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TRANSFER PAYMENTS AND THE MACROECONOMY:  
THE EFFECTS OF SOCIAL SECURITY BENEFIT CHANGES, 1952-1991

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Transfer Payments and the Macroeconomy: The Effects of Social Security Benefit Changes,  
1952-1991

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### **ABSTRACT**

From the early 1950s to the early 1990s, increases in Social Security benefits in the United States varied widely in size and timing, and were only rarely undertaken in response to short-run macroeconomic developments. This paper uses these benefit increases to investigate the macroeconomic effects of changes in transfer payments. It finds a large, immediate, and statistically significant response of consumption to permanent changes in transfers. The response appears to decline at longer horizons, however, and there is no clear evidence of effects on industrial production or employment. These effects differ sharply from the effects of relatively exogenous tax changes: the impact of transfers is faster, but much less persistent and dramatically smaller overall. Finally, we find strong statistical and narrative evidence of a sharply contractionary monetary policy response to permanent benefit increases that is not present for tax changes. This may account for the lower persistence of the consumption effects of transfers and their failure to spread to broader indicators of economic activity.

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## **I. INTRODUCTION**

Government transfer payments are the relative unknowns of fiscal policy. There have been many studies of the short-run macroeconomic effects of changes in government purchases and taxes, but much less research has been done on the aggregate impacts of transfer payments. Yet such payments are substantial. In the United States, for example, federal transfer payments account for about 10 percent of GDP and more than 40 percent of federal spending. This paper takes a step toward filling this gap in our knowledge by examining the macroeconomic impact of changes in Social Security benefits in the United States from 1952 to 1991.<sup>1</sup>

For much of the postwar period, increases in Social Security benefits occurred somewhat randomly. The generosity and breadth of the program was expanded in several steps during the 1950s and 1960s. Until 1974, cost-of-living increases were not automatic, but were legislated at irregular intervals. And from 1974 until the early 1990s, tremendous variation in inflation and occasional bursts of retroactive payments resulting from idiosyncratic factors, as well as a legislated change in the timing of cost-of-living adjustments, led to irregular and variable benefit changes.

We use documents from the Social Security Administration, Congress, and the executive branch to identify the motivation, timing, and size of benefit changes over these decades. This narrative analysis allows us to exclude the benefit changes that were explicitly made for countercyclical purposes and to separate permanent and temporary benefit changes.

We then estimate how aggregate consumer spending responds to the relatively exogenous changes in Social Security benefits. We find that permanent increases in benefits have a nearly one-for-one impact on consumer spending in the month they occur, and that this effect is highly statistically significant. The effect persists for roughly half a year and then appears to wane sharply—though the standard errors become large at longer horizons. Interestingly, we find that

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<sup>1</sup> Oh and Reis (2012) document the importance of changes in transfers in short-run movements in government expenditures, and describe some of the channels through which they could have aggregate effects.

temporary benefit changes (which mainly took the form of one-time retroactive payments in the period we consider) have a much smaller impact on consumption. Neither permanent nor temporary changes in benefits appear to affect broader measures of economic activity, such as industrial production or employment.

In some models of macroeconomic behavior, taxes and transfers have equal and opposite effects on household consumption and overall economic activity. To compare the effects of taxes and transfers, we expand our analysis to also include the relatively exogenous federal tax changes identified in Romer and Romer (2010). Like the permanent Social Security benefit changes, these tax changes were almost all legislated to be very long-lasting. We find very large differences in the response of consumption to permanent benefit increases and tax changes. The effects of benefit increases are faster, but much less persistent and dramatically smaller overall. In both cases, the main component of consumption that responds is purchases of durable goods.

One possible explanation for the behavior of consumption following permanent Social Security benefit increases, and its contrast with the impact of tax changes, involves the response of monetary policy. We find a rise in the federal funds rate in response to benefit increases that is very fast, economically large, and highly statistically significant. Following exogenous tax cuts, in contrast, the federal funds rate moves little for more than a year. The records of the Federal Reserve reveal that policymakers were very aware of the benefit increases and often viewed them as a reason to tighten monetary policy. In contrast, monetary policymakers were much less consistent in advocating for counteracting the likely impacts of tax changes on aggregate demand.

Our paper builds on and speaks to a range of literatures. Many papers examine the response of individuals to particular changes in income. Most find that as long as the changes are not large, individuals respond to them when they occur, even if they could have known about

them in advance or their impact on lifetime resources is small.<sup>2</sup>

Importantly, although this individual-level evidence is suggestive of a macroeconomic impact of changes in transfers, there could be offsetting forces at the aggregate level. For example, there could be Ricardian-equivalence effects: the adverse implications for lifetime wealth of the higher future taxes needed to finance the changes on transfers could exert a downward influence on all individuals' consumption. Likewise, there could be offsetting effects on aggregate consumption through higher interest rates, reduced confidence about government policy, or increased uncertainty about policy. Thus, a finding that when a payment arrives, individuals increase their consumption relative to individuals who do not receive a payment is not enough to establish that changes in transfers have important macroeconomic effects. It is therefore important to look directly at aggregate evidence.

Like us, Wilcox (1989) looks at the response of aggregate consumption to Social Security benefit increases. However, like the individual-level literature, his focus is narrowly on the permanent income hypothesis: since the benefit increases are announced in advance, the hypothesis implies that consumption should not respond to their implementation. He shows that over the period 1965–1985, permanent benefit increases have a statistically significant immediate impact on real retail sales and personal consumption expenditures. Our interest is with the macroeconomic effects of changes in transfers much more broadly. We therefore examine whether benefit increases were made in response to short-run macroeconomic developments, and omit the few that fall into this category from our analysis. In addition, we focus on the magnitude of the effects rather than just whether they are nonzero, examine whether the impact persists and whether it spreads to broader indicators of economic activity, and investigate the response of monetary policy. We also compare the impact of permanent and temporary Social Security benefit increases, and the effects of permanent benefit increases and tax changes.

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<sup>2</sup> See, for example, Agarwal, Liu, and Souleles (2007), Sahm, Shapiro, and Slemrod (2012), and Parker, Souleles, Johnson, and McClelland (2013).

Our paper is also related to recent work on the macroeconomic effects of changes in fiscal policy. These papers use both time-series evidence and cross-state variation.<sup>3</sup> While this literature has generally found a significant positive impact of fiscal expansion, the implied fiscal multipliers differ substantially in both size and timing. Our paper provides another estimate of the effect of fiscal policy, using a type of fiscal change whose timing is relatively exogenous and can be identified quite accurately.

Finally, much recent research has focused on the importance of monetary policy for the effects of fiscal policy (for example, International Monetary Fund, 2010, Christiano, Eichenbaum, and Rebelo, 2011, Woodford, 2011, and Nakamura and Steinsson, 2014). Our study provides both statistical and narrative evidence of a link between Social Security benefit increases and contractionary monetary policy, and of different monetary policy responses to changes in transfers and taxes.

The most important limitation of our study is simply that the amount of identifying variation that we are able to exploit is only moderate. Changes in Social Security benefits are small relative to the large changes in government purchases associated with major wars, and they are noticeably smaller than the tax changes that are the focus of Romer and Romer (2010). Our detailed information about the monthly timing of benefit changes allows us to pin down their effects in the very near term relatively precisely. But once we consider horizons beyond a few months, the limited amount of variation often yields confidence intervals that are wide enough to encompass a range of economically interesting hypotheses. Thus, this paper is only a first step in trying to understand the macroeconomic effects of government transfer payments.

Our analysis is organized as follows. Section II discusses our use of narrative sources to identify the motivation, timing, size, and nature of Social Security benefit changes. Section III

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<sup>3</sup> Among the papers using time-series evidence are Blanchard and Perotti (2002), Hall (2009), Fisher and Peters (2010), Romer and Romer (2010), Barro and Redlick (2011), and Ramey (2011). Among the papers using cross-state variation are Shoag (2010), Chodorow-Reich, Feiveson, Liscow, and Woolston (2012), and Nakamura and Steinsson (2014). Pennings (2014) finds that in response to Social Security benefit increases over the period 1968–1974, labor income rose more in states where Social Security benefits were larger relative to state income, suggesting an impact of the changes on local spending.

examines the response of consumption and other aggregate indicators to relatively exogenous benefit increases. Section IV compares the impact of Social Security benefit changes and tax changes. Section V investigates the response of monetary policy to transfer payments and tax changes using both statistical evidence and evidence from the records of the Federal Reserve. Finally, Section VI presents our conclusions and discusses the implications of our findings.

## **II. IDENTIFYING SOCIAL SECURITY BENEFIT CHANGES**

A central goal of the paper is to use Social Security benefit changes to examine how consumption and other macroeconomic variables respond to changes in transfer payments. Thus, a critical step is to identify changes in Social Security benefits that are useful for this purpose.

### **A. Motivation**

To understand our methodology, it is perhaps helpful to start by considering a straightforward alternative. The National Income and Product Accounts (NIPA) report monthly data on aggregate Social Security payments starting in January 1959. From this, one can easily calculate the change in benefits each month. Why not just use this series as the right-hand side variable in our regressions?

The most prosaic problem with this approach is that it misses the 1950s. Starting in the mid-1970s, Social Security benefits were indexed to inflation, and by the 1990s, benefit changes were small and quite regular. In contrast, in the 1950s, 1960s, and early 1970s, benefit changes were legislated; as a result, they varied greatly in size and timing. This variation makes the early postwar period a particularly promising period for estimating the effects of transfer payments. Losing the 1950s is therefore a serious drawback to using the NIPA data.

Another problem is that the NIPA series reflects both the number of beneficiaries and the size of benefits. Changes in Social Security payments resulting from changes in the number of

beneficiaries are likely to be correlated with other factors affecting the economy, such as demographic changes and endogenous retirement decisions. As a result, they cannot be used to provide reliable estimates of the macroeconomic effects of transfers.

The motivations for the changes can also introduce difficulties. In some cases, Social Security benefits were increased for explicitly countercyclical reasons. In such cases, one might not expect consumption to rise following the increases in benefits, because other factors (that is, whatever was causing the economy to be weak) were operating in the opposite direction. The NIPA series does not allow one to restrict attention to changes in benefits that were undertaken for reasons unrelated to the current or prospective short-run condition of the economy. As a result, using it could lead to estimates of the effects of transfers that are biased downward.

Finally, while most Social Security benefit changes have been intended as permanent, some have been explicitly temporary. For example, some permanent benefit increases have been retroactive for several months. In these cases, in the month of the increase beneficiaries received not only their higher regular monthly benefit, but also a one-time payment for the higher benefits in the preceding months. Many models of consumer behavior predict that permanent and temporary changes in income have very different impacts. For this reason, it is desirable to have a measure of benefit changes that separates permanent and temporary movements. The NIPA series does not do that, and so using it would force us to use imperfect statistical procedures to try to disentangle the two types of changes.

## **B. Methods Used for 1952–1974**

To obtain a measure of changes in Social Security benefits for the first part of the postwar period that is free of the problems we have described, we use the narrative record.

We identify the universe of possible legislated changes using a survey provided by the Congressional Research Service (2001). We exclude several types of actions: ones that affected payments to future beneficiaries relative to what they otherwise would have received, but that

did not directly raise or lower payments to existing beneficiaries; ones involving only small administrative changes; and ones that did not ultimately lead to the enactment of legislation.

For each substantive change, we look at a range of sources. The *Social Security Bulletin* typically has an article describing the specifics of the legislation and providing a detailed account of the Congressional debate. This article often provides the most comprehensive information about the size, timing, and permanence of the action (*Social Security Bulletin*, various issues). The reports of the House Ways and Means Committee and the Senate Finance Committee on the bill typically contain information about the motivation for the action as well as its size, though the final legislation often differs at least slightly from the versions analyzed in these reports (U.S. Congress, various years). The *Economic Report of the President* often discusses both the motivations for the actions and their sizes (U.S. Office of the President, various years). Finally, presidential speeches, particularly those made proposing the legislation or upon the signing of the final bill, are also useful sources (Woolley and Peters, *The American Presidency Project*).

We gather several pieces of information from these sources. We identify the size of the benefit change, measured as the change in spending at an annual rate. We include changes in both old age and disability benefits, since they are often combined in the discussions in our sources. We also include changes to Supplemental Security Income (SSI) benefits, which provide additional support for low-income seniors and disabled individuals. The narrative record makes clear which benefit changes were one-time payments and which were permanent. We also identify the months when Social Security checks reflected the benefit changes.<sup>4</sup>

Finally, we gather information on the motivations for the changes. The vast majority of changes were made either for equity reasons—to alleviate poverty among the elderly and disabled—or to allow benefits to keep up with inflation over the previous several years. A few,

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<sup>4</sup> The timing convention used in official discussions of Social Security is that if a benefit change is “effective” for a given month, it is reflected in the checks that are received early in the *following* month. We therefore date a change that is effective in a given month as taking place the following month. Social Security disability checks are received very late in the month for which they are effective. Since individuals would have had little time within the month to change their spending in response such changes in benefits, we again date these changes as occurring in the month after they become effective.

however, were explicitly undertaken for countercyclical purposes. Because these changes are likely correlated with other factors affecting the economy in the short run, we exclude these anti-recessionary changes from our analysis of the macroeconomic effects of the benefit changes.

A separate [appendix](#) provides a brief description of each legislated change in benefits and the key information about it.

### **C. Methods Used for 1975–1991**

Starting in 1975, Social Security benefits were indexed to inflation. Two features of these adjustments up through the early 1990s make them useful for estimating the effects of transfers. First, their timing varied: they occurred in July until 1982 and in January starting in 1984 (with no adjustment in 1983). Second, because inflation was so variable, there was substantial heterogeneity in the size of the adjustment. The adjustments ranged from 1.3 percent in January 1987 to 14.3 percent in July 1980. As a result, data from this period have the potential to provide considerable identifying variation.

By the 1990s, inflation was very low and the adjustments so regular that it seems unlikely that they greatly affected behavior. Moreover, their regular nature means that any impact on macroeconomic outcomes would probably have been obscured by the seasonal adjustment of the data.<sup>5</sup> For this reason, we only construct a series on these automatic benefit increases through December 1991.

Legislation played a very small role in benefit changes in the 1975–1991 period. The Social Security Amendments of 1983 were the source of the change in the timing of the automatic cost-of-living adjustments. A few laws, such as some disability reforms in the 1980s, affected coverage but did not change payments to existing beneficiaries. There were also some changes to future benefits that did not have any immediate effects (such as the provision of the 1983 amendments that gradually raised the retirement age). Since these changes did not raise (or

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<sup>5</sup> Because the Bureau of Economic Analysis obtains many of the component consumption series only in seasonally adjusted form, it does not construct seasonally unadjusted consumption data. Thus, it is not possible to examine the impact of the regular annual adjustments on seasonally unadjusted consumption.

lower) disposable income significantly at a specific point, we omit them from our analysis.

Although the automatic cost-of-living adjustments were the main source of changes in Social Security benefits in this period, there were also some one-time payments whose timing was effectively random. In particular, there were one-time retroactive payments at various dates based on legal decisions, revisions to case review procedures, and, in one case, the purchase of new computers that sped the processing of appeals. We identify these one-time payments by conducting Google news searches using the terms “Social Security” and “personal income,” and “Social Security” and “retroactive.” In addition to identifying the cost-of-living adjustments (which we are able to find more directly using official documents), these searches find a number of articles about one-time payments.

Because the changes in this period were not legislated, for the most part their sizes are not reported in our sources. Thus, our methods of estimating sizes differ from those we use for the earlier period. For the cost-of-living adjustments, we simply multiply total Social Security payments (as reported in the NIPA data) in the month before the increase by the percentage adjustment. This procedure holds enrollment fixed, and so shows just the increase in payments coming from the increase in average payments per beneficiary.<sup>6</sup>

In the case of the one-time payments, occasionally the news stories discuss the size of a change, but often they do not. To estimate the size of a payment, we therefore take the increase in the NIPA Social Security series in the month for which our news stories identify a payment. Since the usual month-to-month changes in this series are small, most of the changes in the months of substantial one-time payments are likely the result of the payments. Consistent with this interpretation, the estimates based on this approach correlate closely with the figures in the

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<sup>6</sup> Our estimates of changes in benefits for the years before 1975 include changes in SSI payments as well as Social Security. While Social Security payments as defined in the NIPA data include payments from the Disability Insurance Trust Fund, they do not include SSI payments (which are grouped with miscellaneous government transfers in the “Other” category). For this reason, our estimates of the benefit changes from automatic cost-of-living adjustments are not precisely comparable to our estimates for the period before 1975. However, SSI payments are quite small, so this difference is unlikely to be consequential.

news articles in the few cases where the articles report the sizes of the one-time payments. Also, the increases are generally followed by decreases in the NIPA series of roughly the same magnitude the following month, suggesting that the movements were indeed the result of one-time payments.<sup>7</sup>

We classify the automatic cost-of-living increases as permanent and the various one-time payments as temporary. The [appendix](#) provides additional details about the cost-of-living increases and lists the sources of the articles about the one-time payments. Table 1 presents the data for the full 1952–1991 period. They are reported as the dollar change as a percent of aggregate personal income.<sup>8</sup>

#### **D. New Series of Social Security Benefit Increases**

Figure 1 shows our series of Social Security benefit increases, expressed as a percent of personal income. Permanent and temporary changes are shown separately. The change in the monthly NIPA series for Social Security transfers (also expressed as a share of personal income), which begins in 1959, is also shown for comparison.<sup>9</sup>

One fact evident from the graph is that our series and the NIPA series are closely related. The sum of the permanent and temporary increases based on our narrative analysis matches the increases in the NIPA series fairly closely. There are some moderate short-run fluctuations in the NIPA series in the late 1970s and the 1980s that have no counterpart in our series. Whether they reflect one-time payments that were not large enough to be newsworthy or other factors is not clear. In addition, there are many small month-to-month movements in the NIPA series

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<sup>7</sup> The pattern is more complicated when the one-time payments were spread over two months (which occurred in November–December 1983), or when they were immediately followed by an automatic cost-of-living increase (which occurred in December 1983 and December 1984). But the behavior of Social Security payments in these episodes is consistent with the view that the increases in November–December 1983 and December 1984 reflected one-time payments.

<sup>8</sup> The monthly data on personal income are from the Bureau of Economic Analysis, National Income and Product Accounts (NIPA), Table 2.6, [http://www.bea.gov/iTable/index\\_nipa.cfm](http://www.bea.gov/iTable/index_nipa.cfm), downloaded 1/23/2014. For the years before 1959, we use the quarterly personal income figures (from Table 2.1, downloaded 1/23/2014) for each month of the quarter.

<sup>9</sup> The monthly NIPA Social Security data are from the Bureau of Economic Analysis, NIPA, Table 2.6, series for government social benefits to persons—Social Security, downloaded 1/23/2014.

that have no counterpart in our series. At least in part, these movements reflect changes in the number of individuals choosing to enroll in Social Security rather than changes in benefits. There are also a few changes in the NIPA series that we exclude from our series because they were motivated by countercyclical considerations.<sup>10</sup>

The figure also shows several characteristics of the new series. One is that the timing of benefit changes was highly uneven, particularly before 1975. This adds credence to the notion that there is substantial usable variation to exploit. At the same time, the size of the permanent benefit changes varied within a somewhat narrow range. The largest permanent benefit increase was less than 1 percent of aggregate personal income.

In contrast to the fairly modest variation in permanent benefit increases, Figure 1 shows that some temporary benefit increases were quite large. The three largest one-time payments (in 1965, 1970, and 1971) were each between 1 and 2 percent of annual personal income. And most of the later one-time payments, though not as large relative to aggregate personal income, were very large for those receiving them. Our news stories provide figures for the average payment per recipient for three of these one-time payments: those in November–December 1983, December 1984, and July 1986. In 2013 dollars, these payments averaged \$2301 per recipient in 1983, \$1060 in 1984, and \$564 in 1986.

## **E. Identification**

Our goal is to investigate the response of consumer spending and other macroeconomic variables to changes in transfers. It is therefore useful to think about possible identification issues related to using our new series for this purpose. The obvious concern is that there could be factors, such as the cyclical state of the economy, that affect both macroeconomic outcomes and legislated changes in Social Security benefits.

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<sup>10</sup> The NIPA series also often shows a large negative change in the month following a large temporary increase that does not show up explicitly in our series. This just reflects our measurement convention: we record a one-time payment in a single month as a positive value for that month and zero in the next month, rather than as a positive value in that month and an equal and opposite negative value in the next.

As we have described, we take several steps in the construction of our series to minimize such omitted variable bias. We analyze the motivation for legislated changes and screen out those that had an explicit countercyclical purpose. We also focus as much as possible on changes in benefits for existing beneficiaries rather than changes in the number of beneficiaries. Finally, we exclude the period after 1991, when the benefit increases are so small and regular that they could become part of the usual seasonal adjustment factors. However, some issues remain. Three appear particularly important.

**Inflation.** While benefit increases taken for equity reasons are clearly appropriate for our purposes, what about the many changes to keep up with inflation? Since inflation responds to the state of the economy, one might think there could be correlation between benefit increases to keep up with inflation and other factors affecting macroeconomic outcomes.

Although this possibility could be relevant to studies of some relationships, it is unlikely to be problematic for our analysis. Before 1974, the adjustments of benefits to inflation were ad hoc and irregularly spaced. After the adoption of indexing, adjustments still occurred at discrete intervals. Even if the state of the economy was positively affecting Social Security benefits through effects on inflation, one would not expect this omitted variable to cause a sharp rise in consumption in the particular month of the inflation adjustment. Nevertheless, in some of our empirical specifications, we control for lagged consumption growth as a way to ensure that other factors leading to serially correlated changes in consumption growth are not causing spurious results. Likewise, although we see no plausible reason that indexation to inflation at discrete intervals could introduce significant bias into our estimation, for completeness we also consider specifications that include inflation itself as a control variable.

**Social Security Taxes.** Because the Social Security program is explicitly self-financed, legislation increasing benefits has often included provisions raising payroll taxes. For example, the Social Security Amendments of 1954, which increased benefits starting in October of that year, legislated an increase in the Social Security tax base in 1955 and increases in the Social

Security tax rate in 1970 and 1975. The coupling of benefit increases with higher taxes means that there could be an omitted variable (the tax increases) that obscures the effects the benefit increases would have in isolation.

In previous work (Romer and Romer, 2010), we identified these spending-driven Social Security tax changes from the same types of narrative sources described above. These immediate tax increases typically followed the benefit increases by at least a few months. Thus, the tax changes are unlikely to pose a major problem for our analysis, especially when we consider the very short-run effects of benefit increases. And, because we have data on the timing and size of these tax changes, we can consider specifications that control for them.

**Other Fiscal Policy Actions.** Another concern involves the possibility that Social Security benefit increases tended to be made at the times of other expansionary fiscal actions. Our narrative analysis of the history of the benefit increases, however, suggests that this is not the case. Rather, most were self-contained actions, not parts of broader programs of fiscal expansion. This pattern is extremely clear for the second part of the sample, when benefit increases were almost entirely the result of automatic cost-of-living adjustments, and for the 1950s, when Social Security legislation was considered essentially in isolation. But it also appears to be an accurate description of most of the changes in the 1960s and early 1970s. In addition, we explicitly exclude the increases that were parts of countercyclical stimulus packages, such as the one-time payments to seniors in the Tax Reduction Act of 1975.

As a further check on possible confounding effects from other fiscal actions, we include general (relatively exogenous) tax changes as a control variable in some specifications. We create this series using our previous narrative analysis of postwar tax changes.

Explicitly controlling for other changes on the spending side of fiscal policy is harder. Monthly data on government purchases are not available, so there is no obvious control variable to include. However, a first look at the data suggests little reason for concern. In quarterly data, the correlation between our measure of permanent changes in Social Security benefits and the

growth rate of real federal government purchases is  $-0.04$ ; its correlation with the growth rate of all of real federal government spending excluding Social Security benefits is  $-0.05$ ; and its correlation with the measure of shocks to government spending developed by Ramey (2011) is  $-0.01$ .<sup>11</sup>

**Other Concerns.** In addition to omitted variable bias, the other natural concern is accidental correlation in small samples. Perhaps benefit increases happen to occur at the same times that other forces are affecting the economy in one direction or another. To deal with this possibility, we will consider specifications that control for a range of other factors that could affect macroeconomic outcomes.

### **III. THE EFFECTS OF CHANGES IN SOCIAL SECURITY BENEFITS ON MACROECONOMIC OUTCOMES**

The next step is to use the series on Social Security benefit increases to investigate how changes in transfer payments affect the macroeconomy.

#### **A. Outcome Variables and Sample Periods**

**Outcome Variables.** The main outcome variable we consider is real personal consumption expenditures.<sup>12</sup> There are two main advantages of focusing on consumption. First, because changes in Social Security benefits affect households' disposable income directly, any macroeconomic effects might occur more quickly and sharply in consumption than in other aggregate variables. Second, consumption data are available monthly, which allows us to use

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<sup>11</sup> For our measure, we use the permanent Social Security benefit increases, expressed as a percent of personal income. Because the other fiscal indicators are quarterly, we sum the monthly values over the quarter to create a quarterly series. The growth rate of federal government purchases is from the NIPA, Table 1.1.1, series for government consumption expenditures and gross investment, downloaded 1/23/2014. Real federal government spending excluding Social Security benefits is calculated by taking federal current expenditures (NIPA, Table 3.2), subtracting government social benefits to persons [for] Social Security (NIPA, Table 2.1), and dividing by the price index for GDP (NIPA, Table 1.1.4), all downloaded 1/23/2014. We then calculate the difference in logarithms. The Ramey series on government spending news shocks as a share of GDP is from column C of Ramey\_Govt\_Public\_Data.xls, <http://www.econ.ucsd.edu/~vramey/research.html#data>, "Data for Identifying Government Spending Shocks," Summary Data, U.S., 1939-2008, downloaded 7/31/2013.

<sup>12</sup> The data are from the Bureau of Economic Analysis, NIPA, Table 2.8.3, series for personal consumption expenditures, downloaded 1/23/2014.

information about the exact timing of benefit changes more effectively than we could with lower-frequency data.

One drawback of the monthly consumption series is that the data are not available before 1959. However, both quarterly data on real consumption and monthly data on real retail sales (which generally move fairly closely with consumption) are available for the earlier period. We therefore construct monthly consumption data for the period before 1959 using a Chow-Lin procedure.<sup>13</sup>

We consider three other aggregate outcome series: real retail sales, industrial production, and employment. All three are available monthly beginning before 1950.<sup>14</sup> Retail sales are more volatile than consumption but capture a similar aspect of the economy. In contrast, industrial production and employment are broader indicators of economic activity, and so may respond differently to increases in Social Security benefits.

**Sample Periods.** Our baseline sample period is 1952–1991. Starting the sample in 1952 avoids the period of extreme macroeconomic volatility associated with the outbreak of the Korean War. And as described above, ending the sample in 1991 means that we exclude the period when benefit increases consisted of modest, relatively stable cost-of-living increases every January.

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<sup>13</sup> The data on retail sales, adjusted for seasonal variation, for 1947:1–1958:12 are from the U.S. Department of Commerce, *Business Statistics, 1979*, p. 216. We convert it to a real series by dividing by the seasonally adjusted consumer price index for all urban consumers: all items less shelter, Bureau of Labor Statistics, series CUSR0000SAOL2, downloaded from Federal Reserve Economic Data (FRED) <http://research.stlouisfed.org/fred2/>, 1/23/2014. To create an estimate of monthly consumption, we use the Chow-Lin algorithm in RATS, which employs the variant of the Chow-Lin procedure proposed by Fernandez (1981). We estimate the algorithm over the period 1947–1958. The results are similar for this decade when we run the Chow-Lin procedure over the full sample 1947:1–1991:12.

<sup>14</sup> The real retail sales series for 1947–1991 is constructed by taking nominal, seasonally adjusted data for 1967:1–1991:12 from U.S. Department of Commerce, *Business Statistics, 1991*, p. A-56 and p. 37; for 1961:1–1966:12 from *Business Statistics, 1984*, p. 177; and for 1947:1–1960:12 from *Business Statistics, 1979*, p. 216. The series, which do not line up exactly because of data revisions, are combined using a ratio splice—starting with the most recent series and working backwards. The data are converted to real values by dividing by the consumer price index for all urban consumers: all items less shelter. The industrial production series is the total index, seasonally adjusted, Board of Governors of the Federal Reserve System, series INDPRO, downloaded from FRED, 1/24/2014. The employment series is total nonfarm employees, seasonally adjusted, Bureau of Labor Statistics, series PAYEMS, downloaded from FRED, 1/24/2104.

We consider two variants on the baseline sample. The first starts in 1959, and so excludes the period for which we have only estimated consumption data.<sup>15</sup> The second ends in 1974, and so excludes the period when benefit changes were largely the result of automatic cost-of-living adjustments.

## **B. Specifications**

As discussed above, our approach to identifying Social Security benefit changes implies that there should not be systematic correlation between the changes and other factors affecting macroeconomic outcomes. Thus, it is appropriate to examine how macroeconomic variables behave in the wake of the benefit increases without controlling for other factors.<sup>16</sup>

Our baseline specification is therefore a regression of an outcome variable on the contemporaneous and lagged values of our measures of increases in Social Security benefits, with no controls. Since permanent and temporary benefit changes have been quite different in character and might have different effects, we enter them separately. Specifically, the baseline specification takes the form,

$$(1) \quad Y_t = a + \sum_{i=0}^N b_i^{PERM} SS_{t-i}^{PERM} + \sum_{i=0}^N b_i^{TEMP} SS_{t-i}^{TEMP} + e_i.$$

Here,  $Y$  is an outcome variable—for example, the growth rate of real personal consumption expenditures.  $SS^{PERM}$  and  $SS^{TEMP}$  are permanent and temporary increases in Social Security benefits, both measured as a fraction of personal income.  $N$  is the number of lags.

We also consider more complicated specifications. We include various control variables to address specific concerns about omitted variable bias, and as a check for the possibility of accidental correlation between benefit increases and other factors affecting outcomes. We also

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<sup>15</sup> Specifically, since our regressions use the change in consumption, this sample period starts in 1959:2.

<sup>16</sup> Two additional considerations make this argument even more compelling for very short horizons, such as a few months. First, as we have discussed, the exact timing of the benefit changes we consider appears to be largely the result of idiosyncratic factors. Second, in the cases where other fiscal actions were taken in conjunction with the benefit changes (such as increases in Social Security taxes to help finance higher benefits), they were almost always separated from the benefit changes by at least several months.

include lags of the outcome variable, which controls for the usual dynamics of the series and provides a simple way of capturing the effects of any serially correlated omitted variables.

The regressions including lagged values of the outcome variable can be interpreted as simple (that is, univariate) VARs with the Social Security benefit changes treated as exogenous. There are several reasons not to treat them as endogenous. First, the purpose of our narrative work is to identify benefit changes that were not responses to recent or prospective macroeconomic developments. Second, and more importantly, a finding that the benefit changes were typically preceded by systematic movements in macroeconomic variables could reflect reverse causation rather than an endogenous component of the benefit changes. Most obviously, news of coming benefit increases could cause consumption to rise before the increases took effect. Finally, empirically, we find no evidence that benefit changes are predictable on the basis of macroeconomic variables.<sup>17</sup>

Thus, we do not endogenize Social Security benefits in a VAR framework. To test for the possibility of anticipatory responses to benefit changes, in some specifications we include leads as well as lags of the benefits variables.

### **C. Results**

We now turn to the findings. We begin with the simplest specification over the full sample period, and then consider variants.

**Baseline Results.** Figure 2 shows the results of estimating equation (1) using the change in the logarithm of real personal consumption expenditures as the dependent variable over the sample period 1952:1–1991:12, with 12 lags of the right-hand side variables.<sup>18</sup> It shows the

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<sup>17</sup> We perform Granger-causality tests for our series and industrial production, employment, real retail sales, and personal consumption expenditures (PCE). Specifically, we regress our series of permanent Social Security benefit increases (as a percent of personal income) on 12 own lags and 12 lags of the log difference of the relevant macro outcome variable. The F-statistic that the coefficients on the lagged macro variables are all zero has a p-value of 0.78 for industrial production; 0.64 for employment; 0.24 for retail sales; and 0.08 for PCE. For PCE, the near significance is not the result of the short lags, but rather of significant coefficients on the 10<sup>th</sup> and 12<sup>th</sup> lags.

<sup>18</sup> We have examined the narrative record for 1951 and found no permanent or temporary benefit changes

estimated responses of consumption (in logs) to both temporary (one-month) and permanent increases in Social Security benefits of 1 percent of personal income, together with the two-standard-error bands.

The most striking result is the large, immediate response of consumption to a permanent increase in benefits. The point estimates suggest that a benefit increase of 1 percent of personal income raises consumption by 1.2 percent in the month it occurs, and that the effect persists for the next 5 months. The null hypothesis of no effect in the month of the increase is rejected with a  $t$ -statistic of 2.8. As detailed below, this result is very robust.

The standard errors rise as the horizon lengthens. As a result, 5 months after the benefit increase, the point estimate remains large (1.0) but is no longer statistically significant ( $t = 0.9$ ). Thereafter, the estimated effect declines. However, the estimates are sufficiently imprecise that it is not possible to reject either the hypothesis that the effect remains one-for-one or the hypothesis that it returns to zero.

The figure shows that the response to a temporary benefit increase appears considerably weaker. The estimated impact in the month of the increase is only 0.1 ( $t = 0.5$ ). The estimates remain small for several months after the temporary payment. Thereafter they rise considerably, but the standard errors are sufficiently large that the possibility that this pattern is just statistical noise cannot be rejected.<sup>19</sup>

That the results are so different for permanent and temporary Social Security benefit changes suggests that it is important to consider the two types of changes separately. This can

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in this year. We therefore code the two series as zero for the twelve months of 1951, and lose no observations because of the 12 lags.

<sup>19</sup> Two considerations suggest that the large point estimates for the effects of temporary payments at longer horizons likely reflect sampling error rather than true effects of the payments. First, it is hard to think of a plausible mechanism that would cause households to raise their spending greatly 6 or 12 months after receiving a one-time payment. Second, closer examination of the data shows that the substantial estimated response at moderate horizons is largely the result of a few observations. For example, there was a sharp rise in consumption in early 1972, which followed a large retroactive increase in Social Security benefits in June 1971. Conventional accounts of this period attribute the rise to a cut in the excise tax on autos and abundant credit, not to the earlier one-time payment of Social Security benefits (see, for example, *Economic Report of the President*, 1973, p. 23).

only be done with the series derived from the narrative sources. When one uses the change in the monthly NIPA series on Social Security benefit payments, or our new series with the permanent and temporary changes merged into a single right-hand-side variable, the results are a blend of the estimated effects for permanent and temporary changes, with large standard errors.<sup>20</sup>

**Robustness.** The results of the basic regression are very robust. Considering the two alternative sample periods (1959–1991 and 1952–1974) has little impact on the estimated effects of permanent and temporary benefit increases. Using only the pre-1974 sample raises the estimated effects of permanent benefit increases on consumption noticeably, while considering only the post-1959 sample reduces them slightly. In both samples, the initial impact on consumption remains highly statistically significant.

Likewise, adding 12 lags of consumption growth to (1) has little effect. Both the point estimates and the standard errors of the effects of permanent benefit increases on consumption are reduced trivially. The impact of temporary benefit increases is similarly unaffected.

Many studies have found that consumer confidence is a strong predictor of consumption growth (for example, Carroll, Fuhrer, and Wilcox, 1994). We therefore consider a variant of equation (1) that includes the contemporaneous value and 12 lags of the change in the Conference Board index of consumer confidence.<sup>21</sup> To the extent that Social Security benefits affect consumption *through* consumer confidence, controlling for confidence could cause us to understate the overall effects of benefit changes. Again, however, this change has little effect on the results.

We consider a range of other control variables. If the inflation measure to which Social Security benefits are indexed were correlated with other determinants of consumption, it would

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<sup>20</sup> To merge our series for permanent and temporary benefit changes into a single consistent series, it is necessary to express the temporary changes as a positive value in the month they occur, and an equal and opposite value in the subsequent month.

<sup>21</sup> The data are from the Conference Board, <http://www.conference-board.org/ea/index.cfm>, downloaded 7/23/2012. Because the data are only available beginning in 1959, the sample period is 1960:2–1991:12.

help predict consumption growth in all periods, not just periods when Social Security benefits are adjusted for inflation. We therefore include the contemporaneous value and 12 lags of this inflation measure.<sup>22</sup> Including inflation has little impact on the results, and in fact strengthens them slightly. The point estimates for the effect of a permanent benefit increase are now positive at almost all horizons, but the standard errors at longer horizons remain very large.

We do not want to control for all movements in monetary policy, since the response of monetary policy may affect how the benefit changes affect the economy (an issue we investigate in depth in Section V). It is reasonable, however, to control for unusual changes in monetary policy, some of which could coincidentally occur around the time of benefit increases. We therefore control for the contemporaneous value and 24 lags of the dummy variable for contractionary monetary policy shocks constructed by Romer and Romer (1989, 1994). As with adding inflation, including this variable strengthens the findings slightly.

Finally, we find no evidence that consumption responds in advance of higher benefit payments. There is typically a lag of 2 to 4 months from the enactment of legislation to the actual increases in benefits. But when we include 3 leads of benefit changes, the coefficients on the leads are never close to statistically significant, and they are more often negative than positive.

Figure 3 shows what is driving our finding concerning the short-run effects of permanent benefit increases. It is a scatter plot of the partial association of real consumption growth against the contemporaneous permanent change in Social Security benefits as a share of personal income. Specifically, it shows the residuals from regressions of both series on all of the other right-hand-side variables in the baseline specification over the period 1952:1–1991:12. The figure shows a clear, though not overwhelming, upward-sloping relationship. It also shows that there is no single observation driving the results.

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<sup>22</sup> This measure for month  $t$  is CPI inflation over the four-quarter period ending in month  $t - 4$ . The data are from the Bureau of Labor Statistics, <http://www.bls.gov/data/>, series CWUR0000SAO, downloaded 9/14/2013.

### **Understanding the Different Effects of Permanent and Temporary Changes.**

An obvious question is why the effects of permanent and temporary changes in benefits appear to be so different. One possibility involves the sizes of the changes. A common finding in previous work on consumption is that households tend to behave as rule-of-thumb or Keynesian consumers in response to small changes in income, but to follow the predictions of the permanent income hypothesis more closely for large changes (for example, Hsieh, 2003). In our case, the permanent benefit changes we consider are generally small, while the changes that provide the bulk of the identification for temporary changes are large.

The biggest permanent benefit change in our sample is a 20 percent increase in individuals' benefits in October 1972, and only a few of the permanent changes exceed 10 percent. In contrast, the retroactive across-the-board increases in September 1965, April 1970, and June 1971 were all 30 percent or more of individuals' normal monthly benefits. And all three payments were coupled with increases in permanent benefits, so that the total Social Security payments beneficiaries received in the month exceeded their previous monthly benefits by 45 percent or more. In addition, as described in Section II, the various one-time payments in the 1980s were often substantial for the beneficiaries who received them. Thus, our finding that the temporary benefit changes in our sample period for the most part did not lead to large immediate changes in consumption is consistent with previous evidence about consumer behavior.

#### **D. Other Outcome Variables**

We now turn to an analysis of three other monthly measures of macroeconomic outcomes: real retail sales, industrial production, and employment. Table 2 shows the cumulative response of each variable to a permanent benefit increase of 1 percent of personal income. The table also repeats the cumulative response of personal consumption expenditures (PCE) for comparison.

For retail sales, the point estimates suggest a somewhat larger impact of Social Security

benefits than they do for consumption. For example, the estimated effect of a permanent benefit increase of 1 percent of personal income is a rise in retail sales of 1.7 percent in the month it occurs, and a peak increase of 2.1 percent after 4 months. The standard errors, however, are also larger. The *t*-statistic on the contemporaneous effect is 1.7, and that on the maximum effect is 1.0. All of this is consistent with the fact that retail sales are more cyclically sensitive and more volatile than overall consumption.

The point estimates also suggest a nontrivial impact on industrial production. The estimated peak effect is 0.7 percent 3 months after a permanent benefit increase. The dominant feature of the estimates, however, is their imprecision. The *t*-statistics for the estimated positive effects never exceed 1, and the estimated impact turns sharply (but insignificantly) negative after 6 months.

Finally, there is no evidence of an employment response. The point estimates differ trivially from zero for 5 months before turning moderately negative. The hypothesis that the effect is zero cannot be rejected at any horizon.

#### **IV. BENEFIT INCREASES AND TAX CHANGES**

It is useful to extend our previous analysis to include tax changes for at least two reasons. The narrow one is that, as we have described, some benefit increases we consider were paired with Social Security tax increases that occurred at about the same time. If these tax increases had a direct negative effect on consumption, the fact that they are not included in our baseline specification could cause our estimates to understate the effects of the benefit increases.

The broader reason for expanding the analysis is to compare the impacts of taxes and transfers. In very simple Keynesian models, taxes and transfers have equal and opposite effects. Even more sophisticated models tend to imply that the effects are broadly inverse, as long as the incidence and incentive effects are not extremely different. A direct comparison of the estimated effects of taxes and transfers can see if this is the case. To the degree that it is not, the

comparison can suggest possible explanations and directions for further study.

### **A. Data and Specifications**

The tax measure we use is a variant of the one developed in Romer and Romer (2010). In particular, our measure here is the sum of the tax changes that are the focus of that paper—legislated tax changes taken for long-run reasons or to reduce an inherited budget deficit—and legislated changes to finance a roughly contemporaneous increase in Social Security benefits.<sup>23</sup> In the earlier paper, we argue that the first set of tax changes should not be systematically correlated with other factors affecting macroeconomic developments in the short run. And once we control for Social Security benefit increases, the tax increases intended to help finance them should also be uncorrelated with other factors affecting the macroeconomy.

Because our focus here is on consumer behavior and the comparison to Social Security benefit increases, we exclude tax actions that only affected businesses. For example, we exclude the large investment tax credit legislated in the Revenue Act of 1962. We do, however, include any tax action that involved a substantial change in personal income, payroll, or excise taxes, even if some business taxes were also changed by the action.<sup>24</sup> To facilitate the comparison of tax and transfer changes, we follow the convention of expressing tax cuts as positive and tax increases as negative.

One limitation of the tax measure is that it does not separate permanent and temporary tax changes. However, most tax changes in the postwar period that were explicitly temporary were

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<sup>23</sup> Specifically, our measure consists of the “long-run” and “deficit-driven” tax increases from our earlier paper plus the “spending-driven” Social Security tax increases in 1951:1, 1955:1, 1957:1, 1959:1, 1966:1, 1968:1, 1969:1, 1972:1, 1973:1, and 1974:1. We exclude the spending-driven tax increase related to the Social Security Amendments of 1961 because that benefit increase was countercyclical, and so is excluded from the analysis. The size of the tax changes is measured using the revenue estimates in Romer and Romer (2010). For comparability with our measures of benefit changes, we measure the changes as a fraction of personal income. We assign the tax changes to specific months in the same way we assign them to specific quarters in our earlier paper. A tax change is assigned to the month it took effect unless the change occurred after the middle of the month; in that case, it is assigned to the following month.

<sup>24</sup> The particular long-run and deficit-driven tax changes in our sample period identified in Romer and Romer (2010) that we exclude (and their magnitudes, in billions of dollars) are: July 1958 (-0.5); July 1962 (-1.35); November 1962 (-0.9); January 1963 (0.6); June 1967 (-1.6); January 1971 (-2.8); April 1980 (8.2); January 1981 (4.1); January 1982 (4.1); January 1983 (26.4); August 1984 (8.0); and January 1988 (10.8).

adopted for countercyclical purposes, and so are not included in our measure. As a result, the vast majority of the tax changes in our measure are permanent.

Figure 4 shows our series for permanent Social Security benefit increases together with our series for tax changes. The figure makes clear that there is not a simple correlation between benefit increases and tax changes. For example, it is not the case that there is always (or even often) a tax increase in close proximity to the benefit increases. Similarly, benefit increases do not seem to occur systematically around the same time as tax-based fiscal expansions.

To expand the empirical analysis, we estimate equation (1) described earlier including the tax variable. Since our earlier paper finds substantial lags in the effects of tax changes, we include the contemporaneous value and 24 lags of the tax measure.

## **B. Results**

Controlling for tax changes has essentially no effect on the estimated impact of a permanent increase in Social Security benefits on consumption. At medium horizons, the impact is slightly larger when tax changes are included: for example, after 7 months, the impact on consumption of a benefit increase of 1 percent of personal income is 0.02 percent not controlling for taxes and 0.28 controlling for taxes. The difference is in the direction one would expect, but small both in absolute terms and relative to the standard errors. Thus, the results suggest that excluding tax changes from our previous analysis introduced relatively little omitted variable bias.<sup>25</sup>

Figure 5 displays the estimated cumulative responses of consumption to a permanent increase in Social Security benefits and to a tax cut of 1 percent of personal income implied by the regression including both types of changes (and temporary benefit changes as well). The estimated responses are noticeably different. Whereas the effect of a permanent benefit

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<sup>25</sup> Including the tax variable reduces the impact of temporary Social Security benefit changes, particularly at longer horizons. The standard-error bands, however, remain very large. That including the tax variable affects these estimates noticeably at long horizons is consistent with the view discussed previously that those point estimates were being driven by accidental correlation with other factors, such as the automobile excise tax cut in 1971.

increase is strong and immediate, that of a tax cut is much slower. The hypothesis that the contemporaneous responses are the same is rejected at the 1 percent level. At the same time, while the impact of a benefit increase falls after five months and becomes small and imprecisely estimated, that of a tax cut rises steadily and remains highly significant.

The impact of tax changes in this expanded regression is very similar to the estimates in Romer and Romer (2010). The expanded regression includes 24 lags of the tax changes, and so it is possible to carry the cumulative response out for two years. The maximum cumulative impact of a tax cut of 1 percent of personal income on consumption is a rise of 1.9 percent ( $t = 2.8$ ) after 22 months.<sup>26</sup>

Another major difference between benefit changes and tax changes is that the effects of the tax changes also show up in broader measures of economic activity. Whereas benefit increases have economically small and statistically insignificant effects on employment and industrial production, tax changes have large and significant impacts. For example, following a tax cut of 1 percent of personal income, industrial production rises 2.6 percent after 12 months ( $t = 2.5$ ), and the effect continues to increase in both magnitude and significance for at least two years.

### **C. Understanding the Differences**

The comparison of the macroeconomic impacts of benefit increases and tax changes reveals at least three important differences: the initial impact of benefit increases is larger; the impact dies out for benefit increases while it rises steadily for tax cuts; and the overall impact is much larger for tax cuts than for benefit increases. One way to try to make progress in understanding these differences is to look at disaggregated consumer spending data.

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<sup>26</sup> As described above, the tax series used in this analysis excludes tax changes affecting only businesses. When those business tax changes are included as well, the impact of Social Security benefit changes on consumption in the regression including both benefit changes and tax changes is essentially unchanged. The estimated impact of tax changes is slightly larger and more precisely estimated.

In addition, though our baseline specification includes only 12 lags of the Social Security benefit changes, we also try including 24 lags of both permanent and temporary changes, along with the tax variable. The response of consumption to a permanent benefit increase becomes quite large and negative at long horizons, with very large standard errors.

In particular, we look separately at consumer spending on durable and nondurable goods.<sup>27</sup> We regress each category of consumer spending on the contemporaneous value and 12 lags of permanent Social Security benefit increases, the contemporaneous value and 12 lags of temporary benefit increases, and the contemporaneous value and 24 lags of our measure of tax changes.

Panel a of Figure 6 shows the cumulative responses of durables consumption to a permanent increase in Social Security benefits and to a tax cut of 1 percent of personal income from the regression for the full sample period (1952–1991). Panel b shows the two cumulative responses for nondurables consumption. The first fact that is obvious from the figure is that for both benefit increases and tax cuts, the response of durables consumption drives much of the response of overall consumption. Nondurables consumption responds only slightly to a benefit increase, and the impact is never statistically significant. For a tax cut, the estimates suggest a moderate rise in nondurables consumption after about four months, but the response is far smaller than that of durables consumption, and the hypothesis of no effect cannot be rejected.

The large initial response of durables consumption to a permanent increase in Social Security benefits is quite consistent with the individual-level tests of the permanent income hypothesis. Studies such as Parker, Souleles, Johnson, and McClelland (2013) find that households tend to buy durables, particularly autos, in response to a rise in income, and that the response is often rapid and substantial.

In light of these findings, the slow response of spending to tax cuts is puzzling. Some of

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<sup>27</sup> The monthly disaggregate consumer spending data are from the Bureau of Economic Analysis, NIPA, Table 2.8.3, series for personal consumption expenditures of durable goods and nondurable goods, downloaded 1/23/2014.

As with total consumer spending, the monthly data do not begin until 1959. We therefore create monthly data for the period 1952:1–1958:12 using a Chow-Lin procedure and monthly data on retail sales of durables and nondurables. In particular, we interpolate quarterly, seasonally adjusted real consumer spending on durables and nondurables by the corresponding monthly data on real retail sales, also adjusted for seasonal variation. The quarterly disaggregate consumption data are from the Bureau of Economic Analysis, NIPA, Table 2.3.3, series for personal consumption expenditures of durable goods and nondurable goods, downloaded 1/23/2014. The monthly disaggregate retail sales data are from the U.S. Department of Commerce, *Business Statistics, 1979*, pp. 216–217. Because the CPI for durables and nondurables is not available until 1967, we deflate both retail sales series by the CPI for all goods less shelter. We estimate the Chow-Lin algorithm over the period 1947–1958.

this lag may be due to the fact that in Romer and Romer (2010) we date tax changes when they took effect, or when they were passed if they were retroactive. But, when a tax bill is passed or effective may be substantially before it shows up in take-home pay. With the Social Security benefit increases, we date the change in the month when the larger check first arrives. To see if this difference in dating conventions is important, we adjust the timing of the two major tax changes where the change in liabilities and the change in withholding are most different (the 1964 tax cut and the Reagan tax cut) to more closely reflect the change in withholding. This adjustment increases the contemporaneous impact of the tax change on consumption from  $-0.04$  ( $t = -0.23$ ) to  $0.18$  ( $t = 1.21$ ). Thus, differences between when tax changes affected tax liabilities and when they affected take-home pay appear to account for some, but far from all, of our finding of a slow response of spending to tax changes.

Other factors may also play a role in explaining the different initial impacts of benefit increases and tax changes. It is possible that households take more notice of a higher benefit check than of lower tax withholding, and so benefit recipients may respond faster and more strongly. Social Security beneficiaries may feel more liquidity constrained on average than other households, or follow rules of thumb that depend more strongly on current income, and so respond more quickly. Or, more generally, Social Security recipients may simply have a different marginal propensity to consume and speed of adjustment than tax cut recipients.

The fact that the durables consumption response wanes quickly for benefit changes and is smaller overall than that for tax cuts is another puzzle. Faced only with the behavior of consumption following benefit changes, one might be tempted to tell a simple stock-adjustment story. In response to the rise in benefits, household increase durables purchases only for a short period, but then have higher flow consumption as they use the new car or other durable good over time.

However, the fact that consumer spending on durables (and to a lesser degree nondurables) rises consistently for the 24 months following a tax cut suggests this stock

adjustment mechanism cannot be the full explanation. In the case of tax cuts, there is gradual adjustment or a multiplier effect that keeps consumer spending high for an extended period. This is consistent with the fact that tax changes have strong and significant impacts on broad measures of economic activity, such as employment and industrial production.

That those persistent effects are not present following benefit increases (and the effects do not show up in broader measures) would seem to suggest that some force is damping the effects of the benefit changes. One possibility is that monetary policy may have played this role.

## **V. THE RESPONSE OF MONETARY POLICY**

In this section, we examine both statistical and narrative evidence on the response of monetary policy to Social Security benefit increases. Because we find the response of consumption and broader economic indicators to be very different for benefit increases and tax changes, we focus particularly on whether the monetary policy reactions are different as well.

### **A. Specification and Data**

To examine the response of monetary policy to Social Security benefit increases, we estimate regressions analogous to those for consumer spending, using the monthly change in the federal funds rate as the dependent variable. That is, our baseline specification is

$$(2) \quad \Delta FF_t = a + \sum_{i=0}^N b_i^{PERM} SS_{t-i}^{PERM} + \sum_{i=0}^N b_i^{TEMP} SS_{t-i}^{TEMP} + e_i,$$

where  $\Delta FF$  is the monthly change in the federal funds rate and, as before,  $SS^{PERM}$  and  $SS^{TEMP}$  are our new series on permanent and temporary changes in Social Security benefits (as a share of personal income).

One can think of equation (2) as a very simple form of the Federal Reserve's reaction function. This raises at least two issues. The first is the appropriate indicator of policy. We follow the standard practice of using the funds rate as the indicator, even though the period we

consider (1952–1991) includes times when the Federal Reserve was not explicitly targeting the funds rate.

The second issue is whether to include other arguments in the reaction function, such as the measures of inflation and the output gap that are usually included (or expectations of those variables). If the Federal Reserve responds to Social Security benefit changes, it is most likely because it expects them to affect inflation and output. Therefore, asking whether changes in Social Security benefits have an effect above and beyond any actual or anticipated effect on inflation and output would likely miss important channels through which the changes might influence monetary policy. Thus, our basic specification does not include those variables.

Nevertheless, we try including many of the same control variables we use when estimating the response of consumption. To the degree that benefits respond to past inflation, it is possible that the Federal Reserve could be reacting to the inflation and not to the benefit increases. Thus, despite the concern noted above, we also consider some specifications that control for inflation. In addition, because monetary policy shocks have large effects on the funds rate, it is important to check for accidental correlation between such shocks and permanent Social Security benefit increases. Finally, we consider specifications that include several lags of the change in the funds rate as additional explanatory variables. These lags should capture the impact of any serially correlated other factors affecting monetary policy.

We also ask whether leads of our Social Security measures appear to affect policy. Doing so tests whether the Federal Reserve is sufficiently proactive that it responds to news of the benefit changes, rather than to their implementation.

We consider a range of sample periods. Because the Volcker disinflation was associated with dramatic swings in the funds rate that could have a disproportionate influence on the estimates, we place considerable emphasis on the sample period starting in 1952:1 and ending just before the start of the disinflation (1979:9). We also again consider the full sample period, 1952:1–1991:12, and the period when increases in Social Security benefits were individually

legislated rather than the result of automatic cost-of-living adjustments, 1952:1–1974:12.<sup>28</sup>

Monthly data for the federal funds rate are available from the Board of Governors starting in 1954:7.<sup>29</sup> We extend the series back to 1952:1 using data reported by Martens (1958).

## **B. Results**

The consistent finding from the regressions is that the Federal Reserve responds to permanent Social Security benefit increases by raising the funds rate. The response is rapid, substantial, and highly statistically significant. As with the results for consumption, we find no evidence that monetary policy responds to temporary benefit changes.

Figure 7 shows the results from estimating (2) over the pre-Volcker period, 1952:1–1979:9, including 12 lags of the benefit changes. It reports the implied cumulative response of the federal funds rate to a permanent Social Security benefit increase of 1 percent of personal income. The response is 83 basis points in the month of the benefit increase and rises to a maximum of roughly 260 basis points 5 months after the increase. The null hypothesis that monetary policy does not respond is overwhelmingly rejected at short horizons; the maximum  $t$ -statistic is 4.0 after 3 months.

The results for other sample periods are similar. Ending the sample in 1974:12 rather than 1979:9 has little effect other than increasing the standard errors slightly and increasing the estimated response at longer horizons slightly. Extending it through 1991:12, and thus including the period of interest rate volatility during the Volcker disinflation, has almost no impact on the estimated response after 5 months. But it causes the response to be somewhat slower and the standard errors to be somewhat larger. For example, the maximum  $t$ -statistic falls to 2.5.

For temporary benefit changes, the estimated response of the funds rate is generally negative, but not statistically significant. In the baseline sample period (1952:1–1979:9), the

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<sup>28</sup> Since there is no break in the funds rate series in 1959, as there is with personal consumption expenditures, we do not consider the 1959–1991 sample. The results for this sample are similar to those for the full sample.

<sup>29</sup> The data are from the Board of Governors of the Federal Reserve System, series FEDFUNDS, downloaded from FRED, 1/23/2014.

contemporaneous impact is a fall of roughly 10 basis points ( $t = -0.9$ ). After 5 months it is a fall of roughly 80 basis points ( $t = -1.9$ ). This relative lack of an impact is consistent across samples and specifications.

Including the contemporaneous value and 12 lags of inflation in the regression has the somewhat surprising effect of increasing the estimated impact of permanent Social Security benefit increases on the funds rate.<sup>30</sup> Including the Romer and Romer dummy variable for monetary policy shocks reduces the maximum response of the funds rate trivially. In both cases, the impact remains highly statistically significant.

Including 12 lags of the change in the funds rate (in addition to the lags of the benefit increases) has little effect on the estimated impact of the permanent benefit changes on monetary policy; if anything, it raises the estimated response slightly. This suggests that accidental correlation between benefit changes and monetary policy shocks or other factors affecting Federal Reserve behavior does not appear to be driving the results.

As an additional test along these lines, we try excluding the largest permanent benefit increase (in October 1972), which occurred near of the beginning of a very large run-up in the funds rate in 1972–1974. Omitting this increase lowers the maximum response of the funds rate from 259 basis points to 220, accelerates the response slightly, and substantially changes the response at longer horizons. But the maximum  $t$ -statistic is still well over 3 (3.5 after 2 months).

Finally, including three leads of the permanent Social Security benefit increases reveals no evidence of anticipatory Federal Reserve responses. The coefficients on the leads are small and far from statistically significant (0.1, 0.3, and 0.1, starting with the 3-month lead, each with a standard error around 0.3). The coefficients on the contemporaneous and lagged values of the benefit changes are largely unchanged.

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<sup>30</sup> We try including both the measure of inflation used in the modern Social Security indexation formula, which we also use in the robustness checks for consumption, and the more straightforward monthly change in the seasonally adjusted consumer price index for all urban consumers.

### **C. Including Tax Variables**

As discussed in the previous section, changes in taxes are an important source of shocks to the economy, and they sometimes occur at about the same time as benefit increases. It is therefore useful to see if the estimated response of monetary policy to benefit increases is sensitive to the inclusion of a measure of tax changes. More fundamentally, the response of monetary policy to tax changes is of interest in itself. We therefore examine whether such a response is present and how it compares with the response to Social Security changes.

We use the same measure of tax changes as in Section IV, and we again include the contemporaneous value and 24 lags. Adding the tax variable to (2) has little impact on the estimated response of monetary policy to Social Security changes. The estimated monetary policy response is slightly smaller when the tax series is included in the regression, but the effect is still large and highly statistically significant.<sup>31</sup>

Figure 8 shows the cumulative response of the funds rate both to a permanent benefit increase and to a tax cut of 1 percent of personal income from the regression including both variables. As with the responses of consumption, the responses of the funds rate to benefit and tax changes are very different. Whereas the Federal Reserve appears to raise the funds rate quickly and strongly in response to a benefit increase, it actually cuts the funds rate slightly but significantly in response to a tax cut. The difference in the response is highly significant up through month 10. Though not shown in the figure, after 12 months the cumulative response of the funds rate to a tax cut turns positive. After 20 months, the cumulative impact is a rise in the funds rate of 1.0 percentage points ( $t = 1.4$ ); after 24 months, it is 2.0 ( $t = 2.7$ ).<sup>32</sup>

Finally, the estimated responses of the federal funds rate to both Social Security and tax changes in the expanded regression are little changed when we include 12 lags of the funds rate

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<sup>31</sup> The response of monetary policy to temporary Social Security changes is affected somewhat more by the inclusion of the tax series. It becomes more strongly negative, and is statistically significant at longer horizons.

<sup>32</sup> In the results reported in Figure 8, we include 12 lags of the Social Security benefit changes. If we include 24 lags of both permanent and temporary benefit increases, the response of the funds rate to a permanent benefit increase rises gradually over the second year.

as an additional control variable. This suggests that serially correlated omitted variables are not driving our findings.

#### **D. Narrative Evidence**

The regressions provide strong evidence of a link between increases in Social Security benefits and monetary policy. But we can go a step further and ask whether there is direct evidence of Federal Reserve behavior behind such a link. In particular, we examine the detailed accounts of meetings of the Federal Open Market Committee (FOMC), which discuss the reasoning behind monetary policy decisions. If Social Security benefit increases affected the conduct of policy, it should be evident in those records.<sup>33</sup>

**Benefit Increases and Consumption.** Throughout our sample period, the staff of the Federal Reserve and the members of the FOMC were very clear that they believed that Social Security benefit increases had a stimulative impact on consumption. Moreover, they thought that the timing of the effect coincided closely with the actual benefit increases, rather than being anticipatory or working with substantial lags.

One relatively extensive early discussion occurred around the time of the September 1965 benefit increase. According to the staff presentation at the August 10 meeting (*Minutes*, 8/10/65, p. 28),

The mailing of checks to Social Security beneficiaries, including both the new higher scale of payments and lump-sum retroactive benefits, will be adding to disposable personal income shortly. ... How rapidly, and for what goods or services, recipients of the benefits will spend their funds is a big unknown; we have very little basis for estimating the consumption function for this older age group. But it's hard to believe that the bulk of it won't get into the spending stream fairly promptly.

At the next meeting, one FOMC member referred to “the fiscal stimuli the economy would be receiving in the next few weeks,” suggesting that the timing of the perceived effect of the Social

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<sup>33</sup> Through the meeting of March 15–16, 1976, the accounts are thorough summaries prepared after the meetings. They are often over 100 pages for a single meeting, and remarks are attributed to specific participants. These accounts are referred to as “Minutes” through May 1967 and “Memoranda of Discussion” thereafter. For simplicity, we refer to all of these summaries as “Minutes.” After the March 15–16, 1976 meeting, our sources are the meeting transcripts. (Board of Governors of the Federal Reserve System, various years.)

Security increase was closely linked to the timing of the change in benefits (*Minutes*, 8/31/65, p. 48).<sup>34</sup> And at the following meeting, the staff presentation commented (*Minutes*, 9/28/65, p. 17):

Total consumer spending ... will no doubt continue to be strong. The relationship between such spending and personal incomes is relatively stable, and incomes have recently been augmented by a large lump-sum social security benefit payment as well as by an increase in current payments. The spending propensities of the aged are no doubt higher than those of other segments of the population.

The discussion of the September 1965 benefit increase is unusual only in its detail. More commonly, participants appear to have taken it as given that benefit increases, by raising disposable income, raised consumption. They often commented on the impact of changes in benefits on household income, and either stated or implied that those changes would feed through to household spending.

Benefit increases received relatively little attention in the 1950s, when the meeting summaries were often relatively short and benefit increases were generally moderate. However, Federal Reserve officials did occasionally note their impact. For example, in March 1959, following a benefit increase the previous month, the staff presentation noted, “the recent advance [in personal income] reflected mainly a further rise in wage and salary payments, but higher old-age and survivors’ benefit payments were also of importance in causing the rise. With personal income advancing further, retail sales in February were strong” (*Minutes*, 3/24/59, p. 7; see also 11/27/56, p. 7, and 5/27/58, p. 4).

In the 1960s and 1970s, the consumption effects of Social Security benefit changes were mentioned frequently. As described above, there was a long discussion around the 1965 increase. Similarly, in April 1970 (the month of a large benefit increase), the staff presentation commented, “we are now at the point where additional income supplements—including social security payments as well as the Federal pay raise—should begin to stimulate consumer demands” (*Minutes*, 4/7/70, p. 26). Again, the timing of the effect was linked to the timing of

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<sup>34</sup> For simplicity, we refer to all the regional bank presidents and members of the Board of Governors as “members” of the FOMC, even if they were not voting members of the committee.

the actual change in benefits. In November 1972, the staff explicitly attributed the rise in retail sales the previous month partly to the benefit increase that had occurred then: “The upward course in retail sales in real terms is particularly impressive; the sharp rise in October reflects both strength in new car buying and substantial gains in other lines, stimulated in part by the recent boost in social security benefits” (*Minutes*, 11/20/72, p. 5).

After 1974, when Social Security cost-of-living adjustments became standard, the impact of the benefit changes received less attention. However, there were some discussions of them. In late 1974 and early 1975, for example, the staff consistently projected that the first automatic cost-of-living increase, scheduled for July 1975, would boost consumption when it occurred. In September 1974, “The upturn in consumer spending projected by the staff for the latter half of 1975 was based on the increase in disposable income expected to result from increased social security payments and an anticipated redistribution of income toward wage earners .... There was no assumption of a significant decline in the rate of saving” (*Minutes*, 9/10/74, p. 11). And quite late in our sample period, in a discussion of the “consumption function” and the forecast for the path of the saving rate, Federal Reserve Chairman Alan Greenspan stated, “[Unintelligible] COLA on social security, you have to assume [the marginal propensity to consume] is about .9” (*Transcript*, 2/9–10/88, pp. 11–12; brackets in the original).

**Benefit Increases and Monetary Policy.** In light of policymakers’ belief that Social Security benefit increases were expansionary, one might expect that they would view them as calling for tighter monetary policy. The narrative record confirms this expectation. During the core part of our sample period when benefit increases were often discussed, policymakers consistently viewed them as a consideration weighing on the side of more contractionary policy. Interestingly, in the narrative sources, increases in Social Security benefits are often discussed together with other expansionary fiscal actions. That is, in contrast to our empirical finding of little correlation between benefit changes and other changes in fiscal policy, monetary policymakers appear to have perceived such a correlation, at least in some episodes. To the

extent that contractionary monetary policy actions in the wake of the benefit increases were responses to those other fiscal actions rather than to just the benefit increases, our regressions may somewhat overstate the effects of benefit increases on monetary policy.

Again, a particularly extensive discussion occurred at the August 10, 1965 FOMC meeting, shortly before the large permanent benefit increase scheduled for September. At this meeting, four committee members explicitly argued that looser fiscal policy called either for not easing monetary policy or for tightening. For example, one member said: “I would not want to ease policy right now, for a considerable degree of new fiscal stimulus lies immediately ahead of us. Some of this will come from the enlarged Social Security payments” (*Minutes*, 8/10/65, p. 65; see also pp. 48–49, 55, and 70). In addition, the staff presentation stated, “it would seem premature now to add monetary stimulation to the picture—at least not until the dimension of consumer responses to the Social Security payments becomes more evident or the pace of the defense buildup becomes clearer” (p. 29).

This pattern continued in response to other Social Security benefit increases. In April 1968, when the recent benefit increase was cited as one factor stimulating the economy (*Minutes*, 4/2/68, p. 39), many members discussed the link between fiscal policy and appropriate monetary policy in very clear terms. The vice-chair of the committee said, “The appropriateness and timing of an additional discount rate increase must be importantly influenced by the progress or lack of it with respect to Vietnam and on the fiscal front” (p. 49). Another argued that “little real progress had been made in either cutting expenditures or raising taxes. Hence, he felt that movement towards greater monetary restraint was still needed” (p. 50). Another’s view was that “further monetary tightening would be in order if it became clear that fiscal action was not likely to be taken” (p. 84; see also pp. 16, 39, 70, 72, and 87).

The FOMC’s views around the time of the April 1970 benefit increase were similar. Both members and the staff commented on the expansionary effects of the benefit increase (*Minutes*, 1/15/70 pp. 34 and 47; 4/7/70, pp. 26 and 36). A number of members drew implications for

monetary policy from fiscal policy. For example, one argued that “in view of the lessening fiscal restraint and the persistent inflationary expectations of business, he would permit only a very minor shading away from the taut money market conditions of early December” (1/15/70, p. 62; see also pp. 74–75 and 100).

As a final example, consider policy around the time of the 1974 benefit increases. Again, the Social Security increases were discussed as an expansionary influence on the economy (*Minutes*, 1/21–22/74, p. 56; 8/20/74, p. 36; and 9/10/74, p. 11). And again, fiscal policy was thought to be directly relevant to monetary policy. For example, in January, one member’s view was that “fiscal policy might become more stimulative, and monetary policy might have to be more conservative than otherwise if the combination of fiscal and monetary policies was to be moderately conservative—as the Chairman had suggested it should be” (*Minutes*, 1/21–22/74, p. 100; see also p. 107). In February, another member commented simply that “The possibility of an easing in fiscal policy provided an opportunity for the System to ease monetary policy less than it otherwise might” (*Minutes*, 2/20/74, p. 72). And at the same meeting, the vice-chair cited the fact that “the Federal budget was likely to be reasonably stimulative” as a reason not to undertake “a decisive move toward ease” (p. 56).

In short, the record of policymakers’ thinking shows clearly that the statistical association between Social Security benefit increases and tighter monetary policy is not a coincidence. Policymakers consistently believed that benefit increases were expansionary, and that monetary policy should counteract expansionary fiscal policy.

**Comparison with Tax Changes.** Our statistical analysis finds that the monetary policy responses to Social Security benefit increases and tax changes are very different. It is therefore natural to examine whether the narrative record suggests that monetary policymakers had a different view of tax changes than of benefit increases. To keep the analysis manageable, we focus on the largest tax changes in our sample: the Kennedy-Johnson tax cuts of 1964 and 1965, the Reagan tax cuts of 1981–1985, the Nixon tax cut of 1972, the tax increases related to Social

Security and Medicare in 1966, 1973, and 1981, and the 1991 tax increase under George H. W. Bush.

As with benefit increases, monetary policymakers clearly believed that tax changes affected the economy in the conventional direction. For example, in January 1964, the staff presentation stated, “Looking ahead, the stimulative effects of the tax cut on the economy in the first half of 1964 are projected as very large” (*Minutes*, 1/28/64, p. 14). In 1971, just after the announcement of the president’s economic proposals, Federal Reserve Chairman Arthurs Burns said that, “In his judgment the program implied a great deal of stimulus” (*Minutes* 8/24/71, p. 8). At the meeting two months after the signing of the Reagan tax cuts, six members commented on their likely stimulative effects (*Transcript*, 11/17/81, pp. 14, 15, 17, 19, 23, and 24). Because this is such a consistent pattern and is similar to policymakers’ view of Social Security benefit increases, we will not belabor it.

Instead, what is notable is that policymakers’ perception of the appropriate monetary policy response to tax changes was far more complicated than for benefit increases. In most cases, either they believed that they should not try to offset the effects of tax changes, or other considerations muted their response.

In 1964 and 1965, the view that carried the day within the FOMC was that the tax cut was designed to raise long-run growth and lower unemployment, and monetary policy should allow it to do that until inflation was a clear problem. For example, in February 1964, one member’s view was: “the stimulative effect of a tax cut, which was being counted on so heavily by the American people should not be offset by the System until such action was obviously necessary” (*Minutes*, 2/11/64, p. 48). Similarly, in March, another member said in a prepared statement, “I believe we ought to be holding policy unchanged, through the next meeting and beyond, until such time as we will have reaped the full potential noninflationary stimulus of the tax cut” (*Minutes*, 3/24/64, p. 52). In February 1965, yet another member said that the recent surge in consumer spending was “not a larger reaction than sought when it was hoped that the tax cut

might achieve a lower rate of unemployment than 5 per cent,” and that “it should not be tranquilized by monetary restraint” (*Minutes*, 2/2/65, p. 42).

Federal Reserve officials’ view of the 1972 tax cut was similar. As in 1964, the 1972 measure was undertaken to try to obtain a period of above-normal growth, and monetary policymakers largely supported the program. For example, the policy directive adopted at the meeting immediately after the president’s announcements of his proposals said, “it is the policy of the Federal Open Market Committee to foster financial conditions consistent with the aims of the new governmental program” (*Minutes*, 8/24/71, p. 106), and the directives at the next five meetings contained similar language. In December 1971, the staff presentation stated, “There appears to be general agreement that fiscal actions of this magnitude were needed, and are desirable” (*Minutes*, 12/14/71, p. 18).

These episodes and other discussions of fiscal policy around the same time may suggest a broader pattern. Monetary policymakers believed they should counteract the aggregate demand effects of fiscal actions, such as Social Security benefit changes and wartime spending, where those effects were unintended consequences. On the other hand, for many tax changes, the expansionary effects were expected and desired by Congress, and so should not be counteracted.

This general pattern may help explain why monetary policymakers moved quickly to offset the 1991 tax increases that were part of the 1990 budget agreement. In this case, any aggregate demand consequences of the agreement were an undesired side effect of a reduction in the budget deficit. In July 1990, the staff provided a detailed analysis of the monetary policy response needed to offset the effects of the prospective fiscal changes (*Presentation materials*, 7/2–3/90, Prell presentation, pp. 9–11), and the chief economist said, “obviously, we have to have that accommodation” (*Transcript*, p. 4). At the October 1990 meeting, the FOMC explicitly agreed to tie a cut in the target federal funds rate to a budget agreement (*Transcript*, 10/2/90, pp. 57–59); and when a budget agreement was reached four weeks later, the target was indeed cut by 25 basis points.

In the cases of the Social Security tax increases in 1966, 1973, and 1981, a very different factor appears to have prevented a significant monetary policy response. Policymakers believed the increases would damp aggregate demand (see, for example, *Minutes*, 5/15/73, p. 20). But they commented more frequently on the fact that by raising firms' costs, they would tend to increase inflation. For example, just before the 1966 increase, the vice-chairman of the committee referred to "the upward push [to unit labor costs] that will be exerted by the increase in social security taxes" and cited it as one factor that made "the prospects for continued cost stability seem doubtful" (*Minutes*, 12/14/65, p. 30). In April 1973, one member said that "the increase in social security taxes also might provide part of the explanation for the recent high rate of advance in prices" (*Minutes*, 4/17/73, p. 18), and the impact of the tax increase on compensation costs was mentioned in the directive (p. 94). In September 1980, in response to a question about the inflation outlook, a staff member said, "The employer portion [of the increase in Social Security taxes scheduled for January 1] will be cranked directly into unit labor costs obviously, and we assume that businesses will try to pass those increased costs through fairly quickly" (*Transcript*, 9/16/80, p. 13).

In the end, the Social Security tax increases were almost never mentioned in the discussions of policy actions. A likely reason is that because policymakers viewed them as causing both reductions in aggregate demand and increases in inflation at a given level of demand, they believed they did not call for any substantial response.

The Reagan tax cuts occurred during the brief period when the Federal Reserve was putting considerable emphasis on money targets. In the discussion of the targets for 1981 and 1982 in July 1981, FOMC members viewed it as critical to be perceived as gradually lowering money growth and their growth targets. One member said, "it's extremely important to be perceived as doing what we said we were going to do" (*Transcript*, 7/6-7/81, p. 54). Yet they also thought the economic outlook was much weaker than they wanted (pp. 17-21), and so they set their money targets as high as they felt they could without risking the credibility of their anti-

inflation policy (pp. 89–95). The fact that policymakers wanted to be loosening monetary policy more than they felt able to do likely explains why they did not try to fully counteract the expansionary aggregate demand impact of the tax cuts in this episode.

In the cases of the Nixon and Reagan tax cuts, another consideration also caused monetary policy not to respond strongly to our measure of tax cuts. We date tax changes according to when they changed tax liabilities. For example, the Revenue Act of 1971 cut personal income taxes on 1972 incomes, and so we identify a tax cut in January 1972. Policymakers, however, believed the effects were tied to when the cut actually changed disposable income. Because there was considerable overwithholding in early 1972, they therefore expected the effects to be delayed. For example, a staff member said, “It is our view that recent sizable cuts in personal taxes are being offset currently by a swing from underwithholding to overwithholding in withheld taxes. The impact of this overwithholding is to spread the economic effects of recent tax cuts over a longer period” (*Minutes*, 2/15/72, p. 22). Similarly, because the main changes in tax rates in the Reagan tax cuts occurred for the tax years 1982, 1983, and 1984, we identify tax cuts in January of each of those years. The bill, however, framed the main cuts as occurring on July 1, 1982 and July 1, 1983, and withholding was reduced on those dates. Federal Reserve officials and staff members consistently referred to the tax cuts as occurring on those dates, and expected their stimulative effects to occur then (for example, *Transcript*, 11/17/81, pp. 14, 15, 17, 22, and 24). That monetary policymakers expected the main effects of some important tax changes to occur well after the times that liabilities changed may be part of the reason that we find that the monetary policy response to changes in taxes is relatively slow.

The bottom line is that the overall pattern of the narrative evidence about monetary policy is very different for tax changes than for Social Security benefit changes. For a range of reasons, monetary policymakers usually did not think they should move promptly and aggressively to offset the impact of tax changes on aggregate demand. This matches what we find in our regressions: the response of the funds rate to tax changes is much slower and smaller than its

response to Social Security benefit changes.

The very different monetary policy responses cannot explain why the consumption effects of benefit increases occur so much faster than those for taxes. Monetary policy affects the economy with a lag, and we find no evidence of preemptive changes. Moreover, the differences we find in the near-term monetary policy responses would tend to make the short-run effects of tax cuts larger than those for benefit increases, not smaller.

On the other hand, the different monetary policy behavior may explain the very different medium-run responses of consumption and economic activity to benefit increases and tax cuts. The swift and strong contractionary monetary policy response to benefit increases is consistent with our findings that the short-run effects of the benefit increases fade over time and do not spread to other variables. The fact that the monetary policy response to tax cuts is modestly expansionary for the first year and only very gradually contractionary over the second is consistent with the finding that the macroeconomic effects of tax cuts are broad, persistent, and rise over time.

## **VI. CONCLUSIONS AND IMPLICATIONS**

This paper shows that Social Security benefit increases over the period 1952–1991 were highly irregular in timing and size, and presents evidence that most of the increases were not taken in response to current or prospective macroeconomic developments or as part of larger policy programs. As a result, these benefit increases can be used to estimate the short-run macroeconomic effects of changes in transfers.

Our first finding is that transfers matter. Our estimates suggest that a permanent increase in Social Security benefits raises aggregate consumer spending in the first month the larger checks arrive almost one-for-one. The estimated impact remains high for about half a year, and then declines sharply. The initial impact is highly statistically significant, but the standard errors increase as the horizon lengthens. Interestingly, the estimated response of consumption

to temporary benefit changes is small and not statistically significant. And, for both types of changes, we find no evidence of a response before the payments are actually received.

A comparison of the impact of permanent Social Security benefit increases and relatively exogenous tax changes shows a striking contrast. While the tax changes are slower to affect consumption, their effects are much more persistent, more statistically significant, and far larger over an extended period than those for benefit increases. As a result, tax changes affect broader economic indicators, while benefit increases do not. Both types of changes have their primary impact on total consumption by raising expenditures on durable goods.

Monetary policy appears to be important in explaining the differential impacts of benefit increases and tax cuts at medium horizons. The federal funds rate rises sharply and significantly following permanent Social Security benefit increases, but it moves little, or perhaps even falls, during the year following tax cuts. Narrative evidence from Federal Reserve records confirms that monetary policymakers believed that the benefit increases stimulated the economy and called for a contractionary response, whereas their beliefs about the appropriate response to tax changes were far more complicated.

These findings have implications for both research and policy. On the research side, the most important implication is in some ways the most prosaic: it is useful to look at the macroeconomic impact of transfers. The microeconomic evidence on the response of consumption to income changes does not carry over seamlessly to the aggregate level. In the case of Social Security benefit changes between 1952 and 1991, a systematic monetary policy response counteracted the impact of benefit changes on consumption very rapidly. In addition to suggesting caution in trying to generalize from individual-level studies to the macroeconomy, this implication also highlights the importance of considering monetary and fiscal policy jointly in empirical studies, and of including such interactions in our models.

Related to this, our study also casts doubt on the view of traditional Keynesian models and of such authors as Blanchard and Perotti (2002) that the short-run macroeconomic effects of

changes in transfers and changes in taxes are approximately equal and opposite. Our results suggest that the speed, persistence, and size of the responses of consumption and of monetary policy to a permanent increase in transfers and to a tax cut of the same size may in fact be dramatically different. As a result, their impacts on the broader economy may also be very different.

While our study raises questions about whether individual-level studies of consumption carry over to the aggregate level, our aggregate results may nevertheless contribute to our understanding of household consumption behavior. This is especially true of the very short-run behavior of consumption following benefit increases, which are relatively uncomplicated by any monetary policy response. In this regard, our findings echo the results of many previous studies that neither the permanent income hypothesis nor a simple hand-to-mouth model provides a complete description of consumption behavior. As stressed by Wilcox (1989), the fact that aggregate consumption responds when permanent benefit increases are implemented even though the changes are announced in advance contradicts the permanent income hypothesis. At the same time, a hand-to-mouth or liquidity constraints view is contradicted by our finding that consumption does not respond to temporary benefit increases.

Our results are somewhat more supportive of more nuanced views of consumption behavior. The fact that aggregate consumption responds much more to permanent benefit changes than to temporary ones is consistent with the notion that households smooth consumption in response to temporary changes. And, because the temporary changes in our sample are typically quite large, our findings are consistent with previous work suggesting that the permanent income hypothesis describes household behavior reasonably well when large payments are involved.

On the policy side, the fact that we find little impact from large temporary increases in transfers could raise questions about the efficacy of such payments for countercyclical purposes. And, if the findings for temporary transfers carry over to temporary tax changes, our results

could raise similar concerns about the countercyclical effectiveness of such tax changes. However, because the temporary transfers that drive our estimates are quite large, our estimates do not speak directly to the issue of whether small temporary transfers could have a greater impact. In addition, the transfers we consider are targeted to the elderly. Determining why this group responds strongly to permanent increases but not to temporary ones is important for understanding whether our results are likely to hold for other types of changes.

Another policy implication of our study involves the interaction of monetary and fiscal policy. We find that the monetary policy response is a critical determinant of the persistence and strength of the impact of benefit increases and tax changes. These results support the view that the effects of fiscal policy are very dependent on the conduct of monetary policy. And they suggest that if fiscal policymakers want to achieve some objective, coordination with monetary policymakers may be essential.

**TABLE 1**

New Series on Permanent and Temporary Social Security Benefit Increases, 1952–1991  
(Percent of Personal Income)

Date	Permanent	Temporary	Date	Permanent	Temporary
Oct. 1952	0.23		Jul. 1975	0.37	
Oct. 1954	0.21		Jul. 1976	0.31	
Dec. 1956	0.14		Jul. 1977	0.29	
Aug. 1957	0.07		Jul. 1978	0.31	
Oct. 1958	0.05		Jul. 1979	0.46	
Feb. 1959	0.18		Jul. 1980	0.68	
Dec. 1960	0.05		Jul. 1981	0.56	
Jan. 1961	0.06		Jul. 1982	0.39	
Sep. 1965	0.39	1.81	Aug. 1983	0.03	
Jan. 1966	0.03		Nov. 1983		0.17
Nov. 1966	0.02		Dec. 1983		0.21
Mar. 1968	0.49		Jan. 1984	0.19	
Apr. 1970	0.48	0.96	Dec. 1984		0.25
Jun. 1971	0.37	1.46	Jan. 1985	0.19	
Oct. 1972	0.75		Jul. 1985		0.16
Feb. 1973	0.21		Jan. 1986	0.16	
Feb. 1974	0.14		Jul. 1986		0.17
Apr. 1974	0.33		Jan. 1987	0.07	
Jul. 1974	0.19		May 1987		0.16
Aug. 1974	0.01		Jan. 1988	0.21	
			Mar. 1988		0.12
			Jan. 1989	0.19	
			Mar. 1989		0.14
			Nov. 1989		0.08
			Jan. 1990	0.22	
			Jan. 1991	0.27	

Sources: See the [appendix](#) for a detailed description of each benefit change.

**TABLE 2**

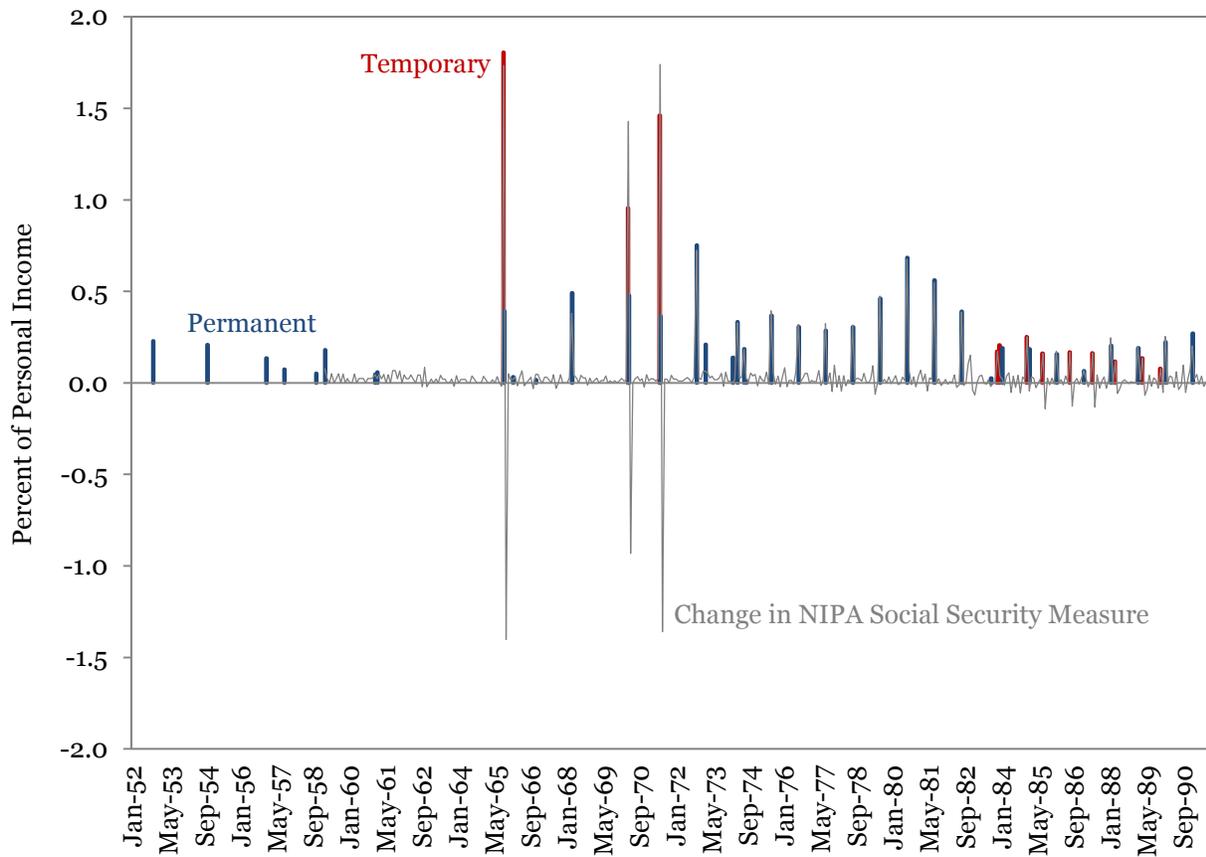
Cumulative Impact of a Permanent Social Security Benefit Increase of 1% of Personal Income on Various Macroeconomic Outcome Variables (Percent)

Month	Real PCE		Real Retail Sales		Industrial Production		Employment	
	Impact	SE	Impact	SE	Impact	SE	Impact	SE
0	1.23	0.43	1.67	0.95	0.37	0.73	0.00	0.21
1	1.04	0.58	2.07	1.28	0.31	0.98	0.06	0.28
2	0.94	0.71	1.64	1.57	0.66	1.20	0.01	0.34
3	1.03	0.83	1.97	1.83	0.67	1.39	0.00	0.40
4	1.01	0.93	2.13	2.05	0.39	1.57	-0.01	0.45
5	0.96	1.03	1.46	2.27	-0.28	1.73	-0.12	0.49
6	0.36	1.13	0.09	2.49	-1.81	1.90	-0.37	0.54
7	0.02	1.23	-0.59	2.71	-2.05	2.06	-0.55	0.59
8	-0.32	1.33	-0.53	2.93	-2.52	2.24	-0.57	0.64
9	-1.09	1.43	-1.58	3.16	-3.40	2.41	-0.80	0.69
10	-1.60	1.53	-2.53	3.38	-4.30	2.58	-1.01	0.74
11	-1.12	1.63	-2.58	3.60	-4.83	2.75	-1.23	0.78
12	-2.49	1.67	-4.08	3.68	-5.48	2.81	-1.37	0.80

Notes: The estimated impact shows the effect on the level of each variable relative to the initial value (in percent), at different horizons. SE is the standard error of the cumulative impact at each horizon. The results are based on estimating equation (1) over the sample period 1952:1–1991:12, including 12 lags each of permanent and temporary benefit changes.

**FIGURE 1**

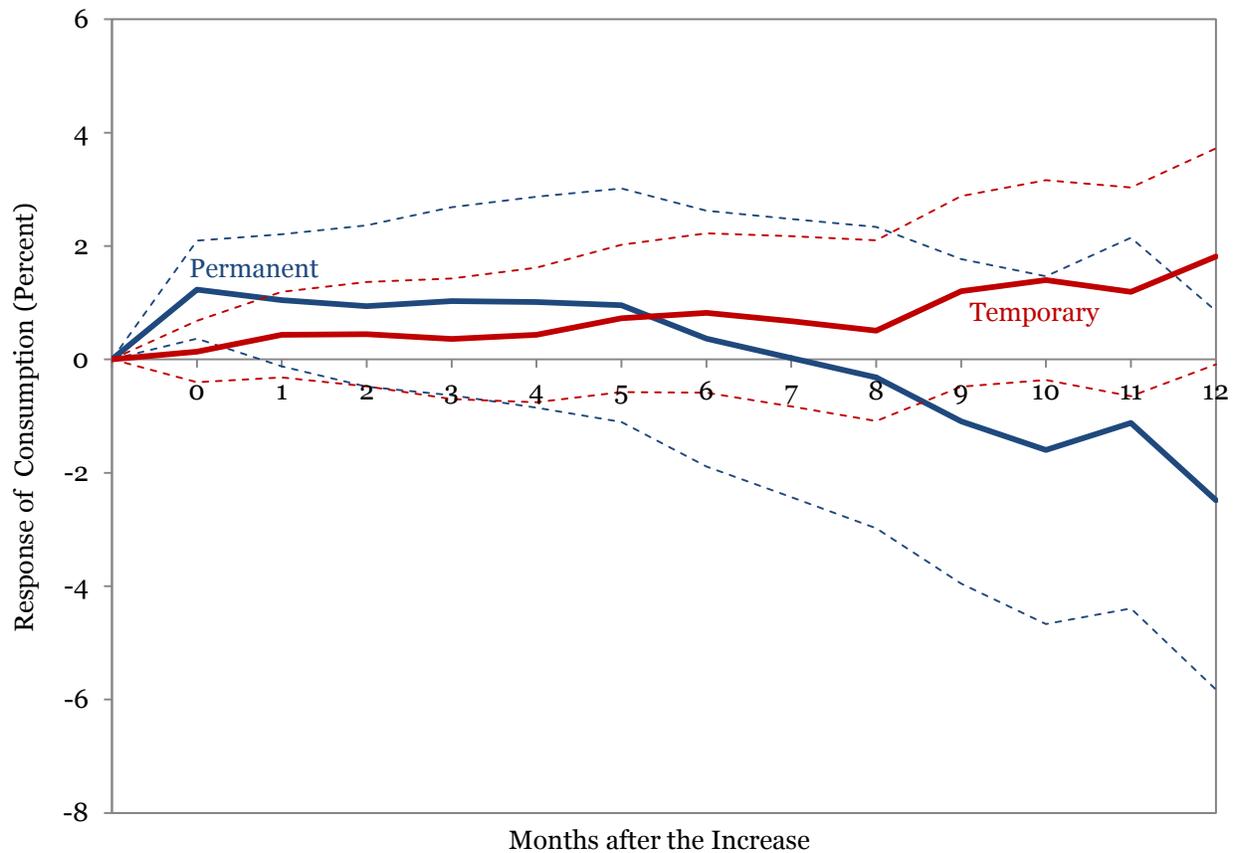
New Series on Permanent and Temporary Social Security Benefit Increases,  
along with the Change in the NIPA Series for Social Security Transfers



Sources: Set text for details of the new series and the source of the NIPA data.

**FIGURE 2**

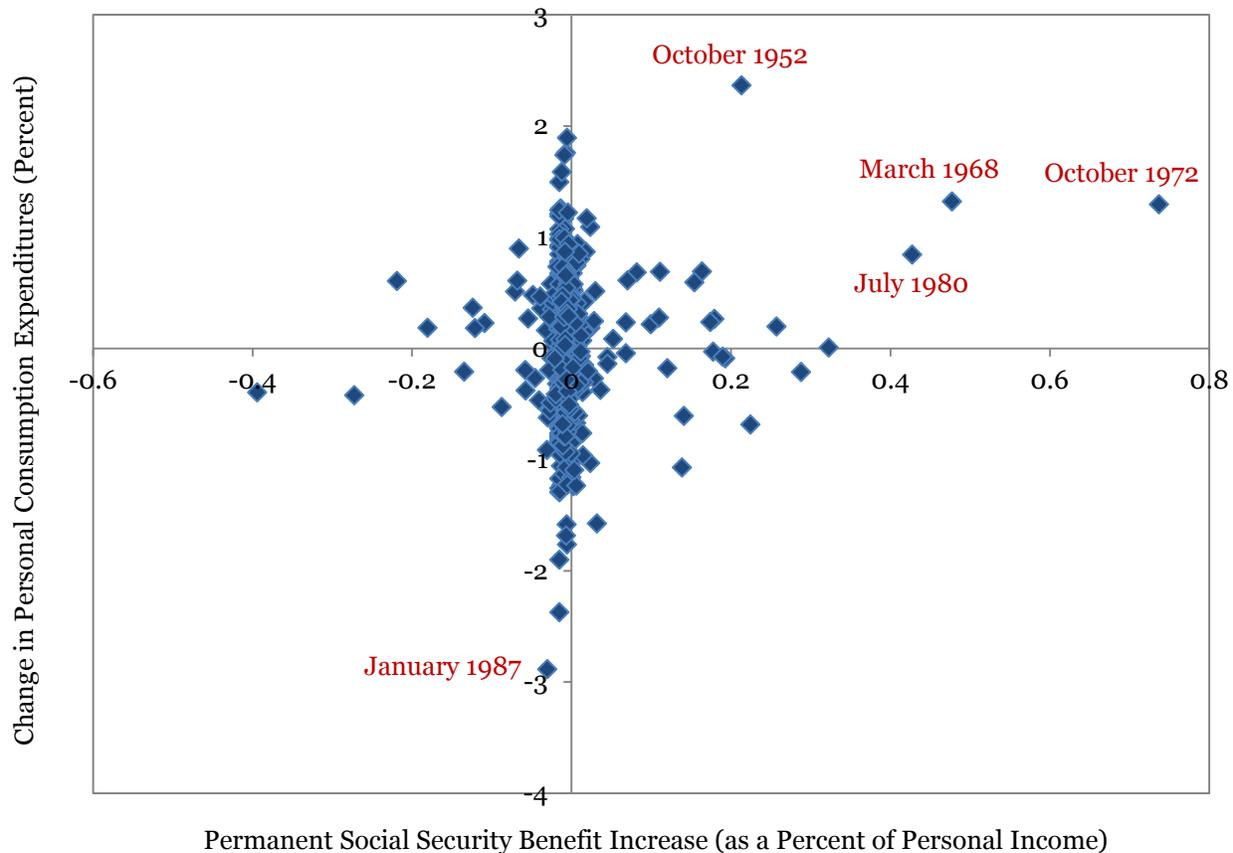
Cumulative Impact of a Permanent and a Temporary Social Security Benefit Increase of 1% of Personal Income on Personal Consumption Expenditures



Notes: The figure shows the results from estimating equation (1) over the sample period 1952:1–1991:12, including 12 lags each of permanent and temporary benefit changes. The dashed lines show the two-standard-error confidence bands.

**FIGURE 3**

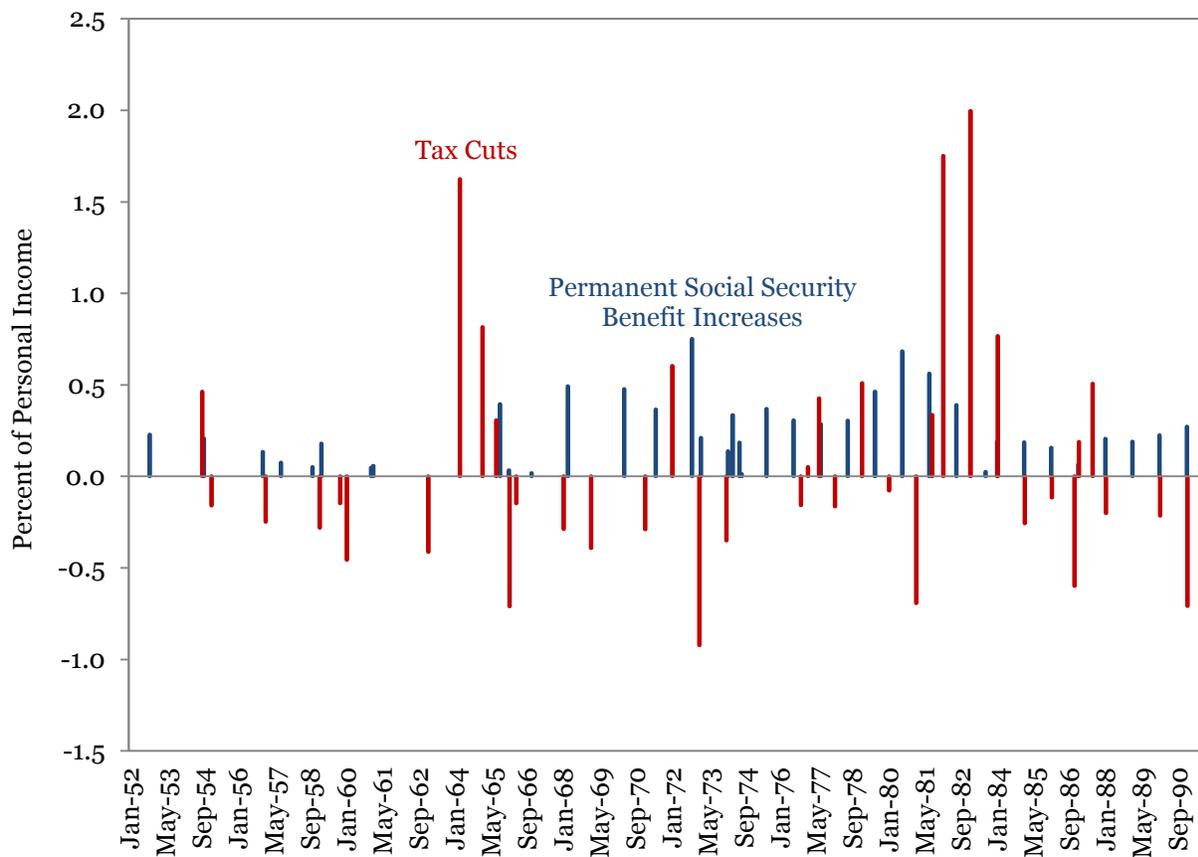
Partial Association of Permanent Social Security Benefit Increases  
and the Contemporaneous Change in Personal Consumption Expenditures



Notes: The figure graphs the residuals of a regression of permanent Social Security benefit increases on all of the other right-hand-side variables in the baseline specification of equation (1) against the residuals of a regression of the percentage change in real PCE on the same variables. The sample period is 1952:1–1991:12.

**FIGURE 4**

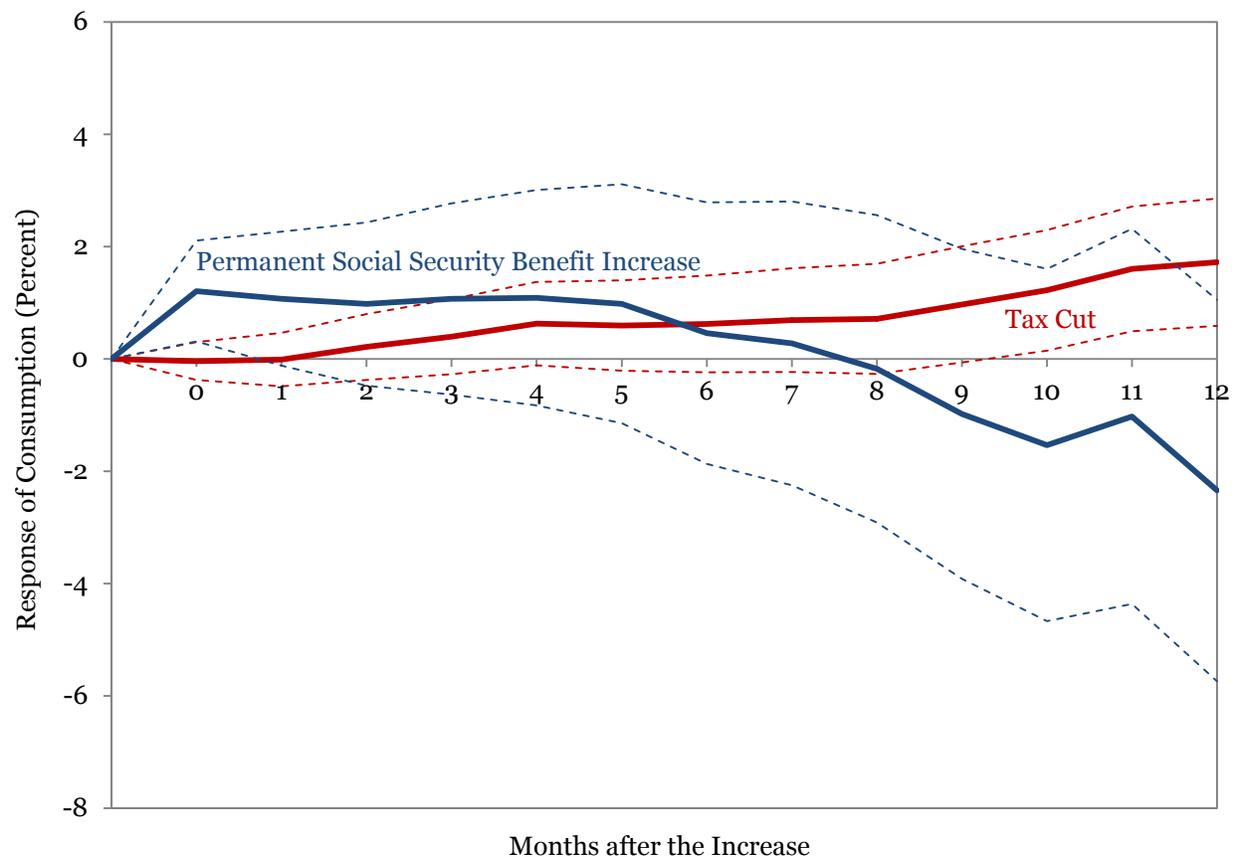
Permanent Social Security Benefit Increases and  
Relatively Exogenous Tax Cuts



Sources: Set text for details of the new Social Security benefit series. The tax series is from Romer and Romer (2010). The series plotted is the sum of tax changes for long-term growth, deficit reduction, and to pay for Social Security spending increases. For comparability with the benefit series, tax cuts are expressed as positive values and tax increases as negative values. As discussed in detail in the text, we exclude tax actions that only affected business taxes.

**FIGURE 5**

Cumulative Impact of a Permanent Benefit Increase and a Tax Cut of 1% of Personal Income on Personal Consumption Expenditures in the Regression Including Both Variables

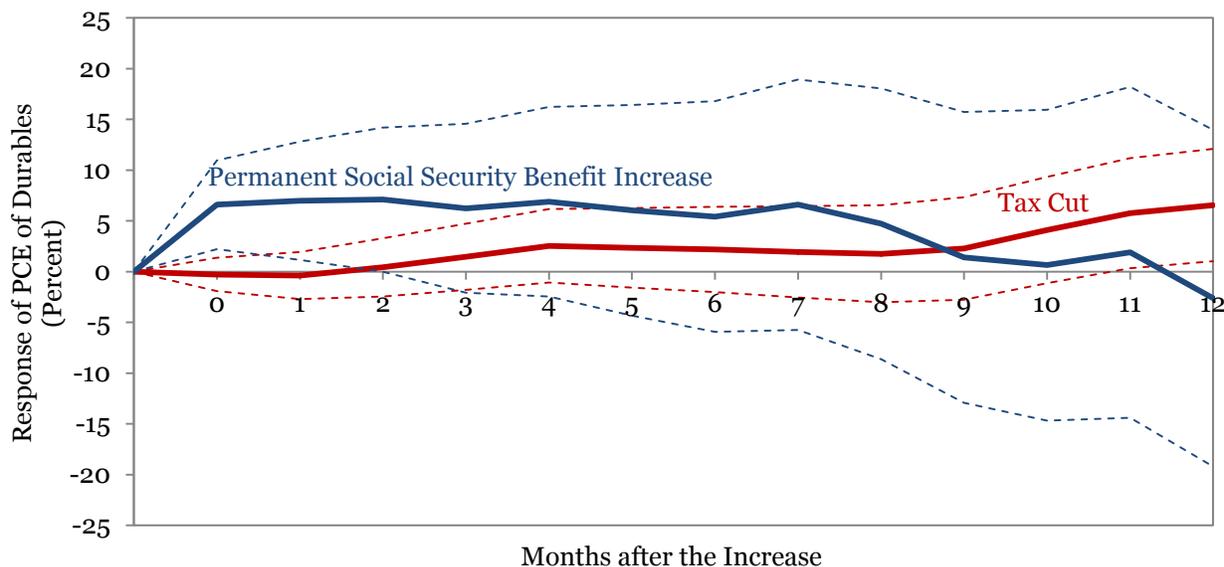


Notes: The results are from estimating equation (1) including the contemporaneous value and 24 lags of the tax variable as an additional control. The sample period is 1952:1–1991:12. The dashed lines show the two-standard-error confidence bands.

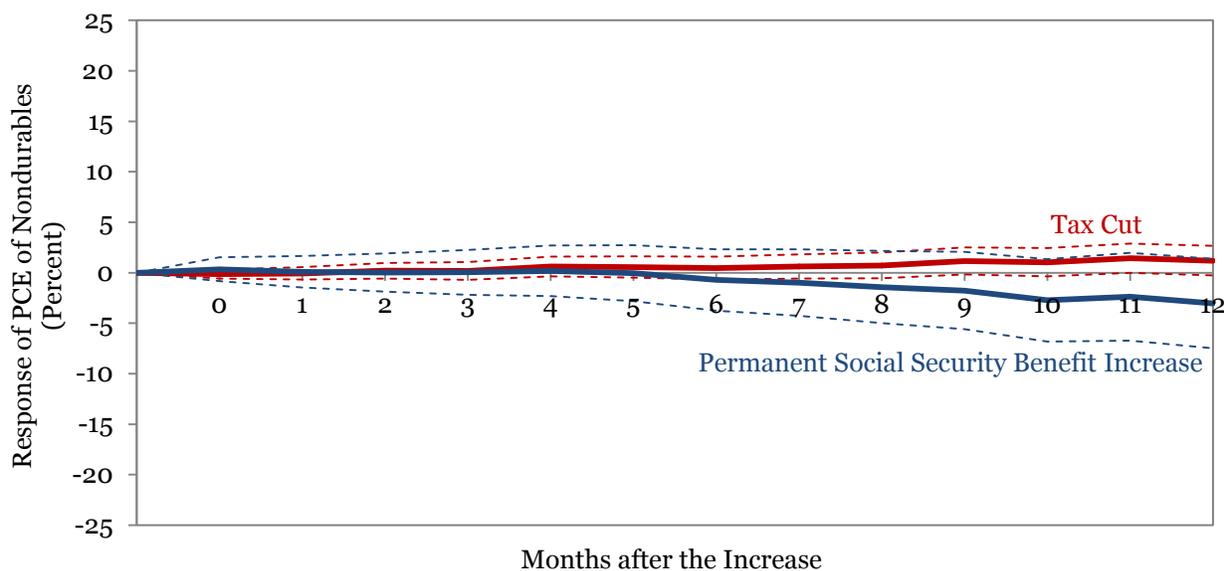
**FIGURE 6**

Cumulative Impact of a Permanent Benefit Increase and a Tax Cut of 1% of Personal Income on Personal Consumption Expenditures by Spending Type

a. Durable Goods



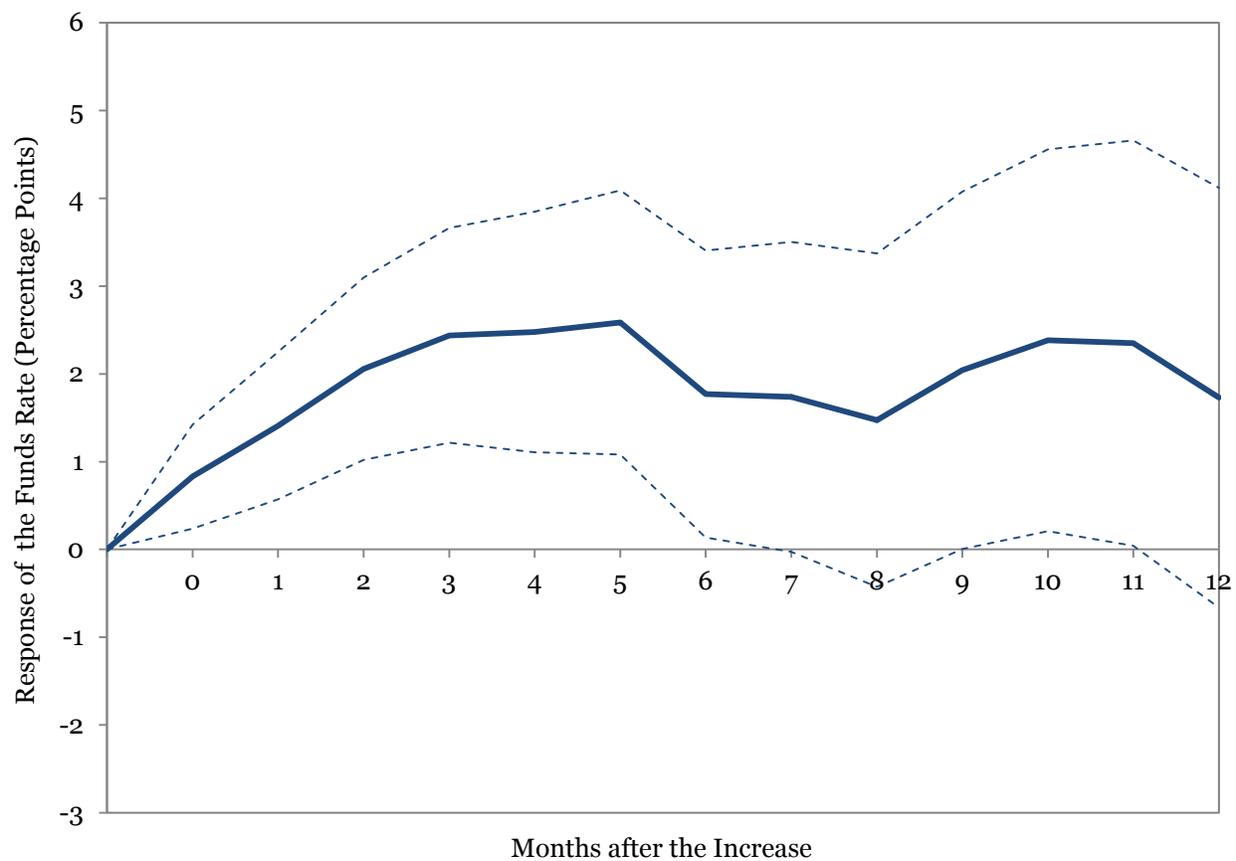
b. Nondurable Goods



Notes: The results are from estimating equation (1) for durables and nondurables consumption separately, including the contemporaneous value and 24 lags of the tax variable as an additional control. The sample period is 1952:1–1991:12. The dashed lines show the two-standard-error confidence bands.

**FIGURE 7**

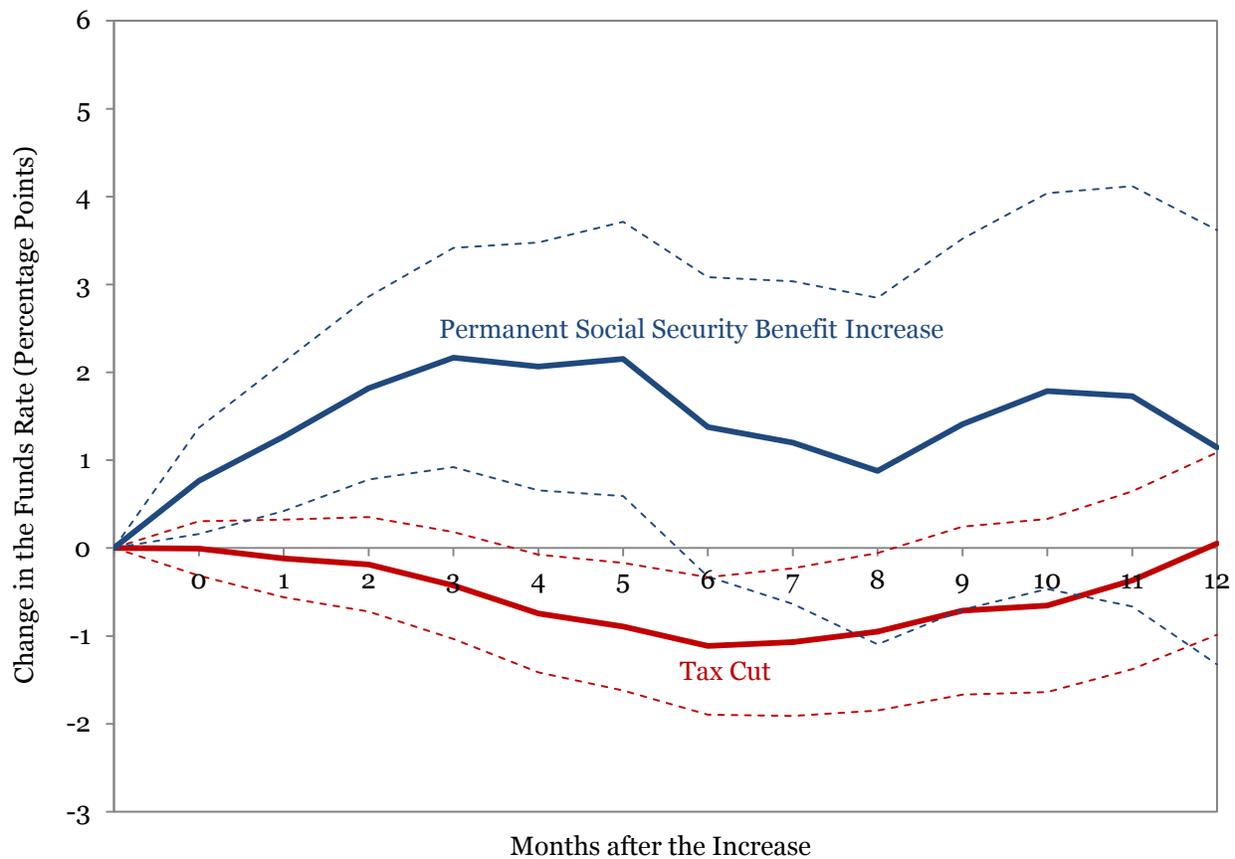
Cumulative Impact of a Permanent Social Security Benefit Increase of 1% of Personal Income on the Federal Funds Rate



Notes: The figure shows the results of estimating equation (2) over the sample period 1952:1–1979:9, including 12 lags each of permanent and temporary benefit changes. The dashed lines show the two-standard-error confidence bands.

**FIGURE 8**

Cumulative Impact of a Permanent Benefit Increase and a Tax Cut of 1% of Personal Income on the Federal Funds Rate in the Regression Including Both Variables



Notes: The results are from estimating equation (2) including the contemporaneous value and 24 lags of the tax shock variable as an additional control. The sample period is 1952:1–1979:9. The dashed lines show the two-standard-error confidence bands.

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