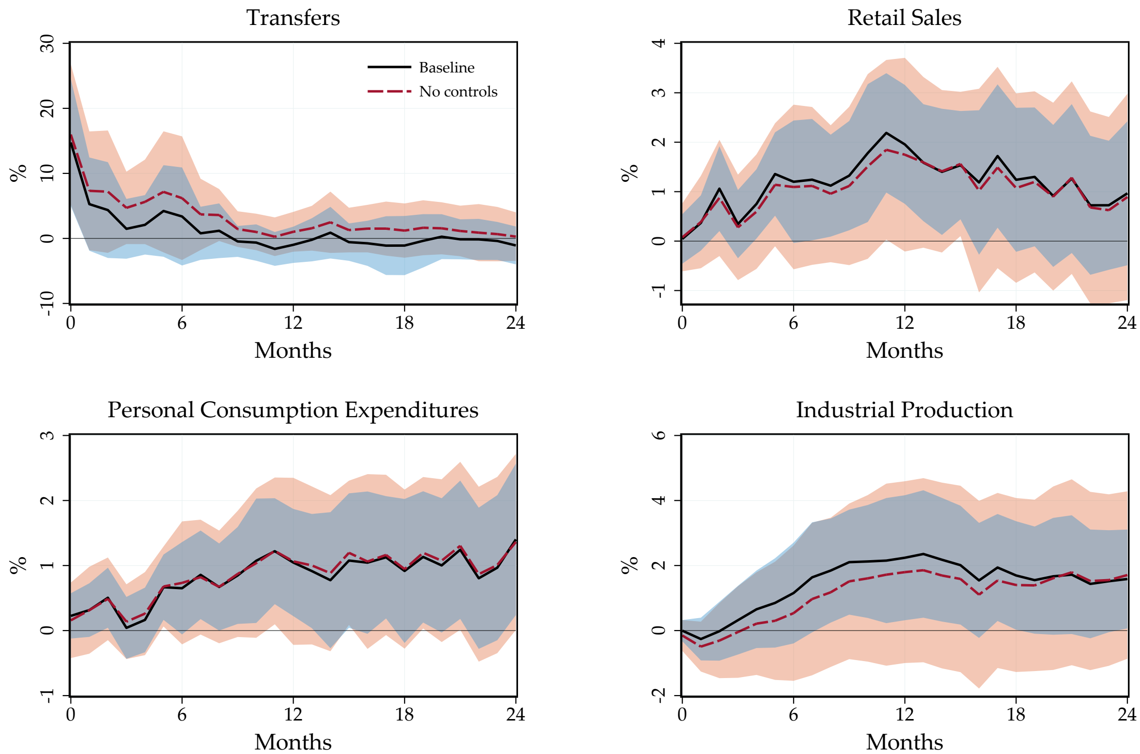
Figure B.10: Sensitivity with Respect to Controls



Notes: Robustness of responses to temporary transfer shocks under alternative control specifications. The alternative specifications add controls for permanent transfer shocks, military spending shocks, monetary policy shocks, and oil price shocks. Solid black lines and blue shaded areas show baseline point estimates and 90% and 95% confidence bands; colored lines show alternative specifications. Sample: 1945m10–2019m12.

As an additional benchmark, we also report estimates from a parsimonious specification without macroeconomic controls, retaining only one lag of the outcome variable to address stationarity concerns and lags of the transfer shock. The point estimates are very similar to the baseline, while the richer control set primarily serves to improve precision (see Figure B.11).

Figure B.11: Specification with No Macro Controls

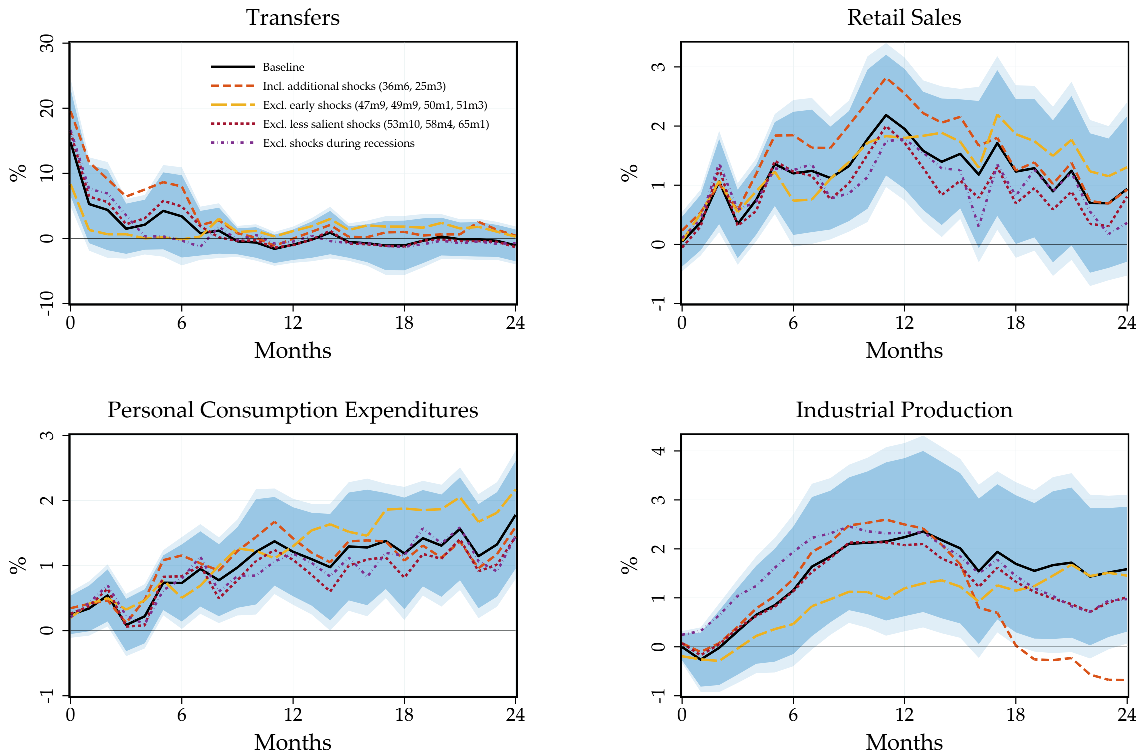


Notes: Impulse responses to temporary transfer shocks under the baseline specification and a parsimonious specification without macroeconomic controls. The latter retains one lag of the outcome variable and lags of the transfer shock. Lines are point estimates; shaded areas are 95% confidence bands.

B.6.2. Shock Selection

Figure B.12 reports the responses of our main variables of interest under alternative selections of transfer shocks. The responses turn out to be similar across different selections of shocks, not only for consumption as shown in the main text, but also for retail sales and industrial production.

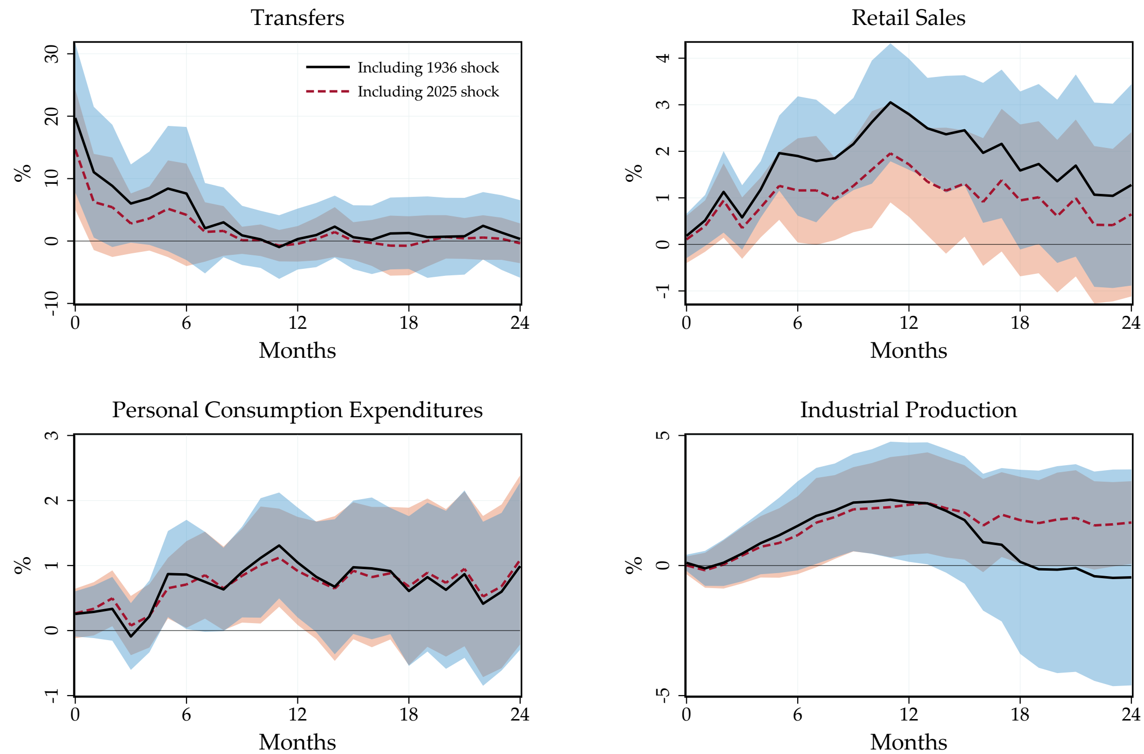
Figure B.12: Sensitivity to Shock Selection



Notes: Robustness of responses to temporary transfer shocks under alternative shock selections. The alternatives include two additional shocks, drop early episodes, exclude less salient shocks, or shocks that occurred in recession periods. Solid black lines and blue shaded areas show baseline point estimates and 90% and 95% confidence bands; colored lines show alternative specifications. Sample: 1945m10–2019m12.

As shown above, jointly including the two transfer episodes excluded from the baseline—the June 1936 veterans' bonus payment, which occurs during the Great Depression, and the March 2025 Social Security Fairness Act payment, which falls in the COVID-era window—produces results that are broadly similar to the baseline. We now study the role of each episode separately by adding them to the sample one at a time. Figure B.13 shows that the results are robust in both cases.

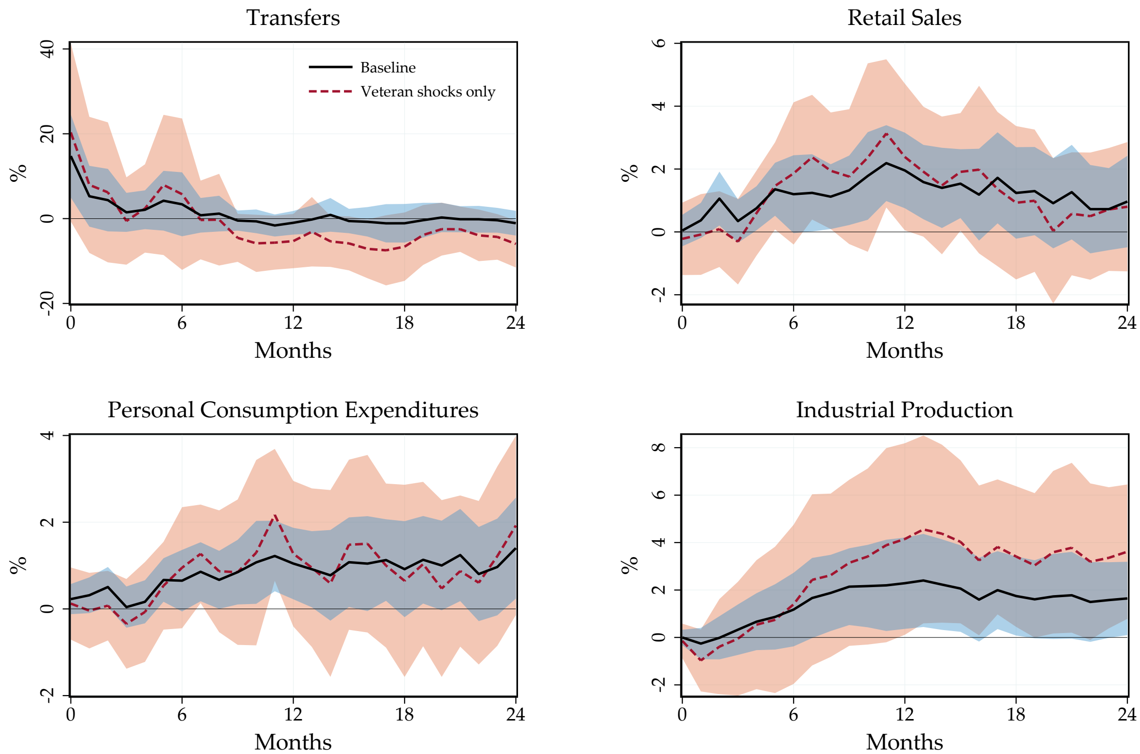
Figure B.13: Robustness to Additional Transfer Episodes (1936 and 2025)



Notes: Robustness of responses to temporary transfer shocks when extending the shock sample. The alternatives add the June 1936 veterans' bonus payment (sample: 1935m1–2019m12) and the March 2025 Social Security Fairness Act payment (sample: 1945m10–2025m12). Lines are point estimates; shaded areas are 95% confidence bands.

How important are veterans' payments compared to the transitory Social Security payments? Figure B.14 shows the responses of our main variables of interest when the shock series is restricted to veterans' payment episodes only. The point estimates are very similar to the baseline across all variables. However, the responses are estimated less precisely and are statistically significant at the 95% level at only a few horizons. This loss of precision is expected, given that the specification relies on fewer episodes and therefore less identifying variation.

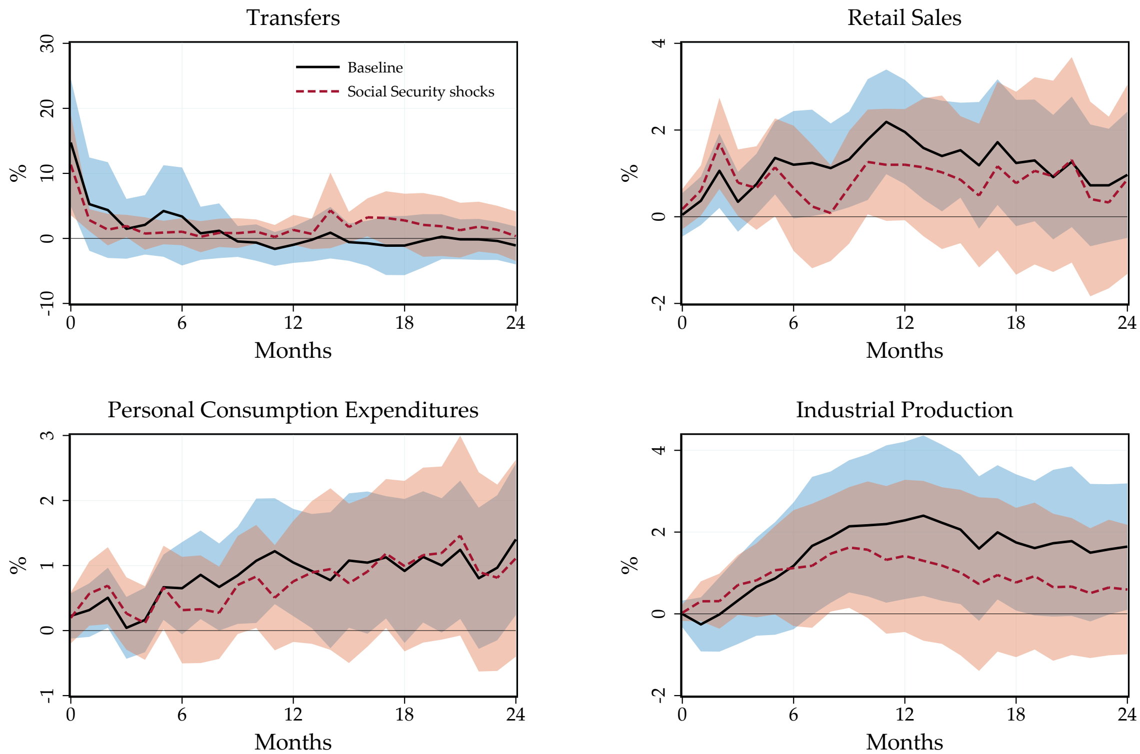
Figure B.14: Using Veteran Shocks Only



Notes: Responses to temporary transfer shocks when restricting the shock series to veterans' payment episodes. Lines are point estimates; shaded areas are 95% confidence bands. Sample: 1945m10–2019m12.

Figure B.15 repeats the exercise using the Social Security transfer episodes only. The point estimates are again broadly similar to the baseline across all variables, although they are somewhat attenuated in some cases and estimated with substantially wider confidence bands. At the 95% level, the responses are largely insignificant. This loss of precision highlights the important role of the veterans' payment episodes in strengthening statistical power. As shown above, augmenting the veterans' payments with the Social Security events helps to further increase power and allows for sharper inference.

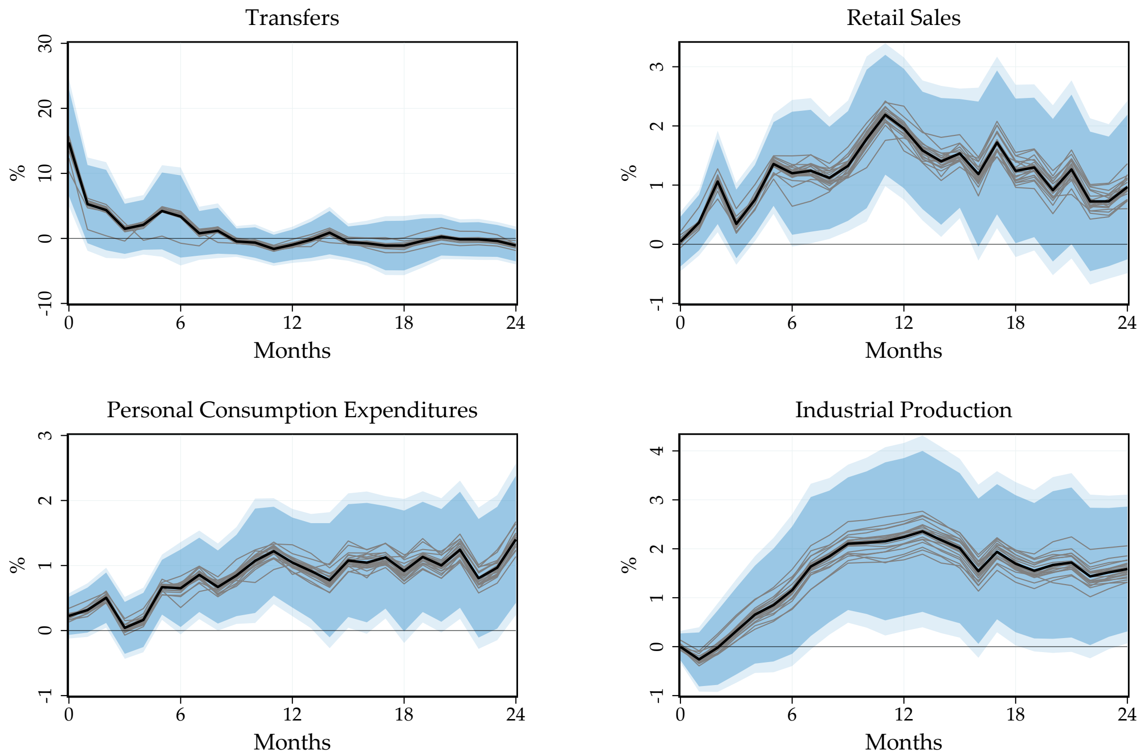
Figure B.15: Using Romer-Romer Transfer Shocks



Notes: Responses to temporary transfer shocks when restricting the shock series to the Romer and Romer (2016) Social Security transfer episodes. Lines are point estimates; shaded areas are 95% confidence bands. Sample: 1945m10–2019m12.

We have shown that the results tend to be very robust with respect to the selection of events. To alleviate concerns about influential episodes, we perform a systematic leave-one-out (jackknife) exercise. For each specification, we re-estimate the impulse responses dropping one transfer shock at a time. Figure B.16 shows the results. The resulting responses are tightly clustered around the baseline estimates and lie well within the 90% confidence bands. This indicates that our findings are not driven by any single episode and are robust to the exclusion of individual shocks.

Figure B.16: Leave-one-out Exercise

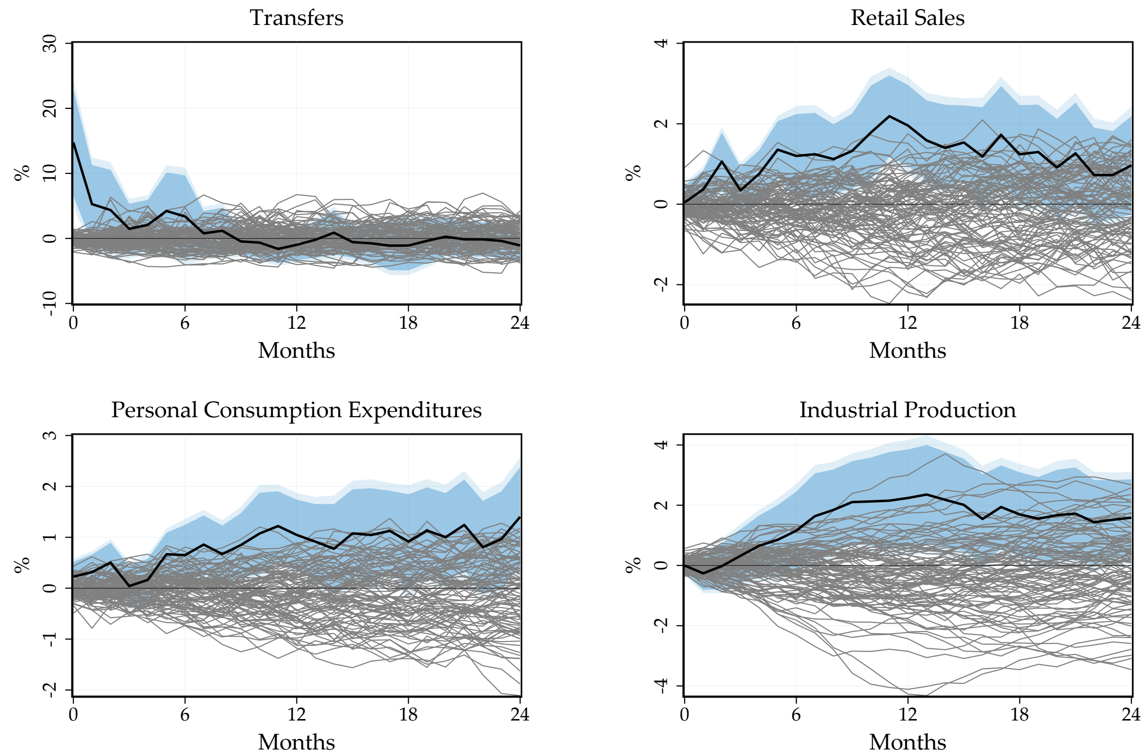


Notes: Sensitivity of responses to individual transfer episodes. Gray lines show estimates obtained by excluding one transfer episode at a time. Solid black lines and blue shaded areas are baseline point estimates and 90% and 95% confidence bands. Sample: 1945m10–2019m12.

Finally, we conduct a placebo exercise to assess whether our estimated responses could arise from spurious timing. We randomly draw 22 placebo event dates from periods that do not coincide with identified transfer shocks and re-estimate the impulse responses. We repeat this procedure 100 times.

Figure B.17 reports the resulting distribution of placebo responses. We find no evidence of systematic effects on transfers or macroeconomic outcomes following these placebo shocks. Moreover, the baseline responses lie at the upper end of the placebo distribution across outcomes, indicating that the estimated effects are unlikely to be driven by chance.

Figure B.17: Placebo Exercise



Notes: Placebo responses based on randomly assigned transfer-shock dates. Placebo shocks are constructed by randomly selecting 22 non-event dates; the procedure is repeated 100 times. Gray lines show responses from each placebo draw. Solid black lines and blue shaded areas are baseline point estimates and 90% and 95% confidence bands. Sample: 1945m10–2019m12.

B.6.3. Shock Size

Our baseline specification estimates reduced-form responses to a pooled narrative transfer shock. This approach deliberately uses only the timing of the transfer episodes, rather than their size. The timing variation is plausibly more exogenous, reflecting administrative delays and institutional features of the payment process, whereas the size of the payments may be partly related to past economic conditions. Importantly, as explained in the main text, the instrumental-variable intuition underlying our empirical design does not require the instrument to measure the transfer impulse perfectly: it only needs to be correlated with the shock of interest and otherwise exogenous. Measurement error in the shock proxy would primarily weaken the first stage and reduce precision.

To see this, consider the following local-projections instrumental-variable (LP-IV) specification. Let T_t denote government transfers, and $\boldsymbol{z}_t$ a vector of narrative instruments. For each horizon $h = 1, \dots, H$, we estimate

$$y_{t+h} = \alpha_h + \theta_h \hat{T}_t + \boldsymbol{\beta}_h' \mathbf{x}_{t-1} + v_{t+h}, \tag{16}$$

$$T_t = \pi_0 + \boldsymbol{\pi}' \boldsymbol{z}_t + \boldsymbol{\gamma}' \mathbf{x}_{t-1} + u_t. \tag{17}$$

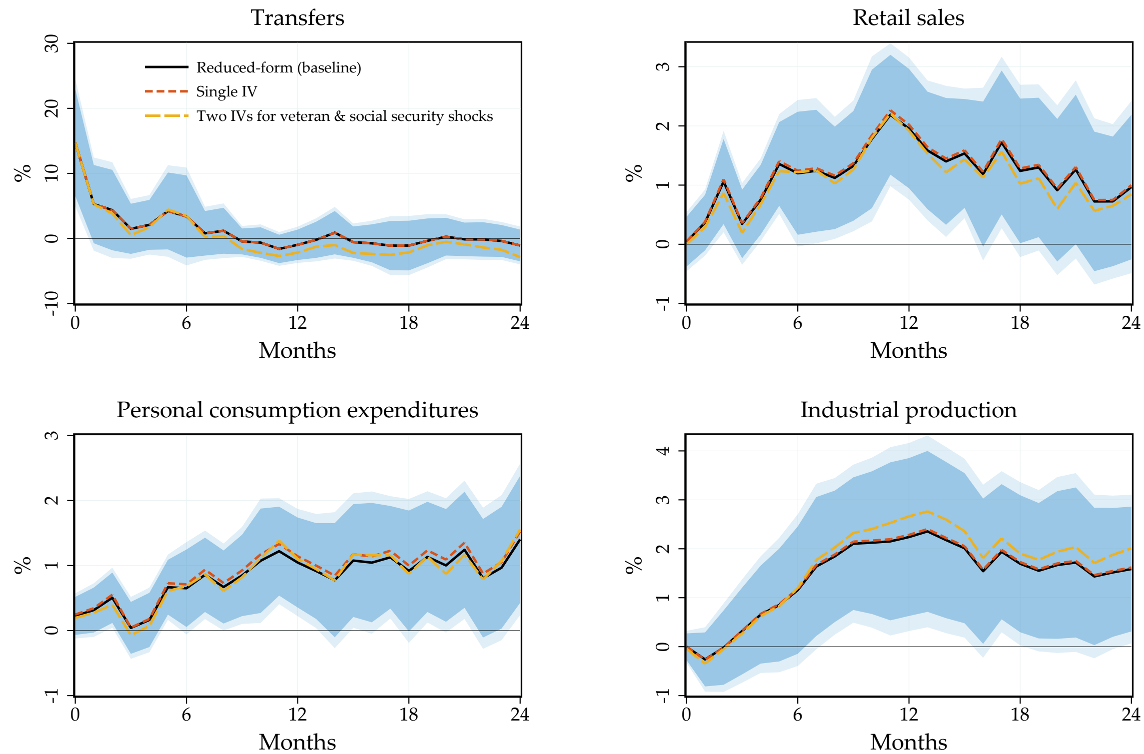
Consider first the case where $\boldsymbol{z}_t$ contains a single instrument: our baseline narrative transfer shock episodes. Figure B.18 shows that the responses are very similar to our reduced-form evidence.[5]

We then consider two extensions that incorporate additional information about the shock into the instrument. First, we construct two separate instruments: one for veterans' payment dates, z_t^v, and one for the transitory Social Security transfer episodes, z_t^s. Let $\boldsymbol{z}_t = ((z_t^v)', (z_t^s)')'$. This specification differs from the baseline in that it does not pool the two sets of episodes into a single narrative shock indicator. Instead, the two instruments enter separately in the first stage, allowing the LP-IV estimator to place different weights on the veterans' and Social Security payment episodes. Figure B.18 shows that the responses remain close to the baseline, indicating that the results are not driven by the particular pooling of the two sets of transfer episodes in the reduced-form specification.

As a second extension, we use information on the size of the transfer payments. Let q_t denote the size of the payment in month t, converted into real dollars and expressed in billions, with $q_t = 0$ in months without a transfer episode. We define the quantitative shock series as $z_{T,t}^q = \log(1 + q_t)$. Adding one keeps the transformation well behaved for payments below one billion dollars and assigns a value of zero to non-event months. We then estimate the reduced-form local projection in (1), replacing the pooled date indicator $z_{T,t}$ with the quantitative shock $z_{T,t}^q$. Figure B.19 shows the impulse responses obtained from this quantitative shock measure. The responses are again similar to the baseline, suggesting that incorporating information on the size and thus putting more weight on large shock episodes does not affect our results materially.

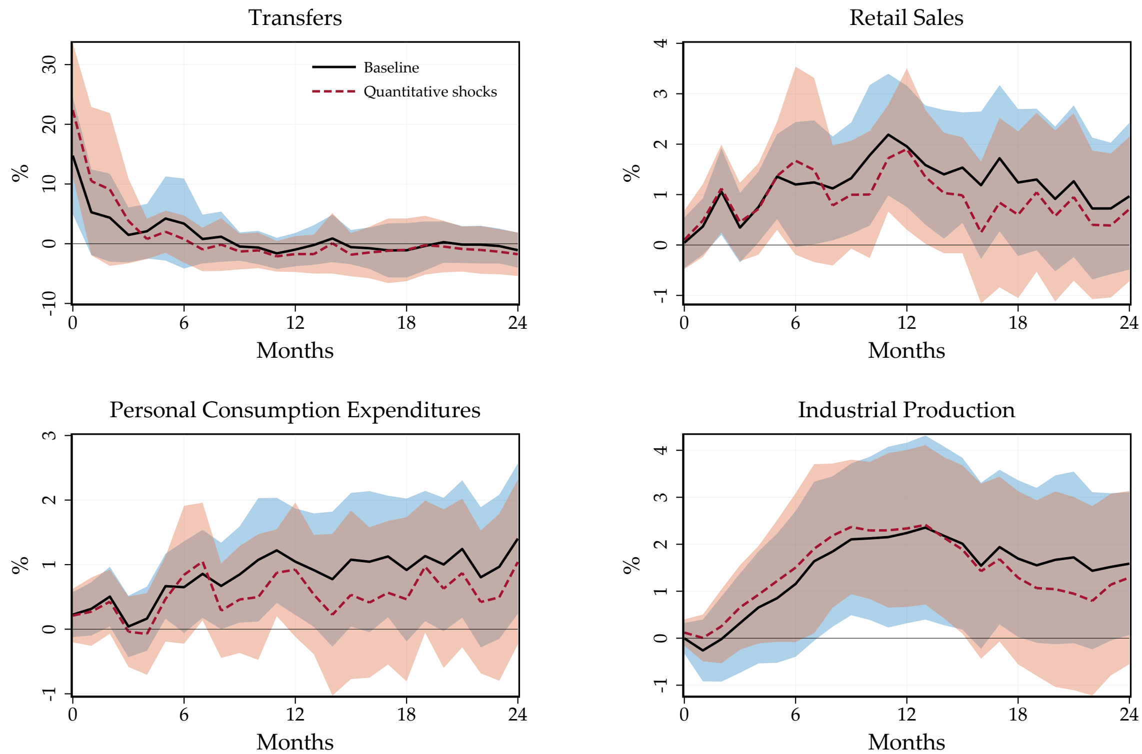
[5] Note that the controls slightly differ across the two specifications because our baseline macro controls $\mathbf{x}_{t-1}$ do not include all outcome variables of interest. With the same controls, the estimates would be the same.

Figure B.18: Local-projection Instrumental-variable Specification



Notes: Responses to a temporary transfer shock under different specifications: (i) the reduced-form baseline specification, which uses the pooled narrative transfer-date indicator, (ii) the LP-IV specification (16) using the same pooled indicator as a single instrument, (iii) the LP-IV specification with two separate instruments: indicators for veterans' payment dates and the Social Security transfer episodes. Lines show point estimates; blue shaded areas are the baseline 90% and 95% confidence bands. Sample: 1945m10–2019m12.

Figure B.19: Using Quantitative Transfer Shock Measure

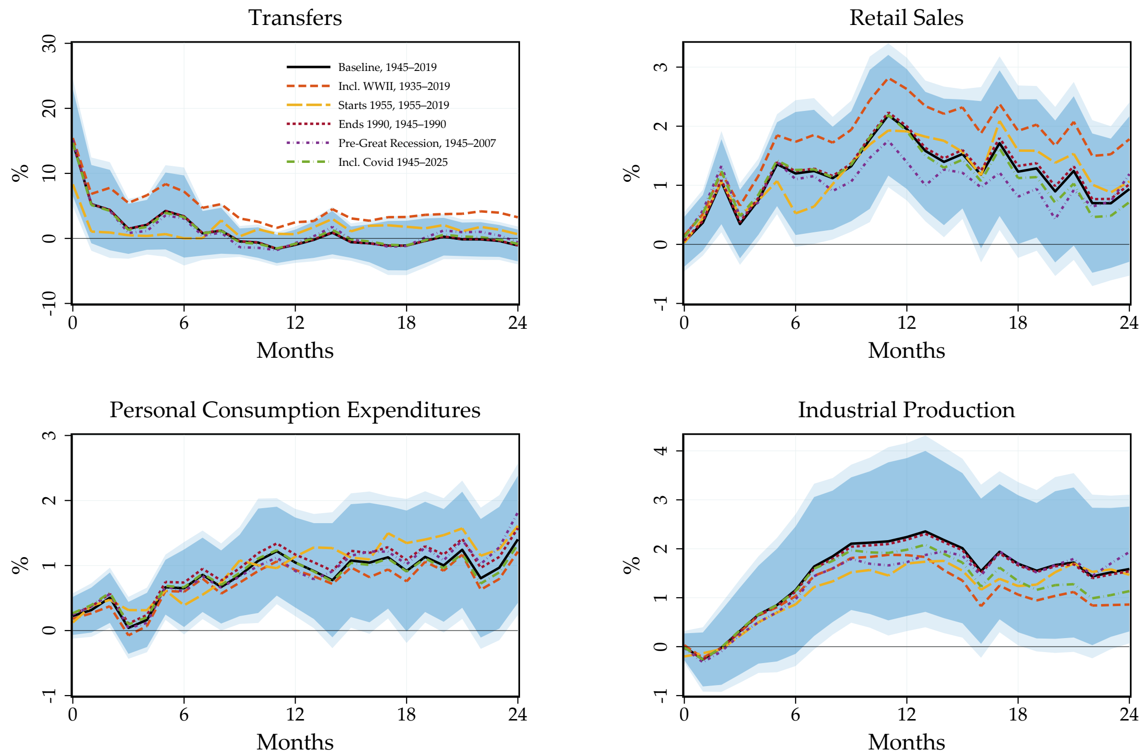


Notes: Impulse responses to a temporary transfer shock, estimated using the reduced-form local projections specification in (1) using the quantitative transfer shock measure $z^q_{T,t}$. Lines are point estimates; shaded areas are 95% confidence bands. Sample: 1945m10–2019m12.

B.6.4. Sample Period

We next examine the sensitivity of the results to the estimation sample. Figure B.20 varies both the start and end dates of the sample, including specifications that start in 1935 or 1955, end in 1990 or before the Great Recession, and extend the baseline sample through the COVID period. Across these alternative estimation samples, the responses of transfers, retail sales, personal consumption expenditures, and industrial production remain close to the baseline and display similar dynamics.

Figure B.20: Robustness to Sample Period



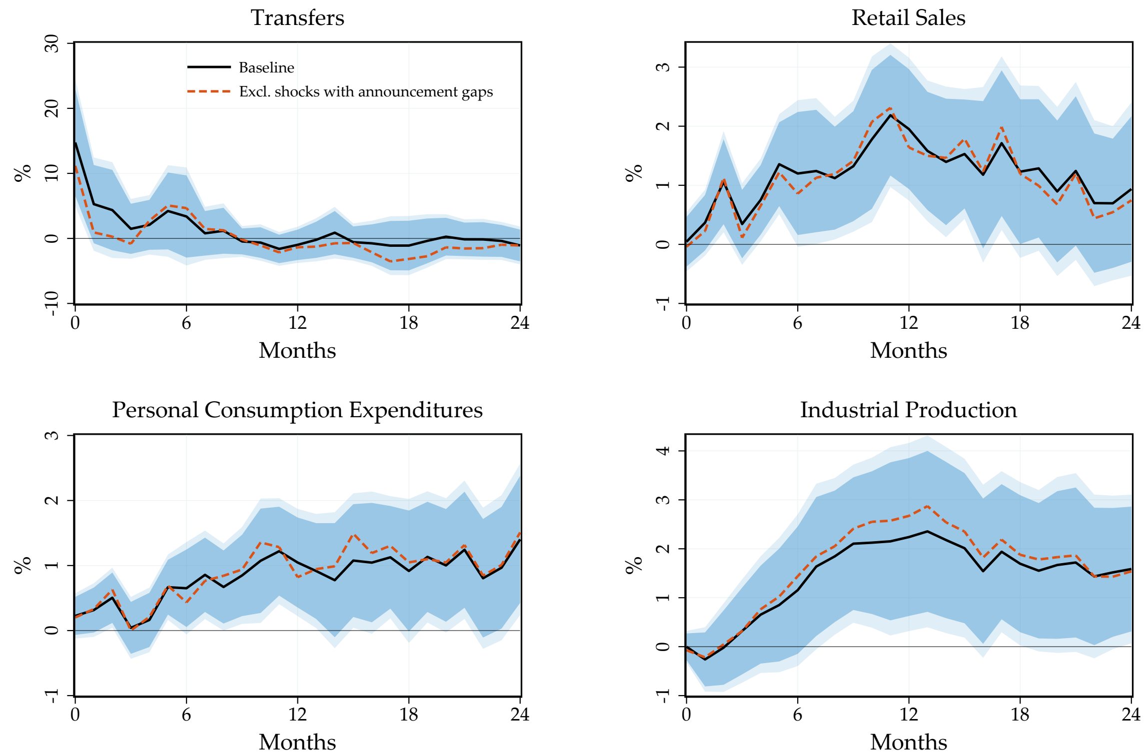
Notes: Impulse responses to a temporary transfer shock under alternative sample periods: 1935m1–2019m12, 1955m1–2019m12, 1945m10-1990m12, 1945m10-2007m12, and 1945m10-2025m12. Baseline sample: 1945m10–2019m12. Solid black lines and blue shaded areas are baseline point estimates and 90% and 95% confidence bands. Colored lines: point estimates under alternative samples.

B.6.5. Anticipation and Pre-trends

A remaining concern is that the estimated responses could reflect anticipation of transfer payments prior to disbursement. Appendix A.5.2 documents a small number of episodes with notable gaps between the announcement and disbursement dates. As discussed there, the effective announcement date is sometimes difficult to identify precisely, especially for non-legislated payments. This creates a potential concern that households may have responded before the disbursement month. At the same time, the relevant gaps are short compared with other fiscal settings in which anticipation is central, such as government spending shocks (Ramey, 2011).

We first address this concern by excluding episodes for which the gap between announcement and disbursement exceeds three months. Figure B.21 compares the resulting impulse responses with the baseline estimates. The responses of transfers, retail sales, personal consumption expenditures, and industrial production are very similar across the two specifications. This suggests that the baseline results are not driven by episodes with the longest potential anticipation windows.

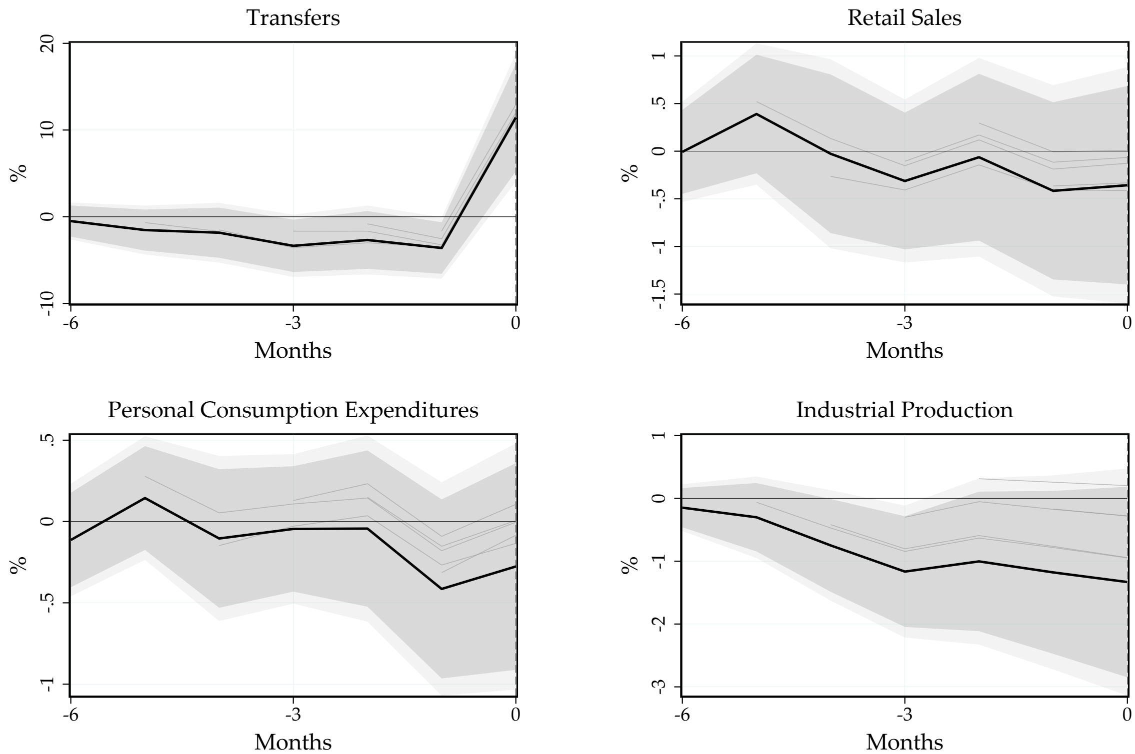
Figure B.21: Anticipation: Excluding Long Announcement Gaps



Notes: Robustness of responses to temporary transfer shocks when excluding episodes with announcement-to-disbursement gaps exceeding three months. Lines are point estimates; blue shaded areas are baseline 90% and 95% confidence bands. Sample: 1945m10–2019m12.

As a second check, we shift the transfer shock series backward relative to the disbursement date. This exercise asks whether the variables of interest move systematically in the months before payments are made. Figure B.22 shows no significant changes prior to the disbursement month. The absence of clear pre-responses provides further evidence that announcement effects are limited in this context.

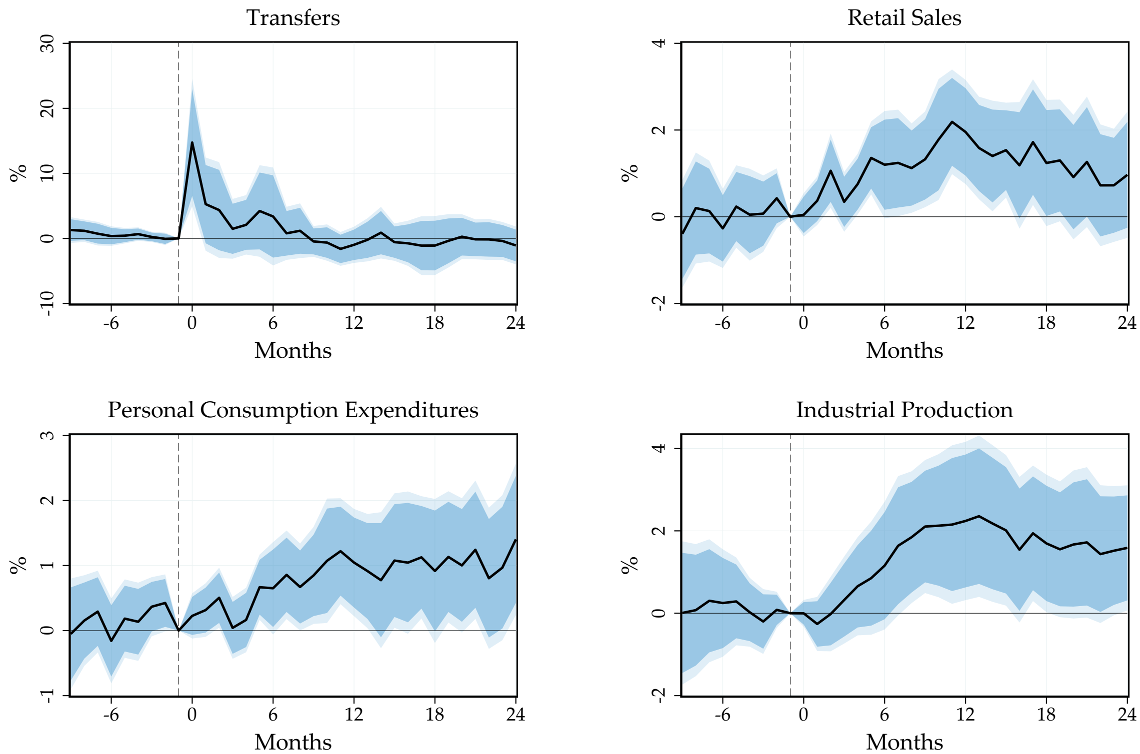
Figure B.22: Anticipation: Shifting the Transfer Shock Series



Notes: Responses to shifted transfer-shock dates. Horizons are measured relative to the original disbursement date. The black line shows the response when shocks are shifted back by six months; gray lines show intermediate shifts. Shaded areas show 90% and 95% confidence bands for the six-month shift. Sample: 1945m10–2019m12.

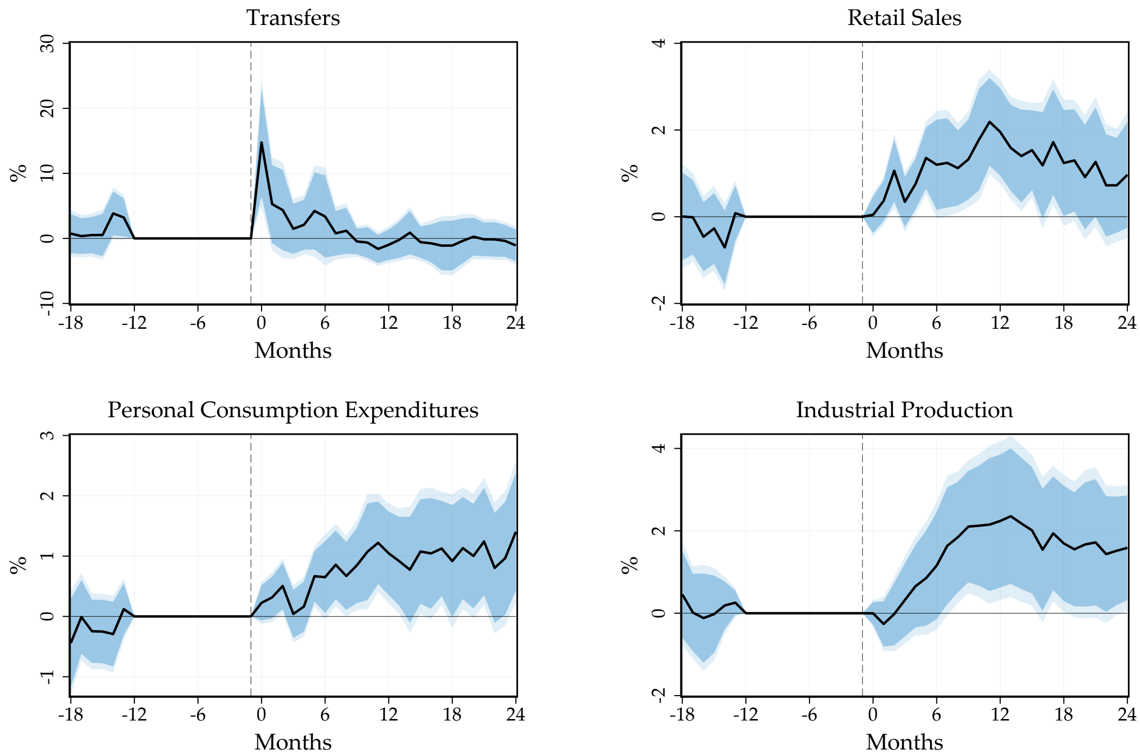
Finally, we study pre-trends directly. We implement two versions of this exercise. In the first, we drop only the controls needed to avoid mechanically forcing the pre-shock coefficients to zero, while otherwise preserving the baseline specification. Figure B.23 shows no systematic movement in the baseline variables before the shock. In the second, we keep the full baseline set of controls but evaluate changes in the outcome variable at horizons that lie before the lag window used as controls. Figure B.24 gives the same conclusion. Taken together, the evidence from dropping long-gap episodes, shifting the shock series, and estimating pre-trends indicates that anticipation effects are unlikely to be quantitatively important for our estimates.

Figure B.23: Pre-trends: Flexible Controls



Notes: Pre-trend estimates for a temporary transfer shock, estimated using the local projections specification (1). For pre-shock horizons, the specification drops only controls that would otherwise mechanically absorb the pre-period variation. Lines are point estimates; blue shaded areas are 90% and 95% confidence bands. Sample: 1945m10–2019m12.

Figure B.24: Pre-trends: Baseline Specification

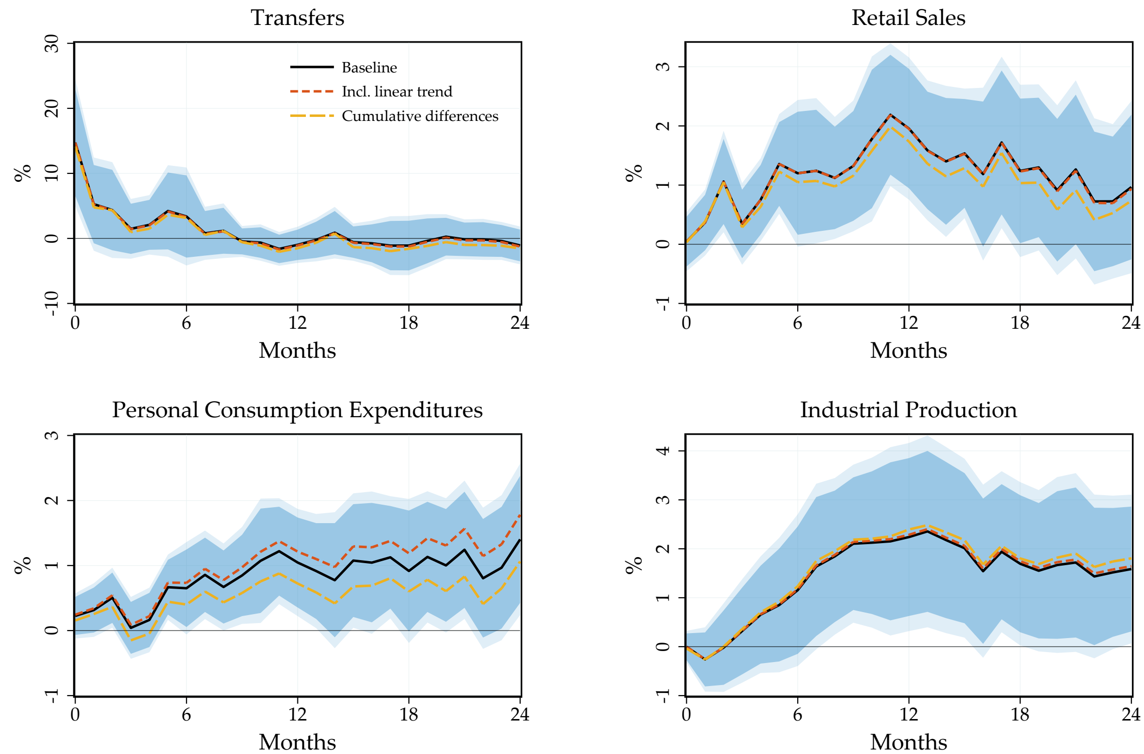


Notes: Pre-trend estimates for a temporary transfer shock, estimated using the local projections specification (1) using the baseline set of controls. The pre-shock coefficients are evaluated at horizons before the baseline lag window. Lines are point estimates; blue shaded areas are 90% and 95% confidence bands. Sample: 1945m10–2019m12.

B.6.6. Other Specification and Estimation Choices

Figure B.25 reports the impulse responses under alternative detrending specifications: estimating the local projections in cumulative differences, or including a linear trend in the levels specification. The results are very similar across specifications, indicating that our findings are not sensitive to the detrending approach.

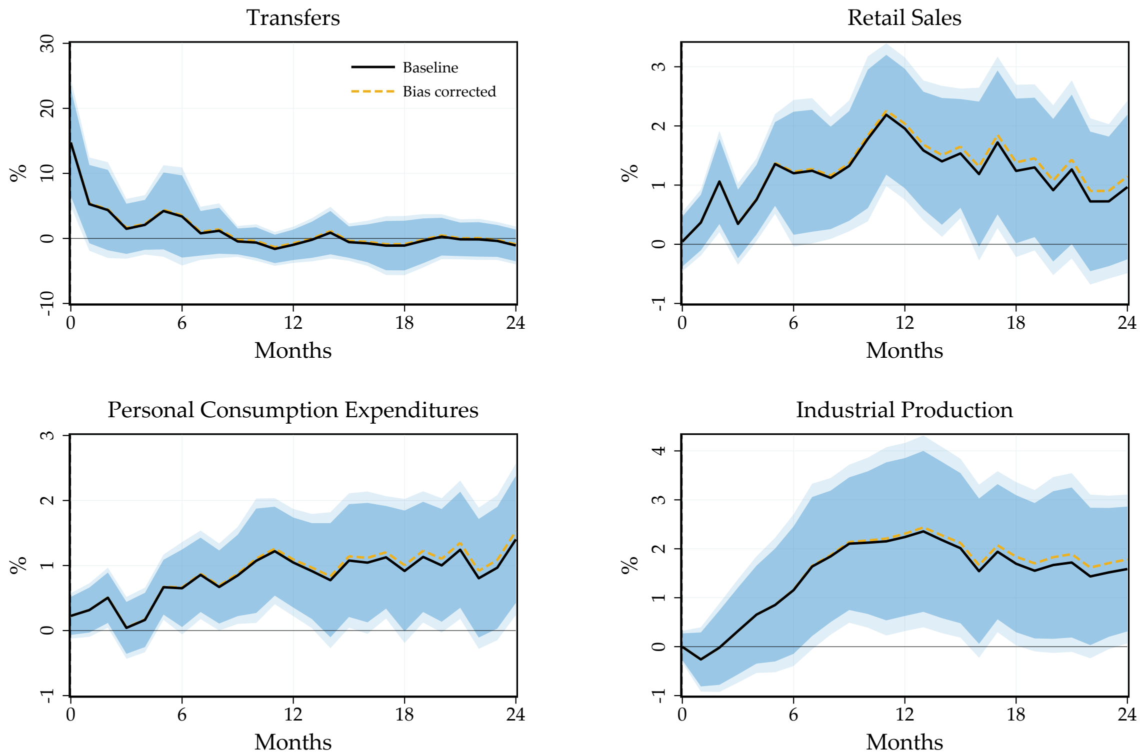
Figure B.25: Robustness to Detrending



Notes: Robustness of responses to temporary transfer shocks under alternative detrending choices. The alternatives include adding a linear trend and estimating responses in cumulative differences. Solid black lines and blue shaded areas show baseline point estimates and 90% and 95% confidence bands; colored lines show alternative specifications. Sample: 1945m10–2019m12.

Finally, we assess whether our estimates could be affected by small-sample bias, as suggested in Olea et al. (2025). Figure B.26 reports the impulse responses when we apply the small-sample bias correction for local projections proposed by Herbst and Johannsen (2024). The bias-corrected responses are virtually identical to the baseline estimates across outcomes and horizons.

Figure B.26: Small-sample LP Bias Correction

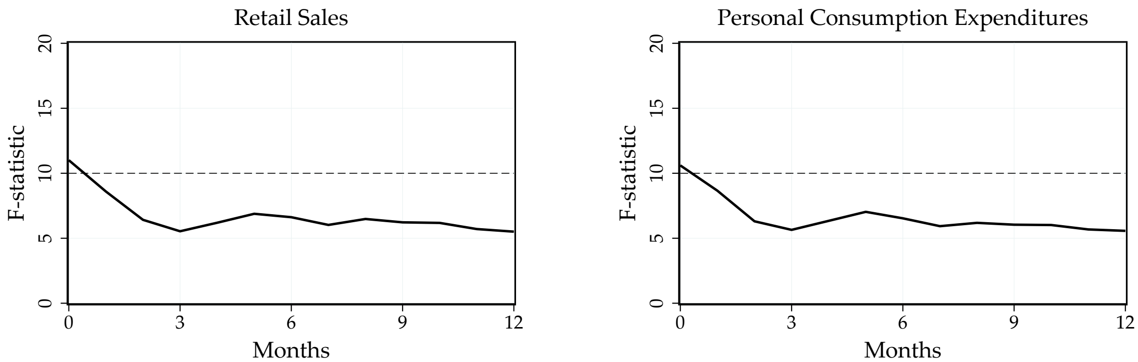


Notes: Responses to temporary transfer shocks with the Herbst and Johannsen (2024) small-sample bias correction. Lines are point estimates; blue shaded areas are baseline 90% and 95% confidence bands. Sample: 1945m10–2019m12.

B.7. First Stage of Multiplier Regression

Figure B.27 reports the first-stage F-statistics underlying the multiplier estimates for retail sales and personal consumption expenditures. The narrative transfer shock series is a strong instrument at short horizons: on impact, the first-stage F-statistics are 11.0 for retail sales and 10.8 for personal consumption expenditures. Instrument relevance declines gradually over the horizon, with F-statistics falling to around 6 after six months. This pattern suggests that the instrument is most informative for the short-run multiplier estimates, while estimates at longer horizons should be interpreted with some caution.

Figure B.27: First-Stage F-Statistics for Transfer Multipliers



Notes: First-stage F-statistics for the transfer-shock instrument underlying the cumulative transfer multiplier estimates from (2). Sample: 1945m10–2019m12.

C. Model Appendix

C.1. Additional Figures and Tables

This appendix contains additional figures and tables from the model section in the main text.

Table C.1: Calibrating Permanent Household Heterogeneity

	Veterans	**Non-veterans**
Demographics		
Population share	22.6%	77.4%
Labor income		
Labor-income share	29.1%	70.9%
Permanent income, $\overline{e}_g$	1.29	0.92
Liquid assets and preferences		
Liquid asset share	18.2%	81.8%
Discount factor, β_g	0.976	0.983

Notes: The population share, labor-income share, and liquid asset share are computed from the Survey of Consumer Finances (SCF). We use the average across reference years of the corresponding survey-weighted moments reported in the SCF microdata memo. The memo pools SCF waves covering reference years 1946–1950 and reports veteran shares of population, liquid assets, wages, and income; liquid assets are defined as savings bonds, interest-bearing government bonds, savings accounts, and checking accounts, with the precise component availability varying by year. Permanent income, $\overline{e}_g$, and the discount factor, β_g, are model parameters calibrated to match these empirical moments. See the text for further details.

Table C.2: Steady-State Calibrated Parameters

Parameter	Description	Value
Preferences and income risk		
σ^{-1}	Elasticity of intertemporal substitution	0.5
ψ^{-1}	Frisch elasticity	0.75
ρ_e	Persistence of idiosyncratic shocks	0.98
σ_e	Dispersion of idiosyncratic shocks	0.92
Fiscal policy		
γ	Tax progressivity	0.181
τ_d	Fiscal feedback (monthly)	0.8%
ϕ	Transfer targeting	0.158
P	Veteran insurance premium	0.0219
Prices and nominal frictions		
r	Monthly real interest rate	0.17%
κ_w	Wage rigidity	0.0062/9

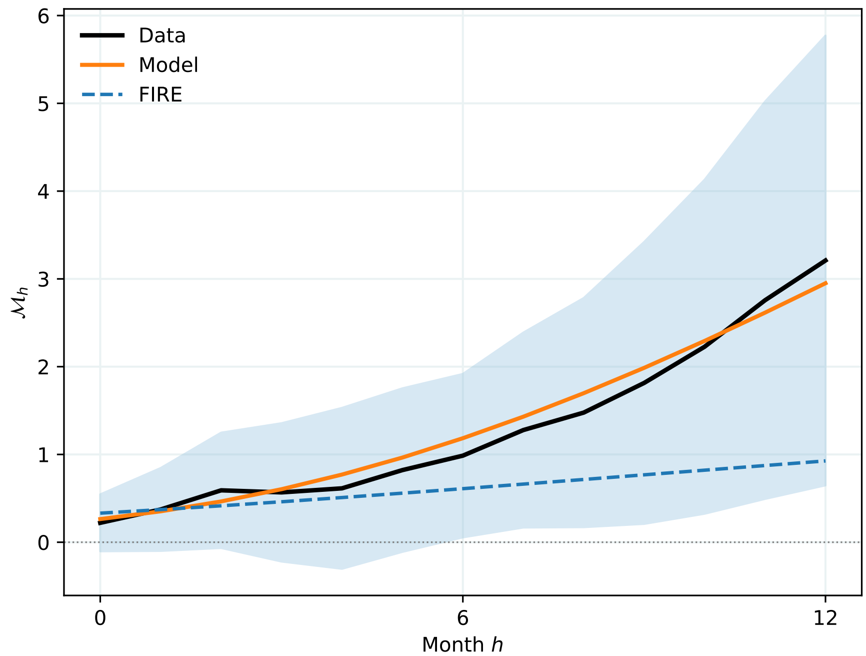
Notes: This table reports the calibrated steady-state parameters used in the quantitative model. Parameters are either set externally using data and standard values from the literature or chosen to match the steady-state targets described in the text. See the text for further details on sources, units, and calibration targets.

Table C.3: Cumulative Transfer Multipliers

	FIRE		**Full Estimated Model**	
Horizon	**Veteran Payment**	**Stimulus Check**	**Veteran Payment**	**Stimulus Check**
6 months	0.61	0.54	1.18	1.09
12 months	0.93	0.82	2.95	2.76

Notes: The table reports cumulative transfer multipliers from the preferred-target quantitative model. Veteran payment refers to the estimated T^v shock. Stimulus check refers to an equal per-person government transfer with the same aggregate path. Multipliers are rounded to two decimal places.

Figure C.1: Model Fit to Cumulative Multiplier



Notes: The solid black line is the empirical response for the cumulative multiplier. The orange line is the full-model-implied path for the cumulative transfer multiplier. The dashed blue line is the path for the cumulative multiplier implied by the estimated FIRE model.

C.2. Derivation of the Wage NKPC

This appendix derives the wage Phillips curve reported in the main text. Let labor be a Dixit-Stiglitz aggregate of differentiated labor services:

$$N_t = \left(\int_0^1 N_{u,t}^{\frac{\mu_w - 1}{\mu_w}} \, du \right)^{\frac{\mu_w}{\mu_w - 1}},$$

where $u \in [0,1]$ indexes unions and $\mu_w > 1$ is the elasticity of substitution across labor types. Cost minimization by competitive firms implies the usual labor-demand schedule for type-u labor,

$$N_{u,t} = \left(\frac{W_{u,t}}{W_t} \right)^{-\mu_w} N_t, \tag{18}$$

where $W_{u,t}$ is the nominal wage set by union u and W_t is the aggregate nominal wage index.

Since final output is linear in the labor aggregate, $Y_t = N_t$, and the representative firm makes zero profits, the real wage equals the price of one unit of output in efficiency units. Let

$$w_t \equiv \frac{W_t}{P_t}$$

denote the real wage. Wage inflation is

$$\pi_t^w \equiv \frac{W_t - W_{t-1}}{W_{t-1}}.$$

C.2.1. Union Problem

Each union chooses its nominal wage taking aggregate objects as given. We maintain the equal-rationing assumption from the main text: every household supplies the same number of hours, so $n_{i,t} = N_t$ in equilibrium. For simplicity, we assume unions evaluate the utility value of additional labor income using the marginal utility of aggregate consumption $u'(C_t)$.

Define

$$Z_t \equiv (1 - \tau_t)(w_t N_t)^{1-\gamma}.$$

Because the after-tax retention function is $y_{i,t} = (1 - \tau_t)(w_t \bar{e}_g e_{i,t} N_t)^{1-\gamma}$, a marginal increase in labor income of type u raises utility in proportion to

$$u'(C_t)(1-\gamma)\frac{Z_t}{N_t}.$$

Using (18), union u solves

$$\max_{\{W_{u,t+h}\}_{h\geq 0}} E_t \sum_{h=0}^{\infty} \beta^h \left[u'(C_{t+h})(1-\gamma)\frac{Z_{t+h}}{N_{t+h}} W_{u,t+h} \frac{N_{u,t+h}}{P_{t+h}} - v'(N_{t+h})N_{u,t+h} - \frac{1}{2\kappa_w}\left(\frac{W_{u,t+h}}{W_{u,t+h-1}} - 1\right)^2 \right]. \tag{19}$$

The first term is the marginal-utility value of labor income generated by union u, the second is the disutility of supplying labor, and the last term is a Rotemberg nominal wage adjustment cost measured in utility units.

Differentiating (19) with respect to $W_{u,t}$ and using

$$\frac{\partial N_{u,t}}{\partial W_{u,t}} = -\mu_w \frac{N_{u,t}}{W_{u,t}}$$

from (18), we obtain

$$\begin{aligned}\left(\frac{W_{u,t}}{W_{u,t-1}} - 1\right)\frac{W_{u,t}}{W_{u,t-1}} = \kappa_w \left[-(\mu_w - 1)u'(C_t)(1-\gamma)\frac{Z_t}{N_t}\frac{W_{u,t}N_{u,t}}{P_t} + \mu_w v'(N_t)N_{u,t} \right] \\ + \beta E_t \left[\left(\frac{W_{u,t+1}}{W_{u,t}} - 1\right)\frac{W_{u,t+1}}{W_{u,t}} \right].\end{aligned} \tag{20}$$

In a symmetric equilibrium, all unions choose the same wage, so $W_{u,t} = W_t$ and $N_{u,t} = N_t$. Since $W_t/P_t = w_t$, (20) becomes

$$\pi_t^w(1+\pi_t^w) = \kappa_w \mu_w \left[v'(N_t)N_t - \frac{\mu_w - 1}{\mu_w}(1-\gamma)Z_t u'(C_t) \right] + \beta E_t \left[\pi_{t+1}^w (1 + \pi_{t+1}^w) \right]. \tag{21}$$

At a zero-inflation steady state, the wage markup condition is

$$v'(N) = \frac{\mu_w - 1}{\mu_w}(1-\gamma)\frac{Z}{N}u'(C). \tag{22}$$

C.2.2. Linearization

Log-linearize (21) around the zero-inflation steady state. Since steady-state inflation is zero, the left-hand side becomes simply π_t^w. The linearized gap in square brackets is proportional to the log deviation of

$$v'(N_t) \qquad \text{relative to} \qquad \frac{Z_t}{N_t}u'(C_t).$$

Let $\widehat{X}_t \equiv dX_t/X$ denote the log deviation of variable X_t from steady state. By definition of the inverse Frisch elasticity,

$$\widehat{v'(N_t)} = \psi \widehat{N}_t.$$

Likewise, under CRRA utility with inverse EIS σ,

$$\widehat{u'(C_t)} = -\sigma \widehat{C}_t.$$

Therefore

$$\left(\widehat{\frac{Z_t}{N_t} u'(C_t)}\right) = \widehat{Z}_t - \widehat{N}_t - \sigma \widehat{C}_t.$$

Using the steady-state condition (22), the linearized version of the bracketed term in (21) is proportional to

$$\psi \widehat{N}_t - \left(\widehat{Z}_t - \widehat{N}_t - \sigma \widehat{C}_t\right) = \psi \widehat{N}_t + \sigma \widehat{C}_t - \left(\widehat{Z}_t - \widehat{N}_t\right).$$

Absorbing the constant of proportionality into the slope coefficient, we obtain

$$\pi_t^w = \kappa_w \left[\psi \frac{dN_t}{N} + \sigma \frac{dC_t}{C} - \left\{\frac{dZ_t}{Z} - \frac{dN_t}{N}\right\}\right] + \beta \mathbb{E}_t[\pi_{t+1}^w],$$

which is the wage NK Phillips curve stated in the text.

C.3. Average Expectations under Noisy Information and Long-Memory Diagnostic Expectations

This appendix derives equation (9) from first principles, following the noisy-information and long-memory diagnostic-expectations model discussed in Bardóczy and Guerreiro (2023). Formally, we assume that individuals see their own individual transfer $\{\omega^v_{i,t}T^v_t, \omega_{i,t}T_t\}$, i.e., they know their current and future transfers. However, they do not know the magnitude of the aggregate shock ϵ. We assume that ϵ is distributed according to a normal distribution, with mean 0 and variance normalized to 1.

Fix a deterministic aggregate innovation at date 0. For any variable of interest $X_{t+h} = \mathbb{M}^X_{t+h}\epsilon$, for some $\mathbb{M}_{t+h} \in \mathbb{R}$. Let

$$\mu_{t,h} \equiv \mathbb{E}_t[dX_{t+h}] = \mathbb{M}_{t+h}\mathbb{E}_t[\epsilon]$$

denote the model-consistent (the econometrician's) response of X_{t+h} to that innovation.

We assume that households do not observe ϵ perfectly. Instead, by date t each household i has seen $t+1$ independent noisy signals

$$s_{i,\ell} = \epsilon + \eta_{i,\ell}, \qquad \ell = 0, 1, \ldots, t,$$

where the noise terms are normally distributed with mean zero and independent across households and dates, and with precision τ. We normalize the prior precision on ϵ to one, so the prior variance is 1. Because the shock is deterministic, the only source of disagreement across households comes from signal noise.

C.3.1. Bayesian Updating under Noisy Information

Conditional on the $t+1$ signals observed up to date t, household i's posterior mean is the standard Gaussian posterior:

$$E^{NI}_{i,t}[\epsilon] = \frac{\tau(t+1)}{1+\tau(t+1)}\left(\frac{1}{t+1}\sum_{\ell=0}^{t} s_{i,\ell}\right). \tag{23}$$

Substituting the signal equation into (23) gives

$$E^{NI}_{i,t}[\epsilon] = \frac{\tau(t+1)}{1+\tau(t+1)}\epsilon + \frac{\tau(t+1)}{1+\tau(t+1)}\left(\frac{1}{t+1}\sum_{\ell=0}^{t} \eta_{i,\ell}\right).$$

Taking the cross-sectional average across households eliminates the idiosyncratic noise term. Hence average noisy-information expectations satisfy

$$\overline{E}_t^{NI}[\epsilon] = \frac{\tau(t+1)}{1+\tau(t+1)}\mathbb{E}_t[\epsilon] \Rightarrow \overline{E}_t^{NI}[dX_{t+h}] = \frac{\tau(t+1)}{1+\tau(t+1)}\mathbb{E}_t[dX_{t+h}]. \tag{24}$$

With only noisy information

$$\frac{E_t^{NI}[dX_{t+h}]}{\mathbb{E}_t[dX_{t+h}]} = \frac{\tau(t+1)}{1+\tau(t+1)} \leq 1.$$

This is the standard under-reaction generated by incomplete information: with finitely precise signals, households place less than unit weight on the true deterministic response.

C.3.2. Adding Diagnostic Expectations with Long Memory

Following Bordalo et al. (2020), we add diagnostic overreaction to the noisy information model. Households distort their noisy-information posterior by over-extrapolating the gap between the current posterior and a reference belief. So, the forecaster overweights representative states by the distorted posterior distribution

$$f^{\theta}(\epsilon|S_{i,t}) = f(\epsilon|S_{i,t})R_{i,t}^{\theta}\frac{1}{Z_{i,t}},$$

where $S_{i,t} \equiv \{s_{i,0},...,s_{i,t}\}$ denotes the history of individual signals up to time t, $\theta > 0$ captures the degree of diagnosticity of expectations, $R_{i,t}$ denotes the direction of representativeness heuristic, and $Z_{i,t}$ is a normalizing factor ensuring the pdf integrates to one.

The representativeness heuristic implies that agents overweight the probability of states that became more likely in light of the newly collected information,

$$R_{i,t} \equiv \frac{f(\epsilon|S_{i,t})}{f^r(\epsilon|S_{i,t-1})}.$$

So, provided $\theta > 0$, the distribution f^{θ} places relatively more weight on states that have become more likely in light of the new information set $S_{i,t}$ relative to a reference distribution which only includes past information $f^r(\epsilon|S_{i,t-1})$.

Bordalo et al. (2020) assume that $f^r(\epsilon|S_{i,t-1}) = f(\epsilon|S_{i,t-1} \cup \{E_{i,t-j}^{NI}[\epsilon]\})$, i.e., the reference distribution is given by the Bayesian expectation using the information set of yesterday and the additional signal at that Bayesian mean. In other words, according to the reference distribution, $\epsilon \sim_{f^r(\epsilon|S_{i,t-1})} \mathcal{N}\left(E_{i,t-1}^{NI}[\epsilon], 1+\tau(t+1)\right)$.

Bardóczy and Guerreiro (2023) note that, to produce the empirical patterns of delayed overreaction, this model must be augmented with a long-memory component. To formalize this process, they assume that the reference distribution is such that $\epsilon \sim_{f^*(\epsilon|S_{i,t-1})} \mathcal{N}\left(\sum_{j=1}^{t} \alpha_j E_{i,t-j}^{NI}[\epsilon], 1+(t+1)\tau\right)$, for a set of memory weights $\alpha_j \geq 0$ for $j = 1, 2, \ldots$ and such that $\sum_{j=1}^{\infty} \alpha_j = 1$. This process generalizes the standard noisy-information diagnostic expectations process and allows for a longer period of overreaction to recent news.

Let the reference belief be a weighted average of past posteriors:

$$E_{i,t}^{r}[\epsilon] = \sum_{j=1}^{t} \alpha_j E_{i,t-j}^{NI}[\epsilon], \qquad \alpha_j \geq 0, \qquad \sum_{j=1}^{\infty} \alpha_j = 1. \tag{25}$$

The household's distorted expectation is

$$E_{i,t}[\epsilon] = E_{i,t}^{NI}[\epsilon] + \theta\left(E_{i,t}^{NI}[\epsilon] - E_{i,t}^{r}[\epsilon]\right), \tag{26}$$

where $\theta \geq 0$ governs the strength of diagnostic over-extrapolation.

Taking cross-sectional averages in (26) and using the linearity of the reference belief yields

$$\overline{E}_t[\epsilon] = (1+\theta)\overline{E}_t^{NI}[\epsilon] - \theta \sum_{j=1}^{t} \alpha_j \overline{E}_{t-j}^{NI}[\epsilon]. \tag{27}$$

Now substitute the noisy-information average from (24) at dates t and $t-j$:

$$\overline{E}_t[\epsilon] = (1+\theta)\frac{\tau(t+1)}{1+\tau(t+1)}\mathbb{E}_t[\epsilon] - \theta \sum_{j=1}^{t} \alpha_j \frac{\tau(t+1-j)}{1+\tau(t+1-j)}\mathbb{E}_{t-j}[\epsilon].$$

So,

$$\overline{E}_t[dX_{t+h}] = (1+\theta)\frac{\tau(t+1)}{1+\tau(t+1)}\mathbb{E}_t[dX_{t+h}] - \theta \sum_{j=1}^{t} \alpha_j \frac{\tau(t+1-j)}{1+\tau(t+1-j)}\mathbb{E}_{t-j}[dX_{t+h}].$$

Because we are considering the response to a deterministic date-0 innovation, the model-consistent path is known once the aggregate state at date t is fixed, so

$$\overline{E}_t[dX_{t+h}] = \left[(1+\theta)\frac{\tau(t+1)}{1+\tau(t+1)} - \theta \sum_{j=1}^{t} \alpha_j \frac{\tau(t+1-j)}{1+\tau(t+1-j)}\right]\mathbb{E}_t[dX_{t+h}],$$

which is exactly equation (9).

C.4. Solution method

We are interested in the first-order impulse responses of the economy in response to transfer shocks. To this end, we adopt the sequence-space Jacobian method of Auclert et al. (2021)

and Auclert, Rognlie, and Straub (2024, 2020). Let $d\mathbf{C}_g \equiv [dC_{g,0}, dC_{g,1}, dC_{g,2}, \ldots]'$ denote the sequence of average consumption changes for group $g \in \{v, nv\}$, and define $d\mathbf{Y}$, $d\mathscr{T}$, $d\mathbf{T}$, and $d\mathbf{T}^v$ analogously. Using this framework, we write the change in average consumption in group g as

$$d\mathbf{C}_v = \mathbf{M}_v^Y(d\mathbf{Y} - d\mathscr{T}) + \mathbf{M}_v^T \overline{\omega}_v d\mathbf{T} + \mathbf{M}_v^{T^v} \frac{1}{\mu_v} d\mathbf{T}^v \tag{28}$$

$$d\mathbf{C}_{nv} = \mathbf{M}_{nv}^Y(d\mathbf{Y} - d\mathscr{T}) + \mathbf{M}_{nv}^T \overline{\omega}_{nv} d\mathbf{T} \tag{29}$$

where $\overline{\omega}_v = \int_{i\in v} \omega_i di/\mu_v$ and $\overline{\omega}_{nv} = \int_{i\in nv} \omega_i di/\mu_{nv}$ denote the average targeting coefficients among veterans and non-veterans, respectively. Here $d\mathbf{T}^v$ denotes the aggregate (per-capita) veterans' fund disbursement, so the factor $1/\mu_v$ in the veteran consumption equation converts it to a per-veteran disbursement. The generalized sequence-space Jacobians $\mathbf{M}_g^X \equiv \left[\partial C_{g,t}/\partial X_h\right]_{t,h=0,1,2,\ldots}$ already incorporate expectations (see also Angeletos, Guerreiro, and Zhang, 2025).

In equilibrium, the change in aggregate output equals the change in aggregate spending, so

$$d\mathbf{Y} = \overline{\mathbf{M}}^Y(d\mathbf{Y} - d\mathscr{T}) + \overline{\mathbf{M}}^T d\mathbf{T} + \mathbf{M}_v^{T^v} d\mathbf{T}^v, \tag{30}$$

where $\overline{\mathbf{M}}^Y \equiv \mu_v \mathbf{M}_v^Y + \mu_{nv}\mathbf{M}_{nv}^Y$ and $\overline{\mathbf{M}}^T \equiv \mu_v \overline{\omega}_v \mathbf{M}_v^T + \mu_{nv}\overline{\omega}_{nv}\mathbf{M}_{nv}^T$ denote the economy-wide average Jacobians. The $\mathbf{T}^v$ term does not carry a bar because population-weighted aggregation cancels the $1/\mu_v$ in the veteran consumption equation: $\mu_v \cdot \mathbf{M}_v^{T^v} \cdot (1/\mu_v) = \mathbf{M}_v^{T^v}$.

Define $\mathbf{M}^Y \equiv \overline{\mathbf{M}}^Y$, $\mathbf{M}^\omega \equiv \overline{\mathbf{M}}^T$, and $\mathbf{M}^v \equiv \mathbf{M}_v^{T^v}$. The aggregate linear equilibrium can then be written as the fixed point

$$d\mathbf{Y} = \mathbf{M}^Y(d\mathbf{Y} - d\mathscr{T}) + \mathbf{M}^\omega d\mathbf{T} + \mathbf{M}^v d\mathbf{T}^v, \tag{31}$$

where $\mathbf{M}^Y$, $\mathbf{M}^\omega$, and $\mathbf{M}^v$ are the sequence-space Jacobians of aggregate demand with respect to disposable income, government transfers under targeting rule ω, and veterans' fund disbursements, respectively. We assume that the general-equilibrium multiplier operator $\mathscr{M}$ exists and satisfies $\mathscr{M}(\mathbf{I} - \mathbf{M}^Y) = \mathbf{I}$.

C.5. Proof of Lemma 1

Proof. The integrated government budget constraint is

$$B_t + \mathscr{T}_t + \mu_v P = (1+r)B_{t-1} + T_t + T_t^v.$$

Rearranging,

$$B_t = (1+r)B_{t-1} - \mathcal{T}_t - \mu_v P + (T_t + T_t^v).$$

Thus transfers enter the law of motion for debt only through the sum $T_t + T_t^v$.

Now substitute the tax rule (8),

$$\mathcal{T}_t = \mathcal{T} + \tau_d(B_{t-1} - B),$$

into the integrated budget constraint to obtain

$$B_t = \left(1 + r - \tau_d\right)B_{t-1} - \mathcal{T} + \tau_d B - \mu_v P + (T_t + T_t^v).$$

Given the initial condition B_{-1}, this recursion determines $\{B_t\}_{t\geq 0}$ uniquely as a function of transfers only through the consolidated payout sequence $\{T_t + T_t^v\}_{t\geq 0}$. Finally, since $\mathcal{T}_t$ is pinned down by the tax rule as a function of B_{t-1}, the tax sequence $\{\mathcal{T}_t\}_{t\geq 0}$ inherits the same property. Therefore both debt and taxes depend on transfers only through total consolidated payouts. ■

C.6. Proof of Proposition 1

Proof. Consider the two policies in Proposition 1. By assumption, they have the same aggregate transfer path, so $T_t = T_t^v$ for all t, together with the same targeting rule, $\omega_{i,t} = \omega_{i,t}^v$. Under a veteran payment, household i receives transfer income

$$\omega_{i,t}^v T_t^v,$$

whereas under a fiscal transfer that targets only veterans, household i receives

$$\omega_{i,t} T_t.$$

Since $T_t = T_t^v$ and $\omega_{i,t} = \omega_{i,t}^v$, the transfer term received by each household is exactly the same under the two experiments. Non-veterans receive zero under both policies because the common targeting rule is zero outside the veteran group. Hence, for every household and every history, the sequence of budget sets is identical under the two experiments.

On the financing side, Lemma 1 implies that taxes and debt depend only on total consolidated payouts. Since the two policies have the same total transfer path by assumption, they generate the same sequences $\{\mathcal{T}_t\}_{t\geq 0}$ and $\{B_t\}_{t\geq 0}$. Therefore all aggregate objects entering household problems are also the same across the two policies.

It follows that every household solves the same dynamic problem under the two experiments and therefore chooses the same consumption path state by state. Aggregating across households implies identical paths for aggregate consumption and, by market clearing, for output and every other aggregate equilibrium object. This establishes the equivalence result. ■

C.7. Differences Across Targeting Rules

Proposition 2 (Differences Across Targeting Rules)**.** *Fix a total-transfer path with* $\{T_t^{\nu}\}_{t\geq 0} = \{T_t\}_{t\geq 0}$*. Let* $d\mathbf{Y}_{\nu}$ *denote the equilibrium output path under veterans' payments, and let* $d\mathbf{Y}_{\omega}$ *denote the equilibrium output path under government transfers with targeting rule* ω*. Then*

$$d\mathbf{Y}_{\omega} = d\mathbf{Y}_{\nu} + \mathscr{M}(\mathbf{M}^{\omega} - \mathbf{M}^{\nu})d\mathbf{T}, \tag{32}$$

where $\mathscr{M}$ *is the general-equilibrium multiplier operator satisfying* $\mathscr{M}(\mathbf{I} - \mathbf{M}^{Y}) = \mathbf{I}$.

Equation (32) decomposes the difference between two transfer experiments into three objects: a general-equilibrium multiplier $\mathscr{M}$, which captures feedback from output to income and consumption; the gap in intertemporal marginal propensities to consume (iMPCs) across recipient pools, $\mathbf{M}^{\omega} - \mathbf{M}^{\nu}$; and the size and timing of the transfer path $d\mathbf{T}$. The intertemporal marginal propensities to consume $\mathbf{M}^{\omega}$ and $\mathbf{M}^{\nu}$ are matrices whose (s,t) entry is the partial-equilibrium consumption response at horizon s to a dollar of transfer paid at horizon t—computed under the broad targeting rule ω for $\mathbf{M}^{\omega}$ and the veterans-only rule ω^{ν} for $\mathbf{M}^{\nu}$. The iMPC matrix is a property of the household block: it depends on who receives the transfer, what fraction of those recipients are constrained, and how recipients smooth the resulting income over time. In a HANK model with binding constraints, the on-impact entries M_{tt} are large because constrained households spend the dollar immediately, and the matrix decays slowly as more households relax their constraints. The general-equilibrium multiplier $\mathscr{M}$ then scales up this partial-equilibrium consumption difference into the full output response, via the feedback from output to disposable income and back to consumption.

Equation (32) shows that any difference between veterans' payments and stimulus checks arises from how the dollars are distributed across households, summarized by the gap $\mathbf{M}^{\omega} - \mathbf{M}^{\nu}$. The size of the transfer and its dynamic path enter only through $d\mathbf{T}$, and the general-equilibrium feedback only through $\mathscr{M}$—and both objects are the same in the two experiments by construction.

The decomposition is useful because it reduces the question of equivalence to the value of iMPCs. Veterans' payments and a stimulus check generate the same aggregate output path if and only if $\mathbf{M}^{\omega} = \mathbf{M}^{\nu}$, i.e., if the iMPCs of the two recipient groups coincide. Therefore a suitably calibrated model—which matches the MPCs of veterans and normal households as well as the time-series evidence on the postwar payments multiplier—can be used to study stimulus checks.

C.8. Proof of Proposition 2

Proof. The proof follows from the fact that under a veterans' transfer output solves

$$d\mathbf{Y}_v = \mathbf{M}^Y(d\mathbf{Y}_v - d\mathcal{T}) + \mathbf{M}^v d\mathbf{T}^v$$

and under a fiscal transfer with targeting rule ω it is given by

$$d\mathbf{Y}_\omega = \mathbf{M}^Y(d\mathbf{Y}_\omega - d\mathcal{T}) + \mathbf{M}^\omega d\mathbf{T}.$$

Using Lemma 1, we have that $d\mathcal{T}$ is the same under both policies, so we can write

$$d\mathbf{Y}_\omega - d\mathbf{Y}_v = \mathbf{M}^Y(d\mathbf{Y}_\omega - d\mathbf{Y}_v) + (\mathbf{M}^\omega - \mathbf{M}^v)d\mathbf{T}.$$

Rearranging, we obtain

$$(\mathbf{I} - \mathbf{M}^Y)(d\mathbf{Y}_\omega - d\mathbf{Y}_v) = (\mathbf{M}^\omega - \mathbf{M}^v)d\mathbf{T},$$

$$d\mathbf{Y}_\omega - d\mathbf{Y}_v = \mathcal{M}(\mathbf{M}^\omega - \mathbf{M}^v)d\mathbf{T},$$

which is the desired result. ■

C.9. Veteran Transfers, Stimulus Check Equivalence Result

This appendix records the special case in which veterans' payments are exactly equivalent to a broad stimulus check. The result is not used in the main paper because the quantitative model allows veterans and non-veterans to differ in patience and wealth, but it clarifies why recipient composition is the only source of nonequivalence.

Proposition 3 (Exact Equivalence with Homogeneous Household Problems)**.** *Suppose that households have common homothetic preferences and common discount factor $\beta_g = \beta$, the idiosyncratic income process is common across groups, and the veteran borrowing limit is adjusted for the capitalized premium:*

$$\underline{a}_v = \underline{a}_{nv} + \frac{1}{(\overline{e}_v)^{1-\gamma}}\frac{P}{r}.$$

Suppose also that veteran payments and stimulus checks use the same within-state targeting ker-

nel,

$$\omega^v_{i,t} = 1_{\{g=v\}} \frac{e^{\phi}_{i,t}}{\mu_v \mathbb{E}_\pi[e^\phi]}, \qquad \omega_{i,t} = \frac{e^{\phi}_{i,t}}{\mathbb{E}_\pi[e^\phi]},$$

for some ϕ. The untargeted stimulus check is the special case $\phi = 0$. Then, for any common aggregate transfer path $d\mathbf{T} = d\mathbf{T}^v$, the aggregate output response to the stimulus check equals the response to veterans' payments:

$$d\mathbf{Y}_\omega = d\mathbf{Y}_v.$$

Proof. Let $s_g \equiv (\overline{e}_g)^{1-\gamma}$ and define normalized variables

$$\tilde{c}_{i,t} = \frac{c_{i,t}}{s_g}, \qquad \tilde{a}_{i,t} = \frac{a_{i,t} + 1_{\{g=v\}} P/r}{s_g}.$$

The household budget constraints divided by s_g take the same form across groups:

$$\tilde{c}_{i,t} + \tilde{a}_{i,t} = \theta_{i,t}(Y_t - \mathcal{T}_t) + (1+r)\tilde{a}_{i,t-1} + \text{normalized transfer}_{i,t}.$$

The shift by P/r removes the constant veteran premium because $(1+r)P/r = P/r + P$. The borrowing-limit assumption implies a common normalized borrowing constraint, $\tilde{a}_{i,t} \geq -\underline{a}_{nv}$, for both groups. With common preferences and the common idiosyncratic process, veterans and non-veterans therefore have the same normalized consumption rule.

Let $D_{t,s}$ be the derivative of normalized average consumption at date t with respect to one unit of normalized transfer at date s under the common within-state targeting kernel e^ϕ. A veteran payment T^v_s is received only by veterans, so the average veteran receives T^v_s/μ_v and the aggregate iMPC is

$$M^v_{t,s} = \mu_v \left(\frac{1}{\mu_v} D_{t,s} \right) = D_{t,s}.$$

A stimulus check with the same kernel is paid to the whole population; each group has the same average response $D_{t,s}$, so

$$M^f_{t,s} = \mu_v D_{t,s} + (1-\mu_v) D_{t,s} = D_{t,s}.$$

Hence $\mathbf{M}^\omega = \mathbf{M}^v$. Applying Proposition 2 gives $d\mathbf{Y}_\omega = d\mathbf{Y}_v$. ■

C.10. Expectations in the data and model

This appendix conducts a validation exercise for the expectations component of the model. The exercise does not test the full HANK economy and does not use other equilibrium conditions. Instead, it takes the expectations model estimated in the main analysis and applies it to an external empirical path.

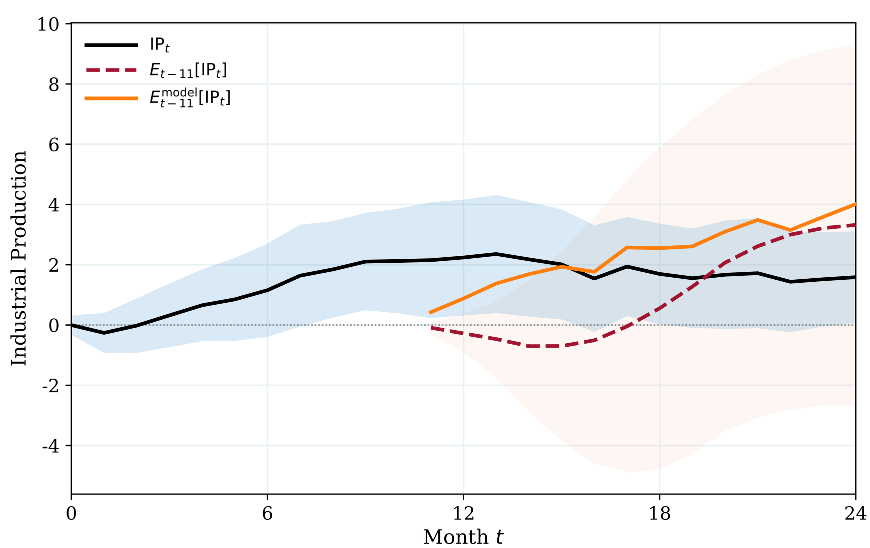
The object is industrial production. We use industrial production because it is the aggregate variable for which we can obtain expectations data over the historical period studied in the paper, although it does not map directly to a model object. Specifically, we use 12-month-ahead industrial production expectations from the Livingston Survey alongside the realized industrial production. Because the Livingston Survey is only available every six months, we obtain monthly expectations by combining this information with monthly industrial production using the Chow–Lin procedure.

Let IP_t denote the estimated empirical response of industrial production t months after the transfer shock. Equation (9) implies that the estimated expectations process maps the model-consistent response of an aggregate variable into average subjective expectations by a horizon-specific coefficient. Let λ_s be this coefficient at forecast date s, computed using the expectation parameters (θ, τ, a, b) from the full model estimated on transfer and consumption IRFs. We then construct the model-implied expectation of industrial production as

$$E_{t-11}^{model}[IP_t] = \lambda_{t-11} \cdot IP_t, \qquad t = 11, \ldots, 24.$$

Thus, the only new input in the validation is the empirical path of industrial production itself. We compare the model-implied path of expectations constructed in this way to their estimated empirical pattern, $E_{t-11}[IP_t]$.

Figure C.2: Industrial Production Expectations: Data and Model



Notes: The solid black line is the empirical response of industrial production. The dashed red line is the empirical expectation series $E_{t-11}[IP_t]$. The dash-dotted line is the model-implied expectation series obtained by applying the expectation operator estimated in the full transfer-consumption IRF exercise to the empirical industrial-production path. Industrial production and industrial-production expectations are not used in that estimation.

Figure C.2 reports the results. Expectations adjust little on impact and remain essentially unchanged for the first six months, even as realized industrial production begins to rise. They increase only gradually thereafter. The delayed adjustment is followed by some overshooting: after about one year, expected industrial production rises by more than realized production. The model-implied expectation series closely follows the observed expectations data: it captures the muted initial response of expectations, and the subsequent overreaction of expectations relative to the realized path of industrial production. This match is informative because the expectation series is not targeted in the full estimation. The evidence, therefore, provides a validation of the estimated belief process.

C.11. Investment Adjustment Costs as an Alternative Mechanism

Investment adjustment costs alone do not reproduce the hump-shaped response of consumption to a transitory transfer shock. Even when they generate a delayed investment response under full-information rational expectations (FIRE), household consumption peaks on impact. To evaluate this mechanism, we extend the baseline model by adding capital accumulation and convex investment adjustment costs. The household, veterans' fund, fiscal, and wage-setting blocks remain as in the baseline model. Households may form imperfect expectations, while firms, capital owners, unions, and the fiscal authority retain model-consistent expectations.

Investment extension. Aggregate output is produced using predetermined capital and current labor,

$$Y_t^{GDP} = AK_{t-1}^{\alpha} N_t^{1-\alpha}, \qquad \alpha \in (0,1), \tag{33}$$

where K_{t-1} is installed capital at the beginning of date t. Flexible product markets imply

$$w_t N_t = (1-\alpha) Y_t^{GDP}, \qquad R_t^K K_{t-1} = \alpha Y_t^{GDP}, \tag{34}$$

where R_t^K denotes the rental rate of capital. A separate class of capital owners owns the capital stock, rents it to firms, finances investment, and consumes the residual capital income. Capital evolves according to

$$K_t = (1-\delta) K_{t-1} + I_t, \qquad \delta \in (0,1), \tag{35}$$

and the date-t resource cost of investment is

$$I_t \left[1 + S\left(\frac{I_t}{I_{t-1}} \right) \right], \qquad S(x) = \frac{\varphi_I}{2}(x-1)^2, \tag{36}$$

where $S(1) = S'(1) = 0$ and $S''(1) = \varphi_I > 0$. Capital-owner consumption is therefore

$$C_t^K = R_t^K K_{t-1} - I_t \left[1 + S\left(\frac{I_t}{I_{t-1}}\right)\right]. \tag{37}$$

Capital owners choose investment and capital to maximize

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \mathcal{Q}_{0,t} C_t^K$$

subject to equations (35) and (37), where $\mathcal{Q}_{0,0} = 1$ and $\mathcal{Q}_{0,t+1} = \mathcal{Q}_{0,t}\mathcal{Q}_{t,t+1}$.

Let q_t^K denote the marginal value of installed capital and let $\mathcal{Q}_{t,t+1} = 1/(1+r_t)$ denote the one-period discount factor. The investment Euler equation and the Tobin's Q recursion are

$$1 + S\left(\frac{I_t}{I_{t-1}}\right) + \frac{I_t}{I_{t-1}} S'\left(\frac{I_t}{I_{t-1}}\right) = q_t^K + \mathbb{E}_t\left[\mathcal{Q}_{t,t+1}\left(\frac{I_{t+1}}{I_t}\right)^2 S'\left(\frac{I_{t+1}}{I_t}\right)\right], \tag{38}$$

$$q_t^K = \mathbb{E}_t\left[\mathcal{Q}_{t,t+1}\left(R_{t+1}^K + (1-\delta) q_{t+1}^K\right)\right]. \tag{39}$$

All expectations in these two equations are model-consistent. Thus, investment adjustment costs introduce an additional source of real propagation without directly changing the household expectation process.

Quantitative implementation. The object entering the heterogeneous-household block is aggregate labor income, $Y_t^L \equiv w_t N_t$, rather than GDP. Equation (34) gives the mapping

$$Y_t^{GDP} = \frac{Y_t^L}{1-\alpha}, \qquad R_t^K K_{t-1} = \frac{\alpha}{1-\alpha} Y_t^L. \tag{40}$$

Capital income is not passed to the transfer-receiving household sector: it accrues to capital owners, finances investment and adjustment costs, and leaves residual consumption C_t^K. To preserve the empirical consumption target used in the baseline estimation, we compare PCE with $C_t^H \equiv \int c_{i,t}\, di$, the consumption of the transfer-receiving household sector. The separate capital-owner payout C_t^K closes the resource constraint but is not included in the model counterpart to empirical PCE. Goods-market clearing is

$$Y_t^{GDP} = C_t^H + C_t^K + I_t\left[1 + S\left(\frac{I_t}{I_{t-1}}\right)\right].$$

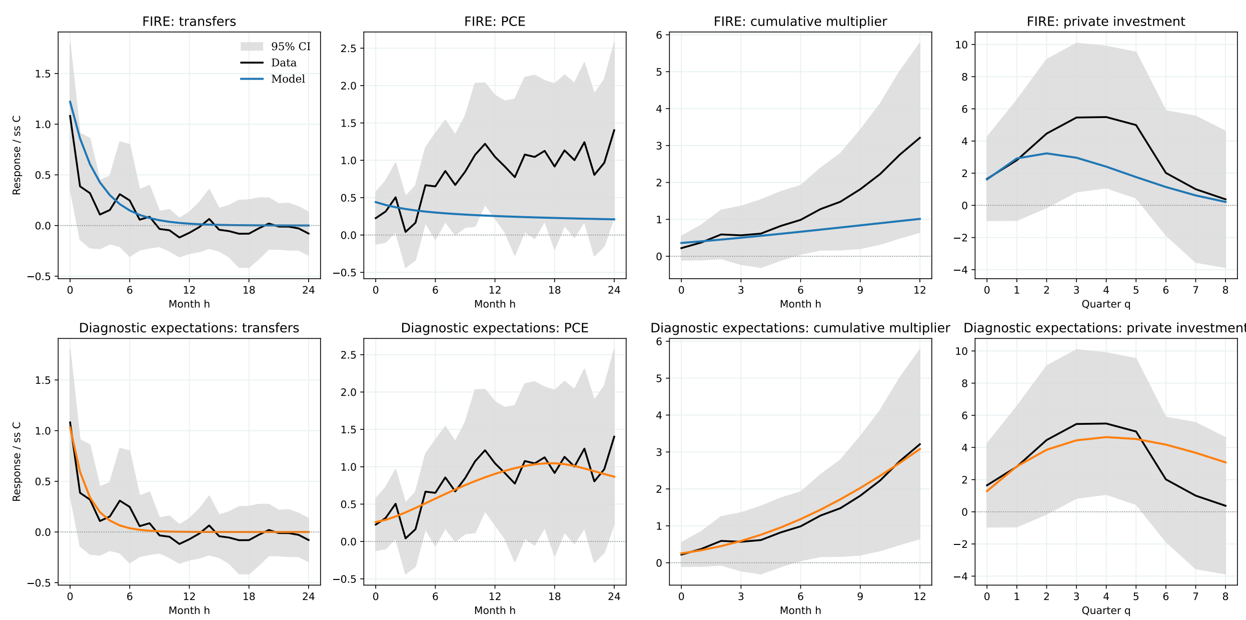
Calibration. The model is solved at a monthly frequency. Following Auclert, Rognlie, and Straub (2020), we set the capital share to $\alpha = 0.24$ and annual depreciation to $\delta^{ann} = 0.053$, converted to a monthly rate using $\delta = 1 - (1-\delta^{ann})^{1/12}$.

Estimation. We estimate the adjustment-cost curvature φ_I jointly with the transfer process under FIRE, and jointly with the transfer and expectation parameters under diagnostic expectations. The estimation targets four blocks: the monthly responses of transfers and PCE over horizons $0,\ldots,24$, the directly estimated cumulative multiplier over horizons $0,\ldots,12$, and the quarterly response of private investment over quarters $0,\ldots,8$. To align the monthly model with the quarterly investment target, we use

$$\widehat{d\log I}_q^Q = \frac{dI_{3q} + dI_{3q+1} + dI_{3q+2}}{3I}, \qquad q = 0,1,\ldots,8. \tag{41}$$

The four target blocks are combined in an inverse-variance-weighted nonlinear least-squares criterion. Because this joint exercise adds the multiplier and investment targets, its fit statistics are not directly comparable to those from the baseline estimation, which targets only transfers and PCE.

Figure C.3: Investment Adjustment Costs and Model Fit



Notes: The top row reports the investment-augmented FIRE model, and the bottom row reports the investment-augmented diagnostic-expectations model. Black lines are empirical estimates, blue lines are FIRE model fits, orange lines are diagnostic-expectations model fits, and gray regions are empirical 95% confidence intervals. Transfers and PCE are monthly and expressed relative to steady-state household consumption. Cumulative-multiplier horizons are monthly, and the units are the cumulative dollar response of PCE per dollar of additional transfers. Private investment is quarterly and expressed as a percent deviation from steady state.

Results. Figure C.3 reports the joint estimates. The estimated adjustment-cost curvature is 0.09 under FIRE and 1.51 under diagnostic expectations. The investment-augmented FIRE model can generate a delayed investment response, but its fitted consumption response peaks on impact and then declines. It also substantially understates the cumulative multiplier. Investment adjustment costs alone therefore do not account for the hump-shaped consumption response in the data. By contrast, the investment-augmented model with diagnostic expectations captures the delayed rise in consumption while also fitting the transfer, multiplier, and investment responses. Its weighted RMSE for the joint target set is 0.52, compared with 1.09 under FIRE. This comparison is descriptive rather than a formal model-selection test because the diagnostic-expectations specification has additional free parameters.

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