Big Government and Dynamism Drain*

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Abstract

Permanent expansions in government purchases have positive short-run effects but can reduce employment and output in the medium run. We analyze shocks raising government purchases in German regions to a permanently higher level. The short-run effects were positive in regions undergoing recessions. However, in regions with strong fundamentals, employment and output per worker fell several years after government purchases had stabilized at a higher level. The negative effects were strongest in high-growth and low-unemployment regions and in industries selling directly to governments. The findings point to a "dynamism drain" effect: firms expect stable future revenues from sales to the government, become passive in their hiring and investment, and ultimately grow more slowly.

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1 Introduction

The state plays a large role in many advanced economies. Government purchases from firms amount to roughly 10% of GDP in the US today, having risen by around 2% per year in real terms in recent decades.[1] How do "big governments" that consistently purchase a large share of output from firms affect the economy? Do permanently high government purchases raise employment and output to a permanently higher level or do they distort competitive incentives and firm dynamics, ultimately reducing employment and output?

It is challenging to study the effects of big governments because researchers rarely observe shocks that increase government purchases to a permanently higher level. Shocks to government purchases often lead to time-varying spending paths, for example, transitory bursts in spending or slow build-ups. Such shocks with time-varying spending paths make it difficult to infer the effects of permanently high purchases maintained by big governments. One reason is that permanently high government purchases may lead firms to expect stable future revenues and may thus weaken firms' incentives to invest and compete in the marketplace, whereas time-varying spending paths may offer firms less certainty. Moreover, the short-run effects of greater purchases may depend on temporary frictions, such as nominal rigidities, that can lead output and employment to rise following every additional increase in government purchases. Such frictions may be less important once government purchases have stabilized at a higher level for several years, implying that the effects immediately following an increase may differ from the medium-run effects.

The contribution of this paper is to show that a permanent increase in government purchases can lower the growth of employment and output per worker in the medium run. We identify a shock to annual government purchases in German regions triggered by a census recount. The shock did not change aggregate government spending in the German economy and was not financed by greater taxes in any region. Instead, the shock permanently reallocated funding and purchases across regions.

Consistent with a standard stimulus effect, we find that greater purchases raised employment and output per worker in regions undergoing recessions, especially during the initial year when purchases increased. However, in booming regions where growth was previously high and unemployment low, employment and output per worker did not change in the short run and decreased several years after purchases had stabilized at a permanently higher level. Our empirical findings are consistent with a "dynamism drain" effect: when government purchases from firms increase permanently, firms con-

[1] Similar numbers apply to Germany and OECD countries on average. See OECD Data Explorer, public finance by economic transaction, 2019 under data-explorer.oecd.org.

centrate on selling to the government, become passive in their efforts to win marketplace customers, hire and invest less, and ultimately reduce employment and output per worker. The findings suggest that permanently elevated government purchases can weaken growth in economies with otherwise strong potential.

To study the effects of permanently elevated government purchases, we use a quasi-experimental cross-region research design. The empirical challenge is that government purchases are endogenous to other determinants of employment and output. We analyze variation in municipal government purchases driven by corrections to official population counts after a census. German federal states distribute funds to their constituent municipalities every year. The greater the population of a municipality, the more funds it receives. In 2011, the German government conducted a census to correct mistakes in official population records. The census led to large, unexpected, and permanent changes in official population counts. The census results were released in 2013. Different states adopted the census for the allocation of funds at different points in time between 2013 and 2017, depending on idiosyncratic political processes and often only announcing the adoption a few months in advance. Municipalities and firms therefore did not anticipate the effects of the census on budgets or economic outcomes. Once adopted in a state, the census led to a redistribution of funds across municipalities. There were no direct effects on current and future taxes in any municipality and no changes in aggregate government purchases in Germany.

We generate a municipality-level treatment variable that measures variation in the population corrections due to the census. Our main analyses compare municipalities within the same state and year, holding constant municipality size and other characteristics. The population corrections were largely driven by random errors in official birth, death, and migration records. Accordingly, the corrections are geographically dispersed and not associated with municipality characteristics.

Government purchases and employment in municipalities with a greater census shock were on parallel trends to other municipalities before the adoption of the census, suggesting that these municipalities were not systematically exposed to different shocks. Moreover, even in the states that adopted the census after the results release in 2013, government purchases and employment were on parallel trends before the adoption, implying no anticipation effects. However, our findings do not depend on the adoption timing or potential anticipation effects since the results are similar when we analyze purely cross-sectional specifications where the shock measure does not depend on the timing. One state never adopted the census and one state adopted only in 2017, allowing us to construct a placebo test documenting that census population corrections were only associated with economic outcomes if they affected the allocation of funds in the state.

We confirm that municipalities exposed to a greater census shock received consistently more funds from higher levels of government throughout the years after the census was adopted, at the expense of municipalities with a lower census shock. Municipalities with a greater census shock used the additional funds to increase government purchases from firms, which include expenditures on intermediate goods and services (e.g., for public education, cultural events, furniture, equipment, etc.), maintenance of assets (cleaning, landscaping, etc.), and rents (for buildings). We do not find evidence that the 2011 census affected other budget positions, such as taxes, debt, saving, transfers to households, or expenditures on government personnel. The finding that governments mostly raise purchases after a funding shock, rather than adjusting other positions, is consistent with the literature on the "flypaper effect" (Hines and Thaler 1995).[2] Since the census-induced changes in municipal funding translated almost entirely into greater government purchases from firms, we interpret the effects of a greater census shock as the result of permanently greater government purchases.

We start by analyzing the average effects on firm employment. In the first year after census adoption, employment in municipalities with a greater census shock increased slightly on average, relative to those with a lower shock. However, average employment in municipalities with a greater census shock was significantly lower starting in the third year after the shock and remained lower until the end of our sample period in the fifth year.

We investigate the economic mechanisms behind the initially positive and subsequently negative average employment effects. Keynesian theory suggests that the effects of government purchases differ between recessions and booms. We therefore separately analyze "bust" municipalities, where growth in the year before census adoption was negative, and "boom" municipalities, where pre-growth was positive. In bust municipalities, we find significant gains in the first two years after the adoption of the census and weakly positive effects in subsequent years. This pattern is consistent with Keynesian models, where government purchases in bust regions can have large effects in the short run (thanks to nominal rigidities or other transitory frictions) and weaker effects in the medium run. The magnitude of the short-run effect in bust municipalities is close to estimates from other papers studying regional recessions (as reviewed by Chodorow-Reich 2019).

In boom municipalities, we find weak effects of the census shock in the first two

[2]While the political mechanisms generating the effect on purchases are not essential for our argument, we discuss below potential reasons for the stronger response of purchases, relative to other budget positions. These include greater flexibility and discretion for municipal governments in making purchases, as opposed to other budget changes, and the relative satiation with long-run capital projects, given that much spending had taken place due to the 2009-11 stimulus following the Great Recession.

years after the adoption of the census. This finding is consistent with empirical results for boom regions in the literature (e.g., Adelino et al. 2017; Berge et al. 2021) as well as the Keynesian notion that government spending has weak effects when growth is already high and there are few slack resources. The short-run effects we document are relatively standard in the literature, so our remaining analysis focuses on the outcomes in subsequent years.

We find negative effects of the census shock on employment in boom municipalities starting in the third year after the adoption of the census. Similarly, we find negative effects in municipalities with below-median unemployment rate starting in the third year. These results do not imply that firms in boom municipalities actively fired workers when government purchases increased, since the estimated impact of a large census shock was below the typical employment growth in boom municipalities (e.g., median growth before census adoption). Instead, the results suggest that firms in boom municipalities became more passive in their hiring and did not grow employment as fast as they otherwise would have.

The negative employment effects could be driven by the direct responses of suppliers to municipal governments that experienced increases in government demand or by general equilibrium spillover effects, such as changes in local factor prices, that affect all local firms. We measure which industries directly sell to municipal governments by reading through municipal tenders and identifying which industry would fulfill the contracts. There were strong negative effects on employment in industries directly selling to municipal governments starting in the third year after census adoption, but no significant effects in other industries. This finding implies that establishments directly supplying governments adjusted their behavior when government purchases increased, which largely generated the negative effects. We also document that the effects were primarily driven by municipalities receiving greater increases in government purchases and not by municipalities facing withdrawals of purchases. This asymmetry may be explained by the fact that firms in boom municipalities faced a tight labor market, making it difficult to significantly expand hiring, or that firms conservatively maintain their previous hiring strategy in response to lower government demand. We do not find any evidence that the relocation of firm establishments or migration of people across municipalities can account for the effects of the census shock.

Going beyond employment, we estimate the effect on output per worker, which is only observed at the level of counties (groups of municipalities). The dynamics of output per worker resemble those for employment. In boom counties with strong pre-growth or low unemployment, the effects on output per worker in the first two years were small before turning negative in subsequent years. Cross-region differences in output per worker growth are driven by differences in firms' investment in production

capital, technologies, or the human capital of workers. The reductions in output per worker growth therefore suggest that firms became more passive in their investment decisions, which led to lower growth in either capital, total factor productivity (TFP), or both.

The empirical evidence on the negative effects of government purchases in boom municipalities is inconsistent with standard models featuring unconstrained firms. In standard Keynesian and neoclassical models, windfall-financed government purchases either raise employment and output or have negligible effects, depending on factor supply elasticities and the availability of slack resources. The evidence in this paper requires a distinct mechanism that leads firms directly selling to governments to become more passive when government purchases increase permanently.

We discuss potential mechanisms that may help to explain the negative medium-run effects in boom municipalities. One possibility is that selling to private market customers involves higher upfront investments than specializing in government sales. For instance, in order to attract private customers, firms may have to regularly innovate new products or spend resources on marketing. In contrast, governments may reliably purchase standardized products requiring little upfront investment. Once government purchases increase permanently, it becomes more attractive to specialize in government sales, especially since many firms can expect to remain government suppliers for decades (Cox et al. 2024). In addition, selling to the government may yield stable and predictable returns, whereas investing into private market sales may be relatively risky (e.g., due to business cycle and innovation risk). As a result, firms may decrease risky private market investments when government purchases rise permanently, resulting in lower average employment and output. This mechanism becomes stronger when firm owners are more risk-averse.

Relatedly, firms may maximize the probability of survival or of reaching earnings targets, rather than maximizing expected profits. Greater government purchases could imply that focusing on government sales and neglecting the marketplace is the most efficient way of reaching these objectives. Alternatively, capacity-constrained firms may find it optimal to forego private sales altogether when government purchases increase, although capacity constraints may not be durable enough to explain effects even after 5 years.

To explore how firm behavior changes in response to government purchases, we survey 5,000 managers. We ask how hard they would try to win additional orders when they are told that government orders will reliably change throughout the next 10 years. Half of the managers are randomly selected to see a statement mentioning "more orders," whereas the other half see a statement mentioning "fewer orders." Managers who are told that government orders will increase are significantly less likely to try

to win additional orders and customers, relative to those who are told that orders will decrease. These results do not specifically favor one mechanism over another, but imply that firms' behavior in the private marketplace changes when government purchases increase, consistent with the view that government purchases can make firms less dynamic.

Taken together, the empirical results suggest that greater government purchases led to dynamism drain after the German census shock: firms in booming regions became more passive in their hiring, investment, and private customer acquisition when government purchases increased. It likely takes time for passive hiring and investment to significantly affect firm growth and stimulus effects may outweigh dynamism drain in the short run. As a result, the negative effects took hold after several years.

Dynamism drain is just one channel through which government purchases affect employment and output. Dynamism drain may not always play a first-order role when government purchases increase. For instance, a stimulus effect dominated in German regions undergoing recession, whereas productivity gains may dominate in response to government spending on R&D. Nonetheless, this paper suggests that policymakers may want to be cautious when permanently expanding government purchases because such measures can depress the dynamics of a market economy.

2 Relation to the Literature

One of the oldest debates in economics concerns the proper role of the state. A large literature shows that permanent government ownership of firms can improve the quality of public services, but may reduce firms' incentives to invest, lowering efficiency (e.g., Aminadav and Papaioannou 2020; Colonnelli et al. 2024; Hart et al. 1997; La Porta et al. 1999; Megginson and Netter 2001; Shleifer 1998). This paper suggests a related but distinct trade-off with respect to permanent government purchases: they can stimulate growth during recessions, but may reduce firms' incentives to compete and invest, thereby lowering medium-run employment and output per worker in booming economies.

Several papers document positive effects of government spending on employment and output using cross-region research designs (reviewed by Chodorow-Reich 2019) and time series studies (reviewed by Ilzetzki et al. 2013 and Ramey 2016, 2019). However, our findings do not contradict this work. The effects of government spending may be heterogeneous and may depend on (at least) three factors: the type of spending undertaken by the government; whether the spending shock is permanent or time-varying; and the time horizon of the analysis. Our analysis differs from existing work because it combines three features that previous papers did not jointly analyze: in-

creased government purchases from firms (as opposed to spending on R&D, military, long-run capital investment, or transfers to households); the dynamic nature of the census shock as a one-off level shifter in permanent government purchases; and the effects several years after a permanent increase in purchases.

By combining these three features, our analysis is relatively well suited to analyzing the effects of "big governments" that reliably purchase goods and services from firms for many years. The types of government purchases triggered by the census shock mostly fall under the OECD category of government intermediate consumption, which on its own accounts for roughly 60% of total government spending in the US, Germany, and on average in the OECD. (The remaining 40% include, for example, investments in R&D, long-run capital, or the military.) Our results therefore speak to a sizable part of government activities in advanced economies.

Our findings should not be taken to imply that government purchases always reduce employment and output. Differences in the type of spending undertaken by governments may partly explain different findings in the literature (Boehm 2020; Cox et al. 2024; Gechert 2015). Military spending is a case in point (Ramey and Shapiro 1998). Antolin-Diaz and Surico (2024) report that US military spending raised R&D and thereby long-run output. Ilzetzki (2024) finds that spending on military airplanes improved productivity during World War II, potentially through the adoption of mass production. Briganti (2023) argues that "learning-by-doing" occurs particularly fast in military production, leading to larger output effects of military spending. Similarly, public R&D can directly improve productivity (Fieldhouse and Mertens 2024; Gross and Sampat 2023; Kantor and Whalley 2024; Moretti et al. 2025) and large infrastructure programs can raise long-run output by improving capital allocation (Donaldson and Hornbeck 2016; Fernald 1999; Ramey 2020).[3] These papers all suggest that government spending can raise output if it targets productivity directly, improves capital allocation, or operates in sectors with high potential productivity gains. We instead focus on government intermediates purchases, which constitute 60% of total government spending in the OECD but may be less effective at stimulating productivity.

In addition, the dynamic nature of the census shock that we study sets apart our analysis from most existing work and may partly explain why we find negative effects. The census shock led to a one-off, stepwise increase in government purchases that lifted purchases to a permanently higher level. This dynamic pattern may affect the strength of two mechanisms: first, firms' incentives to invest in private market sales, and second, nominal rigidities and other transitory frictions.

[3]In the same vein, place-based policies typically operate through infrastructure programs and investment incentives for firms, both of which can facilitate capital reallocation toward the region and can raise regional productivity and output (Glaeser and Gottlieb 2008; Kline and Moretti 2014).

First, firms in municipalities with a greater census shock expected and received a permanently higher level of government purchases after the census shock. As explained in Section 5, this relatively high degree of certainty about sales to the government may reduce the incentives of firms to compete for private customers and invest in new products, which may ultimately generate dynamism drain. Consistent with this view, Cox et al. (2024) show that many firms remain government suppliers for decades, receiving predictable government revenue every year. In contrast, many government spending shocks studied in the literature generate time-varying patterns of annual spending, for example, transitory bursts in spending or slow build-ups and subsequent declines over many years (e.g., Blanchard and Perotti 2002; Mountford and Uhlig 2009; Ramey 2011). In such cases, firms may either expect the shock to be temporary or may be uncertain about its long-run persistence. Hence, following such time-varying shocks, firms may be able to predict future government sales less well, may rely less on the government as a permanent customer, and may thus be more incentivized to compete for and invest in private market sales. In that sense, the permanent nature of the census shock may partly explain why we find negative medium-run effects. The census shock may thus be more informative about shocks leading to permanently higher purchases, but less about shocks with time-varying spending patterns, such as temporary fiscal stimulus.

A second implication of the dynamics of the census shock concerns the role of nominal rigidities and temporary frictions. In some models, the short-run employment effects of greater government spending are larger when nominal wages and prices are rigid (e.g., Nakamura and Steinsson 2014). Nominal rigidities may bind in the initial years after every additional increase in government spending and then take several years to unwind. As a result, if government spending increases steadily every year during a multi-year build-up, employment may increase in tandem every year because nominal wages and prices are rigid relative to the previous year. In that case, the effects of a steady build-up may not be informative about the effects of permanently higher government purchases because temporary rigidities become less important over time. In contrast, the census shock is informative about permanently higher government purchases because purchases increased once in a stepwise fashion and then remained relatively stable for many years afterward. Given the different time profiles, it is therefore difficult to compare results from the existing literature to the estimated

medium-run effects of the census shock.[4]

Despite differences in the permanence of the shock, the positive short-run effects of the census shock are consistent with much of the literature. Moreover, in line with our results, several cross-regions studies find short-run effects that are close to zero or statistically insignificant in high-growth or low-unemployment regions (e.g., Adelino et al. 2017; Berge et al. 2021; Nakamura and Steinsson 2014; Shoag 2016). The time series evidence on state dependence is less clear.

Our approach builds on previous studies using cross-region variation due to population counts (e.g., Corbi et al. 2019; Suárez Serrato and Wingender 2016) and other shocks to study local economic outcomes. Corbi et al. (2019) and Adelino et al. (2017) find positive employment effects of spending in Brazilian and US municipalities, respectively, but estimate short-run effects (rather than medium-run outcomes) and study broad shocks affecting transfers to households, the hiring of government workers, infrastructure investments, and purchases simultaneously. Such broad shocks may have different effects than the government purchases from firms induced by our census shock: government transfers and hiring generate wealth effects for consumers but affect firm incentives differently, whereas infrastructure investments can affect capital allocation and productivity, as already discussed above. Cohen et al. (2011) find that broad spending shocks over congressional terms (6 years on average) reduced employment, consistent with a mechanism operating through household transfers and household wealth effects. In comparison, our findings focus on government purchases (not broader shocks), on firm dynamics in the medium run (not household responses), and are not driven by household wealth effects (as discussed in Section 5.7). Suárez Serrato and Wingender (2016) analyze shocks due to US census recounts. Again, one difference is that these shocks broadly affected transfers to households, government hiring, infrastructure investment, and purchases simultaneously. A further difference is that local government spending steadily increased every year for at least 5 years after US census shocks. As explained above, due to nominal rigidities and other temporary frictions, the effects during the years when spending was increasing may differ from the effects several years after spending has permanently increased. As far as they are comparable, the results in Suárez Serrato and Wingender (2016) are consistent with ours, for instance, we both find positive short-run effects and heterogeneity by pre-growth.[5] Relatedly, Hager and Hilbig (2024) study the effects of the 2011 German

[4]Despite these differences, there is some suggestive evidence in the time series literature that even temporary government purchases can slow dynamics in the medium run. For example, the output effects estimated by Auerbach and Gorodnichenko (2012) for boom periods turn significantly negative 4 years after a shock to government spending. Similarly, the output effects estimated by Ramey and Zubairy (2018) for low-unemployment periods are marginally negative after 4 years. In both cases, however, the shock to spending is transitory and tax changes may also play a role.

[5]The average results of the US censuses may be more comparable to our results on bust regions

census on political vote choices, but do not analyze employment, output per worker, or dynamics over time.

Finally, we note that our cross-region estimates are not directly informative about aggregate effects because of cross-region spillovers, although they can be made comparable through the lens of models (e.g., Auclert et al. 2024; Chodorow-Reich 2019; Farhi and Werning 2016; Nakamura and Steinsson 2014; Ramey 2021, 2019; Wolf 2023). The aim of our paper is not to produce an estimate that directly informs an aggregate effect. Instead, we want to highlight a new dynamism drain mechanism that can reduce labor demand by firms and output per worker. This mechanism shapes the effects of government purchases at any level of aggregation.[6]

3 Empirical Approach

3.1 Institutional Details on the 2011 Census

The funding of municipal governments in Germany crucially depends on population size. The German constitution dictates that federal states must distribute funds among their constituent municipalities. The amount received by each municipality is determined by so-called allocation formulas (*Schlüsselzuweisungen*), in which the most important factor by far is municipal population. The average municipality received 15% of its annual budget through such allocations, around 300 Euro per capita in 2011, so changes in official population records strongly impact municipal budgets.

Population changes are typically measured using records on moves, births, and deaths. These records are subject to error because they rely on household self-reporting and proper handling by government administrators. In 2011, the European Union initiated censuses in all its member states with the aim of generating accurate population records. The most recent censuses had been conducted in 1987 in West Germany and in 1981 in the East, so errors in the population counts had accumulated for over two decades.

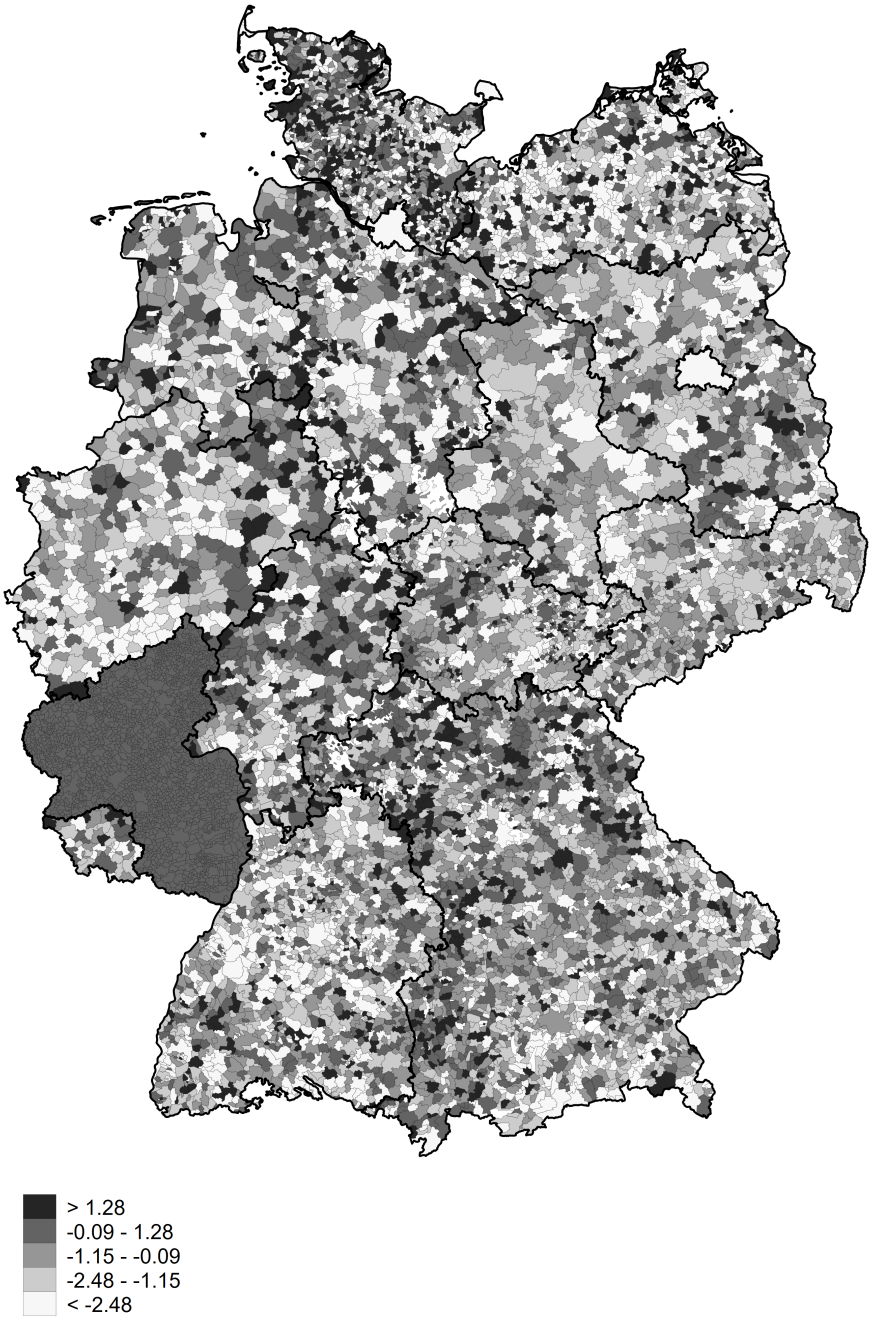
The German government conducted a census in 2011 and released the results in 2013. The results revealed substantial heterogeneity across municipalities in the extent to which previous population records differed from the census figures, as shown by the map in Figure 1. The distribution of census-induced population changes is relatively symmetric, as shown in Figure 2. The distribution is similar in "boom" municipali-

because most US censuses were implemented during or just after US recessions. In the US, the 1970 census was followed by the 1973-75 recession, the 1980 census by the 1981-82 recession, the 1990 census by the 1990-91 recession, and the 2000 census by the 2001 recession.

[6]We find no evidence that firms or households relocated in response to the census shock in Section 4.8, implying that spillovers due to cross-region relocation do not account for the negative effects.

ties with positive employment growth in the year before census adoption and "bust" municipalities with negative employment pre-growth, as shown in Figure A1.

Figure 1: Map of the percentage change in 2011 population due to the census



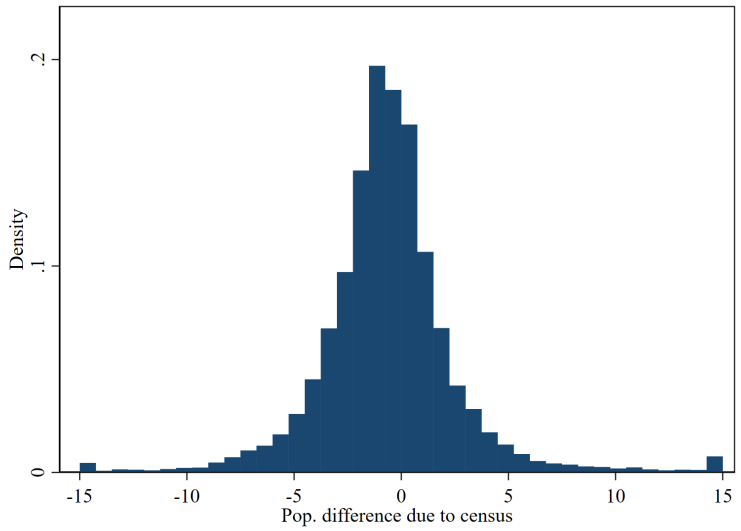
Notes: The map shows the difference in municipal population due to the 2011 census, given by: 100 * (post-census 2011 pop. − pre-census 2011 pop.) / (pre-census 2011 pop.), using data from the German Statistical Office. We set values for Rhineland-Palatine to zero because the census was never adopted in that state.

Table 1: Adoption of the census for the allocation of funds

Federal state	Adoption year
Baden-Württemberg	2014 (1/3), 2015 (2/3), 2016 (full)
Bavaria	2014
Brandenburg	2013
Bremen	2013
Hesse	2014
Lower Saxony	2014
Mecklenburg-Vorpommern	2013
North Rhine-Westphalia	2014 (1/3), 2015 (2/3), 2016 (full)
Rhineland-Palatinate	never
Saarland	2013
Saxony	2013
Saxony-Anhalt	2017
Schleswig-Holstein	2014
Thuringia	2014

Notes: The table reports the year, in which federal states adopted the census to determine the allocation of state funds across municipalities. Baden-Württemberg and North Rhine-Westphalia introduced two phase-in years in 2014 and 2015, during which a weighted average of pre- and post-census population determined the allocation of state funds across municipalities. Berlin and Hamburg are not included because they are city states without constituent municipalities.

Figure 2: Change in 2011 population due to the census



Notes: The figure contains a histogram of the difference in municipal population due to the 2011 census, given by: 100 * (post-census 2011 pop. − pre-census 2011 pop.) / (pre-census 2011 pop.), using data from the German Statistical Office. Values greater than 15 and lower than -15 are combined into two bins for either extreme.

The revised population figures did not immediately affect the funds received by all municipalities. While all states used population-based allocation formulas, state governments had to decide whether and how to adopt the new population figures for the allocation of funds across municipalities. The adoption was delayed in some states due to differences in state politicians' priorities at the time, as shown in Table 1. Some states adopted the census immediately when results were released in 2013. Other states delayed the adoption, for example, Saxony-Anhalt only adopted the census in 2017. Rhineland-Palatinate traditionally had not used census revisions for its internal fund allocation, including the most recent 1987 census, and also never adopted the 2011 census. It was typically not possible for municipalities and firms to predict when exactly the census would be adopted by their state and many state governments only announced the adoption a few months in advance.

Once the census was adopted in a state, it led to a redistribution of funding across municipalities: those with a lower census shock, relative to the median in the state, tended to lose funding, whereas those with a greater census shock, relative to the state median, tended to gain funding. The adoption of the census did not change aggregate funding or any taxes, but redistributed existing tax revenue across municipalities, leading to winners and losers.

In the remainder of the paper, we identify shocks to municipal budgets using municipality-level variation in the population corrections due to the census. In some specifications, we also use state-level variation in the timing of census adoption. The results are similar whether we use only cross-municipality variation or additionally use the variation in adoption timing.

3.2 Data

The Federal Statistical Office provides 2011 municipal population data from the census and the 2011 population records that would have been used in the absence of the census. Both refer to the same date and therefore allow us to calculate the impact of the census on population counts. We calculate our main treatment variable using the relative difference in the official population of municipality i induced by the census:

$$\text{census shock}_i = \frac{\text{post-census pop. } 2011_i - \text{pre-census pop. } 2011_i}{\text{pre-census pop. } 2011_i} \times \frac{1}{3 \text{ s.d.}}. \quad (1)$$

To make the coefficients estimated later on easily readable, we standardize this census shock variable by 3 sample standard deviations of the relative population difference ("3 s.d."). The estimated coefficients on the census shock can thus be interpreted as the effects of a relatively large (3 standard deviation) increase in population due to the census.

Table 2: Summary statistics

VARIABLES	(1) mean	(2) p25	(3) p75	(4) sd
Census shock	-0.10	-0.23	0.029	0.33
Population	13,255	2,779	11,461	43,202
Unemployment rate (% of population)	2.38	1.43	2.86	1.43
Population density (per sq. km.)	285	85.4	327	362
Empl. growth before census adoption (log pts.)	1.55	-0.65	3.78	5.81

Notes: The table presents means, 25th and 75th percentiles, and standard deviations for the 4,949 municipalities in the main employment sample. The census shock is the relative difference in official population scaled by three sample standard deviations, as in (1). Employment growth is the log difference scaled by 100.

Employment data at the municipality level are not publicly available and the labor market research micro data of the Federal Employment Agency do not contain a municipality identifier. However, we were able to commission a new tabulation from the Federal Employment Agency containing the total number of individuals employed by establishments located in different municipalities for the period 2010 to 2018. The agency censors values for municipalities with fewer than three establishments, which in practice implies that we have data for all municipalities with at least 10,000 inhabitants in 2011, for 95% of municipalities with at least 3,000 to 10,000 inhabitants, and for 25% of municipalities with less than 3,000 inhabitants. Our baseline employment sample consists of the 4,949 municipalities, for which we consistently observe employment data in the years 2010 to 2018, as summarized in Table 2. We add several demographic control variables from the Federal Statistical Office.

3.3 Estimation Strategy

The empirical challenge in estimating the effects of government purchases on employment is that purchases are not exogenous to other determinants of employment. There may be reverse causality, for example, employment slumps may lead governments to increase purchases with the hope of stimulating employment. In addition, unobserved shocks to employment may coincide with government purchases. For instance, improvements in information technology in a region could increase local labor demand and employment, while simultaneously inducing the municipal government to purchase new technology equipment from local firms. Both reverse causality and unobserved shocks can thus lead to spurious comovement between government purchases and employment, even if there is no true causal link running from purchases to employment.

We use variation generated by the 2011 census to overcome such spurious comovement. Transfers from federal states to municipalities depend on municipal population,

so the census shock altered the funding of municipalities. We will show that the census shock mainly affected government purchases, and not other government budget positions, and will subsequently test the effects on employment and output per worker.

We conduct difference-in-differences regressions where we compare the growth of government purchases and employment in municipalities with a stronger census shock to other municipalities, both before and after the census was adopted in the state.

Our main specifications take the form:

$$\begin{aligned} &log(y_{it}) - log(y_{ib}) = \\ &\sum \beta_\tau \times \text{census shock}_i \times \mathbb{1}\left[t \in \tau\right] + \beta_c \times \text{controls}_{it} + \epsilon_{it}, \end{aligned} \tag{2}$$

where y_{it} is either employment or government purchases in the municipality. The outcome variable in (2) is the log difference between the value of y in year t and the value of y in a baseline year b, which we define as the final year before the census was adopted.[7] We interact the census shock with indicator variables measuring whether the observation year t is part of a time period τ, given by $\mathbb{1}\left[t \in \tau\right]$. In the richest specification, we include indicators for each individual year relative to when the state adopted the census. The coefficient β_τ captures whether municipalities with a higher census shock experienced a different average growth rate in y between year t and the baseline year b. One advantage of specification (2) is that it allows estimating effects at different time horizons in one specification.

All specifications additionally include a vector of control variables $controls_{it}$. We condition on state-by-year fixed effects in the municipality-level analyses, so the coefficients of interest are identified only using within-state differences in the census shock. We also control for fixed effects for 10 bins of population size in 2011 and a "boom/bust" indicator for municipalities with non-negative employment growth (boom) versus negative growth (bust) in the year before census adoption, both interacted with year fixed effects.[8] In richer specifications, we include an additional set of control variables interacted with year fixed effects and interacted with the boom/bust indicator: state fixed effects; size fixed effects; log total employment in 2011; log number of unemployed in 2011; population density in 2011; log employment growth in the year before census adoption; and log employment in industries directly selling to municipal governments

[7] See Table 1. The base year is 2014 in Baden-Württemberg and North Rhine-Westphalia, since the census determined only one-third of funds allocation in 2014 in those states (this choice does not materially affect any results). Specifications with municipality fixed effects on the right-hand side, instead of subtracting the base year value y_{ib} from the left-hand side, yield very similar estimation results.

[8] The statistical office used a different census methodology in municipalities below 10,000 inhabitants (Hager and Hilbig 2024). We include indicators for municipalities below 10,000 and a wide range of other size controls in our specifications to account for potential effects of this methodological difference, although they do not materially affect the estimates.

Table 3: The census shock and municipality characteristics

Outcome	(1) Census shock	(2) Census shock
Population (log)	-0.054	-0.058
	(0.055)	(0.10)
Employment (log)	-0.0062	0.014
	(0.010)	(0.0090)
Unemployed (log)	-0.00091	0.0072
	(0.017)	(0.040)
Population density (per 100 sq. km.)	0.0016	0.00036
	(0.0031)	(0.0016)
Employment in industries receiving municipal spending (log)	-0.00050	-0.0062
	(0.021)	(0.023)
Employment growth before census adoption (log)	-0.00060	-0.00027
	(0.00069)	(0.00067)
Observations	4,949	4,949
State FE	No	Yes
Size FE	No	Yes
R^2	0.048	0.078

Notes: The table presents a cross-sectional regression at the municipality level. The outcome is the census shock as adopted by the state in 2018 (i.e., it is zero for Rhineland-Palatine and equal to the value defined by (1) for the other states). The regressors are measured in 2011, apart from employment growth before census adoption, which is measured as growth in the baseline year before the state adopted the census for adopting states and as growth between 2013 and 2014 in Rhineland-Palatine. Column 2 contains state fixed effects and fixed effects for population bins in 2011 (0-10k, 10k-50k, 50k-100k, 100k-150k, 150k-200k, 200k-250k, 250k-300k, 300k-400k, 400k-1,000k, >1,000k). Standard errors are clustered by municipality. Statistical significance is denoted by *** p<0.01, ** p<0.05, * p<0.1.

in 2011, to keep constant pre-existing differences driven by the role of municipal governments.[9] We cluster standard errors by municipality.

We exclude Rhineland-Palatine from the main specifications, since it never adopted the census and there is no clear post-treatment period, but we use it for a placebo test in Section 4.4.

3.4 Support for the Identification Assumption

The empirical strategy identifies the effect of the census shock on regional outcomes as long as municipalities with a greater census shock would have evolved in parallel

[9]We identify "industries selling to municipal governments" by reading 1,000 municipal tenders on the public procurement website service.bund.de and classifying which industries would fulfill the tenders. We measure employment in these industries using data commissioned from the Federal Employment Agency. See also Section 4.3.

to other municipalities had the census not been adopted. We present several pieces of evidence in favor of this identification assumption. First, municipalities with a greater census shock did not have significantly different characteristics than other municipalities, so we find no association between the census shock and observables in Table 3. This conclusion is robust to a host of other unreported specifications, including those with only a single characteristic among the regressors on the right-hand side.

Second, municipalities with a greater census shock were exposed to similar shocks as other municipalities before the census adoption. We find that government purchases and total employment in municipalities with a greater census shock were on parallel trends compared to other municipalities before the census was adopted, as shown in Figures 3 and 4. These findings suggest that municipalities would have also continued to evolve in parallel after the census shock. It also suggests that municipalities did not anticipate the census shock, as purchases or employment did not adjust before the census was adopted.

Third, there is no evidence that the census shock had effects in Rhineland-Palatine, which never adopted the census, and in Saxony-Anhalt before it adopted the census in 2017, as we show in Section 4.4. There is therefore no generic association between census shock and municipality outcomes, but only an association if the census affected municipal budgets.

Fourth, we find similar results using simple cross-sectional specifications that do not use variation in the census adoption year of the state in Section 4.4. The cross-sectional regressions are robust to potential issues due to negative weights (Callaway and Sant'Anna 2021) and spurious serial correlation (Ramey 2021), implying that our findings are not driven by such issues. Moreover, the cross-sectional regressions imply that variation in the adoption year across states does not affect the findings, so any potentially endogenous choices of the adoption year do not drive the results.

Finally, to further assuage any concerns about the main specification using the adoption year, we show that the choice of adoption year was not influenced by unusual trends in municipalities before adoption. We find that municipal purchases only changed once the census was adopted for funds allocation and not when the census results were first announced in 2013. Several "late-adoption" states started applying the census only in 2014 or later, so at least a year after the census counts were announced. Municipal governments in the late-adoption states were aware of the population changes due to the census already in 2013. However, they were not aware of the implications for their budget in 2013, since political discussions in their states were still underway, eventually resulting in some states not adopting and other states adopting in different subsequent years. Municipalities acted conservatively and did not adjust their purchases between the census counts announcement in 2013 and the

census adoption year. The parallel pre-trends in Panel A of Figure A2, where we analyze only municipalities in "late-adoption" states, support this view. Similarly, municipalities with a greater census shock in the late-adoption states did not experience different employment trends between 2013 and the adoption year, as shown in Panel B of Figure A2. This finding implies that the adoption year was not driven by unusual economic shocks after the census results announcement in 2013.

3.5 The Census Shock and Government Purchases

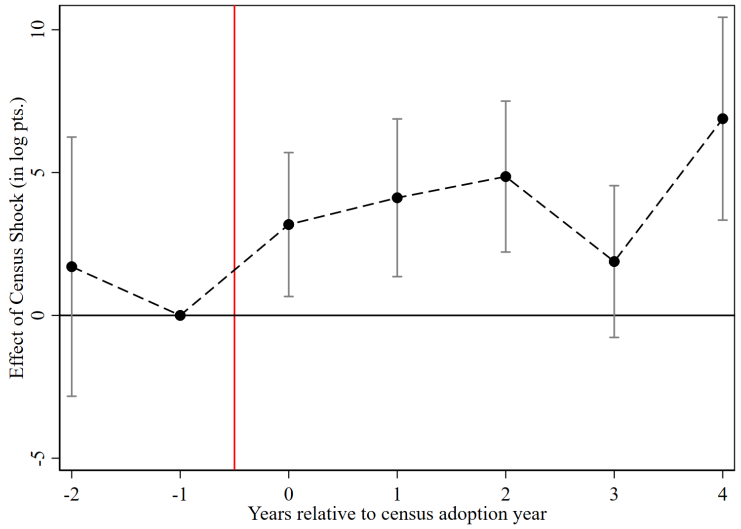
Our empirical strategy requires that the census shock impacted municipal government purchases. A large literature already presents evidence for a "flypaper effect," according to which governments mostly raise expenditures in the initial years after a funding shock, rather than adjusting saving, debt, or taxes (Hines and Thaler 1995). We confirm this effect for German municipalities and show that only government purchases responded significantly to the census-induced funding shock.

We analyze the largest available dataset on municipal budgets provided by the Bertelsmann Foundation, containing data on municipalities with at least 5,000 inhabitants and available in a consistent format starting in 2012 (Bertelsmann Stiftung 2022). Since the census was adopted at the earliest in 2013, the data still allow us to examine the effect of the census shock on budget positions. Many states adopted a wide-ranging municipal accounting reform around 2012, with municipalities in the same state changing the classification of their budget positions at different points in time in the years around 2012 (Schwarting 2016). To ensure that the data are comparable, we only include states in the sample for the budget analysis once they have fully completed the implementation of the accounting reform in all their municipalities.

We begin by studying how the census shock affected government purchases by municipalities. Purchases include expenditures on intermediate goods and services (related to public education, cultural events, sports clubs, catering, office furniture, etc.), maintenance of government-owned assets (cleaning, landscaping, equipment repair, etc.), rents (for buildings, etc.), and other types of administrative expenses, but do not include transfers to households and personnel costs. We estimate a specification based on (2). The outcome is log government purchases relative to the year before census adoption and the main regressors are the census shock interacted with fixed effects for the years that have passed since census adoption. We plot the main coefficients and the corresponding 90% confidence intervals in Figure 3. The first year, during which the census was adopted and determined funds allocation, is given by year 0 in the figure.

There was no significant association between the census shock and the growth of

Figure 3: Effect on government purchases



Notes: The figure plots coefficients and 90% confidence intervals based on (2). The outcome is the difference between log purchases in a given year and log purchases in the base year before the census was adopted, scaled by 100. The main regressors measure the census shock interacted with fixed effects for years relative to the year of census adoption (see Table 1). The base year is 2014 in Baden-Württemberg and North Rhine-Westphalia, since the census determined only one-third of funds allocation in 2014 in those states (this choice does not materially affect any results). The sample does not include Rhineland-Palatine, which never adopted the census. Coefficients reflect a three standard deviation increase in the census shock. The following controls are interacted with year fixed effects: state fixed effects, fixed effects for population bins in 2011 (0-10k, 10k-50k, 50k-100k, 100k-150k, 150k-200k, 200k-250k, 250k-300k, 300k-400k, 400k-1,000k, >1,000k), a "boom/bust" indicator for municipalities with non-negative employment growth (boom) versus negative growth (bust) in the year before census adoption, log 2011 employment in industries directly selling to municipal governments, log total employment in 2011, log number of unemployed in 2011, population density in 2011, and log employment growth in the year before before census adoption. Standard errors are clustered by municipality.

government purchases in the year before census adoption, as evidenced by the statistically insignificant coefficient in year -2. In the first year after census adoption (i.e., year 0 in the figure), government purchases increased significantly in municipalities with a greater census shock. They remained elevated for the remainder of the sample period. This pattern suggests that the census shock persistently increased government purchases once the census was adopted, consistent with the fact that the 2011 census determined the allocation of funds throughout the entire sample period.

We examine the effect on government purchases in more detail in Table 4. The main regressor is the census shock interacted with a post variable capturing the extent of census adoption in the state. The post variable equals 0 for all municipalities before 2013 and 1 starting with the year of full census adoption in the state, as listed in Table 1. Baden-Württemberg and North Rhine-Westphalia had two phase-in years, with the new census determining 1/3 of funds allocation in 2014 and 2/3 in 2015, so the variable

equals 1/3 in 2014 and 2/3 in 2015 in these states. The adoption variable is always 0 in Rhineland-Palatine, which never adopted the census.

Table 4: Effect on municipal purchases and funding

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Purchases		Funding		Purchases rel. to base	Funding purchases
Census shock * post	4.20***	4.00***	47.5**	46.1**	4.71***	5.52***
	(1.36)	(1.36)	(22.1)	(22.7)	(1.57)	(0.88)
Observations	6,226	6,226	6,226	6,226	6,226	6,226
Base controls	Yes	Yes	Yes	Yes	Yes	Yes
All controls	No	Yes	No	Yes	Yes	Yes
R^2	0.32	0.33	0.014	0.026	0.21	0.23
Outcome mean	3.25	3.25	-0.34	-0.34	3.73	0.58

Notes: The table reports regressions based on (2). The outcome in columns 1 and 2 is the difference between log government purchases in a given year and log purchases in the base year before the census was adopted, scaled by 100. The outcome in columns 3 and 4 is the difference between log state funding received in a given year and log funding in the base year before the census was adopted, scaled by 100. The outcome in column 5 is the difference between government purchases in a given year and purchases in the base year before the census was adopted, both divided by purchases in the base year, and then scaled by 100. The outcome in column 6 is the difference between funding in a given year and funding in the base year before the census was adopted, both divided by purchases in the base year, and then scaled by 100. The reported outcome mean is the average of the outcome in the year of census adoption (i.e., log growth in year 0). The main regressor is the census shock interacted with a post variable capturing the extent of census adoption in the state (see Table 1). Coefficients reflect a three standard deviation increase in the census shock. The sample does not include Rhineland-Palatine, which never adopted the census. The base controls include the following variables interacted with year fixed effects: state fixed effects, size fixed effects for population bins in 2011 (0-10k, 10k-50k, 50k-100k, 100k-150k, 150k-200k, 200k-250k, 250k-300k, 300k-400k, 400k-1,000k, >1,000k), a "boom/bust" indicator for municipalities with non-negative employment growth (boom) versus negative growth (bust) in the year before census adoption. The set of all controls includes the following variables interacted with year fixed effects: log total employment in 2011, log number of unemployed in 2011, log 2011 employment in industries directly selling to municipal governments, log employment growth in the year before the census was adopted, and population density in 2011. In addition, the set of all controls includes the following variables interacted with the "boom/bust" indicator: state fixed effects, size fixed effects, log total employment in 2011, log number of unemployed in 2011, log 2011 employment in industries directly selling to municipal governments, log employment growth in the year before the census was adopted, and population density in 2011. Standard errors are clustered by municipality. Statistical significance is denoted by *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

The coefficient in column 1 of Table 4 implies that a unit increase in the census shock—equivalent to a 3 standard deviation population increase due to the census—raised government purchases by 4 log points after the adoption of the census, relative to the year before census adoption. The coefficient is statistically significant at the 1% level. It remains similar in column 2 when we add the full set of controls, all interacted with year fixed effects. Columns 3 and 4 show why municipalities were able to increase

Table 5: Effect on other government budget positions

Outcome	(1) Personnel spending	(2) Other LR spending	(3) Debt	(4) Revenue pr. tax A	(5) Revenue pr. tax B	(6) Revenue bus. tax
Census shock * post	0.59	-4.92	8.43	-0.86	-2.65	-6.81
	(1.92)	(9.30)	(11.2)	(3.78)	(1.70)	(4.53)
Observations	6,226	6,226	6,226	6,226	6,226	6,226
Base controls	Yes	Yes	Yes	Yes	Yes	Yes
All controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.13	0.10	0.10	0.057	0.47	0.11

Notes: The table reports regressions based on (2). The outcomes measure the difference between a log budget position in a given year and the log position in the base year before the census was adopted, scaled by 100. The positions are personnel expenditures, other long-run expenditures (e.g., social programs for youth or long-run capital spending), outstanding debt, tax revenue raised through property tax A, tax revenue raised through property tax B, and tax revenue raised through the business tax. The main regressor, controls, sample, and standard error clustering are explained in Table 4.

purchases: the census shock increased the allocation of state funding by 47 log points, relative to the year before census adoption.

We compare the magnitude of the purchases and funding effects in columns 5 and 6. The outcomes measure the absolute change in purchases and funding, respectively, both divided by purchases in the base year. Using the same normalization for the outcomes makes the magnitude of the coefficients directly comparable. The coefficients show that both purchases and funding increased by roughly 5 percentage points of base year purchases. This finding implies that governments used nearly all of the additional funding for government purchases.

We find no evidence that municipalities adjusted other budget positions, apart from purchases, in Table 5. Governments did not spend more on personnel compensation (column 1) or other long-run expenditures (e.g., social programs for youth or long-run capital spending, column 2). They also did not pay down debt (column 3) or increase revenue from the two types of property tax and the business tax that they control (columns 4 to 6).

A likely reason for the strong response of government purchases, relative to other budget positions, is that municipal politicians can organize purchases with less advance preparation and more at their own discretion, relative to other positions. For example, local business tax rates are usually adjusted once a year and require a vote by the full municipal council (e.g., Fuest et al. 2018). Long-run capital spending often requires coordination with other municipalities in the same county or state and typically takes

at least a year to be initiated (e.g., Buchheim and Watzinger 2023). Moreover, many municipalities had received large long-run capital spending grants from the federal government in the aftermath of the Great Recession from 2009 to 2011 period, so their demand for capital spending was relatively satisfied in the period after the census.

The changes in municipal funding and purchases due to the census did not directly impact taxes, either at the level of affected municipalities or in the aggregate. The census simply led to a redistribution of existing tax revenue from municipalities with a lower census shock to those with a higher census shock. The census shock also did not impact aggregate government purchases in the German economy. We will therefore interpret the effects of the census shock as the effects of a windfall-financed shock to government purchases.

Taken together, the findings on government budget positions suggest that there was a "flypaper effect" in the first 5 years after the census shock because the persistent increases in government funding were almost entirely used to raise government purchases.[10] For the purposes of this paper, the political economy aspects of why governments used the census shock for purchases are not essential. Instead, we take the finding of increased purchases as a starting point for the main analysis of this paper, in which we estimate how census-induced purchases affected employment.

4 Effects on Employment and Output per Worker

In this section, we present the main analysis of how the shock to government purchases affected employment and output per worker. On average, the shock raised growth in the first year, an effect driven by "bust" municipalities with shrinking employment or high unemployment rates before the shock. However, the average effect turned negative in the third year after the shock, an effect driven by "boom" municipalities with positive pre-growth and low unemployment.

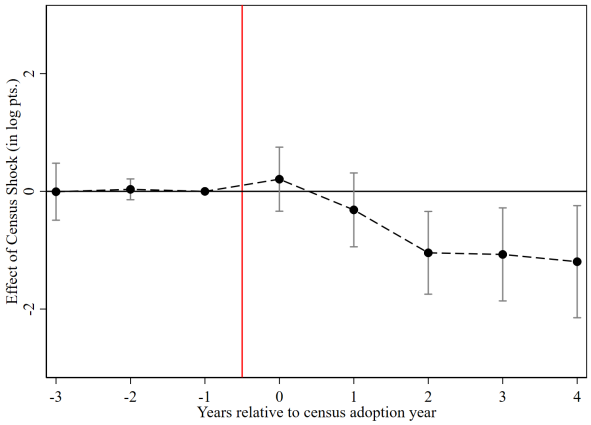
4.1 Overview of the Employment Effects

We graphically analyze employment in establishments located in the municipality using a specification based on (2). The outcome is the difference in log employment between

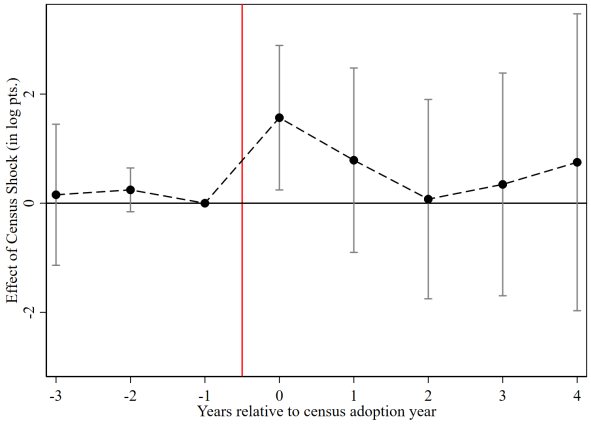
[10]The findings are consistent with evidence from Germany by Baskaran (2016), Hager and Hilbig (2024), and Helm and Stuhler (2024) who document strong spending and weak tax responses in the first 5 years after municipal funding shocks. There is evidence that municipalities also raised long-run capital spending after the 1987 census, whereas they focused on purchases in 2011. A potential explanation is that many municipalities had received large long-run expenditure grants from the federal government following the Great Recession from 2009 to 2011, so their demand was relatively more satisfied in the period after the 2011 census than during the early 1990s where Germany underwent reunification and a recession.

Figure 4: Effect on employment

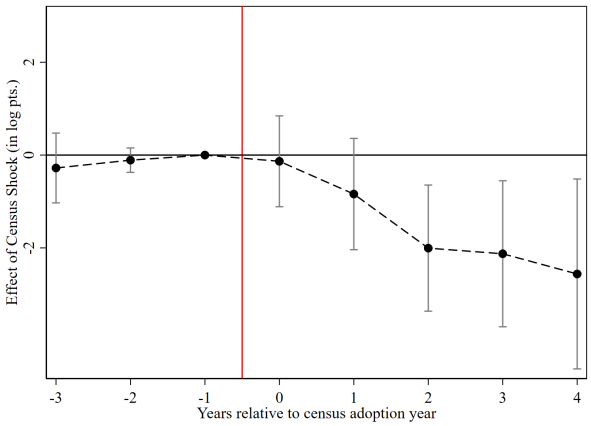
A: All municipalities



B: Bust municipalities with negative pre-growth



C: Boom municipalities with positive pre-growth



Notes: The figure plots coefficients and 90% confidence intervals based on (2). The outcome is the difference between log employment in a given year and log employment in the base year before the census was adopted, scaled by 100. The sample in Panel A includes the full employment sample, except Rhineland-Palatinate, which never adopted the census. The sample in Panel B is further restricted to only "bust" municipalities with employment growth below zero in the year before census adoption and the sample in Panel C to only "boom" municipalities with employment pre-growth above zero. The main regressors, controls, sample, and standard error clustering are as described in Figure 3. See Figure A3 for figures with only a basic set of controls.

a given year and the year before census adoption. We plot the coefficients on the census shock interacted with fixed effects measuring the years that have passed since census adoption in Figure 4. We include a large set of controls in Figure 4, but find similar patterns when controlling for only the interaction of year-by-state and year-by-size fixed effects in Figure A3.

Panel A shows the effects over time for the full sample of municipalities that adopted the census at some point. Coefficients for the years before adoption are small and insignificant, suggesting that municipalities with a greater census shock were on a parallel employment trend before census adoption, in line with the identification assumption. In the first year after adoption, there was a slight uptick in employment in municipalities with a greater census shock, relative to other municipalities, although the average coefficient is insignificant. In the subsequent year, employment in municipalities with a greater census shock was already lower and it settled on a significantly lower level between the third and fifth year after census adoption, relative to other municipalities.

4.2 Greater Employment Growth in Bust Municipalities

We investigate which municipalities drive the initially positive and subsequently negative effects by splitting the sample according to the growth of employment before census adoption. Traditional Keynesian theory suggests that government purchases have larger short-run stimulus effects in recessions than in booms (e.g., see discussions in Auerbach and Gorodnichenko 2012; Ramey and Zubairy 2018), suggesting that municipalities with negative growth experience larger short-run gains. We therefore analyze a subsample of "bust" municipalities, in which employment growth in the year before census adoption was negative, in Panel B of Figure 4.

The effect of the census shock in the bust municipalities was statistically significant and positive in the first year after the shock. This finding is consistent with the Keynesian notion that purchases have the strongest stimulus effects in the short run (e.g., because prices and wages are sticky in the short run) and may become slightly weaker in the medium run. Indeed, the coefficients between the third and fifth year after the census shock are smaller and statistically insignificant, although they remain positive throughout, which is consistent with weakly positive medium-run effects.

We compare the magnitude of the short-run effect in bust regions to other cross-region studies. On average over the first two years, we find that employment rose by 1.5 log points in bust municipalities (see column 2 of Panel B of Table 6), while purchases rose by 4 log points (see column 2 of Table 4), implying a short-run elasticity of employment to purchases of 0.37. We can convert this elasticity into the effect of a USD 100k increase on the number of jobs by dividing the elasticity by the average

purchases-to-employment ratio (and applying the 2011 USD-EUR exchange rate of 0.73). The resulting estimate implies that a 100k USD increase in annual purchases generated roughly 6 jobs per year in bust municipalities in the first two years.

In comparison, Adelino et al. (2017) find an effect of 8 jobs per USD 100k of local government spending in US regions with high unemployment and no significant effect in regions with low unemployment; Buchheim and Watzinger (2023) report 5 jobs per USD 100k during and after the 2009 recession in Germany; and Suárez Serrato and Wingender (2016) find an effect of 3 jobs per USD 100k on average, with substantially stronger effects in US regions with low employment growth. Chodorow-Reich (2019) summarizes the literature, with the bulk of papers analyzing shocks that primarily affected transfers to households, rather than purchases from firms as in our paper. Despite differences in the type of spending and horizon of the shock, the estimated short-run effects are broadly comparable in magnitude. The average effect of USD 100k of spending ranges between 0 and 3 jobs and the effect in regions with low growth or high unemployment often exceeds 5 jobs.

Since positive short-run effects in bust regions are relatively standard in the literature, we focus the remaining discussion on the effects in boom municipalities.

4.3 Lower Employment Growth in Boom Municipalities

We turn to boom municipalities, in which employment growth before the census adoption was positive, in Panel C of Figure 4. The coefficients are close to zero and insignificant in the first two years after the adoption of the census. The results suggest that additional government purchases had at most a weak stimulus effect on booming municipalities in the short run. We are not aware of other studies analyzing permanent shocks to mainly government purchases in booming regions, but the finding of short-run effects close to zero is consistent with several papers analyzing broader shocks to regional government spending in strong regions (e.g., Adelino et al. 2017; Berge et al. 2021; Nakamura and Steinsson 2014; Shoag 2016). Conceptually, if firms are already growing fast and if there are few slack resources, there seems to be little room for government purchases to achieve additional increases in short-run employment.

Starting in the third year after census adoption, the coefficients in Panel C of Figure 4 turn significantly negative. This implies that employment in boom municipalities with a greater census shock was lower, relative to other municipalities. The negative effects found in the full sample of Panel A are thus driven by the boom municipalities.

The magnitude of the negative coefficients in Panel C is smaller than the median employment pre-growth in boom municipalities, which was around 4 log points. The coefficients represent the effect of a relatively large (3 standard deviation) increase

in the census shock. Hence, the results do not imply that greater government purchases, induced by the census shock, lead to negative absolute employment growth and terminations of existing employment in fast-growing municipalities. Instead, the results suggest that greater government purchases can slow down employment growth in otherwise fast-growing municipalities, relative to other fast-growing municipalities that did not experience increased government purchases. This distinction is relevant because the underlying mechanism leading to the negative effects does not require firms to actively reduce employment. Instead, the mechanism solely requires that firms become more passive in response to greater government purchases and do not grow employment as fast as they would otherwise have.

We investigate the employment effects in more detail in Table 6. We separately analyze boom and bust municipalities by interacting the census shock with an indicator for boom municipalities (those with non-negative pre-growth). The variation in the census shock across boom and bust municipalities was similarly dispersed, as shown in Figure A1. We differentiate between short-run effects and subsequent effects by interacting the census shock measures with indicators for observations within the first two years after census adoption and for observations greater than two years after census adoption.

Panel A of Table 6 presents the effects on employment in boom municipalities. The coefficient in column 5 shows that the census shock reduced employment by 1.1 log points in the period greater than two years after census adoption. The point estimate is not sensitive to the inclusion of the controls. The estimates are significant at the 1% or 5% level, depending on the included controls. We find no significant effect in boom municipalities in the first two years after the adoption, consistent with the graphical evidence.

The negative employment effects could be driven by firms that are direct suppliers to municipal governments and adjust their behavior when sales to the government increase. Alternatively, the negative effects could be driven by indirect effects on other firms, such as changes in local factor prices or other general equilibrium spillover effects (as in Huber 2023) triggered by greater government purchases. To differentiate between these two potential mechanisms, we analyze whether the negative effects were larger in industries that receive purchase orders from municipal governments. We identify such "industries selling to municipal governments" by manually reading 1,000 municipal tenders on the public procurement website service.bund.de and classifying which industries would fulfill the tenders.[11] We again rely on data commissioned

[11]The full list of industries selling to governments and their German WZ classification is: 35 Energy supply; 36 Water supply; 37 Sewage disposal; 38 Collection, treatment and disposal of waste; 39 Pollution removal and other disposal services; 45 Sale, maintenance, and repair of motor vehicles and motorcycles; 47 Retail, except of motor vehicles and motorcycles; 55 Provision of lodgings; 56

Table 6: Effect on total employment in boom versus bust municipalities

	(1)	(2)	(3)	(4)	(5)
Outcome	Total employment				
Panel A: Boom municipalities with positive pre-growth					
Census shock *	-0.26	-0.35	-0.42	-0.43	-0.40
first 2 years after adoption	(0.27)	(0.29)	(0.28)	(0.28)	(0.28)
Census shock *	-1.00**	-1.06**	-1.22***	-1.23***	-1.10**
over 2 years after adoption	(0.45)	(0.46)	(0.44)	(0.44)	(0.43)
Panel B: Bust municipalities with negative pre-growth					
Census shock *	1.57**	1.18	1.16	1.22	1.49*
first 2 years after adoption	(0.74)	(0.82)	(0.81)	(0.81)	(0.83)
Census shock *	1.39	0.022	-0.14	-0.071	-0.30
over 2 years after adoption	(1.25)	(1.37)	(1.38)	(1.38)	(1.40)
Observations	41,562	41,562	41,562	41,562	41,562
State*year	Yes	Yes	Yes	Yes	Yes
Size*year	No	Yes	Yes	Yes	Yes
Pre-growth*year	No	Yes	Yes	Yes	Yes
Labor market measures*year	No	No	Yes	Yes	Yes
Pop. density*year	No	No	No	Yes	Yes
All controls	No	No	No	No	Yes
R^2	0.32	0.36	0.42	0.42	0.42

Notes: The table reports regressions based on (2). The main regressors measure the census shock interacted with indicators for observations within the first two years after census adoption and for observations greater than two years after census adoption. The main regressors are additionally interacted with an indicator for municipalities with non-negative growth (boom) versus negative growth (bust) in the year before census adoption. The estimated coefficients for the interactions with boom municipalities are in Panel A and for bust municipalities in Panel B. The outcome is the difference between log total employment in a given year and log total employment in the base year before the census was adopted, scaled by 100. The controls and standard errors are explained in Table 4.

from the Federal Employment Agency to separately measure employment in industries directly selling to municipal governments and other industries.

The outcome in columns 1 and 2 of Panel A of Table 7 is the difference in log employment in industries selling directly to municipal governments, relative to the year before census adoption. We find a negative and statistically significant effect on employment in these industries in boom municipalities in the period greater than two years after census adoption. In comparison, we find small and insignificant coefficients in boom municipalities in industries not directly selling to municipal governments in columns 3 and 4 of Panel A. These results suggest that, to a large extent, the responses of firms directly experiencing an increase in government demand were responsible for the negative employment effects in boom municipalities.

The estimates for bust municipalities in Panel B of Table 7 are less precisely estimated and do not allow a clear conclusion on the importance of direct versus indirect effects. The coefficients are positive but insignificant for both industry types in the first two years, consistent with direct and indirect effects going in the same direction (as in Huber 2018). The negative coefficient for the period greater than two years in column 2 of Panel B may indicate that dynamism drain also plays a role in bust municipalities in the medium run, consistent with the Keynesian notion that government purchases do not have long-run stimulus effects due to the disappearance of temporary frictions. However, the coefficient is only significant conditional on all the controls and not in several other unreported specifications, so we do not emphasize it.[12]

We find that the effects do not just vary between boom and bust municipalities, but that medium-run employment growth was increasingly lower for boom municipalities with increasingly greater pre-growth. In Figure 5, we show that the point estimate for the period greater than two years after census adoption is around -0.7 for municipalities with pre-growth between 0 and 2%, -1.2 for those with pre-growth between 2 and 4%, and -3.1 for those with pre-growth above 4%. This pattern supports the view that variation in the strength of the local economy determines the magnitude of the employment effects. The results again suggest that employment growth did not turn negative in absolute terms as a result of the census shock, since the pre-growth level in each bin is greater than the corresponding coefficient, but that employment growth became less positive as a result of the census shock.

Food services; 62 Information technology services; 63 Information services; 71 Architectural and engineering, technical testing and analysis (physical and chemical); 81 Building management, landscape gardening and landscaping; 84 Public administration and defense, social security; 85 Education; 86 Health services; 87 Residential care; 88 Social work; 90 Creative, arts and entertainment activities; 91 Libraries, archives, museums, botanical and zoological gardens; 93 Sports, entertainment and recreation services. We separately analyze construction industries in column 5 of Table A2.

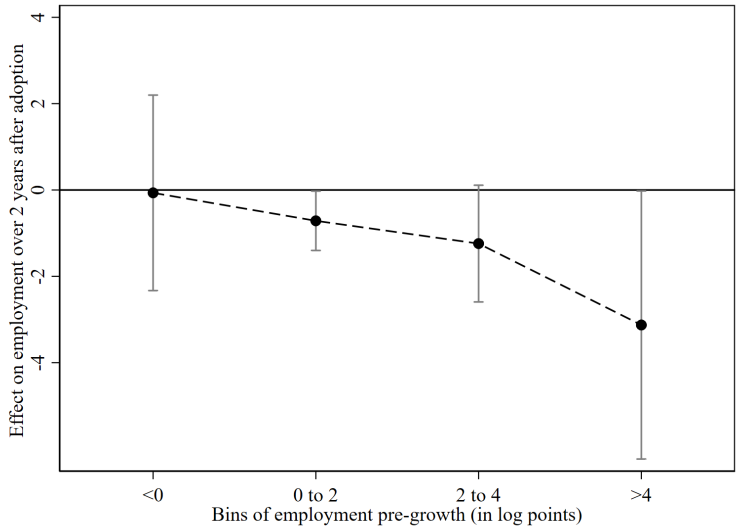
[12]We find no evidence that employment in construction industries changed significantly in column 5 of Table A2, in line with the fact that long-run capital spending did not rise, as shown in Table 5.

Table 7: Effect on employment in industries selling to municipal government

Outcome	(1)	(2)	(3)	(4)
	Employment in industries selling to mun. govt.		Employment in industries not selling to mun. govt.	
Panel A: Boom municipalities with positive pre-growth				
Census shock *	-0.63	-0.87	0.63	0.55
first 2 years after adoption	(0.45)	(0.53)	(0.80)	(0.84)
Census shock *	-1.31*	-1.43**	0.30	0.25
over 2 years after adoption	(0.67)	(0.66)	(0.95)	(1.01)
Panel B: Bust municipalities with negative pre-growth				
Census shock *	0.43	0.24	1.27	1.81
first 2 years after adoption	(1.40)	(1.46)	(1.30)	(1.34)
Census shock * over 2 years after adoption	-2.25	-3.34*	1.97	1.84
over 2 years after adoption	(1.87)	(1.89)	(2.64)	(2.71)
Observations	41,562	41,562	41,562	41,562
Base controls	Yes	Yes	Yes	Yes
All controls	No	Yes	No	Yes
R^2	0.23	0.27	0.10	0.15

Notes: The table reports regressions based on (2). The main regressors measure the census shock interacted with indicators for observations within the first two years after census adoption and for observations greater than two years after census adoption. The main regressors are additionally interacted with an indicator for municipalities with non-negative growth (boom) versus negative growth (bust) in the year before census adoption. The estimated coefficients for the interactions with boom municipalities are in Panel A and for bust municipalities in Panel B. The outcome in columns 1 and 2 is log employment in industries directly selling to municipal governments (see text for the classification). The outcome in columns 3 and 4 is log employment in industries not directly selling to municipal governments, scaled by 100. The controls and standard errors are explained in Table 4.

Figure 5: Effect on employment for the period greater than 2 years after adoption, by bins of pre-growth



Notes: The figure plots coefficients and 90% confidence intervals for the effect of the census shock during the period greater than 2 years after census adoption, estimated separately for 4 groups of municipalities with different employment pre-growth. In contrast to Table 6, (census shock * over 2 years after adoption) is not interacted with an indicator for municipalities with non-negative versus negative employment pre-growth, but with 4 indicators for bins of employment pre-growth. The bins are for municipalities with: pre-growth < 0; $0 \leq$ pre-growth < 2; $2 \leq$ pre-growth < 4; and $4 \leq$ pre-growth. The outcome is the difference between log employment in a given year and log employment in the base year before the census was adopted, scaled by 100. The controls and standard error clustering are described in Figure 3.

We also analyze heterogeneity with respect to the unemployment rate, rather than pre-growth. We categorize municipalities as having low unemployment if the unemployed-to-population ratio in the year before census adoption was below the median of 2%. In these municipalities, we find that the census shock reduced employment significantly in the period greater than two years after the adoption of the census, as shown in columns 1 and 2 of Panel A of Table A1. The slower growth in low unemployment municipalities is again driven by industries selling directly to municipal governments, as evidenced by columns 3 and 4 of Panel A. The findings based on the unemployment rate further strengthen the view that government purchases can slow down employment growth in economically strong municipalities.

4.4 Specifications Independent of Adoption Timing and Placebo Tests

We conduct additional analyses using cross-sectional regressions that exploit only variation in the census shock across municipalities and not variation in the adoption timing. The outcome variable is the log employment change between 2012 and 2016. The

Table 8: Cross-sectional employment regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Main result: excl. RP and ST			Placebos: only RP and ST		
Census shock	-1.19**	-1.36**	-0.49	-0.22	-0.012	0.68
	(0.49)	(0.56)	(1.18)	(1.40)	(1.41)	(4.95)
Sample	All	Boom	Bust	All	Boom	Bust
Observations	4,702	3,307	1,395	524	342	182
Base controls	Yes	Yes	Yes	Yes	Yes	Yes
All controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.24	0.19	0.11	0.38	0.43	0.31

Notes: The table reports regressions based on (3). The regressor measures the census shock. The outcome is the difference between log total employment in 2016 and 2012, scaled by 100. The samples in columns 1 to 3 contain all states except Rhineland-Palatinate and Saxony-Anhalt. The samples in columns 4 to 6 are placebo tests, so they contain only Rhineland-Palatinate (which never adopted the census) and Saxony-Anhalt (which adopted only in 2017). Boom municipalities are those with non-negative employment growth in the year before the census was adopted, if the census was adopted before 2016, and from 2011 to 2012 otherwise. The controls and standard errors are explained in Table 4. Since the specifications are cross-sectional, the controls are not interacted with year fixed effects.

treatment variable is the municipality census shock:

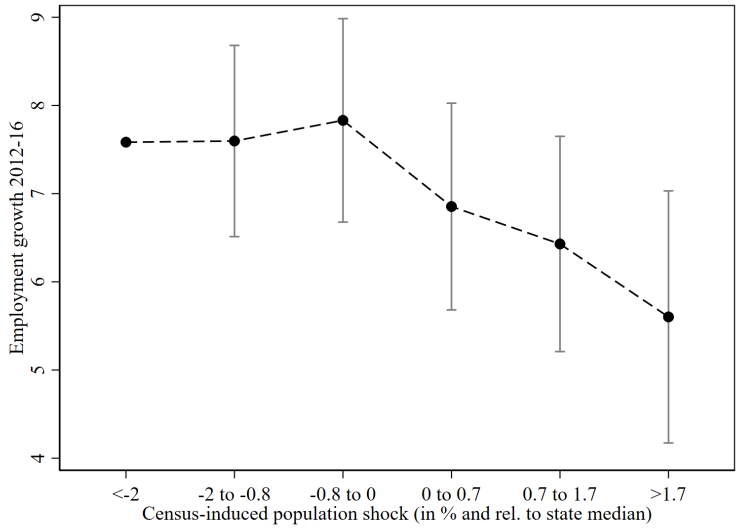
$$log(y_{i2016}) - log(y_{i2012}) = \mu \times \text{census shock}_i + \mu_c \times \text{controls}_i + \upsilon_i. \quad (3)$$

These specifications are immune to concerns about negative weights (Callaway and Sant'Anna 2021) and spurious serial correlation (Ramey 2021).

We first analyze all states that introduced the census in this period, which means all states except Rhineland-Palatinate and Saxony-Anhalt. We find negative, significant coefficients in the full sample in column 1 of Table 8 and in the sample of boom municipalities in column 2. We find a small, insignificant coefficient in the sample of bust municipalities in column 3.

The cross-sectional specifications allow us to conduct a placebo exercise using Rhineland-Palatinate and Saxony-Anhalt. The census was historically never used for fund allocations in Rhineland-Palatine and its adoption was delayed to 2017 in Saxony-Anhalt for political reasons. Accordingly, we find no significant association between employment growth and the census shock in columns 4 to 6. This finding suggests that there is no generic relation between census shock and employment growth, but only a relation where the census shock was adopted and affected municipal budgets.

Figure 6: Employment growth by bins of the census shock in boom municipalities



Notes: The figure plots coefficients and 90% confidence intervals for different bins of the census shock during the period greater than two years after census adoption. The sample contains only municipalities with non-negative employment pre-growth. In comparison to Table 6, the linear measure of the census shock is replaced by indicators for 6 quantile bins of a variable measuring the difference between the census-induced population shock in the municipality and the average in the state. The outcome is the difference between log employment in a given year and log employment in the base year before the census was adopted, scaled by 100. The controls and standard error clustering are described in Figure 3.

4.5 Stronger Effects for More Positive Shocks to Purchases

We now analyze the effects in boom municipalities using a less parametric specification that allows for potentially non-linear effects in the census shock. This analysis reveals that the negative effects are driven by municipalities that received positive shocks to their funding as a result of the census, whereas the effects of negative shocks are weaker.

The census led to a redistribution of funding across municipalities, with some gaining and others losing. Municipalities where the census-induced official population change was larger than the median in the state were likely to receive an increase in funding and therefore to purchase more than they would have in the absence of the census shock. In contrast, municipalities where the census-induced change was below the state median were likely to face funding withdrawals and thus to reduce purchases.

To identify whether positive or negative changes in purchases had larger impacts, we measure the census-induced population change in a municipality and subtract the median in the state. We then construct six quantile bins, three for values below zero—indicating an average reduction in funding received by the municipality—and three for values above zero—indicating an average increase in funding. We run a cross-

sectional specification based on column 2 of Table 8, with only boom municipalities in the sample. The key modification to the specification is that we replace the census shock variable with indicators for the quantile bins. We thereby estimate the average employment growth for different bins of the census-induced population shock relative to the state median, allowing us to estimate potentially non-linear effects of the census shock. The omitted category is the most negative quantile bin. On the graph, we add the unconditional mean of the 2012-16 employment growth to the coefficients.

We plot the coefficients on the quantile bins in Figure 6. The estimates for the negative bins are relatively close to each other, suggesting that going from a small negative change to a big negative change has little impact on employment growth. Intuitively, a greater reduction in government purchases did not lead firms to increase employment by more. In contrast, the estimates for the positive bins display a downward slope. Intuitively, a greater increase in government purchases led firms to slow down employment growth by more. We find a similar pattern when we analyze only municipalities with low unemployment rate in Figure A4.

The negative slope in the sample with increasing government purchases is consistent with firms becoming more passive in their hiring when government purchases increase, leading to lower employment growth. The flat slope in the sample with decreasing government purchases suggests that the reverse is not true: firms did not increase employment by more when government purchases fell by more. One potential reason is that a greater slope in the municipalities with decreasing purchases would have required firms to proactively raise hiring when government purchases fell. It is intuitive that firms did not choose a relatively aggressive expansion policy in response to a strong withdrawal of their government contracts, but instead preferred a more conservative approach of maintaining their previous hiring strategy. A second potential reason for the flat slope in the sample with decreasing purchases is that our analysis in Figures 6 and A4 focuses on boom municipalities where the labor market is relatively tight. It is difficult for firms to attract new workers in such municipalities, so that employment growth cannot easily respond to greater labor demand. We will discuss the mechanisms underlying these empirical results in more detail in Section 5, especially taking up the idea that greater government purchases make firms more passive.

4.6 Robustness Tests for the Employment Results

Some of our results use a sample split by pre-growth. To further guard against potential serial correlation, we show that the main findings are not due to differences in pre-existing trends by controlling for employment growth from 2010 to 2011 in column 2 of Table A2, in addition to our baseline controls using pre-growth in the year before

census adoption.

We find similar results using log population in 2011 as regression weight in column 3 of Table A2. There is no evidence for a different effect on large municipalities (with at least 10,000 inhabitants in 2011) in column 4.

4.7 Effects on Output per Worker

We turn to analyzing how the census shock affected output per worker. The response of output per worker reveals to what extent other factors of production or TFP changed after the census shock. For instance, if firms with a greater census shock increased their investment in capital or new technologies more slowly, output per worker would grow more slowly. In contrast, if the census shock led firms to replace workers with capital or new technologies, output per worker would grow faster.

Data on output per worker are only available at the county level. We therefore sum the municipality-level data to calculate a county-level census shock, which is the relative difference in the county's population due to the census (analogous to the municipality-level measure in (1)). We use an analogous specification to the employment analysis in Table 6: we regress the county-level difference in log output per worker between a given year and the year before census adoption on the census shock, interacted with indicators for the first two years after census adoption and for the subsequent years. The control variables are the county-level versions of those in Table 6, except that we replace fixed effects for state with fixed effects for three broader regions (due to the smaller number of counties) and that we additionally control for GDP growth in the year before census adoption, both interacted with year fixed effects.

The main country-level results are in Table 9. In boom counties, a greater census shock had little effect on output per worker in the first two years after census adoption but reduced output per worker by 2.3 log points in the subsequent years. The effects are relatively similar with and without controls. The coefficients are significant at the 1% and 5% level. In bust counties, output per worker was higher in the first two years, although the effect is imprecisely estimated, and similar again to the pre-census adoption level in the subsequent years.

In Table 9, we define boom counties as those with non-negative GDP growth in the year before census adoption, but we find similar effects when using output per worker pre-growth in column 1 of Table A3 and employment pre-growth in column 2 of Table A3. In line with the municipality-level results, we find that county employment also fell in boom counties in column 3 of Table A3.

The dynamics of output per worker in boom counties resemble those for employment, with substantial growth reductions in the period over two years after census

Table 9: Effect on output per worker (county-level)

	(1)	(2)	(3)	(4)	(5)
Outcome	Output per worker				
Panel A: Boom municipalities with positive pre-growth					
Census shock *	0.49	0.18	-0.51	-0.47	-0.55
first 2 years after adoption	(0.56)	(0.57)	(0.59)	(0.59)	(0.60)
Census shock *	-1.42*	-1.64**	-2.19***	-2.23***	-2.29***
over 2 years after adoption	(0.73)	(0.77)	(0.82)	(0.82)	(0.84)
Panel B: Bust municipalities with negative pre-growth					
Census shock *	0.22	0.24	1.05	1.02	0.97
first 2 years after adoption	(0.68)	(0.74)	(0.73)	(0.74)	(0.81)
Census shock *	-1.84	-1.77	-0.20	-0.25	-0.31
over 2 years after adoption	(1.40)	(1.44)	(1.23)	(1.23)	(1.22)
Observations	3,204	3,204	3,204	3,204	3,204
Region*year	Yes	Yes	Yes	Yes	Yes
Size*year	No	Yes	Yes	Yes	Yes
Pre-growth*year	No	Yes	Yes	Yes	Yes
Labor market measures*year	No	No	Yes	Yes	Yes
Pop. density*year	No	No	No	Yes	Yes
All controls	No	No	No	No	Yes
R^2	0.42	0.45	0.57	0.57	0.57

Notes: The table reports regressions based on (2) using county-level data. The main regressors measure the census shock (calculated as in (1) but using county-level population counts) interacted with indicators for observations within the first two years after census adoption and for observations greater than two years after census adoption. The main regressors are additionally interacted with an indicator for counties with non-negative output per worker growth (boom) versus negative growth (bust) in the year before census adoption. The estimated coefficients for the interactions with boom counties are in Panel A and for bust counties in Panel B. The outcome is the difference between log output per worker in a given year and in the base year before the census was adopted, scaled by 100. The controls include the county-level versions of the controls described in Table 4, except that state fixed effects are replaced with fixed effects for three regions (region 1: SH, HH, NS, HB; region 2: NW,HE,RP,BW,BY,SL; region 3: BE,BB,MV,SN,ST,TH). In addition, we control for log output per worker growth in the year before the census was adopted, separately interacted with year fixed effects and the "boom/bust" indicator. Standard errors are clustered by county.

adoption. The results suggest that greater government purchases do not just reduce employment growth in boom regions, but can additionally reduce output growth by lowering the sum of capital input growth and TFP growth. Cross-region differences in capital and TFP are typically the outcome of investment decisions by firms—either investment in new machines and other production capital, into the adoption and development of new technologies, or into the human capital of their workers. The reductions in output per worker growth therefore suggest that firms also became more passive in their investment, broadly defined, when government purchases increased permanently.

4.8 No Evidence for Relocation of Establishments and Households

We analyze to what extent the relocation of economic activity across regions can account for the effects of the census shock. Data on the number of foundations and exits of firm establishments are available at the county level. In Table A4, we find no evidence that a greater census shock was associated with a significant net change in the number of establishments. This conclusions holds when we analyze the absolute change in establishments (scaled by employment in the year before census adoption) in column 1, the change in the ratio of establishment foundations to exits (again scaled by pre-census adoption employment) in column 2, and the log change in the ratio in column 3.

We investigate immigration and outmigration of people at the municipality level. We find no evidence that migration was associated with the census shock in Table A5, mirroring the establishment results. The weak migration response is consistent with previous evidence by Mertens and Haas (2006) and Huber (2018), suggesting that German establishments and households rarely move in response to low unemployment or low labor demand, even several years after a shock. Taken together, the findings suggest that the negative effects of the census shock on employment and output per worker cannot be accounted for by the relocation of establishments or people across regions.

5 Discussion of Mechanisms Behind the Negative Effects

In this section, we describe why the empirical results in bust regions are consistent with standard models but the results on boom regions are not. We then briefly sketch different non-standard explanations that could account for the negative effects in boom regions. We do not wish to impose one formal framework in this paper, but instead we discuss the merits of potential mechanisms.

5.1 The Challenge for Standard Models

The additional government purchases generated by the census shock represent a demand shock for local firms. In standard models, such a demand shock increases firms' employment and output, as long as the supply of the factors of production is not perfectly inelastic (i.e., depending on the supply of labor and capital). If the supply of factors were perfectly inelastic, employment and output would not increase but would also not decline.

In Keynesian models, greater government purchases raise employment and output most strongly in regions facing recessions and high unemployment because downward nominal price and wage price rigidities are less likely to bite and there are more slack factors in such regions. The empirical results on the positive effects of government purchases in bust regions (with low pre-growth and high unemployment) are consistent with this standard Keynesian logic. The weak short-run effects in boom regions are also consistent with Keynesian models, since the effects on output and employment can be close to zero in regions with strong growth and low unemployment where firms are not demand-constrained and there are few slack resources.

However, the estimated effects of government purchases in boom regions after two years are at odds with standard models. In particular, three results point toward a mechanism not captured by standard models: (1) weaker regional employment growth, (2) particularly in industries directly selling to local governments, and (3) weaker regional output per worker growth, implying lower investment in capital or lower TFP.

5.2 Lower Investment Costs for Government Sales

One potential modification to the standard model is that firms have to pay greater upfront investment costs when selling to private customers than when selling to the government. For example, in order to attract private customers, firms may regularly have to innovate on products and spend resources on marketing (as in Gourio and Rudanko 2014). In contrast, governments may reliably purchase standardized products over long periods and may require low marketing efforts. Indeed, many government purchases involve repeated routine services (e.g., for annual maintenance of property), multi-year contracts (e.g., provision of cultural events), and repeated contracts with the same firm but for different products. As a result, many firms can expect to remain government suppliers for decades.[13]

[13]In the Cox et al. (2024) data on firms supplying the US federal government, the value-weighted median tenure of a firm (i.e., the number of subsequent years a firm continually receives payments from the government) is 18 years. The number would be even larger if one counted firms that do not receive payment every year, but lumpy payments for multi-year contracts or firms that receive regular contracts with short breaks in-between.

Upfront investment costs can act like an annual fixed cost for firms attempting to sell in the private market. This makes specializing in government sales attractive. If government purchases increase permanently, firms can expect greater long-run sales to the government and the benefits to specializing in government sales increase. Firms may thus be less incentivized to compete for and invest in private market sales. In comparison, firms are less likely to scale back their private market investments following temporary spikes in government purchases. If firms know that they will sell to the government only for a few years, they need to rely on private market sales again in the near future, requiring them to maintain their private market investments.

On their own, differences in investment costs between government and private market sales would not necessarily generate negative effects of greater government purchases. Firms could evaluate their government and private market sales separately, so private market sales would not be affected by greater government purchases. However, combined with additional features, like capacity constraints or risk aversion, differences in investment costs may generate negative effects.

5.3 Lower Growth due to Capacity Constraints

Firms may be capacity constrained, forcing them to choose between private market or government sales. For instance, firm surveys suggest that even large firms are subject to organizational constraints, with limited managerial time and inflexible structures commonly cited as factors (e.g., Graham and Harvey 2001). Moreover, small and young firms are often subject to financial constraints. Such constraints may force firms to forego private market sales when taking on extra orders from the government. In Appendix B.3, we sketch a simple model with constrained firms and a private market fixed cost showing that greater government purchases can indeed lead firms to become specialized government suppliers, reducing total employment and output.

It is an open question whether capacity constraints are durable enough to generate medium-run employment and output losses. The results in Panel C of Figure 4 suggest that, if anything, the effects of the census shock become more negative over time. One might expect that firms are able to overcome constraints at least partly after 5 years. In the case of financial constraints, Huber (2018) finds that firms are able to find new lenders 2-3 years after a shock to their main bank. Organizational frictions may be long-lasting, but firm entry could in principle overcome the constraints of existing firms at the regional level.

5.4 Lower Growth due to Risk Aversion

Unconstrained firms that favor riskless projects—rather than just maximizing expected profits—may reduce employment growth when government purchases increase. The assumption that firms consider project risk is not a substantial departure from standard models with uncertainty. A typical assumption is that firms are risk-neutral and optimize financial market value, but that the households owning firms are risk-averse. Implicitly, the standard assumption is therefore that firms take into account the riskiness of different projects, since this maximizes the firms' value to the owners.

Firms may thus take into account the riskiness of different revenue sources when choosing which customers to serve. Selling to the government may yield relatively safe profits, whereas selling to private customers may be a riskier endeavor. For instance, firms may have to invest in innovation and marketing in order to attract private customers, without knowing whether their efforts will generate profitable sales. In addition, choosing to sell to private customers exposes firms to non-diversifiable business cycle risk, whereas the government is a reliable customer independent of the cycle.

Even if firms are risk-averse, greater government purchases should not necessarily reduce firms' total employment and output because firms could satisfy the extra government demand while maintaining their previous sales to private customers. However, as already discussed above, sales to private customers may involve upfront investment costs. In that case, an increase in government sales could make it optimal for a risk-averse firm to forego all other risky activities and rely only on the safe government sales. In Appendix B.3, we sketch a simple model with unconstrained, risk-averse firms facing a private market fixed cost. Under certain utility functions, a 5% increase in sales to the government leads firms to reduce their investment in private market sales, thereby reducing total employment and output. The example highlights that government purchases can drain firms' willingness to take risk by guaranteeing a higher minimum profit level.

5.5 Lower Growth due to Survival and Target Objectives

Some firms may maximize the probability of firm survival, rather than expected profits or utility. Focusing on government projects is a relatively safe way for a firm to operate. Greater government purchases make it possible for firms to survive on government contracts alone. Firms may therefore prefer to forego risky marketplace projects when government purchases increase, even if it lowers their profits, employment, and output.

Objective functions proposed in the behavioral literature can also account for the empirical results. For example, firms may set themselves earnings targets and not take on additional projects after reaching those targets. This possibility has been discussed

in the case of New York taxi drivers (e.g., Crawford and Meng 2011). Government purchases allow firms to reach targets with a greater certainty. It may therefore prevent firms from undertaking risky projects that, on average, would have raised employment growth.

5.6 Survey on Attracting Orders and Customers

The potential mechanisms proposed so far all share the prediction that firms reduce their investment in private market sales when government purchases increase permanently. We present suggestive survey evidence that firms indeed work less hard on winning additional orders and customers when they are told that government purchases will persistently increase. The survey results do not favor one specific mechanism over another, but instead support the general view that firms' willingness to invest in private market sales decreases in response to permanent government purchases.

We ran an online survey among managers of German firms in cooperation with a survey firm. All managers are in executive leadership positions in their firm, primarily in small and medium-sized enterprises. We do not have data on their firms, but we observe demographic information: around 60% are over 50 years old and 40% between 30 and 50; 55% are men; and their regions of residence are relatively evenly distributed across 5 bins of population density and regional purchasing power.

We tell the managers: *Suppose you find out that you will reliably receive significantly [more/fewer] orders from the government over the next 10 years than you have previously received. To what extent would you try to win additional orders?* Half of the managers are randomly selected to see a statement mentioning "more orders," whereas the other half see a statement mentioning "fewer orders." There are five potential replies, ranging from "clearly more than before" to "clearly less than before." We construct two outcome variables, one based on whether firms indicate that they will try to win "clearly more" or "slightly more" additional orders and the other based on a linear coding of the response categories. Both outcomes are transformed to have a standard deviation of 1.

We find that managers are significantly less likely to try winning more additional orders when they are told that they will receive more orders from the government. The coefficients in columns 1 to 3 of Table 10 indicate that being told there will be more government orders leads to a 0.2 standard deviation reduction in managers' attempts at winning additional orders, relative to being told of fewer orders. The coefficient is significant at the 1% level. The coefficient is robust to controlling for fixed effects for age, gender, regional population density, former GDR inhabitants, school degree, and political affiliation.

Table 10: Firm survey: trying to win orders and customers?

Outcome	(1) Try to win additional orders? Dummy	(2) Try to win additional orders? Dummy	(3) Try to win additional orders? Linear	(4) Try to win additional customers? Dummy	(5) Try to win additional customers? Dummy	(6) Try to win additional customers? Linear
More govt. orders	-0.23***	-0.23***	-0.22***	-0.22***	-0.22***	-0.23***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Observations	5,000	5,000	5,000	5,000	5,000	5,000
Controls	No	Yes	Yes	No	Yes	Yes
R^2	0.013	0.026	0.024	0.012	0.026	0.026

Notes: The table reports results from a survey of firm managers. Columns 1 to 3 are based on the question: *Suppose you find out that you will reliably receive significantly [more/fewer] orders from the government over the next 10 years than you have previously received. To what extent would you try to win additional orders?* Columns 4 to 6 are based on the question: *Suppose you find out that you will reliably receive significantly [more/fewer] orders from the government over the next 10 years than you have previously received. To what extent would you try to win additional customers?* Half of the surveys are randomly selected to mention "more orders" and the other half mention "fewer orders." The outcomes in columns 1, 2, 4, and 5 are based on an indicator that is 1 if the respondent selects "clearly more than before" or "slightly more than before" and 0 if the respondent selects "as before," "don't know," "slightly less than before," and "clearly less than before." The outcomes in columns 3 and 6 are based on a linear variable that is 2 if the respondent selects "clearly more than before;" 1 if "slightly more than before;" 0 if "as before" or "don't know;" -1 if "slightly less than before;" and -2 if "clearly less than before." We standardize all outcomes to have mean 0 and standard deviation 1. We regress the outcome on an indicator for surveys mentioning "more orders" and report the coefficient. Controls include fixed effects for age (20-39, 40-49, 50-64, >64), gender (male, female), population density of region (5 bins), resident of former GDR, school degree (full, middle, main, none/unknown), and political affiliation (8 largest parties). Standard errors are clustered by respondent.

Managers are also less likely to try winning additional customers when they are told of more government orders, as shown in coefficients in columns 4 to 6 of Table 10. The coefficients indicate that being told of more government orders leads to a 0.2 standard deviation reduction in managers' attempts at winning additional customers.

The survey results imply that firms adjust their behavior in the private marketplace when government purchases increase persistently. Specifically, firms become less active in winning marketplace business opportunities when government purchases are higher. In standard models, unconstrained firms in perfectly competitive markets would not exhibit this kind of behavior, as firms can simply evaluate the profitability of marketplace opportunities without being influenced by the option to sell to the government. The survey results thus point toward a mechanism that makes firms more passive in the marketplace when government purchases increase.

5.7 Household Wealth Effects and Government Employees Are Unlikely Channels

Government transfers to households in a region can lead to reductions in labor supply and therefore lower employment (Cohen et al. 2011). However, the census shock studied in this paper had little effect on transfers, as shown in column 2 of Table 5, and instead raised government purchases from firms. It is therefore unlikely that lower labor supply generated the employment decline observed in Table 6. In addition, lower labor supply would typically increase output per worker, as firms would have an incentive to increase capital investment to compensate for the lower availability of workers. In contrast, we find that output per worker also declined in Table 9.

It is also unlikely that increases in local government employment caused the reductions in firms' employment growth because municipalities did not raise spending on personnel, as shown in column 1 of Table 5. Greater government employment also should not have lowered output per worker.

6 Conclusion

We show that permanent expansions in government purchases in booming economies can slow the growth of employment and output per worker. We analyze exogenous variation in government purchases of municipal governments in Germany. The variation is driven by a 2011 census population recount that led to permanent revisions in funds allocated to municipal governments.

In municipalities with weak pre-growth or high unemployment, we find a standard stimulus effect: government purchases raise employment growth, especially in the short run. In contrast, in municipalities with strong pre-growth or low unemployment, we find that government purchases have weak short-run effects, but persistently reduce employment starting in the third year after purchases increased. The negative employment effects are driven by industries directly selling to municipal governments. Output per worker also grew more slowly, suggesting that firms invest less into production capital, technologies, or the human capital of their workers.

The empirical findings are consistent with a "dynamism drain" effect: permanent increases in government purchases lead firms to expect stable future revenues without needing to engage in marketplace competition. Firms may therefore forego marketplace projects that require costly investments and instead rely on government contracts. Indeed, survey responses by managers suggest that firms work less hard on winning marketplace customers when government purchases increase. Ultimately, firms become more passive, which can result in lower employment and output per worker a few years

after government purchases permanently increase.

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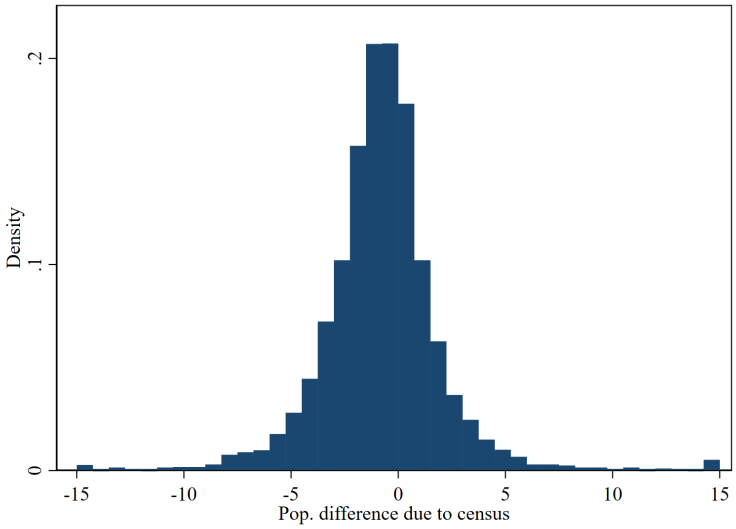
Online Appendix to "Big Government and Dynamism Drain"

Anselm Hager and Kilian Huber

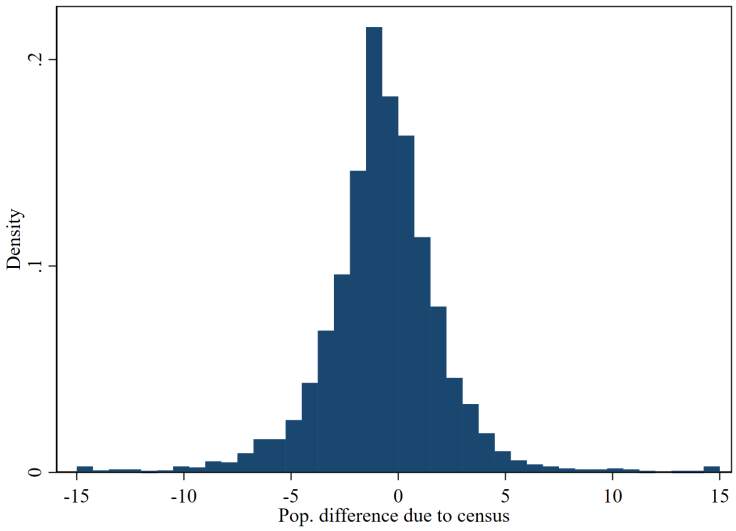
Appendix A Supplementary Figures and Tables

Figure A1: Change in 2011 population due to the census, by boom and bust

A: Only boom municipalities



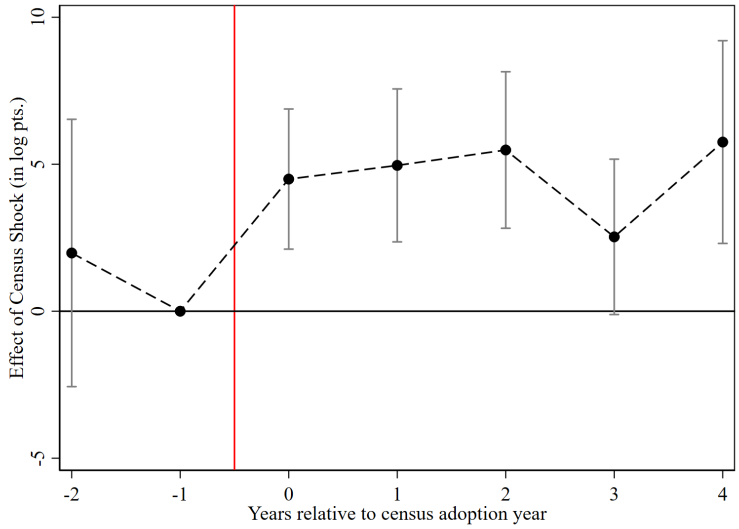
B: Only bust municipalities



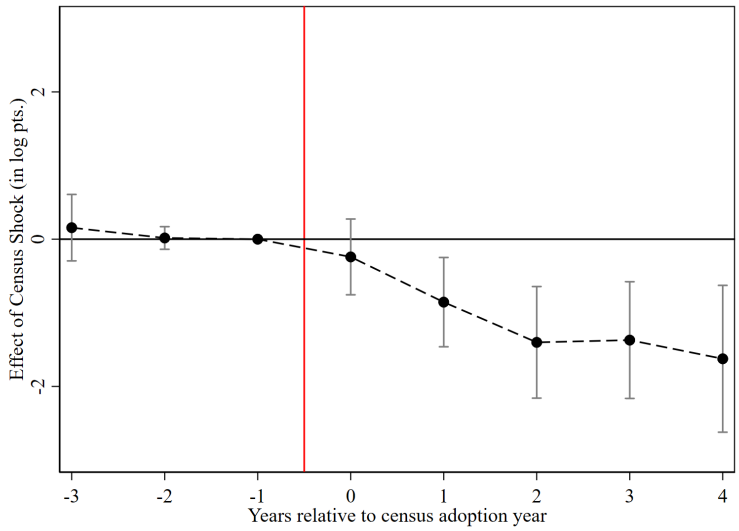
Notes: Panel A reproduces Figure 2 using only boom municipalities with non-negative employment growth in the year before census adoption. Panel B reproduces Figure 2 using only bust municipalities with negative employment growth in the year before census adoption.

Figure A2: Effect on purchases and employment excluding states adopting in 2013

A: Effect on purchases



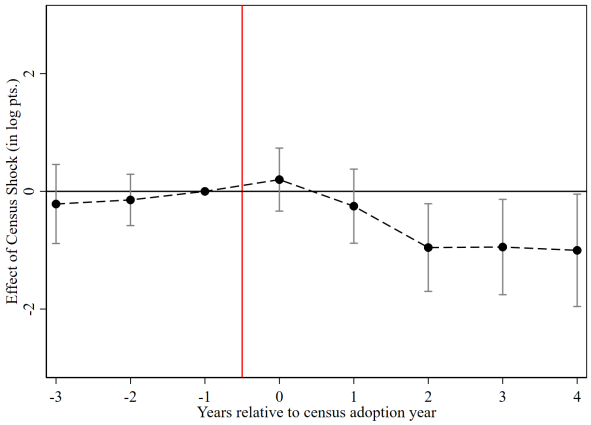
B: Effect on employment



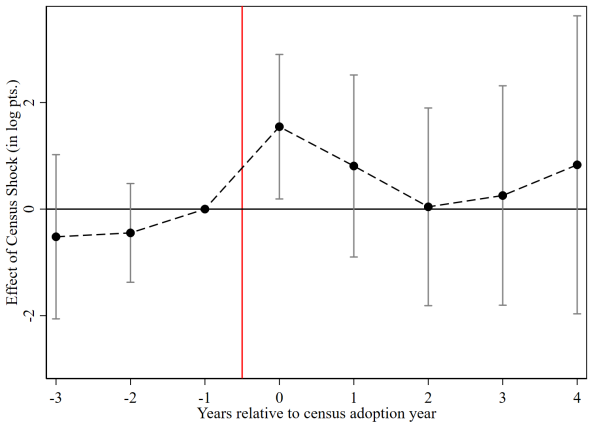
Notes: Panel A reproduces Figure 3, except it excludes municipalities in states that adopted the census immediately in 2013. Panel B reproduces Panel A of Figure 4, except it excludes municipalities in states that adopted the census immediately in 2013.

Figure A3: Effect on employment with basic controls

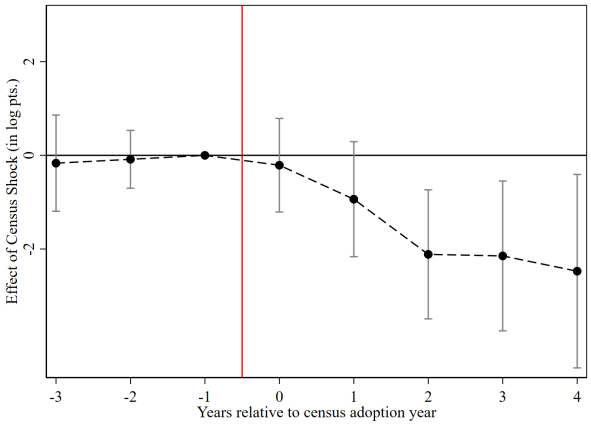
A: All municipalities



B: Municipalities with negative pre-growth

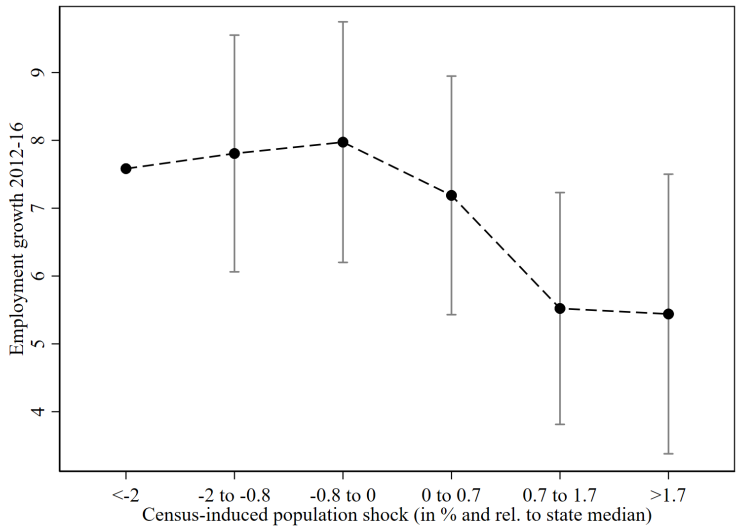


C: Municipalities with positive pre-growth



Notes: The figure is identical to Figure 4, except that all panels only control for the interaction of year fixed effects with state fixed effects and with size fixed effects.

Figure A4: Employment growth by bins of the census shock: only low unemployment municipalities



Notes: The figure reproduces Figure 6 using only municipalities with low-unemployment (below-median unemployment-to-population ratio) in the sample.

Table A1: Effect on employment with low versus high unemployment

Outcome	(1) Total employment	(2) Total employment	(3) Employment in industries selling to mun. govt.	(4) Employment in industries not selling to mun. govt.
Panel A: Municipalities with low unemployment				
Census shock *	-0.35	-0.31	-0.81	0.089
first 2 years after adoption	(0.29)	(0.29)	(0.58)	(0.50)
Census shock *	-1.02**	-1.08**	-1.93**	-0.36
over 2 years after adoption	(0.46)	(0.44)	(0.87)	(0.73)
Panel B: Municipalities with high unemployment				
Census shock *	0.37	0.35	-0.45	0.083
first 2 years after adoption	(0.54)	(0.55)	(0.89)	(0.91)
Census shock *	-0.62	-0.74	-1.54	-0.58
over 2 years after adoption	(0.88)	(0.87)	(1.22)	(1.34)
Observations	41,553	41,553	41,553	41,553
Base controls	Yes	Yes	Yes	Yes
All controls	No	Yes	Yes	Yes
R^2	0.36	0.42	0.27	0.21

Notes: The table reports regressions based on (2). The regressors measure the census shock interacted with an indicator for low-unemployment municipalities (below-median unemployment-to-population ratio) and with indicators for observations within the first two years after census adoption and for observations greater than two years after census adoption. The outcome in columns 1 and 2 is the difference between log total employment in a given year and log total employment in the base year before the census was adopted, scaled by 100. The outcome in column 3 is log employment in industries directly selling to municipal governments and in column 4 it is log employment in industries not directly selling to municipal governments (see text for the classification), scaled by 100. The controls and standard errors are explained in Table 4.

Table A2: Robustness tests for employment results

Outcome	(1)	(2)	(3)	(4)	(5)
	Total employment				Employment in constr.
Panel A: Boom municipalities with positive growth					
Census shock *	-0.40	-0.39	-0.55	-0.40	-0.15
first 2 years after adoption	(0.28)	(0.28)	(0.38)	(0.28)	(0.48)
Census shock *	-1.10**	-1.09**	-1.17**	-1.12**	-0.41
over 2 years after adoption	(0.43)	(0.43)	(0.59)	(0.47)	(0.80)
Census shock * pop.≥10,000 *				0.18	
over 2 years after adoption				(1.07)	
Panel B: Bust municipalities with negative growth					
Census shock *	1.49*	1.50*	1.46**	1.48*	0.39
first 2 years after adoption	(0.83)	(0.83)	(0.74)	(0.83)	(1.47)
Census shock *	-0.30	-0.26	-0.39	-0.29	-0.86
over 2 years after adoption	(1.40)	(1.39)	(1.27)	(1.41)	(2.44)
Observations	41,562	41,562	41,562	41,562	41,562
Base controls	Yes	Yes	Yes	Yes	Yes
All controls	Yes	Yes	Yes	Yes	Yes
Extra pre-trend control	No	Yes	No	No	No
Weighted	No	No	Yes	No	No
R^2	0.42	0.46	0.43	0.42	0.11

Notes: The table reports robustness tests for the main employment analysis in Table 6. The outcome in columns 1 to 4 is the difference between log total employment in a given year and log total employment in the base year before the census was adopted, scaled by 100. Column 1 reproduces the baseline specification from column 2 of Table 6. Column 2 additionally controls for employment growth from 2010 to 2011 interacted with year fixed effects. Column 3 uses log population 2011 as regression weight. Column 4 interacts the main regressor with an indicator for municipalities with at least 10,000 inhabitants in 2011. Column 5 uses employment in construction industries (WZ classifications 45, 46, and 47) as outcome. We find no evidence that employment in construction industries changed significantly, in line with the fact that long-run capital spending did not rise after census adoption, as shown in Table 5.

Table A3: Additional county-level results on output per worker and employment

Outcome	(1) Output per worker	(2)	(3) Employment
Panel A: Boom counties with positive pre-growth			
Census shock *	-1.17	-0.16	0.55*
first 2 years after adoption	(0.72)	(0.61)	(0.28)
Census shock *	-3.58***	-2.10**	-0.92*
over 2 years after adoption	(0.87)	(0.82)	(0.53)
Panel B: Bust counties with negative pre-growth			
Census shock *	1.21**	-0.088	0.11
first 2 years after adoption	(0.60)	(1.21)	(0.43)
Census shock *	0.65	-0.10	-0.96
over 2 years after adoption	(0.86)	(2.04)	(0.71)
Observations	3,204	3,204	3,204
Base controls	Yes	Yes	Yes
All controls	Yes	Yes	Yes
R^2	0.58	0.57	0.87

Notes: The table reports additional county-level results. The main regressors, controls, sample, and standard error clustering are explained in Table 9. Column 1 uses output per worker pre-growth to define boom counties (non-negative growth in the year before census adoption) and bust counties (negative growth in the year before census adoption); column 2 uses employment pre-growth; and column 3 uses GDP pre-growth. The outcome in columns 1 and 2 is the difference between log output per worker in a given year and in the base year before the census was adopted, scaled by 100. The outcome in column 3 is the difference between log employment in a given year and in the base year before the census was adopted, scaled by 100.

Table A4: County-level results on number of establishments

Outcome	(1) Abs. change (scaled by empl.)	(2) Abs. change in ratio (scaled by empl.)	(3) Log change in ratio
Panel A: Boom counties with positive pre-growth			
Census shock *	-0.071	-0.00022	-0.12
first 2 years after adoption	(0.43)	(0.00040)	(0.80)
Census shock *	0.022	-0.000082	-0.12
over 2 years after adoption	(0.37)	(0.00038)	(0.72)
Panel B: Bust counties with negative pre-growth			
Census shock *	1.86	-0.00055	0.086
first 2 years after adoption	(2.42)	(0.0011)	(2.09)
Census shock *	0.50	-0.00035	-0.40
over 2 years after adoption	(1.00)	(0.00077)	(1.28)
Observations	3,204	3,204	3,204
Base controls	Yes	Yes	Yes
All controls	Yes	Yes	Yes
R^2	0.33	0.33	0.34

Notes: The table reports county-level results on the net formation of establishments. The main regressors, controls, sample, and standard error clustering are explained in Table 9. The outcome in column 1 is the difference between the net number of newly registered establishments (i.e., new foundations minus exits) in the county in a given year and in the base year before the census was adopted, scaled by the number of workers in the base year in thousands. The outcome in column 2 is the difference between the ratio of new registrations to exits in the county in a given year and in the base year before the census was adopted, scaled by the number of workers in the base year in thousands. The outcome in column 3 is the difference between the log ratio of new registrations to exits in the county in a given year and in the base year before the census was adopted.

Table A5: Municipality-level results on household immigration

Outcome	(1) Abs. change (scaled by empl.)	(2) Abs. change in ratio (scaled by empl.)	(3) Log change in ratio
Panel A: Boom counties with positive pre-growth			
Census shock *	0.0015	-0.076	-0.55
first 2 years after adoption	(0.57)	(0.056)	(0.77)
Census shock *	0.23	-0.078	-0.37
over 2 years after adoption	(0.49)	(0.078)	(0.66)
Panel B: Bust counties with negative pre-growth			
Census shock *	1.25	-0.028	0.17
first 2 years after adoption	(1.93)	(0.028)	(1.23)
Census shock *	1.08	-0.033	0.57
over 2 years after adoption	(2.01)	(0.029)	(1.32)
Observations	41,534	41,534	41,534
Base controls	Yes	Yes	Yes
All controls	Yes	Yes	Yes
R^2	0.056	0.020	0.11

Notes: The table reports municipality-level results on net migration. The main regressors, controls, sample, and standard error clustering are explained in Table 6. The outcome in column 1 is the difference between net immigration (i.e., people moving in minus people moving out) in the municipality in a given year and in the base year before the municipality was adopted, scaled by the number of workers in the base year. The outcome in column 2 is the difference between the ratio of immigration to outmigration in the municipality in a given year and in the base year before the census was adopted, scaled by the number of workers in the base year. The outcome in column 3 is the difference between the log ratio of immigration to outmigration in the municipality in a given year and in the base year before the census was adopted.

Appendix B Simple Model of a Firm's Decision to Sell in the Marketplace

Appendix B.1 Setup of the Model

To identify which frictions can generate the empirical results from Section 4, we sketch a simple model. A firm in the model can engage in two projects: selling to private customers in the marketplace and selling to the government. We use the model to identify when an increase in government purchases leads the firm to turn away from private marketplace sales and focus on sales to the government, thereby lowering employment and output. We always assume that the firm has access to a private marketplace project with relatively high expected profit. The model therefore mainly applies to firms in boom regions.

The first project, selling in the marketplace, is a risky endeavor. Firms have to pay an upfront fixed cost c, which captures the costs of developing an attractive product and acquiring private customers. With probability p, the firm succeeds in generating a high level of revenue from marketplace sales, x^{high}. With probability $(1-p)$, however, the firm fails and achieves only a low level of marketplace revenue x^{low}. Each unit of revenue requires one worker to produce it, so the firm hires x^{high} workers if its product succeeds and x^{low} workers if it fails. Labor is elastically supplied at wage w. Profits are revenue minus labor costs minus fixed cost. Hence, the marketplace project yields profits:

$$\begin{aligned} \pi^{\text{high}} &= x^{\text{high}} - w \times x^{\text{high}} - \text{c} && \text{if the project succeeds;} \\ \pi^{\text{low}} &= x^{\text{low}} - w \times x^{\text{low}} - \text{c} && \text{if the project fails.} \end{aligned}$$

Selling to the government, in contrast, is safe. The government guarantees the firm a revenue of g without an upfront cost. Profits from the government project are thus:

$$\pi^{\text{g}} = g - w \times g.$$

We assume the wage is below 1 (i.e., $0 < w < 1$), so that the government project always generates positive expected profits (i.e., $\pi^{\text{g}} > 0$).

The firm's problem is to choose which projects to undertake. Expected employment

is:

$$\begin{aligned}\mathbb{E}[\text{empl}] &= p \times (x^{\text{high}} + g) + (1-p) \times (x^{\text{low}} + g) && \text{if both projects chosen;} \\ &= g && \text{if only government project chosen;} \\ &= p \times x^{\text{high}} + (1-p) \times x^{\text{low}} && \text{if only marketplace project chosen.}\end{aligned} \tag{A1}$$

Conditional on firms choosing the same set of projects, greater government purchases raise employment, as the three equations (A1) show. Hence, greater government purchases can only reduce employment if firms change their project selection in response to government purchases. We will therefore analyze under which scenarios firms adjust their project choices in a way that lowers employment.

Appendix B.2 Unconstrained Profit Maximizers Do Not Reduce Growth

As a benchmark case, we first assume that firms are unconstrained and maximize expected total profits Π over all projects, which are:

$$\begin{aligned}\Pi &= p \times (\pi^{\text{high}} + \pi^{\text{g}}) + (1-p) \times (\pi^{\text{low}} + \pi^{\text{g}}) && \text{if both projects chosen;} \\ &= \pi^{\text{g}} && \text{if only government project chosen;} \\ &= p \times \pi^{\text{high}} + (1-p) \times \pi^{\text{low}} && \text{if only marketplace project chosen.}\end{aligned}$$

The firm always chooses the government project because expected profits of the government project are always positive thanks to the assumption that the wage is below 1. As a result, the firm simply needs to decide whether to choose both projects or only the government project. The decision rule is that the firm chooses both projects if the expected profit from the marketplace project is positive (i.e., $p \times \pi^{\text{high}} + (1-p) \times \pi^{\text{low}} > 0$).

This decision rule does not depend on government purchases g. As a result, changes in government purchases do not cause the firm to change its decision on the marketplace project and do not cause employment to fall. Intuitively, unconstrained profit maximizers can always add another project when government purchases increase, as long as labor is supplied somewhat elastically. If labor supply were fully inelastic, employment growth would not increase in response to government purchases, but it would also not decrease.

Appendix B.3 Lower Growth due to Constraints

We show that government purchases can reduce employment growth when firms are constrained, for example, when firms lack the organizational or financial resources for multiple projects. Firm surveys suggest that even large firms are often subject to organizational constraints, with the time of managers and inflexible organizational structures cited as common limiting factors (e.g., Graham and Harvey 2001). Moreover, small and young firms are typically subject to financial constraints, implying that they cannot take on all projects that offer positive expected profits.

To model constraints, we assume the firm in our simple model can only take on one project. The firm prefers the marketplace project as long as government purchases g are relatively low (i.e., if $p \times x^{\text{high}} + (1-p) \times x^{\text{low}} - \frac{c}{1-w} > g$). If government purchases rise sufficiently, the firm switches to becoming a specialized government supplier. This switch can imply that employment growth falls, because there is a region of g where government purchases are preferred but employment in the government project is still lower (specifically: $p \times x^{\text{high}} + (1-p) \times x^{\text{low}} - \frac{c}{1-w} < g < p \times x^{\text{high}} + (1-p) \times x^{\text{low}}$).

As a result, if firms are constrained, greater government purchases can induce firms to become more passive in the marketplace by not paying any of the costs required to attract marketplace customers and instead to focus on government sales, thereby reducing employment growth.

Appendix B.4 Lower Growth due to Risk Aversion

Firms may be risk-averse and thus maximize the expected utility of their owners, rather than expected profits. The assumption that firms maximize expected utility is not a substantial departure from standard models with uncertainty. A typical assumption is that firms are risk-neutral and optimize their value to the owners, but that owners are risk-averse and maximize expected utility. Implicitly, firms therefore maximize the expected utility of their owners.

If the firm chooses both the marketplace and the government projects, expected utility is:

$$\mathbb{E}[\text{U}|\text{marketplace \& government}] = p \times u(\pi^{\text{high}} + \pi^{\text{g}}) + (1-p) \times u(\pi^{\text{low}} + \pi^{\text{g}}), \quad \text{(A2)}$$

where $u(.)$ is a concave utility function. If the firm chooses only the government project, expected utility is:

$$\mathbb{E}[\text{U}|\text{only government}] = u(\pi^{\text{g}}). \quad \text{(A3)}$$

Greater government purchases can imply that the firm is less willing to take risks

and therefore that the firm moves from undertaking both projects to only undertaking the government project. Consider a simple numerical example: utility is quadratic, so $u(\pi) = 10\pi - \pi^2$ for $0 < \pi < 5$; $x^{\text{high}} = 5$; $x^{\text{low}} = 2$; $c = 2$; $p = 0.22$; and $w = 0.2$. If government purchases equal 1.33, the firm chooses both projects (because the expected utility of both projects exceeds the utility of only the government project). If government purchases equal 1.4, the firm chooses only the government project. Hence, a 5% increase in government purchases (from 1.33 to 1.4) can induce the firm to drop the marketplace project.

As a result of dropping the marketplace project, the firm reduces its average employment. Average employment is $0.22 \times 5 + 0.78 \times 2 + 1.33 = 3.99$ when purchases are 1.33 and the firm finds it optimal to choose both projects. It is 1.4 when purchases equal 1.4 and only the government project is chosen.

The example highlights that government purchases can drain firms' willingness to take risk by guaranteeing a higher minimum profit level. This effect depends on the shape of the utility function and the exact parameterization. The key point is not that theory unambiguously shows that government purchases leads to lower employment. Instead, the point is to show that a simple model with utility maximizing firms can predict lower employment.