# The Employment Effects of Countercyclical Public Investments

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**Abstract**

This paper estimates the causal impact of a sizable German public investment program on employment at the county level. The program focused on improving the energy efficiency of school buildings, making it possible to use the number of schools as an instrument for investments. It also enforced tight deadlines reducing potential implementation lags. The program was cost-effective, creating, on average, one job for one year for an investment of €24'000. The employment gains are detectable after nine months and are accompanied by an unemployment reduction amounting to half of the job creation. Employment grew predominately in the directly affected industries.

**Keywords:** Building Investments, Job Creation, Employment Dynamics, Countercyclical Fiscal Policy

**JEL Classification:** E24, E62, H72, J23

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# 1 Introduction

During recessions, quick and cost-effective job creation is a main objective of fiscal policy. Yet, not all policy types are equally effective in this regard: Job creation of government transfers depends on the marginal propensity to consume, which is typically found to be smaller than one (Parker et al., 2013). Government consumption can directly create government jobs, but temporarily hiring additional government employees might be challenging (Dupor and Mehkari, 2015). Investments into the core infrastructure (transport, ICT, and utilities) may create many jobs, but typically only with a substantial time lag (e.g., Leeper et al., 2010; Leduc and Wilson, 2013). Given this heterogeneity, which types of fiscal policy are well-suited as stimulus measures?

Public investment in buildings combines several attractive features of the policies just mentioned. Like government consumption and unlike transfers, they directly create new jobs and likely have shorter implementation lags than investments in the core infrastructure.[1] It is perhaps for these reasons that many stimulus programs during the Great Recessions included substantial provisions for improving public buildings.[2] Yet, so far, it is unknown whether public investment in buildings indeed create jobs quickly and cost-effectively, or whether they crowd out private demand and require planning periods that are too long to counteract a starting recession.

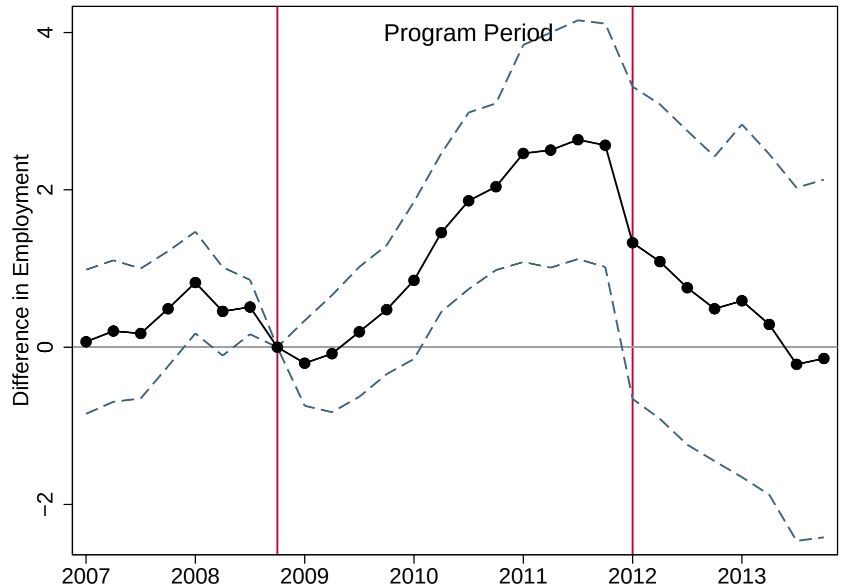
This paper analyzes whether the public investment program that was a major part of the German economic stimulus package during the Great Recession, created jobs quickly and cost-effectively. This program provided €15.8 billion (0.62 percent of German GDP) mainly for the remodeling of public buildings. It also enforced tight deadlines: All funds had to be spent by the end of 2011, two years after the program was passed into law in Q1 2009, meaning that there was little room for anticipation effects or implementation lags. Using cross-sectional data on investments at the county level, we estimate the causal dynamic effect of this program on employment as well as on unemployment.

The unique set-up of the German investment program allows us to address the challenge that stimulus investment programs are by construction endogenous to economic conditions. Specifically, governments may target regions that are hit the hardest by the recession. We

[1] In contrast to investments in the core infrastructure, building investments do not increase the growth potential, but during a downturn this may be of secondary importance.

[2] For example, Dupor and Mehkari (2015) estimate that 70 percent of the American Recovery and Reinvestment Act's (ARRA) education grants from the State Fiscal Relief Fund of $56.8 Billion were used for the modernization and renovation of the educational infrastructure. The ARRA also included around $11 Billion in subsidies for low-income residential construction and renovations. The Spanish stimulus program allotted 1.2 percent of pre-crisis GDP to local investment programs that included local construction activity (Alloza and Sanz, 2019). The main investment program of the German stimulus package focused on upgrading public buildings and is studied here.

Figure 1: Employment Dynamics Caused by the Building Investment Program




**Notes.** The connected dots show the differences in employment per total investments of €100'000 during the program period for each quarter between Q1 2007 and Q4 2013 relative to Q4 2008 and the corresponding 90 percent confidence intervals (dashed) as estimated using IV. The investment program was active at the dates between the two vertical lines. The empirical model is identical to the one used in column (2) of Table 2 and controls for date fixed effects conditional on the county's state and urbanization (measured by an urbanization index) as well as the time-varying impact of county characteristics affecting school demand (education of the workforce, school-aged population). See Section 4.1 for details.

address this endogeneity problem by exploiting the legal structure of the stimulus bill. The bill prescribed that 65 percent of funds had to be spent on investments in the educational infrastructure, in particular on improving the energy efficiency of existing buildings. This implies that the local scope for investments was closely linked to the historically predetermined number of schools. Since the number of schools is a persistent stock variable and thus unrelated to the magnitude of the recession in a county, it constitutes an ideal instrument for local investments.

Figure 1 summarizes the main results by showing the employment dynamics caused by stimulus investments. It displays the IV estimates of the average increase in the number of jobs per investment of €100'000 at each quarter between 2007 and 2013 relative to the number of jobs at Q4 2008, the last quarter before the stimulus program was passed into law.[3] For example, the point estimate of 2.5 in Q1 2011 means that, at that point in time, there were on average about 2.5 additional employees for each €100'000 invested.

Figure 1 shows that the investment program had a quick impact. The employment effects

[3]The model specification equals the one underlying the results in column (2) of Table 2. See Section 4.1 for details.

built up during the year 2010 indicating an implementation lag of three to four quarters relative to the passage of the bill in Q1 2009. This seems reasonable given that the projects had to be planned and approved before implementation. The employment gains peaked in 2011, followed by a rapid drop after the end of the program.

The results also imply that the buildings investments created jobs cost-effectively. Cumulatively, the program created 4.2 job-years for each €100'000 in investments, implying average costs per job-year of €24'000. Compared to the average labor costs in the construction industry of at least €45'000, the mean estimate implies a substantial local "wage multiplier," the ratio between the costs per job-year and the wage, of 1.9. Following the methodology proposed by Chodorow-Reich (2019) these employment gains translate into a fiscal multiplier of about 1.5 that applies to a regime with unresponsive monetary policy.[4]

Additional analyses show that the employment gains are accompanied by a drop in unemployment that amounted to half of the job creation and predominantly increased employment in the directly affected (treated) and non-tradable industries, with the treated industries contributing half of the employment gains.

Our findings suggest that building investments are among the more effective stimulus measures. Specifically, the costs per job-year estimated in this paper are at the lower end of the corresponding estimates of the literature that evaluates the *broad increase in government spending* stipulated by the American Recovery and Reinvestment Act (ARRA); see Chodorow-Reich (2019) for a review. We compare our findings to the ARRA estimates in Section 4.1. Since this is the first paper that uses cross-sectional data to evaluate a European stimulus program, these comparisons are also informative about whether the results for the U.S. are transferable to other countries.

Among the small recent literature that estimates the economic impact of public investments with cross-sectional data, our study highlights that building investments have a substantially shorter time to build lag than investment in the core infrastructure. In particular, for *highway construction* Leduc and Wilson (2013) estimate local multipliers as high as 8, but with substantial lags of six to eight years. These lags, together with the high specialization and the potentially limited regional presence of the road construction industry, may account for the small immediate effects of the ARRA's highway construction grants on local markets

[4]Because we use local variations in investments for identification, our estimates cannot account for potential aggregate effects of monetary policy, geographical spillovers, or Ricardian equivalence. Appendix B.2 provides evidence that spillovers have been small at best, and the results in Nakamura and Steinsson (2014) and Chodorow-Reich (2019) suggest that the effects of Ricardian equivalence are negligible. The aggregate multiplier of the investment program would thus be smaller than 1.5 if monetary policy of the ECB would have been more expansive absent the program, which is conceivable given Germany's weight within the Euro zone.

found by Garin (2019).[5] Finally, the projects implicitly affected by the sudden *contractions of local public works* due to Mafia infiltration in Italy studied by Acconcia et al. (2014) are likely comparable to the type of projects considered here. The local multiplier estimated for these contractions is 1.5-1.9 and thus similar in magnitude to our findings.

The next section describes the German stimulus investment program. Section 3 describes the empirical strategy and the data used. In Section 4 we discuss the main results. The last section concludes. The online appendix includes a large number of additional results and robustness checks.

## 2 The German Stimulus Investment Program

The investment program (called *Zukunftsinvestitionsgesetz*) under consideration was the major government spending measure in the two German stimulus packages enacted during the peak of the Great Recession. It stipulated investments of €13.3 billion, but due to extensive co-financing of the states, €15.83 billion or 0.62 percent of the pre-crisis GDP (in 2008) were in fact spent.[6]

The aim of the investment program was to stimulate the economy at the local level by providing local governments with federal funds. However, because investments at the regional level are within the authority of the states, the German Constitution limits the means of the federal government to finance local investments (Art. 104b *Grundgesetz*). Specifically, admissible local investment programs must fulfill three requirements. First, the provision of funds to the states can only be temporary. Second, the type of projects to be financed must be specified by law. Third, the decisions regarding which projects will receive funding is at the discretion of the state governments.

For these reasons, the stimulus bill (the *Zukunftsinvestitionsgesetz* as well as the accompanying implementation bill) entailed detailed requirements for projects to be financed via federal funds. Specifically, the bill mandated that 65 percent of funds were to be used for investments in the educational infrastructure. This first funding line authorized investments in schools, universities, and research institutes with an emphasis on the energy-saving remodeling of existing buildings. The remaining 35 percent of funds had to be used for investments

[5]In auxiliary results and without accounting for spillovers, Leduc and Wilson (2017) estimate relatively high costs per job-year of $500'000 within the road construction sector.

[6]At the time, the government also passed two smaller investment programs at the federal level (mainly transportation infrastructure), reductions in taxes and social security contributions, increases in child benefits and commuter allowances, and subsidies for households and firms (mainly for a cash-for-clunkers program). The government also intervened directly in the labor market by extendingthe duration of short-time work benefits to workers of firms in temporary financial distress. See Bundesministerium für Wirtschaft und Technologie (2011) for details. In Appendix B.5 we demonstrate that our findings are unaffected when controlling for other fiscal measures.

in the general public infrastructure such as hospitals and broadband infrastructure. To reduce the fungibility of the funds, only "new and additional" projects could be financed. This meant that projects that were already budgeted could not be financed by the program.

In addition to these restrictions on the types of investment projects, the bill required that projects were implemented locally. It mandated that 70 percent of the funds were to be spent on investments at the county or municipal level. The federal government also loosened the rules for public procurement to speed up the implementation of projects: Contracts for projects with values up to €100'000 could be allocated freely, and contracts for projects with values up to €1 million could be allocated via an invited, non-public, tender with at least three offers. According to the German Court of Auditors, the loosening of restrictions substantially increased the share of local contractors for the stimulus projects (Bundesrechnungshof, 2012).

The federal government provided €10 billion for investments and distributed these funds among the states following a standard allocation formula.[7] The federal funds were matching grants financing at most 75 percent of the project costs; the remaining 25 percent of the funds had to be provided by the states or more regional layers of government (counties and municipalities). The latter contributed more than the required €3.3 billion so that the total spending equaled €15.83 billion. The final selection of stimulus projects was at the discretion of the states. While the exact allocation mechanisms differed widely across states, most of them used a combination of the following three procedures: (i) the formulary allocation of funds to local layers of government (based on, e.g., the population, the number of school students, or the area), (ii) a state-wide selection among project proposals, and (iii) the direct implementation of projects through the state government (Slansky, 2010).

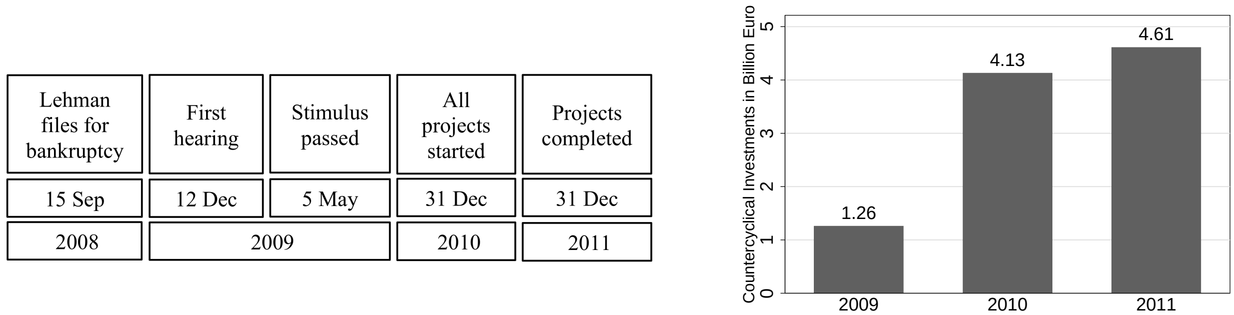
Panel (a) of Figure 2 provides a timeline of the swift implementation of the stimulus program. The first parliamentary hearings took place on January 12, 2009. The parliament passed the bill on March 5. Projects could receive financing from the program only if they had been started after January 27, 2009, and all projects had to be under way by the end of 2010. Projects had to be completed by the end of 2011, less than three years after the passage of the program. Panel (b) of Figure 2 shows the yearly spending of the €10 billion federal funds: 12.6 percent were spent in 2009, 41.3 percent were spent in 2010, and the remaining 46.1 percent were spent in 2011 (Bundesministerium der Finanzen, 2011, 2012, 2013).

[7] The formula is called *Koenigsteiner Schluessel* and is determined by the share of tax revenue and the population share of the states. In the empirical strategy, we account for the potential endogeneity of the allocation of funds to the states due to this formula via time fixed effects at the state level.

Figure 2: Time Structure of the Countercyclical Investment Program

(a) Timeline of Program Implementation

(b) Federal Spending by Year




**Notes.** This figure shows the timeline of the legislative process of the investment program (Panel (a)) and the total federal spending on investment projects by year (Panel (b)). The sum of federal spending equals €10 billion, with the remaining funds provided by state and regional governments.

# 3 Empirical Model, Data and Identification

## 3.1 Empirical Model and Data

The goal of the empirical strategy is to assess both the dynamic employment response and the overall employment effect of the investment program. To this end, we use a generalized difference-in-differences (DiD) framework to estimate the investment-induced employment gains, denoted $\beta_t$, for the duration of the stimulus program (Q1 2009 to Q4 2011), as well as two years prior and after the program. Specifically, for quarterly dates $t \in$ [Q1 2007, Q4 2013], we estimate variants of the following model:

$$(Un)Employment\,p.c._{c,t} = \sum_{t:t\neq \text{Q4 2008}} \beta_t\, Investments\,p.c._c \times Date_t + CountyFE_c$$
$$+ \sum_{t:t\neq \text{Q4 2008}} Date_t \times \boldsymbol{CountyCharacteristics}_c^{'}\,\boldsymbol{\Gamma_t} + \psi\ PopGrowth_{c,t} + \varepsilon_{c,t}, \quad (1)$$

where the index $c$ denotes the county, and "p.c." (for "per capita") in the variable name indicates that the variable is normalized by the county's working-age population measured in 2008. Investments are measured in €100'000.

**Dependent variables** The dependent variables are employment and unemployment at the county level normalized by the county's working-age population measured in 2008. The quarterly employment data counts every employed individual who lives in a county and pays social security contributions, including part-time workers but excluding the self-employed and public servants. The quarterly unemployment data contains every individual who lives within a county and receives unemployment benefits. For both series, we filter out county-specific seasonal fluctuations using the interaction of county and quarter-of-year fixed effects. In addition, we obtained, from 2008 onward, the quarterly series of employment, disaggregated by the three-digit industry code of the workers' employers.

**Countercyclical investments** The main independent variable, $Investments\,p.c._c$, is the total sum of countercyclical stimulus investments between the end of January 2009 and the end of December 2011 in a county normalized by the county's working-age population measured in 2008. The primary source for this data is an administrative database of the 42'530 projects financed by the program, which we obtained from the Federal Ministry of Finance. The database contains the total investment in each project (summing up to €15.83 billion nationwide) during the three year period between 2009-2011 and the location where the project was implemented, but not when the projects were implemented. Based on the project locations, we aggregate the total investments at the county level.

There is significant variation in investments across counties. The inter-quintile range of investments is €132 per capita, which is substantial compared with average investments of €282 per capita. For the mean county with a working-age population of about 127'000 persons, the inter-quintile range corresponds to sizable differences in investments of €16.8 million. Figure A.1 in Appendix A.2 illustrates the geographic variation of investments.

To estimate the dynamic employment response, we interact the cross-sectional data on investments ($Investments\,p.c._c$) with date dummies (denoted $Date_t$). The baseline is Q4 2008, so that all employment gains are measured relative to the last quarterly date before the (retroactive) start of the investment program in January 2009. Thus (1) delivers estimates of $\beta_t$ both for the dates after Q4 2008, when the effects of the investments should be observed, and for the dates Q1 2007 to Q4 2008, for which $\beta_t$ should equal zero, since the investment program was neither active nor expected at that time.

**Controls** The control variables in (1) include county fixed effects (denoted $CountyFE_c$) and date fixed effects for all state-urbanization strata. The urbanization strata are given by the values of an urbanization index published by the German Federal Office for Building and Regional Planning. It classifies each county, based on its total population and population density, as either "very rural," "rural," "city," or "major city." The date fixed effects eliminate all employment differences due to policies at the state or federal level, including those that may dampen the effectiveness of the investment program, such as monetary policy or tax changes to maintain balanced budget requirements (see Nakamura and Steinsson, 2014, for an extensive discussion). The urbanization index controls for potential differences in the employment dynamics across urban and rural counties that may be correlated with urbanization-related differences in the existing (building) infrastructure. Similarly, we control for other county characteristics, $\boldsymbol{CountyCharacteristics_c}$, that may be simultaneously correlated with employment outcomes and investments. In particular, we control for employment shares by education, the number of individuals between 6 and 18 years of age relative to the working-age population, the numbers of hospitals and universities normalized by the working-age population. All these variables are measured in the first quarter of 2008 and interacted with date fixed effects.[8] Finally, we control for population growth using the ratio of the working-age population at $t$ to the working-age population in 2008 (denoted $PopGrowth_{c,t}$). All data are from German official statistics. Appendix A.1 describes the data sources and definitions.

### 3.2 Instrumental Variables

The main concern for identification is that the state governments used investment funds to support those counties that they expected to be hardest hit by the recession. If this is the case, $Investments_c$ and the error term $\varepsilon_{c,t}$ are negatively correlated (at least for $t \geq$ Q1 2009), and the OLS estimates of $\beta_t$ are biased towards zero.

We address this endogeneity problem by exploiting the legal requirements of the stimulus bill to construct an instrument for stimulus investments: 65 percent of stimulus funds had to be used for investments in the local educational infrastructure, particularly for the energy-saving remodeling of existing buildings. The number of buildings of the educational infrastructure—typically schools—within a county thus determined, to a large extent, the scope for investments.

We construct the instrument using data from the German Federal Statistical Office (*Destatis*) on the five major and five minor school types in Germany as of 2008. We aggregate the ten school types according to their size into two categories. The first category, called "academic high schools," encompasses two types of secondary schools that award a degree (*Abitur*), which allows the pursuit of a college education. The second category includes all the remaining school types, namely primary schools as well as secondary schools that offer degrees which are the precondition for vocational training. We call this category "primary and secondary schools." With on average 196 students, the latter schools are substantially smaller than the average "academic high school" with 788 students[9]

There is substantial variation in the number of schools across counties. Figure 3 displays

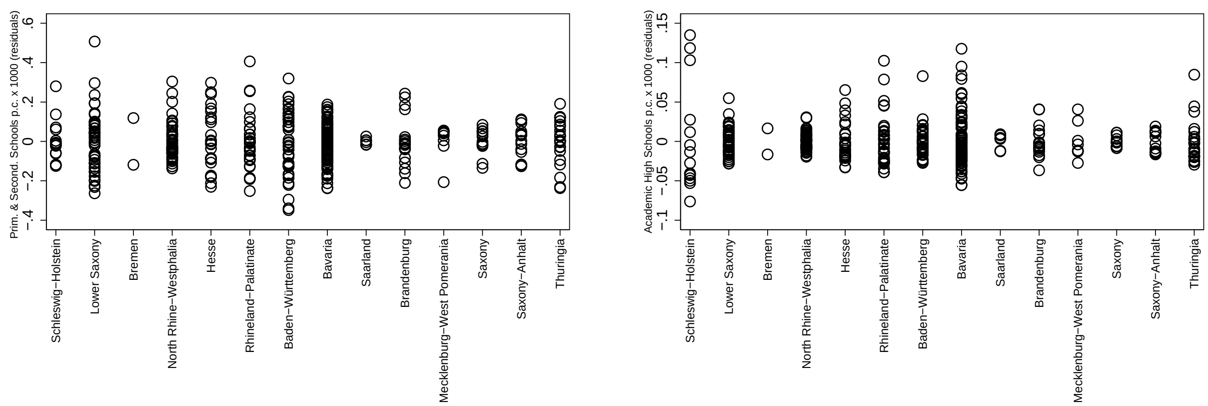
[8] The county characteristics described here are the covariates used in the main empirical analyses. The robustness checks in Section B.5 add additional variables to the empirical model.

[9] Appendix A.3 provides additional information on school sizes and on the classification of schools.

Figure 3: The Distribution of Schools per Capita within States

(a) Primary and Secondary Schools

(b) Academic High Schools




**Notes.** This figure shows the variation in *Academic High Schools* and *Primary and Secondary Schools* per 1000 individuals of working age across states. Each circle corresponds to one county and shows the number of schools net of its state-specific average.

the number of schools per 1000 individuals of working age within the German states. Each circle represents the number of schools within one county relative to their state-specific average. The number of schools varies considerably within states. The maximum difference between the county with the lowest and the county with the highest number of *Primary and Secondary Schools* per 1000 individuals equals at least 0.3 to 0.4, which is large considering that the average is 0.55. The average number of *Academic High Schools* per 1000 individuals is 0.07 so that the maximum difference of 0.05 to 0.1 is sizable as well.

**Relevance of instruments** One main assumption of the IV strategy is that the instruments are relevant, i.e., that the number of schools is a strong predictor of investments. Typically, the relevance of the instruments is tested using the first stage of the IV model. In model (1), every interaction between $Investments_c$ and a date indicator is an endogenous variable so that the first stage for (1) is a system of equations—one equation for each interaction between $Investments_c$ and the indicators for dates $\tau \in \{\text{Q1 2007}, \text{Q2 2007}, \ldots, \text{Q4 2013}\}$—of the following form:

$$
\begin{aligned}
Investments\,p.c._c \times Date_\tau = & \sum_{t: t \neq \text{Q4 2008}} Date_t \times \boldsymbol{Schools}_{\boldsymbol{c}}^{'}\, \boldsymbol{\Theta}_{\boldsymbol{t}}^{\boldsymbol{\tau}} + CountyFE_c^\tau \\
& + \sum_{t: t \neq \text{Q4 2008}} Date_t \times \boldsymbol{CountyCharacteristics}_{\boldsymbol{c}}^{'}\, \boldsymbol{\Lambda}_{\boldsymbol{t}}^{\boldsymbol{\tau}} + \psi^\tau \; PopGrowth_{c,t} + \nu_{c,t}^\tau, \quad (2)
\end{aligned}
$$

where $\boldsymbol{Schools_c} = (Academic\ High\ Schools\ p.c._c,\ Primary\ and\ Secondary\ Schools\ \ p.c._c)$ is a vector containing both categories of schools defined in Section 3.2 and where the index $\tau$ indicates the coefficients of the date-$\tau$ first stage.

Nevertheless, the first stage is almost exclusively identified from cross-sectional variation, so that we can infer the strength of the instruments from estimating a simple cross-sectional variant of Equation (2). This is because the system of equations defined by (2) closely resembles a repeated cross-section (one for every date $\times$ investment interaction), since both the instruments and the controls are interacted with date dummies. Indeed, the only variable without a time-varying coefficient on the right-hand side of (2) is population growth. For this reason, this section presents the result of estimating a variant of equation (2) using only the cross-section of the data in Q4 2008. Appendix A.5 reports the full first stage including test statistics for weak instruments.

Table 1 shows that schools are a strong predictor of total investments in increasingly demanding specifications of the cross-sectional variant of the first stage. Regardless of whether we add measures for school demand—the educational composition of the workforce and the school-age population—in column (2), or other determinants of building investments (the number of hospitals and universities) in column (3), the Kleibergen–Paap Wald statistic of the instruments remains above or very close to the conventional critical value of ten. We also report the Shea Partial $R^2$ of the instruments. The Shea Partial $R^2$ varies less with the inclusion of uninformative instruments than the F statistic. This is important, because the only relevant instruments for each date-$\tau$ equation in the full, dynamic model (2) are the interactions of $\boldsymbol{Schools_c}$ with the respective date-$\tau$ indicator. All other date interactions of $\boldsymbol{Schools_c}$ are uninformative, which mechanically reduces the test statistics for joint significance of all the instruments. In contrast, the Shea Partial $R^2$ remains largely unaffected, so that it provides a useful assessment of the strength of the instruments in the full model.

In terms of magnitude, the coefficients in Table 1 represent the average increase in investments (in €100'000) due to one additional school as both schools and investments are normalized by the working-age population. One additional academic high school is associated with an increase in investments between €1.01 million and €1.75 million, while one additional primary or secondary school leads to an increase in investments of €20'000 to €230'000. Moreover, Table A.3 in Appendix A.4 shows that schools primarily explain school investments. It also sheds light on the source of the difference in the average funding per school evident from Table 1: Academic high schools are not only larger than primary and secondary schools, but also received, on average, twice as many projects per school.

Table 1: First Stage

| | Countercyclical Investments p.c. in €100'000 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Academic High Schools p.c. | 17.47 | 11.81 | 10.09 |
| | (2.71) | (2.74) | (2.98) |
| Primary & Second. Schools p.c. | 0.18 | 2.28 | 1.53 |
| | (0.58) | (0.70) | (0.65) |
| Empl. Share w College /100 | | 1.17 | 0.71 |
| | | (0.36) | (0.34) |
| Empl. Share w Vocational Tr. /100 | | −0.08 | −0.10 |
| | | (0.22) | (0.20) |
| Share School-Age Pop /100 | | −0.64 | 0.01 |
| | | (0.44) | (0.42) |
| Universities p.c. | | | 85.19 |
| | | | (21.92) |
| Hospitals p.c. | | | 4.35 |
| | | | (2.95) |
| State × UrbanIndex FE | yes | yes | yes |
| Kleibergen–Paap F | 21.36 | 17.90 | 9.77 |
| Shea Partial $R^2$ | 0.15 | 0.11 | 0.07 |
| Observations | 400 | 400 | 400 |

**Notes.** The dependent variable *Countercyclical Investments p.c. in €100'000* is the sum of investments normalized by the working-age population (indicated by "p.c." for "per capita") over the years 2009 to 2011. *Academic High Schools p.c.* is the number of high schools in a county that award the "Abitur," the entry requirement for universities. *Primary and Secondary Schools p.c.* is the total number of primary schools and secondary schools that offer degrees that allow the pursuit of vocational training. *Empl. Share w College* and *Empl. Share w Vocational Tr.* are the share of employees with a college degree and vocational training, respectively. *Share School-Age Pop* is the number of individuals between 6 and 18 years of age as a fraction of the working-age population. *Universities p.c.* and *Hospitals p.c.* are the number of universities and hospitals. *State × UrbanIndex FE* are fixed effects for the interaction of indicator variables for the German states and for the values of a four-point urbanization index. *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* are the excluded instruments for the Kleibergen–Paap F statistic and the Shea Partial $R^2$. The sample is the cross-section of counties as measured in Q4 2008. Robust standard errors are in parentheses.

**Exclusion restriction** The second main assumption of the IV strategy is the exclusion restriction, which requires the errors $\varepsilon_{c,t}$ to be independent of the instruments. This implies that, conditional on the covariates, schools may be correlated with employment outcomes only via their effect on investments. Consistent with this assumption, Figure 1 in the introduction as well as the IV results in Table 2 show that instrumented investments are indeed unrelated to employment before the enactment of the stimulus program in Q1 2009.

For the time after the enactment of the program, the exclusion restriction is untestable. However, the number of schools in Germany is very persistent over time and hence unlikely to be correlated with the economic conditions in the short or medium run. Indeed, a regression of the total number of schools in 2008 on the number of schools in 1995 (the earliest date at which this data is available) and state dummies delivers an adjusted $R^2$ of 0.86. This strong persistence is consistent with the age distribution of public buildings from the German census of 2011. Of all non-residential public buildings with one housing unit—the building category applicable to schools that comprise housing for the school's caretaker—43 percent were constructed before 1948, 84 percent before 1978, and 93 percent before 1995. The number of schools was therefore predominantly determined by policy decisions in the 1970s or earlier, so it is likely that schools are independent of employment outcomes during the 2009 recession.

Appendix A.6 reports further evidence on the persistence of the number of schools.

# 4 Results

This section presents the main findings. The empirical analysis shows that the investment program increased employment at low costs, with employment rising three quarters after the program's enactment. The employment gains were accompanied by a drop in unemployment. In Section 4.3, we show that the program primarily generated jobs in the directly affected and non-tradable industries.

## 4.1 Investments Increase Employment Quickly and Cost-Effectively

Figure 1 in the introduction graphically summarizes the main results. It plots the coefficients $\{\beta_t\}_{t:t\neq \text{Q4 2008}}$, along with their 90 percent confidence interval, that measure the average difference in employment at the quarterly date $t$ relative to Q4 2008 for each €100'000 invested. These coefficients are estimated from the model described by (1) and (2), with the following set of covariates: county fixed effects, date fixed effects at the state $\times$ urbanization level, population growth, and factors affecting the demand for schools, namely the the school-aged population and the employment structure by education.

The first finding apparent from Figure 1 is that the stimulus program generated employment quickly. After the passage of the stimulus bill in Q1 2009, employment started to increase in response to investments with a lag of four quarters. The employment gains peaked in 2011, and sharply dropped after the end of the program in 2012 and 2013, similar to the aggregate spending pattern described in Section 2. The second finding is that the employment gains were sizable: €100'000 in investments generated on average about 1.5 additional jobs in 2010, and about 2.5 jobs throughout 2011.

Column (2) of Table 2 reports the quantitative results for the empirical specification underlying Figure 1. Here, we reduce the number of coefficients by estimating the average effect for all quarterly dates before the investment program (Q1 2007 to Q3 2008) and after the end of the program (Q1 2011 to Q4 2013). During the program (2009–2011), we estimate the average employment differences for each year, so that the employment dynamics depicted in Figure 1 can also be read from the table.[10] Before the program and during the first year of the program (2009), the employment gains are statistically indistinguishable from zero. In 2010, investments of €100'000 created 1.55 additional jobs on average, with the 90 percent confidence interval (CI) of [0.5, 2.6], and 2.54 jobs in 2011 (90% CI [1.1, 4.0]). The employment gains for the period after the stimulus program are again statistically indistinguishable from zero.

To quantify the overall employment gains, we compute the cumulative gain in job-years caused by investments of €100'000 during the program period, shown at the bottom of Table 2. For the main specification in column (2), the cumulative employment gains amount to substantial 4.2 job-years (90% CI [1.3, 7.1]), resulting in relatively low average costs per job-year of €24'000 (90% CI [€7'573, €40'151], calculated via the Delta method).

Columns (1) and (3) of Table 2 summarize the results of IV specifications with different sets of covariates. The most parsimonious model in column (1) only controls for county fixed effects, date fixed effects at the state × urbanization level, and population growth. The most demanding specification in column (3) adds the number of hospitals and universities to the model to capture the determinants of investments from funding lines unrelated to schools.[11] In comparison to the main specification in column (2), the parsimonious specification in

[10]Formally, we substitute the empirical model (1) with the following slight modification:

$$Employment\,p.c._{c,t} = \beta_{pre}\, Investments_c \times \mathbb{1}\,(\text{t}\in[\text{Q1 2007,Q3 2008}])$$

$$+ \sum_{Y=2009}^{2011} \beta_Y\, Investments_c \times \mathbb{1}\,(\text{t}\in[\text{Q1 Y, Q4 Y}]) + \beta_{post}\, Investments_c \times \mathbb{1}\,(t \in [\text{Q1 2012, Q4 2013}])$$

$$+ CountyFE_c + \sum_{t\neq \text{Q4 2008}} Date_t \times \boldsymbol{CountyCharacteristics_c'}\, \boldsymbol{\Gamma_t} + \psi\; PopGrowth_{c,t} + \hat{\varepsilon}_{c,t}.$$

[11]Figure B.1 in Appendix B.1 displays the full employment dynamics corresponding to these specifications.

Table 2: The Effects of Countercyclical Investments on (Un)Employment

| | Employment Rate | | | | | | Unemployment Rate | |
|---|---|---|---|---|---|---|---|---|
| | IV Estimates | | | OLS Estimates | | | IV | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Investments p.c. | | | | | | | | |
| × 2007–Q3 2008 | 0.18 | 0.39 | 0.16 | −0.11 | −0.02 | −0.09 | 0.02 | 0.17 |
| | (0.36) | (0.37) | (0.48) | (0.11) | (0.12) | (0.13) | (0.52) | (0.23) |
| × 2009 | −0.10 | 0.10 | −0.10 | 0.35 | 0.13 | 0.12 | 0.14 | 0.13 |
| | (0.37) | (0.42) | (0.54) | (0.16) | (0.14) | (0.15) | (0.44) | (0.16) |
| × 2010 | 0.87 | 1.55 | 1.21 | 0.44 | 0.31 | 0.12 | −0.58 | 0.02 |
| | (0.43) | (0.61) | (0.78) | (0.23) | (0.23) | (0.22) | (0.63) | (0.22) |
| × 2011 | 2.23 | 2.54 | 2.90 | 0.64 | 0.49 | 0.36 | −1.44 | −0.06 |
| | (0.67) | (0.89) | (1.27) | (0.30) | (0.33) | (0.32) | (0.71) | (0.26) |
| × 2012–2013 | 2.50 | 0.52 | 0.54 | 0.70 | 0.14 | 0.19 | −1.47 | −0.02 |
| | (1.06) | (1.26) | (1.67) | (0.45) | (0.45) | (0.44) | (0.80) | (0.27) |
| County Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes | yes | yes | yes | yes |
| Date Fixed Effects × | | | | | | | | |
| State × UrbanIndex | yes | yes | yes | yes | yes | yes | yes | yes |
| Emp. Shares by Educ. | no | yes | yes | no | yes | yes | yes | yes |
| School Age Population | no | yes | yes | no | yes | yes | yes | yes |
| Universities & Hospitals | no | no | yes | no | no | yes | no | no |
| min(Shea Partial $R^2$) | 0.15 | 0.11 | 0.07 | . | . | . | 0.11 | . |
| Cumulative Job-Years | 2.99 | 4.19 | 4.00 | 1.43 | 0.93 | 0.60 | 1.88 | −0.10 |
| SE Cumulative Job-Years | 1.26 | 1.74 | 2.31 | 0.63 | 0.65 | 0.62 | 1.63 | 0.60 |
| Costs per Job-Year | 33401 | 23862 | 25001 | 69980 | 107964 | 166670 | 53102 | -1018747 |
| SE Costs per Job-Year | 14022 | 9880 | 14458 | 30630 | 75958 | 173316 | 45834 | 6198529 |
| Observations | 11200 | 11200 | 11200 | 11200 | 11200 | 11200 | 11200 | 11200 |

**Notes.** The dependent variable in columns (1) to (6) is the employment rate at each quarterly date between Q1 2007 and Q4 2013. The dependent variable in columns (7) and (8) is the unemployment rate. *Investments p.c.* × *2007–Q3 2008* is the interaction of investments in €100'000 with an indicator that equals one for the observations between Q1 2007 and Q3 2008. All the other interactions are defined accordingly; the baseline is Q4 2008. The horizontal lines between the estimates indicate the beginning and the end of the stimulus program. *Population Growth* is the ratio of the current working-age population and the working-age population in 2008. The following variables, measured in 2008, are interacted with the full set of date fixed effects: *State* ×*UrbanIndex* (interactions of indicators for the states and the values of the urbanization index), *Emp. Shares by Educ.* (shares of employees with a college degree and with vocational training), *School-Age Population*, and *Universities and Hospitals*. *Min(Shea Partial $R^2$)* reports the minimum of the Shea $R^2$ of the excluded instruments—the date interactions of *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.*—among all the first stages (one for each interaction of *Investments p.c.*). The number of *Job-Years* is the sum of the coefficients of *Investments p.c.* between 2009 and 2011. *Costs per Job-Year* equal 100'000/*Job-Years*. The standard errors of *Job-Years* and *Costs per Job-Year* are calculated via the Delta method. Standard errors clustered at the county level are in parentheses.

column (1) indicates slightly smaller employment gains throughout 2010 and 2011, but which persist during the years after the program. The most demanding specification, in turn, delivers estimates of similar magnitude as in column (2), but with lower precision. Both specifications imply substantial cumulative job creation and low costs per job-year of €33'400 (90% CI [€10'283, €56'519]) in column (1) and €25'000 (90% CI [€1'165, €48'838]) in column (3).[12]

Columns (4) to (6) of Table 2 present the OLS estimates with the same covariates as the IV estimates in columns (1) to (3). The estimated employment differences during the program period in column (4) are at most half as large as the corresponding IV estimates in column (1). These estimates imply that 1.4 job-years were created per €100'000 spent (90% CI [0.4, 2.5]), such that creating one job-year cost about €70'000 (90% CI [€19'481, €120'479]). Adding covariates leads to a sharp drop in the coefficients so that the estimated employment gains become statistically indistinguishable from zero and economically irrelevant. Note that the small OLS coefficients are consistent with the main endogeneity concern that state governments channeled funds into those counties that were expected to be most affected by the crisis. The resulting negative correlation between $Investments_c$ and the error term $\varepsilon_{c,t}$ would then bias the OLS estimates of $\beta_t$ towards zero.

To put the IV estimates in perspective, we compare the estimate of the costs per job-year in column (2) of Table 2—roughly €24'000— with wages in construction. Different yearly wages can be used for this comparison: the minimum wage in construction of about €23'000, the union wages of construction workers ranging from €30'000 to €38'000, or the average labor costs in the entire construction sector (including benefits and taxes) of about €45'000.[13] These wages imply substantial "wage multipliers," the ratio between the wage and the costs per job-year, between 0.9 and 1.9.

We also map the employment gains to the output multiplier using the production function approach suggested by Chodorow-Reich (2019). He argues that the output multiplier, $\beta_Y$, can be approximated via the following formula:

$$\beta_Y \approx (1-\alpha)(1+\chi)(\text{cost per job year})^{-1} Y/E, \tag{3}$$

where $\alpha \approx 1/3$ is the capital share of output, $\chi$ is the elasticity of hours worked with respect

[12] Columns (1) to (3) of Table 2 also report the minima of the Shea Partial $R^2$ across all the first stage equations of the relevant model to facilitate the comparison of the strength of the instruments with the corresponding cross-sectional first stage equation in Table 1. Because the first stage is identified exclusively from cross-sectional variation, it comes as no surprise that the minima of the Shea Partial $R^2$ are equal to the ones from the cross-sections. For additional details on the first stage see Appendix A.5, which reports the complete first stage estimates corresponding to the baseline specification in column (2) of Table 2.

[13] Appendix A.1 lists the sources for the data on wages.

to employment, and $Y/E$ is output per worker. Output per worker is readily available from the national statistics and equals €39'400 in 2009, €42'800 in 2010, and €44'800 in 2011 for the target industries (these are the numbers for construction; the numbers for retail are similar). The elasticity of hours can be approximated using data of van Rens (2012), which yields $\chi \approx 0.23$. Given the values for $\chi$, output per worker, and the yearly employment gains in the baseline specification (column (2) of Table 2), the output multiplier equals 1.5. This local multiplier does not explicitly account for the potentially counteracting effects of monetary policy or Ricardian equivalence at the aggregate level. Yet, as the Ricardian effects are probably quantitatively small, this local multiplier is roughly equivalent to the aggregate multiplier with inactive monetary policy, e.g., at the zero lower bound (Nakamura and Steinsson, 2014, and Chodorow-Reich, 2019).

The €24'000 costs per job-year correspond to \$33'000 when using the average euro-dollar exchange rate during the program, which is at the lower end of cost per job-year estimates for the American Recovery and Reinvestment Act (ARRA). Closest to this figure are the costs per job-year of \$26'000 for the Medicaid state fiscal relief component of the ARRA (Chodorow-Reich et al., 2012).[14] The remaining ARRA studies estimate the combined effects of its diverse spending components (Feyrer and Sacerdote, 2011; Wilson, 2012; Conley and Dupor, 2013; Dupor and McCrory, 2018; Dupor and Mehkari, 2016; Dube et al., 2018). The costs per job-year reported in these papers differ widely between around \$54'000 (Dupor and McCrory, 2018) and \$200'000 (Conley and Dupor, 2013). Our estimates are at the lower end of this range, suggesting that building investments are among the more effective stimulus measures.[15]

### 4.2 Investments Reduce Unemployment

The employment gains found in the previous section could arise from different sources: a reduction in unemployment, a reduction in the number of inactive individuals, or flows out of self-employment into formal employment, as the German employment data lacks information on self-employment. Of these possibilities, flows out of unemployment are likely to generate sizable economic gains, as they mobilize slack resources and reduce government transfers. Moreover, preventing an increase in unemployment was the major policy objective of the

[14] Note, that Shoag (2010) and Suárez Serrato and Wingender (2016) find costs per job-year of similar magnitude for windfall government spending during normal times as well.

[15] Studies of other specific countercyclical measures in the U.S. do not estimate their overall effectiveness in terms of job creation. For tax rebates, Parker et al. (2013) find a marginal propensity to consume between 0.5 and 0.9. For the cash for clunkers program, Mian and Sufi (2012) and Green et al. (2016) estimate that \$2.85 billion in subsidies caused a short lived demand increase (and a subsequent demand reversal) of 360'000 to 540'000 cars.

investment program. We now ask whether investments led to a reduction—or prevented an increase—in unemployment.

Columns (7) and (8) in Table 2 report the IV and OLS estimates of the main specification in column (2), but this time with the unemployment rate as the dependent variable. The reductions in unemployment amount to around half of the respective employment gains. The IV estimates mirror the corresponding employment dynamics: Unemployment starts decreasing in 2010, and the effect peaks in 2011. While the program was active, investments of €100'000 reduced unemployment by on average 1.9 person-years (90% CI [-0.8, 4.6]). In contrast to employment, however, the unemployment effect persisted in the post-program period. This could be in line with the fading employment effect if parts of the individuals formally employed via the program became self-employed when the investment projects were completed. Finally, as for employment, the OLS estimate of the unemployment effects is considerably smaller than the corresponding IV estimates.

### 4.3 Investments and Employment Gains across Industries

The stimulus program stipulated the upgrade of the local public infrastructure in general and the renovations of schools in particular. Indeed, in the project descriptions, which are available for a subset of states and described in Appendix A.4, the clear majority of school-related projects is concerned with (energy) renovations. Additionally, a small share of projects at schools involve modernizing their ICT installations. Given this, we expect to find employment gains predominantly within the industries concerned with these tasks, with potential additional effects in the local, non-tradable sectors.

This section tests this hypothesis with employment data at the three-digit industry level (German industry classification, 273 industries).[16] We define the construction-related sectors, architects, and industries related to the installation of ICT as *treated* industries.[17] The classification of *non-tradables* follows Mian and Sufi (2014), who categorize industries according to their concentration as measured by a geographical Herfindahl index based on the share of an industry's employment in a county relative to overall employment in that industry.[18] The idea is that non-tradable industries are needed everywhere and are thus geographically dispersed, while tradable industries are geographically concentrated to benefit from specialization. Following this reasoning, the quartile of industries with the lowest index values are classified as *non-tradable* (unless classified as *treated*). As Mian and Sufi (2014),

[16]The industry level data is only available from 2008 onwards due to a major revision of the industry classification in that year.

[17]Appendix A.1 provides the details regarding the composition of the *treated industries*.

[18]For each industry $i$, the index across counties $c$ is defined as follows: $100 \cdot \sum_{c=1}^{N} \frac{empl_{i,c}^2}{\left(\sum_{c=1}^{N} empl_{i,c}\right)^2}$.

we also classify the top quartile as *tradable,* which should be least affected by the stimulus investments. The remaining industries are collected within the residual category *other*.[19]

Table 3 reports the results of estimating the main IV specification with sectoral employment as the dependent variable. For privacy reasons, the sectoral data does not report employment within some county×sector cells. As a consequence, the baseline for the industry partition of the total employment gains displayed in column (1), which is estimated using aggregate industry-level employment as dependent variable, is slightly different from the corresponding column (2) of Table 2.

Columns (2) and (3) show the estimates for employment in the treated and non-tradable industries, where we expect to observe most of the employment gains caused by investments. Indeed, the entire gains in 2009 accrue in the treated industries. In 2010 and 2011, treated and non-tradable industries jointly account for more than 70 percent of the observed increase in employment. The treated industries alone contribute 1.8 job-years per investments of €100'000—precisely estimated with a 90% CI of [0.8, 2.8]—of the total gain of 3.78 job-years. This is in contrast to the tradable industries, where there are no economically or statistically significant employment gains throughout. Within the other industries, the gain of 0.58 jobs in 2011 (90% CI [-0.6, 1.7]) is the only economically sizable, but statistically insignificant estimate.[20]

### 4.4 Additional Results

The online Appendix presents a number of supporting results. Appendices B.2 and B.3 show that there are no detectable geographic spillovers or crowding in or out of funds, respectively. Appendix B.4 complements the industry-level analyses by providing evidence that investments shifted employment towards the treated industries. Appendices B.5 through B.8 report the results of various robustness and sensitivity checks.

## 5 Conclusion

Since the onset of the Great Recession, the effectiveness of fiscal policy in boosting production and employment has received renewed attention from academic economists and policymakers

[19] Note that the *treated* industries are similarly dispersed as the *non-tradables:* the average Herfindahl index within the former group is 0.0231, while in the latter it is 0.0237 (both values are small relative to the overall average of 0.1138), indicating that construction employment is strongly dispersed.

[20] Note that the confidence intervals for the total employment gains in the non-tradable (90% CI [-1.6, 3.5]) and other industries (90% CI [-1.7, 3.2]) are wide, so that we cannot reject both very low or very high gains in these industries. Yet, in combination with the tight confidence bounds in the treated industries, this also shows that the noise in the overall estimates predominantly stems from the industries that were not directly targeted by the program.

Table 3: The Employment Effects of Investments by Industry: IV

| | Aggregate Employment p.c. (1) | Employment p.c. in Treated (2) | Non-tradables (3) | Tradables (4) | Other (5) |
|---|---|---|---|---|---|
| Investments p.c. | | | | | |
| × Q1 2008–Q3 2008 | 0.48 | 0.08 | 0.26 | −0.09 | 0.23 |
| | (0.28) | (0.09) | (0.27) | (0.07) | (0.22) |
| × 2009 | 0.30 | 0.36 | −0.08 | 0.04 | −0.03 |
| | (0.52) | (0.12) | (0.35) | (0.09) | (0.35) |
| × 2010 | 1.35 | 0.58 | 0.39 | 0.15 | 0.22 |
| | (0.61) | (0.19) | (0.53) | (0.18) | (0.57) |
| × 2011 | 2.14 | 0.86 | 0.66 | 0.04 | 0.58 |
| | (0.90) | (0.40) | (0.88) | (0.23) | (0.69) |
| × 2012–2013 | 0.21 | 0.52 | −0.28 | −0.19 | 0.15 |
| | (1.27) | (0.39) | (1.11) | (0.54) | (1.11) |
| County Fixed Effects | yes | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes | yes |
| Date Fixed Effects × | | | | | |
| State × UrbanIndex | yes | yes | yes | yes | yes |
| Emp. Shares by Educ. | yes | yes | yes | yes | yes |
| School Age Population | yes | yes | yes | yes | yes |
| Cumulative Job-Years | 3.78 | 1.80 | 0.97 | 0.24 | 0.77 |
| SE Cumulative Job-Years | 1.79 | 0.63 | 1.55 | 0.48 | 1.50 |
| Observations | 9600 | 9600 | 9600 | 9600 | 9600 |

**Notes.** The dependent variable is aggregated industry-level employment (column (1)), employment in treated industries (column (2)), non-tradable industries (column (3)), tradable industries (column (4) and all remaining industries (column (5)) at each quarterly date between Q1 2008 and Q4 2013, normalized by the working-age population. *Investments p.c. ×2009* is the interaction of investments in €100'000 with an indicator that equals one for the observations in 2009. All the other interactions are defined accordingly; the baseline is Q4 2008. The horizontal lines between the estimates indicate the beginning and the end of the stimulus program. All the remaining variables and statistics are described in Table 2. Standard errors clustered at the county level are in parentheses.

alike. While there is new theoretical and empirical evidence concerning the macroeconomic conditions under which fiscal policy may be effective in general, the evidence regarding which particular types of policies are successful in increasing output and jobs is still scarce. The contribution of this paper is to show that investments in public buildings can quickly and cost-effectively increase employment in the short run, and are therefore a viable tool to counteract an economic slowdown.

An open question is how the effectiveness of public building investments in creating jobs compares with the job creation of other major tools of fiscal policy, like direct transfers to households or tax cuts. Given that job creation is a major policy objective, it is important for policymakers to know which of their tools are most suitable for achieving it. By evaluating the effectiveness of one specific policy, countercyclical investments, this paper takes a first step towards answering this question. More research is needed to inform policy makers about the employment effects of other policy tools at their disposal.

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# Appendix (for online publication)

## A Appendix to Section 3: Data and Identifying Variation

This section provides more details regarding the data and additional results supporting the identification strategy. Section A.1 lists the sources and definitions of all the variables used in this paper (and this appendix). Section A.2 illustrates the geographic distribution of stimulus investments across Germany. Section A.3 describes the common school types of the German education system, and provides additional school statistics.

Section A.4 uses data on the subset of investment projects with project descriptions to show that the number of schools explains school related investments. Section A.5 presents the complete system of first stage equations of the main specification reported in columns (2) and (7) of Table 2. Section A.6 shows that the number of schools is very persistent within Germany.

### A.1 Data Sources and Definitions

Table A.1: Data Sources and Definitions

| Variable | Description | Source |
|---|---|---|
| *Dependent Variables* | | |
| Employment Rate (Tables 2, B.1, B.5, B.6, B.7) | Employees subject to social security contributions in the county of residence normalized by the working-age population. | Federal Employment Agency (*Bundesagentur für Arbeit*) |
| Unemployment Rate (Tables 2, B.6, B.7 | Individuals receiving unemployment benefits in the county of residence normalized by the working-age population. | Federal Employment Agency |
| Employment p.c. in Treated Industries (Table 3) | Employees subject to social security contributions in the county of residence in construction-related industries (industry codes 411-439 (construction), 461, 466, 467, 469, 475 (wholesale & retail with construction material), 711 (architects), 465, 475 (wholesale & retail with ICT) of the German Classification of Economic Activity) normalized by the working-age population. | Employment data at the three-digit industry level requested from the Federal Employment Agency |

| Variable | Description | Source |
|---|---|---|
| Employment p.c. in Non-Tradables (Table 3) | Employees subject to social security contributions in the county of residence in local, non-tradable industries. The non-tradable industries are defined as the bottom quartile of three-digit industries in terms of their geographic Herfindahl index (defined in Footnote 18), unless they are included in the treated industries. | Employment data at the three-digit industry level requested from the Federal Employment Agency |
| Employment p.c. in Tradables (Table 3) | Employees subject to social security contributions in the county of residence in the tradable industries. The tradable industries are defined as the top quartile of three-digit industries in terms of their geographic Herfindahl index (defined in Footnote 18), unless they are included in the treated industries. | Employment data at the three-digit industry level requested from the Federal Employment Agency |
| Employment p.c. in Other Industries (Table 3) | Employees subject to social security contributions in the county of residence in all the industries not included in the "Treated," "Non-Tradable," and "Tradable" Industries normalized by the working-age population. | Employment data at the three-digit industry level requested from the Federal Employment Agency |
| Investment Grants p.c. (Table B.3) | Total investment grants (*Zuweisungen, Zuschüsse für Investitionsförderungen*) from higher layers of government to a county and all of its municipalities (normalized by the working-age population). Yearly data. This data is not available for all the states due to changes in accounting rules. | German Statistical Office (*Destatis*), balance sheet data of counties and municipalities |
| Investment Expenditures p.c. (Table B.3) | Total investment expenditures (*Ausgaben für Sachinvestitionen*) a county and all of its municipalities (normalized by the working-age population). Yearly data. This data is not available for all the states due to changes in accounting rules. | German Statistical Office, balance sheet data of counties and municipalities |

| Variable | Description | Source |
| --- | --- | --- |
| Working-Age Population | The population of working age (between 15 and 65 years of age) in 2008. In our analysis, most variables are normalized by the working-age population (indicated by “p.c.” in the variable name). | German Statistical Office, population statistics |

| Variable | Description | Source |
|---|---|---|
| *Countercyclical Investments and Instruments* | | |
| Investments p.c. in €100'000 (all tables except Table A.2) | The sum of countercyclical investments between 2009 and 2011 within a county and all of its municipalities. We aggregate investments from the project lists using county and municipality identifiers. Projects at the state level (without a county or municipality identifier) are omitted. | Project lists of the Federal Ministry of Finance obtained via personal communication |
| Investments p.c. in €100'000 by spending category (Table A.3) | The sum of countercyclical investments between 2009 and 2011 into schools, universities, hospitals, and all the remaining types of projects. Investments are allocated to project types based on the project descriptions using a textual matching procedure. This is possible for all the states but Saxony-Anhalt, where the project descriptions are not sufficiently detailed. The project descriptions are not reported in the project lists obtained from the federal government described above. For this reason, the exercise in Table A.3 uses project lists obtained from the states. | Project lists of the states obtained from the responsible administrative unit of the states (in most cases the Department of the Treasury or the Department of Commerce) via personal communication |
| Number of School / Other Projects (Table A.3) | The number of investment projects classified as school related projects as well as all the remaining projects (normalized by the working-age population). See above for details. | Project lists of the states (see above) |
| Schools (all tables) | The number of schools within a county measured in 2008 (or 1995 in Table B.5). The official statistics provide the numbers of schools for ten different school types. Based on the size of the school types, these numbers are aggregated into two categories to generate the main instruments *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* See Section A.3 for details. | German Statistical Office, school statistics |

| Variable | Description | Source |
|---|---|---|
| *Control Variables* | | |
| Population Growth (all the tables except Table A.2) | The ratio of the working-age population in any given year and the working-age population in 2008. Yearly data. | German Statistical Office, population statistics |
| Urbanization Index (all the tables except Table A.2) | A four-point urbanization index (*siedlungsstrukturelle Kreistypen*) with the categories metropolitan area (*kreisfreie Großstadt*), city (*städtischer Kreis*), rural county with towns (*ländlicher Kreis mit Verdichtungsansätzen*), little populated rural counties (*dünn besiedelte ländliche Kreise*) | Federal Office for Building and Regional Planning (*Bundesamt für Bauwesen und Raumordnung*) |
| Employment Shares by Education (all the tables except Table A.2 ) | The ratio of employees with a university degree to the total number of employees (also denoted "Empl. Share with College" in Tables 1, B.7,) and the ratio of employees with vocational training to the total number of employees (also denoted "Empl. Share with Vocational Tr." in Tables 1, B.7) as of 2008. The baseline is the share of employees with a lower education than vocational training. | Federal Employment Agency |
| Share School-Age Population (all the tables except Table A.2) | The ratio of the school-age population (between 6 and 18 years of age) to the working-age population as of 2008. | German Statistical Office, population statistics |
| Universities p.c. (all the tables except Tables 3, A.2, A.4, B.1–B.5) | The number of PhD-granting universities with at least 1000 students within a county as of 2015 (download date of the data: February 2015) | University statistics of the German Rectors' Conference (*Hochschulrektorenkonferenz)* |

| Variable | Description | Source |
|---|---|---|
| Hospitals p.c. (all the tables except Tables 3, A.2, A.4, B.1–B.5) | The number of hospitals within a county as of 2008. | German Statistical Office, hospital statistics |
| Short-time work (Table B.5) | The ratio of short-time workers at each quarterly date to the working-age population in 2008. The measure of short-time work is the full-time equivalent (*Beschäftigungsäquivalent*) of short-time workers due to cyclical reasons (*konjunkturelle Kurzarbeit*). | Federal Employment Agency |
| Out-commuter (Table B.5) | The ratio of out-commuters (out of the county) to the working age population as of 2008. | German Statistical Office, employment statistics |
| Population younger than 18 (Table B.5) | The ratio of the population younger than 18 years of age to the working-age population as of 2008. | German Statistical Office, population statistics |
| P25, P50, P75 of wages (Table B.5) | The 25th, 50th, and 75th percentile of the county's monthly gross median wage, averaged over employees, in 2008. . | Wage data requested from the Federal Employment Agency. |
| Bartik shocks (Table B.5) | See Footnote 31 for the formal definition of Bartik shocks $b_{c,t}$. The shock $b_{c,t}$ is normalized by the working-age population in 2008. | Employment data at the three-digit industry level requested from the Federal Employment Agency |

| Variable | Description | Source |
|---|---|---|
| Industry Structure Controls (Table B.5) | A vector of three variables, all as of Q1 2008: the share of employees in agriculture (industry codes 01x–03x), the share of employees in manufacturing (industry codes 05x–39x), and the share of employees in construction (industry codes 41x–43x). The omitted category is the share of employees in services (industry codes 45x–95x). | Employment data at the three-digit industry level requested from the Federal Employment Agency |
| Residential Building Construction (Table B.5) | The number of residential buildings constructed in each year, normalized by the working-age population in 2008 | German Statistical Office, construction statistics |
| 2005 & 2009 Election Outcomes (Table B.5) | The share of votes for the major parties Christian Democrats (*CDU/CSU*), Social Democrats (*SPD*), Greens (*Die Grünen*), Liberals (*FDP*), the Left Party (*Die Linke*) in the general elections of 2005 and 2009, both interacted with date fixed effects | German Statistical Office, election results |
| Age Structure Controls (Table B.5) | The ratio of individuals between 25 and 50 years of age to the working-age population and the ratio of individuals between 50 and and 65 years of age to the working-age population, both as of 2008. Either aggregated or separate by gender. | German Statistical Office, population statistics |
| Area p.c. (Table B.5) | The total area of a county in $km^2$ as of 2008. | German Statistical Office, area statistics |

**Wages in construction** In the introduction and Section 4.1 we compare the cost per job-year to different wages in construction. The wage data have been retrieved from the following sources:

- Minimum wages: German secretary of commerce
- Union wages: Boeckler foundation
- Labor costs: German statistical office (series 62411).

If the data distinguishes between Western and Eastern Germany, we report the wages from West Germany. Hourly wages are translated into yearly wages assuming a 40-hour work week. The data was accessed on November 25, 2016.

**Redistricting** The administrative boundaries of counties changed in three East German states (Saxony-Anhalt in 2007, Saxony in 2008, Mecklenburg-West Pommerania in 2011) during the sample period. These reforms took place in response to the declining rural population in East Germany and mainly merged several former counties to one in order to save administrative costs. We recalculate all the variables from before the administrative reforms to the level of the county boundaries after the reform. All but three former counties are completely merged into new counties, so that the aggregation of these data is straightforward. For the three counties, whose municipalities are assigned to two or three new counties (*Demmin*, county code 13052, in Mecklenburg-West Pommerania, and *Zerbst/Anhalt*, county code 15151, as well as *Aschersleben-Staßfurt,* county code 15352 in Saxony-Anhalt), we disaggregate each statistic based on the relative population shares before the county merger. That is, if the old county A is split to merge into the new counties B and C and if 2/3 of the pre-reform population of county A will be assigned to county B (leaving 1/3 for county C), we reconstruct county B and C before the reform by assigning 2/3 of the value of each statistic (e.g., employment in manufacturing) from county A to the (virtual) county B and 1/3 of the value of each statistic to the (virtual) county C.

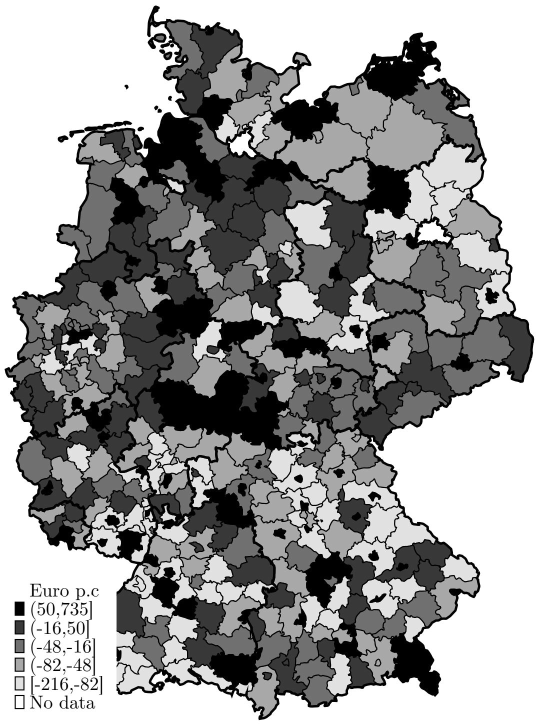
### A.2 The Geographic Distribution of Investments

Figure A.1 plots the geographic variation in investments. Counties are shaded according to their quintile in investments per capita relative to their state-specific means. Figure A.1 shows that, even for the raw data at the state level, there is ample variation in investments across counties without any apparent geographical clustering of regions with large or small investments. As mentioned in the main text, the inter-quintile range of investments is €132 per capita, which is substantial compared with the average investments of €282 per capita across Germany. For the mean county with a working-age population of about 127'000 persons, the inter-quintile range corresponds to sizable differences in total investments of €16.8 million.

### A.3 School Types and Sizes in Germany

There are several types of schools in Germany, both because students typically start specializing in fifth grade and because the school system is organized at the state level, so that

Figure A.1: The Geographic Distribution of Countercyclical Investments




**Notes.** This map shows the geographic distribution of countercyclical investments per capita across counties in Germany. Investments are shown net of their state averages. The shading corresponds to the quintiles in investments; darker shading indicates larger investments.

there is heterogeneity across states. All students attend a primary school *(Grundschule)* first, where children are allocated to schools based on the school district. After primary school, students (and their parents) choose between a number of secondary schools. Two types of secondary schools, *Hauptschule* and *Realschule*, prepare students for vocational training, where the former is more focused on manual work, while the latter is more focused on administrative work. If students intend to go to college, they have to pass A-levels (*Abitur*), for which they need to attend an *Academic High Schools (Gymnasium).* Furthermore, in some states, there are schools that combine *Hauptschule* and *Realschule* (called *Schulen mit mehreren Bildungsgängen* in the school statistics), the first two types of secondary schools, as well as schools that combine all three types of secondary schools (so-called *Comprehensive Schools* or, in German, *Gesamtschulen*). The school statistics also include five minor school types, namely preschools (*Vorschule*), a specific type of middle school (*schulartenunabhängige Orientierungsstufe*), Waldorf schools (*Waldorfschule*, the most prevalent type of private schools), and evening schools (*Abendschule und Kollegs*).

For the empirical analyses, we organize the data on schools as follows. Since some

Table A.2: Summary Statistics: Students per School

| | Students per School (by School Type) | | | |
|---|---|---|---|---|
| | throughout | Percentile of County Avg | | |
| | Germany | P10 | P50 | P90 |
| | Panel A: Primary and Secondary Schools | | | |
| Primary & Secondary Schools | 196 | 139 | 196 | 272 |
| Primary Schools (*Grundschule*) | 180 | 128 | 179 | 237 |
| Secondary Schools - manual work (*Hauptschule*) | 194 | 110 | 199 | 317 |
| Secondary Schools - administrative (*Realschule*) | 404 | 193 | 477 | 710 |
| Secondary Schools - others | 102 | 56 | 110 | 198 |
| | Panel B: Academic High Schools | | | |
| Academic High Schools | 788 | 496 | 834 | 1139 |
| Academic High Schools (*Gymnasium*) | 807 | 518 | 842 | 1154 |
| Comprehensive Schools (*Gesamtschule*) | 697 | 162 | 791 | 1206 |

**Notes.** This table reports the number of students per school by school type. This statistic is reported as the nationwide average given by the ratio of the total number of students and the total number of schools throughout Germany, as well as by its 10th, 50th, and 90th percentile across counties. See the text for a description of the types of schools.

states introduced the *Schulen mit mehreren Bildungsgängen* to combine the non-academic tracks of secondary schools, we add this school type to the number of secondary schools with administrative focus and call the resulting class of schools *Secondary Schools - administrative.* Furthermore, we combine the five minor school types within one category called *Secondary Schools - others*. Finally, as there is a clear dichotomy among all the school types with respect to their size, we aggregate all the school types into two groups: "academic high schools" (the sum of *Comprehensive Schools* and *Academic High Schools,* which both offer A-levels) and *Primary and Secondary Schools* (the remaining school types).

Table A.2 provides statistics on the distribution of school size within the school types. Specifically, it reports the average number of students per school for each major school type throughout Germany, as well as the 10th, 50th, and 90th percentile of the number of students per schools across counties. There is a clear size difference between school types. On the one hand, *Primary Schools, Secondary Schools - manual work,* and *Secondary Schools - others* have, on average, less than 200 students, and have a narrow distribution of averages across counties with the 10th percentile larger than 100 students per school, and the 90th percentile smaller than 320 students per school. *Secondary Schools - administrative* have 404 students on average and are thus slightly larger than the remaining school types within the group of

*Primary and Secondary Schools.* Nevertheless, the 90th percentile of *Secondary Schools - administrative* is smaller than the median number of students per school in *Academic High Schools (Gymnasium)* and *Comprehensive Schools.* These schools are, on average, about four times as large as the average "primary and secondary school."

### A.4 The Number of Schools Predominantly Predicts School Investments

In this section, we show that the number of schools indeed predominantly predicts investments into schools (as opposed to investments that had other purposes). Projects, and, hence, investments, can be linked to their purpose via the project descriptions that states had to communicate to the federal government. These descriptions are missing in the complete list of investment projects obtained from the Federal Ministry of Finance, which is the source of the investment data in the main part of the paper. We were able to obtain project-level data from a second source—the administrative units of the states responsible for the distribution of funds—that includes these descriptions for all the states with the exceptions of Bremen and Saxony-Anhalt. For the states available, these project lists contain 96 percent of the projects and 95 percent of investments. Based on this data, we assign the projects to funding lines (projects related to schools, universities, hospitals, and all the other types of projects) using a textual matching procedure that applies a bag of words algorithm.

Of those projects that can be classified, 48 percent are school related projects. Among the school-related projects, we can classify 46 percent as projects for primary and secondary schools and 13 percent as projects for academic high schools. The project descriptions are not sufficiently detailed to assign the remaining 41 percent of school related projects to particular types of schools.[21] The average value of a school related project is €366'000, where projects related to academic high schools are, on average, almost twice as valuable as projects related to primary and secondary schools (€523'000 vs. €280'000). Similar to the universe of investment projects, school related projects typically have values that do not require a public tender for the allocation of contracts given the temporary change in procurement rules: 43 percent of school related projects have values between €100'000 and €1 million (requiring an invited tender) and 48 percent of school related projects have values below €100'000 (allowing for free contract allocation). We also approximate the total number of projects per school type by scaling the number of projects that we can classify with the respective shares of unclassified projects. Comparing the number of school related projects to the number of schools, this approximation suggests that there were roughly 0.5 projects

[21]For example, we classify projects whose descriptions include the word "gym" as school related projects, as the majority of public gyms belong to schools. However, it is not clear to which type of school a specific gym belongs.

Table A.3: First Stage: Schools Predict School Investments

| | Countercyclical Investments p.c. in € 100’000 | | | | | Projects | |
|---|---|---|---|---|---|---|---|
| | Total (1) | Schools (2) | Universities (3) | Hospitals (4) | Other (5) | Schools (6) | Other (7) |
| Academic High Schools p.c. | 10.09 | 4.54 | 2.86 | 1.59 | 1.95 | 1.08 | 0.17 |
| | (2.98) | (1.75) | (1.81) | (0.72) | (1.25) | (0.43) | (0.39) |
| Primary & Second. Schools p.c. | 1.53 | 1.25 | 0.88 | −0.08 | −0.23 | 0.45 | 0.28 |
| | (0.65) | (0.43) | (0.34) | (0.21) | (0.31) | (0.16) | (0.10) |
| Empl. Share w College /100 | 0.71 | 0.26 | 0.87 | −0.15 | −0.11 | 0.10 | 0.02 |
| | (0.34) | (0.14) | (0.27) | (0.13) | (0.10) | (0.06) | (0.03) |
| Empl. Share w Vocational Tr. /100 | −0.10 | −0.11 | 0.14 | −0.18 | 0.17 | 0.19 | 0.09 |
| | (0.20) | (0.13) | (0.11) | (0.08) | (0.08) | (0.05) | (0.02) |
| Share School-Age Pop /100 | 0.01 | 0.50 | −0.46 | −0.12 | −0.06 | −0.04 | −0.03 |
| | (0.42) | (0.23) | (0.30) | (0.12) | (0.20) | (0.09) | (0.05) |
| Universities p.c. | 85.19 | 8.72 | 77.99 | −6.72 | 3.11 | −0.55 | −1.13 |
| | (21.92) | (9.67) | (16.93) | (5.05) | (7.98) | (2.91) | (2.67) |
| Hospitals p.c. | 4.35 | 0.19 | 0.21 | 0.77 | 1.78 | 0.37 | −0.06 |
| | (2.95) | (1.22) | (1.27) | (0.81) | (2.57) | (0.40) | (0.29) |
| State × UrbanIndex FE | yes | yes | yes | yes | yes | yes | yes |
| Kleibergen–Paap F | 9.77 | 6.99 | 6.20 | 2.46 | 1.82 | 7.82 | 4.21 |
| Shea Partial $R^2$ | 0.07 | 0.09 | 0.04 | 0.01 | 0.01 | 0.05 | 0.03 |
| Observations | 400 | 384 | 384 | 384 | 384 | 384 | 384 |

**Notes.** The dependent variable in column (1), *Countercyclical Investments p.c. in €100’000 – Total,* is the sum of investments normalized by the working-age population (indicated by “p.c.” for “per capita”) over the years 2009 to 2011 using the investment data from the main part of the paper (column (1) is identical to column (4) of Table 1). The remaining columns use a subset of the data that entails the project descriptions to classify investment projects according to their purpose. The dependent variables in columns (2) to (5) are the sum of investments in schools, universities, hospitals, and the sum of investments for all the remaining purposes. The dependent variable in column (6) is the number of projects categorized as school related, and the dependent variable in column (7) is the number of all the other projects. The remaining variables and statistics are defined as in Table 1. *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* are the excluded instruments for the Kleibergen–Paap F statistic and the Shea Partial $R^2$. The sample is the cross-section of counties as measured in Q4 2008; in columns (2) to (7) the counties within the states Bremen and Saxony-Anhalt drop, as the project descriptions are unavailable for these states. Robust standard errors are in parentheses.

per elementary and secondary school and more than one project for each academic high school.

Columns (1) to (5) of Table A.3 present the results of regressing the subsets of investments within different funding lines on the instruments as well as the most extensive set of covariates. Apart from the varying dependent variables, we use the same empirical specification as the one underlying column (3) of Table 1, which we reproduce in column (1) of Table A.3 for comparison. The results show that the number of schools per capita is strongly correlated with investments in schools. Also, the number of universities is strongly correlated with investments in universities. Only for hospital investments, the coefficient of the number of hospitals is not statistically significantly different from zero. Also, the significant coefficients of *Primary and Secondary Schools p.c.,* when the dependent variable is investments in universities, and of *Academic High Schools p.c.,* when the dependent variable is investments in hospitals, are not as expected. However, these results may be due to the necessarily imperfect classification procedure based on textual analysis.

In Columns (6) and (7), the dependent variables are the number of school related investment projects and the number of all the other investment projects, respectively. *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* are strongly correlated with the number of school projects and much less so with the number of other projects. Specifically, there are, on average, more than twice as many projects associated with one *Academic High School* as with one *Primary and Secondary School.* This finding may contribute to explain why the average total investments per *Academic High School* are six to seven times as large as total investments per *Primary and Secondary School* in column (1).[22]

### A.5 The Complete System of First Stage Equations

Table A.4 reports the estimates of the complete system of first stage equations as described by Equation (2). More specifically, Table A.4 presents the first stage estimates of the baseline empirical results in columns (2) and (5) of Table 2.[23] As such, the coefficients of the interactions of *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* along the diagonal can be compared to the coefficients of the purely cross-sectional first stage co-

[22] Another share of this difference in total investments per school may be explained by the different sizes of academic high schools and primary and secondary schools pointed out in Appendix A.3.

[23] In Table 2, we reduce the number of coefficients by interacting investments with indicator variables that equal one for all dates prior to the investment program (Q1 2007 to Q3 2008) and all dates after the end of the program (Q1 2011 to Q4 2013), respectively. For the years of the program (2009–2011), we estimated one coefficient for each year. Footnote 10 gives the formal statement of the relevant second stage. Table A.4 applies the same procedure to the instruments of investments, *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* All the remaining variables are interacted with dummy variables for each quarterly date exactly as described by the models (1) and (2).

Table A.4: The Complete System of First Stage Equations

| | Countercyclical Investments p.c. in € 100'000 × | | | | |
|---|---|---|---|---|---|
| | 2007–Q3 2008 | 2009 | 2010 | 2011 | 2012–2013 |
| | (1) | (2) | (3) | (4) | (5) |
| Academic High Schools p.c. | | | | | |
| × 2007–Q3 2008 | 11.82 | 0.00 | −0.00 | −0.00 | −0.01 |
| | (2.74) | (0.01) | (0.00) | (0.01) | (0.03) |
| × 2009 | −0.01 | 11.80 | 0.00 | 0.00 | 0.01 |
| | (0.05) | (2.73) | (0.01) | (0.02) | (0.05) |
| × 2010 | 0.00 | 0.00 | 11.81 | −0.00 | −0.00 |
| | (0.07) | (0.02) | (2.73) | (0.02) | (0.07) |
| × 2011 | 0.01 | 0.00 | −0.00 | 11.80 | −0.01 |
| | (0.10) | (0.03) | (0.01) | (2.73) | (0.10) |
| × 2012–2013 | 0.20 | 0.06 | −0.02 | −0.07 | 11.62 |
| | (0.23) | (0.07) | (0.04) | (0.08) | (2.75) |
| Primary & Secondary Schools p.c. | | | | | |
| × 2007–Q3 2008 | 2.28 | −0.00 | 0.00 | 0.00 | 0.00 |
| | (0.70) | (0.00) | (0.00) | (0.00) | (0.01) |
| × 2009 | 0.00 | 2.28 | −0.00 | −0.00 | −0.00 |
| | (0.01) | (0.70) | (0.00) | (0.00) | (0.01) |
| × 2010 | 0.01 | 0.00 | 2.28 | −0.00 | −0.01 |
| | (0.02) | (0.01) | (0.70) | (0.01) | (0.02) |
| × 2011 | 0.01 | 0.00 | −0.00 | 2.28 | −0.01 |
| | (0.03) | (0.01) | (0.00) | (0.70) | (0.03) |
| × 2012–2013 | 0.05 | 0.01 | −0.01 | −0.02 | 2.24 |
| | (0.08) | (0.02) | (0.01) | (0.02) | (0.69) |
| County Fixed Effects | yes | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes | yes |
| Date Fixed Effects × | | | | | |
| State × UrbanIndex | yes | yes | yes | yes | yes |
| Emp. Shares by Educ. | yes | yes | yes | yes | yes |
| School Age Population | yes | yes | yes | yes | yes |
| Kleibergen–Paap F | 3.59 | 3.59 | 3.59 | 3.59 | 3.59 |
| Sanderson-Windmeijer F | 8.50 | 8.94 | 8.70 | 8.80 | 8.61 |
| Shea Partial $R^2$ | 0.15 | 0.15 | 0.15 | 0.15 | 0.15 |
| Observations | 11200 | 11200 | 11200 | 11200 | 11200 |

**Notes.** This table presents the first stage equations of column (2) and (5) of Table 2. The dependent variable in column (1) is the sum of investments, normalized by the working-age population, interacted with an indicator that equals one for the observations between Q1 2007 and Q3 2008. All the other dependent variables and interactions are defined accordingly. *Academic High Schools p.c.* is the number of high schools in a county which award the "Abitur." *Primary and Secondary Schools p.c.* is the number of primary schools and secondary schools. The remaining variables and statistics are described in Table 2. Standard errors clustered at the county level are in parentheses.

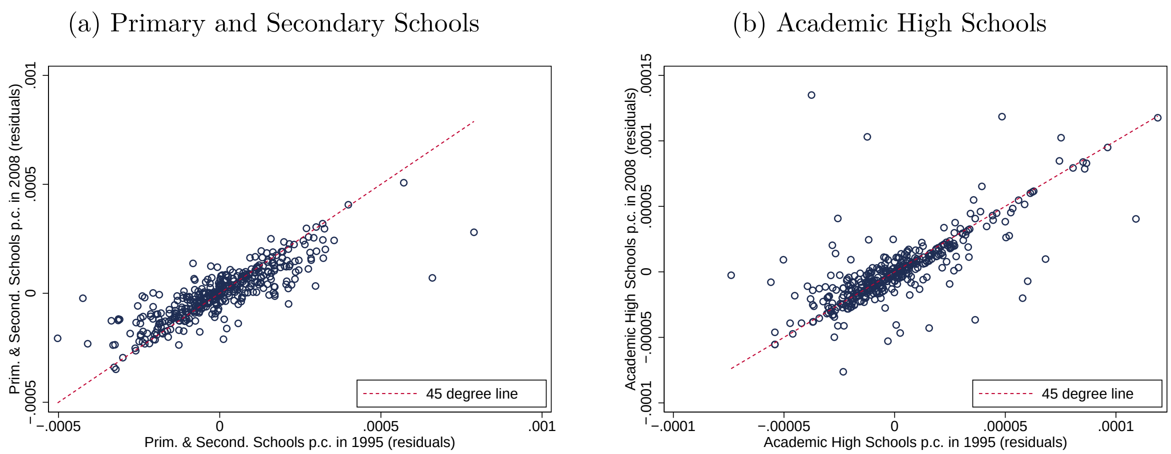
efficients in column (2) of Table 1. Both the coefficients and standard errors of the system of first stage equations are very close to the ones for the single cross-section, in particular for the time periods close to Q4 2008, the date of the cross-section used for the estimations in Table 1. Moreover, the Shea Partial $R^2$ of the first stage equations in Table A.4 and the cross-section are equal. These results are as expected, given that each of the first stage equations in Table A.4 is, by design, identified almost exclusively from cross-sectional variation (only *Population Growth* varies over time, and all the remaining covariates are interacted with the full set of date dummies).

Testing for weak instruments in a setting with many endogenous variables and many instruments is at the frontier of research in theoretical econometrics. Table A.4 presents, for each first stage equation, the F-statistic proposed by Sanderson and Windmeijer (2016). Their test for weak instruments (sketched by Angrist and Pischke, 2009) is based on the application of the Frisch-Waugh-Lovell theorem to each first stage. In a first step, the testing procedure partials out, for one first stage equation the remaining endogenous variables (instrumented by the complete set of instruments) as well as all the exogenous covariates. In a second step, the resulting residuals are regressed on the instruments, and an F-test on the coefficients of the instruments is performed. This is done to assess whether the remaining explanatory power of the instruments is sufficient to identify the first stage equation under consideration. Applied to each first stage equation, the F-statistic proposed by Sanderson and Windmeijer hence allows to evaluate the relevance of the instruments for each endogenous variable separately.

The results in Table A.4 show that the instruments are equally informative for each investment-period interaction. But the F-statistics are below the commonly chosen critical value of ten, potentially indicating that the instruments are weak. However, the Sanderson–Windmeijer F-statistic, which equals the Kleibergen–Paap F-statistic in the single endogenous regressor case displayed in Table 1, only drops because each additional interaction of $\boldsymbol{Schools_c}$ is informative for only one endogenous variable and uninformative for all remaining endogenous variables (as illustrated by the statistically insignificant coefficients off the diagonal in Table A.4). It is hence questionable whether the F-statistic is a good diagnostic for detecting weak instruments in the specific empirical model estimated here (see Angrist and Pischke, 2009, p. 215, for a similar point).

We conduct two further exercise to assess whether the estimates of the dynamic model described by (1) and (2) are potentially biased due to weak instruments. In Appendix B.8, we transform equation (1) to a cross-sectional model that allows us to estimate the job-years created by investments using only one endogenous variable and two instruments. This standard IV setup delivers first stage F-statistics at the same level of the ones reported

Figure A.2: The Autocorrelation of Schools between 1995 and 2008




**Notes.** This figure displays, for each county, the number of schools (net of their state-specific averages and separately for academic high schools and primary and secondary schools) in 2008 against the number of schools in 1995.

in Table 1, and the estimated job-years / costs per job-year, as well as the corresponding standard errors, are very close to the estimates from the main specification reported in Table 2. These results reinforce the notion that the cross-sectional tests for weak instruments are appropriate to evaluate the relevance of the instruments in a specification like ours, in which the first stage is primarily identified from cross-sectional variation. We also demonstrate in Appendix B.7 that the employment dynamics (and their precision) are unchanged when they are estimated via a repeated cross-section, for which the first stages correspond to the cross-sectional first stage in Table 1.

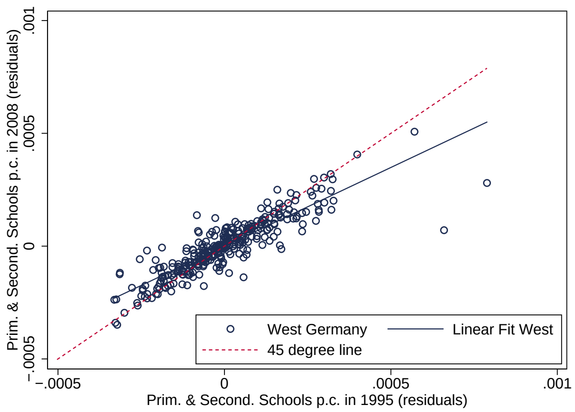
## A.6 The Persistence of the Number of Schools

This section further elaborates on the persistence of the number of schools highlighted in Section 3.2. Figure A.2 illustrates this persistence by plotting the number of schools in 2008 against the number of schools in 1995 (the earliest date at which this data is available). In both years, schools are measured relative to their state averages. For both *Academic High Schools* and *Primary and Secondary Schools*, the data is tightly clustered around the 45-degree line. This demonstrates that there are, indeed, at best minor changes in the number of schools over time.

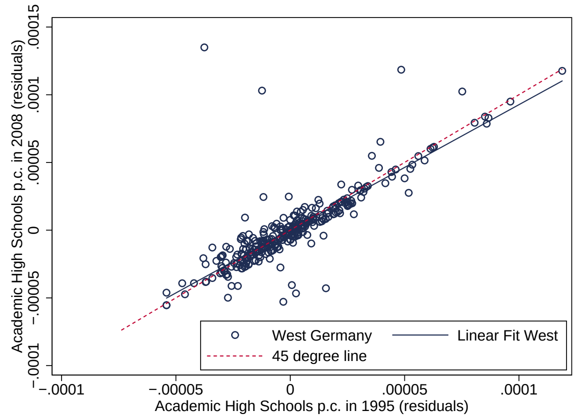
Figure A.3 plots the number of schools in 2008 against the number of schools in 1995, separately for counties in the former West German states (in Panels (a) and (b)) and in the former East German states (in Panels (c) and (d)). As above, schools are measured

Figure A.3: The Autocorrelation of Schools between 1995 and 2008 in West and East Germany

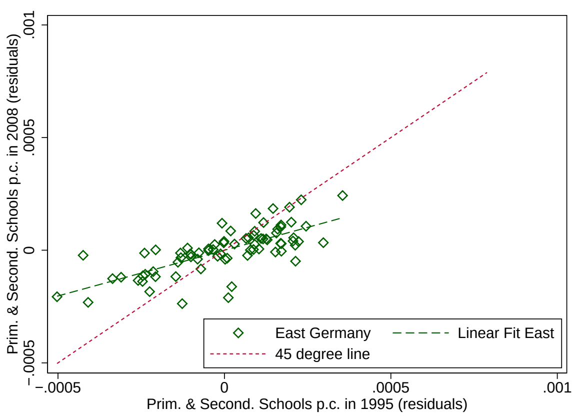
(a) Primary and Secondary Schools (West)



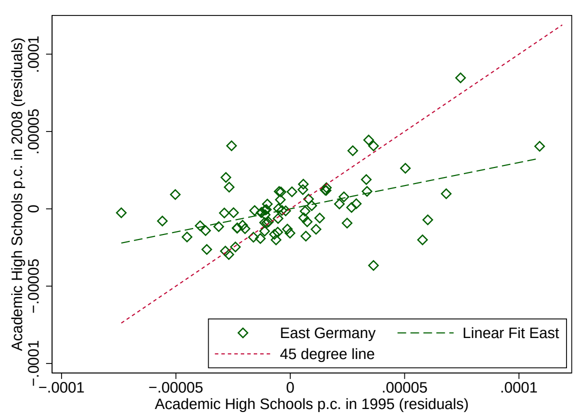

(b) Academic High Schools (West)




(c) Primary and Secondary Schools (East)




(d) Academic High Schools (East)




**Notes.** This figure displays, for each county, the number of schools (net of their state averages and separately for academic high schools and primary and secondary schools) in 2008 against the number of schools in 1995. Panels (a) and (b) show the observations in the former West German states as well as their linear fit, and Panels (c) and (d) show the observations from the East German states.

relative to their state averages. Clearly, the number of schools in the West is more persistent than in the East. This is particularly true for academic high schools, the stronger of the two instruments, where the observations in the West German counties are tightly clustered around the 45 degree line, indicating a high persistence. In the East German counties, in contrast, the best linear fit of the observations is close to a horizontal line indicating a low persistence. This result may be due to the significant restructuring of the administration in the East German states in the wake of reunification. The low persistence of the number of schools in the East German states may be the reason for the weak first stage when using the data from 1995 as an instrument (as revealed by the low Shea $R^2$ in row (11) of Table B.5), further amplifying the lack of statistical power for the sample of the 76 East German counties.

# B Appendix to Section 4: Results

This appendix presents a number of supporting results for the main analysis. Section B.1 complements Figure (1) in the introduction by displaying the full employment dynamics for the remaining IV specifications of Table 2. Section B.2 provides evidence that there are no substantial geographical spillovers. Section B.3 verifies that there is no need to scale the implied multipliers due to crowding in or crowding out of funds. Section B.4 complements the industry-level analyses by providing evidence that investments shifted employment towards the treated industries.

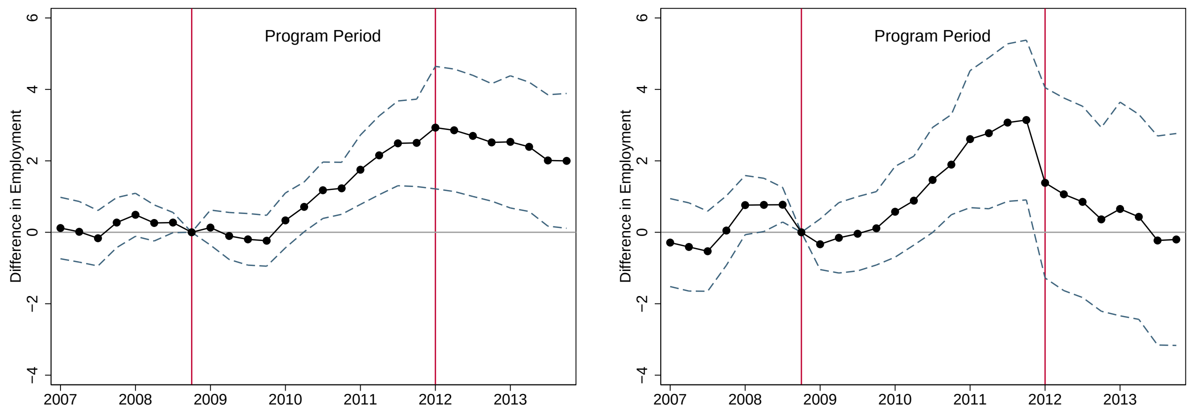
Section B.5 shows that the main results in Table 2 continue to hold for a wide range of robustness and plausibility checks. Section B.6 shows that the estimated employment and unemployment effects do not change significantly when they are estimated relative to average employment or average unemployment between 2007 and 2008 instead of relative to Q4 2008. Section B.7 estimates the employment dynamics caused by the countercyclical investments via a repeated cross-section, resulting in pictures hardly distinguishable from the main result depicted in Figure 1 in the introduction and Figure B.1 in this appendix. Section B.8 demonstrates that collapsing the empirical model (1) to a cross-sectional specification yields nearly the same estimates of job-years or reductions in unemployment years as the dynamic models in Table 2.

## B.1 Employment Dynamics of all IV Specifications

Figure 1 in the introduction displays the employment dynamics corresponding to the empirical specification in column (2) of Table 2. Panels (a) and (b) of Figure B.1 display the employment dynamics corresponding to the empirical specifications in columns (1) and (3)

Figure B.1: Employment Dynamics

(a) Parsimonious Specification (b) Demanding Specification




**Notes.** This figure shows the differences in employment per €100'000 invested, $\beta_t$, at each quarterly date $t$ between Q1 2007 and Q4 2013 relative to Q4 2008, as well as their 90 percent confidence interval as estimated via IV. The empirical model in Panel (a) includes the most parsimonious set of covariates, identical to the one used in column (1) of Table 2. The model in Panel (b) includes the most comprehensive set of covariates, identical to the one used in column (3) of Table 2. The left vertical line indicates the last date before the investment program was passed into law; the right line indicates the first date after the end of the program.

of Table 2. As before, both figures plot the IV coefficients of investments, $\{\beta_t\}_{t:t\neq \text{Q4 2008}}$ estimated via the empirical model described by (1) and (2) (with the same covariates as in the corresponding columns of Table 2), along with their 90 percent confidence interval.

In both specifications, the instrumented (placebo) investments yielded neither employment gains or losses before the passage of the stimulus bill in Q1 2009. As in Figure 1, moreover, employment starts to increase with a lag of three to four quarters after the passage of the bill, until it peaks in 2011. After the end of the program in 2011, the employment gains estimated by the parsimonious specification in Panel (a) are more persistent than in the main specification. In the most demanding specification in Panel (b), the employment gains fall just as sharply in Q1 2012 as in Figure 1.

## B.2 Geographical Spillovers

A plausible concern regarding our findings is that the employment effects may be over- or underestimated due to geographical spillovers. For example, the estimated effects would be too large if investments in one county increased the local wages and thus reduced the employment in other counties within the same region. In contrast, the estimated employment effects would be too small if there were sizable demand spillovers across counties so that an increase in the labor demand within one county boosts employment in adjacent counties as well.

To test whether there are geographical spillovers of economically significant size, we first follow the approach of Acconcia et al. (2014) and add investments in neighboring counties as an additional variable to the main empirical specification. For each county, we consider three possible definitions of neighboring counties: all other counties within the same labor market region (*Raumordnungsregion*), the five closest counties based on the distance between the most populous municipalities of the counties, and the ten closest counties. For each set of a county's neighbors, we calculate investment spillovers as the total investments within the set of neighboring counties, normalized by the county's working-age population. These investment spillovers are instrumented by the aggregate number of schools within the set of neighboring counties (normalized by the county's working-age population).

Table B.1 reports the IV estimates of the investment-induced employment gains that include potential investment spillovers. The effect of investments in neighboring counties on a county's employment is negative in general and more than one order of magnitude smaller than the direct employment effects. This suggests that the investment program did not lead to major geographic shifts in economic activities across nearby counties.

The tests for geographical spillovers in Table B.1 predominantly account for spillovers by distance, and focus less on economic interdependence between counties. To check whether this could be the reason for finding no discernible geographical spillovers, we next implement the method of Dupor and McCrory (2018). They redefine the unit of observation in a way that confines spillovers to commuting regions.

Specifically, this approach groups counties into larger geographical and economically intertwined regions and then splits each into a core and a satellite subregion. The county with the largest population within the geographical region is defined to be the core subregion. The remaining group of counties constitutes the satellite subregion. For each subregion, we aggregate and normalize all county level variables of the main specification, resulting in a data set of core and satellite subregions as just defined. To estimate geographical spillovers, we test whether stimulus investments in the satellite subregion lead to employment gains or losses in the core subregion and vice versa.

We implement this method for two different definitions of geographical regions in Germany. First, we define the so-called narrow labor market regions (*Arbeitsmarktregionen*) as geographical regions. The defining characteristic of narrow labor market regions is that more than 65% of all workers do not commute out of this region. Second, we define regions according to the (broader) labor market regions (*Raumordnungsregion*), whose defining characteristic is that more than 85% of all worker do not commute out of this region. Both types of regions are defined by the German Federal Office for Building and Regional Planning.[24]

[24] In order to still be able to control for state $\times$ date fixed effects, we only consider regions that are located

Table B.1: The Employment Effects of Investments with Geographical Spillovers

| | Employment Rate | | | |
|---|---|---|---|---|
| Set of Neighboring Counties: | Baseline | Labor Market | 5 Closest | 10 Closest |
| | (1) | (2) | (3) | (4) |
| Investments p.c. | | | | |
| × 2007–Q3 2008 | 0.39 | 0.33 | 0.33 | 0.36 |
| | (0.37) | (0.36) | (0.41) | (0.38) |
| × 2009 | 0.10 | 0.41 | 0.34 | 0.34 |
| | (0.42) | (0.50) | (0.48) | (0.50) |
| × 2010 | 1.55 | 2.10 | 2.05 | 1.98 |
| | (0.61) | (0.70) | (0.72) | (0.71) |
| × 2011 | 2.54 | 3.43 | 3.00 | 2.95 |
| | (0.89) | (0.95) | (1.02) | (0.99) |
| × 2012–2013 | 0.52 | 1.68 | 0.89 | 1.17 |
| | (1.26) | (1.23) | (1.30) | (1.24) |
| Investments in Neighboring Counties p.c. | | | | |
| × 2007–Q3 2008 | | 0.01 | 0.01 | 0.01 |
| | | (0.01) | (0.02) | (0.01) |
| × 2009 | | −0.03 | −0.04 | −0.03 |
| | | (0.02) | (0.02) | (0.01) |
| × 2010 | | −0.03 | −0.06 | −0.03 |
| | | (0.03) | (0.04) | (0.02) |
| × 2011 | | 0.00 | −0.03 | −0.00 |
| | | (0.04) | (0.05) | (0.02) |
| × 2012–2013 | | 0.01 | −0.01 | −0.01 |
| | | (0.06) | (0.07) | (0.03) |
| County Fixed Effects | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes |
| Date Fixed Effects × | | | | |
| State × UrbanIndex | yes | yes | yes | yes |
| Emp. Shares by Educ. | yes | yes | yes | yes |
| School Age Population | yes | yes | yes | yes |
| min(Shea Partial $R^2$) | 0.11 | 0.13 | 0.10 | 0.11 |
| Cumulative Job-Years | 4.19 | 5.94 | 5.39 | 5.28 |
| SE Cumulative Job-Years | 1.74 | 1.97 | 2.01 | 1.97 |
| Costs per Job-Year | 23862 | 16834 | 18554 | 18939 |
| SE Costs per Job-Year | 9880 | 5588 | 6913 | 7060 |
| Observations | 11200 | 11200 | 11200 | 11200 |

**Notes.** *Investments in Neighboring Counties p.c. × 2007–Q3 2008* is the interaction of aggregate investments (in €100'000 and normalized by the working-age population) across all other counties in the same labor market region (column (2)), the 5 closest counties (column (3)), or the 10 closest counties (column (4)) interacted with an indicator for the dates Q1 2007–Q3 2008. All the other interactions are defined accordingly. The remaining variables and statistics are described in Table 2. Standard errors are clustered at the level of the 94 labor market regions.

Table B.2: Geographical Spillovers within Regions

| | Employment Rate | | | |
|---|---|---|---|---|
| | Narrow labor market *Arbeitsmarktregionen* | | Broad labor market *Raumordnungsregionen* | |
| | (1) | (2) | (3) | (4) |
| Investments p.c. | | | | |
| × 2007–Q3 2008 | 0.56 | 0.39 | 1.17 | 1.36 |
| | (0.54) | (0.55) | (0.55) | (0.56) |
| × 2009 | 0.55 | 0.79 | 0.00 | 0.07 |
| | (0.52) | (0.57) | (0.82) | (0.80) |
| × 2010 | 1.83 | 1.99 | 1.85 | 1.67 |
| | (0.77) | (0.77) | (1.48) | (1.49) |
| × 2011 | 1.36 | 1.61 | 2.64 | 3.29 |
| | (1.06) | (1.07) | (1.89) | (1.94) |
| × 2012–2013 | −1.43 | −1.17 | 2.09 | 4.60 |
| | (1.89) | (2.03) | (2.46) | (2.57) |
| Investments in Adjacent Region p.c. | | | | |
| × 2007–Q3 2008 | | 0.07 | | 0.03 |
| | | (0.10) | | (0.08) |
| × 2009 | | −0.16 | | −0.06 |
| | | (0.13) | | (0.09) |
| × 2010 | | −0.04 | | 0.25 |
| | | (0.18) | | (0.35) |
| × 2011 | | 0.07 | | 0.35 |
| | | (0.24) | | (0.39) |
| × 2012–2013 | | 0.09 | | 0.54 |
| | | (0.38) | | (0.45) |
| Subregion Fixed Effects | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes |
| Date Fixed Effects × | | | | |
| State × Core Region | yes | yes | yes | yes |
| Emp. Shares by Educ. | yes | yes | yes | yes |
| School Age Population | yes | yes | yes | yes |
| min(Shea Partial $R^2$) | 0.12 | 0.12 | 0.11 | 0.12 |
| Cumulative Job-Years | 3.74 | 4.39 | 4.50 | 5.02 |
| SE Cumulative Job-Years | 2.12 | 2.13 | 3.73 | 3.76 |
| Costs per Job-Year | 26772 | 22782 | 22238 | 19910 |
| SE Costs per Job-Year | 15200 | 11066 | 18430 | 14922 |
| Observations | 4648 | 4648 | 5096 | 5096 |

**Notes.** The sample consists of the core and satellite subregions at the level of the narrow or broad labor market region, as described in the text. For core subregion observations, *Investments in Adjacent Region p.c.* are the aggregate investments (in €100'000 and normalized by the working-age population) within the corresponding satellite subregion and vice versa. *Investments in adjacent regions* are instrumented with the adjacent subregion aggregates of *Academic Highschools p.c.* and *Primary and Secondary Schools p.c.* The remaining variables and statistics are described in Table 2. Standard errors are clustered at the level of the respective labor market region.

Columns (1) and (3) of Table B.2 show the IV results for the main specification and the different (sub-)regions. These baseline effects can also be interpreted as additional robustness check—the sample composition changes as single-county regions are dropped[25]—and a first check for spillovers, as the county aggregates of the satellite subregions should contain all spillovers within the aggregated counties.[26] The results for the subregion samples show that the main results are robust and that there is no evidence from spillovers via aggregation: During the treatment period, all coefficients are statistically indistinguishable from the baseline coefficient (the largest difference of 1.48 for the 2011 interaction in column (1) has a standard error of 1.38[27]).

Columns (2) and (4) include total investments in the adjacent (core or satellite) subregion within the geographical area as an explanatory variable. Similar to the results using the method of Acconcia et al. (2014) in Table B.1, spending in adjacent subregions does not appear to have any detectable spillovers on a subregion's employment. While the coefficients of investments in the adjacent region are positive in column (4) (0.25 with 90% CI [-0.3, 0.8] and 0.35 with CI [-0.3, 0.8]), none of the coefficients are statistically distinguishable from zero.

These results are in contrast to the findings of Dupor and McCrory (2018), who find strong regional spillovers for funding provided by the American Recovery and Reinvestment Act (ARRA). There are at least two plausible reasons for this difference in findings. First, the German stimulus bill was accompanied by a loosening of public procurement rules to allow for the quick implementation of projects. According to the German Court of Auditors, this led to a substantial increase in the share of contracts awarded to local firms for the projects financed by the program (Bundesrechnungshof, 2012). Second, the German stimulus bill studied here explicitly focused on boosting regional economies via numerous projects of comparably small scale. Given that the treated industries appear to be widely dispersed across counties in Germany (see Footnote 19 in the main text), it is likely that the additional demand could be met locally. In contrast, the ARRA funding studied by Dupor and McCrory (2018) comes from nine diverse funding lines, of which only two are primarily dedicated to construction activities. It is hence not clear whether the additional demand generated by these policies could have been met by local firms.

within one state.

[25]The sample defined by the narrow labor market regions drops 175 counties, and the sample for the broad labor market regions drops 5 counties.

[26]There are 10 (of 166) subregions with more than county within the sample defined by the narrow labor market regions, and 56 (of 182) subregions with more than one county for the sample defined by broad labor market regions.

[27]The calculation of the standard error assumes that the covariance of the parameter estimates for 2011 in column (2) of Table 2 and column (1) of Table B.2 equals zero.

### B.3 Crowding In or Out of Countercyclical Investments

A common concern regarding the use of public investments as job creation programs is that federal investment grants crowd out investments of local layers of government. On the other hand, federal investment programs may crowd in local spending if counties contribute more than the required co-financing, for example, to increase the project quality. In either case, a significant degree of crowding in or out alters the total amount spent and requires adjusting the calculations of the multiplier.

In the following, we check whether the stimulus investments led to crowding in or out of other types of investments at the county and municipality level. To this end, we combine our main spending variable—project-level data on countercyclical investments—with data on general investment grants and expenditures from the balance sheets of counties and municipalities. Specifically, the variables of interest in this section are defined as follows:

- The spending variable from the main text, *(Stimulus) Investments*, measures the total stimulus investments at the county level from project-level data. This is the total amount spent on stimulus projects, irrespective of how the funds were budgeted. Specifically, for some projects, the funds may have been directly drawn from the budget of the federal state and may not show up on the budgets of regional layers of government (counties or municipalities) at all. Alternatively, the regional governments may have received investment grants from higher levels of government or may have co-financed parts of the project cost from their budgets.

- The variable *Investment Expenditures* measures the total investment expenditures of regional governments at the county and municipality level. *Stimulus Investments* may have been part of these expenditures, but only if the projects were budgeted at the county level. As noted above, this is not necessarily the case.

- The variable *Investment Grants* measures the total investment grants received by local governments at the county and municipality level. For the stimulus projects budgeted at the county level, these figures include the share of federal and state funds used to cover the project costs.

In short, *Stimulus Investments* measures the total value of all projects that have been at least partly financed by federal stimulus grants but are not necessarily included in the budgets of the local governments at the county and municipality level. *Investment Expenditures* and *Investment Grants* are budget items of the local governments but consist only in part of the stimulus funds studied here.

Given this, *Stimulus Investments* for projects administered at the county level raise both the balance sheet values of *Investment Expenditures* and *Investment Grants*. We observe crowding out if local *Investment Expenditures* increase less in response to *Stimulus Investments* than the *Investment Grants* received from higher levels of government. Conversely, there is crowding in if the increase in *Investment Expenditures* exceeds the increase in *Investment Grants* by more than the required co-financing of local governments.

Hence, to check for crowding out, we regress the difference of *Investment Expenditures* and *Investment Grants* on *(Stimulus) Investments* according to the following model:

$$Inv.\,Expenditures\,p.c._{c,y} - Inv.Grants\,p.c_{c,y} = \sum_{y:y\neq 2008} \eta_y\, Investments\,p.c._c \times \mathbb{1}\,(year = y) + \mathbf{Controls_{c,y}} + CountyFE_c + \epsilon_{c,y}, \quad \text{(B.1)}$$

where the index $y$ refers to years—the balance sheet data is published at yearly frequency—, and where $\epsilon_{c,y}$ is the error term. Negative values of $\eta_y$ are indicative of crowding out, while positive values of $\eta_y$ that are larger than the required co-financing indicate crowding in.

We estimate two variants of (B.1) via OLS and IV (with *Investments p.c.* instrumented by the number of schools). The first variant includes no control variables except for the county fixed effects. It thus estimates the unconditional association between stimulus investments and the difference of expenditures and grants at the county level. This estimate is informative on how the multiplier should be scaled from an ex ante perspective, that is, how *Investments* should be adjusted for crowding in or out before estimation of the main empirical model (1). The second variant includes the same control vector as the main specification of the empirical model in columns (2) and (5) of Table 2. The conditional association estimated from this variant is informative on how much crowding in or out there is for the identifying variation in *Investments.*

Table B.3 summarizes the results. The unconditional effects in columns (1) and (3) show that, between 2009 and 2011, the yearly *Investment Expenditures* exceeded the yearly *Investment Grants* by €0.059 and €0.054 per Euro of *Investments* (with 90% CIs of [0.029, 0.089] and [0.028, 0.080], respectively). During the three years of the program period, these point estimates imply that counties and municipalities spent between €0.162 and €0.177 Cents per Euro invested from their budgets. The required co-financing of the state and regional governments was €0.25 for every Euro invested. Hence, the additional spending implied by the unconditional estimates is only slightly larger than the required co-financing of €0.125 per Euro invested that would apply if the co-financing was equally shared between the state and local governments.

Table B.3: Crowding In or Out

| | Investment Expenditures p.c.$_{c,y}$ – Investment Grants p.c.$_{c,y}$ | | | |
|---|---|---|---|---|
| | IV | | OLS | |
| | (1) | (2) | (3) | (4) |
| Investments p.c. × 2007 | −0.101 | −0.325 | −0.089 | 0.029 |
| | (0.021) | (0.227) | (0.018) | (0.052) |
| Investments p.c. × 2009–2011 | 0.059 | −0.127 | 0.054 | −0.004 |
| | (0.018) | (0.143) | (0.016) | (0.050) |
| Investments p.c. × 2012–2013 | 0.036 | −0.369 | 0.044 | −0.001 |
| | (0.025) | (0.205) | (0.025) | (0.075) |
| County Fixed Effects | yes | yes | yes | yes |
| Population Growth | no | yes | no | yes |
| State × UrbanIndex | no | yes | no | yes |
| Emp. Shares by Educ. | no | yes | no | yes |
| School Age Population | no | yes | no | yes |
| Observations | 2000 | 2000 | 2000 | 2000 |

**Notes.** The dependent variable is the difference between *Investment Expenditures* and *Investment Grants* in year $y$, normalized by the working-age population. *Investments p.c.* $\times$*2007* is the interaction of investments in €100'000 with an indicator that equals one for the observations in 2007. All the other interactions are defined accordingly. The baseline is 2008. In columns (1) and (2), *Investments* is instrumented with *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* All the remaining variables are described in Table 2. The sample consists of year × county cells within all states that report the county- and municipality-level balance sheets at least up to 2011. Standard errors clustered at the county level are in parentheses.

The conditional effects for the years 2009-2011 in columns (2) and (4) are negative, imprecise, and statistically indistinguishable from zero with 90% confidence intervals of [-0.363, 0.109] for the IV and [-0.086, 0.078] for the OLS results. At face value, these estimates suggest that the investment grants received from higher levels of government indeed led to some degree of crowding out, implying potentially even larger multipliers than the ones calculated in Section 4. Note, however, that the OLS results are very close to zero, and the IV estimates are not well centered: The IV estimates for the years before and after the program period are below -0.3, implying that the coefficient for *Investments* × *2009-2011* would be (slightly) positive when estimated relative to 2007 and 2008 combined (instead of relative to 2008 only as in Table B.3).

Overall, the evidence thus provides no clear indication for either crowding in or crowding out, so that there is no need to adjust the effects estimated in Section 4.

### B.4 The Effect of Stimulus Investments on the Share of Employees within Industries

In Section 4.3, we ask whether the countercyclical investment program predominantly created employment in the treated (and non-tradable) industries. A related question is whether the investment program also led to a change in the industry composition of the workforce, i.e., whether higher investments lead to an increase in the share of workers in the treated industries and to a decrease in the share of workers in the other industries. We investigate this question by replacing the dependent variable, $Employment\,p.c._{c,t}$, in the main empirical model (1) by the share of employees, $Employment\,(industry)_{c,t}/Employment_{c,t}$, in the treated, non-tradable, tradable, and other industries. Since these four industries constitute a partition of total employment, the employment shares across industries always sum to one, and an expansion of the employment share in one industry has to be accompanied by a contraction of the employment share of the remaining industries.

Table B.4 presents the results of this exploratory analysis using the same vector of controls as the analysis of the employment gains across industries in Section 4.3. The IV estimates indicate that the investment program shifted employment towards the treated industries. Specifically, the point estimates in column (1) imply that an increase in investments of €1000 per individual of working-age—roughly 3.5 times the mean and 8 times the inter-quintile range of investments within counties—led to a steady increase in the share of employees in the treated industries, peaking at a 1.5 percentage point higher share in 2011 and the years after the investment program than before the onset of the program in 2008. This increase in the share of employment within the "treated" industries is offset by a declining (or constant) employment share of all other sectors. Overall, these results provide suggestive

Table B.4: The Effects of Investments on the Shares of Employees within Industries

| | Share of Employees in | | | |
|---|---|---|---|---|
| | Treated | Non-tradables | Tradables | Other |
| | (1) | (2) | (3) | (4) |
| Investments p.c. (in €1000) | | | | |
| × Q1 2008–Q3 2008 | 0.0005 | 0.0007 | −0.0018 | 0.0005 |
| | (0.0017) | (0.0039) | (0.0014) | (0.0040) |
| × 2009 | 0.0061 | −0.0044 | 0.0006 | −0.0023 |
| | (0.0020) | (0.0050) | (0.0017) | (0.0053) |
| × 2010 | 0.0094 | −0.0070 | 0.0020 | −0.0044 |
| | (0.0034) | (0.0096) | (0.0036) | (0.0100) |
| × 2011 | 0.0149 | −0.0129 | −0.0006 | −0.0014 |
| | (0.0073) | (0.0137) | (0.0046) | (0.0136) |
| × 2012–2013 | 0.0142 | −0.0206 | −0.0039 | 0.0103 |
| | (0.0072) | (0.0173) | (0.0103) | (0.0194) |
| County Fixed Effects | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes |
| Date Fixed Effects × | | | | |
| State × UrbanIndex | yes | yes | yes | yes |
| Emp. Shares by Educ. | yes | yes | yes | yes |
| School Age Population | yes | yes | yes | yes |
| Observations | 9600 | 9600 | 9600 | 9600 |

**Notes.** The dependent variable is the share of employees in treated industries (column (2)), non-tradable industries (column (3)), tradable industries (column (4)) and all remaining industries (column (5)) at each quarterly date between Q1 2008 and Q4 2013, normalized by the working-age population. *Investments p.c. × Q1 2008–Q3 2008* is the interaction of investments in €1000 per individual of working age with an indicator that equals one for the observations between Q1 and Q3 2008. All the other interactions are defined accordingly; the baseline is given by Q4 2008. The horizontal lines between the estimates indicate the beginning and the end of the stimulus program. All the remaining variables and statistics are described in Table 2. Standard errors clustered at the county level are in parentheses.

evidence that the stimulus investment program led to a small shift of employment towards the treated industries.

### B.5 Robustness

In Table B.5, we evaluate the robustness of the empirical results with respect to a number of alternative specifications. For brevity, each row of Table B.5 documents the results of a different specification and reports the average employment difference in 2011 (the peak of the employment gains in the main specification) and its standard error clustered at the county level, the minimum of the Shea Partial $R^2$ of all the first stages, the number of job-years and its standard error, the costs per job-year, and the number of observations. For comparison, row (0) reports these statistics for the main specification (column (2) of Table 2), which serves as the baseline for all robustness checks. Before going into details, note that all the robustness checks, except those using only the East German sample in rows (5) and (10), yield estimates for the costs per job-year that are within one standard deviation of the baseline estimate.

**Model Variants** The first set of robustness checks alters the specification of the empirical model or the estimation strategy. Row (1) estimates the baseline specification using the limited information maximum likelihood (LIML) estimator, which is less susceptible to weak IV bias, but less precise. Weak IV bias should not be an issue for the baseline specification, as the Kleibergen–Paap F statistic of the first stage is 17.9, well above the common critical value of 10 (Table 1). It is nevertheless reassuring that the LIML estimates are very close to their 2SLS counterparts. In row (2), we follow parts of the literature (e.g., Acconcia et al., 2014; Dupor and Mehkari, 2016) and weight the counties by their labor force population in the estimation.[28] Introducing weights leads to a slightly smaller estimate for the number of job-years created, but it remains well within the range of estimates reported in Table 2. In row (3) the standard errors are clustered at the level of 94 labor market regions *(Raumordnungsregion)* to account for the possibility of a local correlation or errors beyond county borders, and in row (4) the standard errors are two-way clustered at the level of the states and the quarterly dates to account for a possible correlation of errors within states and at particular points in time. Both alternatives of clustering leave the standard errors almost

[28] The number of papers in the literature that do and do not weight observations by their population are seemingly roughly equal. Other works that, like this paper, abstain from using weights in their main specifications are those by Nakamura and Steinsson (2014), Wilson (2012), and Suárez Serrato and Wingender (2016). Note that weighing the observations would deal with potentially less precise measurement of employment in smaller counties. However, there should be little concern regarding measurement error in the dependent variable, because we use administrative data on the universe of workers.

Table B.5: Robustness

| | $\beta$(2011) | SE | Shea $R^2$ | Job-Years | SE(Job-Yrs) | Costs p. JY | N |
|---|---|---|---|---|---|---|---|
| (0) Baseline | 2.54 | 0.89 | 0.11 | 4.19 | 1.74 | 23862 | 11200 |
| **Model Variants** | | | | | | | |
| (1) LIML | 2.68 | 0.95 | 0.11 | 4.40 | 1.85 | 22719 | 11200 |
| (2) Weighted by labor force pop | 2.20 | 0.71 | 0.16 | 3.60 | 1.46 | 27780 | 11200 |
| (3) Cluster: labor market region | 2.54 | 0.94 | 0.11 | 4.19 | 1.85 | 23862 | 11200 |
| (4) Cluster: date and state | 2.54 | 0.85 | 0.11 | 4.19 | 1.38 | 23862 | 11200 |
| (5) West Germany only | 2.71 | 0.92 | 0.12 | 4.47 | 1.78 | 22372 | 9072 |
| (6) East Germany only | 0.33 | 2.52 | 0.07 | 0.80 | 5.15 | 124687 | 2128 |
| (7) Employment $\geq$ 25 years of age | 1.71 | 0.78 | 0.11 | 2.95 | 1.57 | 33877 | 11200 |
| **Instruments** | | | | | | | |
| (8) All school types separately | 2.81 | 0.83 | 0.13 | 4.38 | 1.59 | 22811 | 11200 |
| (9) All school types aggregated | 2.01 | 0.91 | 0.08 | 3.95 | 1.91 | 25302 | 11200 |
| (10) Instruments in 1995 (West Germany) | 2.50 | 0.90 | 0.14 | 3.35 | 1.73 | 29830 | 9072 |
| (11) Instruments in 1995 (East Germany) | −1.65 | 2.35 | 0.05 | 0.21 | 5.72 | 475214 | 2128 |
| (12) Schools (2008) $\leq$ Schools (1995) | 2.62 | 1.07 | 0.10 | 4.41 | 2.16 | 22652 | 9688 |
| **Controls** | | | | | | | |
| (13) With short-time work | 2.49 | 0.89 | 0.11 | 4.31 | 1.75 | 23213 | 11200 |
| (14) With wage distribution (p25 p50 p75) | 3.39 | 1.06 | 0.10 | 5.61 | 2.04 | 17836 | 6504 |
| (15) With pop younger 18 (child support) | 2.46 | 0.87 | 0.11 | 4.08 | 1.72 | 24482 | 11200 |
| (16) With new cars | 2.59 | 0.89 | 0.11 | 4.37 | 1.76 | 22863 | 11200 |
| (17) With commuter | 2.60 | 1.15 | 0.07 | 4.18 | 2.20 | 23951 | 11200 |
| (18) With Bartik shocks (baseline: Q1 09) | 2.61 | 0.66 | 0.11 | 5.07 | 1.36 | 19726 | 8000 |
| (19) With industry structure | 2.47 | 0.86 | 0.11 | 4.16 | 1.63 | 24067 | 11200 |
| (20) With residential building construction | 2.67 | 0.88 | 0.11 | 4.47 | 1.70 | 22395 | 11200 |
| (21) With 2005 & 2009 election outcomes | 2.58 | 1.07 | 0.09 | 3.95 | 2.03 | 25323 | 11200 |
| (22) Additional age structure controls | 4.16 | 1.37 | 0.07 | 7.12 | 2.61 | 14039 | 11200 |
| (23) Additional age structure by gender | 3.01 | 1.33 | 0.07 | 5.74 | 2.52 | 17434 | 11200 |
| (24) Date × state FEs, area p.c. | 3.31 | 0.89 | 0.12 | 5.54 | 1.69 | 18062 | 11200 |
| (25) Date × state FEs, area p.c., area$^2$ | 3.29 | 0.89 | 0.12 | 5.48 | 1.68 | 18239 | 11200 |

**Notes.** This table presents the results of various modifications of the baseline empirical specification given by column (2) of Table 2. Each row represents the results of a different specification; see the text for details. Column $\beta$(2011) reports the coefficient estimate of *Investments p.c.* × *2011*; column *SE* reports its standard errors, clustered at the county level (except for the rows (3) and (4)). *Shea* $R^2$ reports the minimum of the Shea Partial $R^2$ of the excluded instruments among all the first stages (one for each interaction of *Investments p.c.*). The number of *Job-Years* is the sum of the coefficients of *Investments p.c.* between 2009 and 2011. *SE(Job Yrs)* is the standard errors of *Job-Years* calculated via the Delta Method. *Costs per Job-Year* equal 100'000/*Job-Years*.

unchanged. Next, we split the sample according to whether the counties were part of the former West or East Germany. The results in rows (5) and (6) suggest that the employment effects are strong in West German counties but negligible in the East. Yet, the estimate for the East is very imprecise, reflecting low statistical power within the small sample of the 76 East German counties. In the context of our IV strategy, the low power due to fewer observations is potentially amplified by the weaker first stage, as indicated by the lower Shea Partial $R^2$ for the East German sample. The first stage in East Germany may be weaker because the backlog of public buildings in need of renovation is likely to be low due to the numerous infrastructure investment programs implemented after reunification. Finally, row (7) estimates the employment effects for employees older than 25 years of age to account for the potential concern that counties with a high number of schools are populated by a relatively young labor force with potentially distinct labor market dynamics. Despite excluding the part of the labor force with the most elastic labor supply, economically and statistically significant effects remain.

**Instruments** The second set of robustness checks modifies the instrumental variable strategy. In row (8) the two instruments used in the main specification—the number of primary and secondary schools and the number of academic high schools—are replaced by the number of schools within each of the six school types included in the latter two categories (see Appendix A.3 for details). Conversely, in row (9), the aggregated number of schools across all school types is used as the only instrument. Both alternative specifications of the instruments yield estimates that are very close to the baseline specification. The focus of the investment program was on renovating school buildings, so that old schools are expected to constitute a particularly good instrument provided that they persist over time. We test this conjecture in rows (10) and (11) by instrumenting, separately for West and East Germany, stimulus investments via the number of primary and secondary schools and the number of academic high schools in 1995, the earliest date for which this data is publicly available. The results for West Germany are the same as in the main specification, and the Shea Partial $R^2$ indicates that schools in 1995 are a strong instrument for investments. For East Germany, the estimates are very noisy, possibly reflecting low statistical power, as mentioned above. Also, the low persistence of the number of schools (as shown in Figure A.3 in Appendix A.6) in East Germany, which reflects the extensive administrative restructuring in the wake of the German reunification, probably contributes to the weak first stage manifested in the low Shea Partial $R^2$. Row (12), in turn, rules out the concern that having a growing number of schools reflects a healthy local economy by restricting the sample to those counties, for which the total number of schools in 2008 is weakly lower than in 1995. Doing so leaves the empirical findings unchanged.

**Controls** The third set of robustness checks explores whether altering the set of control variables of the baseline specification leads to different empirical results. First, we control for a range of additional policy measures that were (partially) introduced to counteract the recession (see Bundesministerium für Wirtschaft und Technologie (2011) for a list of these measures). In row (13), we verify that controlling for short-time work (relative to the labor force population), a sizable part of the German stimulus package, does not affect the empirical estimates.[29] We control for short-time work using the full-time work equivalents of short-time workers. In addition, the German stimulus package also reduced income taxes and mandatory social security contributions. To see whether these (implicit) tax rebates confound the results, we allow for date-specific effects of the 25th, 50th and 75th percentile of the wage distribution in 2008 in row (14). The stimulus package also raised the tax and flat-rate bonuses for dependent children; row (15) hence controls for the ratio of children younger than 18 years of age and the labor force population (interacted with date fixed effects). Another part of the stimulus program was a cash for clunker scheme and changes of the taxes for motor vehicles that mostly benefited the owners of new, fuel-efficient cars. In row (16) we control for the number of newly purchased private cars per capita by county and year.[30] Finally, the government raised the commuting allowance. To account for this policy, row (17) accounts for date-specific effects of the number of commuters at the county level. Neither of these additional covariates changes the results. In fact, the cost per job-year estimates remain within the range of €18'000 to €24'500.

Next, we investigate whether the results are driven by industry-specific shocks that are, for some indeterminate reason, correlated with the instruments. To this end, row (18) includes quarterly "Bartik shocks" as an additional control variable (Bartik, 1991).[31] This specification only includes data from 2009 onwards, because the employment data at the two-digit sector level is only available starting in 2008 and because one year of data is

[29]Short-time work is an employment subsidy paid by the German employment agency (*Bundesagentur für Arbeit*) to workers who are idle due to a temporary drop in demand below output potential. Firms have to request the subsidy for their employees, the requirements of which were loosened during the crisis resulting in a sharp increase in the number of workers receiving short-time work benefits (see, e.g., Burda and Hunt, 2011, for a detailed description of the policy). We control for short-time work using the full-time work equivalents of short-time workers. The data is published at quarterly frequency by the German employment agency.

[30]We thank Ines Helm at Stockholm University for sharing her data.

[31]Bartik shocks are defined as a county's predicted employment level if its employment in each two-digit industry would have grown at the same rate as employment within this industry across all the remaining counties. Formally, the Bartik shock $b_{c,t}$ of county $c$ on quarterly date $t$ is given by $b_{c,t} = \sum_{s \in \text{2-digit industries}} [(e_{-c,t,s} - e_{-c,t-4,s})/e_{-c,t-4,s}] \times e_{c,t-4,s}$, where $e_{-c,t,s}$ ($e_{c,t,s}$) is employment in industry $s$ on date $t$ in all counties other than $c$ (in county $c$).

needed to compute the shocks. The employment differences in this specification are thus estimated relative to employment in Q1 2009. Row (19) uses an alternative approach to account for industry-specific shocks. Here, the employment shares within each of the main sectors of the economy—agriculture, manufacturing, and construction (the share in services serves as the baseline)—measured in 2008 are interacted with date fixed effects, allowing for very flexible, date-specific shocks correlated with the industry structure. Neither of the ways of controlling for industry-specific shocks affects the empirical results. The remaining five specifications explore alternative ways to control for potential correlates of investments or the number of schools. Row (20) controls explicitly for private activity in the construction sector by controlling for the number of residential houses built in the respective years. Row (21) flexibly accounts for the potential allocation of stimulus funds along political party lines by controlling for the electoral shares of all major parties (Christian Democrats, Social Democrats, Greens, Liberals, Left Party) in the federal elections of 2005 and 2009, both interacted with date fixed effects. Row (22) includes more extensive controls for the age structure by adding the share of the population within the age brackets of 25 to 50 years of age and 50 to 65 years of age to the set of country characteristics that are interacted with date fixed effects. Row (23) adds all of these age brackets (including the school-age population between 6 and 18 years of age) separately for each gender. The final two specifications introduce other means of controlling for population density. Instead of the interactions of date, state, and the value of the urbanization index, we add date fixed effects at the state level as well as counties' area per capita interacted with date fixed effects to the set of covariates in row (24). Row (25) also adds the square of area per capita. Each of these alternative specifications yields estimates of employment gains equal to or larger than the ones from the baseline specification.

### B.6 Employment Dynamics Relative to Averages of Employment and Unemployment between Q1 2007 and Q4 2008

In the main text, we estimate the employment gains and unemployment reductions of the investment program relative to Q4 2008, the last quarterly date before the program was active. Calculating the gains and reductions relative to a single date allows us to evaluate whether the instrumented investments are correlated with (un)employment dynamics before the crisis. This comes at the potential cost that (un)employment levels at the reference date may be (spuriously) correlated with the instruments, resulting in potentially misleading estimates.

To rule out this potential concern, this section estimates the (un)employment gains relative to average (un)employment during the years 2007 and 2008, the entire pre-program period in the data. Specifically, we slightly modify the empirical model underlying the main results in Table 2 as follows

$$
\begin{aligned}
(Un)Employment\,p.c._{c,t} = &\sum_{Y=2009}^{2011} \beta_Y\; Investments_c \times \mathbb{1}\,(\mathrm{t}{\in}[\mathrm{Q1\ Y, Q4\ Y}]) + \\
&\beta_{post}\; Investments_c \times \mathbb{1}\,(t \in [\mathrm{Q1\ 2012, Q4\ 2013}]) + CountyFE_c + \\
&\sum_{t \neq \mathrm{Q4\ 2008}} Date_t \times \boldsymbol{CountyCharacteristics}_{\boldsymbol{c}}^{'}\,\boldsymbol{\Gamma_t} + \psi\; PopGrowth_{c,t} + \tilde{\varepsilon}_{c,t}.
\end{aligned}
$$

The only difference to the model underlying the main results outlined in Footnote 10 is that the investment coefficient of the pre-program period vanishes.

Columns (1)-(6) of Table B.6 show the results of estimating the model above with *Employment p.c.* as the dependent variable, and columns (7) and (8) show the results for *Unemployment p.c.* as the dependent variable. Across specifications, the employment gains are slightly smaller than the ones reported in Table 2, and the unemployment reductions are larger than the ones in Table 2. Overall, however, both the employment gains and unemployment reductions are of similar magnitudes as the corresponding estimates in the main text.

### B.7 Employment Dynamics as Estimated via a Repeated Cross-Section

The main empirical model (1) is predominantly identified from cross-sectional variation in the data, as most of the variables are interacted with indicator variables for the quarterly dates. An alternative strategy to estimate the employment dynamics of the countercyclical investment program would hence be, to estimate a repeated cross-section—one empirical model for each quarterly date—with similar sets of covariates as the ones included in the main empirical analysis. Specifically, we follow Chodorow-Reich et al. (2012) and estimate the following cross-sectional model

$$
\begin{aligned}
(Employment\,p.c._{c,t} - Employment\,p.c._{c,\mathrm{Q4\ 2008}}) = &\beta_t\; Investments\,p.c._c + CountyFE_c \\
&+ \boldsymbol{CountyCharacteristics}_{\boldsymbol{c}}^{'}\,\boldsymbol{\Gamma_t} + \psi_t\; PopGrowth_{c,t} + \varepsilon_{c,t} \quad \text{(B.2)}
\end{aligned}
$$

separately for each quarterly date $t \in \{\mathrm{Q1\ 2007, Q2\ 2007}, \ldots, \mathrm{Q3\ 2008, Q1\ 2009}, \ldots, \mathrm{Q4\ 2013}\}$. The total $Investments\,p.c._c$ during the program period are instrumented with $Academic\ High\ Schools\ p.c._c$ and $Primary\ and\ Secondary\ Schools\ \ p.c._c$, as usual.

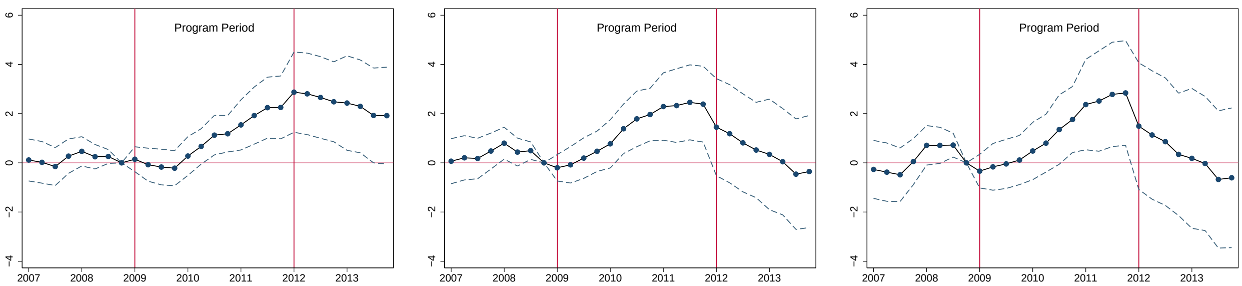
Figure B.2 displays the employment dynamics estimated via the repeated cross-sections with the same set of covariates as the specifications underlying the employment dynamics

Table B.6: (Un)Employment Effects Relative to Pre-Program Averages

| | Employment Rate | | | | | | Unmployment Rate | |
|---|---|---|---|---|---|---|---|---|
| | IV Estimates | | | OLS Estimates | | | IV | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Investments p.c. | | | | | | | | |
| × 2009 | −0.26 | −0.24 | −0.24 | 0.45 | 0.15 | 0.20 | 0.12 | −0.01 |
| | (0.50) | (0.54) | (0.72) | (0.19) | (0.18) | (0.19) | (0.60) | (0.18) |
| × 2010 | 0.71 | 1.21 | 1.06 | 0.54 | 0.32 | 0.21 | −0.60 | −0.12 |
| | (0.54) | (0.70) | (0.92) | (0.24) | (0.24) | (0.24) | (0.63) | (0.20) |
| × 2011 | 2.07 | 2.20 | 2.76 | 0.74 | 0.50 | 0.44 | −1.46 | −0.20 |
| | (0.72) | (0.94) | (1.36) | (0.31) | (0.34) | (0.34) | (0.72) | (0.23) |
| × 2012–2013 | 2.34 | 0.18 | 0.40 | 0.80 | 0.16 | 0.27 | −1.49 | −0.17 |
| | (0.99) | (1.25) | (1.68) | (0.45) | (0.46) | (0.46) | (0.83) | (0.28) |
| County Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Population Growth | yes | yes | yes | yes | yes | yes | yes | yes |
| Date Fixed Effects × | | | | | | | | |
| State × UrbanIndex | yes | yes | yes | yes | yes | yes | yes | yes |
| Emp. Shares by Educ. | no | yes | yes | no | yes | yes | yes | yes |
| School Age Population | no | yes | yes | no | yes | yes | yes | yes |
| Universities & Hospitals | no | no | yes | no | no | yes | no | no |
| min(Shea Partial $R^2$) | 0.15 | 0.11 | 0.07 | . | . | . | 0.11 | . |
| Cumulative Job-Years | 2.52 | 3.17 | 3.58 | 1.73 | 0.98 | 0.84 | 1.94 | 0.34 |
| SE Cumulative Job-Years | 1.58 | 2.00 | 2.74 | 0.67 | 0.71 | 0.71 | 1.78 | 0.57 |
| Costs per Job-Year | 39656 | 31546 | 27936 | 57807 | 102538 | 118489 | 51433 | 297010 |
| SE Costs per Job-Year | 24787 | 19898 | 21345 | 22443 | 74681 | 100026 | 47103 | 499662 |
| Observations | 11200 | 11200 | 11200 | 11200 | 11200 | 11200 | 11200 | 11200 |

**Notes.** The dependent variable in columns (1) to (6) is the employment rate at each quarterly date between Q1 2007 and Q4 2013. The dependent variable in columns (7) and (8) is the unemployment rate. *Investments p.c. × 2007–Q3 2008* is the interaction of investments in €100'000 with an indicator that equals one for the observations between 2007 and Q3 2008. All the other interactions are defined accordingly; the baseline are the years 2007 and 2008. The horizontal lines between the estimates indicate the beginning and the end of the stimulus program. *Population Growth* is the ratio of the current working-age population and the working-age population in 2008. The following variables, measured in 2008, are interacted with the full set of date fixed effects: *State ×UrbanIndex* (interactions of indicators for the states and the values of the urbanization index), *Emp. Shares by Educ.* (shares of employees with a college degree and with vocational training), *School-Age Population*, and *Universities and Hospitals*. *Min(Shea Partial $R^2$)* reports the minimum of the Shea $R^2$ of the excluded instruments—the date interactions of *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.*—among all the first stages (one for each interaction of *Investments p.c.*). The number of *Job-Years* is the sum of the coefficients of *Investments p.c.* between 2009 and 2011. *Costs per Job-Year* equal 100'000/*Job-Years.* The standard errors of *Job-Years* and *Costs per Job-Year* are calculated via the Delta method. Standard errors clustered at the county level are in parentheses.

Figure B.2: Employment Dynamics Estimated via Repeated Cross-Sections




**Notes.** This figure shows the differences in employment per €100'000 invested, $\beta_t$, at each quarterly date $t$ between Q1 2007 and Q4 2013 relative to Q4 2008, as well as their 90 percent confidence interval as estimated via IV. The results are obtained by estimating repeated cross-sections of the model (B.2). The empirical specification underlying Panel (a) includes the most parsimonious set of covariates identical to the one used for Panel (a) of Figure B.1. The empirical specification underlying the results in Panel (b) and Panel (c) use the same set of covariates as the ones used in Figure 1 and Panel (b) of Figure B.1, respectively. The left vertical line indicates the last date before the investment program was passed into law; the right line indicates the first date after the end of the program.

in Figures 1 and B.1, respectively. Both the estimates and their precision shown in Figure B.2 are nearly identical to their counterparts. This comes as no surprise, given that the main empirical model (1) and the repeated cross-sections, as defined by (B.2), are primarily identified by the same cross-sectional variation in the data.

## B.8 The Estimated (Un)Employment Effects of Investments Using the Cross-Sectional Dimension of the Data

In the main empirical model (1), we interact the cross-sectional data on investments across counties with indicator variables for the quarterly dates to estimate the dynamic effect of the countercyclical investment program. This strategy results in many endogenous variables. We instrument these endogenous variables with date interactions of the instruments, *Academic High School p.c.* and *Primary and Secondary Schools p.c.*, which also vary predominantly along the cross-sectional dimension of the data. As pointed out in Appendix (A.5), the properties of IV models with many endogenous variables and many instruments are poorly understood. Also, every date interaction of the number of schools is uninformative for all but one of the endogenous variables so that the F-statistics of the excluded instruments in the first stage models in Table A.4 at values that typically indicate weak instrument problems. This is despite the fact that the number of schools seems to be a sufficiently relevant instrument in the cross-section, as shown in Table 1.

However, we can also estimate the main statistics of interest from a cross-sectional specification similar to the one used, e.g., by Dupor and McCrory (2018). Starting with (1), we subtract $\textit{Employment p.c.}_{c,Q4\,2008}$ on both sides, multiply by 1/4 and sum over all the quarterly dates between Q1 2009 and Q4 2011, the dates during which the stimulus program was active. Noting that in (1) we set all the coefficients of the date interactions to zero for the baseline date Q4 2008, this gives

$$
\begin{aligned}
1/4 \sum_{t=\text{Q1 2009}}^{\text{Q4 2011}} (\textit{Employment p.c.}_{c,t} - \textit{Employment p.c.}_{c,\text{Q4 2008}}) = \\
\left(1/4 \sum_{t=\text{Q1 2009}}^{\text{Q4 2011}} \beta_t\right) \textit{Investments p.c.}_c + \\
\boldsymbol{CountyCharacteristics}_c^{'} \left(1/4 \sum_{t=\text{Q1 2009}}^{\text{Q4 2011}} \boldsymbol{\Gamma}_t\right) + \\
1/4\,\psi \sum_{t=\text{Q1 2009}}^{\text{Q4 2011}} (\textit{PopGrowth}_{c,t} - \textit{PopGrowth}_{c,\text{Q4 2008}}) + \\
1/4 \sum_{t=\text{Q1 2009}}^{\text{Q4 2011}} (\varepsilon_{c,t} - \varepsilon_{c,\text{Q4 2008}}) .
\end{aligned}
\qquad \text{(B.3)}
$$

Note that (B.3) is a cross-sectional model, as we sum across dates. Also, the coefficient of *Investments p.c.*, $1/4 \sum_{t=\text{Q1 2009}}^{\text{Q4 2011}} \beta_t$, directly gives the number of job-years created by the program, which is the main statistic of interest reported throughout the main text. Estimating (B.3) thus recovers the statistic of interest with only a single endogenous variable, for which we can instrument by the cross-section of the number of schools. The advantage of restating the empirical model in terms of (B.3) is that we can use all the standard results regarding the estimation of IV models with a single endogenous variable. The disadvantage is that the estimates of (B.3) are uninformative about the dynamics of the employment effects.

Columns (1)-(6) of Table B.7 report the estimates of (B.3), and columns (7) and (8) reports the estimates for the variant of (B.3) in which the dependent variable is the compound of the unemployment differences instead of the employment differences. The coefficients of *Investment p.c.* estimated via IV and their standard errors are very close to the job-year estimates from the main specifications in Tables 2.[32] As with the panel model, the IV estimates from the cross-sectional model thus imply that the investment program led to substantial gains in employment and sizable reductions in unemployment. The OLS estimates from the cross-sectional specification, in contrast, are weakly smaller than their counterparts in the main text. These estimates imply that the investment program had both statistically and economically irrelevant effects on (un)employment. Finally, the Kleibergen–Paap F-statistics

[32] With unemployment as the dependent variable, the IV results are virtually identical.

Table B.7: (Un)Employment Effects from a Cross-Sectional Specification

| | $1/4\sum_{t=Q1\ 2009}^{Q4\ 2011}\left(\text{Empl. Rate}_t - \text{Empl. Rate}_{Q4\ 2008}\right)$ | | | | | | $1/4\sum_{t=Q1\ 2009}^{Q4\ 2011}(\text{UR}_t - \text{UR}_{Q4\ 2008})$ | |
|---|---|---|---|---|---|---|---|---|
| | IV Estimates | | | OLS Estimates | | | IV | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Investments p.c. | 2.80 | 4.04 | 3.82 | 0.71 | 0.24 | −0.06 | −1.88 | 0.05 |
| | (1.32) | (1.72) | (2.27) | (0.61) | (0.62) | (0.60) | (1.64) | (0.62) |
| Population Growth | 0.40 | 0.37 | 0.39 | 0.42 | 0.40 | 0.42 | 0.07 | 0.05 |
| | (0.03) | (0.04) | (0.04) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Empl. Share w College /100 | | 10.69 | 13.31 | | 13.36 | 14.83 | −4.21 | −5.56 |
| | | (3.65) | (3.50) | | (3.02) | (3.07) | (3.23) | (2.94) |
| Empl. Share w Vocational Tr. /100 | | 4.63 | 5.85 | | 4.60 | 5.83 | −6.63 | −6.62 |
| | | (2.79) | (2.85) | | (2.78) | (2.79) | (2.68) | (2.65) |
| Share School-Age Pop /100 | | 11.52 | 8.28 | | 4.88 | 5.01 | 5.05 | 8.40 |
| | | (6.67) | (6.44) | | (5.67) | (5.85) | (5.06) | (4.63) |
| Universities p.c. | | | −412.83 | | | −71.73 | | |
| | | | (300.45) | | | (193.29) | | |
| Hospitals p.c. | | | 50.46 | | | 83.92 | | |
| | | | (31.47) | | | (23.00) | | |
| State × UrbanIndex FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Kleibergen–Paap F | 22.62 | 18.30 | 10.27 | . | . | . | 18.30 | . |
| Shea Partial $R^2$ | 0.16 | 0.11 | 0.07 | . | . | . | 0.11 | . |
| Cumulative Job-Years | 2.80 | 4.04 | 3.82 | 0.71 | 0.24 | −0.06 | 1.88 | −0.05 |
| SE Cumulative Job-Years | 1.32 | 1.72 | 2.27 | 0.61 | 0.62 | 0.60 | 1.64 | 0.62 |
| Costs per Job-Year | 35703 | 24735 | 26199 | 141661 | 422292 | -1674511 | 53217 | -2192174 |
| SE Costs per Job-Year | 16876 | 10516 | 15584 | 121879 | 1097258 | 16700532 | 46350 | 29669835 |
| Observations | 400 | 400 | 400 | 400 | 400 | 400 | 400 | 400 |

**Notes.** The dependent variables are the sum of the (un)employment differences relative to Q4 2008 across all the quarterly dates during the program period. *Investments p.c. in € 100'000* is the sum of investments normalized by the working-age population (indicated by "p.c." for "per capita") over the years 2009 to 2011. *Population Growth* is the sum of the yearly growth of the working-age population relative to 2008 given by $1/4\sum_{t=Q1\ 2009}^{Q4\ 2011}(WorkingAgePop_{c,t}/WorkingAgePop_{c,2008}-1)$. *Empl. Share w College* and *Empl. Share w Vocational Tr.* are the share of employees with a college degree and vocational training, respectively. *Share School-Age Pop* is the number of individuals between 6 and 18 years of age as a fraction of the working-age population. *Universities p.c.* and *Hospitals p.c.* are the number of universities and hospitals. *State × UrbanIndex FE* are fixed effects for the interaction of indicator variables for the German states and for the values of a four-point urbanization index. *Academic High Schools p.c.* and *Primary and Secondary Schools p.c.* are the excluded instruments for *Investments p.c.* underlying the Kleibergen-Paap F statistic and the Shea Partial $R^2$. The sample is the cross-section of counties as measured in Q4 2008. Robust standard errors are in parentheses.

of the excluded instruments are above the common critical value of ten, indicating that the instruments are relevant.