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

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Abstract

Do stimulus checks—one-off, deficit-financed and lump-sum payments from the government to households—boost the economy? We study a natural experiment: payments to U.S. veterans in the post-World War II period that mimic stimulus checks but were plausibly exogenous to the economy. With newly digitized data, we show that these payments led to a temporary increase in transfers and a large, persistent increase in consumption. The transfer multiplier—the cumulative response of consumption to transfers—is 1 after six months and over 2 after a year. We calibrate a heterogeneous-agent New Keynesian model to match our estimates. The standard model cannot match the persistent consumption response. But a version with imperfect expectations can, with households underreacting on impact but overreacting at longer horizons.

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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 worked. 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).

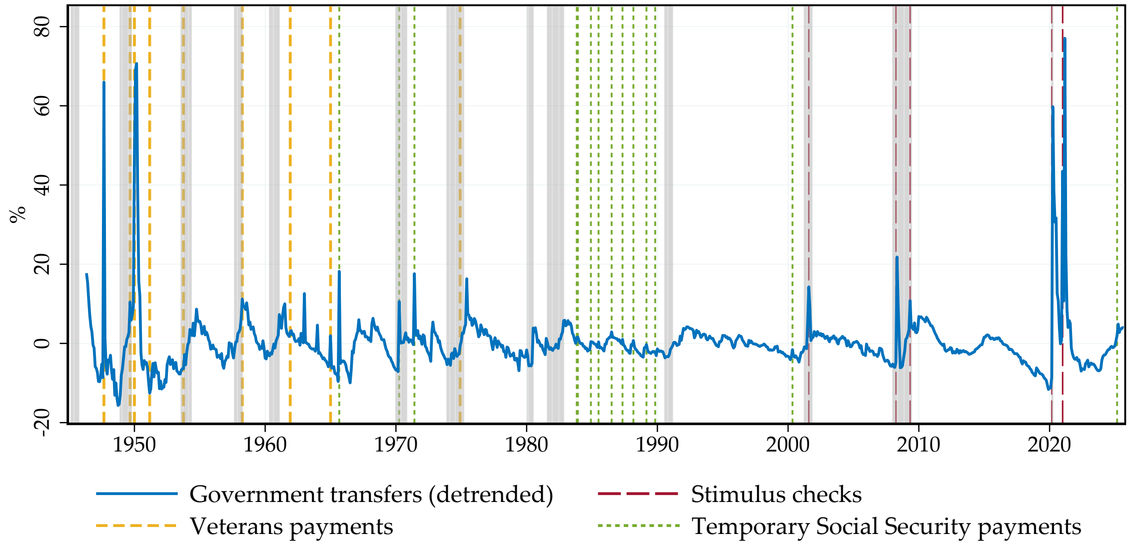
There is surprisingly little direct evidence on the macroeconomic effect of stimulus checks. Many papers study how individual households respond to stimulus checks and related shocks, often finding sizable spending responses (e.g. Parker, Souleles, Johnson, and McClelland, 2013). But these studies hold 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.[1]

This paper studies a natural experiment that isolates large transfer payments, which 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 not made 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. We provide a detailed narrative account of postwar veterans' payments,

[1] There is more evidence on the aggregate effects of other kinds of fiscal policy, such as government spending and tax shocks (e.g. Romer and Romer, 2010; Mertens and Ravn, 2013a; Ramey and Zubairy, 2018). But these estimates are not informative about stimulus checks, which in theory operate through different mechanisms.

Figure 1: Transfers and Veterans' Payments



Notes: The solid line is monthly deviation of log real government transfers, measured with a Hodrick-Prescott filter with smoothing parameter 129,600 (Ravn and Uhlig, 2002). Vertical markers indicate postwar veterans' payments and temporary, exogenous Social Security transfer shocks based on Romer and Romer (2016) and extended through our own narrative analysis; shaded bars are NBER recessions. Stimulus-check months are also marked. The sample runs from 1945 to 2025.

identifying the exact months in which lump-sum transfers were disbursed. To increase statistical power, we augment these episodes with a second, less powerful series of transfer shocks: the temporary and plausibly exogenous Social Security payments identified by Romer and Romer (2016).[2]

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 newly assembled data on retail sales—separately for durables and nondurables—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.

As motivating evidence, we begin with a case study of the large January 1950 payment. We

[2] As we shall see, these transfer shocks—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.

examine a six-month window on either side of the disbursement. Contemporary accounts indicate no other major macroeconomic shocks during this period, which ends with the outbreak of the Korean War in late June. Prior to January, both transfers and economic activity are stable. 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. The sharp nature of the episode—a discrete spike in transfers, followed by an immediate reversal and no competing macroeconomic disturbances—suggests that the subsequent rise in consumption was driven by the payment itself. We also present corroborating micro-evidence, from 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.

We then estimate the macroeconomic effects of transfers by exploiting the full set of plausibly exogenous temporary transfer payments. 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 timing of postwar payments is unrelated to macroeconomic developments. We support this argument 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 transfers on the macroeconomy, controlling for 12 lags of the outcome and the treatment, and a rich set of macroeconomic variables.

The postwar payments constitute a temporary transfer shock with large and persistent real effects. 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 only modestly, and the ex post short-term real interest rate remains essentially unchanged. We summarize 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 more than 2 after one year.

We probe the identification assumption 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. Finally, a jackknife exercise that sequentially excludes one shock at a time leaves the estimates virtually unaffected. We also show that including the veterans' payments is crucial. Using only the temporary Social Security shocks from Romer and Romer (2016) yields noisy and imprecise estimates.

We close the paper by setting up a model in order to answer two questions. First, what is the relationship between the postwar payments multiplier and the more familiar stimulus checks multiplier? Second, what sort of model can match our finding of a large, persistent response to a temporary shock? We start with a standard heterogeneous agent New Keynesian (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 dynamics of the consumption response. We assume that households form forecasts of aggregate income using a diagnostic-expectations framework with noisy information (Bordalo, Gennaioli, Ma, and Shleifer 2020; Bardóczy and Guerreiro 2023). This specification can capture a central feature of observed expectations: following a shock, expectations initially underreact, but subsequently overshoot realized outcomes (Angeletos, Huo, and Sastry 2021; Auclert, Monnery, Rognlie, and Straub, 2026).

The first result from the model is that the postwar payments multiplier and the stimulus checks multiplier 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. The implication is that a suitably calibrated model—which matches the postwar payments evidence—can be used to study stimulus checks.[3]

We calibrate the model to match the marginal propensity to spend of veterans and the liquid wealth of veterans and non-veterans, using the 1951 *Survey of Consumer Finances*. We then discipline the aggregate expectations parameters by choosing them to match the empirical impulse response of consumption, following Christiano, Eichenbaum, and Evans (2005). We compare this calibration to a benchmark version of the model with full-information rational expectations (FIRE).

The second result of 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. Under FIRE, the model matches the initial rise in consumption but predicts that consumption peaks on impact and subsequently declines. In the data, by contrast, consumption continues to in-

[3]Monetary policy and the fiscal rule for debt issuance must also be the same for stimulus checks and veterans' payments.

crease 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.

Imperfect expectations creates a general equilibrium force that leads consumption to gradually rise. 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. This paper joins the half-century-old quest asking 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, meaning consumption is 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; Fagereng, Holm, and Natvik, 2021; Boehm, Fize, and Jaravel, 2025). By construction, this approach does not identify the aggregate response to the payment—the familiar “missing intercept” problem (Wolf, 2023). This paper takes a different approach: studying macro-level shocks that mimic stimulus checks.[4]

Ramey (2019) observes that there “has been very little work on the aggregate effects of transfers”. We now turn to the few papers on this topic. As discussed above, 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. Only when com-

[4] 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).

bined with the postwar veterans' payments do we obtain enough statistical power to estimate the effects of temporary transfers.

A second important paper is Hausman (2016), who, like us, studies payments to veterans—in his case, the Veterans' Bonus of 1936. The primary focus is an estimate of the marginal propensity to consume, using novel historical micro data. The paper does also examine the aggregate effects of the stimulus, but tentatively, because there is a single aggregate observation. We make progress by studying a time series of payments to veterans.

A third group of papers focuses on the 2008 stimulus checks and other related episodes (Orchard, Ramey, and Wieland, 2025; Ramey, 2025; Orchard, Ramey, and Wieland, 2026). These papers emphasize that during stimulus check episodes, consumption often changes little even as income spikes. The counterfactual—what would have happened to consumption absent stimulus checks—is hard to know, and the authors construct the counterfactual with a series of models. We study stimulus check-like payments that happened quasi-randomly, for reasons that were not to do with recessions. Therefore we can estimate the macroeconomic effect of stimulus checks without a model-based counterfactual.[5]

Our findings offer an empirical benchmark for the calibration of heterogeneous agent models—given that in these models, temporary transfers can generate sizable aggregate effects (Kaplan, Moll, and Violante, 2018; Auclert, Rognlie, and Straub, 2024; Bilbiie, 2025). We argue that for HANK models to match the transfer multiplier, they 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).[6] Our approach is closest to Eichenbaum, Guerreiro, and Obradovic (2025), who also study the impact of deviations from rational expectations for the transfer multiplier. The Ricardian Non-Equivalence effect they emphasize is also present in our baseline model. We emphasize an additional source of persistence from the delayed overreaction of expectations.

Outline. The paper proceeds as follows. Section 2 introduces the institutional setting, the new monthly dataset, and the narrative discussion. Section 3 outlines the empirical strategy and reports the main estimates. Section 4 develops a theoretical framework to interpret the estimates. Section 5 concludes.

[5] 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., Mertens and Ravn, 2013a; Fieldhouse, Mertens, and Ravn, 2018; Fieldhouse and Mertens, 2024; Cloyne, 2013; Cloyne and Surico, 2017).

[6] In doing so, we also contribute to a growing literature studying behavioral HANK models, including Farhi and Werning (2019), Auclert, Rognlie, and Straub (2020), Farhi, Petri, and Werning (2020), Pfäuti and Seyrich (2022), Bardóczy and Guerreiro (2023), Guerreiro (2023) and Angeletos, Guerreiro, and Zhang (2025).

2. Setting and Data

This section begins by introducing new monthly data on government transfers dating back to 1945. We then provide historical background on the large payments made to veterans in the late 1940s and subsequent decades in the United States. Next, we discuss which of these postwar payments can plausibly be viewed as exogenous to business cycle conditions. We then introduce the temporary Social Security payments that complement the veterans' payments and increase statistical power. Finally, we present evidence on the properties of the transfer shock series.

2.1. New Data on Transfers

The data requirements for this project are demanding. Estimating the effects of transfers on the postwar economy requires monthly data extending back to 1945. 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 taken here; for the full details, see Appendix A.1. While the necessary transfers data is not available in digital form, most components of transfers are available at monthly frequency in historical publications from the United States Treasury. Moreover, a quarterly frequency transfer series (specifically, the National Income and Product Account, NIPA, series "personal current transfer receipts: government social benefits to persons") is available back to 1947, and an annual transfer series is available 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.[7] 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 shocks we exploit. The broad measure additionally includes unemployment insurance.

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

To ensure consistency, we take two steps. First, we rescale the monthly series so that the quarterly sum of the monthly series equals the publicly available quarterly series. Second, we assume that the components of aggregate transfers that are not reported in the historical tables were paid in equal sizes in each month of the quarter (in the absence of better information).

We next assemble monthly information on retail spending. We combine three datasets. First, there is already a dataset for total retail spending available from Romer and Romer (2016), from 1947-1991, sourced from the *Survey of Current Business Statistics*. We extend this dataset backwards using earlier vintages of the same survey, to 1935. We extend the dataset forwards to 2020, using datasets from the Census Bureau (Advanced Monthly Retail Trade Survey). These data enable a classification of retail spending into either durable or non-durable goods. Appendix A.2 contains more information on how we create these series.

We combine our monthly dataset with various standard monthly variables that are readily available: industrial production, non-farm employment, and consumer prices. All of the dataset is seasonally adjusted, either as provided, or using the X-11 algorithm.

Overall, the maximum time series available is from the start of 1935 until the end of 2025. Two useful variables are unavailable at monthly frequency over our full sample. First, consumption (personal consumption expenditures) is only available at monthly frequency since January 1959. We extend this back through a Chow-Lin procedure using quarterly consumption and monthly retail spending. We describe our data cleaning procedure in Appendix Section A.3.

Finally, we make use of micro data. Remarkably, the 1951 *Survey of Consumer Finances* asked questions about what veterans did with their payment in 1950. The unit of observation is the spending unit (i.e., a household). The survey question recorded whether the spending unit received the payment, how much it received, and how the respondent said the money was used. There is also additional micro data on veterans from the *Survey* that we will use. Appendix Section A.4 describes our procedure for processing the *Survey*.

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 guaranteed payout to designated 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. Many veterans retained their policies after the war, which the program encouraged.

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 Treasuries with the rest of the government, in return receiving the face value in cash. The rest of the government would have to raise more debt, or increase taxes, to fund the cash.

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. However, these projections—particularly those concerning mortality—proved overly pessimistic (Board of Governors of the Federal Reserve System, 1949; U.S. Department of Veterans Affairs, 2021). Therefore, the fund had a surplus. Legislation required that if the fund were too large, the excess 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 in part because so many had served in WWII, with veterans comprising 35% of all adult males (Vespa, 2020). However, individual payments were also large. The typical veteran received around 170 dollars, or around 2,500 dollars as of 2025 (U.S. General Accounting Office, 1951).

The January 1950 dividend was the first payment to WWII veterans out of the life insurance fund. However, there were several more of these large, one-off "special dividends", designed to eliminate the surplus. 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 over time because from 1952 onwards, the administration introduced a mechanism designed to prevent so many large payments. Veterans would receive "regular dividends" on the anniversary of their enrollment date.

These regular dividends were disbursed continuously throughout the year, and do not show up as "spikes" in the time series of aggregate government transfers. 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, instead of evenly distributed throughout the year. Dividends out of government life insurance funds became less common from the Vietnam War onwards, at which point the life insurance fund used any excess to lower premia (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). As part of their contracts, service members received vacation during WWII, but many were unable to take all of it. After the war, the government owed veterans money for the leave not taken. In 1946, the government paid for this leave not with cash, but with bonds. Initially, these bonds were not allowed to be redeemed until five years after they had been issued. However in September 1947, the Truman administration allowed veterans to convert the bonds to cash. The scale of these payments was also enormous, at 3.3% of quarterly GDP—hardly smaller than the largest life insurance dividend (U.S. Department of the Treasury, 1949). A second, smaller, one-off payment was made to veterans in December 1974. In December, 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 in the previous quarter. 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?

Our identification strategy will exploit variation in the timing of veterans' payments, rather than variation in their size. The key requirement is that payment dates were not systematically related to other forces affecting the economy.

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 One insurance dividend	Sep. 1949	$40 million	0.06%	Date: Sullivan Daily Times (1949); Size: U.S. Department of Veterans Affairs (2021)
World War Two insurance dividend	Jan. 1950	$2.9 billion	4.12%	Date and size: U.S. Department of Veterans Affairs (2021)
World War Two insurance dividend	Mar. 1951	$685 million	0.81%	Date and size: U.S. Department of Veterans Affairs (2021)
World War One insurance dividend	Oct. 1953	$64 million	0.07%	Date and size: The Fayette County Record (1953)
World War One 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 Two 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.

To classify which payments can be treated as exogenous, 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 the timing of each payment reflected administrative or institutional factors rather than contemporaneous macroeconomic conditions.

January 1950 special dividend. We start by discussing the largest shock, the dividend to Second World War veterans paid in January 1950. Contemporary accounts are clear that the main reasons for the timing were 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). In that speech, Truman noted that "*[t]his dividend cannot be paid, however, until the financial liabilities of the fund and its legal status are determined ... [t]he Veterans'*

Administration is gradually catching up on the processing of the basic insurance records."

The process of figuring out both the legal status and the basic paperwork would take a long time. 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 Veterans' Administrator Harold Breining saying that the payments could take as long as July 1950 to arrive (St. Louis Star and Times, 1949). Part of the delay was because the veterans themselves had to apply for the dividend, and their application had to be cross-checked with Veterans' Administration records. One imagines the scale of the task without modern technology. Breining alludes to the "*printing and distribution of about 80,000,000 application forms. The forms are to be mailed probably in late August or early September to about 15,000,000 servicemen and women*" (St. Louis Star and Times, 1949). These administrative issues dictated when the dividend was to be sent out. For instance, in an October 9th interview with the Atlanta Daily World, 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, which calls for the first checks to be dispatched sometime around the middle of January*" (Atlanta Daily World, 1949).

March 1951 special dividend. The next dividend out of the World War II life insurance fund, paid in March 1951, again appears to have been determined by administrative lags. It had taken so long to process and pay the first dividend that, by the time the payment was over, a second dividend was due. The first dividend made payments on the basis of the excess in the fund as of January 1948, when Truman had made the original Budget speech to Congress. However, over the following three years, a further excess had built up. This excess had to be disbursed too. For instance, the M-29 Insurance User Manual writes that "*[t]his dividend was based on premiums paid from the policy anniversary date in 1948 (or effective date if the policy was issued between 1948 and 1950, both dates inclusive) to the policy anniversary date in 1951.*" 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). The process to finalize the paperwork began in December 1950, before dividends were mailed the following March.

Armed Forces Leave Bond. The Armed Forces Leave Bond payment—the payment for unused leave during the Second World War—was initially scheduled to be made in 1951, 5 years after the bonds had initially been created. The timing was changed to September 1947 due to political pressure by Congress to pay long-suffering veterans what they were owed. To add insult to injury, officers had been paid their leave at the end of the war, but enlisted men had not.

The payment was made over the objections of a Truman administration that did not wish for macroeconomic stimulus. These facts are clear from debate in the House of Representatives and the Senate, and the Presidential speech as the payments were made. For instance, in the House of Representatives debate on passing the bill altering the timing, Republican Congressman Blackney from Michigan said "*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). Similarly, at the Senate Armed Forces Committee hearing on the bill, Democrat Senator Pepper from Florida spoke of justice for veterans and obstruction by the Presidency. He said "*[i]t is certainly not fair to permit officers to receive their terminal leave pay in cash and yet deny the ordinary GI the same right ... we do not understand the position of the Treasury Department and the Bureau of the Budget on this bill.*" When President Truman signed the bill, in July 1947, he made a last effort to discourage the spending that would result, 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 ... [i]f a sizable proportion of these bonds should be redeemed in the near future, general inflationary pressures, which we have been endeavoring to control, would receive a substantial boost*" (Truman, 1947).

September 1949 special dividend. The timing of this payment—which was for World War One veterans—again appears to be determined by administrative considerations, as well as paying veterans their entitlement. In his Annual Budget Message of 1948, when President Truman discusses making payments out of the World War One fund, Truman refers to the "*substantial accumulations"* in the fund as the sole reason for making the payments (Truman, 1948). In contemporary accounts discussing the reason for the disbursement, the only motivation discussed is "*shar[ing] insurance money with veterans ... because premiums paid were higher than necessary to offset actual deaths*" (Kent Stater, 1949).

December 1961 special dividend. The timing of this payment, made to veterans of the Korean War, was likely due to political delays. There was no straightforward mechanism to pay out dividends from the Korean War insurance fund. Therefore, legislation had to be passed in order to make the dividend happen (U.S. Senate Committee on Finance, 1961). The legislation took almost a year to pass. The bill, HR 4539 "special dividends for certain NSLI policy holders" was first introduced into the House of Representatives on 17th February 1961. The bill was passed in the House on 21 March 1961. The bill passed the Senate on 1 September 1961, and was signed into law (becoming Public Law 87-223) on 13 September 1961 (U.S. Congress, 1961). With the usual delays, the payments were made the following December.

December 1974 veterans' compensation. These payments occurred in December due to legislative delays. Congress had timed the payments to be made in September, to coincide with the "*academic year beginning on or about September 1, 1974*" (recall that the payments were for education benefits). Likewise, 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, due to President Ford's veto, the payments were only sent out in December (United States Congress, 1974). More broadly, the motivation for these payments appears to have been the ongoing difficulty that Vietnam War veterans had reintegrating into society. 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).

October 1953 and April 1958 special dividends. To our knowledge, there is no contemporary discussion of the timing 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 dropping these events.[8]

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. The most recent fiscal stimulus, the Revenue Act of 1964 that famously constituted President Kennedy's tax cut stimulus, took place an entire year earlier. 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 Depression, however, veterans pressed for early payment. After several years of lobbying and political

[8] 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.

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, February 1977, and June 1992) coincided with a recession, as well as other fiscal stimulus.

2.4. Social Security Payments

To increase statistical power, we augment the postwar veterans' payments 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. In the same spirit as our approach for veterans' payments, they use detailed historical evidence to isolate variation in transfers that is plausibly exogenous to macroeconomic conditions. These shocks are smaller than the veterans' payments, 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. Importantly, they exclude benefit increases that were explicitly undertaken for short-run macroeconomic stabilization.

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. These episodes generate discrete, transitory increases in household income that are not tied to contemporaneous economic conditions. Like the veterans' payments we identify, they closely resemble stimulus checks: they are one-off, lump-sum payments that are largely deficit-financed. We incorporate the subset of Romer and Romer (2016) events corresponding to these exogenous, temporary payments.

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. The first payment is the Senior Citizens' Freedom to Work Act of May 2000, which induced a one-off payment of approximately \$1.6 billion (0.06% of GDP). 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 May, 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 payment is the Social Security Fairness Act of March 2025. 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.

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 postwar veterans' payments 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.

Taken together, the veterans' and temporary Social Security payments provide a unified series of stimulus-check-like transfer shocks. While the Social Security events are smaller and less informative, combining them with the veterans' payments increases statistical power and makes the estimates more robust to the influence of any single episode.

2.5. Diagnostics of the Narrative Transfer Shock Series

The narrative transfer shocks pass a series of common diagnostic checks. The corresponding figures and tables are reported in Appendix B.1. Most importantly, transfer shock 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 series also does not appear to proxy for other well-known macroeconomic disturbances. We correlate the transfer shocks to military spending shocks, tax shocks, monetary policy shocks, and oil supply shocks. Across specifications, the series is essentially orthogonal to these alternative shocks, consistent with it capturing an independent source of variation in disposable income.

Finally, we analyze the time-series properties of the shock series itself. The narrative transfer shocks exhibit some autocorrelation. This feature partly reflects the binary nature of the shock series and the relatively small number of events, which tend to cluster in certain decades. In our empirical specifications, we therefore include lags of the shock to control for any serial dependence.

3. Estimates: the Macroeconomic Effect of Postwar Payments

How do transfers affect the macroeconomy? This section starts with a motivating case study: the largest payment in our shock series, which took place in January 1950. We then explain our identification strategy and main specification. Finally, we turn to our main estimates of the macroeconomic effect of transfer payments.

3.1. Motivating Case Study: the January 1950 Payment

To illustrate our empirical approach, we begin with a case study around the January 1950 dividend, providing a transparent view of the economy around a single, well-identified event. Specifically, we study the response of the economy in a window after January 1950. The length of the window is determined by two considerations. First, the window should be wide enough to capture some of the effects of the shock. Second, the window should not include any other large, atypical shocks affecting the economy. We choose a window of 6 months.

Over this period, narrative accounts suggest that the dividend payments were the main shock hitting the economy. This fact is precisely the advantage of the natural experiment—unlike stimulus checks, the dividend payments were not implemented in response to contemporaneous macroeconomic conditions. 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, 1950a). Likewise, the May 1950 edition of the *Survey* reads: "*[e]xpanded income [as a result of the dividend] lifted retail trade in the first quarter with substantial gains in sales of most durable-goods stores*" (U.S. Department of Commerce, 1950b). Neither survey mentions other shocks driving the increase in consumption. We end the window in June 1950 to avoid contamination from a major subsequent shock. On June 25, North Korea invaded South Korea. Within days, the United States committed military forces, and the resulting geopolitical developments had immediate effects on economic activity.[9]

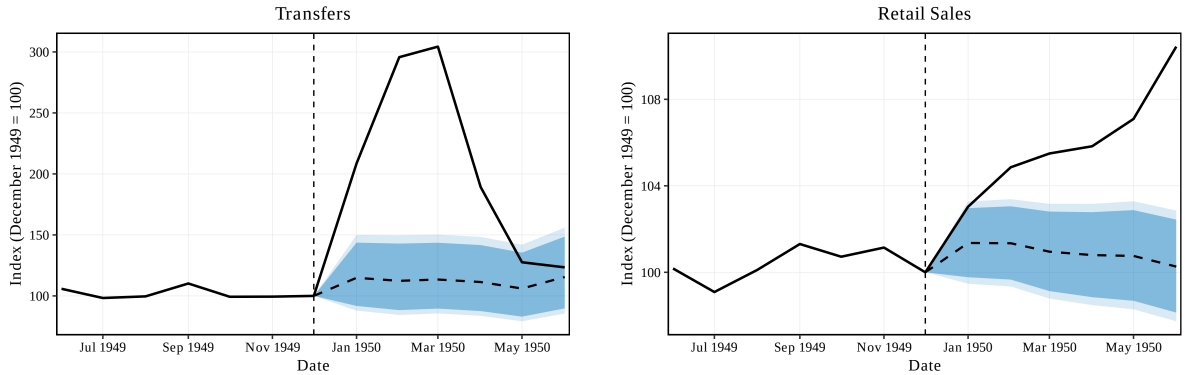
We compare dynamics of macroeconomic variables to a counterfactual path based on their behavior before the event. Specifically, we construct forecasts using a VAR with 6 lags, estimated on data from 1945–1949. The VAR includes our narrow transfer series, retail sales, industrial production, and the consumer price index, all expressed in logs and in real terms. These forecasts provide an estimate of the path the economy would have followed, absent the stimulus. 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.2 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. From Figure 2, we can see that monthly transfers increased by as much as 300%, 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 therefore 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 movements in retail sales, as predicted by the VAR.[10]

[9] In our formal regression analysis, we account for potentially confounding events such as the Korean War using a set of macroeconomic controls that will include news about defence spending.

[10] Appendix Figure B.3 shows that both durable and non-durable retail increases after the payment, but the response is stronger for durables; consistent with evidence that temporary income shocks disproportionately raise expenditures on durable goods (Parker et al., 2013; Boehm, Fize, and Jaravel, 2025).

Figure 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 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.

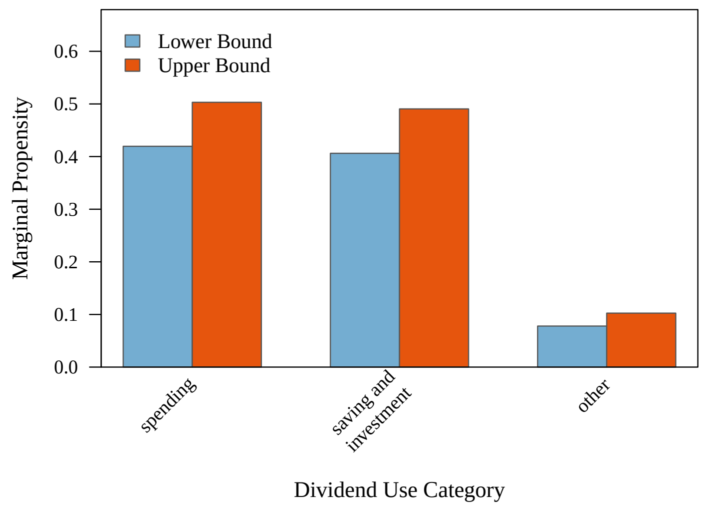
Micro data corroborate these results, suggesting a significant impact from the transfers. In particular, we make use of a one-off question, from the 1951 *Survey of Consumer Finances*, which asked: "Did you (or anyone in your spending unit) receive a dividend on G.I. insurance in 1950?"; "How much did you get?"; and "What did you do with it?" For the final question, respondents could select up to two options; we map these observations to spending, saving and investment, or other categories. As we discuss in Appendix Section B.4, we use this information to bound the fraction of the transfer spent. Figure 3 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).[11,12]

This episode is striking: usually, one-off policy changes are hard to detect in the aggregate time series, because they are anticipated, gradual, or coincide with other macroeconomic disturbances. However, the January 1950 payment is large and discrete, so its effects are visible in raw data. Nevertheless, the evidence is limited to a single event, and we cannot fully rule out confounding factors.

[11] Some previous papers estimate the marginal propensity to spend out of the dividend using microdata (Bodkin, 1959; Bird and Bodkin, 1965). However these data used a single cross section of data, since panel data are unavailable. Souleles (1999) points out that identification is challenging.

[12] 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.

Figure 3: 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. Counts are weighted by sample weight times dividend amount. The lower bound counts dividend dollars from recipients whose reported uses map only to that category; the upper bound additionally includes recipients who report that category alongside one other broad category.

3.2. Identification Strategy and Main Specification

We now estimate the dynamic effects of transfer payments using the full set of postwar payments: the 24 plausibly exogenous transfer events discussed in Section 2 and summarized in Appendix Table A.6.

Figure 1 introduces the key variation we exploit. It displays the cyclical component of government transfers together with the postwar veterans' and temporary Social Security benefit payment dates. The figure shows that these episodes take the form of large, discrete spikes in transfers that reverse quickly. That is, the payments closely resemble stimulus checks: they are one-off, lump-sum payments that are deficit-financed. As discussed above, many of these payments are quantitatively large, with several episodes reaching a non-trivial share of quarterly GDP. At the same time, unlike stimulus checks—which are enacted precisely in response to cyclical downturns—postwar payments were not designed as countercyclical policy, making them well suited for identifying causal effects. Indeed, many of the identified events occur outside NBER recessions.

Our identification strategy focuses on the timing of these events rather than their size. As explained in Section 2.3, our narrative analysis selects payments whose dates were determined by legislative and administrative processes and were often fixed in advance, making their timing a natural source of variation. By contrast, payment amounts may be partly endogenous.

For example, the size of veterans' dividends depends on the accumulated performance of the underlying insurance fund, which reflects past economic and financial conditions. For this reason, we treat transfer shocks as discrete events and use their timing to identify the dynamic causal effects of transfers (see Boer and Lütkepohl (2021) and Plagborg-Møller (2022) for an econometric justification).

Empirical framework and specification. 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 our narrative transfer shocks, defined as the series of plausibly exogenous transfer shock dates. 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 timing restriction, ruling out correlation with past or future structural shocks and thus excluding anticipation or systematic policy responses to evolving macroeconomic conditions.

Provided that the narrative transfer shock series satisfies these three conditions, we can estimate its dynamic causal effects using local projections as in Jordà (2005). Specifically, for each horizon $h = 1, \ldots H$, 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. Since the shock is a dummy variable, episodes with particularly large transfer changes—such as the 1950 dividend payment—do not receive a higher weight. For inference, we use heteroskedasticity-robust standard errors, following Montiel Olea and Plagborg-Møller (2020).

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, and the log level of real government transfers, measured using the broad transfer series. Given this rich set of controls, the identifying assumption is more likely to be satisfied: transfer shocks should be orthogonal to other shocks affecting the outcome. This assumption is supported by the narrative evidence discussed above and by the fact that lagged macroeconomic variables do not predict the shocks.

We focus on standard macroeconomic outcomes, including transfers, industrial production, employment, retail sales, personal consumption expenditures, and the consumer price index (CPI), 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 CPI. Our main sample spans October 1945 to December 2019. We begin the sample after World War II to avoid the substantial macroeconomic disruptions associated with the Great Depression and wartime period. In a similar vein, we end the sample prior to the COVID-19 pandemic. 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.

We also report pre-trends for horizons $h < 0$, following Fukui, Nakamura, and Steinsson (2025). These coefficients are computed analogously to the post-shock impulse responses, but shift the control variables to the period immediately before the outcome being predicted. This approach avoids mechanically setting the first pre-shock coefficients to zero due to the inclusion of lagged controls. The resulting coefficients test whether future transfer shocks predict deviations in the outcome before the shock occurs. Under our identifying assumption, these coefficients should be close to zero.

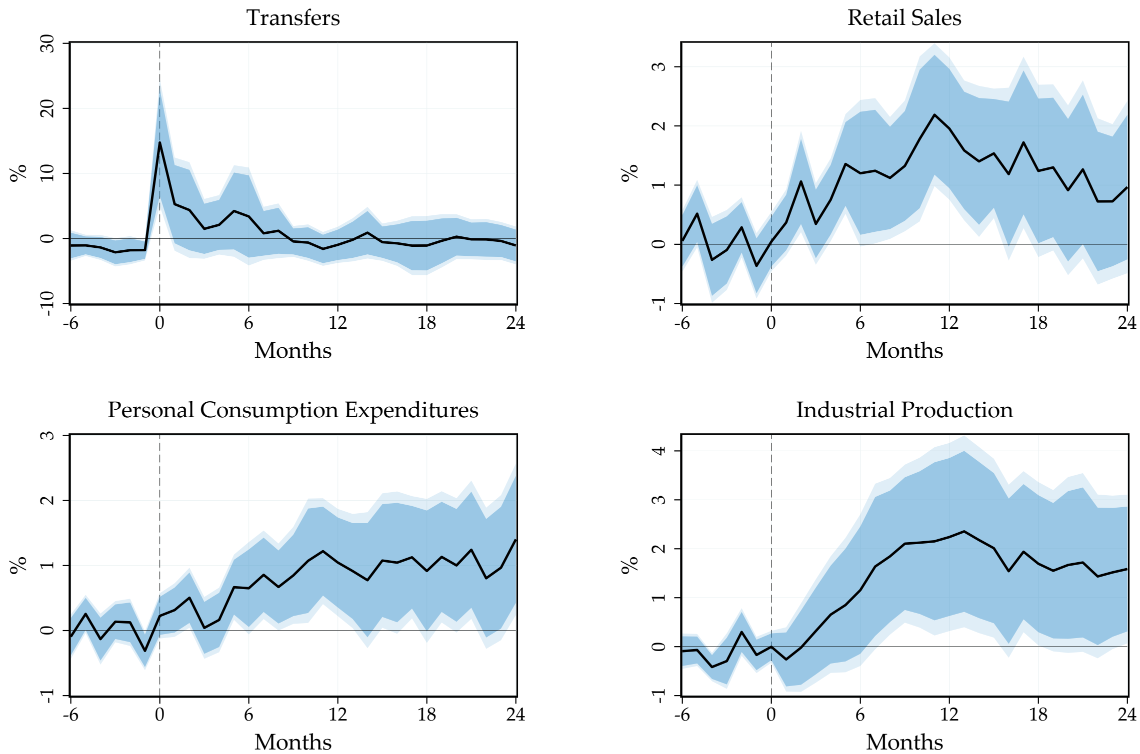
3.3. Main Estimates: the Macroeconomic Effect of Postwar Payments

We now turn to our main estimates. Using the local projections framework described above, we trace the dynamic responses of key macroeconomic variables to an exogenous transfer shock.

Figure 4 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. Appendix Figure B.4 shows that this increase corresponds to roughly a 10% rise in the broader transfer series and to an increase of close to 1 percentage point in transfers as a share of consumption—meaning the shock is not only transitory but economically meaningful.

Real activity responds persistently. Retail sales increase by around 2% (Figure 4, top-right panel), real personal consumption expenditures rise by more than 1% (bottom-left panel), and

Figure 4: The Macroeconomic Effects of Transfer Payments



Notes: Impulse responses of government transfers (narrow definition), retail sales, personal consumption expenditures, and industrial production to a temporary transfer shock, estimated using the local projections specification (1) using the plausibly exogenous transfer shock dates. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands. Sample: 1945m10–2019m12.

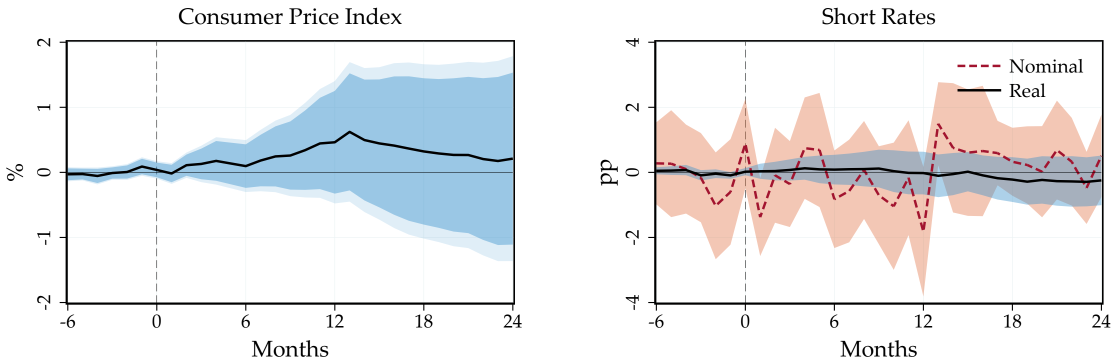
industrial production expands by roughly 2% (bottom-right panel).[13] These effects are not only economically meaningful but also statistically significant at the 95% level. We also find no evidence of pre-existing trends or anticipatory dynamics.

Overall, these results point to large and persistent real effects of temporary transfer shocks, with responses peaking around 12 months after the shock. Hump-shaped responses of real activity are common in response to macroeconomic shocks, including monetary policy shocks (Christiano, Eichenbaum, and Evans, 2005). The key distinction here is that those shocks are typically persistent themselves. By contrast, the transfer shock we study is transitory, yet generates a delayed and persistent response.

The response of prices is smaller. Figure 5 (left panel) shows that consumer prices rise by around 0.5%, but the effect is not statistically significant. We also examine the response of monetary policy to transfer shocks. The right panel shows the responses of the nominal short-term

[13] Appendix Figure B.5 further documents meaningful labor market impacts with a significant increase in nonfarm employment, and a fall in the unemployment rate.

Figure 5: Responses of Prices and Monetary Policy



Notes: Impulse responses of consumer prices and interest rates to a temporary transfer shock, estimated using the local projections specification (1) with the plausibly exogenous transfer shock dates. The left panel shows the response of the consumer price index. Black line: point estimate. Blue shaded areas: 90% and 95% confidence bands. The right panel shows the responses of the 3-Month Treasury Bill rate together with the corresponding ex-post real rate. Solid and dashed lines: point estimates. Shaded areas: 95% confidence bands. Sample: 1945m10–2019m12.

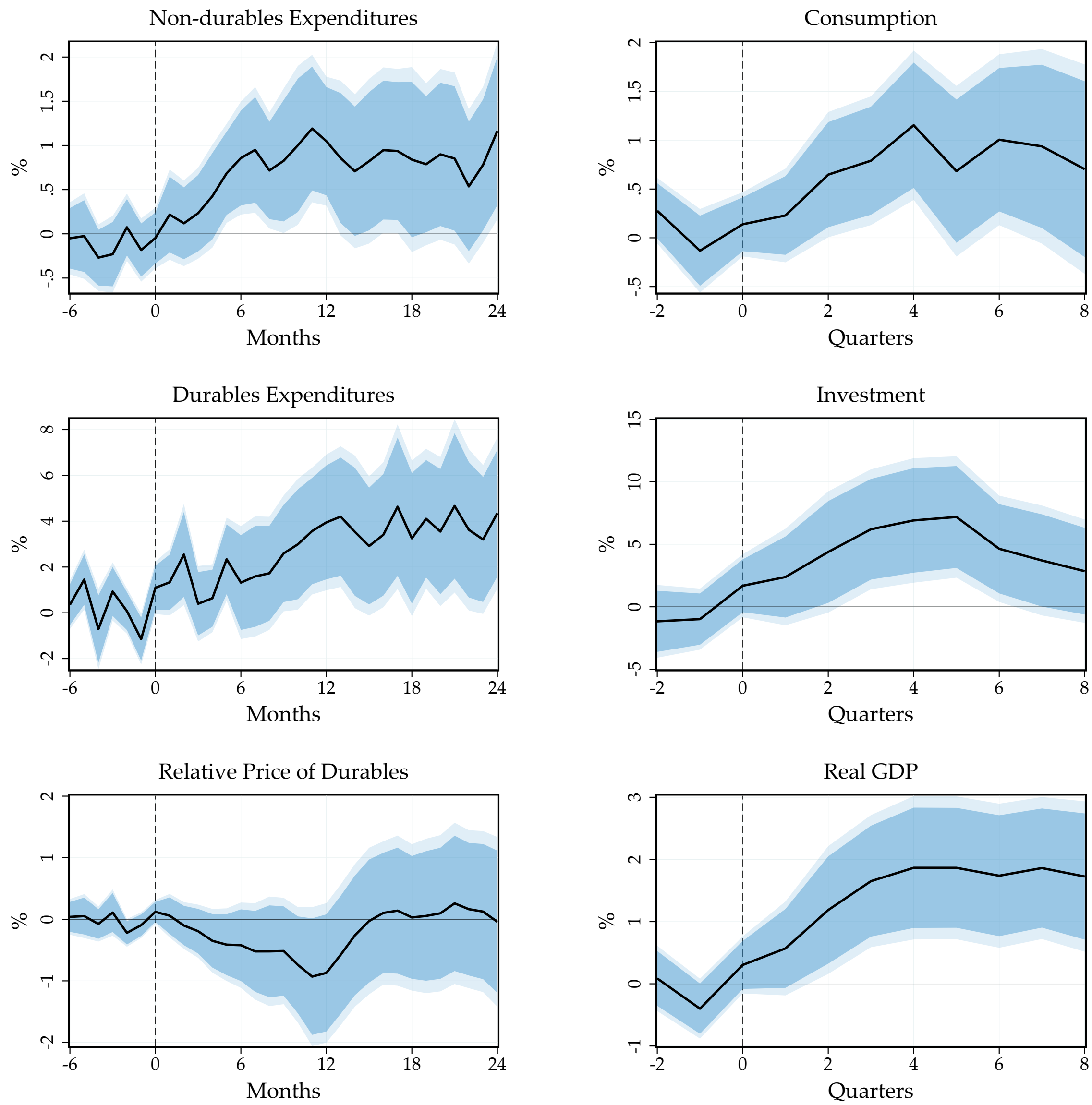
interest rate and the ex-post real rate. We find little evidence of a systematic monetary policy reaction. The responses for both the nominal and real short-term rates are small and statistically indistinguishable from zero at all horizons. Thus, there is little evidence of a monetary tightening that would offset the expansionary effects of the transfer shock.

Which components of spending drive the aggregate response? Figure 6, 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, however, 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 aggregate consumption, investment, and real GDP. Figure 6, right panels, shows that the increase in household spending is accompanied by a broader expansion in economic activity. Aggregate consumption rises persistently, 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 gradually and persistently over the following year, peaking at around 1.5%. Overall, the results suggest that temporary transfer payments have effects beyond the di-

Figure 6: Consumption Components, Relative Prices, and Broader Impacts

(a) Non-durable/Durable Responses (b) GDP and Components



Notes: Impulse responses of non-durables and durables to a temporary transfer shock, estimated using the local projections specification (1) using the plausibly exogenous transfer shock dates. Panel a shows the responses of non-durable and durable expenditures over the sample 1947m1–2019m12, as well as the relative price of durables, measured as the producer price index for durable goods relative to the aggregate producer price index, over 1945m10–2019m12. Panel b presents the responses of aggregate consumption, investment, and real GDP estimated using quarterly data over 1945q4–2019q4. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

rect household spending response, generating a broader expansion in aggregate demand and output.

The shock is accompanied by macroeconomic expectations that appear to underreact on impact and overreact later on—a fact that we will revisit with our model. Appendix Figure B.6 reports the response of 12-month-ahead industrial production expectations from the Livingston Survey alongside the realized response of industrial production. 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 industrial production.[14]

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. Appendix Figure B.13 shows that the estimates are similar to the baseline in terms of both magnitude and persistence. However, the estimates are less precise, given the smaller number of events. 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.14). 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.10 shows that including either episode leaves the responses of transfers and personal consumption expenditures broadly unchanged.

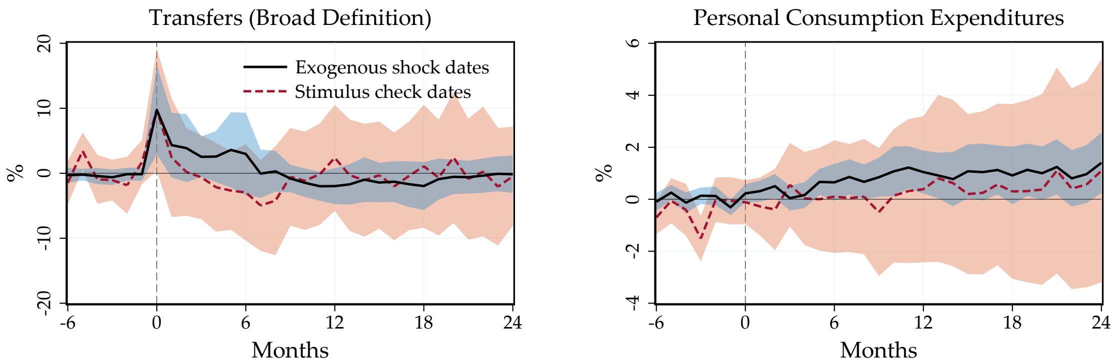
Exogenous versus endogenous shocks. How important is the endogeneity of stimulus checks? To examine this question, we compare our baseline estimates to a set of transfer episodes that are clearly endogenous to the economy. This exercise provides a useful benchmark: if our narrative shocks isolate plausibly 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.[15]

Figure 7 shows that stimulus check dates also generate a transitory increase in transfers. However, the dynamics of consumption are different in a way that suggests identification is important. For stimulus-check dates, pre-trends are visible and statistically significant—consistent with stimulus checks being implemented in response to deteriorating macroeco-

[14] We caution that the results are imprecise, and Livingston Survey expectations are only available every six months. We obtain monthly expectations by combining this information with monthly industrial production using the Chow-Lin procedure.

[15] In Appendix B.6.2, we alternatively consider all endogenous transfer payments, including the endogenous veteran and Social Security payments.

Figure 7: Exogenous vs. Endogenous Shocks



Notes: Impulse responses to plausibly exogenous and endogenous temporary transfer shocks, estimated using the local projections specification (1). The figure compares the responses of transfers (left), measured according to the broad definition, and personal consumption expenditures (right) under our baseline exogenous shock series (solid black lines) to those obtained from stimulus-check dates (dashed red lines), including the rebate payments during the dot-com recession and the financial crisis. The responses are normalized to match the peak increase in transfers in our baseline specification. Sample: 1945m10–2019m12. Blue shaded areas: 90% and 95% confidence bands for the baseline exogenous responses.

nomic conditions. The consumption response to the shock is also different: whereas exogenous transfer shocks are followed by a persistent increase in personal consumption expenditures, consumption does not respond after stimulus check dates. Of course, this lack of response does not mean stimulus checks have no effect. Rather, there could be a confounding effect from the recession that led to the checks.

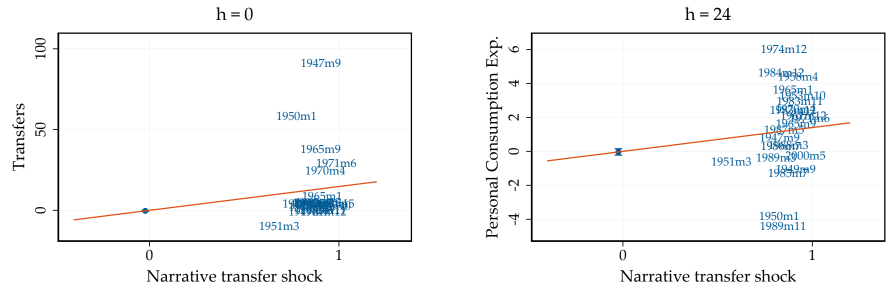
Regional evidence. As an additional sense check, we examine whether the effects of temporary transfer shocks are stronger in states that were more directly exposed to the payments. Since state-level consumption data is not available with the frequency and historical coverage needed for our analysis, we focus on employment responses. Veterans' payments should generate larger income shocks in states with higher veteran shares, while temporary Social Security payments should matter more in states with higher shares of Social Security recipients. Appendix B.5.2 shows this result: states with greater exposure have stronger employment responses after transfer shocks.

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

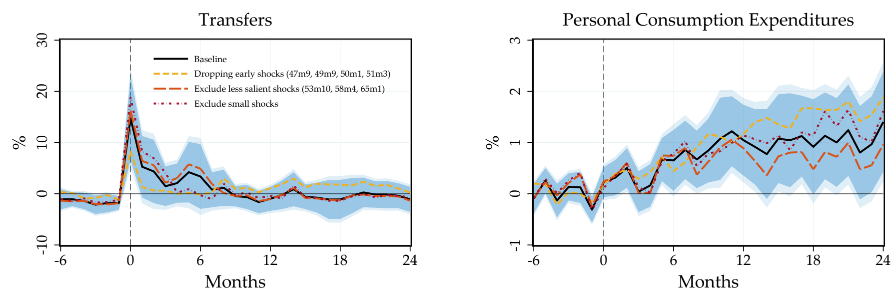
Figure 8 summarizes our key robustness checks. We first visualize the identifying variation underlying the estimates. Panel a presents scatter plots of the residualized outcome variables against the narrative transfer shock, after partialling out the baseline controls. The left panel

Figure 8: Sensitivity Analyses

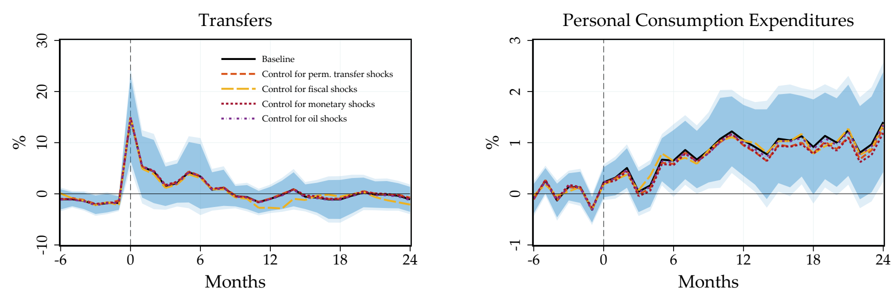
(a) Scatter Plots



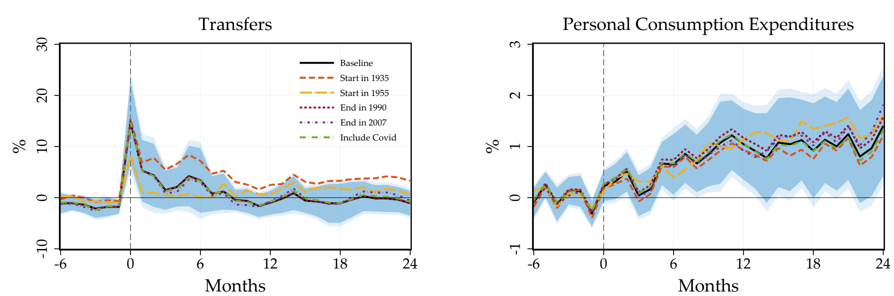
(b) Robustness with Respect to Shock Selection



(c) Robustness with Respect to Controls



(d) Sample Period



Notes: Sensitivity of transfer and personal consumption expenditure responses to a temporary transfer shock, estimated using the local projections specification (1). Panel a displays scatter plots of transfers and personal consumption expenditures at impact ($h = 0$) and at a 6-month horizon ($h = 24$). Panel b shows robustness to alternative shock selections, including dropping early episodes, excluding less salient shocks, and restricting attention to larger events. Panel c adds controls for permanent transfer shocks, military spending shocks, monetary policy shocks, and oil price shocks. Panel d shows robustness to alternative sample periods. Sample: 1945m10–2025m12 unless stated otherwise. Lines: point estimates. Blue shaded areas: 90% and 95% confidence bands for baseline responses.

shows the impact response of transfers, while the right panel shows the response of personal consumption expenditures two years after the shock. 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, we examine robustness with respect to shock selection. Panel b shows that 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 restrict attention to larger events by excluding transfer shocks whose real size falls below the bottom quartile. The dynamic response of consumption is remarkably stable across these alternative shock samples, indicating that our findings are not sensitive to specific episodes in the narrative series. In addition, Appendix Figure B.12 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.

Third, we show in Panel c 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 macroeconomic disturbances, including military spending shocks from Ramey (2011), monetary policy shocks from Romer and Romer (2004), tax shocks from Mertens and Ravn (2013b), and oil price shocks (Känzig, 2021).[16] The consumption response remains quantitatively similar across specifications, suggesting that our transfer shocks are not proxying for broader fiscal, monetary, or energy-related shocks.

Finally, Panel d 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.

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. One way to compute this multiplier would be to rescale the estimated percentage responses by the average ratio of spending to transfers. In practice, however, this approach can be sensitive to the sample period because the transfer share changes substantially over time. We therefore estimate the multiplier directly, following the approach of Ramey and Zubairy (2018). Specifically, for each horizon h, we regress the cumulative dollar change in con-

[16] 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.

sumption 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:

$$\sum_{j=0}^{h} \frac{\Delta C_{t+j}}{\tilde{C}_{t-1}} = \beta_h \sum_{j=0}^{h} \frac{\Delta T_{t+j}}{\tilde{C}_{t-1}} + \Gamma_h' X_t + \varepsilon_{t+h}, \tag{2}$$

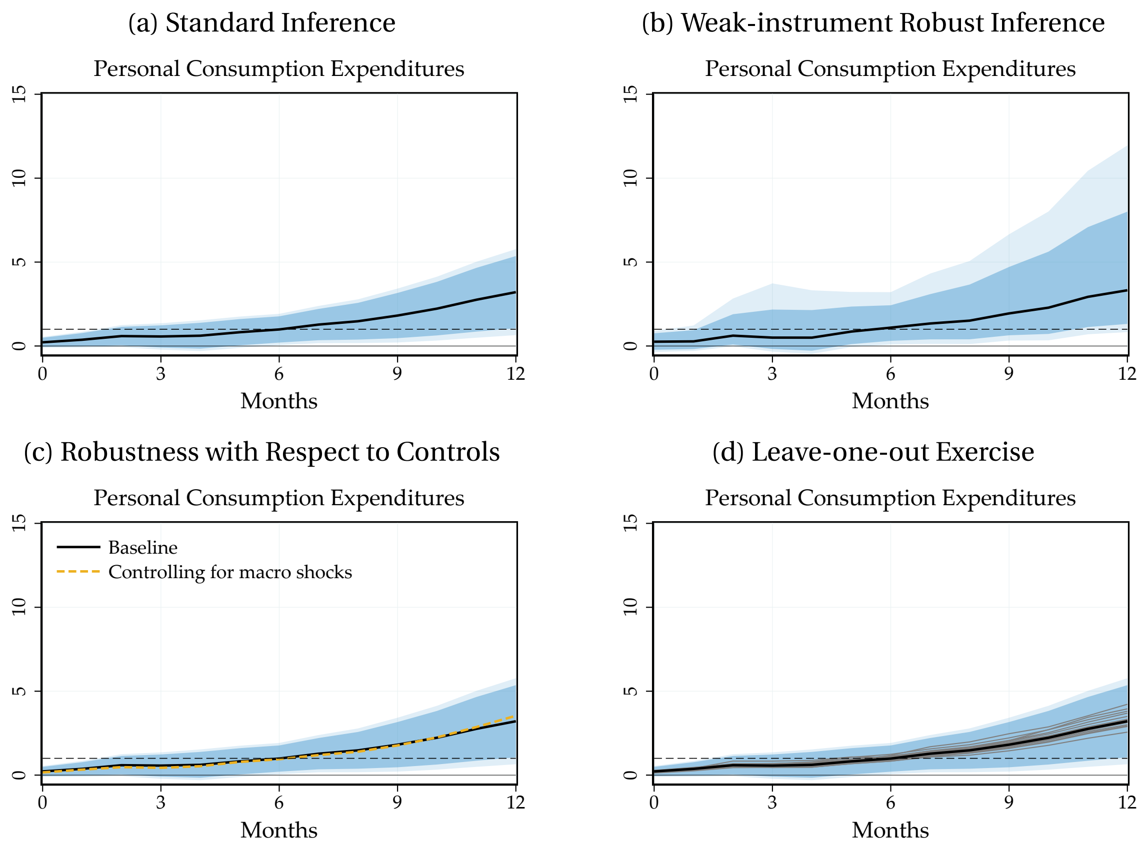
where C denotes personal consumption expenditures in dollars and T denotes government transfers; we instrument for the right hand side with our shock series. Both variables are measured in levels and expressed relative to their pre-shock values, so that β_h directly captures the cumulative dollar response of spending per dollar of transfers. To account for trending behavior, we normalize both variables by $\tilde{C}_{t-1}$, 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. We include as controls 12 lags of changes in consumption and transfers (relative to $\tilde{C}$), CPI inflation, and the transfer shock. This approach delivers a horizon-specific multiplier that naturally incorporates dynamic effects.

Figure 9 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 for personal consumption expenditures. Instrument relevance declines at longer horizons, with F-statistics falling to around 6 after six months (see Appendix B.5.3). 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 longer horizons. To address potential concerns about weak instruments, we also report weak-instrument robust inference based on Anderson–Rubin confidence bands in the top-right panel. These bands are initially similar to those obtained under standard inference, but widen substantially further out; eventually, both the upper and lower bands become too wide to deliver informative inference. For this reason, we focus on the multiplier over the first year. Importantly, over this one-year horizon, the estimated multipliers remain statistically different from zero under weak-instrument robust inference.

In the lower panels of Figure 9, we also assess the robustness of the multiplier estimates using two complementary exercises. First, we include a rich set of macroeconomic shocks, including government spending shocks, tax shocks, monetary policy shocks, and oil price shocks, as additional controls. The bottom-left panel shows that the resulting multiplier paths are similar to the baseline estimates.

Second, we perform a leave-one-out exercise. Specifically, we re-estimate the cumulative multiplier repeatedly, each time excluding one transfer shock episode from the shock series.

Figure 9: Consumption Multipliers



Notes: Cumulative transfer multipliers of personal consumption expenditures, estimated based on (2). Panel a provides bands under standard inference. Panel b provides weak-instrument robust Anderson-Rubin confidence bands. Panel c reports robustness to additional controls for macroeconomic shocks, including fiscal shocks, monetary policy shocks, and oil price shocks. Panel d reports a leave-one-out exercise, in which the multiplier is re-estimated excluding one transfer episode at a time. Sample: 1945m10–2019m12. Black lines and blue shaded areas: baseline point estimates and 90% and 95% confidence bands. Colored or gray lines: estimates under alternative specifications.

Figure 9d reports the resulting multiplier paths. The estimates remain remarkably stable across these specifications. In particular, the medium-run multiplier continues to exceed one at horizons beyond six months across virtually all leave-one-out samples. These results indicate that the estimated transfer multiplier is not driven by any single episode and instead reflects a systematic pattern across the full set of postwar transfer shocks.

These results imply that temporary lump-sum transfers generate a large and persistent increase in aggregate spending. Although the transfer shock itself is short-lived, the response of personal consumption expenditures builds gradually and remains elevated. These estimates provide a useful empirical benchmark for quantitative macroeconomic models of temporary transfer payments.

4. Matching Estimates to a HANK Model

We develop a heterogeneous-agent New Keynesian (HANK) model, for two purposes. First, we use the model to characterize conditions under which the multiplier for stimulus checks and for veterans' payments are the same. Second, we use the model to ask what mechanisms are required to match the 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 persistent response of consumption to the transitory shock.

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.[17] 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

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

[17] A constant-real-interest-rate policy can be viewed as a Taylor rule with a unit coefficient on expected inflation; see Auclert, Rognlie, and Straub (2024) for a detailed discussion.

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,

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

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 expectations 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

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

where $a_{i,t}$ denotes financial assets at the end of period t. The variables T_t^v 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 $\omega_{i,t}^v \geq 0$ and $\omega_{i,t} \geq 0$ determine the cross-sectional allocation of veteran payments and government transfers. They satisfy $\int \omega_{i,t}^v \, di = 1$ and $\int \omega_{i,t} \, di = 1$, with $\omega_{i,t}^v = 0$ 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 $\theta_{i,t} \equiv \frac{e_{i,t}^{1-\gamma}}{\bar{\varepsilon}^{1-\gamma}}$ 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 $\mathcal{T}$ 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.

Following 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), and 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 Kalman gain—the optimal Bayesian weight that households place on new signals. Suppose that τ is finite. Then the gain is below one and households 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. Together, the two forces—noisy information and long-memory diagnostic expectations—can deliver initial underreaction followed by overreaction at longer horizons.

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). Let $d\mathbf{Y} \equiv [dY_0, dY_1, \ldots]'$, and define $d\mathscr{T}$, $d\mathbf{T}$, and $d\mathbf{T}^v$ analogously. The linear equilibrium is the fixed point

$$d\mathbf{Y} = \mathbf{M}^Y (d\mathbf{Y} - d\mathscr{T}) + \mathbf{M}^\omega d\mathbf{T} + \mathbf{M}^v d\mathbf{T}^v, \tag{10}$$

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

4.2. Veteran Payments and Stimulus Checks: Equivalence Results

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 the consolidated government budget constraint

$$B_t + \mathscr{T}_t + \mu_v P = (1+r)B_{t-1} + T_t + T_t^v. \tag{11}$$

The consolidated budget constraint (11) 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 $\{\mathscr{T}_t\}_{t\geq 0}$ implied by (11) 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.[18]

The result formalizes the sense in which a veterans' payment is a special case of a fiscal 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. The next proposition states what happens when the targeting of veterans' payments and stimulus checks is different.

Proposition 2 (Differences Across Targeting Rules)**.** *Fix a total-transfer path with* $\{T_t^v\}_{t\geq 0} = \{T_t\}_{t\geq 0}$. *Let* $d\mathbf{Y}_v$ *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*

$$d\mathbf{Y}_\omega = d\mathbf{Y}_v + \mathscr{M}(\mathbf{M}^\omega - \mathbf{M}^v)d\mathbf{T}, \tag{12}$$

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

Equation (12) 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, $\mathbf{M}^\omega - \mathbf{M}^v$; and the size and timing of the transfer path $d\mathbf{T}$. The intertemporal marginal propensities to consume $\mathbf{M}^\omega$ and $\mathbf{M}^v$ 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^v for $\mathbf{M}^v$. 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.

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

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 (12) shows that any difference between veterans' payments and stimulus checks arises from how the dollars are distributed across households, summarized by the gap $\mathbf{M}^{\omega} - \mathbf{M}^{v}$. 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 $\mathbf{M}^{\omega} = \mathbf{M}^{v}$, 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.

4.3. Estimating the HANK model

We now estimate the 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, $u(c) = c^{1-\sigma^{-1}}/(1-\sigma^{-1})$ and $v(n) = \zeta n^{1+\psi^{-1}}/(1+\psi^{-1})$. We set the intertemporal elasticity to 0.50, equivalently an inverse-EIS coefficient of 2, and set the Frisch elasticity to $\psi = 0.75$. 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. 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 $P/\overline{e}_v = 0.0169$.

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 27% of income. Table C.1

reports the resulting group-level moments. Together with the permanent-productivity normalization above, these moments pin down $\overline{e}_v = 1.29$ and $\overline{e}_{nv} = 0.92$.

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 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 $\phi = 0.158$. The wage Phillips curve slope is $\kappa_w = 0.0062$, consistent with the estimates of Hazell, Herreno, Nakamura, and Steinsson (2022).

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, an annual MPC out of the GI-dividend transfer of 0.45, and the veteran liquid asset share of 18.2%. The solution is $\beta_v = 0.975$, $\beta_{nv} = 0.983$, 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 and personal consumption expenditure, covering horizons $h = 0, \ldots, T$. 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.

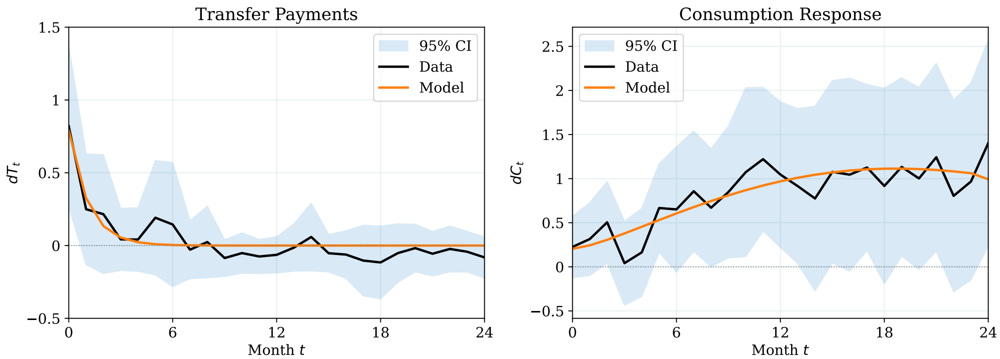
The full diagnostic-expectations specification is indexed by $\Theta^{DE} = (\epsilon, \rho, \theta, \tau, a, b)$. As in Bianchi, Ilut, and Saijo (2024) and Bardóczy and Guerreiro (2023), the memory weights are approximated by a beta-binomial distribution over $n = 24$ months, with $\alpha_j = \Pr(J = j \mid n = 24, a, b)$ 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:

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

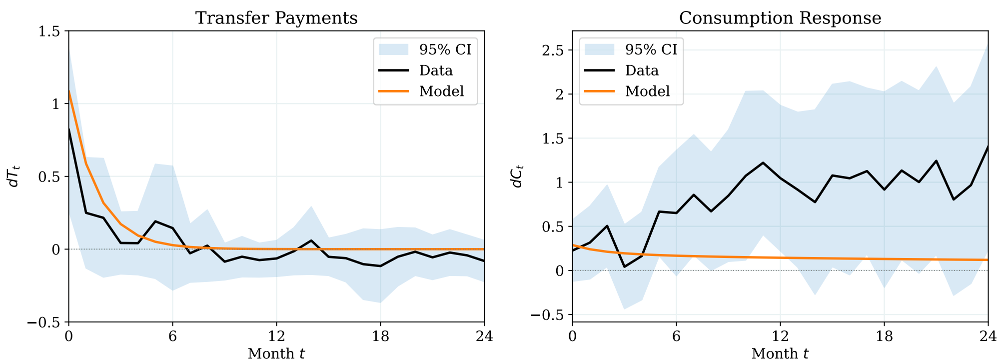
The question is whether the spending response requires both pieces of the expectation process. We compare the full specification with the full-information rational expectations (FIRE) model, which imposes $\theta = 0$ and $\tau = \infty$. In the FIRE economy, we reestimate the admissible parameters ϵ and ρ to give that model its best chance to match the transfer and spending IRFs.

Figure 10: Model Fit to Transfer and Consumption Impulse Responses

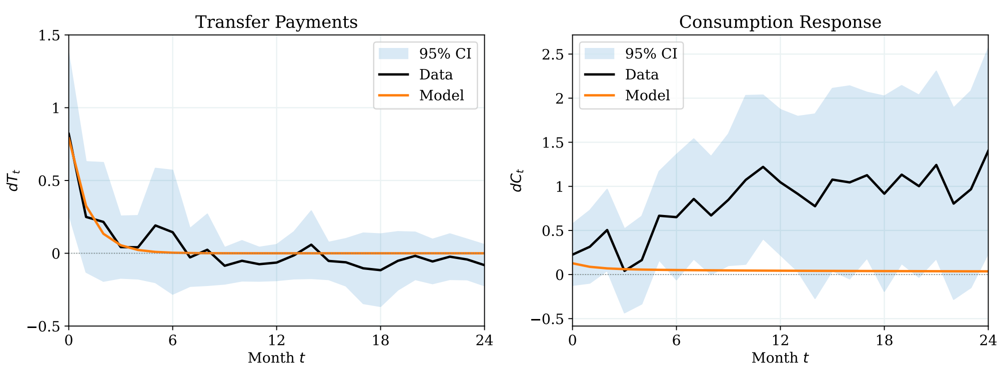
(a) Full Model



(b) Full Information Rational Expectations



(c) Underreaction-only Model



Notes: Panel a reports the impulse responses for transfers and consumption under the estimated full model. Panel b reports the impulse responses for transfers and consumption under the estimated FIRE specification. Panel c reports the impulse responses for transfers and consumption in the underreaction-only economy, where we impose $\theta = 0$.

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 response of consumption. Figure 10a 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. Appendix Figure B.6 shows that the estimated response of expectations is consistent with what the model predicts—namely, initial underreaction and later overreaction, relative to the data.

To understand these forces, Figure 10b 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. The root mean squared error (RMSE) of the full model is 0.62, while the RMSE of the FIRE model is 1.31. The full model therefore fits the data twice as well as the rational expectations specification, and the improvement is driven by the ability to match the timing of the spending response.

Figure 10c 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. The initial underreaction is not enough to generate the delayed strength of spending.

Expectations in the data and model. Appendix C.9 validates our estimate of expectations. We estimate the response of expectations of industrial production to the transfer shock. We 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. We conclude that imperfect expectations are a promising way of explaining the delayed response of consumption.

4.5. 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.

Table C.3 reports the cumulative transfer multipliers for the veteran payment and the stimulus check under both the full estimated model and the rational expectations specification. The multipliers are computed as the cumulative change in output over the first 6 and 12 months after the shock, divided by the cumulative change in transfers over the same horizon.

The multipliers are higher under the full model than under rational expectations, reflecting the stronger and more persistent spending response generated by the combination of high MPCs and distorted expectations. The multipliers for the stimulus check are slightly lower than those for the veteran payment, since the broader population includes households with lower MPCs than veterans. However, the difference in multipliers between the two transfer types is small, suggesting that the estimates of the veterans' payment multipliers are informative about stimulus checks.

5. Conclusion

This paper studies a natural experiment that isolates the macroeconomic effects of large, one-off, deficit-financed transfer payments, which closely resemble stimulus checks. Postwar payments to U.S. veterans, funded by an unexpected surplus in a government-managed life-insurance fund, generated sharp and temporary spikes in aggregate transfers that were unrelated to contemporaneous business cycle conditions. Combined with a set of plausibly exogenous temporary Social Security payments identified by Romer and Romer (2016), these events form a series of temporary shocks to transfers. Using a newly assembled monthly dataset on government transfers and consumption back to 1945, we find that a temporary increase in transfers generates a large and persistent rise in personal consumption expenditures. The resulting transfer multiplier reaches roughly 1 after six months and exceeds 2 after twelve months, with little response from monetary policy or the price level.

To interpret our results, we develop a HANK model. Through the logic of the consolidated balance sheet of the government, we show that the postwar payments multiplier and the stimulus checks multiplier coincide under plausible conditions. A suitably calibrated model is informative not only about the postwar payments themselves, but also about stimulus checks. However, standard specifications of the HANK model cannot match the persistence of the consumption response: under full-information rational expectations, consumption peaks on impact and decays thereafter. Matching the hump-shaped response in the data requires expectations that underreact to shocks on impact and overreact at longer horizons.

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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:

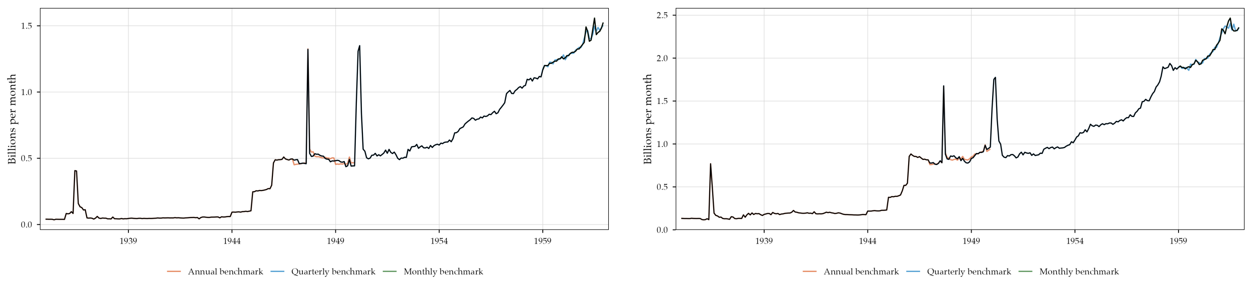
$$T_t^{\text{bench}} = \widetilde{T}_t + \frac{R_P}{N_P}, \qquad t \in P, \tag{5}$$

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

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.

Figure A.3: Benchmark candidates and stitched series

(a) Narrow transfers (b) Broad transfers

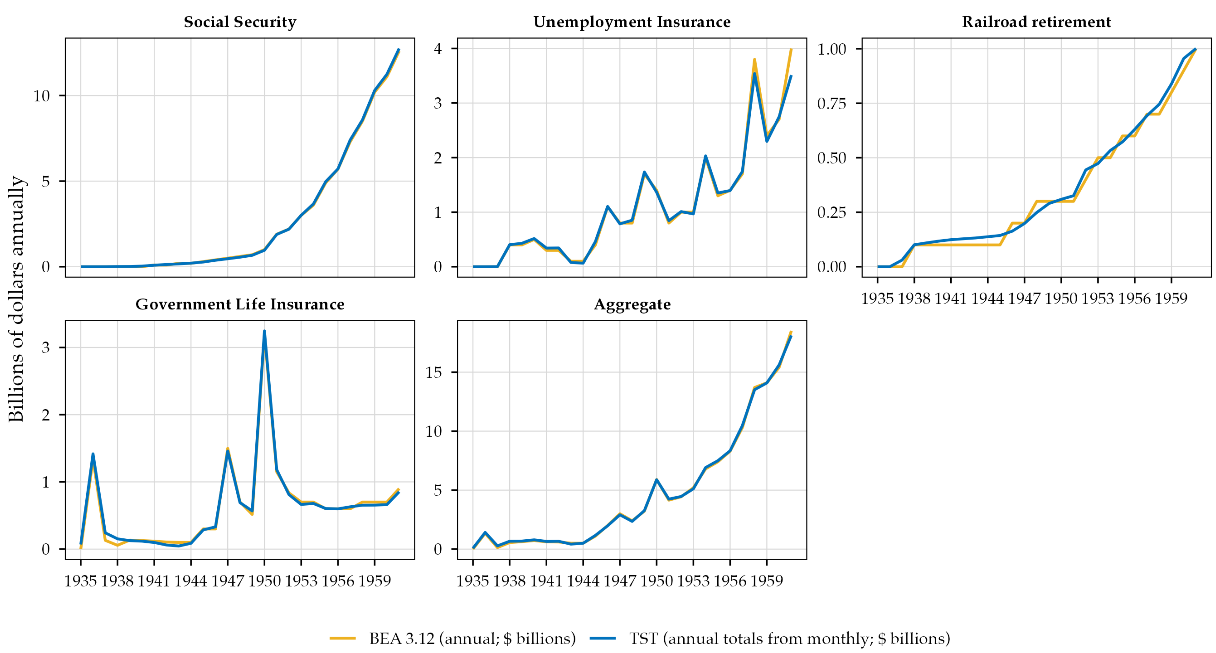


Notes: Benchmark-consistent candidates constructed using annual, quarterly, and monthly benchmarks where available, and the stitched preferred series used in the paper.

A.1.3. Validation and coverage

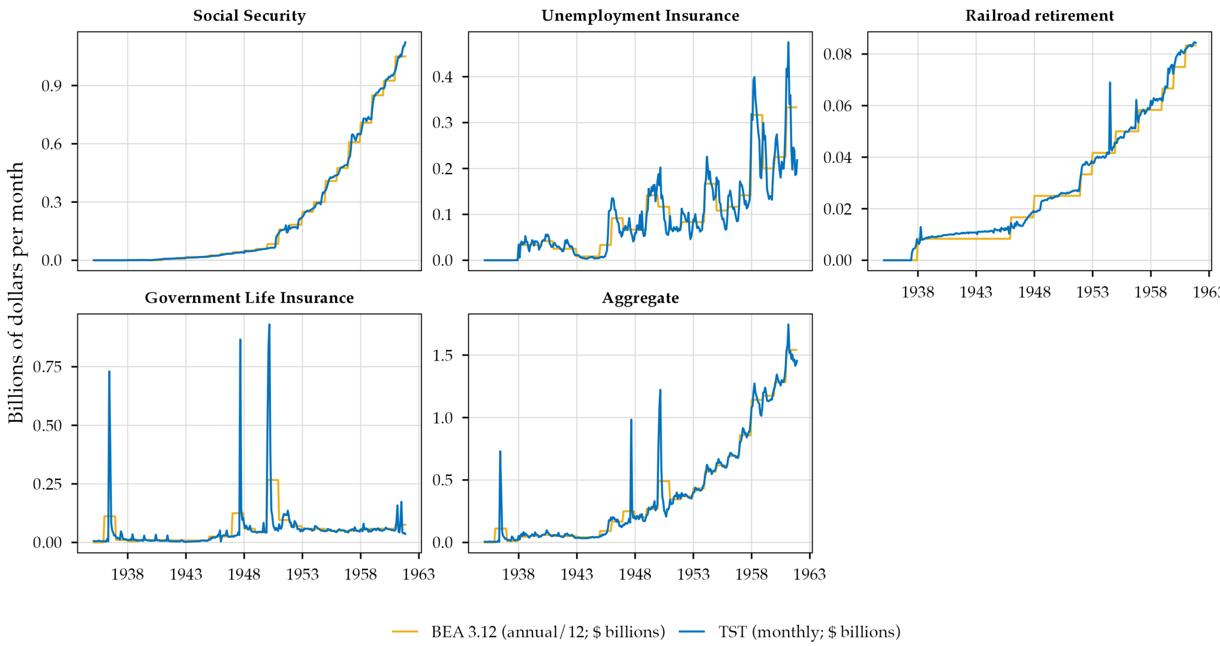
BEA Table 3.12 validation. We compare annual totals implied by summing Treasury monthly flows within year to annual line items from BEA Table 3.12 (*Government social benefits*) for Social Security, unemployment insurance, railroad retirement, and veterans' insurance-related payments. Figure A.4 shows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



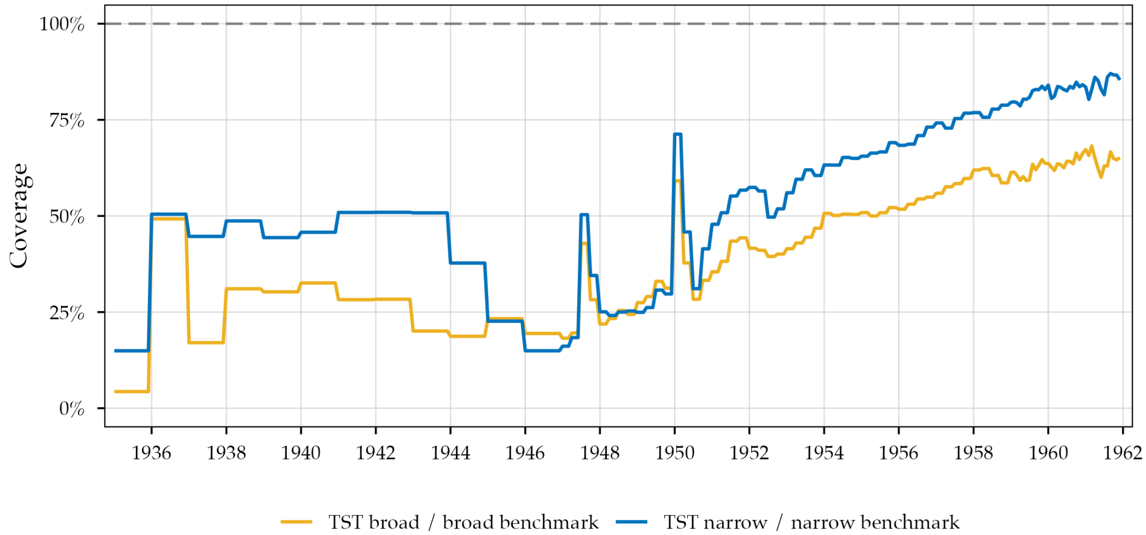
Notes: Treasury monthly flows compared to BEA annual totals divided by twelve.

Coverage. We compute coverage ratios:

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

where P denotes the benchmark period (annual for 1935–1946, quarterly for 1947–1958, monthly thereafter). Figure A.6 plots these coverage ratios.

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.

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 1935m1–2025m9. 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 digitised MARTS releases. We use the growth rate between 1991m12 and 1992m1 implied by the digitised 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 digitised 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.

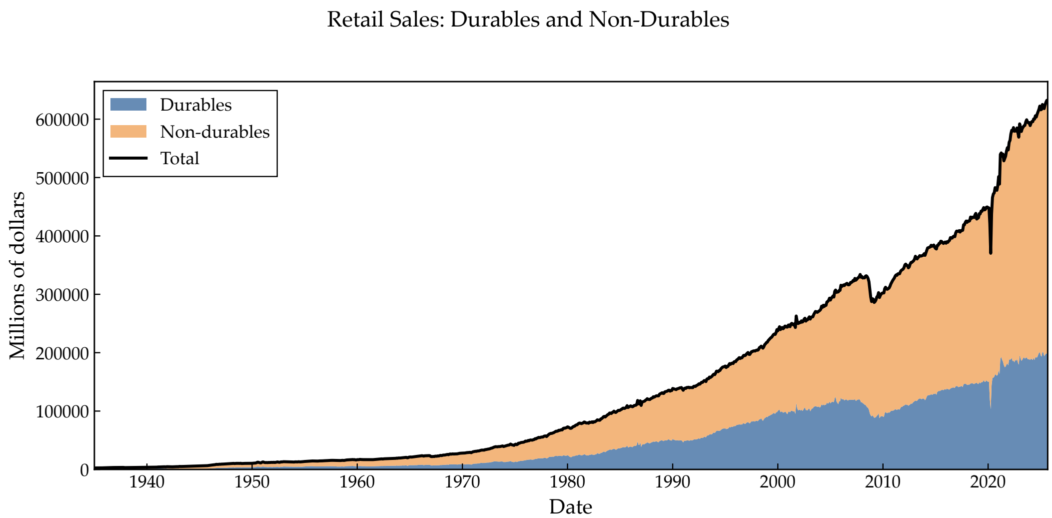


Figure A.7: U.S. retail sales and durable/non-durable decomposition (1935m1–2025m9).

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 was 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 1935M1-2024M10.

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 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 1947M1-2024M10.

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 1954M07) and extend this back using the 3-Month Treasury Bill Secondary Market Rate from FRED (available since 1934M01). We construct real rates by subtracting realized 1-year ahead inflation from the short-term nominal rate.

[1] We use this quarterly consumption series directly in our quarterly local projections from Figure 6b. We describe below the construction of the additional quarterly series in that Figure.

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
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 Domestic Private 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 subsection 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.

A.5.1. 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 in 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 following political pressure, with President Truman signing the amendment in July 1947.

Table A.6: Postwar Veterans and Social Security Payments

Programme	First month	Nominal size	Size / GDP	Baseline
World War One bonus payment	Jun. 1936	$1.8 billion	8.41%	
Armed Forces Terminal Leave	Sep. 1947	$2.1 billion	3.36%	✓
World War One insurance dividend	Sep. 1949	$40 million	0.06%	✓
World War Two insurance dividend	Jan. 1950	$2.9 billion	4.12%	✓
World War Two insurance dividend	Mar. 1951	$685 million	0.81%	✓
World War Two insurance dividend	Oct. 1953	$64 million	0.07%	✓
World War Two insurance dividend	Apr. 1958	$32 million	0.03%	✓
Korean War insurance dividend	Dec. 1961	$60 million	0.04%	✓
World War Two 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 final 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 1945 using the series in Ramey and Zubairy (2018).

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 1950 that the divided 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 authorised 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 does 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 Country 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 announcement 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 authorised via the Senior Citizens' Freedom to Work Act, enacted on 7 April 2000.

A.5.2. 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.

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 com-*

Table A.7: Endogenous Temporary Postwar Veterans Payments

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

pleted 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 empirical results not presented in the main text and provides an extensive set of robustness checks for our baseline empirical findings.

B.1. 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.

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.

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 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

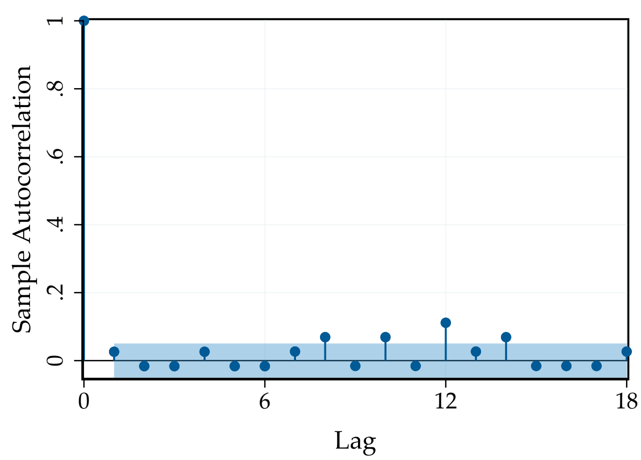
Table B.2: Correlation with Other Shock Measures

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.

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



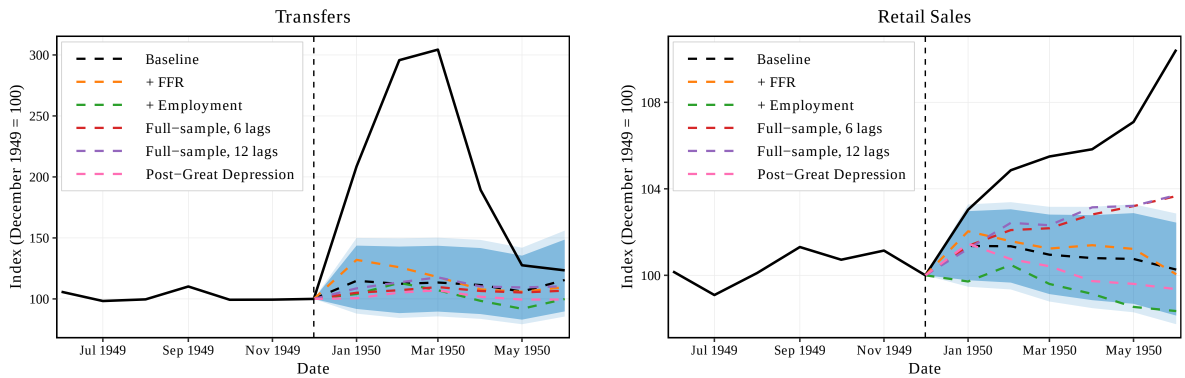
Notes: Autocorrelation function of narrative transfer shock series with 95% confidence bands.

B.2. 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 2 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 base-

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 the main text.

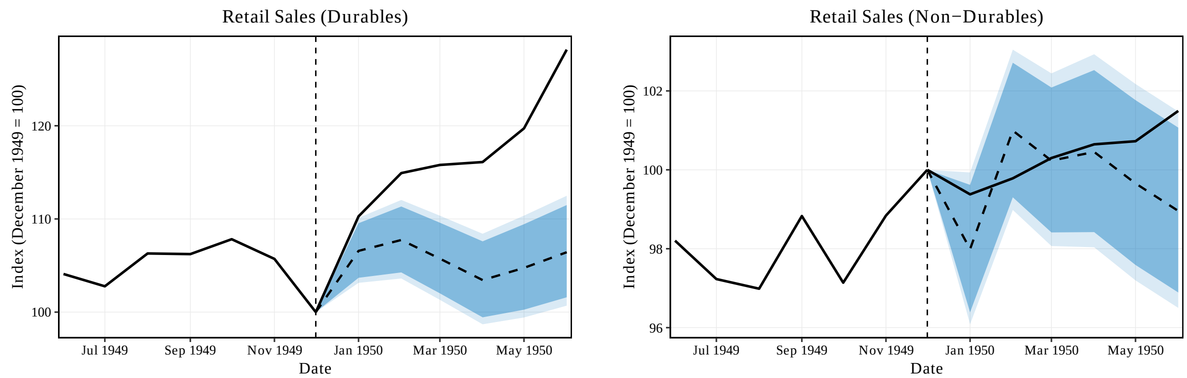
line VAR specification estimated on a longer sample, starting after the Great Depression (1940), shown in the pink line. We also consider pseudo-out-of-sample forecasts from a VAR estimated over the same sample as the local projection (1945-2019) – both with six lags (red line), as well as 12 lags (purple line). In all cases we find that the observed data for both transfers and retail sales lie well above the estimated forecast.

B.3. Durable and Non-Durable Retail Sales in the 1950 Case Study

Figure B.3 provides additional evidence on the composition of the spending response around the January 1950 payment.[2] Both durable and non-durable retail sales increase following the payment, but the response is markedly stronger for durables. Durable goods spending rises sharply on impact and accounts for the bulk of the overall increase in retail sales, whereas non-durable spending responds more gradually and with less precision. This pattern is consistent with narrative accounts from the period, which provide further color on the composition of additional spending. For example, the *Survey of Current Business* for March 1950 notes: "A particular feature of the developments in January orders was the high level of new business for durables. The motor vehicle industry...contributed largely to the January gain, with an increase of more than one-third from the preceding month." The April 1950 edition similarly notes a "sharp recovery in durable goods", concentrated in household durables such as refrigerators, televisions and television-radio-phonograph combination sets. This is consistent

[2]To compute forecasts for durables and non-durables, we augment the baseline VAR by one variable at a time.

Figure B.3: Durable and Non-Durable Retail Sales around the January 1950 Payment



Notes: The figure shows the evolution of durable and non-durable retail sales in a six-month window around the January 1950 veterans' payment. The vertical dashed line indicates the timing of the payment. The dashed lines show forecasts from a five-variable VAR estimated on pre-event data from 1945–1949, and the shaded areas represent the 90 and 95% bootstrap forecast intervals.

with evidence showing that temporary income shocks disproportionately raise expenditures on durable goods, such as automobiles and household furnishings (Parker et al., 2013; Boehm, Fize, and Jaravel, 2025). Finally, the timing and composition of the response are difficult to reconcile with a simple post-recession rebound, as the pickup in retail sales coincides closely with the arrival of transfers and is concentrated in categories typically most sensitive to such payments.

B.4. MPCs from the 1950 Case Study

This section studies how we estimate MPCs from the 1950 case study, as in Figure 3 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

$$\underline{m}_C = \frac{\sum_i p_i \mathbf{1}\{S_i = \{C\}\}}{\sum_i p_i}, \qquad \overline{m}_C = \frac{\sum_i p_i \mathbf{1}\{C \in S_i\}}{\sum_i p_i}. \tag{7}$$

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 3 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.

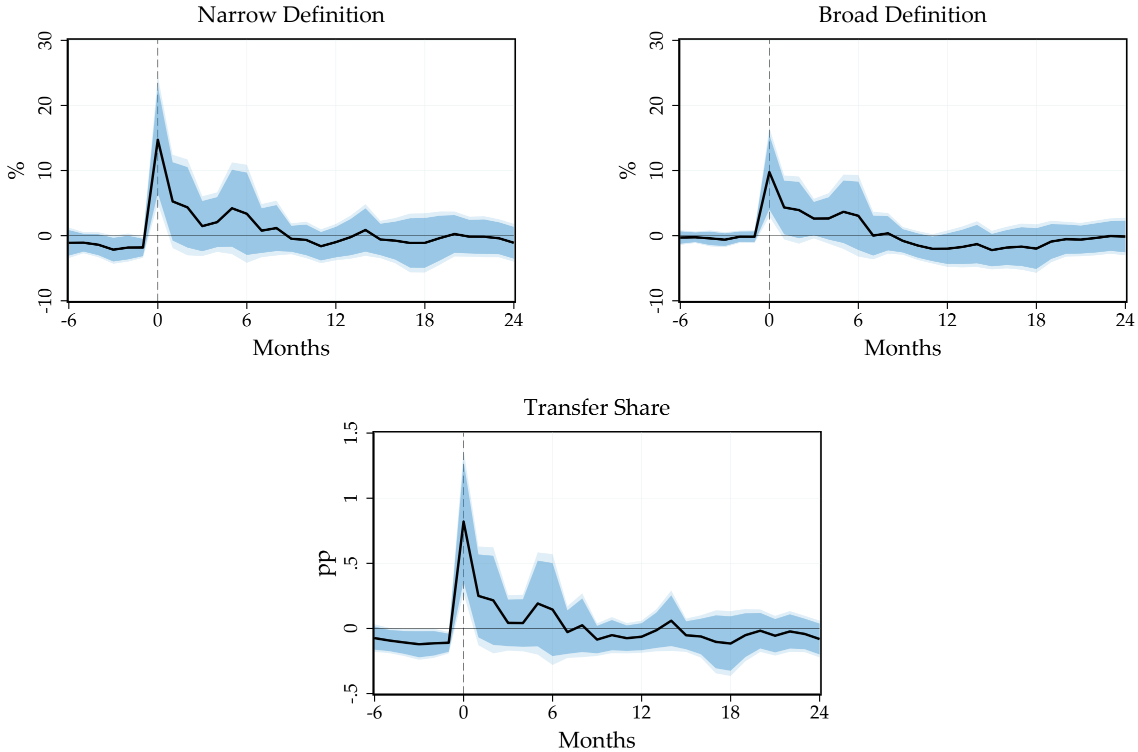
B.5. Additional Evidence

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.4 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.

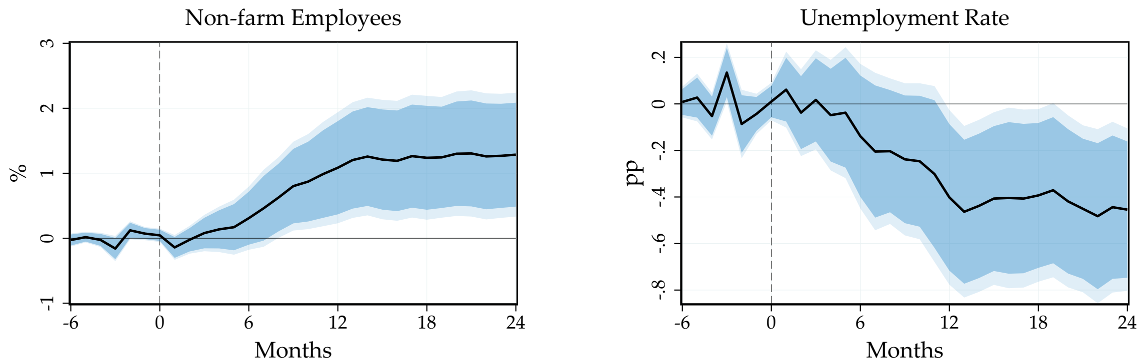
Figure B.4: Transfer Responses under Alternative Definitions



Notes: Impulse responses of government transfers 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. The transfer share is computed relative to trend personal consumption expenditures, where the trend is obtained using an HP filter. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

We next consider a set of additional outcome variables to further characterize the effects of temporary transfer shocks. Figure B.5 reports impulse responses of retail sales, non-farm employment, and the unemployment rate to a temporary transfer shock. The response of retail sales closely mirrors the response of personal consumption expenditures discussed in the main text, although the increase is somewhat more pronounced. We also document significant and persistent labor market effects: non-farm employment rises following the shock, while the unemployment rate declines steadily over the same horizon.

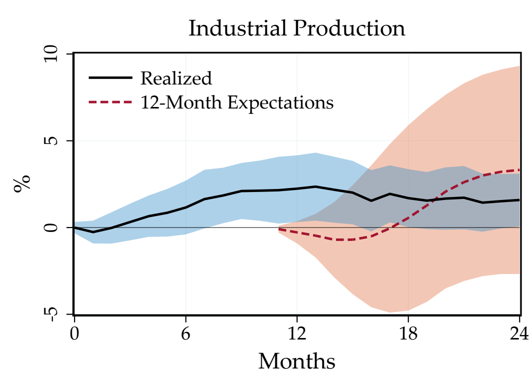
Figure B.5: Employment Responses



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. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure B.6 reports the response of expectations to transfer shocks using data from the Livingston Survey. The left panel shows realized industrial production, while the right panel reports 12-month-ahead expectations. Consistent with the results in the main text, expectations adjust only sluggishly. At short horizons, there is little to no response, and expectations begin to increase only gradually after several months. Moreover, the adjustment in expectations lags behind the realized response, which rises more promptly. This pattern provides additional evidence of delayed expectation adjustment following transfer shocks.

Figure B.6: Response of Industrial Production: Expected vs. Realized



Notes: Impulse responses of realized industrial production and 12-month-ahead industrial production expectations to a temporary transfer shock, estimated using the local projections specification (1) with the narrative transfer shock dates. Expectations are measured using the median forecast from the Livingston Survey. Livingston Survey expectations are only available every six months. We obtain monthly expectations from combining this information with monthly industrial production, via the Chow-Lin procedure. Sample: 1946m12–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

B.5.2. Regional Evidence

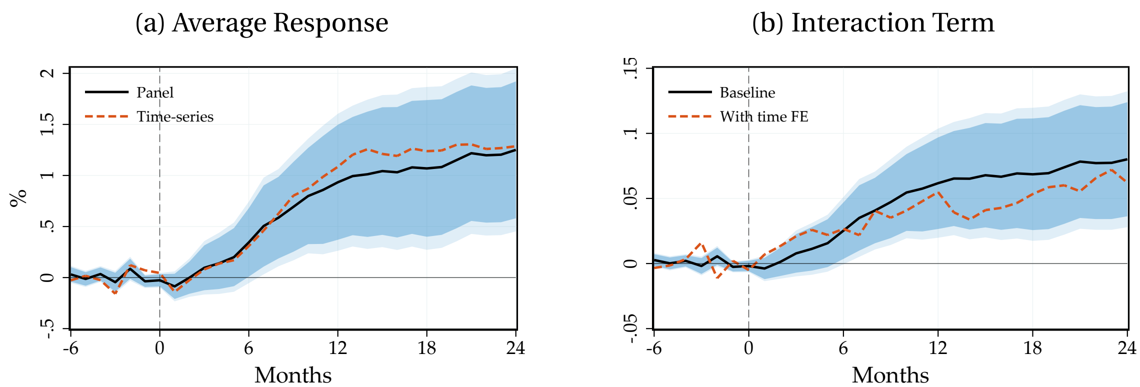
A complementary way to assess the economic effects of temporary transfer shocks is to exploit cross-state variation in exposure to the payments. In particular, veterans' payments should generate larger income shocks in states with a higher share of veterans, while temporary Social Security payments had 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

$$y_{i,t+h} = \alpha_{i,h} + \theta_h z_{T,t} + \mathbf{x}'_{i,t-1}\boldsymbol{\gamma}_h + \mathbf{x}'_{t-1}\boldsymbol{\beta}_h + v_{i,t+h}, \tag{8}$$

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.7: Regional Employment Responses



Notes: Impulse responses of state-level employment to a temporary transfer shock, estimated using panel local projections. Panel a compares the average response from the panel specification to the aggregate time-series estimate from the main text. Panel b reports estimates from a heterogeneous-exposure specification that interacts the transfer shock with the state-level exposure to each respective shock episode (veterans share or Social Security recipient share). The specification additionally includes time fixed effects. Solid and dashed lines denote point estimates. Blue shaded areas correspond to 90 and 95 percent confidence bands. Sample: 1948m6–2019m12.

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. Specifically, we estimate

$$y_{i,t+h} = \alpha_{i,h} + \varphi_h \left(z_{T,t} \times s_{i,t}\right) + \mathbf{x}'_{i,t-1}\boldsymbol{\gamma}_h + \mathbf{x}'_{t-1}\boldsymbol{\beta}_h + v_{i,t+h}, \tag{9}$$

where $s_{i,t}$ denotes the share of veterans in the state population or the share of Social Security recipients for the respective episode. 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 share across states, while Panel b reports the average Social Security recipient share. There is substantial geographic heterogeneity in both measures, providing meaningful variation for identification. States in the South and parts of the West generally exhibit higher exposure to veterans' payments, while Social Security recipient shares are particularly elevated in states with older populations. In addition to the cross-sectional heterogeneity, both measures also display meaningful time variation over the sample period, particularly the Social Security recipient share, reflecting demographic changes and the gradual expansion of Social Security over time.

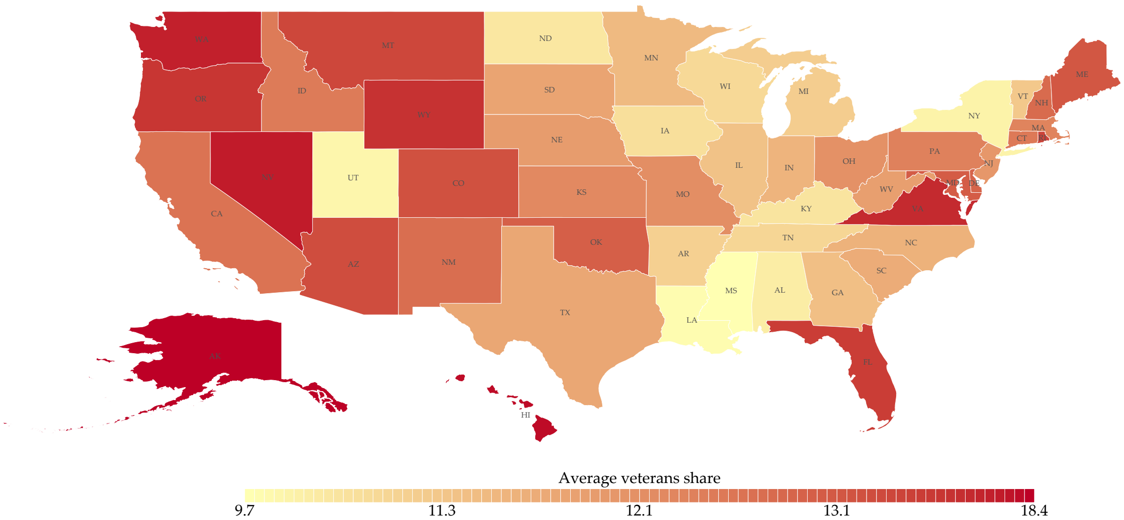
Because identification now comes from differential exposure across states, we can additionally include time fixed effects $\lambda_{t,h}$, thereby absorbing all aggregate shocks and common macroeconomic dynamics:

$$y_{i,t+h} = \alpha_{i,h} + \lambda_{t,h} + \varphi_h \left(z_{T,t} \times s_{i,t}\right) + \mathbf{x}'_{i,t-1}\boldsymbol{\gamma}_h + v_{i,t+h}. \tag{10}$$

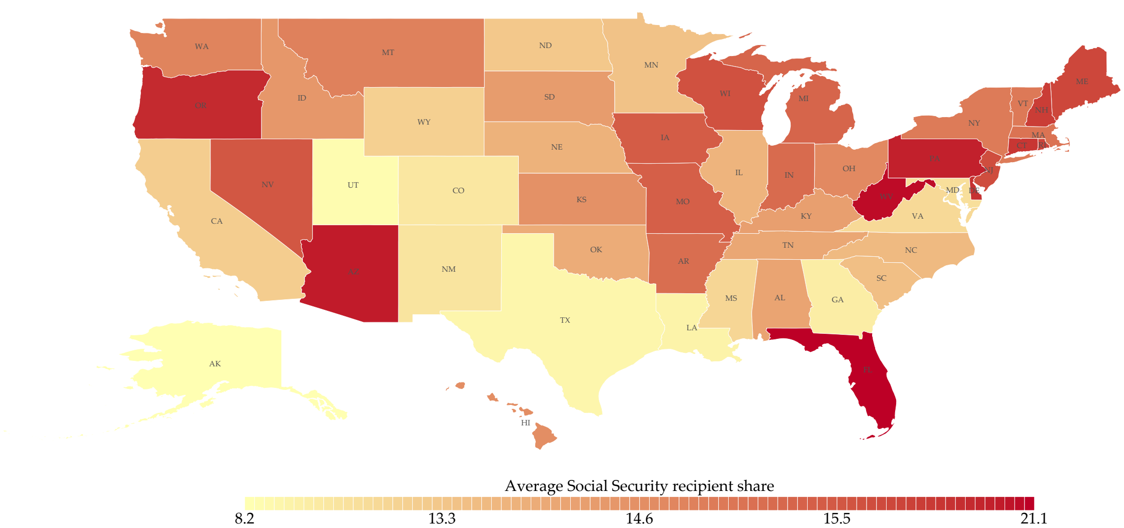
Figure B.7b presents the estimated response. The estimates point to economically meaningful heterogeneity across states. The shape of the interaction response closely mirrors the baseline effect, with employment gradually increasing over time and peaking after roughly one year. Quantitatively, a 1 percentage point increase in the veterans' or Social Security recipient share raises the employment response by approximately 0.05 percent at peak. Hence, states with larger populations directly exposed to the transfers experienced systematically stronger labor market expansions following the shocks. Importantly, these results remain very similar

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

(a) Veterans Share



(b) Social Security Share



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.

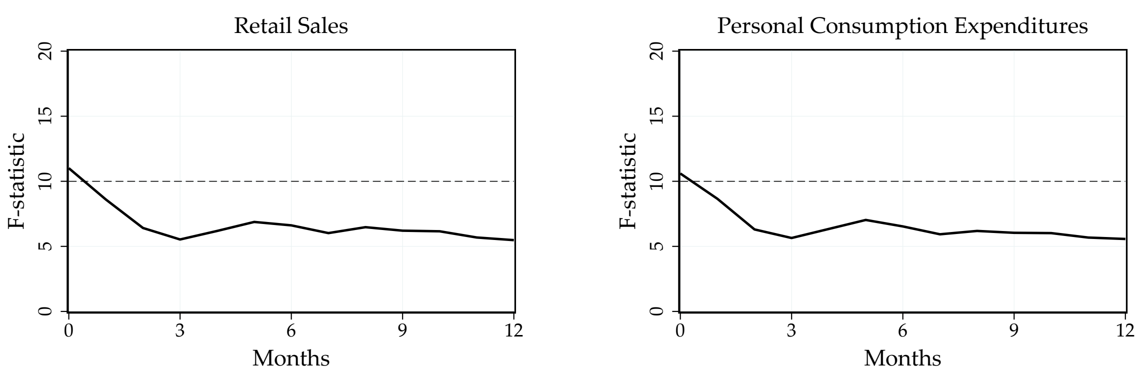
even after including time fixed effects, which absorb all aggregate macroeconomic fluctuations and common national shocks. This suggests that the aggregate controls included in the baseline specification do a good job capturing these common macroeconomic factors.

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.

B.5.3. First Stage of Multiplier Regression

Figure B.9 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.9: First-Stage F-Statistics for Transfer Multipliers



Notes: The figure reports first-stage F-statistics for the transfer-shock instrument underlying the cumulative transfer multiplier estimates. Sample: 1945m10–2019m12.

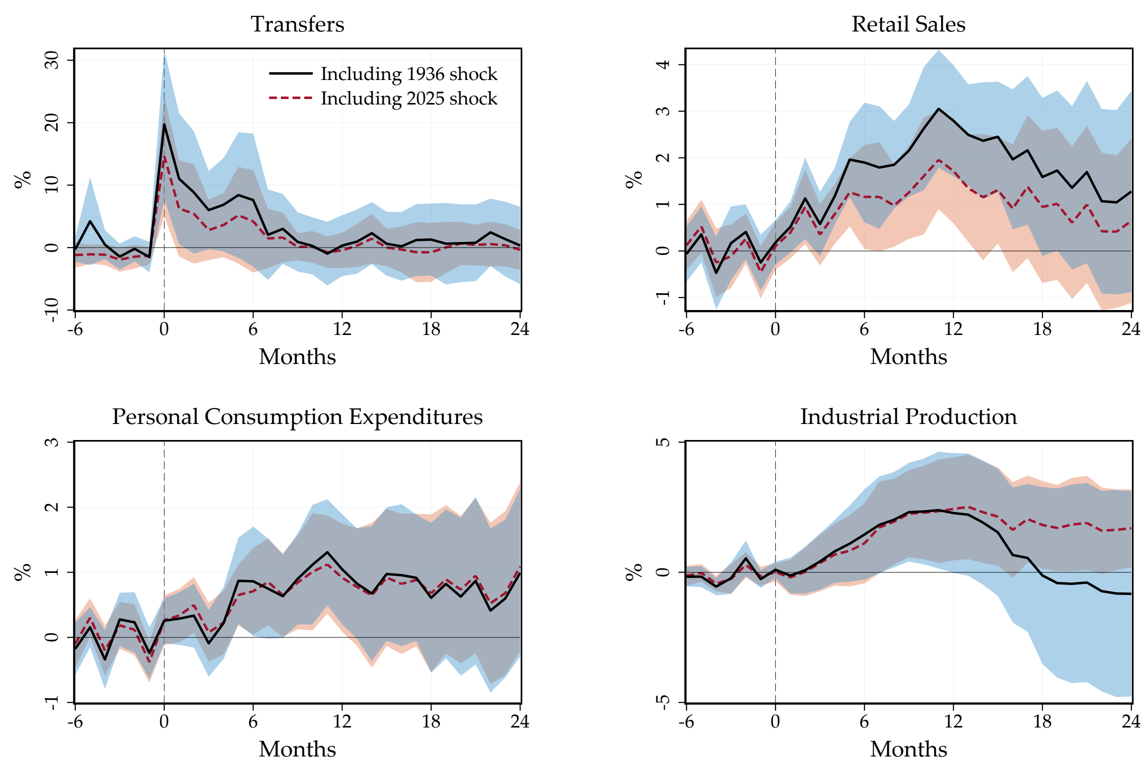
B.6. Additional Sensitivity

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. Shock Selection

Figure B.10 examines robustness to extending the sample to include 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. The estimated responses of transfers and personal consumption expenditures are broadly similar to the baseline across both extensions, indicating that our findings are not sensitive to the exclusion of these episodes.

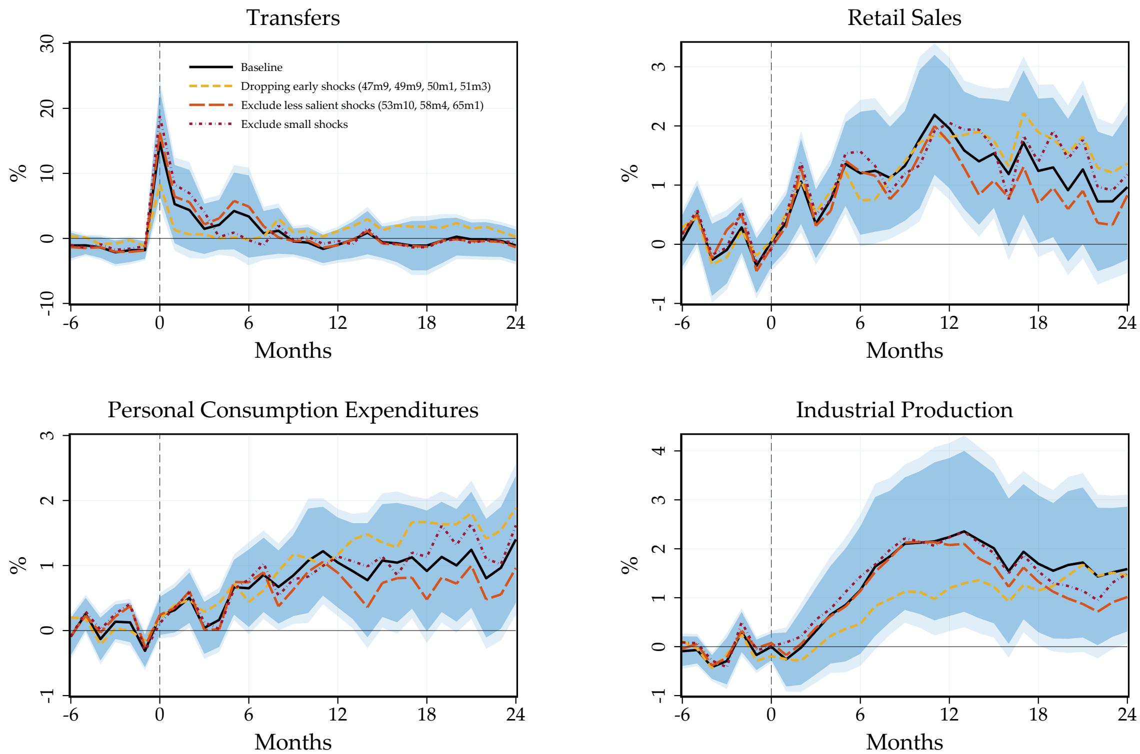
Figure B.10: Robustness to Additional Transfer Episodes (1936 and 2025)



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1) with the narrative transfer shock dates. The figure shows robustness to including the June 1936 bonus payment (sample: 1935m1–2019m12), and including the March 2025 Social Security Fairness Act payment (sample: 1945m10–2019m12), respectively. Solid and dashed lines: point estimates. Shaded areas: 95% confidence bands.

Figure B.11 reports the responses of all baseline variables under alternative selections of transfer shocks.

Figure B.11: Sensitivity to Shock Selection



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1) with the narrative transfer shock dates. The figure shows robustness to alternative shock selections, including dropping early episodes, excluding less salient shocks, and restricting attention to larger events. Sample: 1945m10–2019m12. Solid black lines and blue shaded areas denote the baseline point estimates and corresponding 90% and 95% confidence bands; colored lines denote alternative specifications.

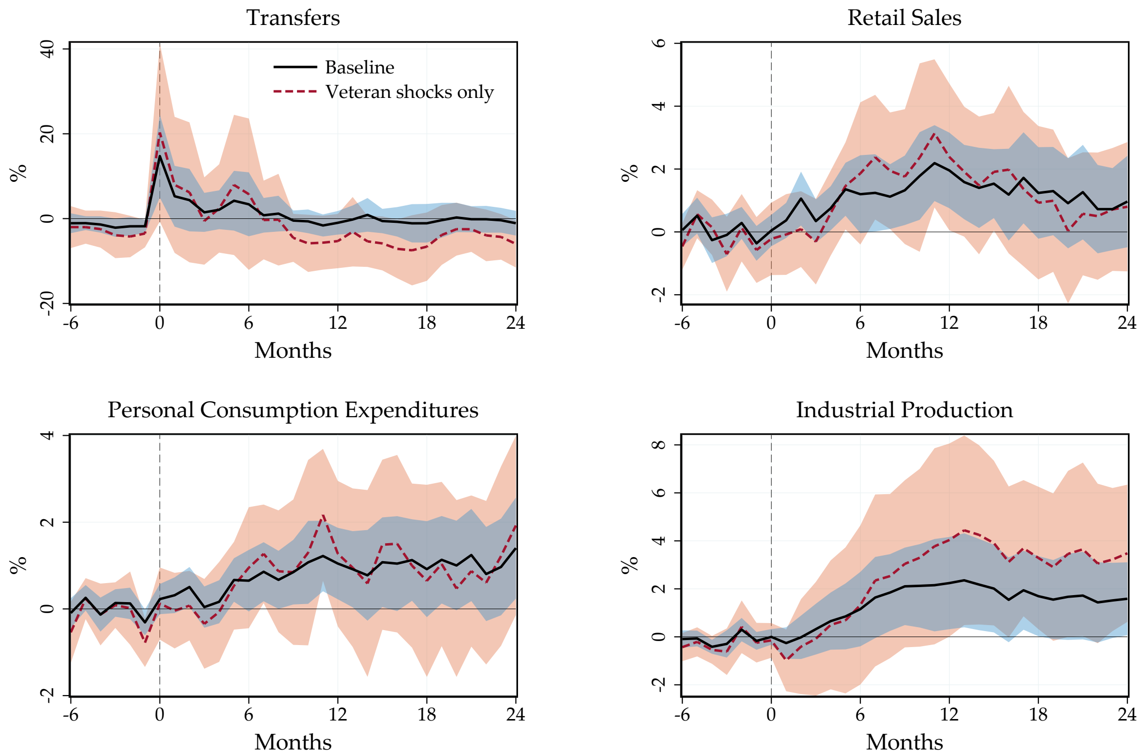
Figure B.12 presents a systematic leave-one-out (jackknife) exercise. For each specification, we re-estimate the impulse responses dropping one transfer shock at a time. The resulting responses are tightly clustered around the baseline estimates and lie well within the 90% confidence bands. This indicates that our findings are not driven by any single episode and are robust to the exclusion of individual shocks.

Figure B.12: Leave-one-out Exercise

Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1), excluding one event in the transfer shock series at a time. Sample: 1945m10–2019m12. Solid black lines and blue shaded areas denote the baseline point estimates and corresponding 90% and 95% confidence bands. Gray lines report jackknife estimates, obtained by re-estimating the specification while excluding one transfer shock at a time.

Figure B.13 reports the responses of all baseline variables when the shock series is restricted to the veterans' payment episodes only. The point estimates are very similar to the baseline across all variables, although the responses are estimated less precisely and are only borderline statistically significant, at best at the 95% level.

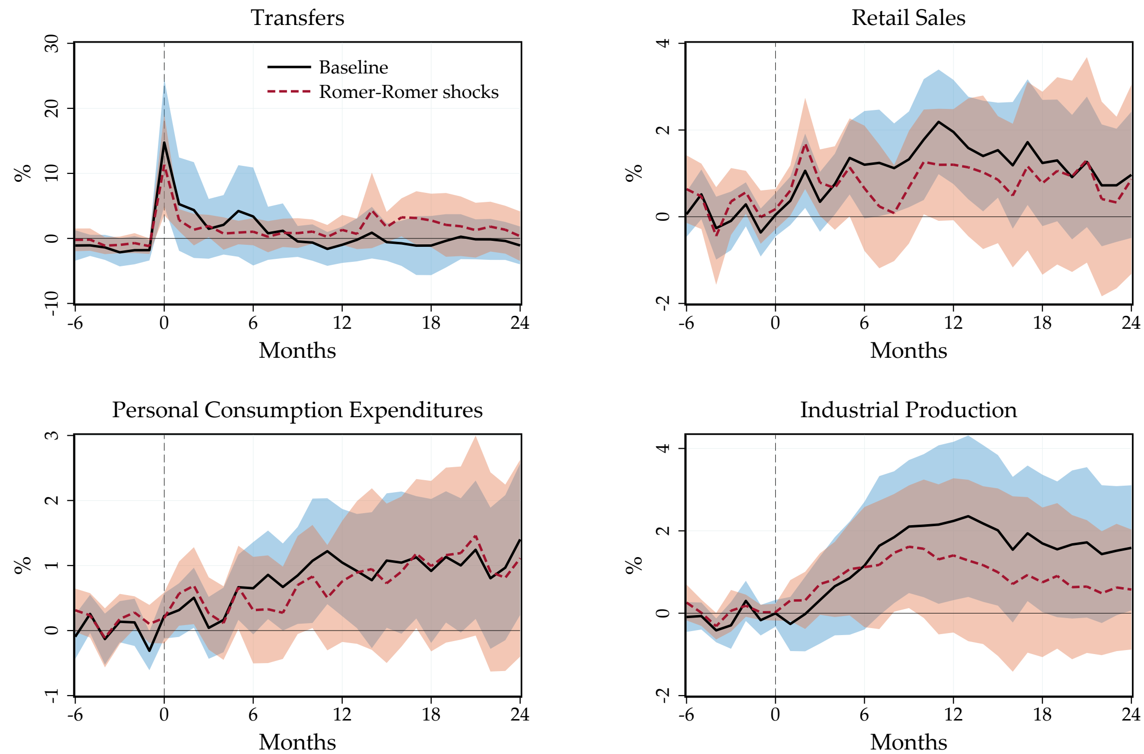
Figure B.13: Using Veteran Shocks Only



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1) with the narrative transfer shock dates restricted to veterans' payment episodes. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure B.14 reports the responses of all baseline variables when the shock series is restricted to the Romer and Romer (2016) Social Security transfer episodes. The point estimates are broadly similar to the baseline across all variables, although somewhat attenuated and estimated less precisely, with responses generally only borderline statistically significant at the 95% level. These results illustrate that neither set of transfer shocks in isolation—neither the veterans' payments nor the Social Security episodes—provides sufficient statistical power to estimate the effects precisely.

Figure B.14: Using Romer-Romer Transfer Shocks

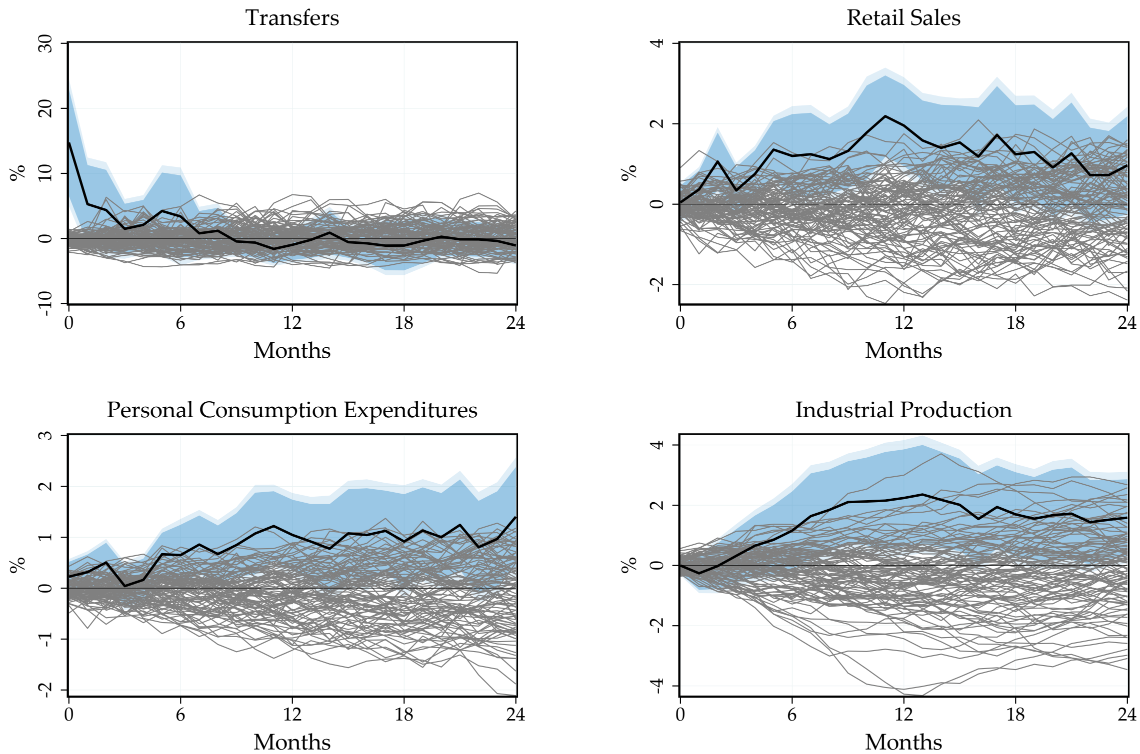


Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1) with the narrative transfer shock dates restricted to the Romer and Romer (2016) Social Security transfer episodes. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

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 200 times.

Figure B.15 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.15: Placebo Exercise



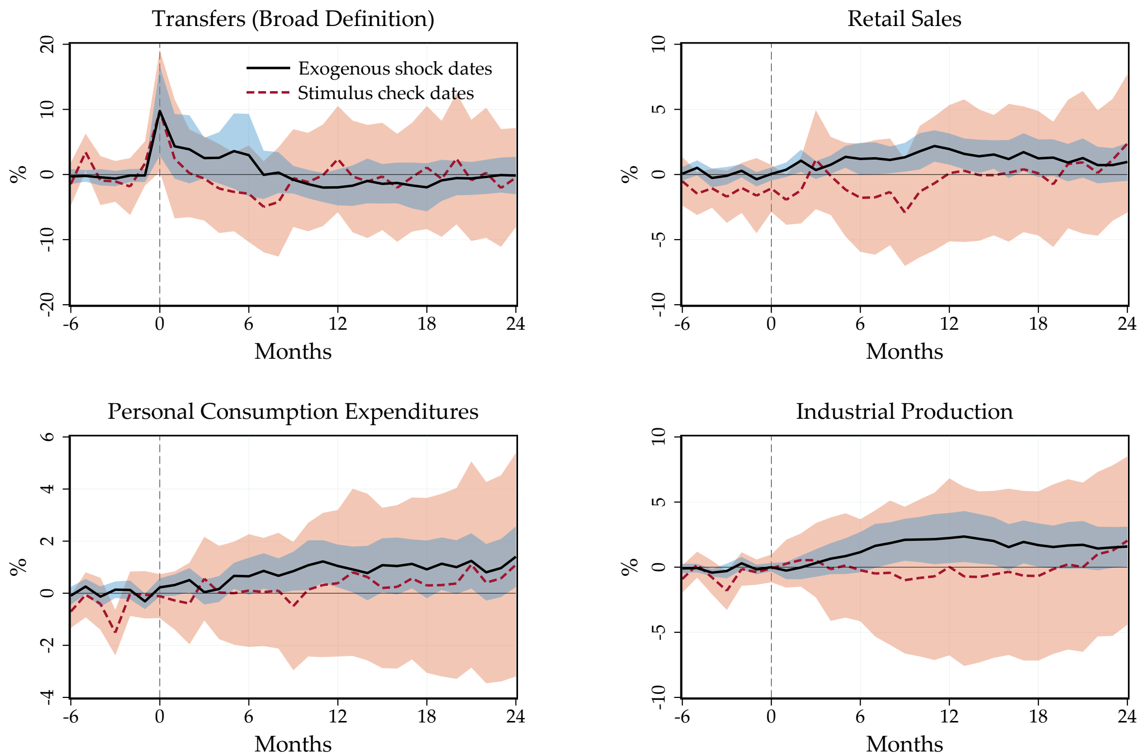
Notes: Impulse responses to placebo transfer shocks, estimated using the local projections specification (1). Placebo shocks are constructed by randomly selecting 22 event dates that do not coincide with identified transfer episodes; the procedure is repeated 200 times. Gray lines: impulse responses from each placebo draw. Solid black lines and blue shaded areas: baseline point estimates and corresponding 90% and 95% confidence bands. Sample: 1945m10–2019m12.

B.6.2. Endogenous Versus Exogenous Shocks

As discussed in the main text, we benchmark our baseline results against responses obtained from plausibly endogenous transfer episodes. The main text focuses 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. In this appendix, we 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.16 compares the responses obtained from our baseline exogenous transfer shock dates to those generated by the 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.

Figure B.16: Endogenous Versus Exogenous Transfer Shocks



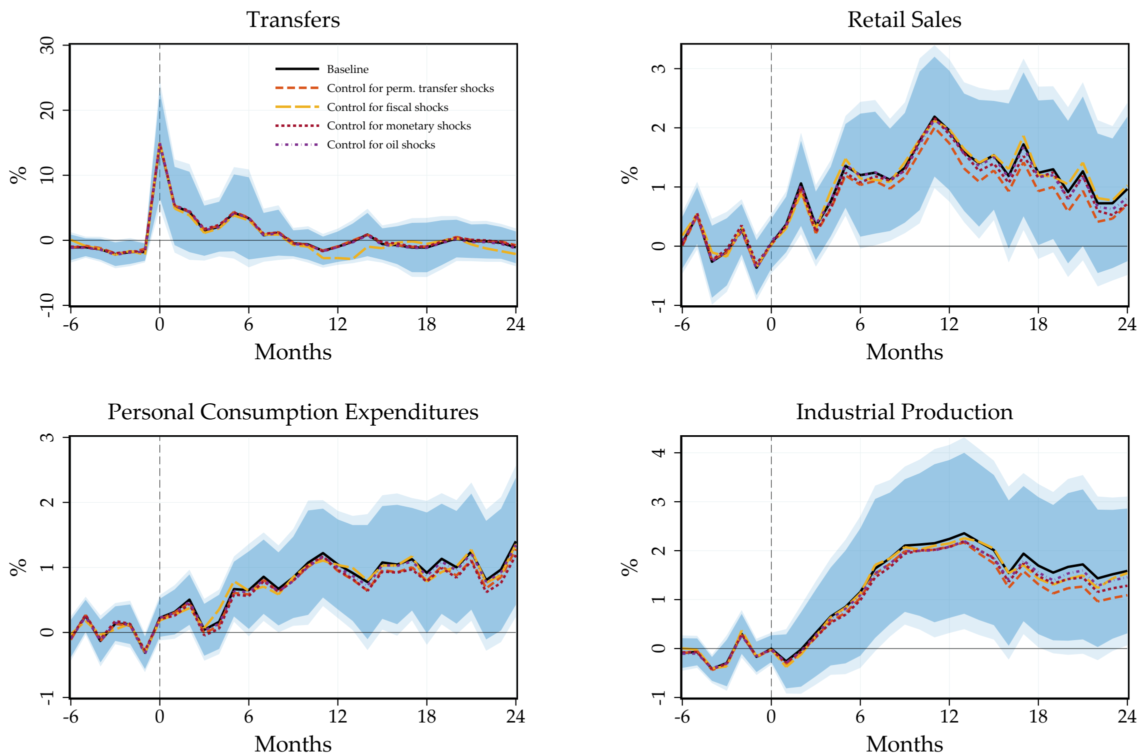
Notes: Impulse responses to a temporary transfer shock estimated using the local projection specification (1). The black solid lines and blue shaded areas show the baseline responses with 95% confidence bands obtained using the exogenous narrative transfer shock dates. The red dashed lines and brown shaded areas show responses with 95% bands using endogenous transfer shock dates, including endogenous veteran payments, a temporary Social Security payment identified by Romer and Romer (2016), and major stimulus check programs. Endogenous shocks are scaled such that the impact response of transfers (broad definition) matches that from the exogenous shock responses. Sample: 1945m10–2019m12.

B.6.3. Controls and Sample Period

In this appendix, we examine the robustness of our results to alternative control variables and sample definitions.

Figure B.17 reports the responses of all baseline variables under alternative control specifications, extending the analysis in the main text beyond transfers and consumption. The results remain very similar across specifications, indicating that our findings are not sensitive to the inclusion of additional controls.

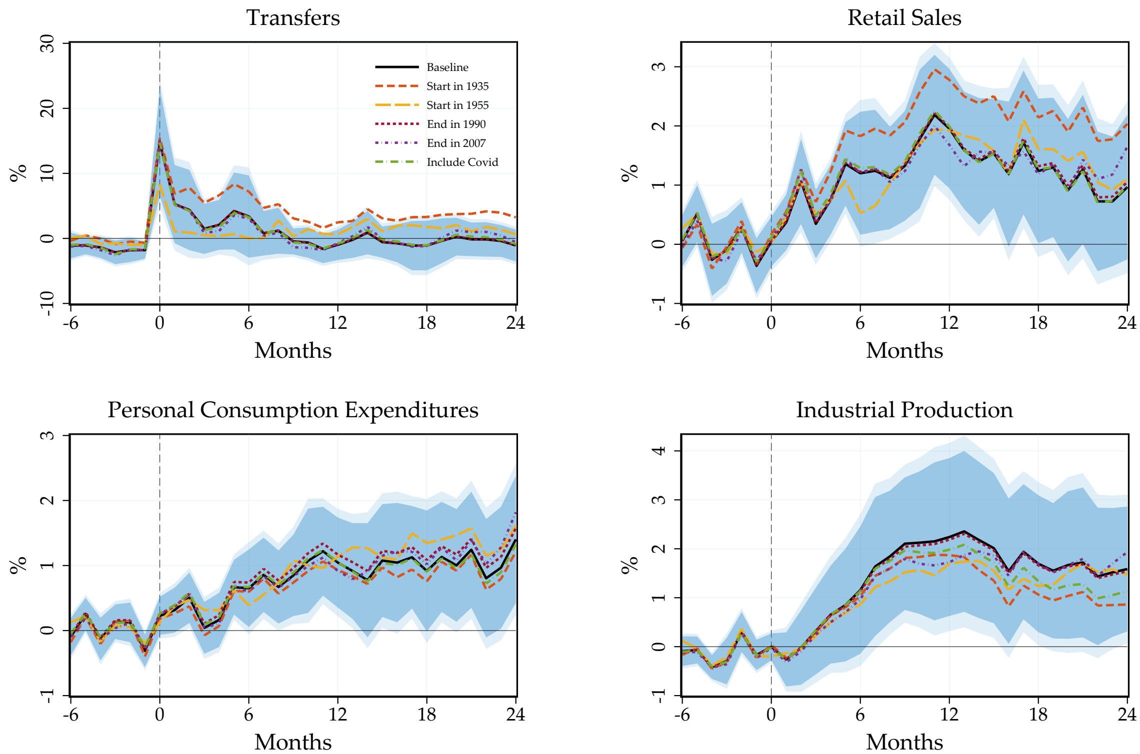
Figure B.17: Sensitivity with Respect to Controls



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1) with the narrative transfer shock dates. The figure reports robustness to alternative control specifications, including adding controls for permanent transfer shocks, military spending shocks, monetary policy shocks, and oil price shocks. Sample: 1945m10–2019m12. Solid black lines and blue shaded areas: baseline point estimates and corresponding 90% and 95% confidence bands. Colored lines: point estimates under alternative specifications.

Figure B.18 reports the responses of all baseline variables under alternative sample periods. The results are similar across sample periods, including different start and end dates and the inclusion of the COVID period, indicating that our findings are not sensitive to the chosen sample period.

Figure B.18: Robustness with Respect to Sample Period

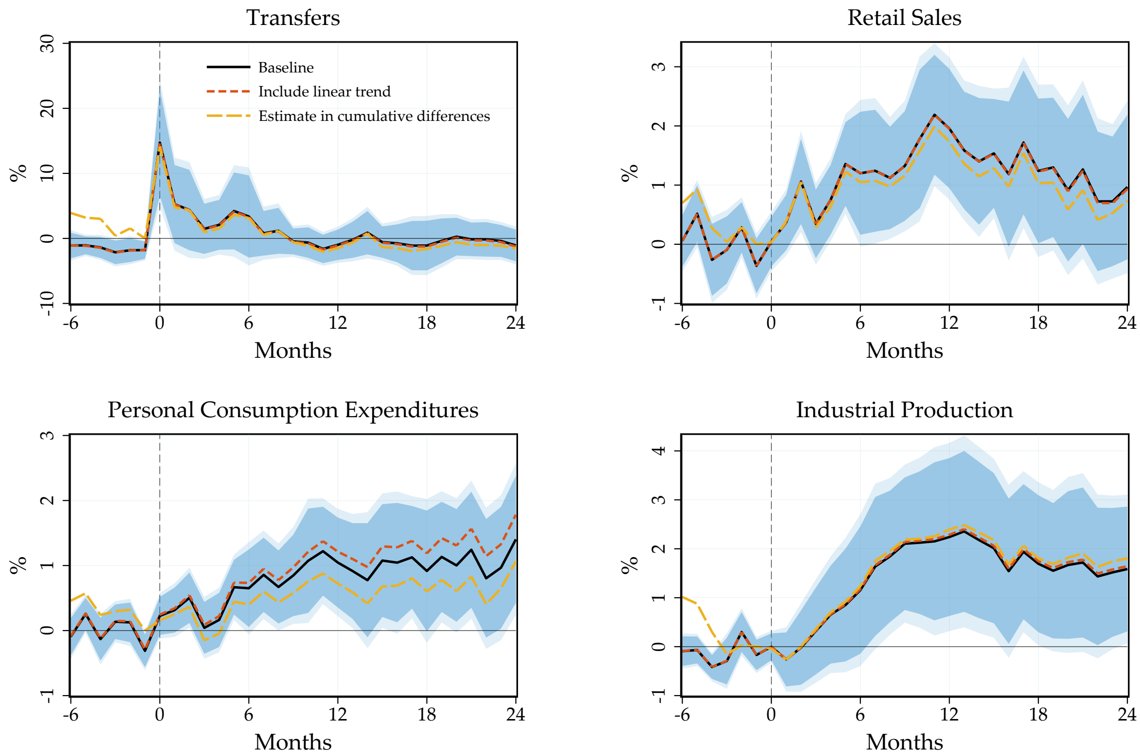


Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1) under alternative sample periods: 1935m1–2019m12, 1955m1–2019m12, 1945m10-1990m12, 1945m10-2007m12, and 1945m10-2025m12. Baseline sample: 1945m10–2019m12. Solid black lines and blue shaded areas: baseline point estimates and corresponding 90% and 95% confidence bands. Colored lines: point estimates under alternative specifications.

B.6.4. Other Specification and Estimation Choices

Figure B.19 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.

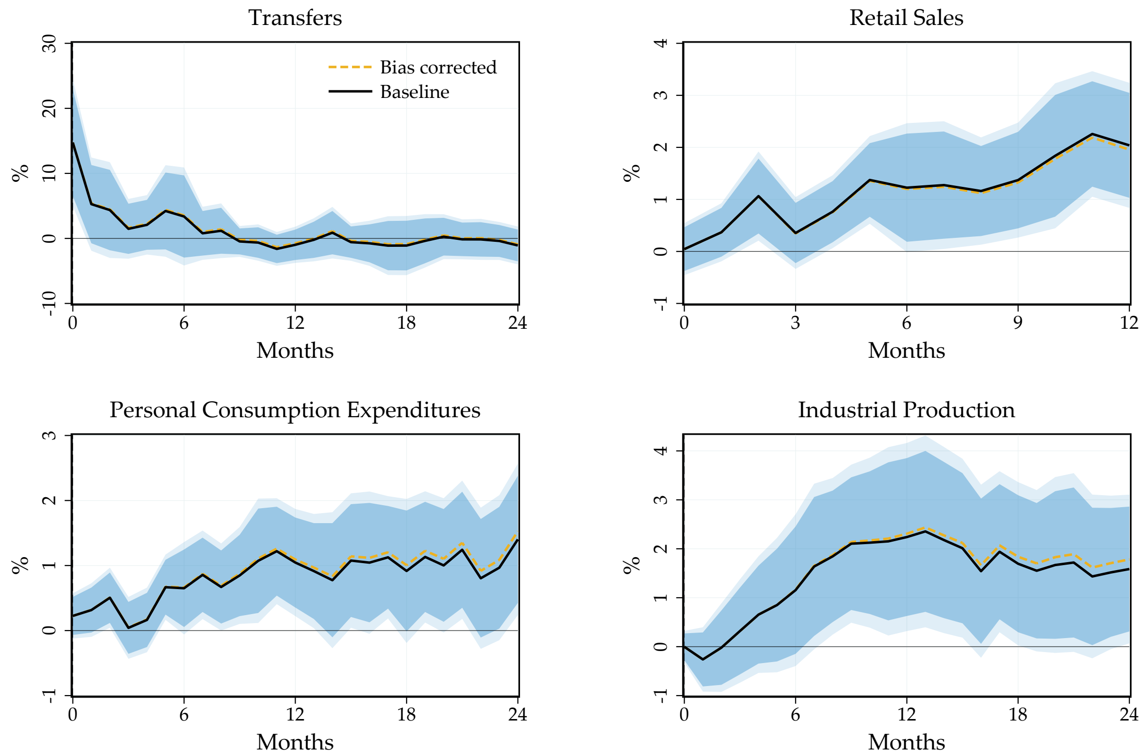
Figure B.19: Robustness with respect to detrending



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1), under alternative detrending choices (including linear trend or estimating responses in cumulative differences). Sample: 1945m10–2019m12. Solid black lines and blue shaded areas: baseline point estimates and corresponding 90% and 95% confidence bands. Colored lines: point estimates under alternative specifications.

Finally, we assess whether our estimates could be affected by small-sample bias, as suggested in Olea et al. (2025). Figure B.20 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.20: Robustness with respect to small-sample LP bias correction



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (1). Sample: 1945m10–2019m12. Solid black lines and blue shaded areas: point estimates with the Herbst and Johannsen (2024) small-sample bias correction and corresponding 90% and 95% confidence bands. Dashed yellow lines: baseline local projection estimates.

C. Appendix to Section 4

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	Elasticity of intertemporal substitution	0.5
ψ	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	0.8%
ϕ	Transfer targeting	0.158
P	Veteran insurance premium	0.0219
Prices and nominal frictions		
r	Real interest rate	0.17%
κ_w	Wage rigidity	0.0062

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	0.63	0.55	2.04	1.96
12 months	1.01	0.90	5.81	5.64

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

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,

$$N_{u,t} = \left(\frac{W_{u,t}}{W_t} \right)^{-\mu_w} N_t, \tag{11}$$

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 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 (11), union u solves

$$\max_{\{W_{u,t+h}\}_{h\geq 0}} E_t \sum_{h=0}^{\infty} \beta^h \left[u'(C_{t+h})(1-\gamma)\frac{Z_{t+h}}{N_{t+h}} W_{u,t+h}\frac{N_{u,t+h}}{P_{t+h}} - v'(N_{t+h})N_{u,t+h} - \frac{1}{2\kappa_w}\left(\frac{W_{u,t+h}}{W_{u,t+h-1}}-1\right)^2\right]. \tag{12}$$

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 (12) 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 (11), we obtain

$$\begin{aligned}\left(\frac{W_{u,t}}{W_{u,t-1}}-1\right)\frac{W_{u,t}}{W_{u,t-1}} = \kappa_w&\left[-(\mu_w-1)u'(C_t)(1-\gamma)\frac{Z_t}{N_t}\frac{W_{u,t}N_{u,t}}{P_t} + \mu_w v'(N_t)N_{u,t}\right]\\ &+\beta E_t\left[\left(\frac{W_{u,t+1}}{W_{u,t}}-1\right)\frac{W_{u,t+1}}{W_{u,t}}\right].\end{aligned} \tag{13}$$

In a symmetric equilibrium, all unions choose the same wage, so $W_{u,t} = W_t$ and $N_{u,t} = N_t$. Since $W_t/P_t = w_t$, (13) becomes

$$\pi_t^w(1+\pi_t^w) = \kappa_w\mu_w\left[v'(N_t)N_t - \frac{\mu_w-1}{\mu_w}(1-\gamma)Z_t u'(C_t)\right] + \beta E_t\left[\pi_{t+1}^w(1+\pi_{t+1}^w)\right]. \tag{14}$$

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

$$v'(N) = \frac{\mu_w-1}{\mu_w}(1-\gamma)\frac{Z}{N}u'(C). \tag{15}$$

C.2.2. Linearization

Log-linearize (14) 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 (15), the linearized version of the bracketed term in (14) 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 $\{\omega_{i,t}^{v} T_t^{v}, \omega_{i,t} T_t\}$, 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 $X_{t+h} = \mathbb{M}_{t+h}^{X}\epsilon$, 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:

$$E_{i,t}^{NI}[\epsilon] = \frac{\tau(t+1)}{1+\tau(t+1)} \left(\frac{1}{t+1} \sum_{\ell=0}^{t} s_{i,\ell} \right). \tag{16}$$

Substituting the signal equation into (16) gives

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

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

$$\overline{E}_t^{NI}[\epsilon] = \frac{\tau(t+1)}{1+\tau(t+1)}\mathbb{E}_t[\epsilon] \Rightarrow \overline{E}_t^{NI}[dX_{t+h}] = \frac{\tau(t+1)}{1+\tau(t+1)}\mathbb{E}_t[dX_{t+h}]. \tag{17}$$

With only noisy information

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

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 to 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 $\epsilon \sim_{f^*(\epsilon|S_{i,t-1})} \mathcal{N}\left(\sum_{j=1}^{t} \alpha_j E^{NI}_{i,t-j}[\epsilon], 1+(t+1)\tau\right)$, 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:

$$E^r_{i,t}[\epsilon] = \sum_{j=1}^{t} \alpha_j E^{NI}_{i,t-j}[\epsilon], \qquad \alpha_j \geq 0, \qquad \sum_{j=1}^{\infty} \alpha_j = 1. \tag{18}$$

The household's distorted expectation is

$$E_{i,t}[\epsilon] = E^{NI}_{i,t}[\epsilon] + \theta\left(E^{NI}_{i,t}[\epsilon] - E^r_{i,t}[\epsilon]\right), \tag{19}$$

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

Taking cross-sectional averages in (19) and using the linearity of the reference belief yields

$$\overline{E}_t[\epsilon] = (1+\theta)\overline{E}^{NI}_t[\epsilon] - \theta \sum_{j=1}^{t} \alpha_j \overline{E}^{NI}_{t-j}[\epsilon]. \tag{20}$$

Now substitute the noisy-information average from (17) 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 of Auclert et al. (2021) and Auclert, Rognlie, and Straub (2024, 2020). Let $\mathbf{C}_g \equiv [dC_{g,0}, dC_{g,1}, dC_{g,2}, \ldots]'$ 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 $d\mathbf{T}^{\nu}$ analogously. Using this framework, we write the change of average consumption in group g as

$$d\mathbf{C}_v = \mathbf{M}_v^Y (d\mathbf{Y} - d\mathscr{T}) + \mathbf{M}_v^T \overline{\omega}_v d\mathbf{T} + \mathbf{M}_v^{T^\nu} \frac{1}{\mu_v} d\mathbf{T}^\nu \tag{21}$$

$$d\mathbf{C}_{nv} = \mathbf{M}_{nv}^Y (d\mathbf{Y} - d\mathscr{T}) + \mathbf{M}_{nv}^T \overline{\omega}_{nv} d\mathbf{T} \tag{22}$$

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 $d\mathbf{T}^\nu$ 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

$$d\mathbf{Y} = \overline{\mathbf{M}}^Y (d\mathbf{Y} - d\mathscr{T}) + \overline{\mathbf{M}}^T d\mathbf{T} + \mathbf{M}_v^{T^\nu} d\mathbf{T}^\nu, \tag{23}$$

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 $\mathbf{T}^\nu$ term does not carry a bar because population-weighted aggregation cancels the $1/\mu_v$ in the veteran consumption equation: $\mu_v \cdot \mathbf{M}_v^{T^\nu} \cdot (1/\mu_v) = \mathbf{M}_v^{T^\nu}$. These aggregate objects correspond to $\mathbf{M}^Y$, $\mathbf{M}^\omega$, and $\mathbf{M}^\nu$ in the main text.

C.5. Proof of Lemma 1

Proof. The integrated government budget constraint is

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

Rearranging,

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

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

Now substitute the tax rule (8),

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

into the integrated budget constraint to obtain

$$B_t = \big(1 + r - \tau_d\big)B_{t-1} - \mathscr{T} + \tau_d B - \nu P + (T_t + T_t^v).$$

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 $\mathscr{T}_t$ is pinned down by the tax rule as a function of B_{t-1}, the tax sequence $\{\mathscr{T}_t\}_{t\geq 0}$ 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 $\{\mathscr{T}_t\}_{t\geq 0}$ 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. Proof of Proposition 2

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

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

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

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

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

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

Rearranging, we obtain

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

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

which is the desired result. ■

C.8. 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 kernel,

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

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:*

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

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

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

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, $\tilde{a}_{i,t} \geq -\underline{a}_{n\nu}$, 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_s^ν is received only by veterans, so the average veteran receives T_s^ν/ν and the aggregate iMPC is

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

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

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

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

C.9. 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 condition. Instead, it takes the expectations model estimated in the main analysis and applies it to an external empirical path.

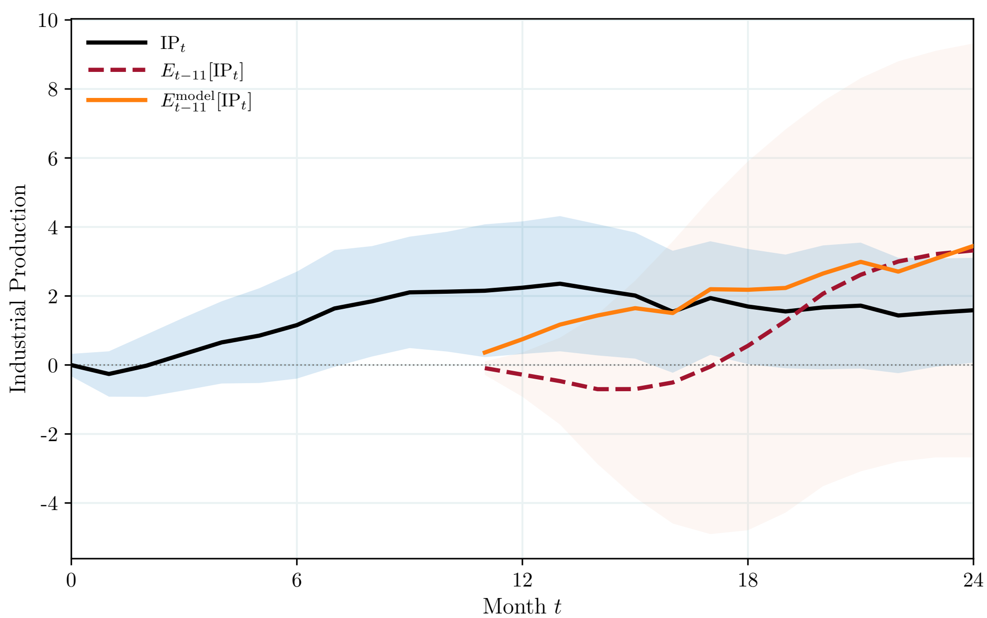
The object is industrial production. We use industrial production because it is the aggregate variable for which we can obtain expectations data over the historical period studied in the paper, although it does not map directly to a model object. The construction of this expectation series is described above; here, we use it only as an external moment for the belief block.

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.1: 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.1 reports the result. 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.

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