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 late 1940s and 1950s that mimic stimulus checks. 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, defined as the cumulative response of consumption to the cumulative response of transfers, reaches 1 after six months and over 2 after a year. We calibrate a heterogeneous-agent New Keynesian model to match our estimates. Standard specifications struggle to reproduce the persistent response of consumption, pointing to a role for imperfect expectations with underreaction on impact and overreaction at longer horizons.

JEL classification: E62, E21, E32, H31

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1. Introduction

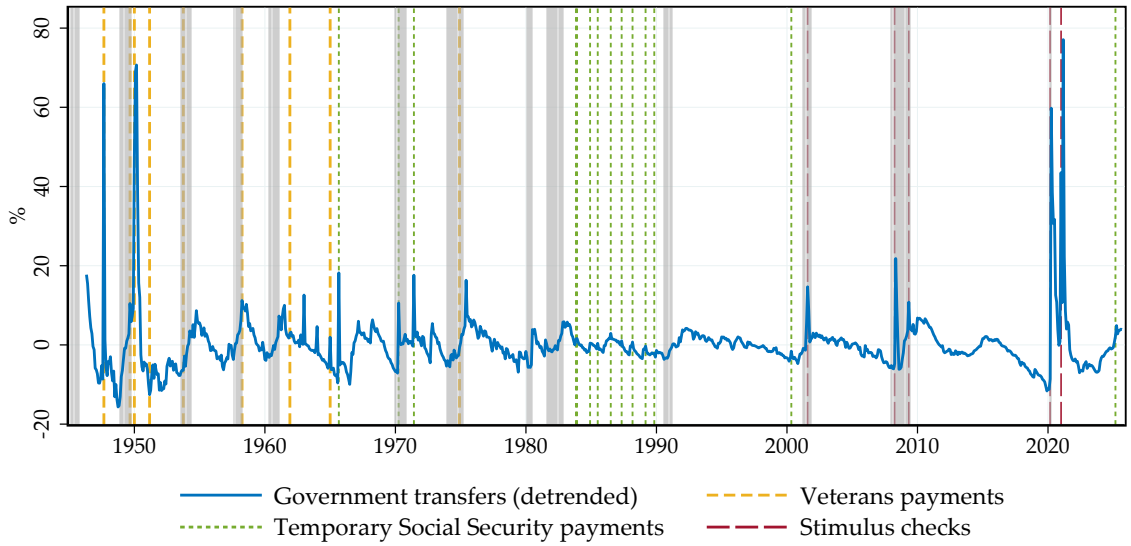
What is the effect of stimulus checks on the economy? In other words: does consumption go up when the government makes a one-off, deficit-financed, lump-sum payment to households? The answer matters because in recessions, the US government often pays out stimulus checks, and policymakers need to know 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, households spend the transfer (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 et al., 2013). But these studies hold aggregate conditions fixed, and cannot identify the macroeconomic impact of the policy. Aggregate time-series evidence is hard to interpret. Stimulus checks are normally a response to an oncoming recession, which confounds the effect of the policy. As a result, we do not yet know whether stimulus checks raise aggregate consumption—and if so by how much. By contrast, there is much 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, 2013; Ramey and Zubairy, 2018).

This paper studies a natural experiment isolating large-scale transfer payments, which 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 did not arise from countercyclical policy decisions but from 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. Because unexpectedly, bond yields turned out to be high and mortality low, 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 throughout the 1950s and 1960s. 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,

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 from Romer and Romer (2016); 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, smaller series of transfer shocks: the temporary and plausibly exogenous Social Security payments identified by Romer and Romer (2016).[1]

To study the macroeconomic effects of these transfer shocks, we assemble a new monthly dataset on government transfers and consumption spanning the postwar period. We construct a monthly series of aggregate transfers going back to 1945 by digitizing historical Treasury tables. We also compile 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 monthly deviation of log real transfer payments from its trend (measured by a Hodrick-Prescott filter). We also indicate the months with veterans payments, with temporary Social Security payments and, for comparison, the months with 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 closely resemble stimulus checks. Unlike stimulus checks, however, these payments were not enacted in response to recessions. While the postwar episodes occurred decades before recent stimulus programs—and the struc-

[1] 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.

ture of the economy may have evolved since—they nonetheless provide rare and informative variation to identify the macroeconomic effects of large, temporary transfer shocks.

To illustrate our approach, we begin with a case study of the large January 1950 payment. We 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 and personal consumption expenditures rise rapidly and remain elevated over the subsequent months. The sharp and isolated nature of the episode leaves little doubt about the interpretation: the 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 40-50% of the transfer in the subsequent year.

Motivated by this case study, we estimate the macroeconomic effects of temporary transfer payments more systematically, using the full set of postwar payment dates. Our narrative transfer shock series consists of the dates on which postwar veterans' payments and the temporary Social Security transfers were disbursed. We focus on the timing of these events—rather than their size—because the timing was largely predetermined by administrative processes and thus plausibly exogenous to macroeconomic conditions.

The key identification assumption is that within a twelve-month window around each event, no other shocks systematically coincide with these payments. This assumption is supported by 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. In contrast, some payments in the 1960s were explicitly motivated by macroeconomic stabilization; we exclude these episodes from our shock series.

We use local projections to estimate the dynamic responses of macroeconomic variables up to twelve months after each event, controlling for 12 lags of the outcome and the treatment as well as a rich set of macroeconomic indicators capturing economic activity, prices, and interest rates.

The postwar payments constitute a temporary transfer shock with large and persistent real effects. On average, transfers increase by about 10% 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 by 1% after twelve months. These responses are not only economically but also statistically significant at the 95% level. The price level rises only modestly, and the ex post short-term real interest rate remains essentially unchanged. Both durable and nondurable spending increase, while the relative price of durables is stable.

We summarize these results with a "postwar payments multiplier", defined as the cumulative response of consumption relative to the cumulative response of transfers following a transfer shock. The multiplier is approximately 1 after six months and exceeds 2 after one year.

We probe the identification assumption in several ways. First, we find no evidence of pretrends, and the timing of payments is not predictable using a broad set of macroeconomic variables. Second, our results are robust to controlling for observable shocks—including monetary policy, government spending, permanent government transfers, and oil price shocks. The estimates remain largely unchanged, suggesting that omitted variables are unlikely to drive the results (Oster, 2019). Finally, a jackknife procedure that sequentially excludes influential episodes 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 imprecise estimates, reflecting limited statistical power.

How does the postwar payments multiplier relate to the more familiar stimulus checks multiplier? Postwar payments are not identical to stimulus checks, and neither multiplier constitutes a structural object. To address this issue, we formulate a standard HANK model in the spirit of Auclert, Rognlie, and Straub (2024, 2025). To rationalize the empirical evidence—namely, a persistent response to a transitory shock—we extend the model to incorporate imperfect macroeconomic expectations. Following Bardóczy and Guerreiro (2023), we assume that households form forecasts of aggregate variables using a diagnostic-expectations framework with imperfect and noisy information (Bordalo et al. 2020). This specification captures a central feature of observed expectations: following a shock, expectations initially underreact, but subsequently overshoot realized outcomes (Angeletos, Huo, and Sastry 2021; Bardóczy and Guerreiro 2023).

We enrich the fiscal block with a veterans' life-insurance fund that distributes dividends to a subset of households, financed through an exchange of bonds for cash with the remainder of the government (the "Treasury"). The model also includes a monetary rule governing real interest rates, a tax rule governing the speed at which publicly held debt is retired, and a targeting rule determining which households receive payments; we refer to these jointly as the policy rules.

Within this environment, we establish an equivalence result. Holding fixed the policy and targeting rules, the multiplier associated with stimulus checks is identical to that associated with postwar payments. The argument proceeds through the government's consolidated bal-

ance sheet. The life-insurance fund pays veterans by exchanging its government bond holdings for cash from the Treasury. The Treasury, in turn, finances this cash outlay by issuing additional debt. From the perspective of the consolidated government, the operation is therefore equivalent to a one-time stimulus check. We further extend the result to characterize how the two multipliers may differ under alternative targeting rules.

This equivalence result provides a basis for measuring the macroeconomic effects of stimulus checks in one of two ways. First, if the policy rules governing postwar payments and stimulus checks are the same, then one may directly apply the postwar payments multiplier to stimulus checks. Second, if policy rules differ—for example, because monetary policy behaved differently in 1950 than in 2008—then one should calibrate the model to match the postwar payments multiplier, jointly identifying the relevant policy rules and other parameters. The stimulus-checks multiplier can then be computed by recalibrating the policy rules while holding the remaining model parameters fixed.

We calibrate the HANK model to match the postwar payment multipliers. Our findings indicate that reproducing the large and persistent response of consumption to a temporary transfer shock requires two ingredients: high marginal propensities to consume (MPCs) and substantial departures from full-information rational expectations (FIRE). High MPCs generate meaningful deviations from Ricardian equivalence, thereby allowing the transfer shock to significantly affect aggregate demand. At the same time, the dynamics of expectations are essential for generating the persistent, hump-shaped response of personal consumption expenditures observed in the data. Because expectations become progressively more optimistic following the shock, the model produces a hump-shaped path for expenditures. We show, however, that delayed overreaction is critical for this result. A recalibrated version of the model that includes only underreaction (in a standard noisy-information rational-expectations limit of our framework) fails to replicate the empirical consumption dynamics. The overreaction of expectations, driven by diagnostic expectations, is therefore central to the quantitative success of the model.

Related literature. This paper joins the half-century-old quest to learn 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 use an instrument that we think is more plausible, the postwar payments.

Modern work moved from the time series to the cross section, and from generic income shocks to one-time payments such as stimulus checks. In the typical cross sectional design, one estimates how consumption responds for some households who receive the payment, compared to others who do not; time fixed effects absorb all factors common to households (see e.g. Johnson, Parker, and Souleles, 2006; Parker et al., 2013; Hausman, 2016; Fagereng, Holm, and Natvik, 2021; Kueng, 2018; Boehm, Fize, and Jaravel, 2025). However this regression cannot detect the macroeconomic effect of payments, which the time fixed effects absorb. This "missing intercept" problem can be solved only by combining the micro-level estimates with additional structure, as Wolf (2023) advocates.[2] This paper takes a different approach: studying macro-level shocks that mimic stimulus checks.

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 imprecise estimates. Only when combined with the postwar veterans' payments do we obtain sufficient statistical power to estimate the effects of interest precisely.

A second important paper is Hausman (2016) who, like us, focusses on 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 set of papers is the case study evidence on the 2008 stimulus checks in Orchard, Ramey, and Wieland (2025), as well as the assessment of other stimulus check episodes in Ramey (2025) and Orchard, Ramey, and Wieland (2026).[3] These papers point out 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. The authors contemplate a series of model-based counterfactuals. 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 having to simulate a model-based counterfactual.

Methodologically, our paper relates to the fiscal multiplier literature, which exploits nar-

[2] A related approach uses cross sectional variation at the regional level to estimate the response of the economy to transfers (Corbi, Papaioannou, and Surico, 2019; Pennings, 2021). Again, one must use a model to go from regional to aggregate variation.

[3] See Hazell and Hobler (2025) for a related approach, using high frequency variation in inflation expectations, around the shock to transfers from Democrats' 2021 Georgia Senate election victory.

rative or institutional sources of exogenous policy variation. Much of this work studies government spending shocks—often military spending or defense news—to identify aggregate demand effects (Ramey, 2011; Barro and Redlick, 2011; Ramey and Zubairy, 2018). We adopt a similar narrative approach but focus on temporary lump-sum transfers rather than government purchases, allowing us to estimate a transfer multiplier from plausibly exogenous aggregate variation.

Our findings also speak to modern heterogeneous-agent macroeconomic models, in which temporary transfers can generate sizable aggregate effects (Kaplan, Moll, and Violante, 2018; Auclert, Rognlie, and Straub, 2024; Bilbiie, 2025). By providing estimates of the dynamic response of consumption, prices, and interest rates to temporary transfer shocks, we offer a transparent empirical benchmark that can serve as a calibration target for HANK and related models. We argue that in order for HANK models to match the aggregate 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). 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), Eichenbaum, Guerreiro, and Obradovic (2025), and Angeletos, Guerreiro, and Zhang (2025). Our paper is closest to Eichenbaum, Guerreiro, and Obradovic (2025), who also study the impact of deviations from FIRE for the transfer multiplier. The Ricardian Non-Equivalence effect they emphasize is also present in our baseline model. In addition, we emphasize an additional source of persistence of the spending response arising from the delayed overreaction of expectations.

Outline. The paper proceeds as follows. Section 2 introduces the institutional setting, describes the construction of the new monthly dataset, and presents the narrative identification of transfer shocks. Section 3 outlines the empirical strategy, reports the main reduced-form evidence on the macroeconomic effects of temporary transfers, and quantifies these effects in terms of cumulative transfer multipliers. Section 4 develops a theoretical framework to interpret the empirical findings and to map postwar transfer shocks into stimulus-check–type policies. Section 5 concludes.

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 post-

war 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 diagnostic evidence on the properties of the resulting narrative 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 monthly frequency.

The first step in our data construction is to build a monthly transfer series. We outline the steps taken here, while Appendix A.1 contains the full details. 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. These series provide the backbone for our historical reconstruction.

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.[4] We additionally digitize monthly payments reported in the *Monthly Treasury Bulletin* for the Armed Forces Leave Bond program. Combining these sources allows us to construct a comprehensive and consistent account of monthly transfers in the postwar period.

From this 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.

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

[4] Health-related transfers, such as Medicare and Medicaid, are quantitatively small in the early part of our sample.

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. We construct a series for the short term interest rate by extending back the Federal Funds Rate, available only from July 1954, using the 3 month Treasury Bill rate (available from January 1934). 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. Second, the consumer price of durables is only available at monthly frequency since December 1955. We extend this back through a Chow-Lin procedure using quarterly data for the consumer price of durables alongside monthly (aggregate) consumer prices and durables prices from the Producer Price Index (series: furniture and household durables).

Finally, we also present corroborating evidence on the micro-level effect of the transfers. 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.

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 (Veterans' Special Life Insurance) and continued in subsequent years. Many veterans retained their policies after the war, a transition actively encouraged by the program.

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, they 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.

In general, if a life insurance fund is overfunded, there are two options: either to pay the excess back as dividends, or lower premia going forward. In this case, legislation mandated the first option. 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, Souleles, Johnson, and McClelland, 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 timing was fixed long in advance by administrative timelines. President Truman first announced that the dividend would be paid in a speech in January 1948. However, given the state of technology, making the payments was slow. Assembling the recipients, figur-

ing out how much each was owed, and so on, took two years. We return to the timing in more detail below when discussing the identification of these transfer shocks.

The January 1950 payment was the first 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 excess. In March 1951 there was a second special dividend to pay more of the excess out of the WWII fund. There were further special dividends associated with the WWII fund in July 1961 and January 1963. There were also similar, but much 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. In part, the mistakes in premia-setting were not so large from the 50s onwards. Moreover, from 1952 onwards, the administration introduced a mechanism designed to prevent so many large, and potentially destabilizing payments. Veterans would receive "regular dividends" on the anniversary of their enrollment date. This mechanism meant that excess payments out of the fund would be made continuously throughout the year. Therefore unlike special dividends, regular dividends did 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 that point, government life insurance used excess in the fund to lower premia, instead of paying dividends (U.S. Department of Veterans Affairs, 2025).

There were two other large payments made to veterans after World War II. In September 1947, the Armed Forces Leave Bond (U.S. Department of the Treasury, 1949). As part of their contracts, service members received vacation days during WWII. However many were unable to take leave due to the demands of the war, meaning that their leave accrued. Therefore after the war ended, 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, as we will discuss in more detail, there had been mounting political pressure to allow veterans to redeem these bonds early. The political pressure rose in large part because officers had been paid immediately for unused leave at the end of the war, whereas enlisted men had been forced to wait. Bowing to the pressure, the Truman administration allowed veterans to convert the bonds to cash in September 1947. 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. In the first column is the payment, corresponding to either the Armed Forces Leave Bond, dividends out of one of three life insurance funds, or compensation for benefits. In the second column is the first month of payments. The source for this date is in the third column. The dates are sourced from either official documents (e.g. the Veterans Benefits Insurance Manual) or for the more obscure payments from newspaper articles. These dates will form the chronology of our shock series. The fourth and fifth column present the size of the shock, either in nominal terms or scaled to quarterly GDP in the previous quarter. We take these shocks to be exogenous to the business cycle; the next subsection explains the rationale for this classification in detail. We also discuss some additional candidate transfer payments, which we consider, but discard because they are endogenous.

2.3. Which Veteran Payments Are Exogenous?

Our identification assumption is that the timing of veterans payments is not correlated with other factors affecting the economy, within a 12 month window. We do not make assumptions about whether the size of the payments is random with respect to the economy.

To identify which payments are exogenous, we scrutinize archival documents from the Veterans' Administration (principally the Department of Veterans Affair's M-29 Insurance User Manual); the Congressional Record; presidential speeches; and contemporary press accounts.

January 1950 special dividend. We start by discussing the largest shock, the dividend to Second World War veterans paid in January 1950. Here, 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 indeed take

Table 1: Postwar Veterans Payments

Programme	**First month**	**Date source**	**Nominal size**	**Size / quarterly GDP**	**Size source**
Armed Forces Terminal Leave	Sep. 1947	U.S. Department of the Treasury (1949)	$2.1 billion	3.36%	U.S. Department of the Treasury (1949)
World War One insurance dividend	Sep. 1949	Sullivan Daily Times (1949)	$40 million	0.06%	U.S. Department of Veterans Affairs (2021)
World War Two insurance dividend	Jan. 1950	U.S. Department of Veterans Affairs (2021)	$2.9 billion	4.12%	U.S. Department of Veterans Affairs (2021)
World War Two insurance dividend	Mar. 1951	U.S. Department of Veterans Affairs (2021)	$685 million	0.81%	U.S. Department of Veterans Affairs (2021)
World War One insurance dividend	Oct. 1953	The Fayette County Record (1953)	$64 million	0.07%	The Fayette County Record (1953)
World War One insurance dividend	Apr. 1958	The Brantley Enterprise (1958)	$32 million	0.03%	U.S. Department of Veterans Affairs (2021)
Korean War insurance dividend	Dec. 1961	Atlanta Daily World (1962)	$60 million	0.04%	U.S. Senate Committee on Finance (1961)
World War Two insurance dividend acceleration	Jan. 1965	U.S. Department of Veterans Affairs (2021)	$200 million	0.11%	U.S. Department of Veterans Affairs (2021)
Veterans Compensation Act	Dec. 1974	United States Congress (1974)	$217 million	0.05%	United States Congress (1974)

Notes: this table contains the set of postwar veterans payments.

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 reports 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 2 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, especially because officers had already received payment and the enlisted 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 stated (U.S. House of Representatives, 1947):

> *"We have been informed by the Treasury Department that to pass such a bill would increase the hazards of inflation. I say that those who hold that view have not considered all of the facts. Many of our veterans need their terminal-leave pay in cash now, for many reasons. They need their money to pay the butcher and the grocer and to keep a roof over their heads. They need it to supplement their income in the case of those who are going to school, and they need it to help start small businesses ... enactment of this bill will correct a great injustice. 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."*

Similarly, 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.

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. However again, results are robust to dropping this shock.

June 1936 veterans bonus. There is an additional payment to veterans that is not part of the baseline shock series in Table A.4, but we will study in certain additional exercises. This payment is the bonus paid to World War I veterans in 1936, during the middle of the Great Depression. This payment is the focus of Hausman (2016), which provides a detailed discussion of the motives. In particular, World War I veterans had been granted the bonus in 1924, as a reward for service during the war. The bonus vested in 1945; however veterans wished to receive the payment earlier due to the hardship of the Great Depression. The timing of the bonus appears to have been exogenous to the state of the business cycle, at least within a 12 month window. The bonus was paid in 1936 due to protracted lobbying by veterans, spanning multiple years. While Congress wished to help veterans, the Presidency demurred due to the fiscal implications, meaning the bonus was passed over a Presidential veto after extended negotiations. We will not include this payment in the baseline, in order to focus on the less volatile macroeconomic data after World War II.

We now discuss an additional set of veterans payments that we deem endogenous. These veterans payments are summarized in Appendix Table A.5.

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 con-

structs a narrative measure of changes in Social Security benefits over the period 1952–1991, using detailed historical sources to identify the timing, size, and motivation of benefit changes. In the same spirit as our approach for veterans' payments, they use narrative evidence to isolate variation in transfers that is plausibly exogenous to macroeconomic conditions. These shocks are smaller than the veterans payments (as Figure 1 shows).

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 classify each policy change according to its motivation and persistence. Importantly, they exclude benefit increases that were explicitly undertaken for short-run macroeconomic stabilization.

Within this framework, 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. Just as 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 the analysis in Romer and Romer (2016) to include 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 the narrative framework of exogenous transfer shocks. The first payment is the Senior Citizens' Freedom to Work Act of May 2000, which generated 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 from 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 Social Security 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.4 provides an overview of all 24 identified events, comprising both postwar veterans' payments and temporary Social Security payments. As the table makes clear, there are 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' payments and the temporary Social Security payments provide a unified series of stimulus-check-like transfer shocks. While the Social Security events are smaller and individually less informative, their inclusion helps improve precision in estimating the macroeconomic effects of temporary transfers. As we will see, by themselves these shocks are not quite powerful enough to estimate the macroeconomic effect of temporary transfers.

2.5. Diagnostics of the Narrative Transfer Shock Series

In Appendix B.1, we assess the empirical properties of our narrative transfer shock series to provide further support for its use in identifying exogenous variation in transfers. A key requirement for our approach is that the timing of transfer shocks is not systematically related to prior macroeconomic conditions. To this end, we examine whether transfer shock dates are predictable using lagged macroeconomic variables. We find no evidence that past values of standard macroeconomic indicators forecast the occurrence of transfer shocks, consistent with the view that these events are not systematically driven by the business cycle.

We also examine whether the narrative transfer shocks are correlated with other identified structural shocks. In particular, we consider military spending shocks, tax shocks, monetary policy shocks, and oil supply shocks. Across specifications, we find that the transfer shock series is orthogonal to these alternative sources of macroeconomic fluctuations, 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. By construction, the narrative transfer shocks exhibit some degree of autocorrelation. However, this feature is largely mechanical given the dummy nature of the shock variable. In our empirical specifications, we account for this by including lags of the narrative transfer shock, ensuring that the estimated dynamic responses are not driven by serial dependence in the shock series. Additional details, along with the corresponding figures and tables, are provided in Appendix B.1.

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 the postwar payments.

3.1. Motivating Case Study: the January 1950 Payment

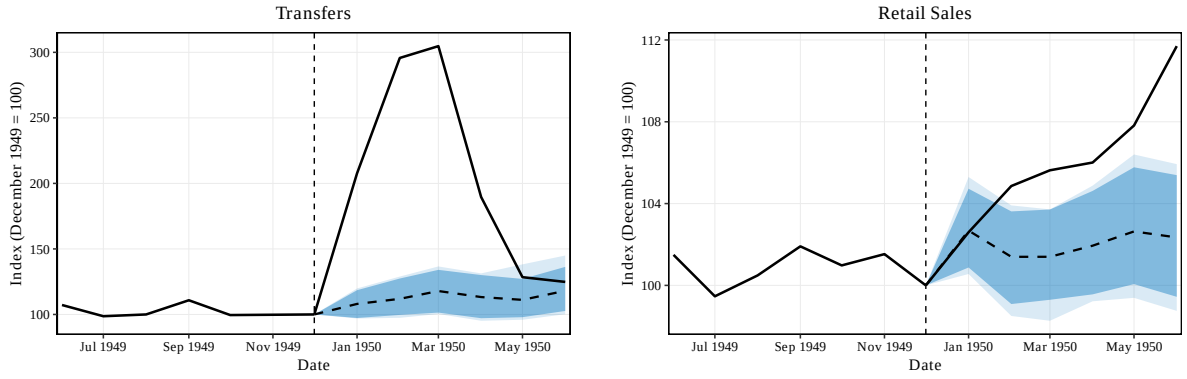
The motivating example is the largest payment in our sample, the January 1950 dividend. This case study is not meant to substitute for the more formal, regression-based evidence that follows. Rather, it serves to illustrate our empirical approach and to provide a transparent view of the behavior of key macroeconomic variables around a single, well-identified transfer event.

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 stimulus checks 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. The *Survey of Current Business* for August 1950 notes that "*the impact of the Korean developments was felt immediately on department-store sales*" (U.S. Department of Commerce, 1950c). To avoid conflating these effects with those of the transfer, we restrict attention to the

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 three-variable VAR estimated on pre-event data from 1945–1949, and the shaded areas represent the 90 and 95% confidence intervals constructed using a block boostrap with blocks of length 4-months.

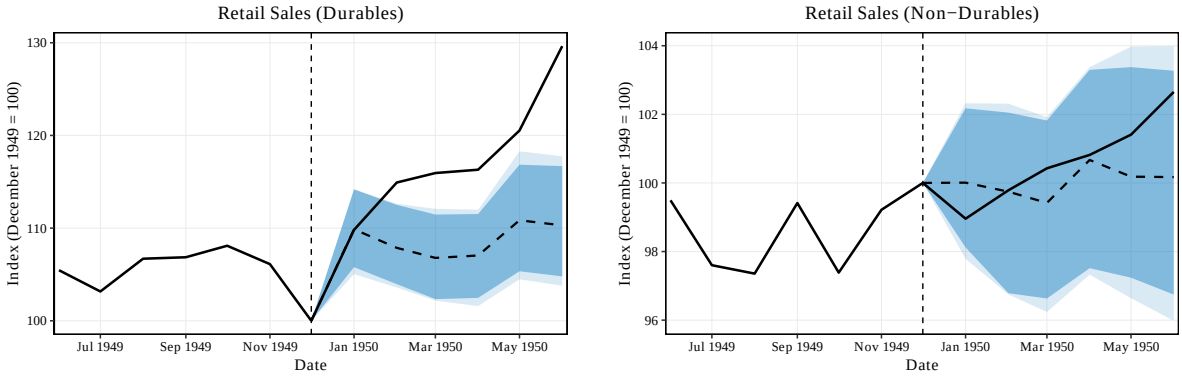
period prior to July 1950.[5]

To summarize the dynamics around the event, we compare the evolution of macroeconomic variables to a counterfactual path based on their pre-event behavior. Specifically, we construct forecasts using a VAR with 6 lags, estimated on data from 1945–1949. The VAR includes the outcome variable, industrial production, and the consumer price index, all expressed in logs. These forecasts provide an estimate of what trend the economy would have been on, absent the stimulus—for instance, capturing any dynamics associated with the mild recession that ended in October 1949. The forecasts also benchmark the size of typical fluctuations in the data, allowing us to assess whether post-payment movements are unusually large relative to recent historical variation. To achieve this benchmark, we report confidence bands around the forecasts, which tests the null hypothesis that movements after the transfer event were of typical size.

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. Moreover 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% confidence bands; suggesting the transfer shock

[5] In our formal regression analysis, we account for potentially confounding events such as the Korean War using a rich set of macroeconomic and financial controls.

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



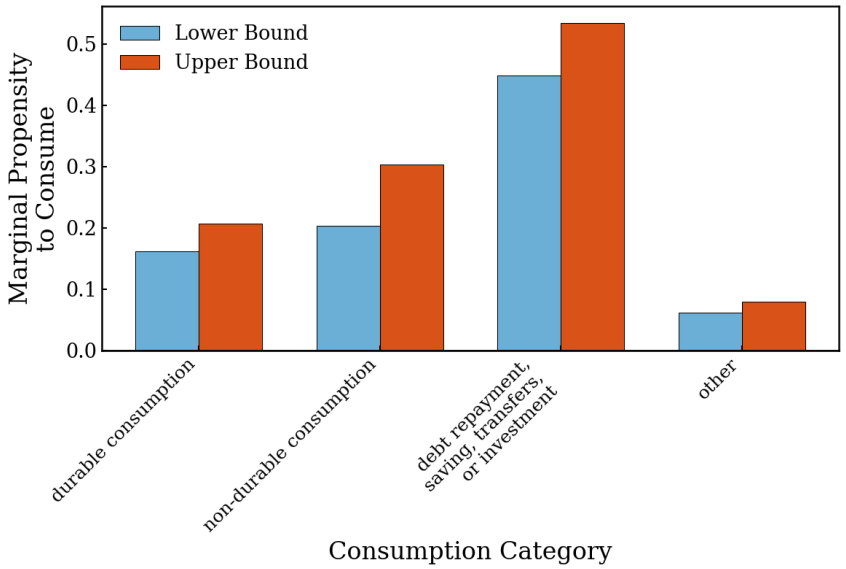
Notes: The figure shows the evolution of durable and non-durable retail sales in a six-month window around the January 1950 veterans' payment. The vertical dashed line indicates the timing of the payment. The dashed lines show forecasts from a three-variable VAR estimated on pre-event data from 1945–1949, and the shaded areas represent the 90 and 95% confidence intervals constructed using a block boostrap with blocks of length 4-months.

had a large effect on sales. That is, movements in retail sales after the event are more extreme than even the 95th percentile of typical movements in retail sales, as predicted by the VAR.

Figure 3 provides additional evidence on the composition of the spending response. Both durable and non-durable retail sales increase following the January 1950 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 emphasize that the dividend payments were a key driver of increased consumer demand, with no clear indication of other contemporaneous shocks affecting spending. It is also in line with prior evidence showing that temporary income shocks disproportionately raise expenditures on durable goods, such as automobiles and household furnishings (Parker et al., 2013). 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.

We also present corroborating micro-level evidence, suggesting a significant impact from the transfers. In particular, we make use of the one-off question, from the 1951 *Survey of Consumer Finances*, which asked veterans how they spent the 1950 dividend. The survey question recorded whether the spending unit received the payment, how much it received, and how the respondent said the money was used. The questions were: "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. As we

Figure 4: Marginal Propensity to Consume out of 1950 Dividend



Notes: Self-reported uses of the 1950 G.I. insurance dividend, from the 1951 Survey of Consumer Finances ($T = 290$ recipient spending units). The lower bound counts only recipients whose reported use(s) map exclusively to that broad category; the upper bound additionally includes recipients who report a use in that broad category alongside a use in a different broad category.

discuss in Appendix Section B.2, we can use this information to provide bounds on the marginal propensity to consume out of the transfer, for separate categories of consumption. The lower bound counts only recipients whose reported uses map exclusively to that broad category; the upper bound additionally includes recipients who report a use in that broad category alongside a use in a different broad category. Figure 4 presents the results. Veterans say that they spent 40-50% of the transfer, with a roughly even split between durable and non-durable consumption.[6]

This episode is striking in that it provides unusually transparent evidence on the effects of a transfer shock. In most settings, policy changes are difficult to isolate in aggregate time series, either because they are anticipated, gradual, or coincide with other macroeconomic disturbances. By contrast, the January 1950 payment constitutes a large and discrete shock whose timing is clearly identified and whose effects are directly visible in the data. At the same time, this evidence is necessarily limited to a single event, and we cannot fully rule out the presence of confounding factors. For this reason, the next section formally exploits the full set of plausibly exogenous postwar transfer payments to estimate the dynamic causal effects of transfer

[6]This even split contrasts with the evidence from Figure 3, that in the first 6 months veterans spent more on durables. The difference may be that the survey asks about spending over the whole of 1950, as opposed to the first 6 months. Our coming regressions suggest that durable spending is more front loaded than non-durables.

shocks.

3.2. Identification Strategy and Main Specification

Having motivated our empirical design in the case study, we now estimate the effects of transfer payments more systematically using the full set of postwar disbursement dates. As discussed in Section 2, we identify 24 plausibly exogenous transfer events, detailed in Appendix Table A.4.

Figure 1 displays the cyclical component of government transfers together with the timing of identified postwar veterans' payments and temporary Social Security benefit increases. The figure highlights that these episodes take the form of large, discrete spikes in transfers that reverse quickly. In this sense, they closely resemble modern stimulus checks: they are one-off, lump-sum payments that are largely deficit-financed. Many of these payments are quantitatively large, with several episodes reaching a non-trivial share of quarterly GDP, underscoring their macroeconomic significance. At the same time, unlike modern stimulus checks—which are enacted precisely in response to cyclical downturns—these postwar payments were not designed as countercyclical stabilization policy, making them well suited for identifying the causal effects of temporary transfers. Indeed, many of the identified events occur outside NBER recessions, consistent with our aim of isolating transfer changes that are not systematically driven by contemporaneous macroeconomic conditions.

Our identification strategy focuses on the timing of these events rather than their size. The timing of payments is determined by legislative and administrative processes and is often fixed in advance, making it a natural source of variation. By contrast, the size of payments is more difficult to measure consistently and may be partly endogenous. For example, the magnitude 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 of this approach).

Empirical framework and specification. We assume that the economy admits a structural vector moving-average representation:

$$\mathbf{y}_t = \sum_{l=0}^{\infty} \boldsymbol{\theta}_l \boldsymbol{\varepsilon}_{t-l}, \tag{1}$$

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 $z_{T,t}$ de-

note 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:

$$\mathbb{E}[z_{T,t}\varepsilon_{T,t}] = \zeta \neq 0 \tag{2}$$

$$\mathbb{E}[z_{T,t}\boldsymbol{\varepsilon}_{-T,t}] = \mathbf{0}, \tag{3}$$

$$\mathbb{E}[z_{T,t}\boldsymbol{\varepsilon}_{t+j}] = \mathbf{0}, \quad \text{for } j \neq 0. \tag{4}$$

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 (2)-(4), we can estimate its dynamic causal effects using local projections à la Jordà (2005). Specifically, for each horizon $h = 1, \dots H$, we estimate

$$y_{t+h} = \alpha_h + \theta_h z_{T,t} + \boldsymbol{\beta}_h' \mathbf{x}_{t-1} + \nu_{t+h}, \tag{5}$$

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, 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. A key advantage of local projections is their robustness to misspecification, which can help reduce bias and improve the reliability of uncertainty quantification (Olea et al., 2024, 2025). For inference, we follow the lag-augmentation procedure (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 government transfers, measured using the broad transfer series. Conditioning on this extensive information set, including controls for potential confounding factors, further strengthens the case that the narrative transfer shocks are orthogonal to other contemporaneous structural disturbances (Stock and Watson, 2018).

We focus on standard macroeconomic outcomes, including transfers, industrial production, employment, retail sales, personal consumption expenditures, and consumer prices, 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.

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.4). We extend the sample to include these shocks in alternative specifications.

3.3. Main Estimates: the Macroeconomic Effect of Postwar Payments

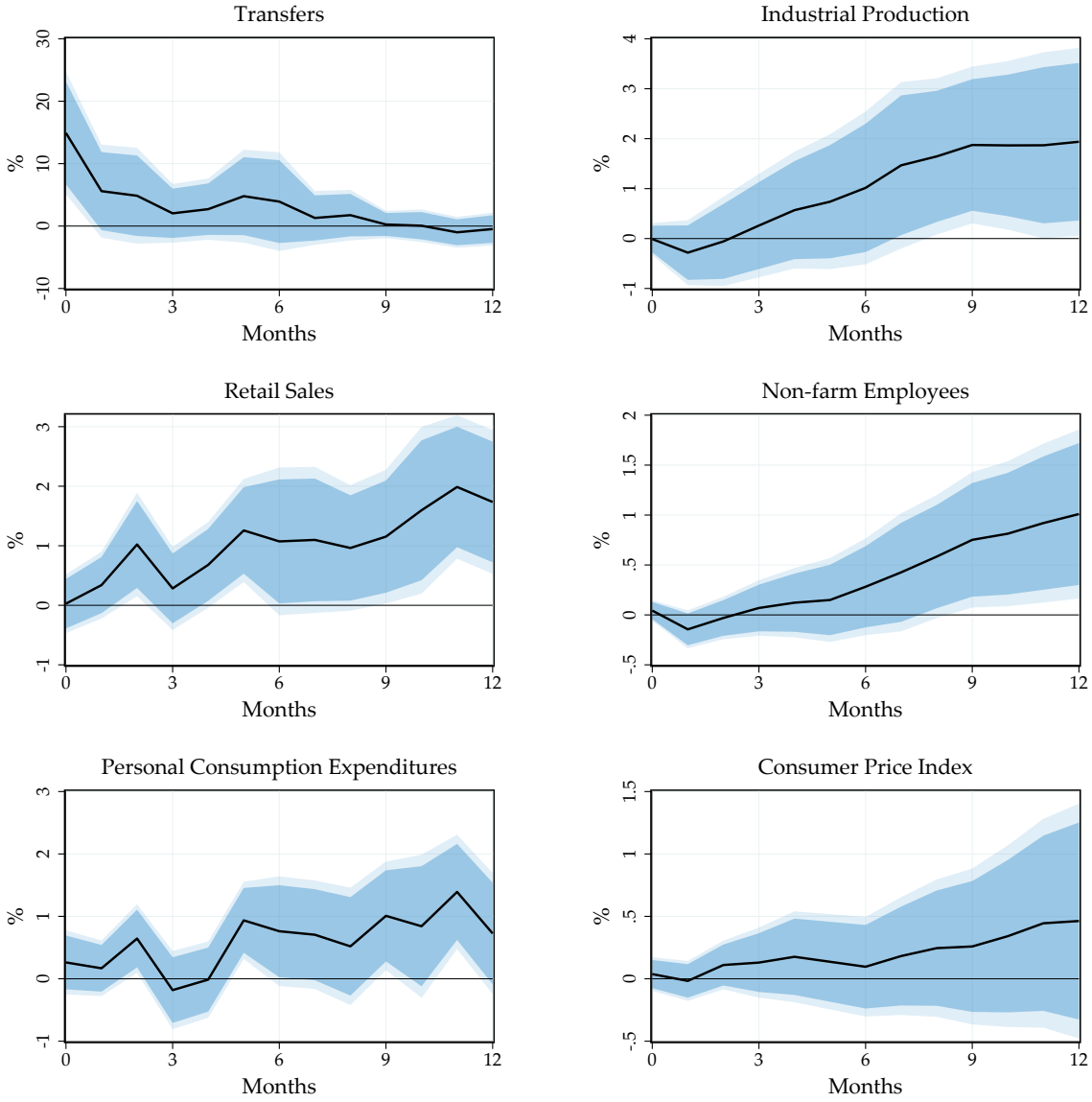
We now present the main results from our reduced-form specification. Using the local projections framework described above, we estimate the dynamic responses of key macroeconomic variables to an exogenous transfer innovation. In our baseline, we only exploit information about the timing of the postwar payments.

Figure 5 reports the impulse responses to a temporary increase in transfers. By construction, transfers rise sharply on impact and then unwind over subsequent months, confirming the transitory nature of the shock. The narrow transfer series increases by around 15% on impact. This increase is sufficiently large that it is also visible in the broad transfer series. Relative to personal consumption expenditures, this corresponds to an increase in transfers of close to 1 percentage point (see Appendix Figure B.2).

Real activity responds positively and persistently. A 15% increase in transfers increases industrial production by approximately 2%, while nonfarm employment expands by close to 1% over the subsequent year. Retail sales rise by roughly 2% at peak and personal consumption expenditures by more than 1%. These effects are not only economically meaningful but also statistically significant at the 95% level. In contrast, price responses are modest. Consumer prices rise by around 0.5%, but the effect is not statistically significant, suggesting limited short-run inflationary pressure. Overall, the results point to economically meaningful real effects of temporary transfer shocks, accompanied by muted price responses.

Figure 6a decomposes the consumption response into durable and non-durable expenditures. Durable spending responds strongly and immediately following a transfer shock, rising by close to 4% at peak. By contrast, non-durable spending increases more gradually and reaches a more modest peak of roughly 1%. The much stronger and more immediate response of durables is consistent with standard consumption theory: temporary, lump-sum income shocks tend to disproportionately affect purchases of durable goods, which are more easily postponed and exhibit a higher income elasticity. The results therefore suggest that transfer shocks primarily stimulate intertemporal reallocation toward big-ticket items, while still generating a meaningful, though more gradual, expansion in everyday consumption.

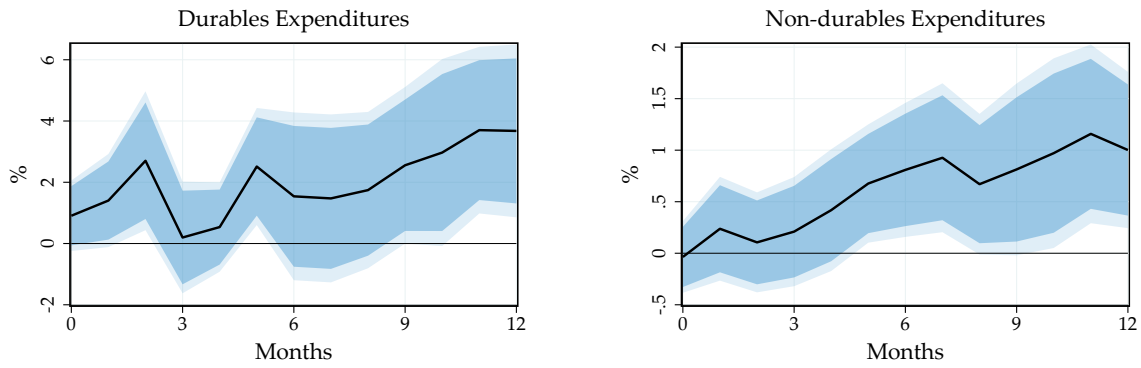
Figure 5: The Macroeconomic Effects of a Transfer Shock



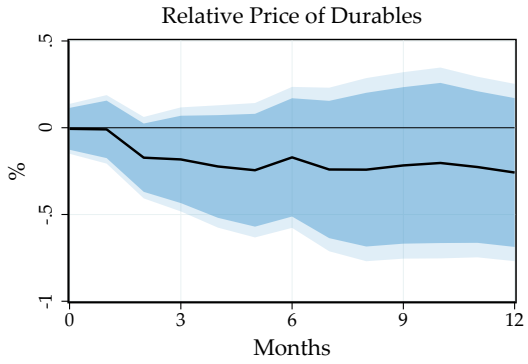
Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) using the plausibly exogenous transfer shock dates. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure 6: Durable and Non-Durable Consumption Responses

(a) Expenditure Responses

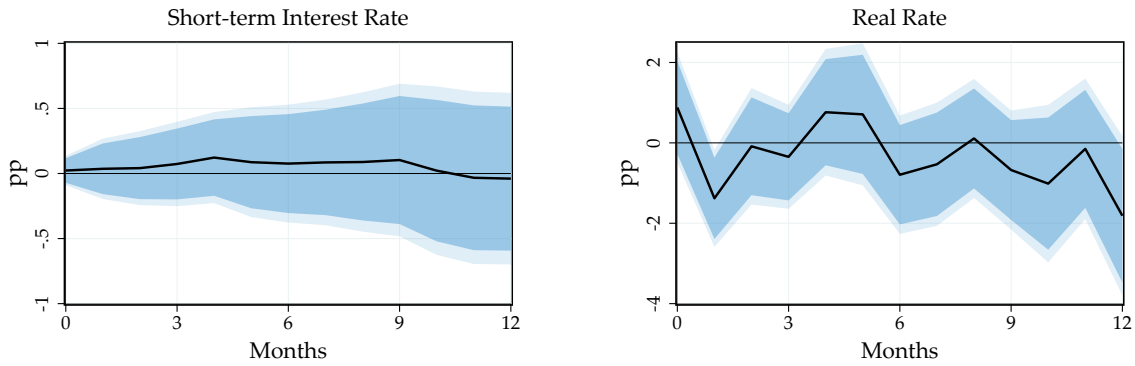


(b) Relative Price Response



Notes: Impulse responses of non-durables and durables to a temporary transfer shock, estimated using the local projections specification (5) using the plausibly exogenous transfer shock dates. Panel a shows the expenditure responses; Panel b shows the response of the relative price of durables. Sample: 1947m1–2019m12 for expenditure variables; 1945m10–2019m12 for relative price. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure 7: Monetary Policy Response



Notes: Impulse responses of nominal and real short-term interest rates to a temporary transfer shock, estimated using the local projections specification (5) using the plausibly exogenous transfer shock dates. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Does the increase in durable expenditure put upward pressure on prices? Figure 6b shows the response of the relative price of durables. The estimated response is small and statistically indistinguishable from zero at all horizons. If anything, the point estimates suggest a modest decline in the relative price of durables over the first year following the shock. Thus, although durable spending rises strongly in response to the transfer, there is no evidence that this increase is accompanied by a relative price increase.

This finding contrasts with recent evidence suggesting that transfer-induced demand expansions may put upward pressure on durable prices (Orchard, Ramey, and Wieland, 2025). In our historical setting, however, the absence of a relative price response indicates that the increase in durable consumption operates primarily through quantities rather than through price adjustment.

We next examine the response of monetary policy to transfer shocks. Figure 7 shows the impulse responses of the nominal short-term interest rate and the ex post real rate. We find little evidence of a systematic monetary policy reaction. The point estimates for both the nominal and real short-term rates are small and statistically indistinguishable from zero at all horizons. In particular, we do not observe a persistent increase in interest rates that would offset the expansionary effects of the transfer shock. These results suggest that monetary policy does not materially dampen the transmission of temporary transfers in our historical episodes.

A notable feature of the macroeconomic impacts is their persistence. This is surprising given that the underlying transfer shock is highly transitory. Indeed, Appendix Figure B.3 shows that the effects remain economically large even 24 months after the shock. While the estimates are mostly statistically insignificant at longer horizons, they decay only gradually.

One potential explanation for this persistence is sluggish adjustment of expectations. To

shed light on this mechanism, we study the response of expectations to transfer shocks. Appendix Figure B.4 reports the response of industrial production expectations from the Livingston Survey. We find that expectations adjust only very sluggishly. At short horizons, there is essentially no response for up to six months following the shock. Expectations begin to increase only gradually thereafter and remain imprecisely estimated. Notably, the adjustment in expectations lags behind the realized response of industrial production, which rises more promptly. This pattern suggests that agents do not immediately internalize the effects of transfer shocks on aggregate activity, consistent with sluggish expectation formation.[7]

Additional shocks. Our baseline specification excludes two transfer episodes that fall outside the main sample: a June 1936 payment and a March 2025 payment. We exclude the 1936 episode because it occurs during the Great Depression, a period characterized by large and unusual macroeconomic fluctuations that could complicate identification. We similarly exclude the 2025 episode, as our baseline sample ends prior to the COVID-19 period, which was marked by substantial and unusual variation in both consumption and transfers.

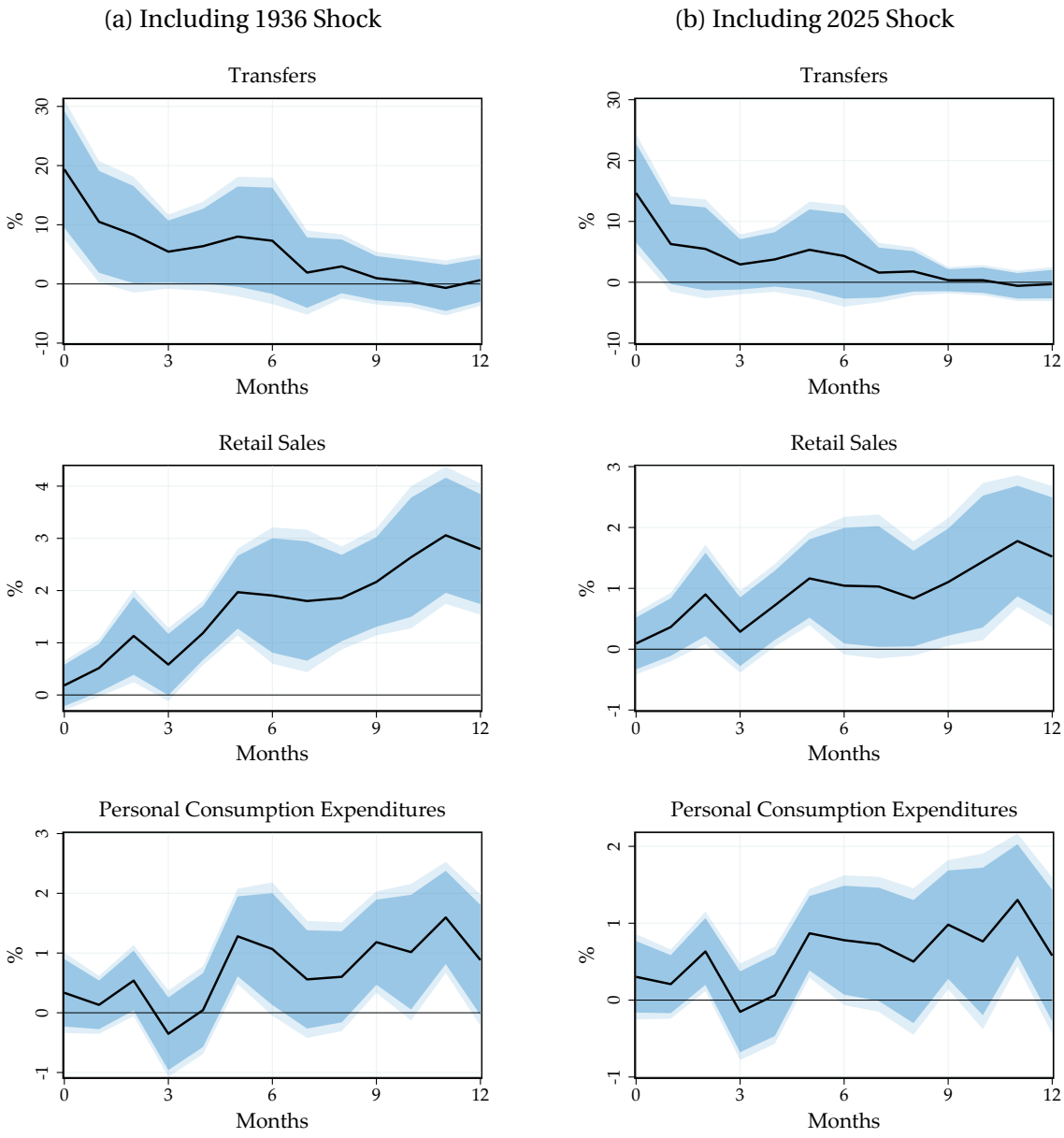
Figure 8 assesses the robustness of our results to including these additional shocks. The left panels report impulse responses from a specification estimated over 1935m1–2019m12 that incorporates the 1936 event, while the right panels report responses from a specification estimated over 1945m10–2025m12 that includes the March 2025 payment.

Across specifications, the estimated responses are broadly similar to the baseline results. In particular, transfers exhibit a sharp and temporary increase, while retail sales and personal consumption expenditures display a persistent rise following the shock. Including the 1936 episode does not materially alter the shape or magnitude of the responses; if anything, the retail sales response becomes more pronounced and precisely estimated, despite the very different macroeconomic environment in which this event occurs. Likewise, incorporating the 2025 payment leaves the qualitative dynamics unchanged, suggesting that our findings are not driven by the exclusion of recent transfer episodes.

These results indicate that our baseline estimates are robust to extending the sample to include additional transfer shocks, even when these occur in periods with atypical macroeconomic conditions, suggesting that our controls adequately account for variation in the broader economic environment.

[7] We caution that 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.

Figure 8: Responses under Alternative Sample Periods



Notes: Impulse responses of transfers, retail sales, and personal consumption expenditures to a temporary transfer shock, estimated using the local projections specification (5) using the plausibly exogenous transfer shock dates. Panel 8a uses a sample spanning 1935m1–2019m12 and includes the June 1936 bonus payment. Panel 8b uses a sample spanning 1945m10–2025m12 and includes the March 2025 payment. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

3.4. 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 outcome variables are reported in Appendix B.4.

First, we directly visualize the identifying variation underlying our estimates. Figure 9a presents scatter plots of transfers and personal consumption at impact and at a 6-month horizon, after partialling out our controls. For non-shock dates, we report the mean and corresponding 95% confidence bands. The positive relationship between transfer events and subsequent consumption growth is evident, and the fitted line is not driven by a single influential observation. In particular, no individual episode appears to account disproportionately for the estimated multiplier.

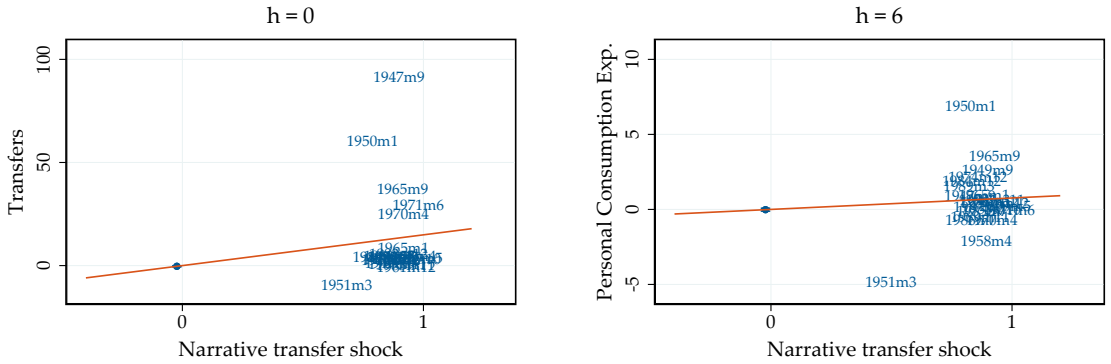
Second, we examine robustness with respect to shock selection. Panel b shows that our results remain largely unchanged when we exclude the early episodes in the immediate aftermath of World War II, drop less salient shocks with limited historical coverage, or restrict attention to larger events by excluding 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.6 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 assess the role of veterans versus other transfer payments by restricting the shock series to the nine veterans' payment dates only. Panel c shows that the resulting impulse responses are very similar to our baseline estimates in terms of both magnitude and persistence. However, the responses are estimated less precisely, reflecting the smaller number of events. A similar pattern emerges when restricting attention to the Romer and Romer (2016) shocks alone: the estimated responses are less precise and somewhat attenuated (see Appendix Figure B.8). Taken together, these results suggest that combining veterans' payments with other transfer episodes improves statistical precision without materially affecting the estimated dynamics.

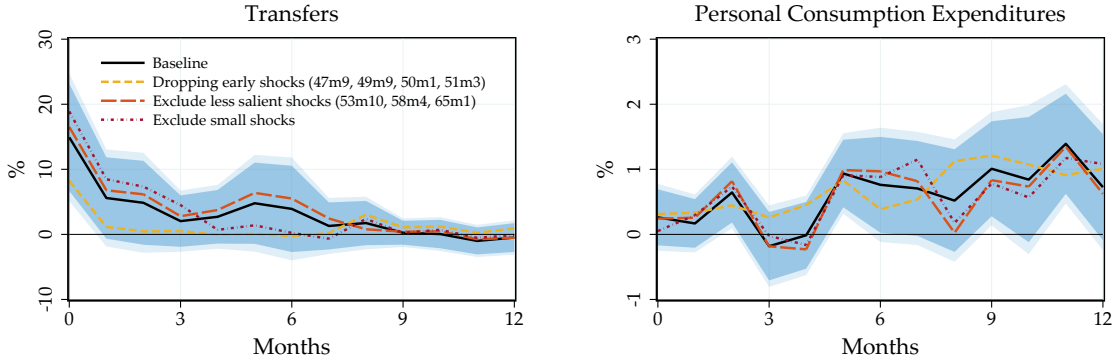
Figure 10 reports additional robustness exercises. Panel a shows that the results are largely unchanged when we control for permanent transfer shocks identified by Romer and Romer (2016), as well as for other prominent macroeconomic disturbances, including military spending shocks from Ramey (2011), monetary policy shocks from Romer and Romer (2004), and oil

Figure 9: Sensitivity Analyses I

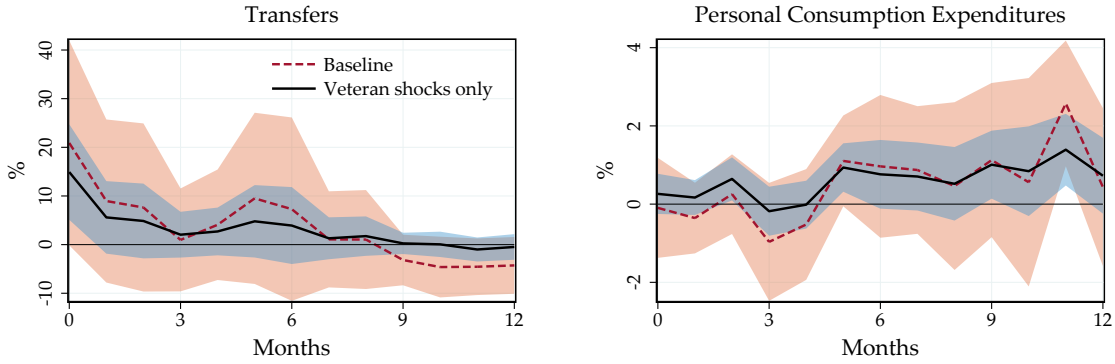
(a) Scatter Plots



(b) Robustness with Respect to Shock Selection



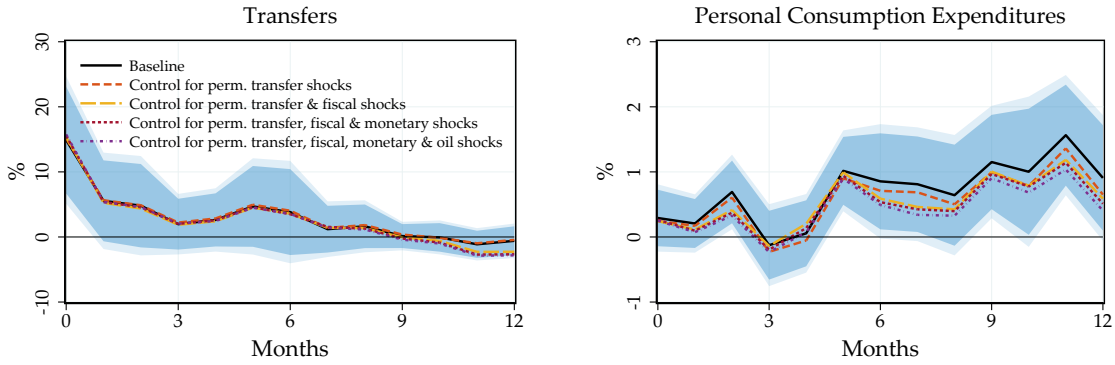
(c) Veterans' Payments vs. Full Shock Series



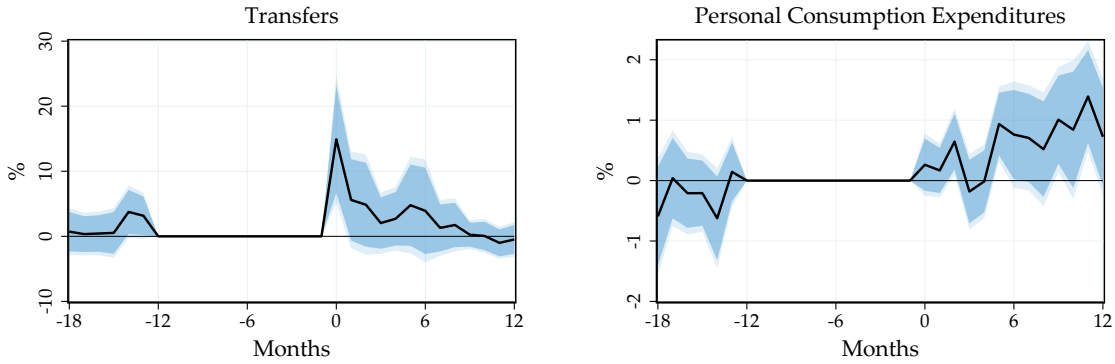
Notes: Sensitivity of transfer and personal consumption expenditure response to a temporary transfer shock, estimated using the local projections specification (5). Panel a displays scatter plots of transfers and personal consumption expenditures at impact ($h = 0$) and at a 6-month horizon ($h = 6$). Panel b shows robustness to alternative shock selections, including dropping early episodes, excluding less salient shocks, and restricting attention to larger events. Panel c reports robustness to restricting the shock series to veterans' payments only, excluding the Social Security episodes. Sample: 1945m10–2025m12. Lines: point estimates. Panel b reports 90% and 95% confidence bands for baseline responses, while Panel c reports 95% confidence bands.

Figure 10: Sensitivity Analyses II

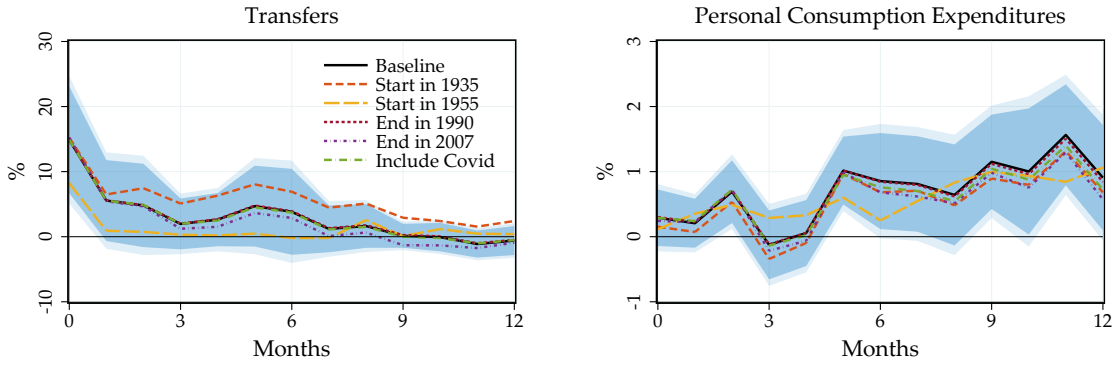
(a) Robustness with Respect to Controls



(b) Pre-trends



(c) Sample



Notes: Sensitivity of transfer and personal consumption expenditure response to a temporary transfer shock, estimated using the local projections specification (5). Panel a adds controls for permanent transfer shocks, military spending shocks, monetary policy shocks, and oil price shocks. Panel b extends to lags of the outcome variable to test for pre-trends. Panel c 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.

price shocks (Känzig, 2021).[8] The consumption response remains quantitatively similar across specifications, suggesting that our transfer shocks are not proxying for broader fiscal, monetary, or energy-related innovations.

Panel b examines pre-trends by extending the local projection to lags of the outcome variable. By construction, the first 12 months prior to the shock are zero due to the inclusion of lagged controls. Beyond this window, we find no evidence of anticipatory dynamics: coefficients prior to the shock are small and statistically indistinguishable from zero for both transfers and consumption. This supports the identifying assumption that payment timing is orthogonal to contemporaneous macroeconomic conditions.

Finally, Panel c shows that the results are robust to alternative sample periods. Starting the sample in 1935 or 1955, or truncating it before 1990 or 2008, yields consumption responses that are very similar in magnitude and persistence. Similarly, extending the sample to include the Covid period produces similar results.

Overall, these exercises indicate that our baseline findings are not driven by outliers, particular shock definitions, omitted macroeconomic disturbances, anticipatory behavior, or specific sample choices. The results remain stable across alternative shock selections, additional controls for other macroeconomic disturbances, and different sample periods. We find no evidence of anticipatory dynamics or pre-existing trends. The consistency of the consumption response across these specifications strengthens the interpretation that the estimated response reflects a genuine causal effect of temporary lump-sum transfers rather than coincidental comovement or omitted shocks.

3.5. The Transfer Multiplier

To quantify the aggregate impact of temporary transfer shocks, we estimate the transfer multiplier. Rather than focusing on percentage impulse responses, we compute the cumulative dollar response of spending relative to the cumulative dollar increase in transfers. Specifically, for each horizon h, we estimate the cumulative change in the outcome variable—either personal consumption expenditures or retail sales—relative to its lagged level, and regress it on the cumulative change in government transfers over the same horizon, also expressed relative to its lagged level. Formally, we estimate:

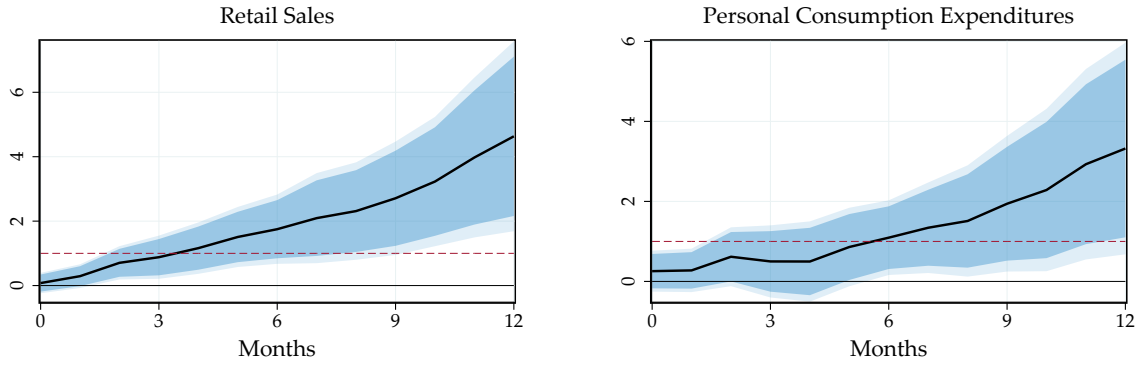
$$\sum_{j=0}^{h} \frac{\Delta Y_{t+j}}{\tilde{Y}_{t-1}} = \beta_h \sum_{j=0}^{h} \frac{\Delta T_{t+j}}{\tilde{Y}_{t-1}} + \Gamma_h' X_t + \varepsilon_{t+h}, \tag{6}$$

[8] 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.

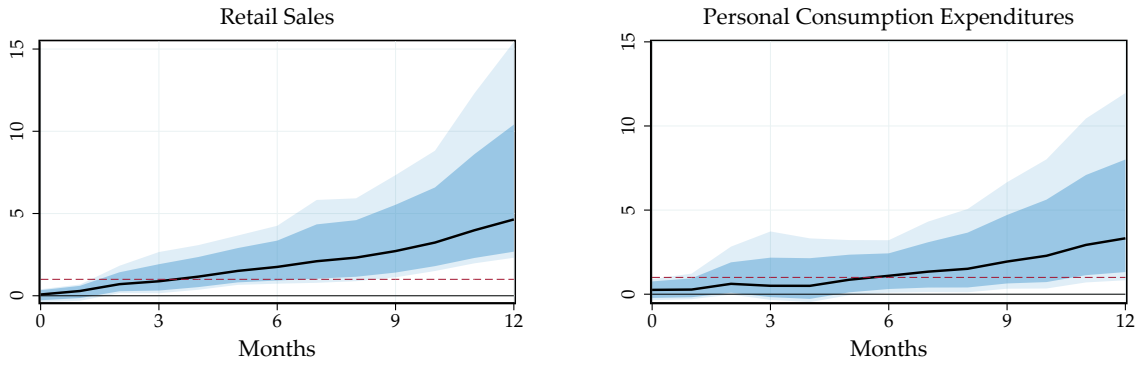
where Y denotes the outcome variable in dollars and T denotes government transfers. Both variables are measured in levels and expressed relative to their pre-shock values, so that β_h directly captures the cumulative dollar response of spending per dollar of transfers. To account for trending behavior, we normalize both variables by $\tilde{Y}_{t-1}$, 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 the outcome variable and transfers (relative to $\tilde{Y}$), CPI inflation, and the transfer shock. This approach delivers a horizon-specific multiplier that naturally incorporates dynamic effects.

Figure 11: Retail Sales and Consumption Multipliers

(a) Standard Inference



(b) Weak-instrument Robust Inference



Notes: Cumulative transfer multipliers of retail sales and personal consumption expenditures, estimated based on (6). Panel a provides bands under standard inference. Panel b provides weak-instrument robust Anderson-Rubin confidence bands. Sample: 1945m10–2019m12. Lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure 11a reports the estimated multipliers for retail sales and personal consumption expenditures. The multiplier rises steadily over time. For retail sales, the cumulative response reaches approximately 1.5 within six months and continues to increase thereafter. For personal consumption expenditures, the multiplier follows a similar pattern, reaching roughly 1 after six

months and exceeding 2 after twelve months. The confidence bands indicate that the medium-run responses are statistically significant at conventional levels.

Our narrative transfer shock series proves to be a strong instrument, at least at short horizons. The corresponding first-stage F-statistics on impact are 11 for retail sales and 10.8 for personal consumption expenditures. However, instrument relevance declines over time, with F-statistics falling to around 6 after six months. To address potential concerns about weak instruments at longer horizons, we also report weak-instrument robust inference in Figure 11b. Specifically, we construct Anderson–Rubin confidence bands, which are robust to weak identification. The lower confidence bands are very similar to those obtained under standard inference, while the upper bands are somewhat wider. Nevertheless, the estimated multipliers remain statistically different from zero, particularly at longer horizons, indicating that our results are not affected by weak-instrument concerns.

We also assess the robustness of the multiplier estimates using two complementary exercises. First, we augment the baseline specification with additional controls for macroeconomic shocks, including fiscal shocks, monetary policy shocks, and oil price shocks. Figure 12a shows that the resulting multiplier paths are very similar to the baseline estimates. Both the magnitude and the dynamic profile of the multiplier remain largely unchanged, indicating that our results are not driven by omitted macroeconomic disturbances.

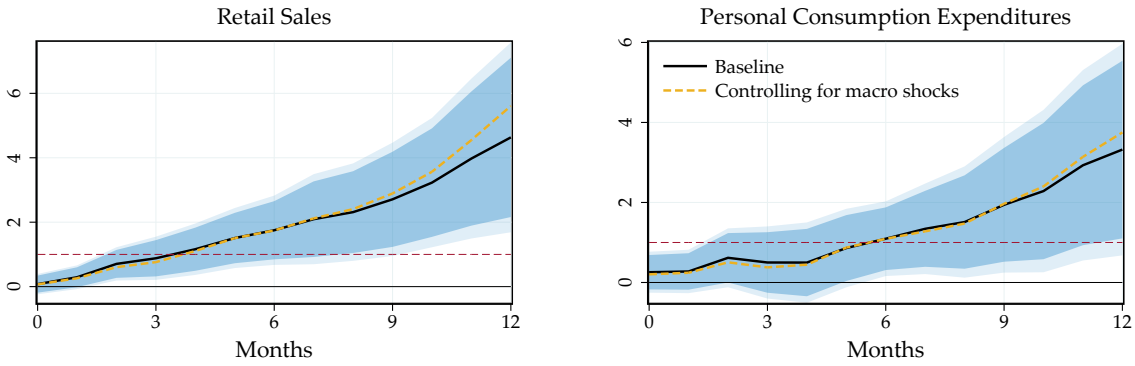
Second, we perform a leave-one-out exercise. Specifically, we re-estimate the cumulative multiplier repeatedly, each time excluding one transfer shock episode from the shock series. Figure 12b reports the resulting multiplier paths. The estimates remain remarkably stable across these specifications. While omitting individual episodes mechanically affects precision, the overall magnitude and upward trajectory of the multiplier are largely unchanged. 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 retail sales and personal consumption expenditures builds gradually and remains elevated for an extended period. The fact that the multiplier exceeds one at longer horizons reflects not only a strong contemporaneous spending response, but also sustained increases in expenditure well beyond the initial rise in transfers. In other words, a temporary boost to transfers translates into a prolonged expansion in aggregate spending.

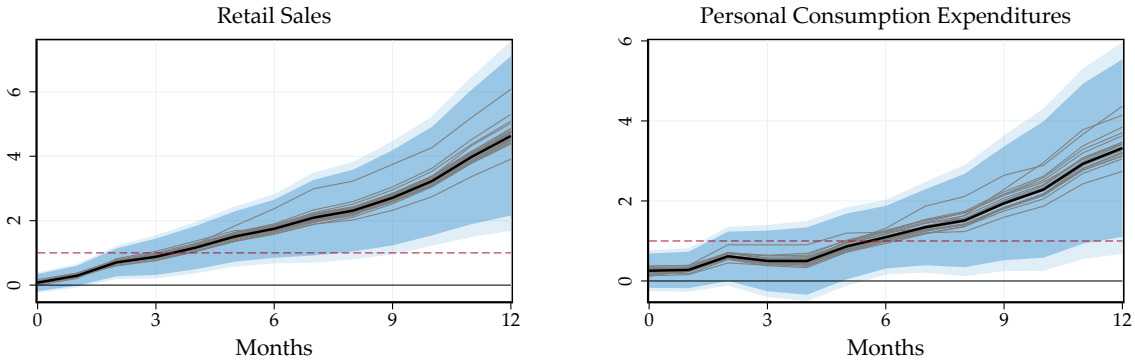
Taken together, the estimates point to a substantial aggregate demand impact of temporary transfers in the postwar United States. The magnitude and persistence of the multiplier provide

Figure 12: Sensitivity of Multipliers

(a) Robustness with Respect to Controls



(b) Leave-one-out Exercise



Notes: Sensitivity of cumulative transfer multipliers. Panel a reports robustness to additional controls for macroeconomic shocks, including fiscal shocks, monetary policy shocks, and oil price shocks. Black line and blue shaded areas: baseline estimate and 90% and 95% confidence bands. Other lines: responses under alternative specifications. Panel b reports a leave-one-out exercise, in which the multiplier is re-estimated excluding one transfer episode at a time. Gray lines show leave-one-out estimates excluding one episode at a time. Black line and blue shaded areas: baseline estimate and 90% and 95% confidence bands. Gray lines show alternative estimates. Sample: 1945m10–2019m12.

a useful empirical benchmark for quantitative macroeconomic models.

We now develop a structural model to answer two questions. First: what is the relationship between our estimates—the response of the economy to temporary payments to veterans—and the more familiar stimulus checks multiplier? Second, we ask: what features of a model are required to match our estimates? In particular, we document a delayed consumption response to a temporary transfer shock. The next section asks what models can rationalize this pattern.

4. Model

We develop a heterogeneous-agent New Keynesian (HANK) model that incorporates the institutional features of the postwar veterans' payments and nests standard stimulus checks as a special case. The model serves two purposes. First, it establishes conditions under which veterans' payments and stimulus checks have identical aggregate effects, thereby clarifying how the postwar multiplier can discipline the effect of modern transfer programs. Second, it provides a framework for learning what ingredients are required of a model, in order to produce a realistic response of consumption to transfer shocks.

Time is discrete and infinite, $t = 0, 1, 2, \ldots$. Initially, the economy is in steady state and we compute its impulse response to an unanticipated increase in transfers. The economy is populated by a unit mass of households indexed by i. A perfectly competitive final-goods producer operates a linear technology $Y_t = N_t$, where Y_t is output and N_t is aggregate labor in efficiency units. We assume sticky nominal wages à la Erceg, Henderson, and Levin (2000) and that monetary policy directly controls the real interest rate r, which we hold constant throughout the analysis.[9]

Households are divided into two groups: veterans ($g = v$), who constitute a share ν of the population, and non-veterans ($g = nv$), who make up a $1 - \nu$ share of the population. Each household i draws idiosyncratic productivity $e_{i,t}$ from a finite-state Markov process with transition matrix Π and ergodic distribution π.

Firms. A continuum of identical firms with flexible prices compete in the final goods market. They hire labor and produce according to $Y_t = N_t$. It follows that, in equilibrium, the price level is equal to the nominal wage, $P_t = W_t$. So, price inflation equals wage inflation $\pi_t = \pi_t^w$.

Households. Households have preferences over consumption, $c_{i,t}$, and labor supply, $n_{i,t}$, according to

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

where $E_{i,0}$ is the household's subjective expectations operator, β_g is the discount factor, and u and v are standard increasing and concave/convex period utilities over consumption and labor, respectively. Under the union-based wage-setting protocol, all households supply identical hours $n_{i,t} = N_t$.

After-tax labor income follows a progressive retention function à la Heathcote, Storesletten,

[9] This constant-real-interest-rate assumption can be viewed as a Taylor rule with a coefficient of one on expected inflation; see Auclert, Rognlie, and Straub (2024) for a detailed discussion.

and Violante (2017):

$$y_{i,t} = (1-\tau_t)(w_t e_{i,t} N_t)^{1-p},$$

where τ_t is a time-varying tax-rate intercept and p governs the degree of progressivity. The flow budget constraint of a veteran household is given by

$$c_{i,t} + a_{i,t} = y_{i,t} + (1+r)\, a_{i,t-1} + (T-P) + \frac{1}{\nu} T_t^v + \omega_i T_t^f, \tag{7}$$

where $a_{i,t}$ denotes financial assets at the end of period t. The variable P denotes the steady-state payments, and T the steady-state level of transfer outlays from the veteran fund. The variables T_t^v and T_t^f denote, respectively, transfer shocks from the veteran's fund and the government. We assume that non-veterans do not receive transfers from the veteran fund, $T_{nv,t}^v = 0$ for all t. Furthermore, we let $\omega_i \geq 0$ denote the targeting coefficient, i.e, what proportion of the transfer go to agent i. These targeting coefficients are parameterized so that $\int \omega_i di = 1$. The flow budget constraint of a non-veteran household is given by

$$c_{i,t} + a_{i,t} = y_{i,t} + (1+r)\, a_{i,t-1} + \omega_i T_t^f, \tag{8}$$

Households are also subject to a group-specific borrowing constraint

$$a_{i,t} \geq -\underline{a}_g.$$

Expectations. We assume that households possess information about the variables that enter their contemporaneous budget constraint, but do not have perfect expectations of the future evolution of the aggregate variables. To isolate the impact of expectations about aggregate conditions, we maintain the assumption that households correctly forecast the evolution of their idiosyncratic states.

As in Bardóczy and Guerreiro (2023), we assume that households form beliefs according to a structural model that combines noisy information with long-memory diagnostic expectations, building on Bordalo et al. (2020) and Bianchi, Ilut, and Saijo (2024). The crucial feature of this model is that it allows us to capture the initial under-reaction and delayed over-reaction pattern of survey data on expectations emphasized in Angeletos, Huo, and Sastry (2021).

In the appendix, we show that the average expectation of the change in some aggregate variable X_{t+h} is given by

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

where $\mathbb{E}_t[\cdot]$ denotes the true model-consistent expectations conditional on information set at time t, τ denotes the relative precision of the signals, θ denotes the degree of over-extrapolation in expectations, and $\alpha_j \geq 0$ control the memory length and satisfy that $\sum_{j=1}^{\infty} \alpha_j = 1$. If $\tau \to \infty$ and $\theta = 0$, the model converges to full-information and rational expectations. When $\theta = 0$, this model nests the standard noisy-information rational expectations framework (e.g., Lucas Jr, 1972, Woodford, 2003, and Angeletos and Huo, 2021). When $\theta > 0$ and $\tau \to \infty$, the model nests the standard full-information diagnostic expectations models of Bordalo, Gennaioli, and Shleifer (2018), with $\alpha_1 = 1$, and the long-memory version of Bianchi, Ilut, and Saijo (2024), with $\alpha_j \leq 1$.

Wages and Labor Supply. We assume that wages are labor supply are decided by monopolistically competitive unions (Erceg, Henderson, and Levin, 2000; Schmitt-Grohé and Uribe, 2005). As in Auclert, Rognlie, and Straub (2024), we assume that unions require every worker to supply the same number of hours $n_{i,t} = N_t$. In appendix XX, we show that the union problem generates the NK 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 and σ denote, respectively, the inverse Frisch and inverse EIS elasticities and $Z_t \equiv (1-\tau_t)(w_t N_t)^{1-\gamma}$.

Veterans' life insurance fund. During the war, service members purchased government-provided life insurance, paying regular premia P into a trust fund that holds special-issue Treasury securities F_t. The fund sends predictable payments T^p to veterans and, on occasion, disburses a lump-sum surplus T_t^v. Its budget constraint is

$$F_t = (1+r)F_{t-1} + \nu(P - T) - T_t^v + \Xi_t, \tag{10}$$

where Ξ_t denotes direct injections from the Treasury.

Government. The government issues marketable public debt B_t and supports the special-issue securities F_t held by the fund. Total tax revenue is

$$B_t + F_t + \mathcal{T}_t = (1+r_t)(B_{t-1} + F_{t-1}) + T_t^f + \Xi_t. \tag{11}$$

where $\mathcal{T}_t = Y_t - \int y_{i,t} di$ denotes total income-tax revenue. We assume that the government raises taxes to ensure that in the long run government debt converges to B,

$$\mathcal{T}_t = \mathcal{T} + \tau_d (B_{t-1} - B), \tag{12}$$

where $\mathcal{T}$ denotes the level of taxes in the steady-state allocation and $\tau_d > 0$ parameterizes the speed of fiscal adjustment.

Market clearing. 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 this economy using the standard Sequence-Space Jacobian methods of Auclert et al. (2021) and Auclert, Rognlie, and Straub (2024), appropriately extended to deviations from full-information and rational expectations (Auclert, Rognlie, and Straub, 2020). The details of this solution method are relegated to the appendix. We characterize the response of equilibrium aggregate output $d\mathbf{Y} \equiv [dY_0, dY_1, ...]$ as

$$d\mathbf{Y} = M^Y (d\mathbf{Y} - d\mathcal{T}) + M^f d\mathbf{T}^f + M^v d\mathbf{T}^v, \tag{13}$$

where M^Y, M^f, and M^v denote the sequence-space Jacobians of aggregate demand to changes in output, fiscal transfers, and veteran payments, respectively.

4.1. Veteran Payments and Fiscal Transfers: Equivalence Results

An important implication of our analysis is that, by guaranteeing the solvency of the veteran's fund, the government effectively operates under an integrated government budget constraint. To see this, let us combine equations (10) and (11) to obtain the following integrated budget constraint:

$$B_t + \mathcal{T}_t + \nu(P - T^p) = (1 + r)\, B_{t-1} + T_t^f + T_t^v. \tag{14}$$

We use this result to establish a key implication of the consolidated budget constraint: in this economy, the path of debt and taxes depends only on the total size of transfers, inclusive of the veteran's payments, see Lemma 1.

Lemma 1 (Taxes Depend only on Consolidated Payouts)**.** *Fix the initial level of government debt B_{-1}. Then the sequences $\{B_t\}_{t\geq 0}$ and $\{\mathcal{T}_t\}_{t\geq 0}$ implied by (14) and the tax rule (12) depend on transfers only through the total transfer outlays inclusive of veteran payments $\{T_t^f + T_t^v\}_{t\geq 0}$.*

Once the veterans' fund is consolidated with the Treasury, the internal positions F_t and Ξ_t net out of the government budget constraint. What remains is only the total net payout that

must be financed, namely $T_t^f + T_t^v$. For this reason, government debt evolves solely with the sum of these two objects. Since taxes respond to debt through the fiscal rule, they inherit the same property: the government cares about the overall cash outflow, not whether it is labeled a regular fiscal transfer T_t^f or a surplus distribution T_t^v.

We now use this model to establish two key results regarding the mapping of veteran payments to fiscal transfer experiments. First, in Proposition 1, we show that the veteran payments are equivalent to a fiscal transfer that targets only veterans.

Proposition 1 (Veteran Payments are Equivalent to a Transfer Targeting Veterans)**.** *Consider two one-time policies with the same path of total expenditure in transfers:*

1. *a veteran payment of size* $\{T_t\}_{t\geq 0}$*;*
2. *a fiscal transfer of size* $\{T_t\}_{t\geq 0}$ *which only targets veterans, i.e., a fiscal transfer with targeting coefficients* $\omega_i = \frac{1\{i\in v\}}{v}$*.*

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

This result is not surprising and the logic works directly through the consolidated government budget constraint. As we discussed in Section 2, when the life insurance fund makes a payment to veterans, it redeems its special-issue Treasury securities for cash. The Treasury finances this redemption by issuing marketable debt. From the standpoint of the consolidated government, the transaction amounts to a one-time, debt-financed cash transfer to a subset of households—precisely the defining feature of a stimulus check. Under the same policy rules, the two instruments are therefore equivalent.

This result formalizes the observation that a veteran payment is a special case of a fiscal transfer: it is a debt-financed cash payment that happens to target veterans. Any difference in macroeconomic effects between veterans' payments and a general stimulus program must therefore arise from differences in the composition of recipients. In Proposition 2, we consider the differences between the veteran payments and a general fiscal transfer policy.

Proposition 2 (Differences Across Targeting Rules)**.** *Fix a total transfer* $\{T_t^v\}_{t\geq 0} = \{T_t^f\}_{t\geq 0} = \{T_t\}_{t\geq 0}$ *path. The equilibrium output paths under the veterans' payment* ($d\mathbf{Y}_v$) *and under a fiscal transfer with arbitrary targeting rule* ω ($d\mathbf{Y}_\omega$) *satisfy*

$$d\mathbf{Y}_\omega = d\mathbf{Y}_v + \mathscr{M}\left(\mathbf{M}^f - \mathbf{M}^v\right) d\boldsymbol{T}, \tag{15}$$

where $\mathscr{M}$ *is the general-equilibrium multiplier operator, assumed to exist, and is such that* $\mathscr{M}(\mathbf{I} - \mathbf{M}) = I$*.*

Equation (15) has a transparent economic interpretation. The difference in aggregate effects between two targeting rules is the product of three objects: (i) the general-equilibrium multiplier $\mathscr{M}$, which captures the feedback from output to income to consumption; (ii) the gap in average iMPCs between the fiscal-transfer recipients and veterans, $\mathbf{M}^f - \mathbf{M}^v$, which measures the partial-equilibrium difference in spending propensities to spend; and (iii) the size of the transfer $d\mathbf{T}$.

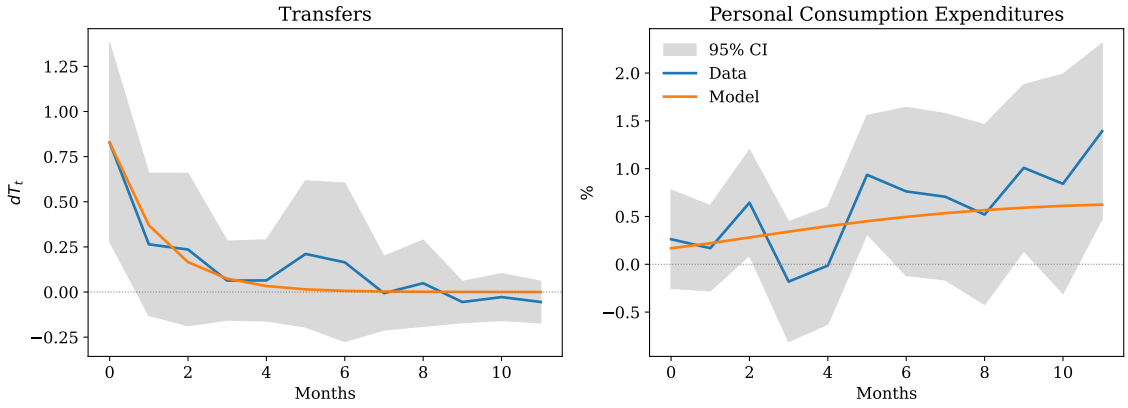
Corollary 1 (Equivalence of Veterans Payments and Stimulus Checks)**.** *Suppose furthermore that: (i) there is no preference differences $\beta_g = \beta$ for all g, and (ii) suppose that the borrowing limit is set in terms of total financial position, i.e., $\underline{a}_v = \underline{a}_{nv} + \frac{T-P}{r}$. Consider an untargeted fiscal transfer of the same magnitude as the veteran payments size, i.e., $\{T_t^{Tf}\}_{t\geq 0} = \{T_t^{Tv}\}_{t\geq 0}$ and $\omega_i = 1$ for all i. Under these conditions, the iMPCs are the same between the targeted population and the veterans $\mathbf{M}^f = \mathbf{M}^v$. So, the induced path of output is equal under the fiscal transfer or the veteran payments: $d\mathbf{Y}_\omega = d\mathbf{Y}_v$.*

Under the conditions of the corollary, the distinction between veterans and the rest of the population is behaviorally irrelevant: they have the same preferences and, once the borrowing limit is written in terms of total financial position, the same relevant wealth distribution. It follows that they solve the same optimization problem and therefore have identical iMPCs. The result $d\mathbf{Y}_\omega = d\mathbf{Y}_v$ is then an immediate implication of Proposition 2.

4.1.1. Quantitative Results

We now examine whether the model can account for the hump-shaped response of consumption observed in the data. At this stage, our calibration should be viewed as preliminary.

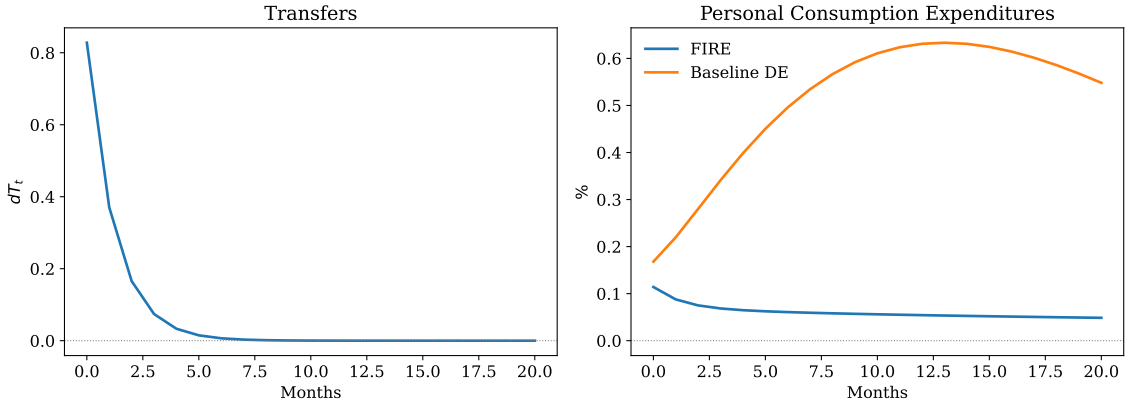
Figure 13: Impulse Responses to Transfer Shock



Notes: Impulse responses to a temporary transfer shock.

Figure 13 shows the impulse responses of transfers and personal consumption expenditures in the model relative to the data. We see that the model can provide a good account of our empirical findings.

Figure 14: Impulse Responses to Transfer Shock

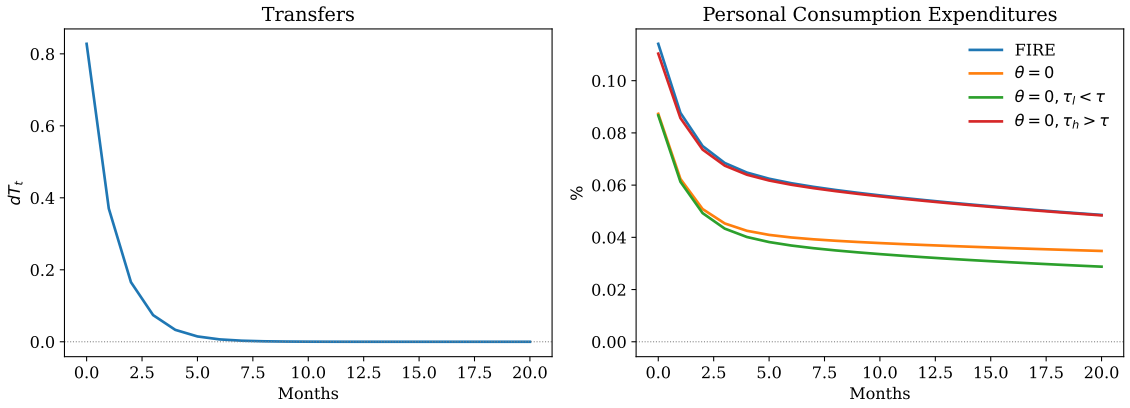


Notes: Impulse responses to a temporary transfer shock.

Figure 14 compares the implied impulse response of personal consumption expenditures in the HANK model under our baseline specification with noisy-information diagnostic expectations with those under full-information rational expectations (FIRE). Under FIRE, consumption expenditures peak on impact, at date 0, and then gradually return to steady state. This pattern reflects a familiar limitation of HANK models under FIRE: they do not generate a hump-shaped response of consumption expenditures. By contrast, the model with noisy-information diagnostic expectations succeeds in producing the hump-shaped profile required to match the empirical evidence. This result is consistent with the findings in Bardóczy and Guerreiro (2023): the combination of initial underreaction and subsequent overreaction in expectations, as documented in survey data, can generate a hump-shaped response of consumption expenditures. Intuitively, the model implies that expectations are initially sluggish, so households do not immediately anticipate a substantial expansion in aggregate activity. Over time, however, the diagnostic component of expectations makes agents increasingly optimistic, thereby contributing to a gradual buildup in spending and, in turn, to the hump-shaped response.

A noteworthy implication is that slow updating of expectations alone is not sufficient to generate a hump-shaped consumption profile in this exercise. Rather, the delayed overreaction implied by diagnostic expectations plays a central role in producing the gradual increase in consumption expenditures. Figure 15 illustrates this point. There, we shut down diagnosticity by setting $\theta = 0$, so that the expectations block collapses to a noisy-information rational expec-

Figure 15: Impulse Responses to Transfer Shock with Noisy Information Rational Expectations



Notes: Impulse responses to a temporary transfer shock.

tations model.[10] We evaluate the model under three alternative signal structures: the baseline degree of noisy information, a specification with more informative signals $\tau_H = \tau * 200$, and a specification with less informative signals, $\tau_L = \tau/200$. In all three cases, the model fails to generate the hump-shaped expenditure profile required by the data. We therefore conclude that the joint presence of the two features embedded in our baseline specification—initial underreaction and delayed overreaction in expectations, as documented in Angeletos, Huo, and Sastry (2021)—is essential for reproducing the empirical consumption dynamics.

5. Conclusion

What is the macroeconomic effect of stimulus checks? Using a historical natural experiment, this paper provides direct aggregate evidence that temporary, lump-sum transfers can generate large and persistent increases in spending. The postwar veterans' payments and temporary Social Security transfers studied here produce sharp but short-lived increases in aggregate transfers. Consumption, however, responds gradually and remains elevated well beyond the initial spike in payments. The cumulative transfer multiplier reaches approximately one within six months and exceeds two after one year. A temporary fiscal shock thus gives rise to a spending response that is considerably more persistent than the shock itself.

The empirical strategy exploits the timing of disbursements that were largely predetermined by administrative processes rather than enacted in response to deteriorating macroeconomic conditions. A detailed narrative record supports this identifying assumption, and the results

[10] In this limit, the model displays the same qualitative patterns as sticky-expectations frameworks such as Mankiw and Reis (2002), Carroll et al. (2020), and Auclert, Rognlie, and Straub (2020).

prove robust across a wide range of specifications. We find no evidence of pre-trends, the estimates remain stable when controlling for other fiscal, monetary, and oil shocks, and leave-one-out exercises confirm that the multiplier is not driven by a single episode. Taken together, the evidence points to a sizable and systematic aggregate demand effect of temporary transfers in the postwar United States.

To interpret these findings and connect them to modern policy, we develop a heterogeneous-agent New Keynesian model disciplined by the empirical multiplier. The key challenge is to rationalize the persistence of consumption in response to a transitory shock. High marginal propensities to consume are necessary but not sufficient. Under standard full-information rational expectations, consumption responds too quickly and lacks the hump-shaped dynamics observed in the data. Matching the empirical evidence instead requires departures from full information: households must initially underreact to the shock and subsequently revise their expectations upward, generating a delayed amplification of spending. This mechanism aligns with the diagnostic-expectations framework and allows the model to replicate the gradual and persistent response of consumption.

Within this environment, we show that postwar veterans' payments and modern stimulus checks are equivalent objects from the perspective of the consolidated government: both correspond to one-time, debt-financed transfers to households. This equivalence provides a disciplined mapping from the historical episodes to contemporary policy analysis. Calibrated to match the postwar multiplier, the model implies that stimulus checks can materially support aggregate consumption. In their absence, consumption would decline more sharply and recover more slowly under plausible policy configurations.

More broadly, the results speak to a central divide in macroeconomics. In representative-agent models with Ricardian equivalence, temporary transfers have limited aggregate effects. In heterogeneous-agent environments with high MPCs and non-standard expectation formation, redistribution and endogenous propagation mechanisms can amplify and prolong the impact of fiscal policy. The postwar evidence strongly supports the latter view. Temporary stimulus checks can meaningfully boost aggregate consumption, with effects that persist well beyond the initial transfer.

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Online Appendix

The Macroeconomic Effect of Stimulus Checks

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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

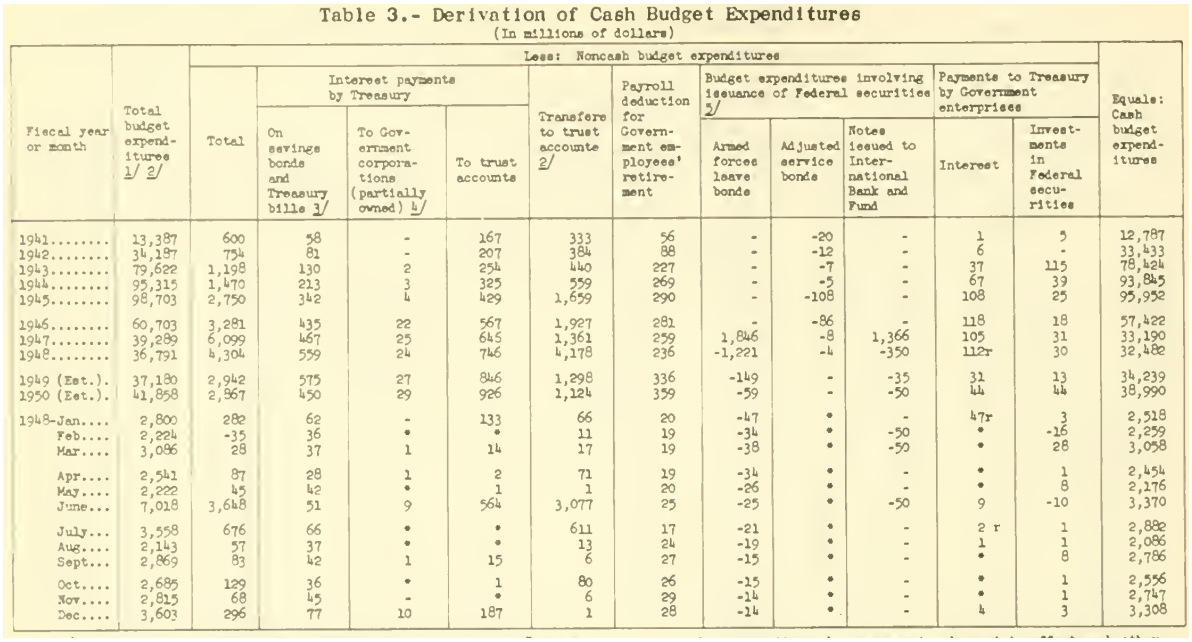


Table 3.- Derivation of Cash Budget Expenditures
(In millions of dollars)

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 2/			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:

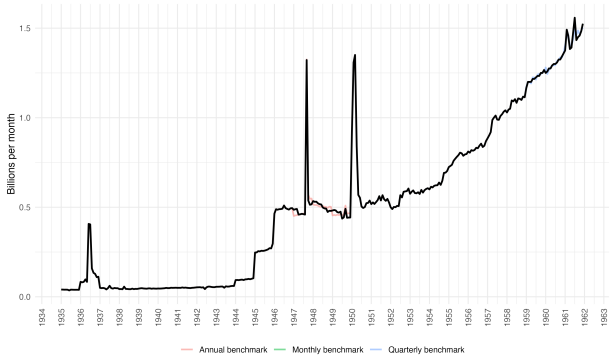
$$T_t^{\text{bench}} = \widetilde{T}_t + \frac{R_P}{N_P}, \qquad t \in P, \tag{5}$$

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

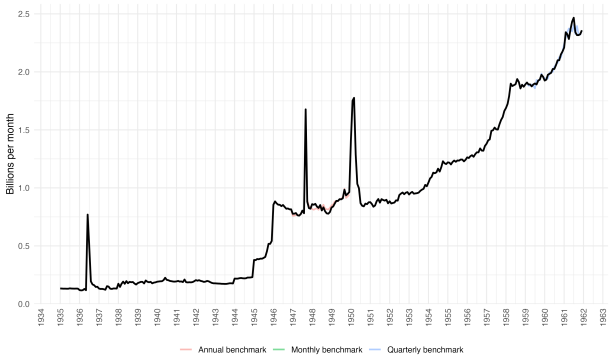
Splice. For each aggregate (narrow and broad), the preferred splice is: 1935–1946 annual benchmarking; 1947–1958 quarterly benchmarking; 1959 onward monthly benchmarking. Figure A.3 shows the benchmark-consistent candidates and the stitched preferred series.

Figure A.3: Benchmark candidates and stitched series

(a) Narrow transfers



(b) Broad transfers



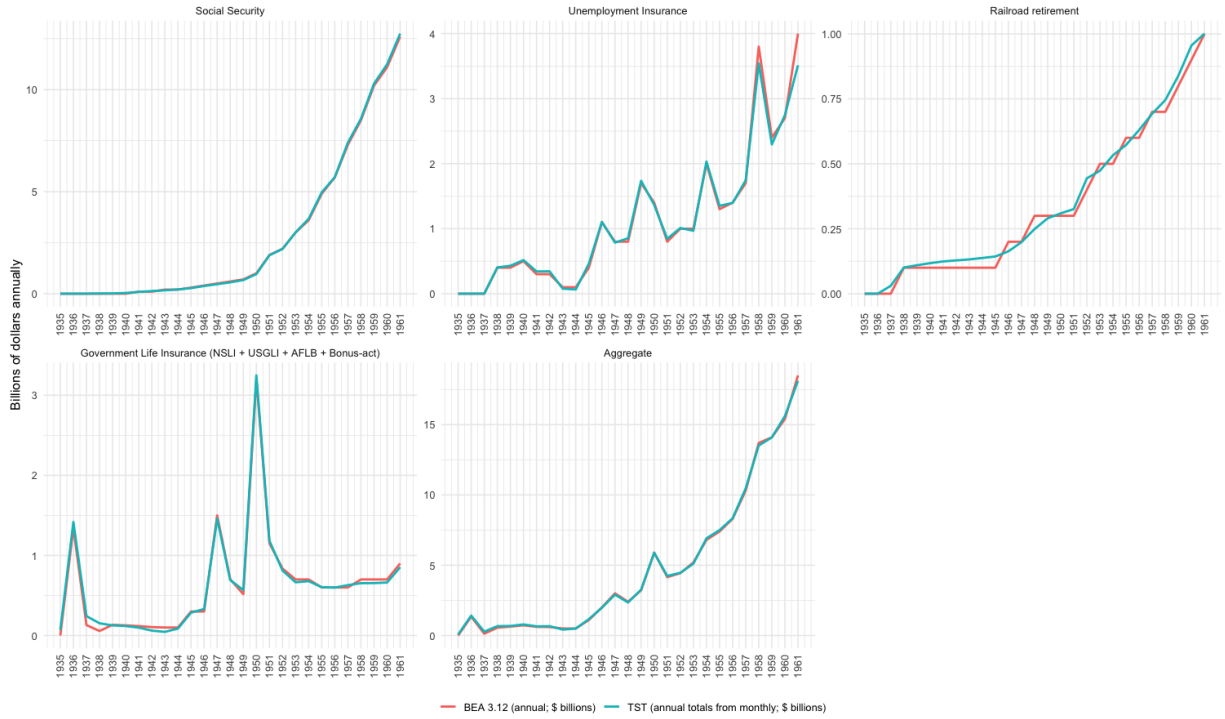
x

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

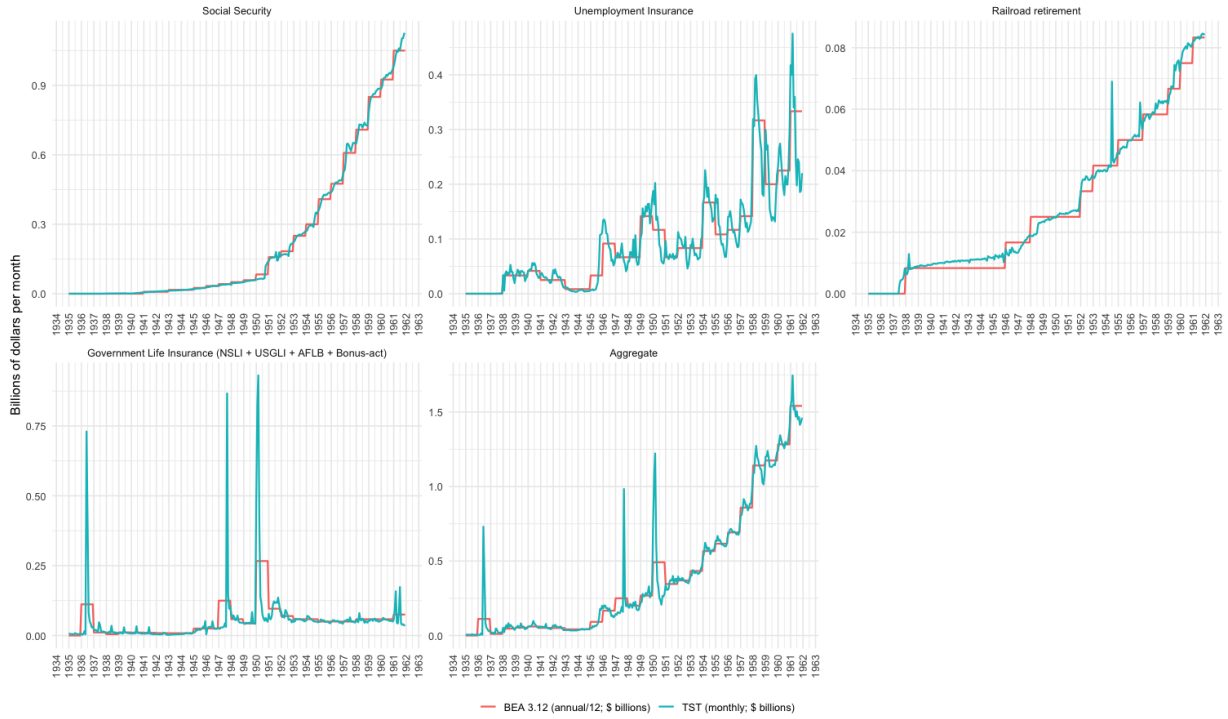
BEA Table 3.12 validation. We compare annual totals implied by summing Treasury monthly flows within year to annual line items from BEA Table 3.12 (*Government social benefits*) for Social Security, unemployment insurance, railroad retirement, and veterans' insurance-related payments. Figure A.4 shows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



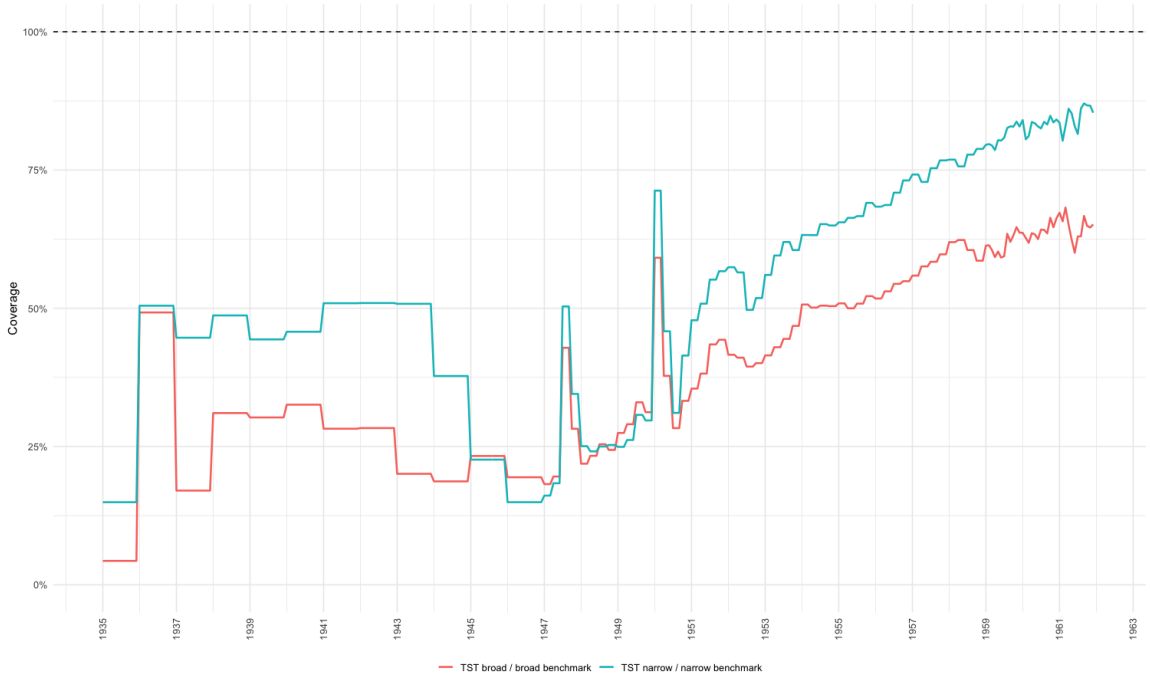
Notes: Treasury monthly flows compared to BEA annual totals divided by twelve.

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.

The following figures display the resulting retail sales series and its durable/non-durable decomposition over 1935–2025.

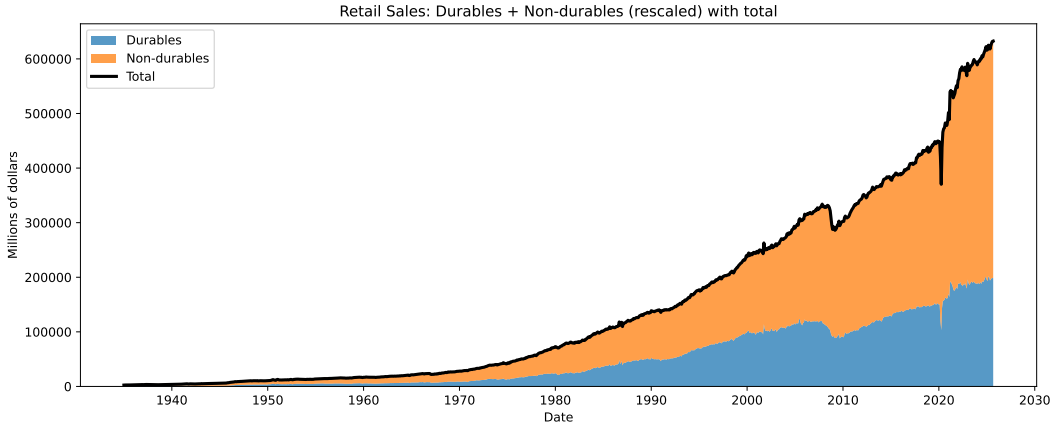


Figure A.7: U.S. retail sales and durable/non-durable decomposition (1935m1–2025m9).

A.3. Narrative Transfer Shock Episodes

In this Appendix, we provide more detail on the narrative analysis underlying our transfer shock series. Our primary focus is on the postwar veterans' payments, for which we construct the narrative record directly from historical sources. We document the institutional background, timing, and motivation of each episode, drawing on contemporary accounts and administrative records to establish the plausibly exogenous nature of these payments.

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.4 lists the full set of shocks used in the analysis.

Table A.4: 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).

Additionally, Table A.5 lists the endogenous veterans shocks that we do not include in our baseline series.

Table A.5: 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 dividend acceleration	Jan. 1963	$327 million	0.21%
World War Two insurance special dividend	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.

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.

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

Table B.1: Granger causality test

Variable	p-value
Transfer shock	0.790
Transfers	0.252
Industrial production	0.158
Retail sales	0.815
Employees nonfarm	0.439
Unemployment rate	0.294
Personal consumption expenditure	0.745
Consumer price index	0.669
Commodity price index	0.968
WTI crude	0.898
Short rate	0.767
Overall	1.000

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

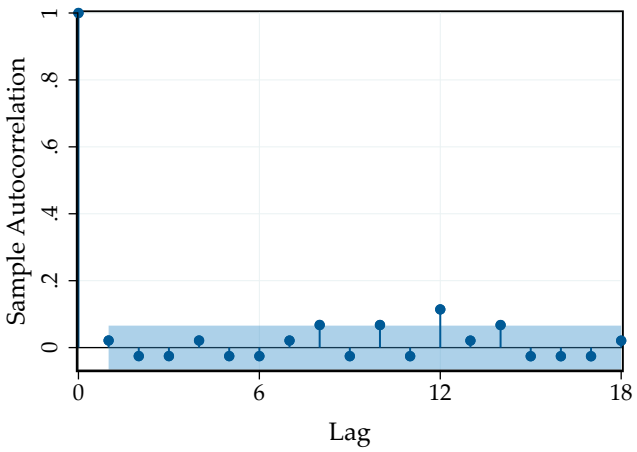
Table B.2: Correlation with Other Shock Measures

Shock	Description	Source	ρ	p-value	n	Sample
Government spending	Military news	Ramey (2011)	-0.03	0.36	849	1945m1– 2015m9
Tax	Narrative shock	Romer and Romer (2004)	0.01	0.75	756	1945m1–2007m12
Monetary policy	Narrative shock	Romer and Romer (2010)	0.03	0.57	468	1969m1–2007m12
Oil supply	Oil supply news	Känzig (2021)	-0.03	0.41	600	1975m1–2024m12

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.

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 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. MPCs from the 1950 Case Study

This section studies how we estimate MPCs from the 1950 case study, as in Figure 4 from the main text. The dataset is an extract from the 1951 postwar 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. A spending unit is equivalent to a household.

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)".

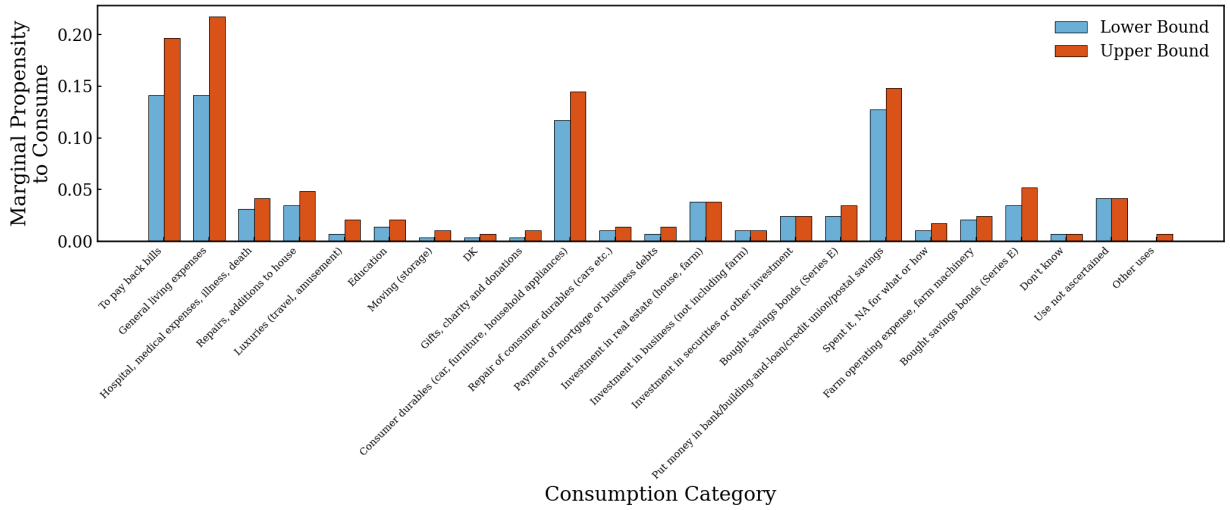
The narrow answers are coded spending-use categories: bills, general living expenses, medical expenses, repairs to the house, luxuries, education, moving, consumer durables, debt repayment, deposits into bank accounts, savings bonds, investment, gifts/transfers, and a small number of residual DK or other codes. The instrument allows up to two coded uses. The raw data therefore contain two use slots. Multiple responses are relatively uncommon: 43/290 = 14.8% of G.I.-dividend recipients give two coded uses.

Ideally, everyone would answer that they only spent the payment on one thing, which would let us point-identify MPCs. Since some people use the payment for multiple things, we cannot point identify MPCs—but we can bound them. Let T be the number of spending units that received the payment. For a narrow category C, let A be the number of recipients who report only C, and let B be the number of additional recipients who report C alongside one other category. Then A/T is a lower bound and $(A+B)/T$ is an upper bound on the fraction of the payment allocated to C.

We repeat the same logic repeated after first mapping narrow categories into four buckets: durable consumption, non-durable consumption, debt repayment/saving/transfers/investment, and other. If the two raw responses map into the same broad bucket, the second response is dropped so that the bounds tighten rather than mechanically double-counting the same broad use. We use the following mapping:

Narrow category	Broad category
To pay back bills	Debt repayment, saving, transfers, or investment
General living expenses	Non-durable consumption
Hospital, medical expenses, illness, death	Non-durable consumption
Repairs, additions to house	Durable consumption
Luxuries (travel, amusement)	Non-durable consumption
Education	Non-durable consumption
Moving (storage)	Non-durable consumption
DK	Other
Gifts, charity and donations	Debt repayment, saving, transfers, or investment
Consumer durables	Durable consumption
Repair of consumer durables	Durable consumption
Payment of mortgage or business debts	Debt repayment, saving, transfers, or investment
Investment in real estate	Debt repayment, saving, transfers, or investment
Investment in business	Debt repayment, saving, transfers, or investment
Investment in securities or other investment	Debt repayment, saving, transfers, or investment
Bought savings bonds (Series E)	Debt repayment, saving, transfers, or investment
Put money in bank	Debt repayment, saving, transfers, or investment
Spent it, NA for what or how	Other
Farm operating expense, farm machinery	Debt repayment, saving, transfers, or investment
Bought savings bonds (Series E)	Debt repayment, saving, transfers, or investment
Don't know	Other
Use not ascertained	Other
Other uses	Other

We now show MPCs for narrow categories of goods:



Notes: Self-reported uses of the 1950 G.I. insurance dividend, from the 1951 Survey of Consumer Finances ($T = 290$ recipient spending units). Each bar shows the fraction of recipients reporting a given narrow spending category. Respondents could report up to two uses. The lower bound counts only recipients whose sole reported use is that category; the upper bound additionally includes recipients who report that category alongside one other use. Categories are mutually exclusive survey codes.

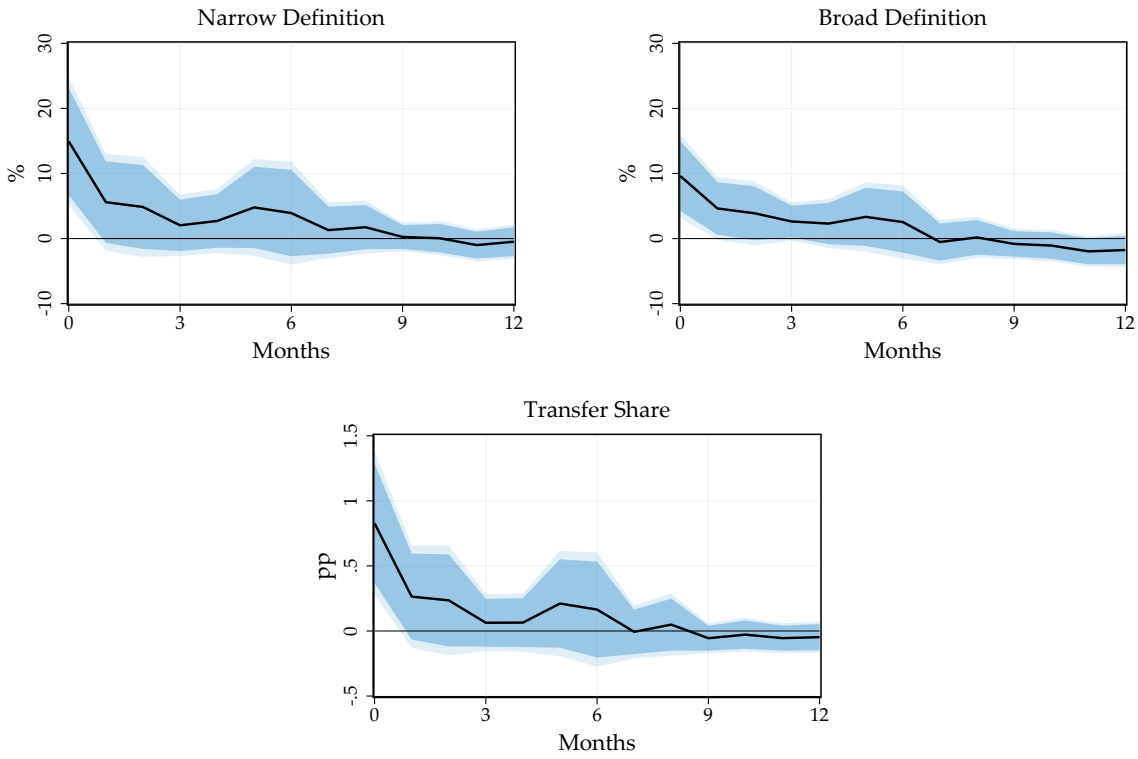
The figure in the main text shows the equivalent for MPCs with broad categories of goods.

B.3. Additional Results

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

Figure B.2 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.

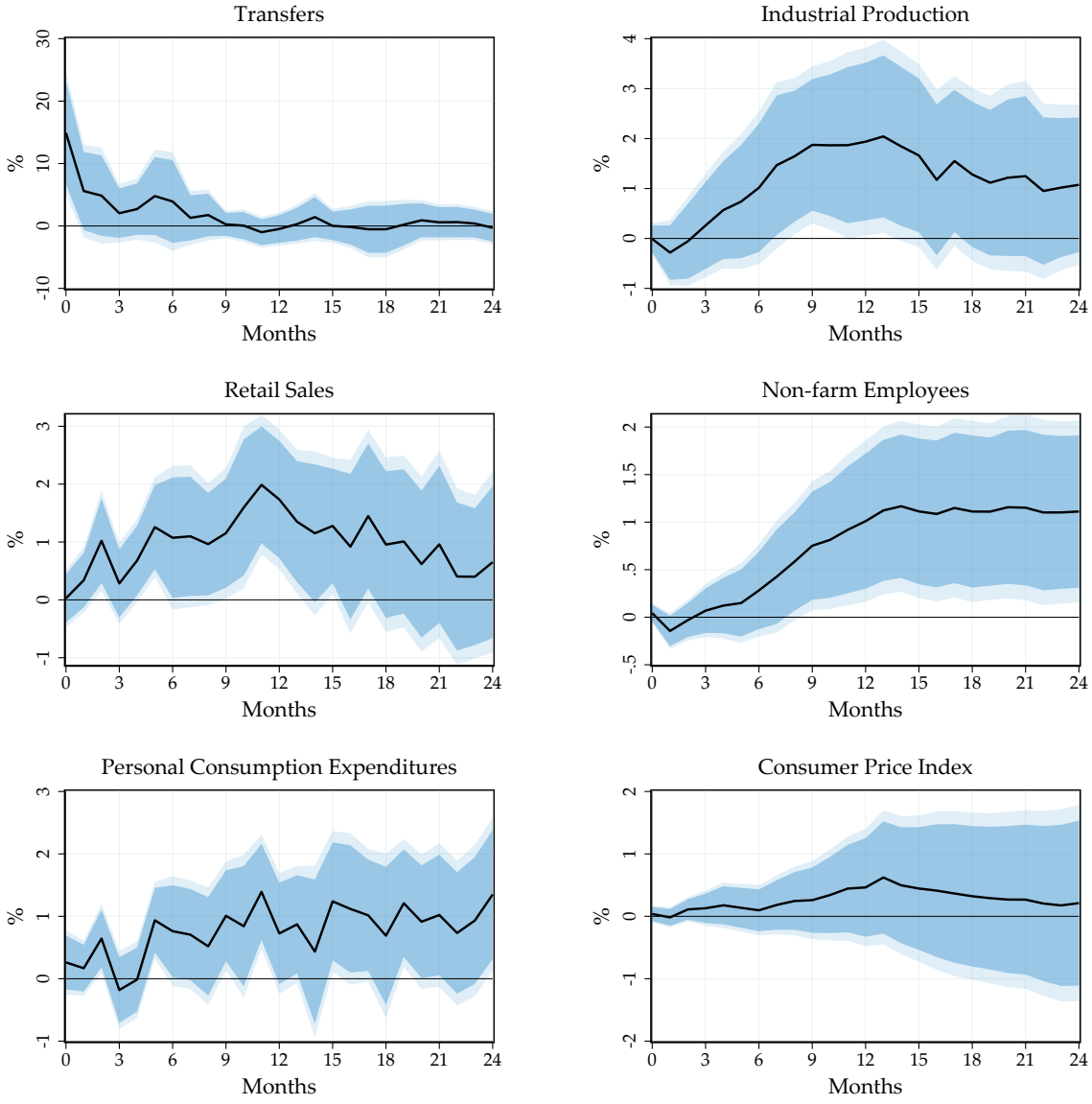
Figure B.2: Transfer Responses under Alternative Definitions



Notes: Impulse responses of government transfers to a temporary transfer shock, estimated using the local projections specification (5) using the plausibly exogenous transfer shock dates. The narrow measure includes social security, railroad retirement, and government life insurance-related discursements; 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.

Figure B.3 extends the baseline impulse responses to a 24-month horizon. The responses display substantial persistence: despite the transitory nature of the transfer shock, real activity remains elevated well beyond the first year. While the estimates become less precise at longer horizons, the effects decay only gradually, indicating that the impact of temporary transfers is long-lasting.

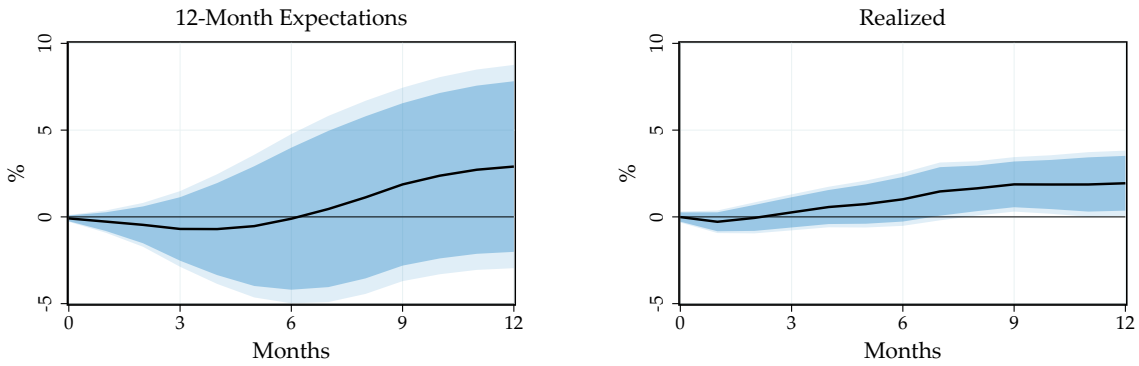
Figure B.3: Longer-term Responses



Notes: Longer-term responses to a temporary transfer shock, estimated using the local projections specification (5) using the plausibly exogenous transfer shock dates. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure B.4 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.4: 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 (5) 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.

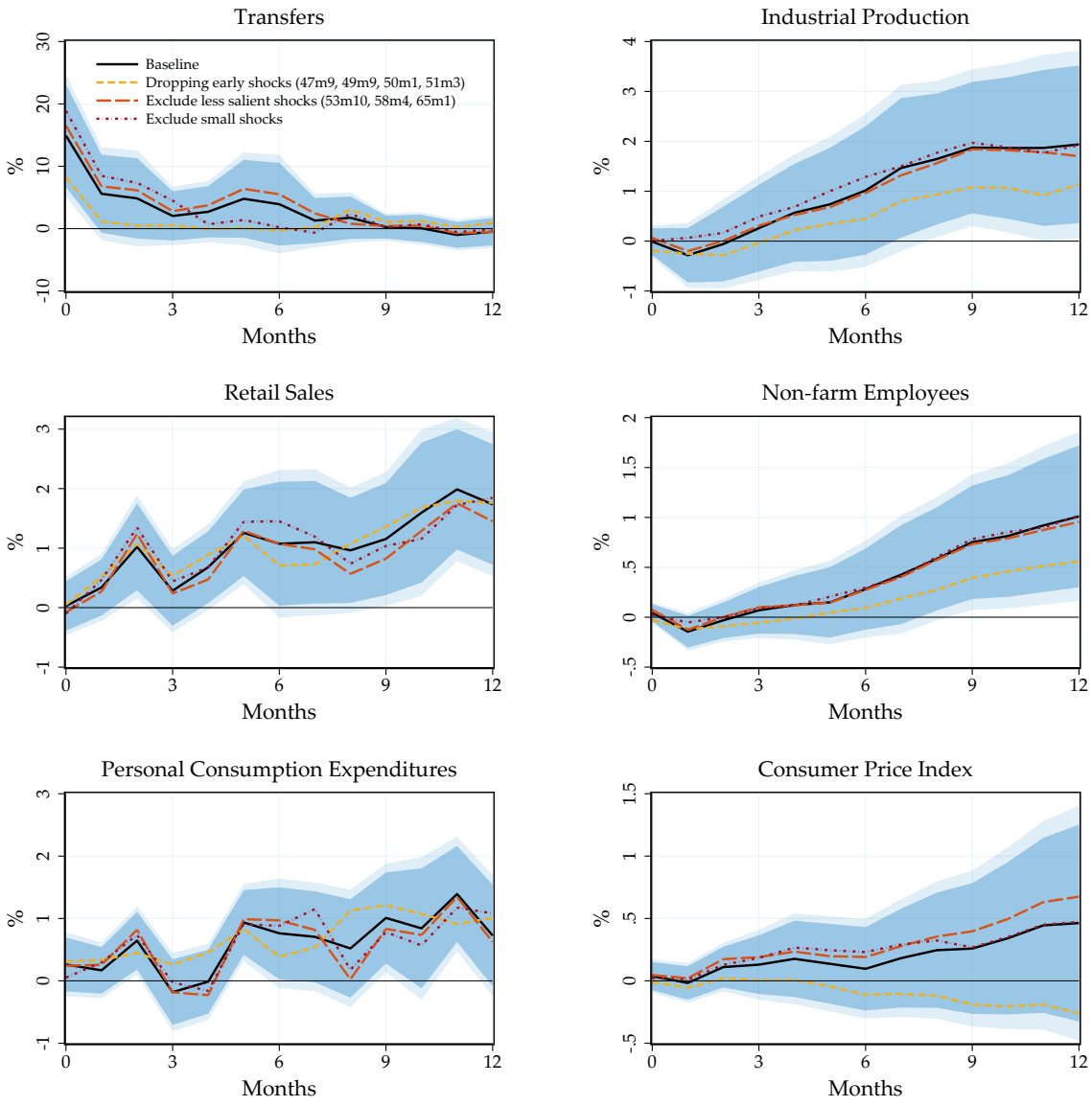
B.4. Additional Sensitivity

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.4.1. Shock Selection

Figure B.5 reports the responses of all baseline variables under alternative selections of transfer shocks.

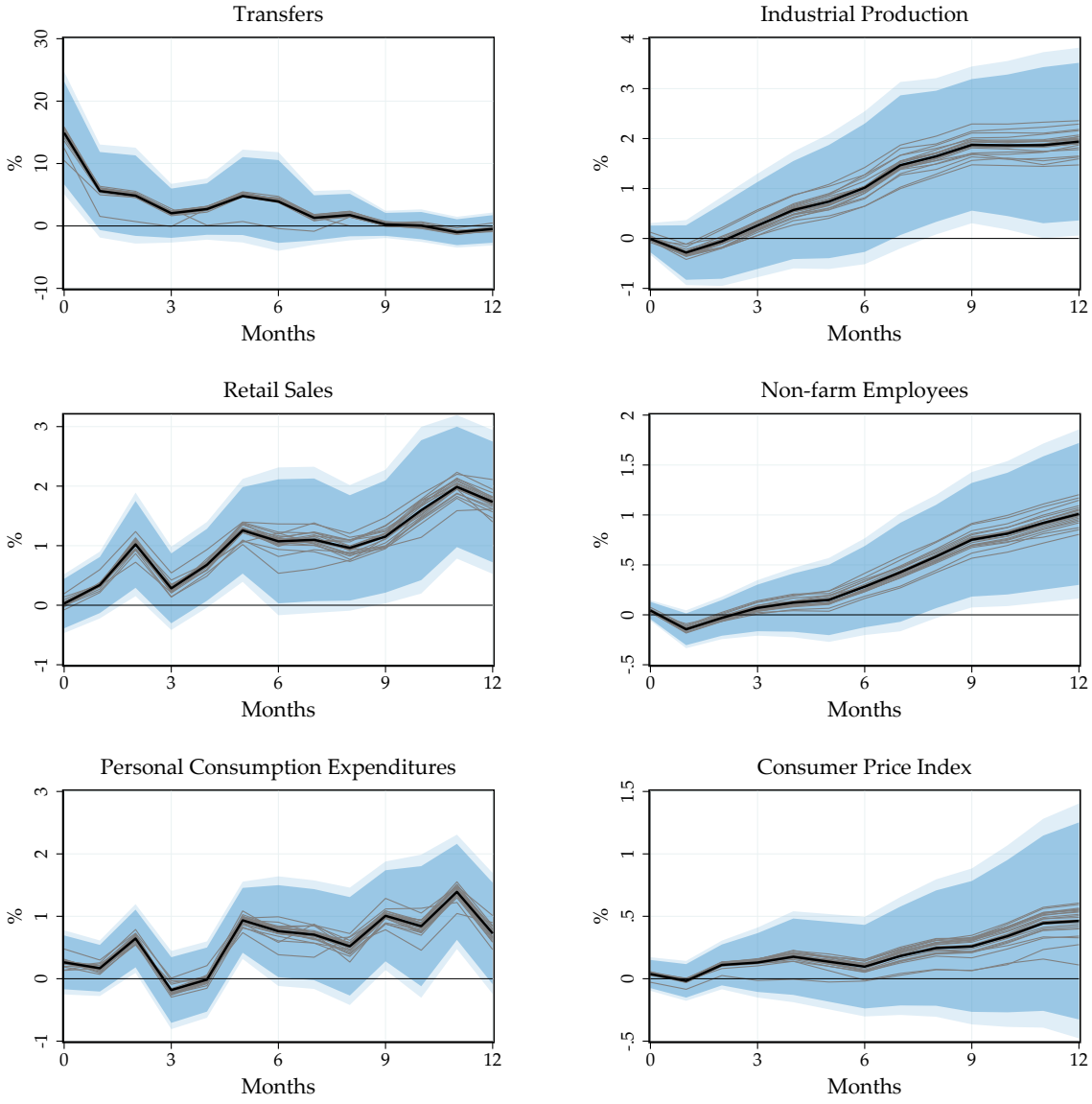
Figure B.5: Sensitivity to Shock Selection



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) 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.6 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.

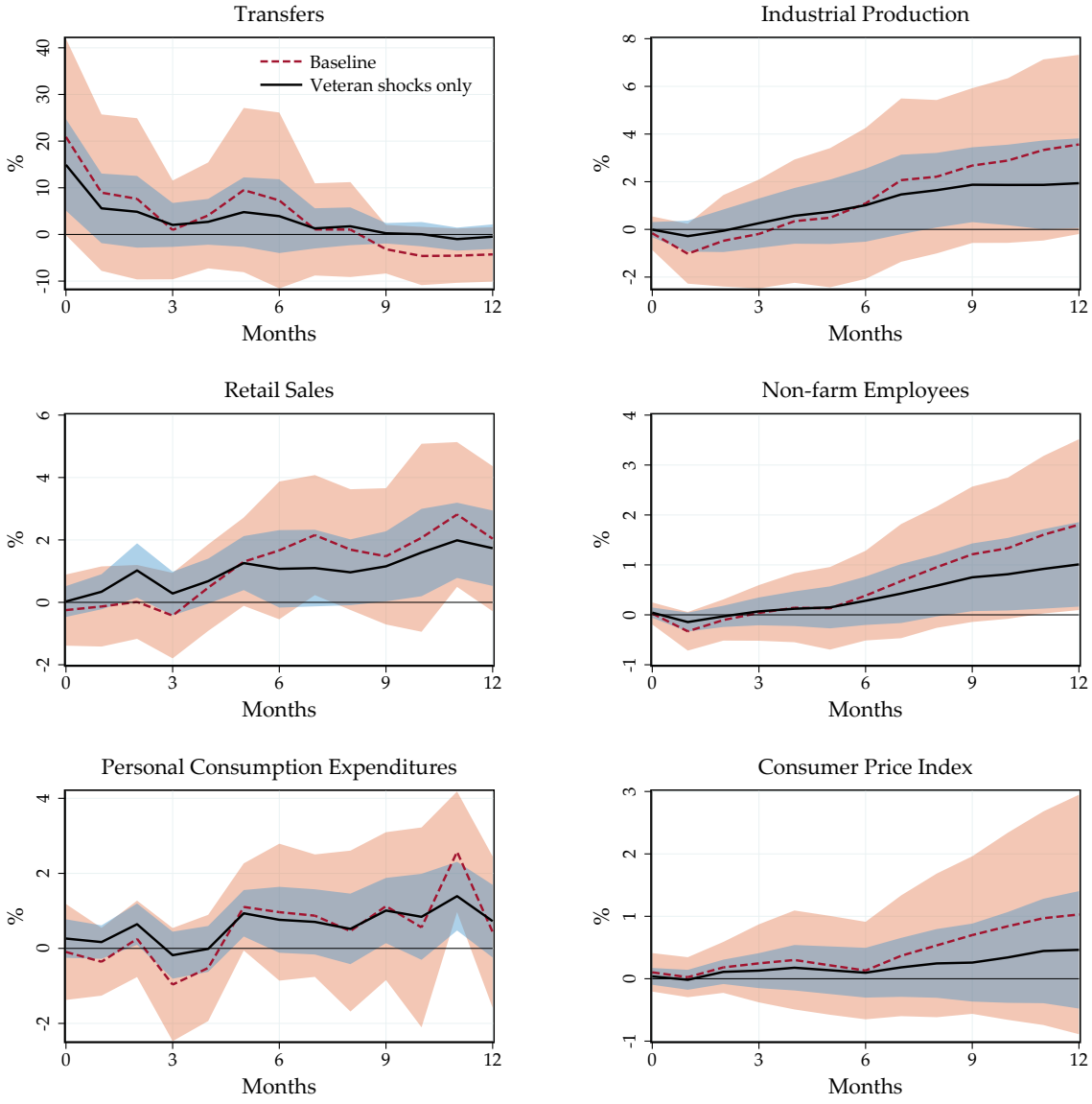
Figure B.6: Leave-one-out Exercise



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5), 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.7 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.

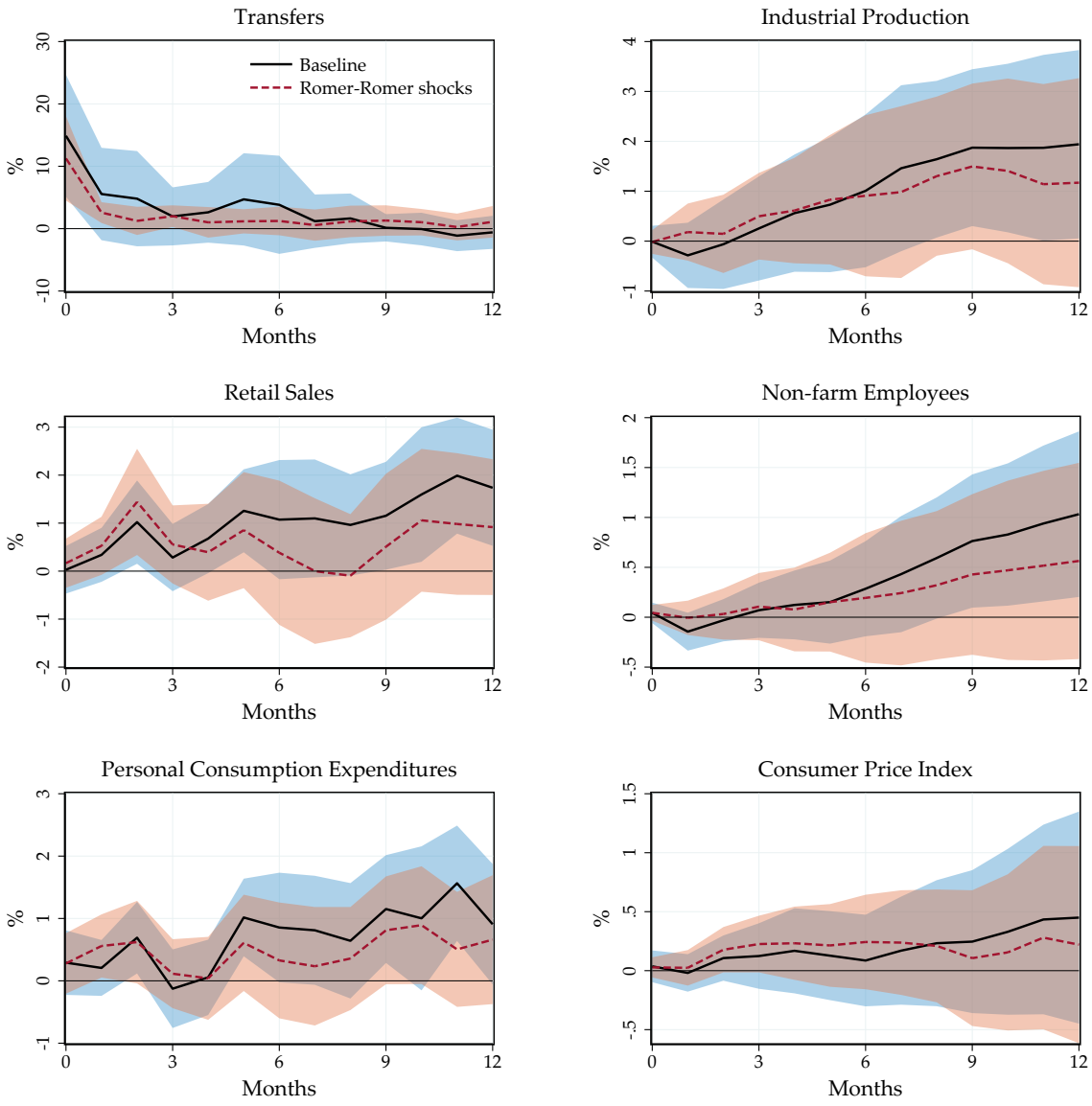
Figure B.7: Using Veteran Shocks Only



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) 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.8 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.8: Using Romer-Romer Transfer Shocks

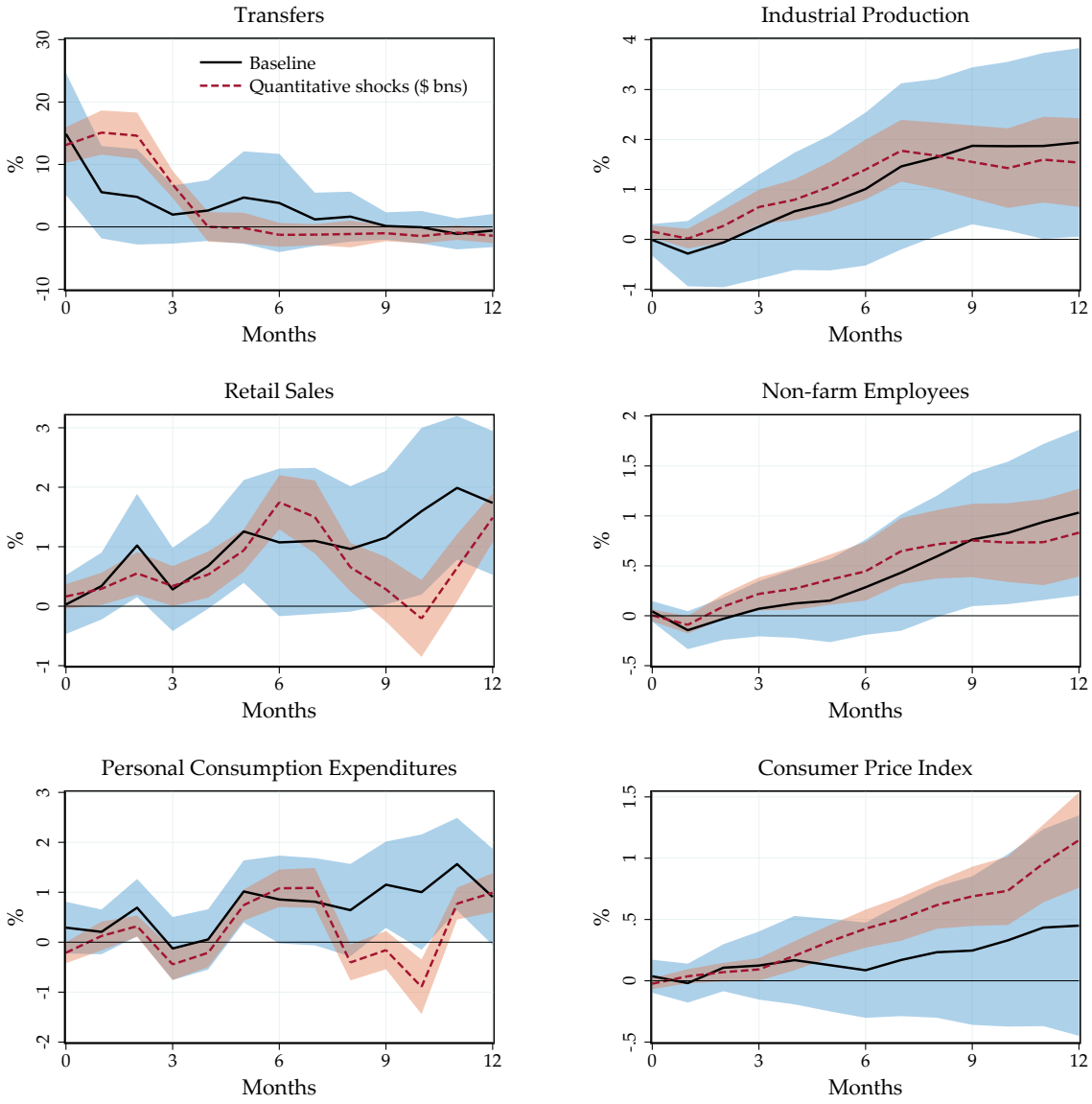


Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) with the narrative transfer shock dates restricted to the Romer and Romer (2016) Social Security transfer episodes. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure B.9 reports the responses of all baseline variables when transfer shocks are measured using the dollar magnitude of each episode rather than a binary indicator. The estimated responses are broadly comparable in magnitude and persistence to the baseline and are in some cases more precisely estimated. However, the consumption responses are less stable across horizons, likely reflecting the influence of extreme observations. This suggests that while quantitative shocks can increase statistical precision, they may also introduce additional sensitivity

to large episodes.

Figure B.9: Quantitative Transfer Shocks



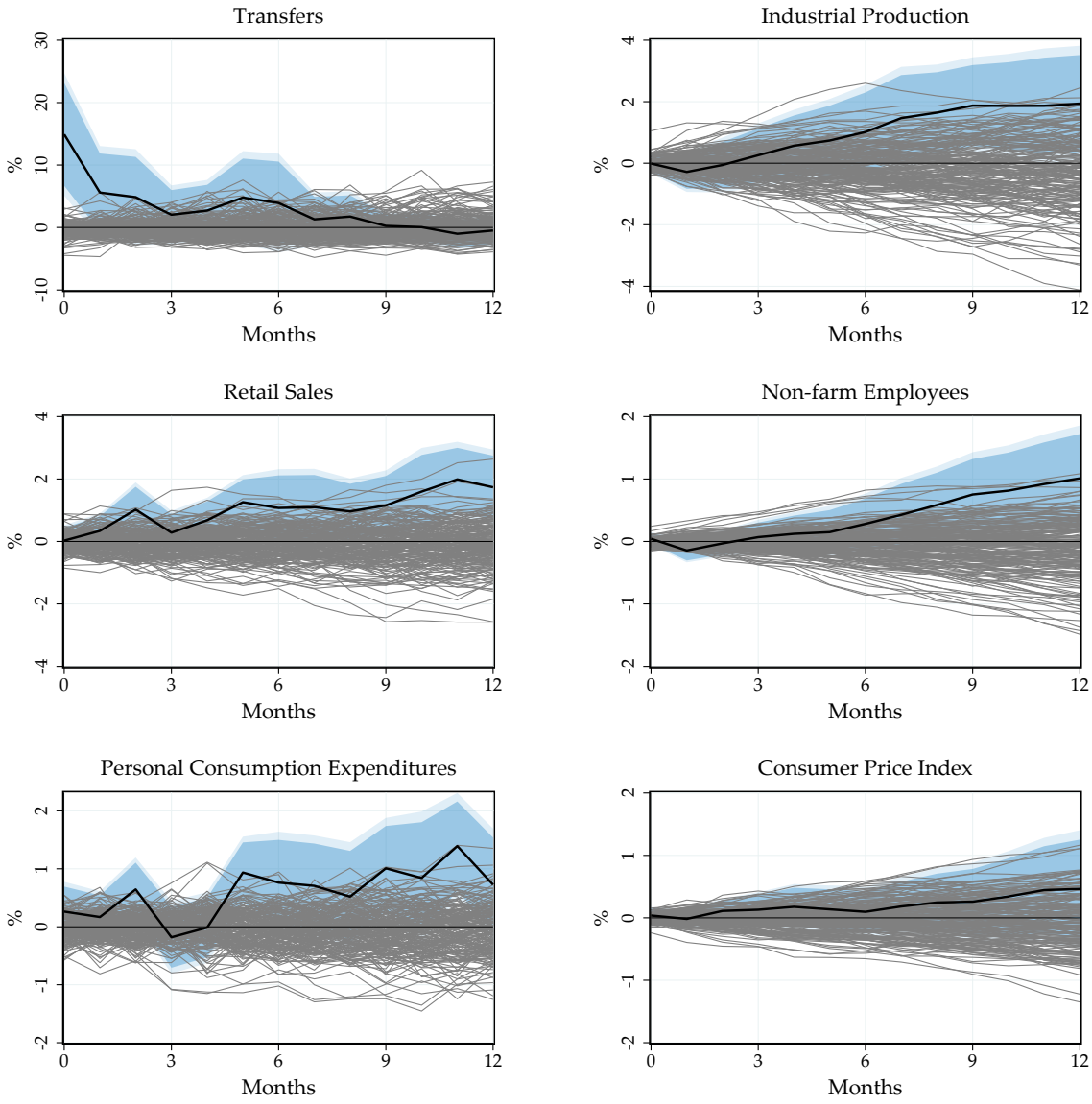
Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) with quantitative transfer shocks based on the (real) dollar magnitude of each episode. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence 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.10 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.10: Placebo Exercise



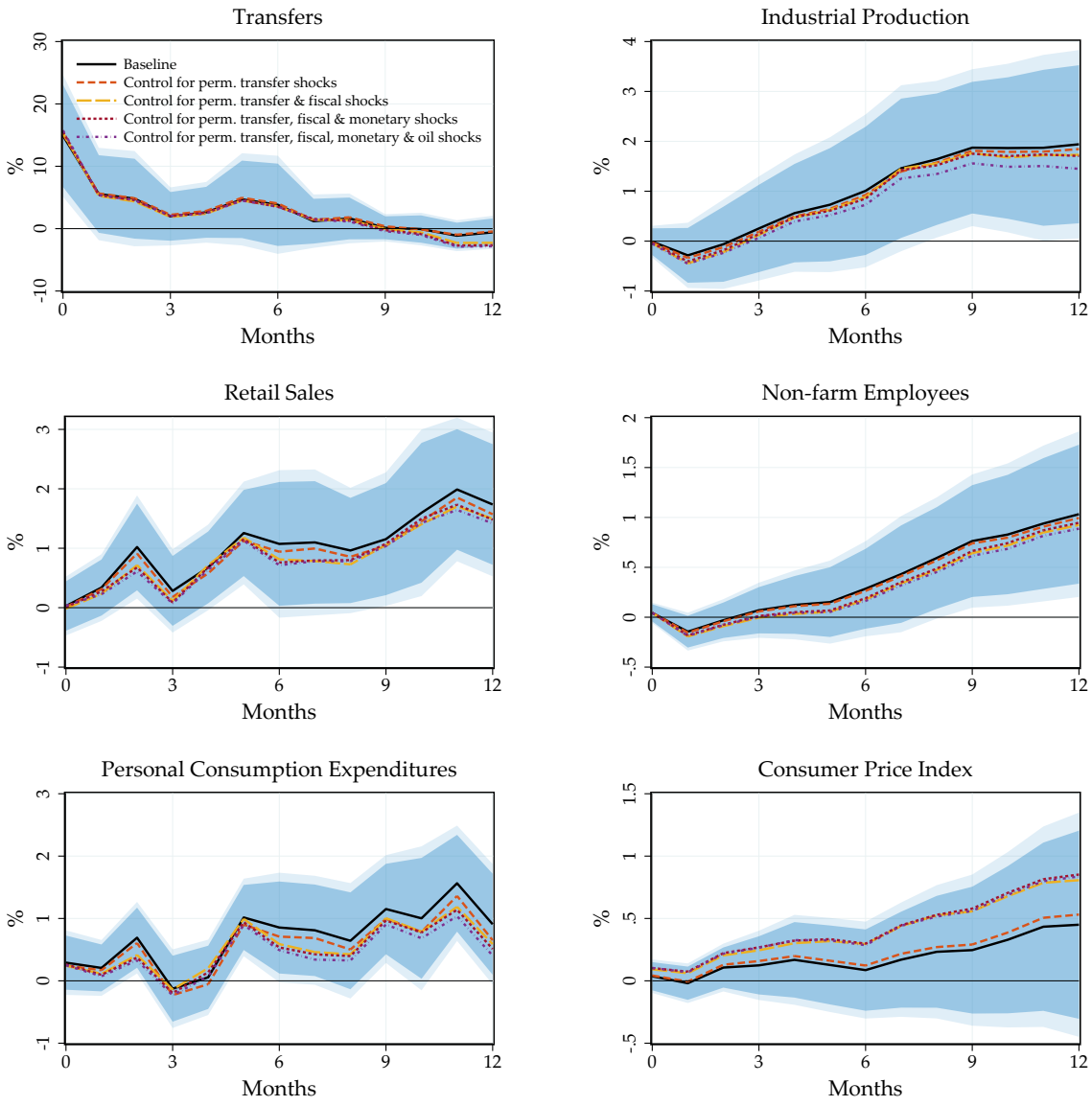
Notes: Impulse responses to placebo transfer shocks, estimated using the local projections specification (5). Placebo shocks are constructed by randomly selecting 22 event dates that do not coincide with identified transfer episodes; the procedure is repeated 200 times. Gray lines: impulse responses 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.4.2. Controls, Pre-trends and Sample Period

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

Figure B.11 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.

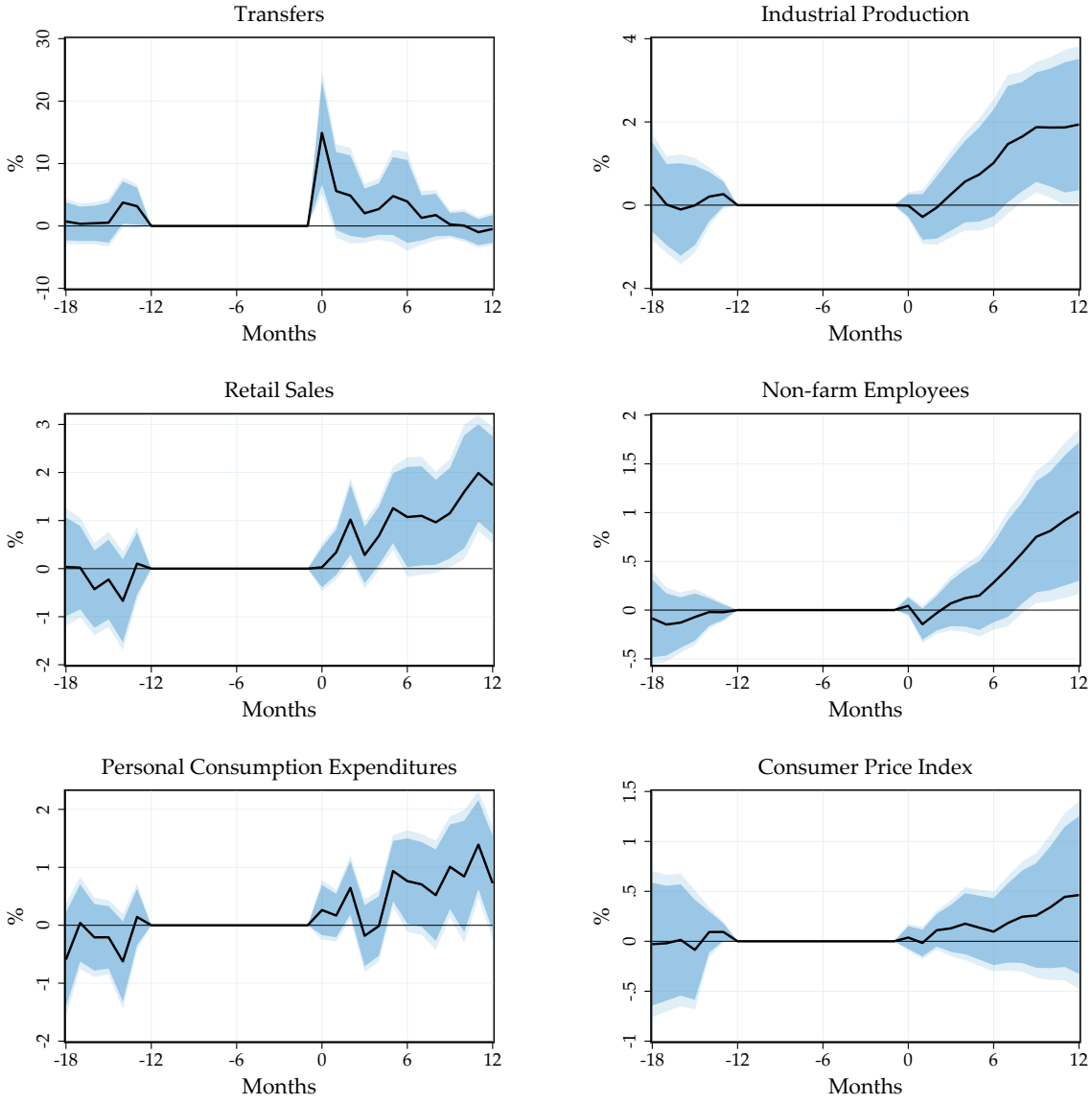
Figure B.11: Sensitivity with Respect to Controls



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) 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.12 examines pre-trends for all baseline variables by extending the specification to include additional lags of the outcome variable. Beyond the mechanically zero pre-period implied by the lag structure, we find no evidence of anticipatory dynamics: coefficients prior to the shock are small and statistically indistinguishable from zero across all variables.

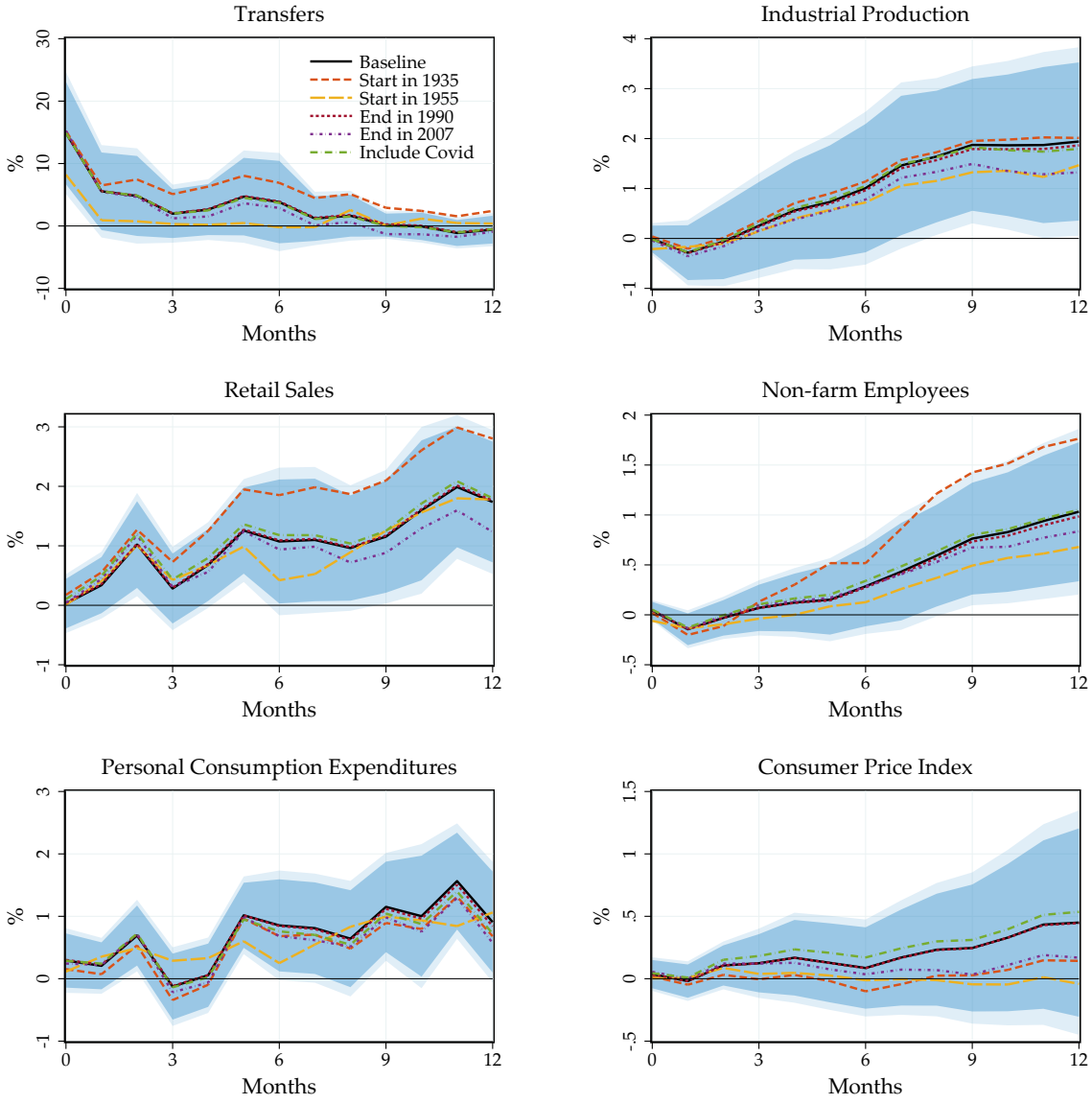
Figure B.12: Pre-trends



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5), extended to lags of the outcome variable to test for pre-trends. Sample: 1945m10–2019m12. Solid black lines: point estimates. Blue shaded areas: 90% and 95% confidence bands.

Figure B.13 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.13: Robustness with Respect to Sample Period

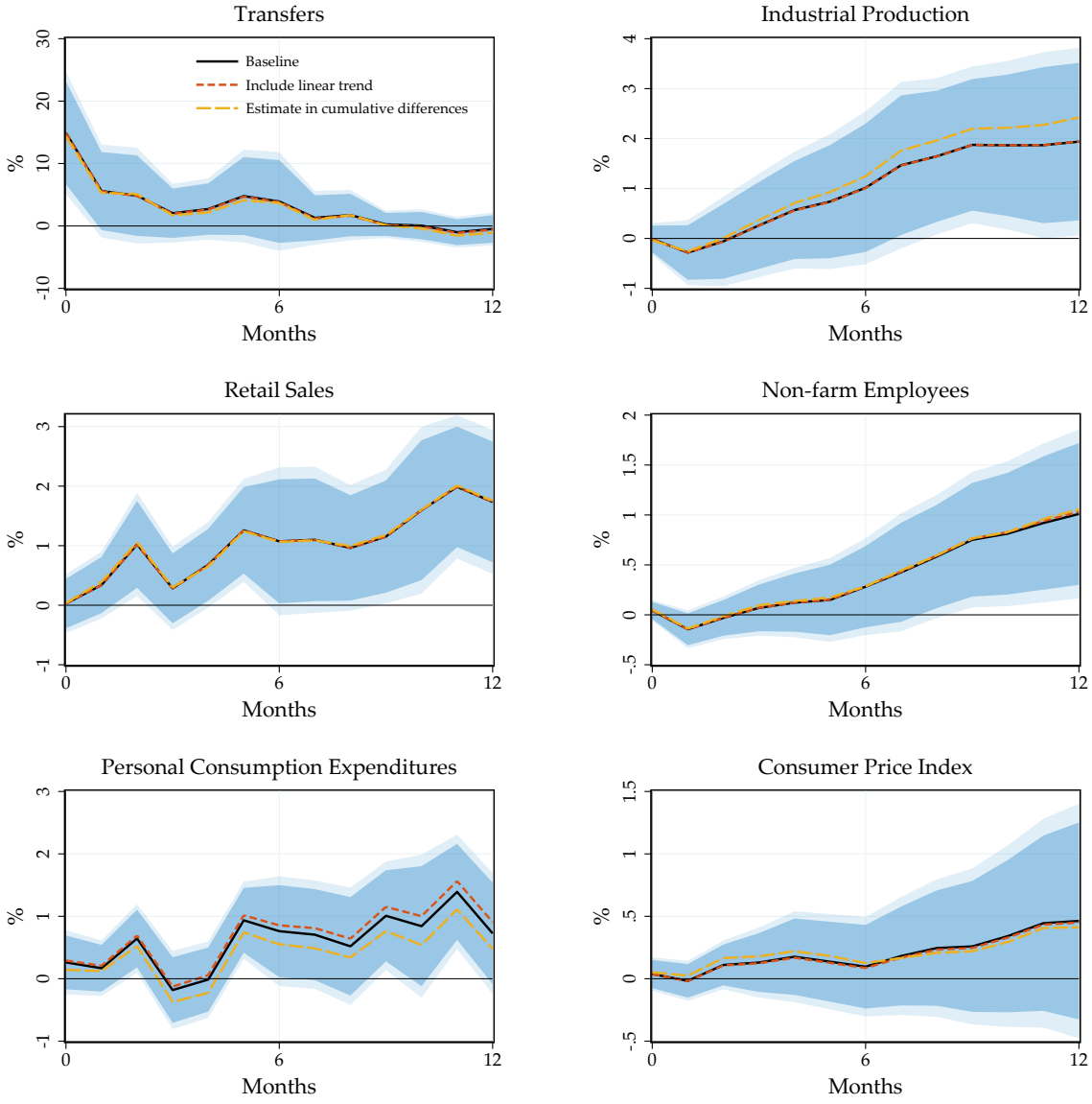


Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5) under alternative sample periods: 1935m1–2019m12, 1955m1–2019m12, 1945m10-1990m12, 1945m10-2007m12, and 1945m10-2025m12. Baseline sample: 1945m10–2019m12. Solid black lines and blue shaded areas: baseline point estimates and corresponding 90% and 95% confidence bands. Colored lines: point estimates under alternative specifications.

B.4.3. Other Specification Choices

Figure B.14 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.14: Robustness with respect to detrending



Notes: Impulse responses to a temporary transfer shock, estimated using the local projections specification (5), 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.

C. Model

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). Using this framework, we write the change

of average consumption in group g as

$$d\mathbf{C}_v = M_v^Y (d\mathbf{Y} - d\mathcal{T}) + M_v^{T^f} \overline{\omega}_v d\mathbf{T}^f + M_v^{T^v} \frac{1}{\nu} d\mathbf{T}^v \tag{7}$$

$$d\mathbf{C}_{nv} = M_{nv}^Y (d\mathbf{Y} - d\mathcal{T}) + M_{nv}^{T^f} \overline{\omega}_{nv} d\mathbf{T}^f \tag{8}$$

where $\overline{\omega}_v = \int_{i \in v} \omega_i di/\nu$ and $\overline{\omega}_{nv} = \int_{i \in nv} \omega_i di/(1-\nu)$ denote the average targeting coefficients among veterans and non-veterans, respectively, and $d\mathbf{X} \equiv [dX_0, dX_1, dX_2, ...]$. In Appendix X, we show how to compute the generalized sequence-space Jacobians $M_g^X \equiv \left[\frac{\partial C_{g,t}}{\partial X_h}\right]_{t,h=0,1,2,...}$, which already incorporate expectations (see also Angeletos, Guerreiro, and Zhang, 2025).

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

$$d\mathbf{Y} = \overline{M}^Y (d\mathbf{Y} - d\mathcal{T}) + \overline{M}^{T^f} d\mathbf{T}^f + M_v^{T^v} d\mathbf{T}^v, \tag{9}$$

where $\overline{M}^Y \equiv \nu M_v^Y + (1-\nu) M_{nv}^Y$ and $\overline{M}^{T^f} \equiv \nu \overline{\omega}_v M_v^{T^f} + (1-\nu) \overline{\omega}_{nv} M_{nv}^{T^f}$ denote the economy-wide average Jacobians.

References Appendix

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