

The Aggregate and Relative Economic Effects of Medicaid and Medicare Expansions*

Bill Dupor[†] and Rodrigo Guerrero[‡]

April 23, 2018

Abstract

Government-financed health care (GFHC) expenditures, through Medicare and Medicaid, have grown from roughly zero to over 7.5 percent of national income over the past 50 years. Recently, some advocates (e.g., the Council of Economic Advisers (2014)) have argued that an expansion of GFHC (in particular Medicaid) has large positive employment effects. Using quarterly data for 1978-2016, this paper estimates the impact of GFHC spending on prime-age employment using an instrumental variables strategy that exploits exogenous variation in Medicare spending. We show that the so-called relative (or local) multiplier approach based on the state-level panel provides similar estimates to those based on aggregate data. Although the employment effects using aggregate data are estimated imprecisely, they are considerably sharper when estimated using state-level data. Our baseline estimate of the multiplier suggests that an increase in GFHC spending equal to 1 percent of income over a two year horizon causes the employment-population ratio to increase by 58 basis points. This implies a job-creation cost of \$84,900 per job year. We then explore the dynamic employment response and estimate a four-year cumulative multiplier equal to 0.11, which implies a job-creation cost of \$448,000 per job-year. In other words, we find that an exogenous GFHC expansion has a moderate positive employment response in the short run and a muted cumulative response in the long run.

*Thanks to Mahdi Ebsim for helpful research assistance, Kathy Cosgrove for help with the data, and Lydia Johnson for editorial advice. For their useful comments, we also thank Peter McCrory, Valerie Ramey, Benjamin Sommers, Christian Wolfe and Barbara Klees, as well as audience members at the Federal Reserve Bank of St. Louis. The analysis set forth does not reflect the views of the Federal Reserve Bank of St. Louis or the Federal Reserve System. First draft: January 2017.

[†]Federal Reserve Bank of St. Louis, william.d.dupor@stls.frb.org, billdupor@gmail.com.

[‡]Federal Reserve Bank of St. Louis, rodrigo.guerrero@stls.frb.org.

1 Introduction

Government-financed health care (GFHC), through Medicare and Medicaid, has grown from roughly zero to over 7.5 percent of national income from 1966 to 2015.¹ National defense, by comparison, equals 3.6 percent of national income. In 2015, 44 percent of all medical care was financed by the federal and state governments.

The question of how GFHC spending affects the economy has come to the forefront as a policy issue as states have chosen whether to expand Medicaid as part of the Affordable Care Act (ACA). Some observers view the taxes used to finance the expansions as discouraging investment and work and, in turn, slowing the job creation rate. Moreover, the income ceiling for Medicaid eligibility may discourage the supply of labor, and public health insurance coverage may liberate individuals otherwise locked into employment to access employer-sponsored health insurance. Others claim that Medicaid expansion has caused employment booms in states that undertook it. In addition, if an increase in health care spending leads to an improvement in health and a reduction in disruptive health emergencies, then it could lead to an increase in employment and hours worked. Other channels that either positively or negatively affect employment are imaginable.

The existing evidence on GFHC's impact on labor market outcomes is inconclusive. The Council of Economic Advisers (2014) finds that one impact of the ACA Medicaid expansions would have been to increase employment by 520,000 job-years in 2014 and 2015 had every state chosen to expand its program. Along those same lines, Weller and Gelzins (2017) project that the repeal of the ACA Medicaid expansions would result in a loss of 1.7 million jobs through 2022. In contrast, Garthwaite, Gross and Notowidigdo (2014) study a large public health insurance disenrollment episode in Tennessee and find a positive effect on labor supply, primarily along the extensive margin. Similarly, Dague, DeLeire and Leininger (2014) find a sizable and statistically significant reduction in employment as a result of enrollment into public insurance, and Dave et al. (2015) estimate a negative effect of expansions in Medicaid eligibility on pregnant women's labor supply. Finally, others find no effect. In a randomized evaluation, Baicker et al. (2014) report no impact of Medicaid enrollment on labor supply or mean annual earnings. Similarly, Gooptu et al. (2016), Kaestner et al. (2017), and Leung and Mas (2016) find no significant impact of ACA Medicaid expansions on employment.

Some of the studies mentioned above focus on the impact of public health insurance coverage on an enrollee's labor supply. Others evaluate the impact of GFHC on state employment, capturing only local effects. This paper studies the macroeconomic effects of changes in GFHC spending; it answers two questions: (1) How do changes in GFHC spending influence national employment? (2) What can be learned about the first question using a disaggregate analysis?

Although Medicaid and Medicare are typically thought of as transfer programs, we focus on

¹The importance of GFHC to the economies of particular states is even more striking. Mississippi, New Mexico and West Virginia each have a GFHC-income ratio of nearly 0.12.

aspects of the programs that are more closely related to the literature on government spending. Government spending is traditionally defined as the National Income and Product Accounts' government consumption and investment, whereas Medicaid and Medicare are categorized as government transfers. Yet the distinction between government spending and transfers is notably blurred for GFHC. Medicaid and Medicare dollars are not paid to individuals, who in turn have broad latitude for using those payments. Rather, the payments are made by the government directly to medical care providers. While benefits are recorded as personal income in the form of transfer receipts, the spending by the government for Medicaid and Medicare is counted towards personal consumption expenditures.² The stimulative effect of government spending may differ from that of transfer payments because transfers allow individuals to save a portion of the payments.³

Fortunately, for the sake of empirical work, there are 50 years of data on the start-and-stop growth in GFHC spending from Medicare and Medicaid. Using macroeconomic data, we estimate the causal impact of a plausibly exogenous change in GFHC spending on employment. There are several reasons that this type of spending varies over time, including variation in the generosity provided by these programs, changes in enrollment requirements, and changes in the price of health care. Similarly, spending varies across states due to differences in population health status, prices and demographic characteristics. Some changes in GFHC spending are endogenous to the business cycle.

The fundamental source of endogeneity is enrollment in (and thus spending on) Medicaid. Medicaid is a means-tested social health care program that is jointly administered by the federal and state governments. Medicaid spending tends to be countercyclical. As newly unemployed persons fall below the program's income cutoffs, they are able to replace lost employer-provided health care with Medicaid. Failing to account for this endogeneity would downwardly bias the estimated employment effect of the program.

To correct for endogeneity implicit in using total GFHC spending as a treatment variable, we instrument using a component that is plausibly exogenous to the business cycle: Medicare spending. Medicare is a federal-government administered social insurance program for Americans age 65 or older as well as some younger people. Unlike Medicaid, program participation is largely determined by age.⁴ Medicare spending represents a commitment by the government to provide a certain amount and quality of health care to a set of individuals, in contrast to unemployment insurance benefits, for example, which give specified dollar transfers. Because of the qualitative nature of this social contract, overall Medicare spending fluctuates for a number of reasons. For example, as technological innovations, such as open-heart surgery, become covered by Medicare, government expenditure increases.

²See Mandel (2014) for greater detail.

³Romer and Romer (2016) study the macroeconomic effects of transfer payments by looking at changes in Social Security benefits. They find a positive response of consumption and no evidence of an employment response.

⁴Medicare spending does fluctuate for demographic reasons, which might affect employment; we use prime-age employment as the baseline dependent variable and add demographic controls in our regressions for this reason.

Our econometric approach, stated succinctly, is to regress changes in prime-age employment on changes in GFHC spending, using changes in Medicare spending as an instrument.⁵ Our estimate of the aggregate multiplier suggests a statistically insignificant causal effect of GFHC spending on employment. More specifically, a GFHC spending increase equal to 1 percent of national income accumulated over a two-year horizon causes the prime-age employment-population ratio to increase by 8 basis points (accumulated over that same horizon). While our estimate is not statistically different from zero, it is precise enough that we can reject a strong positive employment response after five quarters and a moderate positive response after nine quarters.⁶

Our econometric specification, which lumps Medicare and Medicaid spending together, implicitly assumes that Medicare spending influences the economy in a similar manner as Medicaid spending. While the populations served by Medicare and Medicaid are somewhat different, both forms of spending feed dollars into the medical care industry. Moreover, second round “Keynesian” effects of the spending are likely to be the same across the two spending types. While imperfect, the external validity assumption is at least as plausible here as in other places in the economics literature, e.g., that defense spending influences an economy in the same manner as government spending more generally.

Another concern regarding our instrumental variables specification is that if Medicaid and Medicare spending were uncorrelated, then our baseline second-stage estimation would be using variation only from Medicare spending; that is, the inclusion of Medicaid spending in our measure of GFHC would simply add noise. We do, however, find a positive correlation between changes in Medicare and Medicaid spending, and this effect is statistically different from zero in the disaggregate regression.⁷

Section 3.6 presents an alternative specification that circumvents both of these issues: we focus on the reduced-form regression to understand the employment effects of changes in Medicare spending only. Given that Medicare spending made up 54 percent of GFHC and 20 percent of total government spending in 2016, we argue that even if the external validity assumption is not satisfied and we cannot use the reduced-form results to draw conclusions about the stimulative impact of Medicaid expansions, our results still provide valuable evidence of the impact of government spending on economic activity.

Our second question asks what the estimated effect of GFHC would be if one uses disaggregate data to address the same issue. This question is interesting because a number of recent studies have used cross-sectional variation in fiscal policy to estimate the effect of policy on economic

⁵Two reasons justify our focus on labor market variables as a measure of economic activity. First, much of the debate around Medicaid expansion has been related to job creation. Second, while we initially attempted to also estimate aggregate income and output multipliers, these estimates lacked sufficient precision to be informative.

⁶In Section 3, we define a “moderate employment effect” as one associated with a job-creation cost of \$56,000 per job-year, the median employee compensation in 2016. Similarly, we define a “strong employment effect” as one associated with a job-creation cost of \$28,000 per job-year.

⁷Section 3.6 explores some channels through which Medicare spending may crowd in Medicaid spending.

activity.⁸ The estimates resulting from these studies are known as “relative multipliers” or “local multipliers.”⁹

While looking at disaggregate (such as state-level) variation at first may seem to dominate an aggregate approach because it provides additional data points, importantly, it informs policymakers about the relative effects of a policy across regions, but not necessarily the policy’s aggregate effect. Suppose, for example, that the government makes additional purchases in state X , but not in state Z . The relative multiplier approach would interpret the effect of the purchases as the difference in outcomes, in terms of employment or output, between states X and Z . If, however, spending in state X were financed with taxes on both states’ populations, then the distortionary impact of the taxes might induce a negative effect on state Z . The resulting relative multiplier would be an upwardly biased estimate of the aggregate multiplier because it would not account for the negative spillover on state Z .¹⁰

Alternatively, this spillover might be positive. Suppose that government purchases in state X increase that state’s residents’ income. If, in turn, state- X residents use some of their additional income to purchase more state- Z goods, then this would induce a positive spillover. The resulting relative-effects estimate would be an downwardly biased estimate of the aggregate multiplier.

One might conclude that, while aggregate multipliers are useful for macroeconomic questions, a regional or state policymaker would be satisfied to look at relative (or local) multipliers to answer the question of how the policy intervention will affect his or her own region or state. This would be incorrect. Going back to our interstate-trade example, suppose that government purchases in state X rise by \$1 and result in a \$2 increase in state- X income. Further assume that state- X residents use 25 cents of their additional income to purchase goods from state Z . Then the disaggregate data would show that state- X income and state- Z income increase by \$1.75 and \$0.25, respectively. Note that government purchases in state Z have remained unchanged. Thus, the relative multiplier would imply that a state’s income rises by \$1.50 ($= \$1.75 - \0.25) in response to a \$1 increase in government purchases in that state, understating the benefit of additional government spending there. Clearly, the presence of spillovers raises challenges for applying the relative/local approach for both macroeconomic and regional questions.

Papers that estimate relative multipliers usually include a caveat that relative multipliers cannot necessarily be interpreted as aggregate multipliers. Unfortunately, in public policy discussions, commentators often disregard this caveat and interpret estimated relative multipliers as evidence of the aggregate effects of fiscal policy.¹¹

⁸See, for example, Chodorow-Reich et al. (2012), Clemens and Miran (2012), Conley and Dupor (2013), Mian and Sufi (2012), Nakamura and Steinsson (2014), Suárez Serrato and Wingender (2014) and Shoag (2012).

⁹See Chodorow-Reich (2017) for a review of the expanding literature on relative fiscal multipliers.

¹⁰The issue of relative versus aggregate multipliers is related to the violation of the stable unit treatment value assumption in statistics. Cox (1958) states this as a requirement that “the [potential outcome] observation on one unit should be unaffected by the particular assignment of treatments to the other units.” We do not pursue that connection in the current paper.

¹¹See, for example, Boushey (2011), Greenstone and Looney (2012) and Romer (2012).

Whether and/or when this leap is OK is an open research question. We address this question by using an identical data set to estimate both relative multipliers and aggregate multipliers.¹² A primary reason that this type of analysis has rarely been undertaken may be because, without sufficient time-series variation, it is unclear how one might identify the spillover (and therefore the full aggregate) effect of fiscal policy without bringing significantly more economic structure to the problem.¹³

Note that looking at aggregate data subsumes (positive and/or negative) cross-region spillovers and thus avoids the relative multiplier problem. The effect on employment, estimated from aggregate data, provides a useful benchmark with which to compare the estimates from state-level data. This second approach uses both cross-state and cross-time variation to compute the employment multiplier.

Our second key finding is that the state-level, panel-based estimates, i.e., relative multiplier estimates, are similar in magnitude to the aggregate ones. In our benchmark specification, the two-year cumulative multiplier equals 0.58 (SE = 0.27). This point-estimate suggests a moderate positive employment effect and a job-creation cost of \$84,900 per job-year (2016 dollars). Because of increased precision using the state-level data, we are able to reject both negative and large positive effects on employment. In addition, beyond the nine-quarter horizon, we estimate a muted cumulative response and we are able to reject a moderate positive cumulative response. The similarity across the two approaches suggests that, at least in this instance, the relative multiplier approach may be informative about the aggregate multiplier.

The outline of the remainder of the paper is as follows. Section 2 presents the econometric model and discusses the Medicare spending instrument. Section 3 gives the empirical results. Section 4 discusses related research, and the final section concludes.

2 Econometric Model and Medicare Instrument

2.1 Aggregate Model

Let $N_{i,t}$ and $P_{i,t}$ denote the prime-age employment in and population of state i during quarter t . We construct the quarterly state-level seasonally adjusted prime-age employment series from the Current Population Survey (CPS) microdata, available from IPUMS-CPS.¹⁴ Let $Y_{i,t}$ and $G_{i,t}$ denote the real per capita quarter- t state- i income and GFHC spending, respectively. We use the core Consumer Price Index (CPI) to construct real values of their nominal counterparts. GFHC consists of payments made in the form of Medicare and Medicaid and are available from the state personal

¹²The only similar empirical paper in this regard is Dupor and Guerrero (2017).

¹³Acemoglu and Restrepo (2017), in a study of the effect of robots on jobs, handle the distinction between relative-versus-aggregate effects by augmenting a traditional cross-region comparison with an economic model that accounts for general equilibrium spillover effects.

¹⁴In this paper, prime-age refers to the ages 16 through 54. As explained later in this section, our exclusion of older workers is motivated by the confounding effects of demographic shifts on Medicare spending and employment.

income data published quarterly by the Bureau of Economic Analysis. The Medicare data consist of payments from the Hospital Insurance (HI) trust fund (Medicare Part A) and the Supplementary Medical Insurance (SMI) trust fund (Medicare Parts B and D). Greater detail on the components of Medicare and their financing sources appear in the next subsection. The Medicaid data consist of payments made to vendors for care provided to individuals under the Medicaid program.

Define $\bar{Y}_{i,t}$ to be the fitted value from a regression of real per capita income on a linear and quadratic trend. Let national prime-age employment be $N_t = \sum_i N_{i,t}$; other aggregate variables are defined similarly.

Next, define the following aggregate variables: Let $N_{t,\delta}^c$ be the cumulative increase in national prime-age employment over a δ -quarter horizon relative to a quarter $t - 1$ baseline, all of which is scaled by population P_{t-1} :

$$N_{t,\delta}^c = \left(\sum_{j=1}^{\delta} N_{t+j-1} - \delta N_{t-1} \right) / P_{t-1} \quad (2.1)$$

Defining cumulative variables allows us to study the accumulated response of employment to GFHC spending. Ramey and Zubairy (2018) argue compellingly that cumulative multipliers are more useful from a policy perspective than other (sometimes reported) statistics, such as peak multipliers and impact multipliers. Cumulative multipliers take into account both the full employment increase and full cost of government spending across time. It is useful to keep in mind the relationship between growth rates and cumulative growth rates. If a variable grew steadily each quarter by a total of 1 percent over a year, the corresponding cumulative growth rate would equal 2.5 percent.¹⁵

Similarly, let $G_{t,\delta}^c$ be the cumulative increase in GFHC spending over a δ -quarter horizon relative to a quarter $t - 1$ baseline, all of which is scaled by trend income \bar{Y}_{t-1} :

$$G_{t,\delta}^c = \left(\sum_{j=1}^{\delta} G_{t+j-1} - \delta G_{t-1} \right) / \bar{Y}_{t-1} \quad (2.2)$$

Thus, at the aggregate level, the second-stage equation is

$$N_{t,\delta}^c = \alpha_{\delta} + \phi_{\delta} G_{t,\delta}^c + \beta'_{\delta} X_t + \gamma'_{\delta} S_t + \eta_{\delta} R_t + v_{t,\delta}, \quad (2.3)$$

where X_t consists of four lags of the change in Shiller's cyclically adjusted price-earnings ratio, S_t is a vector of changes in the share of the resident population age 65 or older at various leads,¹⁶ and R_t is the Ramey defense-spending news variable scaled by last-period's trend income.

¹⁵We arrive at 2.5 percent as the sum, $0.25 + 0.50 + 0.75 + 1$. The above calculations ignore compounding, the effects of which would be small, given the small rates we are using and the limited short horizons over which we calculate growth rates.

¹⁶Specifically, $S_t = [s_t, s_{t+7}]$, where s_t is the change in the share of the population age 65 or older between quarters t and $t - 8$.

In our baseline specification, we set $\delta = 8$. In every case, we examine $\delta \geq 4$. By the form it takes, equation (2.3) implements the Jordà (2005) local projections approach. The choice of δ amounts to choosing the frequency for our analysis. Choosing a large δ would reduce the number of observations available and limit our ability to say anything about the short (and potentially intermediate) responses of employment to GFHC spending. On the other extreme, setting $\delta = 1$ would imply we are using only quarter-to-quarter growth in the variation of the GFHC-income ratio. This would provide insufficient variation in the treatment to precisely identify the causal impact. For example, the standard deviation of $G_{t,1}^c$ is only 0.05 percent. However, because quarter-to-quarter GFHC-income growth is serially correlated, we can increase the variation by choosing an intermediate value of δ . At our benchmark value of $\delta = 8$, we lose the ability to say anything about the very short-run employment response to GFHC spending, but the volatility of the treatment variable is much higher: the standard deviation of $G_{t,8}^c$ equals 1.9 percent.

The standard errors of our estimates reflect the degree of variation in the instrument and endogenous variable; however, one might be concerned that GFHC changes simply do not fluctuate enough to identify the multiplier. Here a comparison is useful. One common instrument used in fiscal multiplier research is military spending.¹⁷ This research has produced precise and reliable estimates of the government spending multiplier. If we replace national GFHC spending with national defense spending in equation (2.2), we can compute the standard deviation of this variable (which is also available at the quarterly frequency). Call this new time series $D_{t,\delta}^c$. For the 1976-2016 period, the standard deviation of $D_{t,8}^c$ equals 2.9 percent.¹⁸ Thus, cumulative GFHC spending growth fluctuations are of a similar, albeit slightly smaller, magnitude of the commonly used defense-spending variables.

We choose prime-age employment, rather than total employment, as our dependent variable because the latter is more likely to be correlated with demographic changes that simultaneously drive Medicare expenditures. Specifically, consider a positive shock to the birth rate (e.g., the 1940's "baby boom"): as "baby boomers" reach retirement age, we would simultaneously observe an increase in Medicare spending and a decrease in employment.¹⁹ Hereafter, unless otherwise stated, employment refers to the number of people ages 16 through 54 that are employed.

The coefficient ϕ_δ is then the cumulative increase in the employment-population ratio through horizon δ in response to an increase in national GFHC spending (cumulative through horizon δ) equal to 1 percent of national income.

We include the change in the share of the U.S. population age 65 or older in our baseline estimation to control for any additional confounding effects of demographic shifts on Medicare spending and employment. We also include lags in the change of the price-earnings ratio because

¹⁷See, for example, Barro and Redlick (2011), Hall (2009) and Ramey (2011).

¹⁸The corresponding value for the period 1951-2016, which importantly includes both the Korean and Vietnam wars, is 5.5 percent.

¹⁹For most of our sample, 65 was the full-benefit retirement age in the United States, which coincides with the age of automatic enrollment in Medicare.

it is often viewed as a leading economic indicator for the business cycle. Including the Ramey news variable controls for any budgetary pressure that increases in defense spending might have on spending on social programs, such as Medicare and Medicaid.

GFHC spending changes are likely correlated with the error term because one of its components is Medicaid. Failing to account for the likely endogeneity of Medicaid spending would lead to a downwardly biased estimate of GFHC's employment effect. As such, we seek an instrument that is correlated with this endogenous variable and that is uncorrelated with the error term. We use the accumulated change in Medicare spending through quarter eight, $Z_{t,8}^c$, as an instrument for cumulative changes in the sum of Medicare and Medicaid spending, $G_{t,8}^c$.

Note that the cumulative horizon over the dependent variable, endogenous variable and the instrument are identical. Having the dependent variable and endogenous variable so aligned is standard in this line of research (see, for example, Ramey and Zubairy (2018)). It eases the interpretation of the corresponding regression coefficient as a cumulative response. Having the instrument share the same horizon as the endogenous variable is slightly less standard, although the approach is taken by Nakamura and Steinsson (2014). Using a forward-looking variable for the instrument ($Z_{t,8}^c$) is akin to assuming perfect foresight for the instrument. That is, there is no need to introduce a forecasting procedure for the instrument as part of the identification.

2.2 The Medicare Spending Instrument

This section justifies the use of Medicare spending as an instrument for GFHC spending. First, we provide background information regarding the composition of the Medicare program and its financing sources. Here, we examine how much of our data should be considered government spending and identify points of potential concern regarding the satisfaction of the exclusion restriction.

Second, we describe the historical drivers of Medicare spending since its inception. The analysis will show that Medicare spending changes have been caused mainly by factors that are plausibly exogenous to the business cycle.

Third, we consider and address several mechanisms by which our use of Medicare spending could violate the exclusion restriction.

2.2.1 Medicare Composition and Financing

The Medicare program originally consisted of only two subprograms: Medicare Part A and Medicare Part B. Part A, also known as Hospital Insurance, provides beneficiaries with coverage of inpatient hospital services, skilled nursing facility services, home-health visits and hospice services. Medicare Part A is available to almost everyone age 65 or older and is premium-free for most of those age eligible; a far smaller number of people under age 65 are covered (those having certain disabilities). That is, enrollment in Part A of Medicare is largely determined by age and not by business cycle conditions.

Part B of Medicare, on the other hand, covers medical services, such as physician services, laboratory services, durable medical equipment, and outpatient hospital services. Though enrollment is voluntary and requires a monthly premium, most beneficiaries with Part A also enroll in Part B.²⁰ In 2016, 92.3 percent of Part A beneficiaries were enrolled in Part B (The Boards of Trustees, 2017). The fact that Medicare Part B has a premium component could potentially result in enrollment (and spending) being procyclical. In Section 2.2.3 we argue this is not the case.

In an attempt to expand beneficiaries' choices of health insurance plans beyond traditional Medicare and to benefit from the efficiencies and cost saving of the private sector, private insurance plans were introduced into the Medicare program in 1985. This initiative, known as Part C or Medicare Advantage, provides private-plan options to individuals already enrolled in both Part A and Part B. In 2016, 32.4 percent of Medicare beneficiaries chose to obtain their benefits from a Medicare Advantage plan (The Boards of Trustees, 2017). Note that these individuals still get complete Part A and Part B coverage.

Finally, Medicare Part D began operating in 2006. This program finances outpatient prescription drugs. In 2016, 76.0 percent of all Medicare beneficiaries were enrolled in Medicare Part D (The Boards of Trustees, 2017).

Figure 1 shows the national enrollment in each part of Medicare since the program's inception in 1966. It is important, for the purpose of this paper, to identify and understand the different parts that make up the Medicare program because each part is financed differently. As explained in Section 2.2.3, the source of financing may play a role in evaluating the exogeneity of Medicare spending.

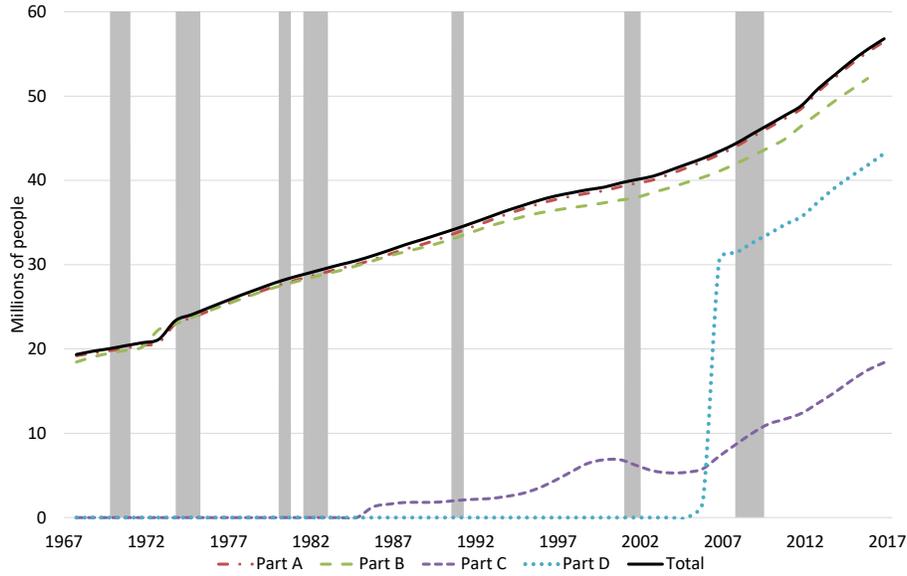
Direct government contributions to the Medicare program operate through two trust funds: the HI trust fund and the SMI trust fund. The Boards of Trustees for Medicare oversee the financial operations of these funds. The HI trust fund finances Medicare Part A, whereas the SMI trust fund finances Medicare Parts B and D. Before we discuss how Part C is financed, we describe the revenue sources of these two trust funds.

The HI trust fund is designed to be self-supporting, and its main source of revenue consists of payroll taxes paid by employees and employers. Specifically, each pays 1.45 percent of an employee's taxable earnings and self-employed individuals pay 2.9 percent (2017 rates). Additional revenue sources include a portion of the federal income taxes paid on Social Security benefits, interest on federal securities held by the trust fund, and premiums paid by voluntary enrollees who are not eligible for the premium-free Medicare Part A.

The SMI trust fund, on the other hand, is not self-supporting, and it is mainly financed by a combination of general revenues and monthly premiums. The SMI fund is divided in two accounts for Part B and Part D. The Part D account additionally receives payments from state governments. These payments represent a portion of the amounts states would have been expected to pay for

²⁰In 2017, the standard Part B monthly premium was \$134; starting in 2007, higher-income beneficiaries pay higher premiums.

Figure 1: Medicare enrollment by parts



Notes: Shaded regions indicate NBER-dated recessions.

Source: Annual Reports of Trustees of the Federal Insurance Supplementary Medical Insurance Trust Fund; Medicare Managed Care Contract Plans Monthly Summary Report.

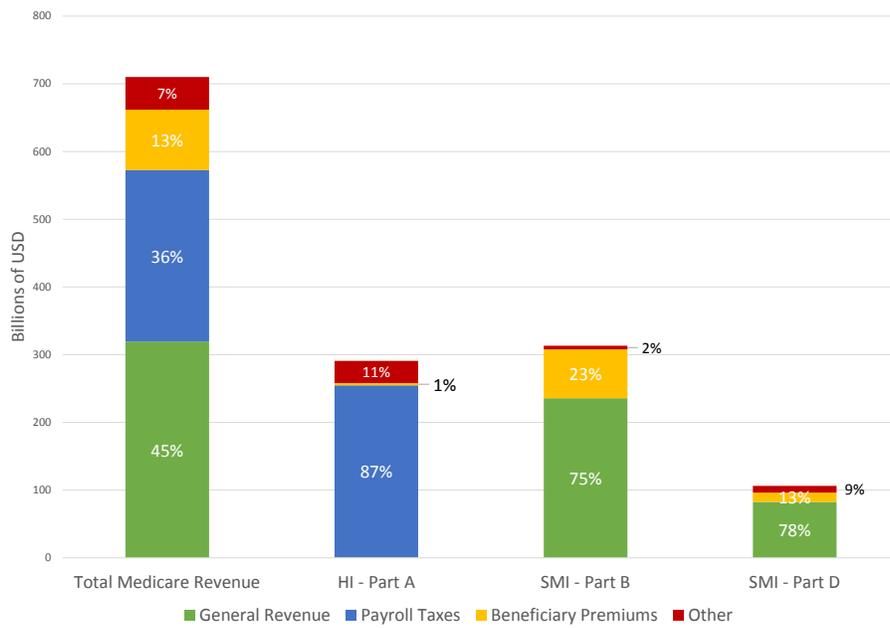
drugs under Medicaid if drug coverage for individuals eligible for Medicare and Medicaid had not been transferred to Part D. As of 2011, high-income Part D beneficiaries are required to pay higher premiums.

Figure 2 shows the revenue sources of the HI trust fund and the SMI trust fund.

Medicare Part C is provided privately and is financed as follows: private insurance plans receive a fixed, monthly, risk-adjusted subsidy per enrollee from the HI trust fund and the SMI trust fund in appropriate parts. This amount is specific to each county and is primarily determined by a benchmark/bidding process that uses that county’s average per-beneficiary cost of Medicare Parts A and B. In addition, beneficiaries typically pay a premium to the private plans. Recall that Part C beneficiaries are enrolled in Parts A and B as well, so they are required to pay the Part B premium and the Part D premium, if enrolled in that plan.

Given that Medicare spending is partially financed with beneficiaries’ premiums, one may wonder how much of our data is actual government spending. However, in 2016, only about 12.3 percent of Medicare spending was financed by premiums. For further detail on this concern, see Section 2.2.3.

Figure 2: Sources of Medicare revenue, 2016



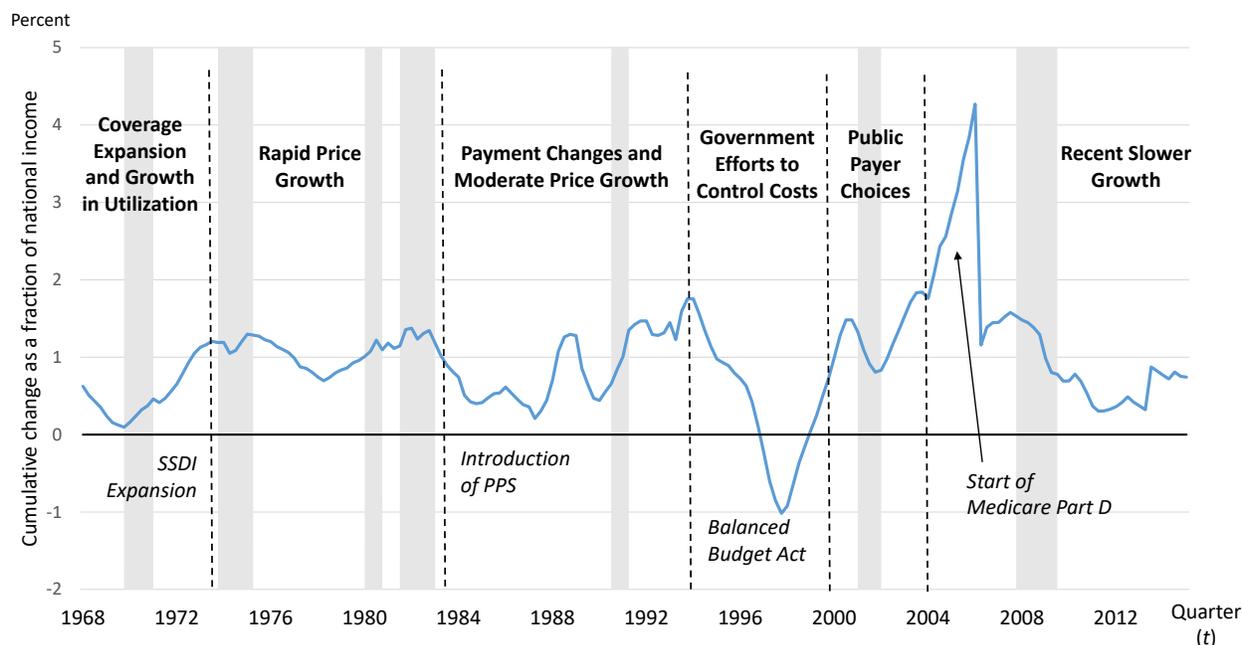
Notes: HI refers to the Hospital Insurance trust fund, and SMI refers to the Supplementary Medical Insurance trust fund.

Source: 2017 Annual Report of the Boards of Trustees of the Federal Hospital Insurance and Federal Supplementary Medical Insurance Trust Funds.

2.2.2 Historical Drivers of Medicare Spending

We divide the history of Medicare into six periods, roughly following the demarcations used by Catlin and Cowan (2015) in their detailed history of health spending in the United States.²¹ Figure 3 contains the time series for our instrument: the eight-quarter accumulated change in Medicare spending between quarters $t - 1$ and $t + 7$ as a percentage of trend income at quarter $t - 1$.

Figure 3: Accumulated two-year change in Medicare spending as a fraction of national income



Notes: The figure plots $Z_{t,8}^e$, the accumulated change in Medicare spending between quarters $t - 1$ and $t + 7$ as a fraction of trend national income. Shaded regions indicate NBER-dated recessions. As explained in Section 2.1, the cumulative variables are constructed by accumulating over quarters and should not be interpreted as annual growth rates. SSDI is Social Security Disability Insurance, and PPS is Prospective Payment System.

Coverage Expansion and Growth in Utilization (1966-1973)

The Medicare program, along with Medicaid, went into effect in July 1966. The passage of the enabling legislation was motivated by public concerns that a significant fraction of those over age 65 lacked health insurance. The lack of coverage for this age group in the pre-Medicare period was likely due to the high premiums private insurers charged this group. Since inception, Medicare covers these individuals without regard to medical history, income or health status. Nowhere in discussions of the beginning of Medicare did we find evidence that it was in response to an economic downturn or a way to increase employment.

²¹We do not use exactly those authors' period definitions because their divisions correspond to national health care spending rather than Medicare in particular.

For the years between 1966 and 1969, Catlin and Cowan (2015) cite increased utilization of services, increases in hospital costs and wider use of skilled nursing facilities as primary sources of the rapid growth in program spending. In the following three years, Medicare expenditures continued to increase at a great rate, particularly because of the cost of providing outpatient hospital services.

In 1973, Medicare was expanded to cover people with permanent disabilities who are younger than age 65. Through this expansion, Social Security Disability Insurance (SSDI) beneficiaries become eligible for Medicare coverage two years after they begin receiving SSDI payments.²²

Rapid Price Growth (1974-1982)

The CPI for medical care outpaced overall CPI for most of this period, which largely accounts for the Medicare spending growth over this period.²³ Besides medical care price growth, expanded coverage for the disabled (in late 1973) also caused increased Medicare spending, according to Catlin and Cowan (2015).

Non-price factors also contributed to growth of medical care spending in general. Regarding that era, Gibson (1980, p.4) writes that “increased concern over liability for malpractice has contributed to the number and complexity of diagnostic series performed, adding to the cost of physicians’ services.” In addition, influenza epidemics between 1979 and 1981 along with a 1980 heat wave contributed to increased use of medical services.

Another factor contributing to fast Medicare spending growth was the relatively rapid increase in enrollment of disabled and end-stage renal disease patients, both intense users of health care.²⁴

Payment Changes and Moderate Price Growth (1983-1992)

Between 1966 and 1982, Medicare expenditures became a larger and larger fraction of federal government spending and of the economy overall. Gornick et al. (1985, p.16) cite a host of reasons for increased costs of health care during the first 20 years of Medicare: “the rise in wages and price levels in the health care industry; increases in the number of certain customary services such as laboratory tests; the development of new and costly medical technologies such as open-heart surgery; changes in the organization of care, such as the growth of intensive care units in hospitals and increases in personnel; and the growth of institutions for long-term care.”

Concern about this growth led to the Tax Equality and Fiscal Responsibility Act of 1982. One outcome of the legislation was the implementation of a new Medicare payment system (PPS) in which payments for services were made according to predetermined costs for treatment depending on specific diagnoses.²⁵ While the law’s passage was associated with an attempt to reduce spending

²²Eventually, exceptions to the two-year waiting rule were enacted for a few diseases: end-stage renal disease and amyotrophic lateral sclerosis (Cubanski, Neuman and Damico (2016)).

²³Because the data in Figure 3 are detrended by the overall CPI, the high economy-wide inflation does not contribute to the Medicare spending growth exhibited in the figure.

²⁴See Gibson and Fisher (1978).

²⁵Using around 400 diagnostic categories, Gibson et al. (1984, p.22) write that, beginning in 1983, “hospitals will be paid based on the diagnosis group into which a patient falls, regardless of services provided or of length of stay.”

growth, it was not undertaken in response to a recession or fluctuations in employment.

Davis and Burner (1995) explain that, before PPS, hospital payments from Medicare grew at over 18 percent per year. The effect of introducing PPS was dramatic. Inpatient hospital spending slowed to 5.7 percent per year in the six years that followed. The authors state that “These changes were prompted by concerns about trust fund solvency and about equity in compensation” (p. 233). Moreover, in the next section, we show that the changes in deficits and the debt are generally poor predictors of future values of our Medicare instrument.

Between 1983 and 1985, there were temporary freezes on physician fees and changes in laboratory fee schedules to reimburse at 60 percent of prevailing charges.²⁶ Taken together, these policy shocks had a substantial effect on the rate of Medicare expansion in the mid-1980s.

Besides Medicare program changes, technological developments shifted treatments from more-expensive inpatient to outpatient procedures. These included new less-invasive procedures and improved diagnostic tools, such as MRIs. These changes are evident in our instrument series, plotted in Figure 3.

Government Efforts to Control Costs (1993-1999)

Figure 3 shows a sharp decline in $Z_{t,8}^c$ starting at $t = 1997$. This is the only period when the variable becomes negative for a sustained number of quarters. The change was largely due to government cost-control efforts implemented through the Balanced Budget Act of 1997 (BBA). It reduced or fixed payment amounts for most services, including a freeze on Medicare payments for inpatient hospital admissions (see Catlin and Cowan (2015)). Levit et al. (2003, p.160) explain that a “mandated conversion from a cost-based reimbursement system to a prospectively determined payment system precipitated a decline of \$19 billion in Medicare payments in 1999.” Note that the BBA was enacted at a time when neither federal deficits were abnormally high nor the economy was in a recession. Thus, it would be difficult to argue that this effectively negative spending shock was an endogenous response to macroeconomic conditions.

According to Savord (1999), efforts in the late 1990s to reduce fraud and abuse also contributed significantly to lower expenditure growth in the home-health category and inpatient hospital costs.²⁷

Public Payer Changes (2000-2002)

As discussed above, the BBA was intended to limit Medicare (and Medicaid) spending growth. The effect of the law was so severe that it led to changes in some of the law’s provisions through new legislation, passed in 1999 and 2000.²⁸ The laws stopped or delayed some of the BBA’s payment reductions. According to Catlin and Cowan (2015, p.21), the severity of parts of the BBA “coupled with expanding Federal budget surpluses, led to the passage of these two laws.”

²⁶See Gibson et al. (1984).

²⁷See also Foster (2000).

²⁸These were the Balanced Budget Refinement Act of 1999 and the Benefits Improvements and Protection Act of 2000.

Since this evidence suggests the possibility that this backlash period was in part due to expanding budget surpluses, and thus the state of the business cycle, we will investigate the influence of this period. We show that the addition of the deficit-income ratio as a control does not affect our qualitative results. Finally, even if there was a residual endogeneity bias, the direction of that bias would be that of a stronger stimulative response to a GFHC expansion. In other words, if there was such a bias, correcting for it would result in an even smaller employment response.

Recent Slower Growth (2003-2016)

Generally, Medicare spending growth slowed over this period. In part, this was due to slower increases in retail prescription drug expenditures as a larger percentage of dispensed drugs became generic and thus much less costly. Moreover, the number of new-product introductions (which tend to be expensive initially) slowed during these years. We contend that these features are exogenous to the business cycle.

One source of a dramatic expenditure increase that affected $Z_{t,8}^c$ is the implementation of Medicare Part D. The program, enabled by the Medicare Modernization Act of 2003, subsidized the cost of prescription drugs and prescription drug insurance premiums for most Medicare participants. Unlike the Medicaid program, there were no major changes in federal Medicare funding as a response to the 2007-2009 recession.

2.2.3 The Exclusion Restriction

We now consider and address seven potential channels through which our use of Medicare spending as an instrument for GFHC could violate the exclusion restriction. We argue that these channels are likely inconsequential.

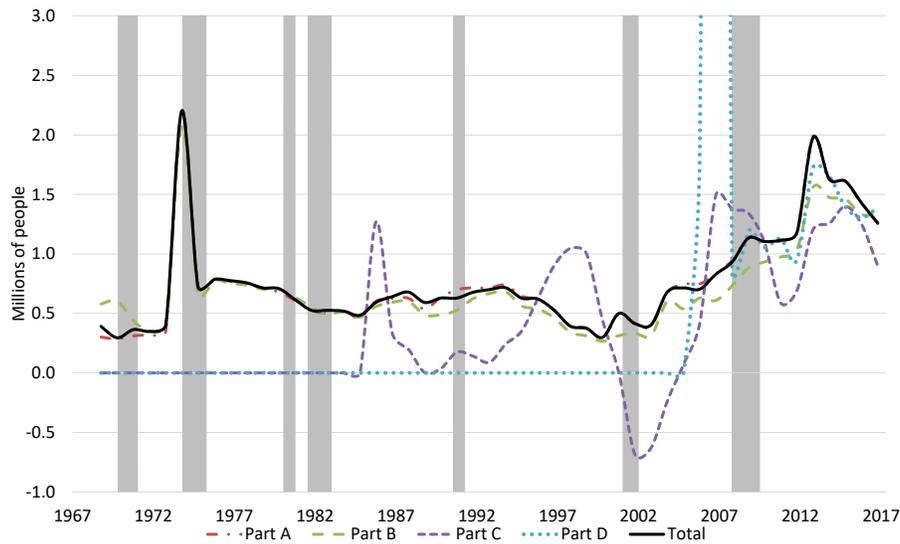
Premiums and Enrollment

As explained in Section 2.2.1, some components of Medicare, such as Part B, require enrollee premiums. One might then be concerned that instrument exogeneity could be violated according to the following reasoning. Suppose that, during an economic downturn, enrollees who cannot afford the premiums decide to drop Part B coverage. By this potential channel, macroeconomic conditions could cause a decrease in one component of government contributions to Medicare spending.

There is good reason, however, to think that this channel is not operative. First, the parts of Medicare that involve premiums are highly subsidized; thus, it is unlikely that many enrollees would give up the subsidy by unenrolling. Second, the majority of Medicare recipients are retirees; thus, their income is somewhat insensitive to economic conditions.

To get a sense of the importance of endogenous disenrollment, Figure 4 plots the year-over-year changes in enrollments by part, with the shaded regions indicating NBER-dated recessions. There is no systematic tendency for enrollments in any part to decline (or increase) during recessions in ways that differ from slow-moving trends.

Figure 4: Change in Medicare enrollment by parts



Notes: In 2006, Part D enrollment increased by 28.7 million. Shaded regions indicate NBER-dated recessions.

Sources: Annual Reports of Trustees of the Federal Insurance and Federal Supplementary Medical Insurance Trust Funds; Medicare Managed Care Contract Plans Monthly Summary Report.

There is one decline in enrollments in Part C (the dashed purple line) around the 2001 recession. This resulted from *involuntary* unenrollments caused by the BBA. As explained in Section 2.2.2, the more drastic aspects of that act were rescinded by later legislation. Part C enrollments eventually began increasing, as evidenced by the figure.

Finally, note that there is a slowdown in total enrollment that coincides with the 1973-1975 recession. This is a product of the spike in new enrollees caused by the expansion in coverage to SSDI beneficiaries in 1973. As explained in the previous section, this policy was not motivated by business cycle conditions.

Although economic downturns may not significantly affect Medicare enrollment, one may still be concerned that, because of out-of-pocket expenses, beneficiaries might cut back on their medical care usage. The majority of beneficiaries, however, have some form of supplemental insurance that lowers out-of-pocket expenses. Levine and Buntin (2013) estimate that, between 2008 and 2010, the median out-of-pocket spending for a Medicare beneficiary (excluding premiums) was less than 5 percent of that individual's income.

Health Status and Business Cycles

A strand of the health economics literature has focused on the impact of business cycles on health, health-related behaviors and mortality rates. Note that if, for example, recessions worsened the health of Medicare beneficiaries, then this might drive up spending on the program, contam-

inating our instrument. Alternatively, if the Medicare population engaged in healthier behavior during economic downturns, then this might result in reduced medical care usage, which in turn would decrease Medicare spending.²⁹

Estimates on the impact of recessions on health, however, are inconclusive. Ruhm (2000) finds that total mortality is procyclical. He also reports that strong economic conditions are associated with an increase in smoking and obesity as well as a reduction in physical activity. Other studies find similar results regarding the procyclicality of mortality rates.³⁰ In contrast, Sullivan and von Wachter (2009) and McInerney and Mellor (2012) report that joblessness is associated with an increase in mortality rates.

Even if business cycles do influence health outcomes, note that we are mainly concerned about whether this would translate into changes in medical care usage by the Medicare population. Several studies that report procyclicality of mortality rates rule out increased medical care as the channel that links reduced mortality and recessions (McInerney and Mellor, 2012). Similarly, Levine and Buntin (2013) estimate the effects of changes in wealth and income on elderly Medicare beneficiaries' use of health care services and found no significant relationship.

To address this potential source of concern, one of our robustness checks adds state-level changes in the age-adjusted mortality rate in the 65+ population to our baseline specification. The results of this exercise show a negligible change to our baseline relative multiplier estimate. Although the aggregate multiplier estimate is closer to zero, it is well within the 95 percent confidence interval of our baseline estimate. Thus, we contend that the cyclicity of health status (proxied by mortality rates) is unlikely to lead to a violation of the exclusion restriction.

Medicare Spending and Supplier-Induced Demand

Another strand of the health economics literature focuses on supplier-induced demand, i.e., the demand in excess of what would exist if patients had the same information as providers. Note that if changes in Medicare spending were driven by supplier-induced demand, one would worry that this induced demand might be affected by the business cycle. In other words, physicians and health care institutions may attempt to offset the negative impact of recessions by overbilling patients.³¹ This would lead to a downwardly biased estimate of GFHC's employment effects.

We first note that research on the extent of supplier-induced demand in the health care sector is inconclusive. Although some studies (e.g., Delattre and Dormont (2003)) have found evidence of physician-induced demand, others (see, for example, Grytten and Sørensen (2001)) have found no such evidence. Moreover, even among studies that report supplier-induced demand in the health care sector, it is not clear whether this excess demand results from medical providers' readiness to increase their revenue. Richardson and Peacock (2006), for instance, argue that physicians' "asym-

²⁹Note that this would bias our results toward finding a stronger employment response to GFHC spending.

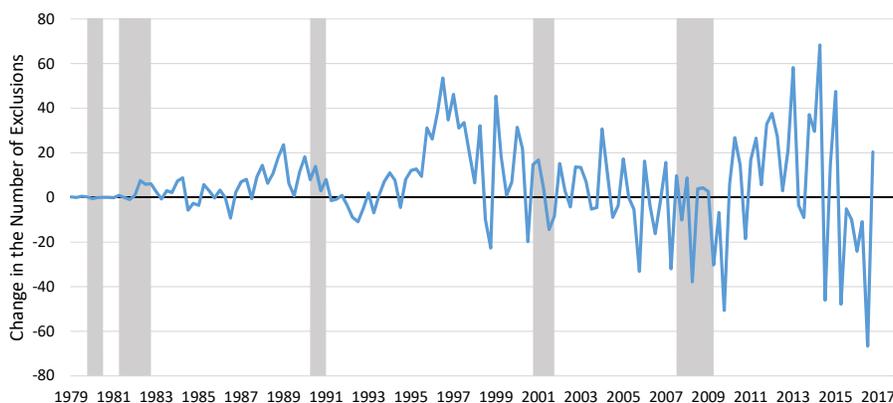
³⁰See, for example, Lin (2009), Miller et al. (2009) and Stevens et al. (2015).

³¹By overbilling, we mean several unlawful and unethical practices that range from recommending unnecessary medical procedures to billing for services or supplies that were not provided.

metrical ability and willingness to exercise judgement in the face of uncertainty” is an important driver of supplier-induced demand in health care. Note that this channel is unlikely to be affected by business cycle conditions. Therefore, the presence of supplier-induced demand in the health care sector does not necessarily imply that Medicare spending would increase in response to an economic downturn.

Next, to address this potential concern, Figure 5 shows the change in the number of health care providers excluded by the Office of Inspector General from receiving Federal government payments because of overbilling and other cases of malpractice.³² If overbilling of Medicare patients were more pervasive during economic downturns (consistent with the supplier-induced demand argument), we would expect to observe an increase in exclusions during recessions. Figure 5, however, shows that there is no systematic tendency for exclusions to increase (or decrease) during recessions in ways that differ from slow-moving trends.

Figure 5: Exclusions from federally funded health care programs



Notes: We counted the number of exclusions in each quarter and then took a two-year moving average of this series. The figure shows the quarter-to-quarter change in the resulting series. Shaded regions indicate NBER-dated recessions.

Source: Office of Inspector General, U.S. Department of Health and Human Services.

Medicare Participation through Disability

In addition to the elderly, individuals receiving SSDI are permitted to receive Medicare benefits. Besides the medical prerequisites, SSDI eligibility requires that participants must not engage in “substantial gainful activity,” which sets a ceiling on monthly earnings.³³ This could potentially introduce endogeneity to Medicare spending. The conceivable chain of events would be as follows. The economy enters into recession, and SSDI participation increases from younger individuals who

³²The Office of Inspector General imposes exclusions under the authority of sections 1128 and 1156 of the Social Security Act for, among other related reasons, “claims for excessive charges or unnecessary services.”

³³In 2017, this cap was \$1,170 per month.

lose their jobs or have poorer employment prospects. New SSDI recipients in turn join Medicare.

There are two reasons this channel is likely inconsequential for our results. First, the fraction of Medicare participants under age 65 is small (especially early in the sample). Cubanski, Neuman and Damico (2016) report that this share made up only 7 percent of Medicare in 1973 and 16 percent in 2016. Second, for almost all participants under age 65 there is a two-year waiting period between first receiving SSDI and the later Medicare coverage. Since we primarily examine the two-year cumulative multiplier, alternative shocks that drive the business cycle would not move Medicare spending through the SSDI channel until beyond our primary multiplier horizon.³⁴

Dual Eligibility

Some individuals are eligible for enrollment in both Medicare and Medicaid. This group mostly consists of low-income elders, who may exhibit chronic conditions and require costly care above the average for non-dual-eligible individuals. In 2008, the dual eligible comprised 20 percent of Medicare enrollees but 31 percent of Medicare spending, and 15 percent of Medicaid enrollees but 39 percent of Medicaid spending (Jacobson, Neuman and Damico, 2012).

Note that the dual-eligible population contributes to the positive correlation between Medicare and Medicaid spending: if an individual enrolled in both programs is in need of medical care, then both Medicare and Medicaid spending are likely to increase. This speaks to the relevance of our instrument and should not be taken per se as evidence against the exogeneity of Medicare spending.

Other nuances regarding the dual-eligible population may, however, pose a potential threat to the exogeneity of our instrument. Consider how Medicare spending would change if a Medicare beneficiary is suddenly eligible for Medicaid as a result of a drop in his or her income. First, one could worry that the newly available Medicaid coverage may act as a substitute for Medicare coverage; that is, a portion of this individual's medical care would now be covered under Medicaid, reducing Medicare spending. This scenario, however, would not occur because the Medicaid program is the payer of last resort, and so, for dual-eligible beneficiaries, Medicare pays first (Centers for Medicare and Medicaid Services, 2017).

Second, note that if a Medicare beneficiary becomes eligible for Medicaid, his or her health insurance coverage has effectively improved, potentially resulting in smaller co-payments and out-of-pocket expenses. This improved coverage could incentivize individuals to seek medical care that was previously unaffordable. This mechanism, nonetheless, is unlikely to be quantitatively meaningful because most Medicare beneficiaries are retirees, and thus their poverty status is not a function of business conditions. Therefore, it is improbable that a substantial share of Medicare participants would enroll and disenroll in Medicaid in response to macroeconomic fluctuations.

Medicare Spending and Retirement

The Social Security Administration's full retirement age has been between 65 and 67 throughout

³⁴The two-year waiting period is, effectively, actually 29 months because there is a five-month waiting period between applying for and first receiving SSDI benefits.

our sample.³⁵ Importantly, 65 is also the age at which an eligible individual is automatically enrolled in Medicare Part A. That is, an individual is likely to exit the labor force around the same time that he or she starts receiving Medicare benefits. This connection is important for two reasons.

First, the generosity of Medicare may affect the willingness of those over age 65 to remain in the labor force. In other words, a reduction in the generosity of Medicare benefits (and thus a reduction in Medicare spending) could incentivize individuals to postpone retirement, which would have a positive effect on total employment. There is, in fact, evidence that health insurance is a central determinant of retirement decisions.³⁶ Note, however, that this does not imply a violation of the exclusion restriction. It is simply a channel through which Medicare spending affects total employment. Moreover, a similar channel is also potentially operative in the case of Medicaid benefits: a reduction in Medicaid generosity may disincentivize individuals from quitting a job that sponsors health insurance. Thus, this point should not be considered a violation of the external validity assumption either.³⁷

Second, as explained in Section 2.1, shifts in the population’s age distribution may simultaneously affect total employment and Medicare spending, biasing our results downward. The rationale, once again, is that an increase in the share of the age 65+ population drives up Medicare spending and reduces the employment-population ratio. To address this issue, we choose prime-age employment, and not total employment, as our benchmark dependent variable.

Medicare Spending and Federal Budget Stress

Next, we ask if Medicare spending responds to overall federal budget stress. If, for example, a recession causes federal tax revenues to fall, then that could potentially cause a reduction in Medicare spending. In this case, our instrument might suffer from endogeneity. Note, importantly, that this would bias our results towards finding a strong positive employment multiplier, which we do not find.

Table 1 contains estimates in which our Medicare variable $Z_{t,8}^c$ is the dependent variable and our independent variable is one of several different measures of budget stress. In each regression, we include the baseline controls. To ease interpretation, we standardize each variable to have unit variance; therefore, the coefficient of interest can be interpreted as the number of standard deviations of the left-hand-side variable in response to a 1-standard-deviation change in the right-hand-side variable.

Column (1) contains the coefficient on the Romer-Romer exogenous tax shock. Column (2) regresses the instrument on the $t - 1$ deficit-income ratio. Column (3) uses the change in the $t - 1$ deficit-income ratio. The coefficients in columns (1) and (3) are less than 0.10 in absolute value and not statistically different from zero. Column (4) uses the lagged debt-income ratio. The

³⁵As of 2018, a person may start receiving Social Security benefits as early as age 62 or as late as age 70.

³⁶See Gruber and Madrian (2004) for a review of the literature.

³⁷Any remaining concerns about the impact on Medicare generosity on retirement decisions should be mitigated by our use of prime-age employment, and not total employment, as the dependent variable.

Table 1: Effect of various measures of fiscal stress on Medicare spending, aggregate regressions with standardized coefficients

	(1)	(2)	(3)	(4)
	Coef./t-stat	Coef./t-stat	Coef./t-stat	Coef./t-stat
(a) Romer-Romer tax shock	-0.02 [-0.27]	-	-	-
(b) Deficit-income ratio $_{t-1}$	-	0.31** [2.49]	-	-
(c) Change in deficit-income ratio $_{t-1}$	-	-	0.07 [0.94]	-
(d) Debt-income ratio	-	-	-	-0.13 [-1.53]
N	102	113	113	113

Notes: t-statistics appear in brackets beneath the standardized coefficient and are robust with respect to heteroskedasticity and autocorrelation. Each regression includes the benchmark controls.

* $p < .1$, ** $p < .05$, *** $p < .01$.

coefficient is negative, which implies high debt correlates with slow Medicare spending growth. The effect is not statistically different from zero. Finally, the coefficient in column (2) is positive and statistically different from zero, suggesting that Medicare spending growth is positively correlated with the lagged deficit-income ratio. As one of our robustness checks in Table 6, we control for the lagged deficit-income ratio. This exercise results in a even smaller estimate of the employment response.

It is worth noting that we do not claim that Medicare spending does not respond to any budget considerations. In our discussion of Medicare’s history, a few episodes of past rapid growth in Medicare spending motivated policymakers to adjust program parameters (e.g., reimbursement rates to providers) to curtail current spending growth. These kinds of adjustments induce mean reversion, or alternatively revision to trend growth, in the shock process rather than endogeneity of the instrument itself.

2.3 Medicaid

Medicaid plans are administered at the state level, although each state must meet guidelines set by the federal government. The cost of Medicaid is borne almost entirely through cost sharing by the federal and state governments. Currently, some states charge premiums for Medicaid coverage or require cost-sharing payments for particular services. These are capped; for example, premiums may not exceed 5 percent of family income.³⁸ Nonetheless, Medicaid spending is almost entirely

³⁸The federal government prohibits states from charging Medicaid premiums on families earning less than 150 percent of the federal poverty level (see Kaiser Family Foundation (2013)). For families below the federal poverty

federal and state government expenditures.³⁹

Also, whether Medicaid spending responds endogenously to the business cycle as a result of households dropping coverage (because of premiums) or foregoing particular services (because of copayments) during recessions is not relevant because of our instrumental variables strategy.

2.4 Disaggregate Model

Note that all the national-level variables defined above are constructed by aggregating state-level variables. Thus, we are able to also estimate the model at the state level.

At the state level, the second-stage estimation equation is

$$N_{i,t,\delta}^c = \varphi_\delta + \xi_{i,\delta} + \psi_\delta G_{i,t,\delta}^c + \pi'_\delta X_t + \gamma'_\delta S_t + \eta_\delta R_t + w_{i,t,\delta} \quad (2.4)$$

Note that our specification for the disaggregate analysis includes state fixed effects.

Next, we construct an instrument to estimate equation (2.4). Though Medicare spending is arguably exogenous at the national level, the exclusion restriction may not be satisfied at the state level because richer states tend to be healthier and spend less on Medicare. According to a report by the Kaiser Family Foundation (2015), the 20 counties with the highest Medicare per capita spending have much sicker and poorer beneficiary populations.

Thus, to address the endogeneity in the geographic distribution of Medicare spending, we construct a Bartik-style instrument. We operationalize this by multiplying our national instrument $Z_{t,\delta}^c$ by a state-specific scaling factor. This factor is the ratio of a state's share of national Medicare plus Medicaid spending, $s_{i,t}^G$, divided by the state's share of national income, $s_{i,t}^Y$. Both shares are computed as the state's average in quarters $t - 8$ through $t - 1$:

$$Z_{i,t,\delta}^c = (s_{i,t}^G / s_{i,t}^Y) Z_{t,\delta}^c \quad (2.5)$$

3 Results

The models outlined above are estimated using the generalized method of moments, which in this case has a two-stage least-squares (2SLS) interpretation.⁴⁰

Throughout our analysis, we exclude the 2007-2009 recession period, specifically 2007Q4 through 2009Q2, because changes in employment were driven largely by financial factors upon which we do not condition our regressions. Also, our sample starts in 1978 because this is the first year where

level, any out-of-pocket costs are tightly capped.

³⁹According to the Department of Health and Human Services (2015, p.3), "Beneficiary cost sharing, such as deductibles or co-payments, and beneficiary premiums are very limited in Medicaid and do not represent a significant share of the total cost of health care goods and services for Medicaid enrollees."

⁴⁰Heteroskedasticity and autocorrelation corrected standard errors are reported throughout the paper. The estimates are computed using Stata V.14 and the *ivreg2* command with the options *robust* and *bw*.

quarterly state-level prime-age employment counts are available. In addition, note that at each successive horizon we lose one observation from the most-recent quarters due to the construction of the cumulative variables. Thus, to make the estimates comparable across horizons, we fixed the sample to that available at $\delta = 16$, the longest horizon considered in the paper.

Table 2 contains the instrumental variables estimate. Column (1) reports the aggregate multiplier. First, observe that Medicare spending is a strong instrument for GFHC spending, with a partial F statistic equal to 44.72. Next, the second-stage estimate equals 0.08 (SE = 0.66). This implies that a GFHC spending increase equal to 1 percent of national income accumulated over a two-year horizon causes the employment-population ratio to increase by 8 basis points (accumulated over the same two-year horizon). Note that this effect is estimated imprecisely and one cannot reject a negative or moderately positive response.

To interpret these results, we use the estimated multipliers to calculate the cost of job creation. Using national income and population data from 2016, an employment multiplier of 0.08 implies a job-creation cost of \$615,662 per job-year.⁴¹ Note that while this figure informs us of the amount of spending required to create one job, it says nothing about the wage associated with each job created. In the worst-case scenario, for instance, each job created could pay the federal minimum wage. For simplicity, we assume that the distribution of employee compensation in the jobs created by an increase in GFHC roughly follows the same distribution as the existing nation-wide employee compensation, with a median of \$56,000 in 2016.⁴² This allows us to define a “moderate employment effect” as one associated with a job-creation cost of \$56,000 per job-year; this is implied by a multiplier of 0.88. Similarly, we define a “strong employment effect” as one associated with a job-creation cost per job-year equal to half of the median compensation (\$28,000); this is implied by a multiplier of 1.76.

Increases in the price-earnings ratio have a positive effect on the accumulated change in employment, as expected. We include Valerie Ramey’s news defense shock scaled by trend income as an additional control; its effect on the dependent variable is not statistically different from zero. Moreover, the point estimates and standard errors do not vary much when this control is excluded (see Table 6).

Column (2) of Table 2 presents the relative multiplier estimated using the model described in Section 2.4. These estimates are interesting, not because they are necessarily informative about the aggregate effects of GFHC, but because they speak to the methodology used in other studies on the impact of fiscal policy. These studies attempt to use geographic variation either in a panel or a

⁴¹The calculation is as follows: in 2016, national income was \$15.93 trillion and population was 323,406,000. Thus, an increase in GFHC equal to \$159.3 billion (1 percent of national income) leads to an increase in employment equal to 258,725 (0.08 percent of population). That is, an increase in GFHC equal to \$615,662 creates one job-year.

⁴²Our calculation of the median compensation is as follows: the median of weekly earnings by full-time wage and salary workers, as reported by the Bureau of Labor Statistics, was \$833 in 2016. Multiplying by 52 gives an annual figure of \$43,316. This number, however, underestimates the total employer cost for employee compensation, which includes benefits such as insurance and retirement contributions. Since these benefits are about 30 percent of wages and salaries, we increase this number by 30 percent to \$56,311.

Table 2: Aggregate and relative employment multipliers at an eight-quarter horizon, instrumental variables

	Aggregate data	State-level panel data
	(1)	(2)
	Coef./SE	Coef./SE
Employment multiplier	0.08 (0.66)	0.58** (0.27)
Change in 65+ share	0.23*** (0.06)	0.26*** (0.02)
Change in 65+ share _{t+7}	-0.15*** (0.04)	-0.18*** (0.02)
Ramey news shock	0.09 (0.22)	0.22** (0.10)
Change in PE ratio _{t-1}	0.78*** (0.30)	0.63*** (0.11)
Change in PE ratio _{t-2}	0.56** (0.23)	0.63*** (0.11)
Change in PE ratio _{t-3}	0.41** (0.19)	0.38*** (0.09)
Change in PE ratio _{t-4}	0.10 (0.22)	0.08 (0.10)
Partial F statistic	44.72	214.14
N	113	5626

Notes: The state-level regression includes state fixed effects. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

cross-section to infer the relative (or local) impact of government spending.⁴³ This issue has been discussed in Cochrane (2012), Furth (2013), Nakamura and Steinsson (2014) and Ramey (2011).

In the presence of cross-border spillovers, using cross-sectional variation identifies the relative differences in outcomes across states rather than the aggregate outcomes. Spillovers might arise, for example, through interstate trade in goods, movements of labor or common fiscal and monetary policies.

As shown in Column (2), the coefficient equals 0.58 (SE = 0.27), implying that the cumulative employment-population ratio rises by 58 basis points in response to a GFHC increase equal to 1 percent of income accumulated over a two-year horizon. The effect is precisely estimated, and we can reject a negative employment response with 95 percent confidence. Note that the estimated multiplier of 0.58 implies a job-creation cost of \$84,919 per job-year, with a 90 percent confidence interval equal to [\$48,092, \$362,554]. Although we cannot reject a moderate positive employment response, we can reject a strong positive employment response.

3.1 Dynamic Employment Response

In this section, we explore the dynamic path of the multiplier. Figure 6 plots the multiplier as one varies the horizon δ ; the markers represent the point estimates and the lines show the 90 percent confidence interval (robust to heteroskedasticity and autocorrelation). The dots and solid green lines correspond to the aggregate estimates, while the “x” marks and dashed purple lines correspond to the panel-based estimates. We let the dependent variable, endogenous variable and instrument vary with each horizon. The controls are the same as those reported in Table 2.

Figure 6 additionally plots two horizontal lines at 0.88 and 1.76. As argued in the previous section, an employment multiplier of 0.88 would imply a moderate stimulative response to accumulated changes in GFHC, while a multiplier of 1.76 would indicate a strong stimulative response. The panel-based estimates of the cumulative employment multiplier are statistically different from zero for the first ten horizons. We are able to reject a large stimulative effect at all horizons. However, we cannot reject a moderate effect for the first ten horizons.

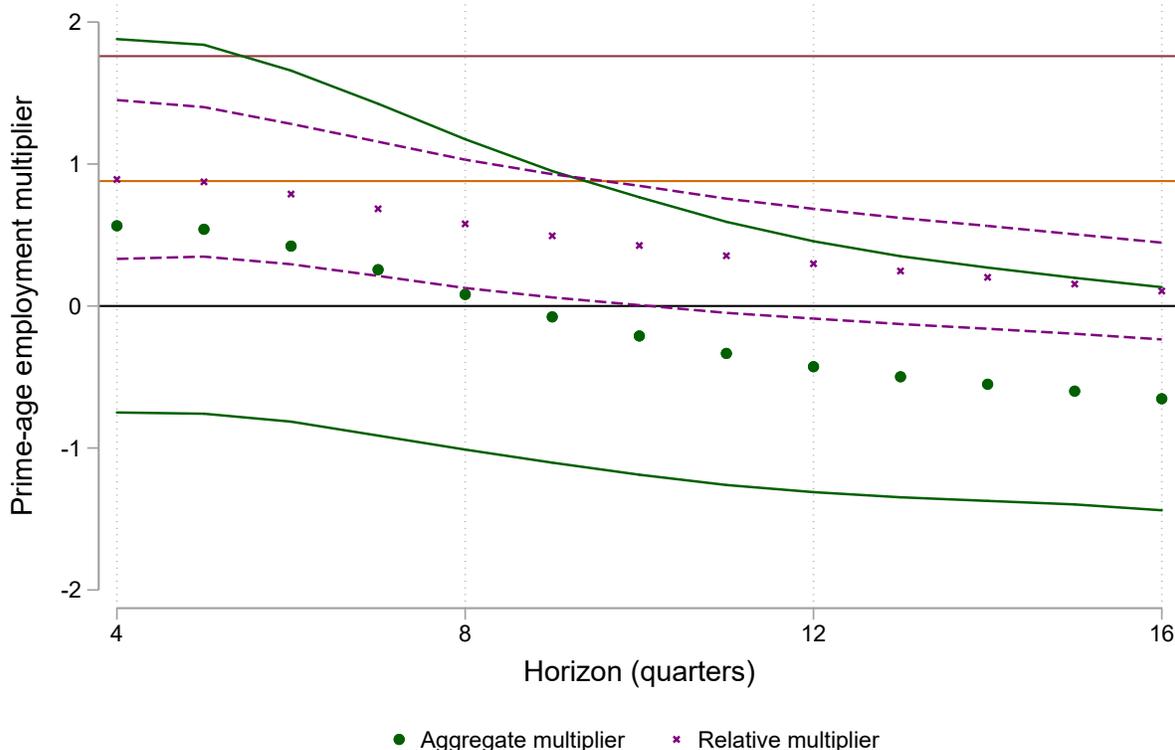
The cumulative multiplier path is smooth, and the point estimates stabilize around 0.2 for the panel-based estimate and -0.6 for the aggregate-based estimate. Note that these multipliers estimate the employment response accumulated over δ horizons. Thus, the near-zero long-run estimates reported imply an initial positive response followed by a negative employment response.

The estimated aggregate and relative multipliers are similar and so are their dynamic paths; moreover, there is considerable overlap of the confidence bands.⁴⁴ Although the aggregate employment response is imprecisely estimated, we are able to improve precision with the disaggregate

⁴³Examples of these types of studies appear in the paper’s introduction.

⁴⁴The fact that, across all horizons, the relative multiplier is consistently estimated to be higher than the aggregate multiplier could hint at the presence of negative inter-state spillovers. Nevertheless, the large standard errors in the aggregate regression preclude us from quantifying a potential spillover effect.

Figure 6: The cumulative employment response to cumulative changes in GFHC spending at various horizons



Notes: The controls are the same as those reported in Table 2. The lines at 0.88 and 1.76 represent a moderate and strong stimulative employment response, respectively (see text). The markers show the point estimates and the enveloping lines show the 90 percent confidence interval (robust to heteroskedasticity and autocorrelation). The dots and solid green lines correspond to the aggregate estimates, while the “x” marks and dashed purple lines correspond to the panel-based estimates.

data. Thus, the results presented in Table 2 and Figure 6 summarize three of the main conclusions of the paper: (1) the relative multiplier approach provides a similar estimate to that based on aggregate data, (2) we cannot reject a moderate positive employment response to increases in GFHC spending at the two-year horizon, and (3) we estimate a muted cumulative effect in the long run (beyond the three-year horizon).

3.2 The First Stage

This section examines the robustness of our instrument choice. Table 3 shows the first-stage results used in the estimation of equations (2.3) and (2.4), which indicate a strong first stage. The point

estimate on the Medicare instrument in the aggregate regression (column (1)) implies that each dollar of additional Medicare spending is associated with \$1.21 of additional GFHC spending. Since GFHC is made up of Medicare and Medicaid, the first-stage results suggest that additional Medicare spending crowds in Medicaid spending. One channel that explains this result lies in the nature of the dual-eligible (Medicare-Medicaid) beneficiaries. This connection is explored in Section 2.2.3.

Figure 7 shows the dynamic path of the first-stage point estimate. By the 12-quarter horizon, the coefficient stabilizes at 1.3.

Table 3: Aggregate and relative employment multipliers at an eight-quarter horizon, first-stage estimates

	Aggregate data (1)	State-level panel data (2)
	Coef./SE	Coef./SE
Medicare instrument	1.21*** (0.18)	1.24*** (0.08)
Partial F statistic	44.72	214.14
N	113	5626

Notes: The state-level regression includes state fixed effects. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

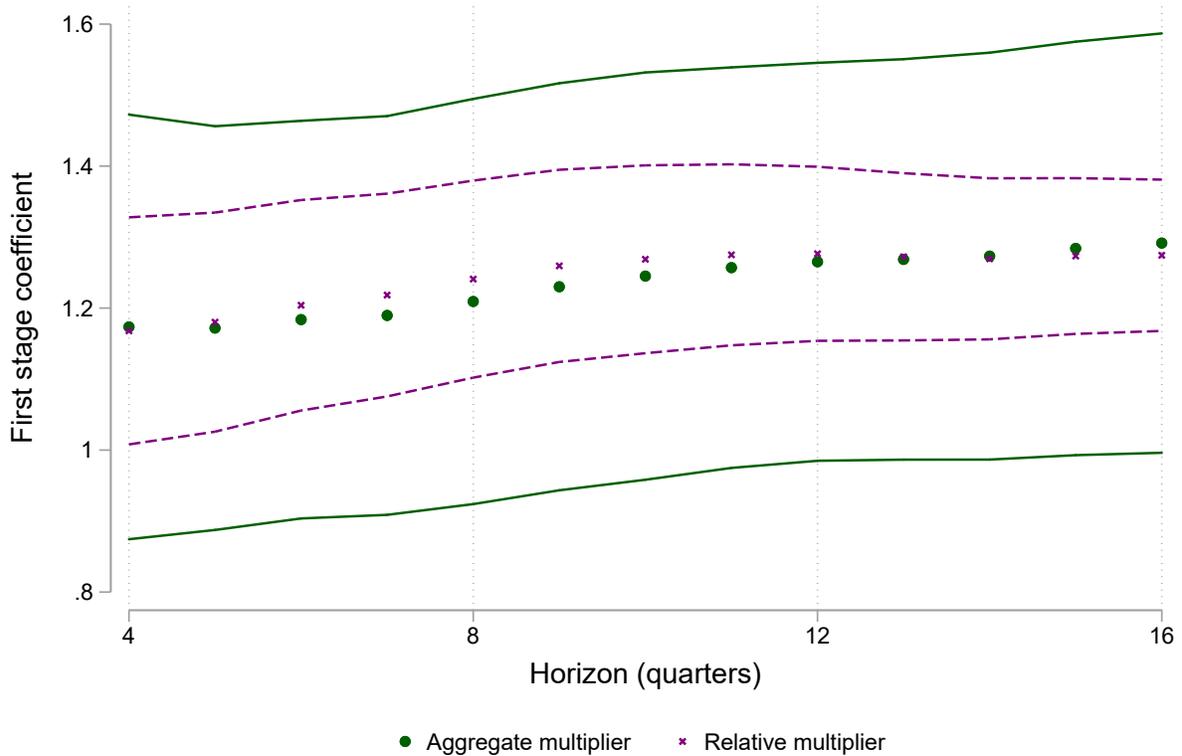
Table 4 presents several alternative specifications that reflect a robust conclusion of this section: failing to correct for the countercyclicality of total GFHC results in downwardly biased estimates that suggest strongly contractionary responses.

For comparison purposes, row (a) of Table 4 reproduces the multiplier estimates obtained from the benchmark model, first reported in Table 2. Row (b) contains the least-squares estimate corresponding to the benchmark specification. As one might expect, the coefficient falls. This is consistent with our conjecture that Medicaid spending is countercyclical to the business cycle, because enrollments tend to increase during economic downturns.

Our identification assumption—that Medicare spending is orthogonal to the equation’s error term—attempts to control for the contaminating effect that Medicaid might have on treating GFHC as exogenous to the error term. To stress the endogeneity in GFHC from the Medicaid component, row (c) presents the benchmark specification, except that changes in Medicaid, not Medicare, are used as the instrument. In this case, the negative employment response is even stronger than it was for the least-squares estimation.

Section 2.4 discusses the potential endogeneity in the geographic distribution of Medicare spending and addresses it by constructing a Bartik-style instrument (see Bartik (1991)). Now, we examine how the relative multiplier estimates change if one ignores such concerns and uses the accumulated change in state Medicare spending (scaled by state income) as an instrument. Row (d) shows the

Figure 7: Cumulative GFHC response to cumulative changes in Medicare spending



Notes: The controls are the same as those reported in Table 2. The markers show the point estimates and the enveloping lines show the 90 percent confidence interval (robust to heteroskedasticity and autocorrelation). The dots and solid green lines correspond to the aggregate estimates, while the “x” marks and dashed purple lines correspond to the panel-based estimates.

results of this exercise. As expected, failing to account for the potential cross-state endogeneity of Medicare spending biases the estimate downwards, suggesting a moderately negative employment effect.

Finally, row (e) replaces the benchmark instrument with the period- t one-quarter change in the Medicare spending scaled by $t-1$ income. This might be important if there was a dynamic feedback from Medicare spending to the real economy and then back again to future values of Medicare. Not surprisingly, the partial F statistic falls for both specifications (not reported). In this case, the aggregate and relative multipliers slightly increase. This may be evidence of fiscal foresight, of the type studied by Leeper, Traum and Walker (2017).

Table 4: Aggregate and relative employment multipliers at an eight-quarter horizon, alternative instrument specifications

	Aggregate data	State-level panel data
	(1)	(2)
	Coef./SE	Coef./SE
(a) Benchmark	0.08 (0.66)	0.58** (0.27)
(b) Least squares	-1.20*** (0.30)	-0.68*** (0.07)
(c) Medicaid instrument	-1.72*** (0.57)	-1.46*** (0.17)
(d) No Bartik correction	-	-0.51*** (0.19)
(e) 1-quarter change instrument	0.36 (1.56)	0.80 (0.59)

Notes: The state-level regression includes state fixed effects. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

3.3 State-Contingent Multipliers

Recent research has suggested that the stimulative effects of government spending may be higher when there is slack in the labor market. For instance, Michailat (2014) develops a New Keynesian model in which the government spending multiplier doubles when the unemployment rate increases from 5 percent to 8 percent. Auerbach and Gorodnichenko (2012) and Nakamura and Steinsson (2014) find evidence that multipliers tend to be substantially higher during periods of slack. On the other hand, Barro and Redlick (2011), Owyang, Ramey and Zubairy (2013) and Ramey and Zubairy (2018) find that spending multipliers are less than or close to 1 under both labor market conditions.

One reason why government spending multipliers may depend upon the unemployment rate is that during times of high unemployment the government's demand for goods and services could be met with otherwise idle workers. Thus, additional public spending need not bid up wages significantly or crowd out private demand. Whether multipliers are contingent upon the state of the economy is particularly relevant because policymakers are most likely to call for spending stimulus when many are unemployed and vacancies are scarce.

Table 5 presents the relative multiplier estimates from splitting our sample in two according to a state unemployment rate threshold. Following the approach taken by Barro and Redlick (2011)

and Nakamura and Steinsson (2014), we set the state unemployment rate threshold equal to the median rate in our sample, 5.77 percent.⁴⁵ Column (1) shows the results of estimating our baseline equation on the subsample in which the state unemployment rate in quarter $t - 1$ is above 5.77 percent. Column (2) presents the analogous results for the remainder of the sample. Although the point estimate is higher in the high-unemployment sample, we are unable to reject equality of multipliers with 90 percent confidence (p -value = 0.41).

Table 5: State-contingent multipliers

	High unemployment (1) Coef./SE	Low unemployment (2) Coef./SE
Employment multiplier	0.52 (0.52)	0.04 (0.29)
Change in 65+ share	0.18*** (0.03)	0.27*** (0.04)
Change in 65+ share _{$t+7$}	-0.16*** (0.02)	-0.16*** (0.04)
Ramey news shock	0.01 (0.15)	0.28** (0.13)
Change in PE ratio _{$t-1$}	0.69*** (0.19)	0.46*** (0.14)
Change in PE ratio _{$t-2$}	0.63*** (0.21)	0.61*** (0.13)
Change in PE ratio _{$t-3$}	0.43*** (0.16)	0.32*** (0.11)
Change in PE ratio _{$t-4$}	0.18 (0.16)	0.13 (0.12)
Partial F statistic	42.37	311.43
N	2792	2834

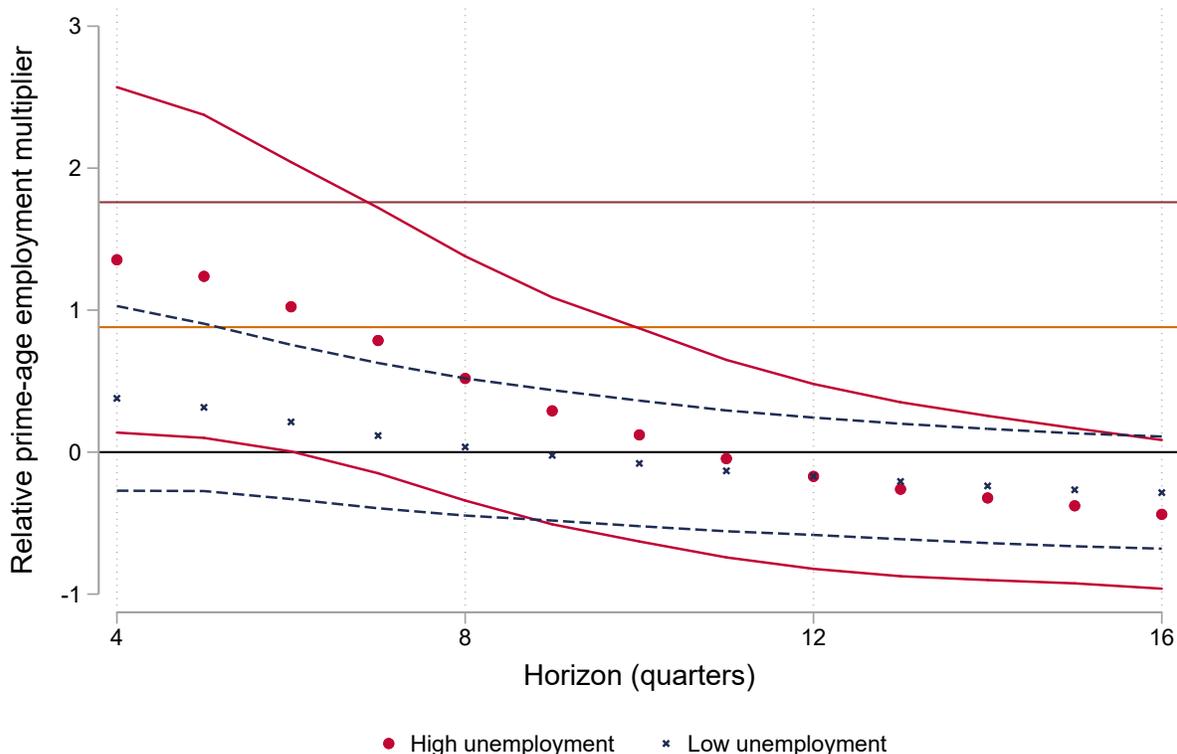
Notes: We split the sample in two according to a 5.77 percent state unemployment rate threshold. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

Next, Figure 8 shows the dynamic paths of the relative employment multipliers when splitting the sample according to the unemployment rate threshold. We note three points. First, although the multiplier estimated with the high-unemployment sample is higher than that estimated with the low-unemployment sample for the first twelve horizons, we cannot reject equality of the point estimates at any horizon with 90 percent confidence. Second, the multiplier estimated with the low-unemployment sample is near zero at all horizons. In contrast, its high-unemployment counterpart is above 1 (and statistically different from zero) at horizon 4, but it smoothly converges to the

⁴⁵We use only the disaggregate data to examine the issue of state-contingent multipliers, as there is not enough time-series variation in GFHC spending over the two states of the economy to achieve precise aggregate estimates.

multiplier estimated with the low-unemployment sample. Third, when unemployment is low, we are able to reject a strong employment effect at all horizons and a moderate employment effect after horizon 5. When unemployment is high, we are able to reject a strong employment response beyond horizon 6 and a moderate employment response after horizon 10.

Figure 8: Dynamic response of prime-age employment to GFHC during times of high and low state unemployment



Notes: We split the sample in two according to a 5.77 percent state unemployment rate threshold. The controls are the same as those reported in Table 5. The lines at 0.88 and 1.76 represent a moderate and strong stimulative employment response, respectively. The markers show the point estimates and the enveloping lines show the 90 percent confidence interval (robust to heteroskedasticity and autocorrelation). The dots and solid red lines correspond to the estimates from the high-unemployment sample, while the “x” marks and dashed blue lines correspond to the estimates from the low-unemployment sample.

3.4 Robustness Checks

Table 6 shows variations to our benchmark specification (reproduced in row (a)). Row (b) extends the sample to include the Great Recession years (2007Q4–2009Q2). Rows (c) through (e) drop

the three controls one by one, and row (f) drops all controls. This exercise shows that dropping the demographic controls increases the multiplier estimates; however, the point estimates in row (d) are well within our benchmark 90 percent confidence interval. Note that the inclusion of Ramey's defense news series has a negligible impact on the estimation of the aggregate and relative multipliers. On the other hand, dropping the lagged price-earning controls from the specification has a large effect on the aggregate multiplier. It decreases to -0.95 (SE = 0.50). The change also affects the relative multiplier.

Table 6: Aggregate and relative unemployment rate multipliers at an eight-quarter horizon, robustness checks

	Aggregate data (1) Coef./SE	State-level panel data (2) Coef./SE
(a) Benchmark	0.08 (0.66)	0.58** (0.27)
(b) Including 2007-2009	0.22 (0.67)	0.38* (0.23)
(c) Drop PE controls	-0.95* (0.50)	-0.33 (0.21)
(d) Drop demographic controls	0.46 (0.79)	0.87*** (0.28)
(e) Drop Ramey control	0.09 (0.67)	0.58** (0.28)
(f) Drop all controls	-0.79 (0.64)	-0.23 (0.22)
(g) Defense medical spending	0.08 (0.68)	-
(h) Add deficit-income ratio	-0.26 (0.64)	0.24 (0.27)
(i) Add change in 65+ death rate	0.06 (0.62)	0.55** (0.27)

Notes: The state-level regression includes state fixed effects. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

Row (g) of Table 6 adds health care spending from the Department of Defense and Department

of Veterans Affairs to the spending from programs used in the benchmark case (i.e., Medicare and Medicaid). These additional data come from the National Health Expenditure survey and are available only at the national level. Thus, we cannot estimate the corresponding state-level specification. Also, the data are annual, so we construct the quarterly series via linear interpolation. The addition of these types of GFHC spending do not alter our baseline estimate.

Section 2.2.3 analyzes the effect of the federal budget stress on Medicare spending and shows that Medicare spending growth is positively correlated with the lagged deficit-income ratio. Row (h) of Table 6 adds the lagged deficit-income ratio as a control. This exercise results in a smaller estimate of the employment response.

Section 2.2.3 also addresses the possibility that the effect of business cycles on health status may result in a violation of the exogeneity assumption and bias our results. Row (i) of Table 6 adds changes in the age-adjusted mortality rate of the age 65+ population in state i during quarter t as a control to our baseline specification.⁴⁶ The coefficients on the mortality rate controls (not reported) are positive, suggesting that mortality rates are procyclical. This is consistent with some of the existing literature on the link between unemployment and mortality. Importantly, note that the addition of these controls has negligible impact on the aggregate and relative multipliers.

One concern may be that one of the factors driving Medicare spending—the price of health care—itself has a direct influence on employment. One could then misassign a shock to health care prices as causing employment to change solely through the effect of Medicare. To address this possibility, we modify our benchmark specification to control for several alternative measures of medical care price inflation. We use the change (computed various ways) in the natural log of the ratio of the medical care price index to the overall core CPI.

These results are presented in Table 7. Column (1) contains the benchmark specification (i.e., no medical care price inflation control), and the remaining three columns control for medical care price inflation using various measures. There are two things to note. First, controlling for medical care prices does not substantially reduce the power of the instrument, as indicated by the partial F statistics. That is, there is plenty of Medicare spending variation not attributable to changes in the price of medical care. Second, the point estimate for the multiplier remains not statistically different from zero for each of the specifications.

3.5 Response of Other Macroeconomic Variables

In this section, we study the response of other macroeconomic variables to changes in GFHC spending. Table 8 replaces the dependent variable in the benchmark specification with several alternatives.

⁴⁶Specifically, we include $d_{i,t}$ and $d_{i,t+7}$, where $d_{i,t}$ is the change in the age-adjusted mortality rate of the 65+ population in state i between quarter t and $t - 8$. The mortality rate controls for the aggregate regression are defined similarly. We obtained the age-adjusted mortality rates data from the Centers for Disease Control and Prevention’s compressed mortality dataset. The data are annual, and we interpolate it to quarters.

Table 7: Aggregate eight-quarter multiplier, adding medical price inflation controls

	Benchmark (1) Coef./SE	Future inflation (2) Coef./SE	Past inflation (3) Coef./SE	2 lags of inflation (4) Coef./SE
Employment multiplier	0.08 (0.66)	0.54 (0.80)	-0.49 (0.62)	-0.36 (0.69)
2-year change in log of relative CPI_{t+7}	-	-0.52* (0.30)	-	-
2-year change in log relative CPI_{t-1}	-	-	0.58** (0.27)	-
1-year change in log relative CPI_{t-1}	-	-	-	0.28 (0.68)
1-year change in log relative CPI_{t-5}	-	-	-	0.85** (0.34)
Partial F statistic	44.72	37.54	35.06	30.88
N	113	113	113	113

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. Each regression includes the benchmark controls. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 8: Response of macroeconomic variables to changes in GFHC spending

	Aggregate data (1) Coef./SE
(a) Employment	-1.10 (0.88)
(b) Edu. & health employment	0.01 (0.07)
(c) Accumulated deficit	2.05** (0.81)
(d) Core CPI - cumulative change	3.48 (3.44)
(e) Core CPI - change	0.68 (0.75)

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

In row (a), the cumulative change in prime-age employment is replaced with the cumulative change in total employment (scaled by population). The negative, yet not statistically different from zero, employment multiplier is consistent with our concern of the confounding effects of demographic shifts on Medicare spending and employment. An increase in retirees as a share of population simultaneously causes an increase in Medicare spending and a decrease in the total employment-population ratio. This results in a downwardly biased estimate of the employment response to changes in Medicare spending.

In row (b) the cumulative change in total employment (scaled by population) is replaced with the corresponding variable for employment in the health and education sector.⁴⁷ The aggregate multiplier for employment in this sector is not statistically different from zero. The quantitative magnitude of the coefficient is small, most likely because only a small fraction of the population works in this sector.

Row (c) uses the eight-quarter accumulated change in the deficit (as a fraction of trend income) as the dependent variable.⁴⁸ The estimate equals 2.05 (SE = 0.81). A coefficient of 1 would imply that the identified changes in GFHC transfer one-for-one to create a larger deficit. This suggests that exogenous GFHC changes have been mainly deficit financed rather than financed with higher current aggregate taxes.

Rows (d) and (e) report the response of inflation to changes in GFHC. Row (d) shows the effect on the cumulative change in core CPI. The estimate equals 3.48 (SE = 3.44). Similarly, row (e) presents the response of the two-year percentage change in core CPI. The point estimate is 0.68 (SE = 0.75). It implies that if GFHC spending goes up by 1 percent of national income, then inflation over that two-year period will increase at an annualized rate of 0.34 basis points.⁴⁹

Figure 9 contains the impulse responses of other macroeconomic variables, based on the aggregate data, to a GFHC-shock equal to 1 percent of trend national income. Each panel contains a cumulative response except the federal debt panel. Commonalities across each panel are that each response function converges by the end of the horizon and each is estimated with substantial imprecision.

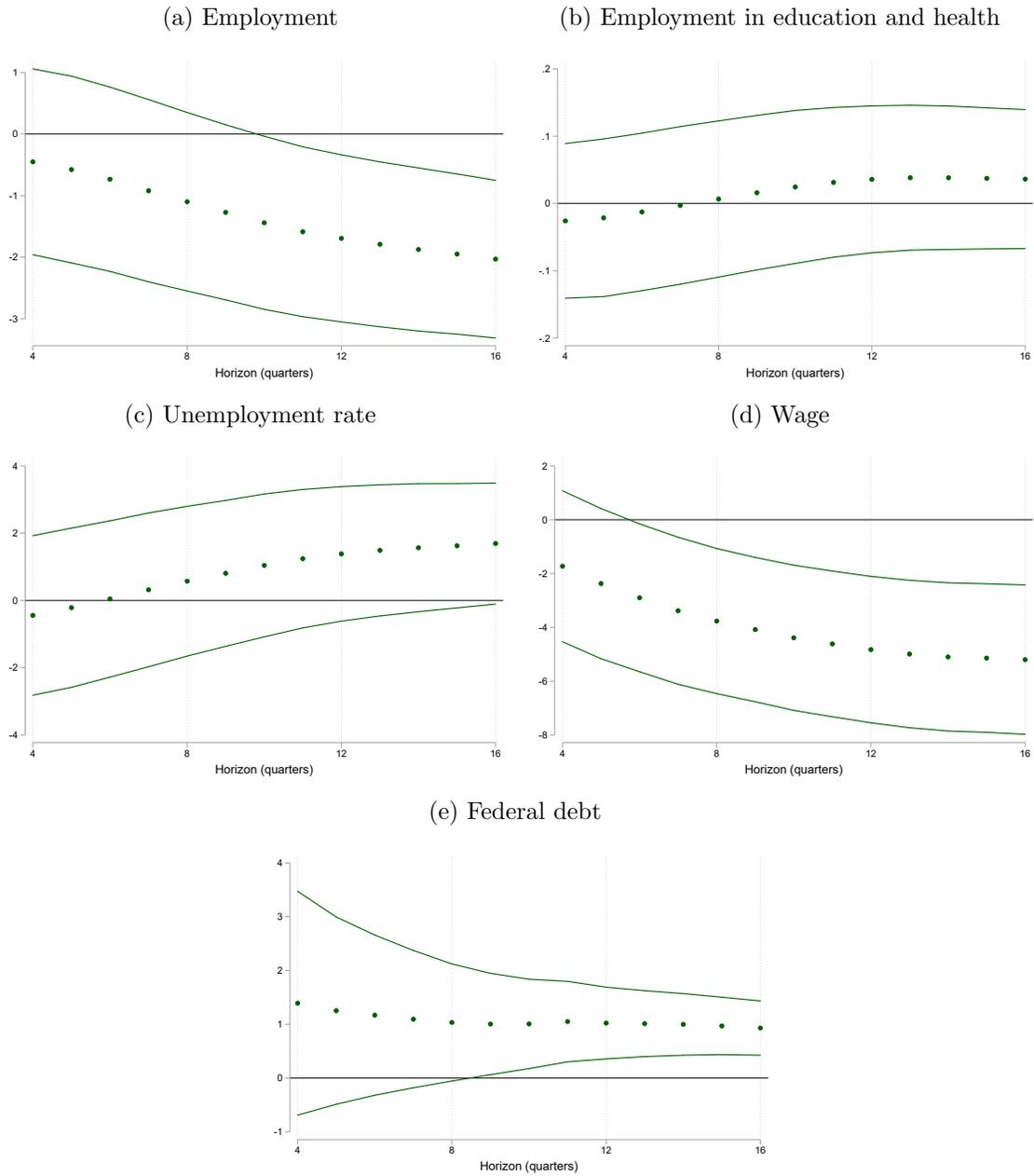
Panel (a) contains the employment response relative to the population level. The point estimate is close to zero at horizon 4 and then declines to approximately -2. One can reject a positive employment response after approximately horizon 10. As explained earlier, the strong negative employment effect—absent when replacing the dependent variable with prime-age employment—is most likely due to demographic factors. Panel (b) plots the response of employment in the health and education sectors relative to the population. The function begins below but close to zero and then rises above zero by horizon 8; however, the estimates are sufficiently imprecise, so one cannot

⁴⁷We report only the aggregate multiplier for this specification because much of the sample does not contain state-level sectoral employment data.

⁴⁸We report only the aggregate multiplier because state-level deficit data are not available for most of the sample.

⁴⁹See Dupor and Li (2015) for a discussion of the inflation responses to several government spending shocks identified using other approaches.

Figure 9: Response of other macro variables



Notes: Each panel contains a cumulative response except for the federal debt panel. The lines indicate the pointwise robust 90 percent confidence intervals.

reject either a positive or negative response at any horizon.

Panel (c) contains the response of the unemployment rate. Let U_t denote the national unemployment rate, reported by the Bureau of Labor Statistics. Then the dependent variable in panel (c) is the cumulative increase in U_t over a δ -quarter horizon relative to a quarter $t - 1$ baseline:

$$U_{t,\delta}^c = \sum_{j=1}^{\delta} U_{t+j-1} - \delta U_{t-1} \quad (3.1)$$

The positive unemployment rate response is consistent with the contractionary employment response shown in panel (a). Note that the national unemployment rate is also susceptible to shifts in demographics that simultaneously drive Medicare spending. This is because the U.S. unemployment rate decreases with age.⁵⁰ Thus, holding all else constant, a shock to the birth rate drives the unemployment rate and Medicare spending in the same direction when the generation in question retires. Figure 10 illustrates this mechanism for a simplified economy. Note that this mechanism results in a upwardly biased estimate of the unemployment response to changes in Medicare spending.

Figure 10: Example of confounding effects of demographics on unemployment and Medicare spending

	Age 1 UR = 50%	Age 2 UR = 0%	Age 3 Not in LF	National UR
Year 1	E E U U	E E	R R	33%
Year 2	E U	E E E E	R R	17%
Year 3	E U	E E	R R R R	25%

Notes: Each letter represents one individual, who may be employed (E), unemployed (U) or retired (R). Individuals are born at age 1 and die a year after they reach age 3. The unemployment rate (UR) at each age is fixed and independent of the number of individuals who are that age (50 percent for age 1 and 0 percent for age 2). Year 1 sees a one-time positive shock to the birth rate. When this generation retires in year 3, there is an increase in both the unemployment rate and Medicare spending (proxied by the number of retirees in the economy).

Panel (d) plots the real wage response, which falls in response to the increased government spending. One possible explanation would be that, in the presence of sticky nominal wages, if

⁵⁰In August 2017, the unemployment rate for the age groups 16-19, 20-24, 25-54, and over 55 were, respectively, 13.6 percent, 7.1 percent, 4.0 percent, and 3.2 percent.

increased government spending drives up inflation, then the real wage would fall. Alternatively, recall that employers pay a percentage of workers’ wage income in the form of taxes (up to legal caps) for the HI trust fund. If increased Medicare spending drives up taxes, then employers might put the burden of these higher taxes on workers by reducing their wage.

Finally, panel (e) plots the response of the level of the federal debt to the trend income ratio. As one might expect, higher Medicare spending increases the federal debt. The coefficient is greater than 1 at short horizons and then converges to 1 eventually.

3.6 An Alternative Reading: The Reduced Form

In this section we consider an alternative econometric specification. In the preceding sections, we use changes in Medicare spending as an instrument for changes in total GFHC spending. Whether the results estimated using this approach provide relevant information about the impact of Medicaid expansions hinges on the non-zero correlation between changes in Medicare and Medicaid spending. In other words, if changes in Medicare and Medicaid spending were uncorrelated, then the inclusion of Medicaid spending in our measure of GFHC would merely be adding noise to the second-stage estimation. If this were the case, then estimating the reduced-form equation—i.e., the employment response to Medicare spending in a least-squares regression—would be more appropriate.

Table 9 presents the results of regressing changes in Medicaid spending on changes in Medicare spending. Conditional on our baseline set of covariates, we do find a positive correlation between changes in Medicare and Medicaid spending, and this effect is statistically different from zero in the disaggregate regression. This result suggests that Medicare spending crowds in Medicaid spending. As mentioned in Section 3.2, one channel that explains this lies in the nature of the dual-eligible (Medicare-Medicaid) beneficiaries, who are low-income individuals over age 65. This particularly vulnerable group of beneficiaries require costly care that is above the average for non-dual-eligible individuals and makes up a substantial portion of GFHC spending.⁵¹ If an individual enrolled in both programs is in need of medical care, then both Medicare spending and Medicaid spending are likely to increase. As explained in Section 2.2.3, this channel is unlikely to cause a violation of the exclusion restriction of our instrument. Other channels via which Medicare and Medicaid spending co-move—such as the impact of medical technological innovations on benefit coverage—are imaginable.

Table 10 presents the reduced-form regression results, estimated with aggregate and disaggregate data.⁵² Not surprisingly, the employment response to changes in Medicare spending is consistent with the response to changes in GFHC spending instrumented by Medicare spending. The aggregate multiplier implies a muted impact on employment that is not statistically different from zero. On the other hand, the relative multiplier suggests a moderate-to-strong employment response; notably,

⁵¹In 2008, the dual eligible comprised 20 percent of Medicare enrollees but 31 percent of Medicare spending, and 15 percent of the Medicaid population but 39 percent of Medicaid spending (Jacobson, Neuman and Damico, 2012).

⁵²For the regressor of main interest we use the variable $Z_{i,t,\delta}^c$, as defined in equation (2.5).

Table 9: Effect of Medicare spending on Medicaid spending

	Aggregate data	State-level panel data
	(1)	(2)
	Coef./SE	Coef./SE
Medicare Spending	0.21 (0.17)	0.27*** (0.05)
Change in 65+ share	0.01 (0.02)	0.00 (0.00)
Change in 65+ share _{t+7}	-0.01 (0.01)	-0.01*** (0.00)
Ramey news shock	-0.10*** (0.03)	-0.10*** (0.01)
Change in PE ratio _{t-1}	-0.09 (0.07)	-0.09*** (0.02)
Change in PE ratio _{t-2}	-0.04 (0.05)	-0.04*** (0.02)
Change in PE ratio _{t-3}	-0.01 (0.04)	0.01 (0.01)
Change in PE ratio _{t-4}	-0.03 (0.05)	-0.01 (0.02)
N	113	5626

Notes: The state-level regression includes state fixed effects. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

we can reject a multiplier equal to zero with 95 percent confidence. Figure 11 traces the dynamic path of the cumulative employment response to changes in Medicare spending as one varies the horizon δ .

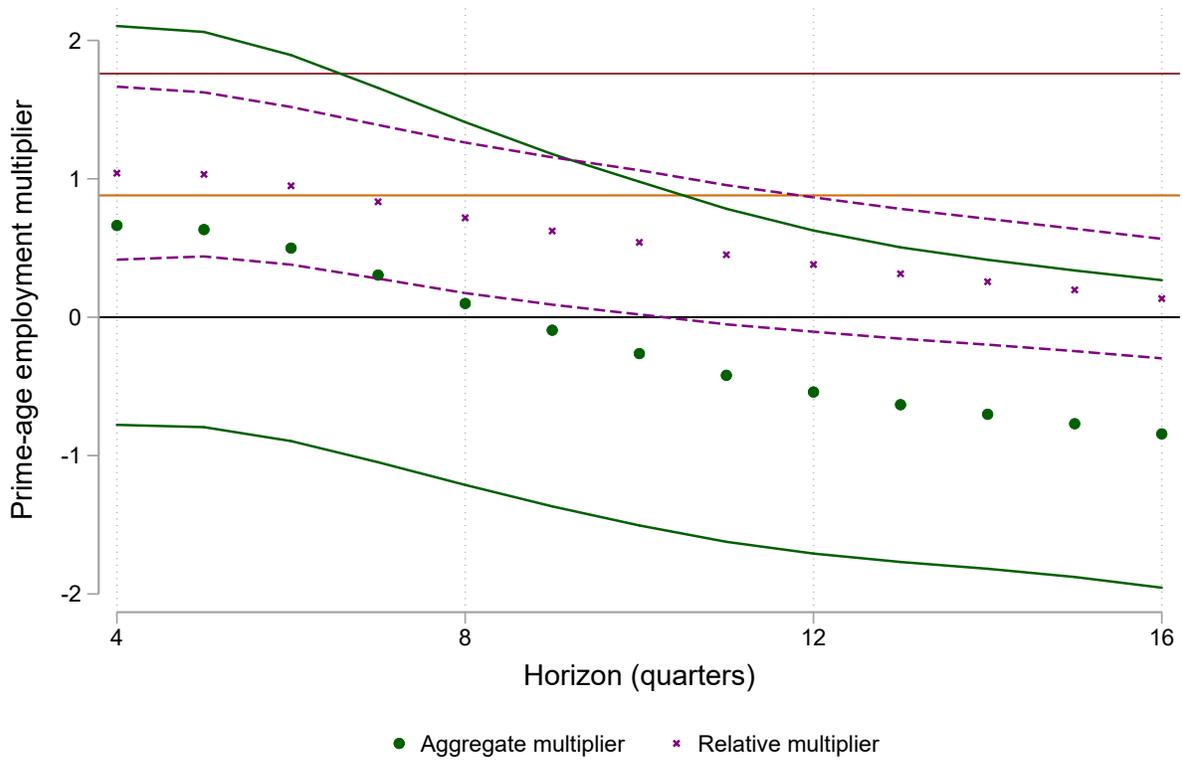
Table 10: Aggregate and relative employment multipliers at an eight-quarter horizon, reduced form

	Aggregate data	State-level panel data
	(1)	(2)
	Coef./SE	Coef./SE
Employment Multiplier	0.10 (0.80)	0.72** (0.33)
Change in 65+ share	0.23*** (0.06)	0.26*** (0.02)
Change in 65+ share e_{t+7}	-0.15*** (0.04)	-0.18*** (0.01)
Ramey news shock	0.08 (0.20)	0.16* (0.10)
Change in PE ratio $t-1$	0.77*** (0.28)	0.57*** (0.10)
Change in PE ratio $t-2$	0.56*** (0.21)	0.60*** (0.10)
Change in PE ratio $t-3$	0.41** (0.19)	0.38*** (0.09)
Change in PE ratio $t-4$	0.10 (0.21)	0.08 (0.09)
N	113	5626

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

In order for the reduced-form results to be informative about the employment response to Medicaid expansions, one ought to assume that Medicaid spending affects employment in a similar manner as Medicare spending. One can imagine several reasons why this assumption might not hold. For instance, the populations enrolled in these programs are quite different, and the impact of subsidizing health care of young low-income individuals may be different from that of financing the medical needs of retirees. Moreover, Medicaid coverage expansions may have some extensive-margin effects—such as the construction of new hospitals or the hiring of additional medical staff—that may not be observed as a result of changes in Medicare spending if this variation is driven by new technology or price increases. Nevertheless, the external validity assumption is more plausible here than in other instances in the fiscal policy literature, e.g., that military purchases influence private economic activity in the same manner as government spending more generally. First, both Medicare and Medicaid spending feed dollars into the medical care industry. Moreover, second round “Keynesian” effects of the spending are likely to be the same across the two spending types.

Figure 11: The cumulative employment response to cumulative changes in Medicare spending at various horizons



Notes: The controls are the same as those reported in Table 2. The lines at 0.88 and 1.76 represent a moderate and strong stimulative employment response, respectively (see text). The markers show the point estimates and the enveloping lines show the 90 percent confidence interval (robust to heteroskedasticity and autocorrelation). The dots and solid green lines correspond to the aggregate estimates, while the “x” marks and dashed purple lines correspond to the panel-based estimates.

Finally, note that in 2016 Medicare spending made up 54 percent and 20 percent of GFHC and total government consumption and investment, respectively. Thus, even if one is skeptical about the strength of the external validity assumption, our results still provide compelling evidence of the impact of government spending on economic activity.

4 Related Research

Other papers have identified the macroeconomic effects of government spending shocks by focusing on particular components of government spending. The defense spending component is particularly

compelling because it is unlikely to suffer from endogeneity issues, since these spending decisions are driven by international geopolitical factors. Examples include Hall (2009), Barro and Redlick (2011), Ramey (2011) and Ramey (2012).

Second, there is a line of research on the differences between relative and aggregate multipliers. Dupor and Guerrero (2017) follow a similar strategy to the one taken in the current paper. They use state-level defense spending to estimate an aggregate multiplier (based on their aggregated data). That paper finds no or small effects on employment, which is broadly consistent with the results of the current paper. Then, they estimate the relative multiplier based on the state-level panel.⁵³ They find that the relative multiplier is in the same range as the aggregate multiplier, which is consistent with the results of the current paper. In a survey paper, Ramey (2011) catalogues estimates from many macroeconomic time-series studies (i.e., aggregate multipliers) and cross-sectional studies (i.e., relative multipliers). She finds a general tendency of aggregate multiplier estimates to be smaller than local multiplier estimates.

Third, Nakamura and Steinsson (2014) address the relative versus aggregate multiplier question using a different approach. First, they estimate relative multipliers using a defense spending state-level panel. However, they do not estimate the aggregate multiplier using this data set. Instead, they proceed by building a two-region dynamic equilibrium model with regional government spending shocks. They consider alternative calibrations (e.g., varying the degree of price rigidity and the stance of monetary policy). Using model-generated data, they estimate both aggregate and relative multipliers and show that relative and aggregate multipliers can differ dramatically across parameterizations. Thus, while Nakamura and Steinsson (2014) demonstrate a potential importance of distinguishing between the two types of multipliers, they do not estimate both using real-world data.

Next, there has been almost no academic work on the short-run labor market effects of Medicare purchases. This speaks to exogeneity of the instruments: policymakers and economists have not used Medicare as a counter-cyclical fiscal tool and therefore have not conducted research from this perspective. As the government currently spends more on GFHC than on defense, there is now a Medicare/Medicaid gap in the literature on government spending and economic activity. The need to fill the gap is particularly timely as proponents of the ACA Medicaid expansion have used economic stimulus as one justification for new GFHC spending (see Council of Economic Advisers (2014)).

Very recently, a few studies have addressed the economic impact of Medicaid expansions under the ACA. Since some states have adopted Medicaid expansion while others have not, one approach has been to compare the outcomes across the two sets of states, in effect generating a control and treatment group.⁵⁴ While the findings of these studies vary, most suffer from the potential methodological problem that motivates our work. These papers estimate relative effects of the

⁵³They use the term local multiplier throughout their paper.

⁵⁴These include Garrett and Kaestner (2015), Gooptu et al. (2016) and Leung and Mas (2016).

difference in spending amounts rather than the total effect. Without additional time-series variation in GFHC spending, these *diff-in-diff* approaches may be biased in the presence of cross-state spillovers.

5 Conclusion

This paper make three contributions. First, we overcome the potential endogeneity in GFHC expenditures by using Medicare spending as an instrument. Medicare expenditures are highly correlated with total GFHC spending; moreover, by examining the history of the Medicare program, we explain that changes in its expenditures have not been causally impacted by the business cycle.

Second, we use the new instrument to estimate the effect of GFHC spending on national prime-age employment. Although the aggregate multiplier is estimated imprecisely, we are able to reject a strong positive cumulative response after the seven-quarter horizon and a moderate positive response after the ten-quarter horizon. That is, our aggregate analysis suggests that an exogenous GFHC expansion results in a muted *cumulative* employment response in the long run.

Third, we repeat our analysis using the panel of state-level data on GFHC spending and employment. Instead of using aggregate time-series variation, using cross-sectional variation generates estimates of the relative (or local) effect of spending. At the two-year horizon, we find a moderate positive cumulative employment response. This implies a job-creation cost of \$84,900 per job-year. We also report a near-zero cumulative effect beyond the three-year horizon. Our results provide an empirical example in which relative multipliers may be a reliable indicator of the aggregate effects of fiscal policy.

Our results are likely to be important as the push by some for further expansion of government provided medical care, e.g., movement to a single-payer system, is likely to continue. While there may be socially desirable reasons for expanding GFHC, doing so as part of an effective long-run job creation agenda is not supported by the data.

References

- Acemoglu, D. and P. Restrepo (2017), “Robots and Jobs: Evidence from U.S. Labor Markets,” NBER working paper.
- Auerbach A. and Y. Gorodnichenko (2012), “Measuring the Output Responses to Fiscal Policy,” *American Economic Journal: Economic Policy*, 4, 1-27.
- Baicker, K., A. Finkelstein, J. Song and S. Taubman (2014), “The impact of medicaid on labor market activity and program participation: Evidence from the Oregon health insurance experiment,” *American Economic Review*, 104, 322-328.
- Barro, R. and C. Redlick (2011), “Macroeconomic Effects from Government Purchases and Taxes,” *Quarterly Journal of Economics*, 126, 51-102.
- Bartik, T. (1991), *Who Benefits from State and Local Economic Development Policies?*, W.E. Upjohn Institute for Employment Research.
- The Boards of Trustees (2017), *2017 Annual Report of the Boards of Trustees of the Federal Hospital Insurance and Federal Supplementary Medical Insurance Trust Funds*.
- Boushey, H. (2011), “Now Is the Time to Fix Our Broken Infrastructure,” Center for American Progress, September 22.
- Catlin, A. and C. Cowan (2015), “History of Health Spending in the United States,” Centers for Medicaid and Medicaid Services, Nov. 19.
- Centers for Medicare and Medicaid Services (2017), “Dual Eligible Beneficiaries Under Medicare and Medicaid,” Medicare Learning Network booklet.
- Chodorow-Reich, G. (2017), “Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?,” working paper.
- Chodorow-Reich, G., L. Feiveson, Z. Liscow and W. Woolston (2012), “Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act,” *American Economic Journal: Economic Policy*, 4, 118-145.
- Chodorow-Reich, G., and L. Karabarbounis (2017), “The Limited Macroeconomic Effects of Unemployment Benefit Extensions,” working paper.
- Clemens, J. and S. Miran (2012), “Fiscal Policy Multipliers on Subnational Government Spending,” *American Economic Journal: Economic Policy*, 4, 46-68.
- Cochrane, J. (2012), “Manna from Heaven: The Harvard Stimulus Debate,” Web blog post, *The Grumpy Economist*, March 4, accessed August 14, 2015.

- Conley, T. and B. Dupor (2013), “The American Recovery and Reinvestment Act: Solely a Government Jobs Program?” *Journal of Monetary Economics*, 60, 535-549.
- Council of Economic Advisers (2014), “Missed Opportunities: The Consequences of States Decisions Not to Expand Medicaid,” July.
- Cox, D. (1958), *Planning of Experiments*, New York: Wiley.
- Cubanski, J., T. Neuman and A. Damico (2016), “Medicare’s Role for People Under Age 65 with Disabilities,” Kaiser Family Foundation Issue Brief, August.
- Dague, L., T. DeLeire and L. Leininger (2014), “The Effect of Public Insurance Coverage for Childless Adults on Labor Supply,” *American Economic Journal: Economic Policy*, 9, 124-154.
- Dave, D., S. Decker, R. Kaestner and K. Simon (2015), “The Effect of Medicaid Expansions in the Late 1980s and Early 1990s on the Labor Supply of Pregnant Women,” *American Journal of Health Economics*, 1, 165-193.
- Davis, M. and S. Burner (1995), “DataWatch: Three Decades of Medicare: What the Numbers Tell Us,” *Health Affairs*, 14, 231-243.
- Delattre, E. and B. Dormont (2003), “Fixed fees and physician-induced demand: A panel data study on French physicians.” *Health Economics*, 12(9), 741-754.
- Department of Health and Human Services (2015), “2015 Actuarial Report on the Financial Outlook for Medicaid,” Office of the Actuary, Center for Medicare and Medicaid Services.
- Dupor, B. and R. Guerrero (2017), “Local and Aggregate Fiscal Policy Multipliers,” *Journal of Monetary Economics*, 12, 16-30.
- Dupor, B. and R. Li (2015), “The Expected Inflation Channel of Government Spending in the Postwar U.S.,” *European Economic Review*, 74, 36-56.
- Foster, R. (2000), “Trends in Medicare Expenditures and Financial Status, 1966-2000,” *Health Care Financing Review*, 22, Fall.
- Furth, S. (2013), “Research Review: What Can Be Learned from Local Multiplier Estimates,” Heritage Foundation.
- Garrett, B. and R. Kaestner (2015), “Recent Evidence on the ACA and Employment: Has the ACA Been a Job Killer?” The Urban Institute.
- Garthwaite, C., T. Gross and M. Notowidigdo (2014), “Public Health Insurance, Labor Supply, and Employment Lock,” *The Quarterly Journal of Economics*, 129, 653-696.

- Gibson, R. (1980), "National Health Expenditures, 1979," *Health Care Financing Review*, Summer.
- Gibson, R. and C. Fisher (1978), "National Health Expenditures, Fiscal Year 1977," *Social Security Bulletin*, 41, 7, 3-20.
- Gibson, R., K. Levit, H. Lazenby and D. Waldo (1984), "National Health Expenditures, 1983," *Health Care Financing Review*, 6, 3, 1-29.
- Gooptu, A., A. Moriya, K. Simon and B. Sommers (2016), "Medicaid Expansion Did Not Result in Significant Employment Changes or Job Reductions in 2014," *Health Affairs*, 35, 1, 111-118.
- Gornick, M., J. Greenberg, P. Eggers and A. Dobson (1985), "Twenty years of Medicare and Medicaid: Covered Populations, Use of Benefits, and Program Expenditures," *Health Care Financing Review*, December, 13-59.
- Greenstone, M. and A. Looney (2012), "The Role of Fiscal Stimulus in the Ongoing Recovery," The Hamilton Project, July 6.
- Gruber, J. and B. Madrian (2004), "Health Insurance, Labor Supply, and Job Mobility: A Critical Review of the Literature," in McLaughlin, C. (ed.), *Health Policy and the Uninsured*, Washington, D.C.: Urban Institute Press.
- Grytten, J. and R. Sørensen (2001), "Type of contract and supplier-induced demand for primary physicians in Norway," *Journal of Health Economics*, 20(3), 379-393.
- Hall, R. (2009), "By How Much Does GDP Rise If the Government Buys More Output?" *Brookings Papers on Economic Activity*, Fall, 40, 183-249.
- Jacobson, G., T. Neuman and A. Damico (2012), "Medicare's Role for Dual Eligible Beneficiaries," Kaiser Family Foundation Medicare Policy Issue Brief, April.
- Jordà, O. (2005), "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95, 1, 161-182.
- Kaestner, R., B. Garrett, J. Chen, A. Gangopadhyaya and C. Fleming (2017), "Effects of ACA Medicaid Expansions on Health Insurance Coverage and Labor Supply," *Journal of Policy Analysis and Management*, 36, 608-642.
- Kaiser Family Foundation (2013), "Premiums and Cost-Sharing in Medicaid," Kaiser Commission on Medicaid and the Uninsured, February.
- Kaiser Family Foundation (2015), "The Latest on Geographic Variation in Medicare Spending," Report, October.

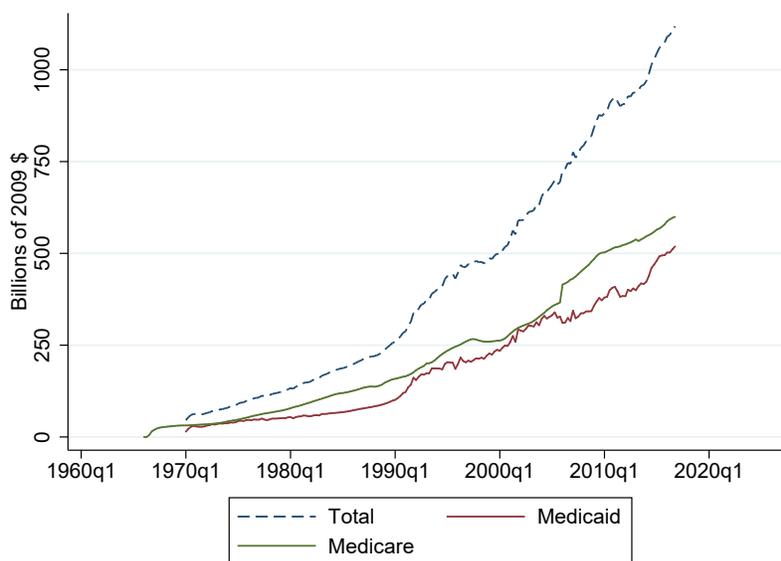
- Leeper, E. N. Traum and T. Walker (2017), "Clearing Up the Fiscal Multiplier Morass," *American Economic Review*, 107, 8, 2409-2454.
- Leung, P. and A. Mas (2016), "Employment Effects of the ACA Medicaid Expansions," NBER Working Paper no. 22540.
- Levine, M. and M. Buntin (2013), "Why Has Growth in Spending for Fee-for-Service Medicare Slowed?," Congressional Budget Office Working Paper.
- Levit, K., C. Smith, C. Cowan, H. Lazenby, A. Sensenig and A. Catlin (2003), "Trends in U.S. Health Care Spending, 2001," *Health Affairs*, 22, 1, 154-164.
- Lin, S. (2009), "Economic fluctuations and health outcome: A panel analysis of Asia-Pacific countries," *Applied Economics*, 41(4), 519-530.
- Mandel, B. (2014), "Affordable Care Act Transactions in the National Income and Product Accounts," *BEA Briefings*, June 2014.
- Marinescu, I. (2017), "The General Equilibrium Impacts of Unemployment Insurance: Evidence from a Large Online Job Board," *Journal of Public Economics*, 150, 14-29.
- McInerney, M. and J. Mellor (2012), "Recessions and seniors' health, health behaviors, and health-care use: Analysis of the Medicare Current Beneficiary Survey," *Journal of Health Economics*, 31, 744-751.
- Mian, A. and A. Sufi (2012), "The Effects of Fiscal Stimulus: Evidence from the 2009 Cash for Clunkers Program," *Quarterly Journal of Economics*, 127, 3, 1107-1142.
- Michaillat, P. (2014), "A Theory of Countercyclical Government Multiplier," *American Economic Journal: Macroeconomics*, 6, 190-217.
- Miller, D., M. Page, A. Stevens and M. Filipksi (2009), "Why Are Recessions Good for Your Health?," *American Economic Review: Papers & Proceedings*, 99, 2, 122-127.
- Nakamura, E. and J. Steinsson (2014), "Fiscal Stimulus in a Monetary Union: Evidence from U.S. Regions," *American Economic Review*, 104, 753-792.
- Owyang, M., V. Ramey and S. Zubairy (2013), "Are Government Spending Multipliers Greater during Periods of Slack? Evidence from Twentieth-Century Historical Data," *American Economic Review: Papers & Proceedings*, 103, 129-134.
- Ramey, V., (2011), "Can Government Purchases Stimulate the Economy?" *Journal of Economic Literature*, 49, 673-685.

- Ramey, V. (2012), "Government Spending and Private Activity," NBER Working Paper.
- Ramey, V. and S. Zubairy (2018), "Government Spending Multipliers in Good Times and in Bad: Evidence from U.S. Historical Data," *Journal of Political Economy*, 126, 2, 850-901.
- Richardson, J. and S. Peacock (2006), "Supplier-Induced Demand," *Applied Health Economics and Health Policy*, 5(2), 87-98.
- Romer, C. (2012). "The Fiscal Stimulus, Flawed but Valuable," *New York Times*, October 20.
- Romer, C. and D. Romer (2016), "Transfer Payments and the Macroeconomy: The Effects of Social Security Benefit Increases, 1952-1991," *American Economic Journal: Macroeconomics*, 8, 1-42.
- Ruhm, C. (2000), "Are Recessions Good For Your Health?," *Quarterly Journal of Economics*, 115(2), 617-650.
- Savord, G. (1999), "Analysis of PPS Hospital Case-Mix Change Between 1997 and 1998," Office of the Actuary, Health Care Financing Administration, November 19.
- Shoag, D. (2012), "Using State Pension Shocks to Estimate Fiscal Multipliers Since the Great Recession," *American Economic Review*, 103, 3, 121-124.
- Stevens, A., D. Miller, M. Page and M. Filipki (2015), "The Best of Times, the Worst of Times: Understanding Pro-cyclical Mortality," *American Economic Journal: Economic Policy*, 7, 4, 279-311.
- Suárez Serrato, J. and P. Wingender (2014), "Estimating Local Fiscal Multipliers," Duke University, working paper.
- Sullivan and von Wachter (2009), "Job Displacement and Mortality: An Analysis Using Administrative Data," *The Quarterly Journal of Economics*, 124, 3, 1265-1306.
- Weller, C. and G. Gelzinis (2017), "Approximated Employment Effects of the American Health Care Act," Center for American Progress website, March 20, accessed Aug. 22, 2017.

A The Data

Our GFHC data are from Bureau of Economic Analysis (BEA) state personal income reports. The BEA obtains the data from the Centers for Medicare and Medicaid Services (CMS).⁵⁵ Specifically, we use two state-level quarterly series from these reports: Medicare benefits and Medicaid benefits. Figure A.1 plots Medicaid and Medicare spending at the national level, as well as the sum of the two.

Figure A.1: Government spending, national measure and aggregated state-level data



Notes: Annualized rate.

A.1 Medicare Benefits

The BEA series on Medicare benefits consists of federal government payments made directly or through intermediaries to vendors for care provided to individuals under the Medicare program (BEA, 2016). The state-level figures are estimated by the CMS. The BEA constructs quarterly estimates of Medicare benefits at the state level by extrapolating annual trends in state shares of the nation. Furthermore, due to lag in availability, the data for 2010 forward have been extrapolated by the BEA using Medicare enrollment.

These data cover all expenditures (with the exception of administrative expenses) from the Hospital Insurance trust fund and the Supplementary Medical Insurance trust fund. Hence, the

⁵⁵We accessed the data via Haver Analytics.

data we use provide a comprehensive measure of the benefits provided by Parts A through D of the Medicare program.

The Medicare benefits panel is available at a quarterly frequency beginning in the third quarter of 1966, soon after the program was established. Our last observations correspond to the first quarter of 2017. Note that while Medicare Part A and Part B were instituted in 1966, Medicare Part C—which subsidizes privately managed plans—began operating in 1985. Similarly, Medicare Part D—which covered outpatient prescription drugs—started in 2006. The BEA allocated the national estimate for Part D benefits to states using the enrollment counts reported by the CMS (BEA, 2016).

Table A.1 decomposes national data for 2016 into the four parts that currently make up the Medicare program. In 2016, Parts A, B, and D represented, respectively, 41.9 percent, 43.2 percent, and 14.9 percent of the national Medicare data. Note that Part C beneficiaries must also be enrolled in Parts A and B, and payments are made in appropriate parts from the HI and SMI trust funds to the private health insurance plans. When considered separately, 28.2 percent of total benefits paid in 2016 consisted of subsidies to private plans via Medicare Part C.

Table A.1: Medicare data for calendar year 2016 (billions of USD)

	HI trust fund Part A	SMI trust fund Part B	Part D	Total
Total benefits	\$280.5	\$289.5	\$99.5	\$669.5
Part C	\$85.2	\$103.4	-	\$188.6
Other benefits	\$195.3	\$186.1	\$99.5	\$480.9

Source: Authors’ calculations based on the 2017 Boards of Trustees for Medicare Report.

It should be noted that the Medicare state panel we use reflects expenditures on benefits irrespective of the sources of financing of these expenses. As explained in Section 2.2.1, the HI trust fund and the SMI trust fund receive their revenue from multiple sources, including payroll taxes, general revenue funds, premiums, and transfers from states.

A.2 Medicaid Benefits

The BEA series on Medicaid benefits consists of payments made directly or through intermediaries to vendors for care provided to individuals under the federally assisted, state-administered Medicaid program, and the Title XIX Medicaid expansion portion of the Children’s Health Insurance Program (BEA, 2016).

The BEA distributes the annual estimates of Medicaid benefits into quarters and extrapolates using quarterly data from the CMS-64 Quarterly Expense Report. The state-level estimates of these benefits are also constructed by the BEA based on data from the CMS (BEA, 2016).