The Employment Effects of Countercyclical Public Investments

By Lukas Buchheim and Martin Watzinger *

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.

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The Great Recession has sparked renewed interest in evaluating the effectiveness of fiscal policy in stimulating output and employment (for reviews, see Chodorow-Reich, 2019, and Ramey, 2019). Most of the literature focuses on the effects of composite fiscal spending on the economy, despite the fact that government spending is fundamentally heterogeneous, both in normal phases of the economy (see Cox et al., 2020; Proebsting, 2020) and during recessions (e.g., the American Recovery and Reinvestment Act of 2009 comprised of 22 funding lines). Consequently, there is little evidence on which types of policies are successful in increasing output and employment.

This paper analyzes whether investment in the renovation of public buildings, a type of spending that was included in many stimulus programs during the Great Recession, creates jobs both quickly and cost-effectively.[1] Investment in

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[1]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

the renovation of public buildings has potentially attractive characteristics as an economic stimulus: Public buildings are available in every region and their renovation can provide demand for the local firms with potentially short gestation lags. However, it is not yet known whether building renovations will actually create jobs quickly and cost-effectively or whether they will crowd out private demand and require planning periods that are too long to counteract an imminent recession.

Does the renovation of public buildings create jobs quickly and cost-effectively? To answer this question, we evaluate a major component of the German economic stimulus package, which provided €15.8 billion (0.62 percent of German GDP) mainly for upgrading public buildings. This investment program 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 enables us to address the challenge of stimulus investment programs being by construction endogenous to economic conditions. For example, governments may target regions that are hardest hit by the recession. We 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 predetermined stock variable and thus unrelated to the magnitude of the recession in a county, it constitutes an ideal instrument for local investments.

Our results show that the investment program had a quick impact. The employment effects built up during 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.[2] The employment gains peaked in 2011, followed by a rapid decline after the end of the program.

The results also imply that the buildings investments created jobs cost-effectively. Cumulatively, the program created 4.1 job-years for each €100'000 in investments between Q1 2009 and Q4 2011, implying average costs per job-year of slightly more than €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," that is, the ratio between the costs per job-year and

stimulus program allotted 1.2 percent of pre-crisis GDP to local investment programs that included local construction activity (Alloza and Sanz, 2020). The main investment program of the German stimulus package focused on upgrading public buildings and is studied here.

[2]The employment response to actual spending may be potentially even faster than these results suggest, as spending on projects might be delayed or gradual.

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 which applies to a regime with unresponsive monetary policy.[3]

Additional analyses show that the employment gains are accompanied by a drop in unemployment amounting to half the job creation. Moreover, employment increased predominantly 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 in 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 III.A. 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 United States apply to other countries.

Among the limited recent literature that estimates the economic impact of public investments with cross-sectional data, our study reveals 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 level of 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 found by Garin (2019). Finally, the projects implicitly affected by the sudden *contractions of local public works* due to Mafia infiltration in Italy studied by Acconcia, Corsetti and Simonelli (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 II describes the empirical strategy and the data used. In Section III, we discuss the main results. The online appendix includes a large number of additional results and robustness checks.

I. The German Stimulus Investment Program

The investment program (called *Zukunftsinvestitionsgesetz*) under consideration was the major government spending measure in the two German stimulus

[3]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.

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.

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, decisions regarding which projects will receive funding are 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 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 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.

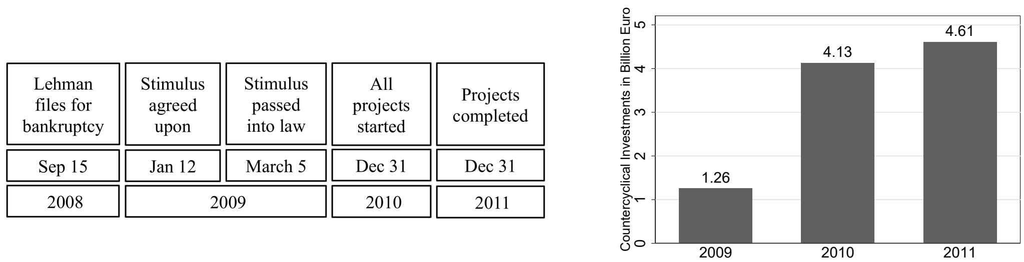
In addition to these restrictions on the types of investment projects, the bill required that projects be implemented locally: It mandated that 70 percent of the funds were to be spent on investments at the county or municipal level. The federal government also loosened the rules for public procurement in order 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 based on the share of tax revenue and the population share of the states in earlier years. 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 amounted to €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 used

Figure 1. : Time Structure of the Countercyclical Investment Program

(a) Timeline of Program Implementation (b) Federal Spending by Year



Note: This figure shows the legislative process timeline 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 being provided by state and regional governments.

a combination of the following three procedures: (i) the formulary allocation of funds to local layers of government (based, for example, on 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). Note that some of these procedures imply that the allocation of funds was driven by regional structural economic conditions: For example, North Rhine-Westphalia, Germany's largest state by population, allocated a significant share of funds based on the tax revenue and social security burden (including unemployment insurance) of municipalities and counties (Land Nordrhein-Westfalen, 2009). Similarly, the state of Bavaria, Germany's second largest state by population, assigned the majority of funds to its seven "Regierungsbezirke", a government entity between the state and the county level, largely based on their fiscal strength (Bayerische Staatsregierung, 2009). To the extend that structurally disadvantaged regions were also hit harder by the crisis, this type of targeting may induce OLS estimates of the job creation of the stimulus investments to be downward biased.

Panel (a) of Figure 1 provides a timeline of the swift implementation of the stimulus program. The governing coalition agreed on the economic stimulus program on January 12, 2009. Parliament passed the bill on March 5. Projects could receive financing from the program only if they had commenced 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 1 shows the yearly spending of the €10 billion federal funds: 12.6 percent were spent in 2009, 41.3 percent in 2010, and the remaining 46.1 percent in 2011 (Bundesministerium der Finanzen, 2011, 2012, 2013).

II. Empirical Model, Data and Identification

A. Empirical Model and Data

The aim 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 β_t, for the duration of the stimulus program (Q1 2009 to Q4 2011), as well as two years prior to and after the program. Specifically, for quarterly dates $t \in$ [Q1 2007, Q4 2013], we estimate variants of the following model:

(1)

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

where the index c denotes the county, and "p.c." (for "per capita") in the variable name indicates that it 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 particular county and pays social security contributions, including part-time workers, but excluding those who are self-employed or public servants. The quarterly unemployment data comprise 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, from 2008 onward, we obtained 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 sum of countercyclical stimulus investments between the end of January 2009 and the end of December 2011 in a county normalized by its working-age population measured in 2008. The primary source of 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 to €15.83 billion nationwide) during the three-year period between 2009 and 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.

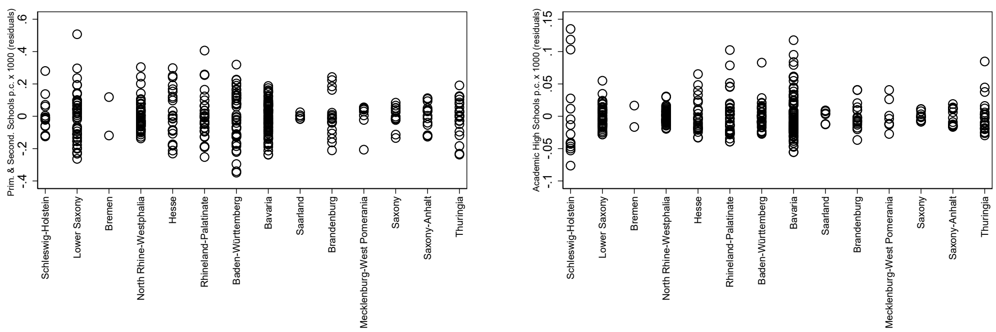
There is significant variation in investments across counties. The inter-quintile range of investments is €132 per capita, which is substantial compared with average investments of €282 per capita. For the mean county with a working-age population of about 127'000, the inter-quintile range corresponds to sizable differences in investments of €16.8 million. Figure A.1 in the online appendix depicts 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 β_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 β_t should equal zero, as 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 based on 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 "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 intended 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 (collected in the vector $\boldsymbol{CountyCharacteristics_c}$) 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, and 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. 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 provides exact definitions.

Figure 2. : The Distribution of Schools per Capita within States

(a) Primary and Secondary Schools (b) Academic High Schools



Note: 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.

B. *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 β_t are biased towards zero.

We address this endogeneity problem by exploiting a legal requirement of the stimulus bill to construct an instrument for stimulus investments: A total of 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 in the educational infrastructure—typically schools—within a county thus largely determined the scope for investment.

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 into two categories according to their size. The first category, called "academic high schools," encompasses two types of secondary school that award a diploma (*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 diplomas 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. Appendix A.3 provides additional information on school sizes and on the classification of schools.

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

2 shows 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 - 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 - 0.1 is sizable as well.

Relevance of instruments. — One main assumption of the IV strategy is that the instruments are relevant, that is, 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:

(2)
$$Investments\,p.c._c \times Date_\tau = \sum_{t:t\neq \text{Q4 2008}} Date_t \times \boldsymbol{Schools_c^{'}}\,\boldsymbol{\Theta_t^\tau} + CountyFE_c^\tau$$
$$+ \sum_{t:t\neq \text{Q4 2008}} Date_t \times \boldsymbol{CountyCharacteristics_c^{'}}\boldsymbol{\Lambda_t^\tau} + \psi^\tau\ PopGrowth_{c,t} + \nu_{c,t}^\tau,$$

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 II.B, and where the index τ indicates the coefficients of the date-τ 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), as both the instruments and the controls are interacted with date dummies. Notably, 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 results of estimating a variant of equation (2) using only the cross-section of the data in Q4 2008. Appendix A.4 reports the full first stage, including test statistics for weak instruments.

Table 1 shows that schools are a strong predictor of total investments in all 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 investment in buildings (the number of hospitals and universities) in column (3),

Table 1—: First Stage

	Countercyclical Investments per capita in €100'000		
	(1)	(2)	(3)
Academic High Schools p.c.	17.67	11.98	10.26
	(2.73)	(2.77)	(3.01)
Primary & Second. Schools p.c.	0.21	2.35	1.59
	(0.59)	(0.71)	(0.66)
% Empl. w. Univ.		1.23	0.76
		(0.38)	(0.36)
% Empl. w. Vocational T.		−0.08	−0.10
		(0.22)	(0.20)
Share School-Age Pop /100		−0.59	0.06
		(0.45)	(0.44)
Universities p.c.			86.78
			(22.35)
Hospitals p.c.			4.28
			(2.96)
State × UrbanIndex FE	yes	yes	yes
Shea Partial R^2	0.15	0.11	0.07
Effective F	18.89	17.77	10.20
Critical value 5% bias	22.64	9.62	14.38
Critical value 10% bias	14.16	6.78	9.59
Observations	400	400	400

Note: 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 diploma allowing 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 aged between 6 and 18 years 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 effective F-statistic following Olea and Pflueger (2013) and the Shea Partial R^2. For the effective F-Statistic we report the critical value for a Nagar bias of 5% and 10%. The sample is the cross-section of counties as measured in Q4 2008. Robust standard errors are in parentheses.

the effective F-statistic of the instruments remains above the critical value for a 10 percent asymptotic IV bias (Nagar bias) as suggested by Olea and Pflueger (2013) and the rule of thumb critical value of 10 suggested by Andrews, Stock and Sun (2019). 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-τ equation in the full, dynamic model (2) are the interactions of $\boldsymbol{Schools_c}$ with the respective date-τ indicator. All other date interactions of $\boldsymbol{Schools_c}$ are uninformative, which mechanically reduces the test statistics for joint significance of all the instruments. By 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.

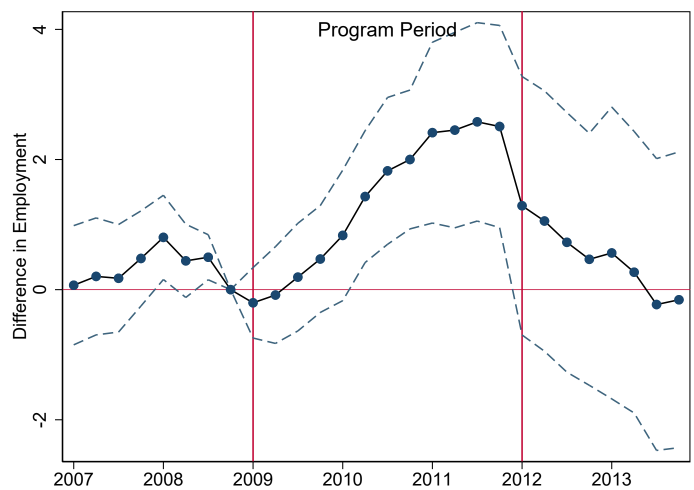
In terms of magnitude, the coefficients in Table 1 represent the average increase in investment (in €100'000) due to one additional school as both schools and investments are normalized by the working-age population. One additional academic high school is associated with an increase in investment between €1.03 million and €1.77 million, while one additional primary or secondary school leads to an increase of €20'000 to €230'000. Furthermore, Table A.4 in Appendix A.5 shows that schools explain investments in schools more than investments in universities or hospitals.

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 through their effect on investments. Consistent with this assumption, the IV results in the following section—displayed in Figure 3 and Table 2—show that instrumented investments are indeed unrelated to employment before the enactment of the stimulus program in Q1 2009.

In addition, the number of schools in a county is stable over time and therefore unlikely to be correlated with short or medium run economic conditions. Indeed, a regression of the total number of schools in 2008 on the number of schools in 1995 (the earliest date for which this data is available) and state dummies delivers an adjusted R^2 of 0.86. This high stability of the number of schools 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 include 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 determined predominantly by policy decisions in the 1970s or earlier, so it is likely that the number of schools were independent of employment outcomes during the 2009 recession.

Appendix A.6 provides further evidence on the stability of the number of schools over time.

Figure 3. : Employment Dynamics Caused by the Building Investment Program



Note: The connected dots show the differences in employment per total investment 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 for the periods 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).

III. Results

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

A. *Investments Increase Employment Quickly and Cost-Effectively*

Figure 3 and Table 2 below summarize the main results. Figure 3 shows graphically the baseline results from our preferred specification in column (2) of Table 2. It plots the coefficients $\{\beta_t\}_{t:t\neq \text{Q4 2008}}$, along with their 90 percent confidence intervals, 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 the equations (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. Statistical inference is based on standard errors clustered at the county level throughout, which are robust to potential serial correlation of errors.[4]

The first finding apparent from Figure 3 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 dropped sharply after the end of the program in 2012 and 2013. Comparing the employment dynamics to the dynamics of the federal outlays in Figure 1—the best available source for the spending dynamics—it becomes clear that the employment gains lag behind spending only slightly.[5] The second finding is that the employment gains were substantial: €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. 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 shown in Figure 3 can also be read from the table. Before the program and during its first year (2009), the employment gains are statistically indistinguishable from zero. In 2010, investments of €100'000 created 1.52 additional jobs on average, with the 90 percent confidence interval (CI) of [0.5, 2.5], and 2.49 jobs in 2011 (90% CI [1.0, 3.9]). 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 4.1 job-years (90% CI [1.3, 6.9]), resulting in relatively low average costs per job-year of €24'400 (90% CI [€7'649, €41'070], 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) controls only for county fixed effects, date fixed effects at the state $\times$ urbanization level, and population growth. The specification in column (3) adds the number of hospitals and universities to the main specification in column (2) to capture the determinants of investments from funding lines unrelated to schools. In comparison to the main specification in column (2), the parsimonious specification in column (1) indicates slightly smaller employment gains throughout 2010 and 2011, but these persist during the years after the program. The specification in column (3), in turn, delivers estimates of similar magnitude as in column (2), but

[4]Table B.5 in Appendix B.5 further shows that clustering at the labor market region—and, hence, allowing for regional and serial correlation of errors—has no sizable effect on the standard errors.

[5]In 2009, 13 percent of the total federal investment was spent, and 2 percent of the employment gains were realized. In 2010, 41 percent was spent, and 37 percent of the employment gains were realized. In 2011, 46 percent was spent, and 61 percent of the employment gains were realized.

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	0.38	0.16	−0.12	−0.02	−0.09	0.02	0.17
	(0.35)	(0.36)	(0.47)	(0.11)	(0.11)	(0.12)	(0.50)	(0.23)
× 2009	−0.10	0.09	−0.10	0.34	0.11	0.10	0.13	0.13
	(0.37)	(0.41)	(0.53)	(0.15)	(0.14)	(0.15)	(0.43)	(0.16)
× 2010	0.86	1.52	1.18	0.42	0.27	0.09	−0.58	0.03
	(0.42)	(0.60)	(0.76)	(0.23)	(0.23)	(0.22)	(0.61)	(0.21)
× 2011	2.20	2.49	2.83	0.60	0.43	0.30	−1.42	−0.05
	(0.66)	(0.88)	(1.24)	(0.30)	(0.34)	(0.33)	(0.70)	(0.25)
× 2012–2013	2.45	0.50	0.50	0.63	0.06	0.10	−1.44	−0.01
	(1.05)	(1.23)	(1.63)	(0.46)	(0.47)	(0.46)	(0.78)	(0.26)
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	2.96	4.11	3.90	1.35	0.82	0.48	1.86	−0.10
SE Cumulative Job Years	1.24	1.71	2.26	0.62	0.67	0.64	1.59	0.58
Costs per Job Year	33827	24360	25610	73994	122625	206995	53717	-968050
SE Costs per Job Year	14232	10136	14822	34065	100328	274599	46022	5468730
Observations	11200	11200	11200	11200	11200	11200	11200	11200

Note: 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-YearsCosts per Job-Year* are calculated via the Delta method. Standard errors clustered at the county level are in parentheses.

with less precision. Both specifications imply substantial cumulative job creation and low costs per job-year of €33'800 (90% CI [€10'363, €57'290]) in column (1) and €25'600 (90% CI [€1'173, €50'046]) in column (3).[6]

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 [0.3, 2.4]), such that creating one job-year cost about €74'000 (90% CI [€17'832, €130'156]). 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 β_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—approximately €24'000— with wages in the construction sector. 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 (see Appendix A.1 for the data sources). 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), who argues that the output multiplier, β_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, χ is the elasticity of hours per worker with respect 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 per worker can be approximated using data from van Rens (2012), which yields $\chi \approx 0.22$. Given the values for χ, output per worker, and the yearly employment

[6]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 those from the cross-sections. For additional details on the first stage, see Appendix A.4, which reports the complete first stage estimates corresponding to the baseline specification in column (2) of Table 2.

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 an inactive monetary policy, for example, 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).[7] 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, Kaplan and Zipperer, 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 investment are among the more effective stimulus measures.[8]

B. *Investments Reduce Unemployment*

The employment gains reported 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, the latter because 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 main policy objective of the investment program. We now consider 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 IV estimates reflect 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, 4.5]), around half of the respective employment gains. Therefore, the remaining employment gains must derive from

[7] 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.

[8] Studies of other specific countercyclical measures in the United States 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.

worker flows from self-employment, which is not captured in the social security data , or from workers moving from inactivity to employment.[9] Furthermore, the unemployment effect persisted in the post-program period. This could be in line with the declining employment effect if some individuals formally employed via the program became self-employed or inactive 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.

C. *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. Notably, in the project descriptions, which are available for a subset of states and described in Appendix A.5, 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. Accordingly, 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). We define the construction-related sectors, architects, and industries related to the installation of ICT as *treated* industries (Appendix A.1 provides the details). 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.[10] The idea, going back to at least Krugman (1991), is that non-tradable industries are needed everywhere and are thus geographically dispersed, while tradable industries are geographically concentrated in order 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), 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*.

Table 3 reports the results of estimating the main IV specification with sectoral employment as the dependent variable. For privacy reasons, the sectoral data do not report employment within some county×sector cells. Therefore, the baseline for the industry partition of the total employment gains shown in column

[9]Note that we measure the number of employees, i.e., workers with at least one, but possible multiple jobs. The difference between the employment gains and the reduction in unemployment can hence not be explained by some workers working multiple jobs.

[10]For each industry i, the index across counties c is defined as follows: $100 \cdot \sum_{c=1}^{N} \frac{empl_{i,c}^2}{(\sum_{c=1}^{N} empl_{i,c})^2}$. Earlier industry concentration measures based on the Herfindahl index can be found, e.g., in Ellison and Glaeser (1997) and Jensen and Kletzer (2005).

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

	Aggregate Employment per capita	Employment per capita in			
		Treated	Non-tradables	Tradables	Other
	(1)	(2)	(3)	(4)	(5)
Investments p.c.					
× Q1 2008–Q3 2008	0.47	0.08	0.25	−0.09	0.22
	(0.28)	(0.09)	(0.26)	(0.07)	(0.21)
× 2009	0.29	0.36	−0.08	0.04	−0.03
	(0.51)	(0.12)	(0.34)	(0.09)	(0.34)
× 2010	1.33	0.57	0.39	0.15	0.21
	(0.60)	(0.18)	(0.52)	(0.18)	(0.56)
× 2011	2.09	0.84	0.64	0.04	0.56
	(0.88)	(0.39)	(0.86)	(0.23)	(0.68)
× 2012–2013	0.20	0.51	−0.27	−0.18	0.15
	(1.24)	(0.38)	(1.08)	(0.53)	(1.08)
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
Universities & Hospitals	no	no	no	no	no
Cumulative Job Years	3.71	1.77	0.95	0.23	0.75
SE Cumulative Job Years	1.76	0.60	1.52	0.47	1.47
Observations	9600	9600	9600	9600	9600

Note: 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.

(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 investment. Notably, 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 3.7 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 0.56 jobs in 2011 (90% CI [-0.6, 1.7]) is the only economically substantial, but statistically insignificant estimate.

D. 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.

Appendix B.5 evaluates the robustness of the results in various dimensions. First, we show that changing the estimation strategy does not change the results. Specifically, we show that the results remain unchanged if they are estimated via limited information maximum likelihood, which is less susceptible to weak IV bias. Considering the recent debate between Ramey (2019) and Chodorow-Reich (2020) on whether weighting by population alters state-level estimates of employment multipliers in the United States, we show that our results are insensitive to weighting. We also show that three quarters of the employment gains are for employees older than 25 years, which alleviates the potential concern that our instrument, the number of schools, spuriously predicts employment dynamics of young workers.

Second, we demonstrate that the exact grouping of school types into instrumental variables does not affect the results. Moreover, the idea behind our empirical strategy suggests that the historical number of buildings may be a good instrument, as these are potentially the ones in need of renovation. We show that using the number of schools in 1995, the earliest date for which this data is available, delivers the same results for the former West German states. For the former East German states, the number of schools in 1995 is only a weak instrument, possibly reflecting the extensive administrative restructuring and renovation of school buildings in the wake of reunification.

Third, we show that our results are robust to the inclusion of a wide range of additional controls. One set of controls accounts for other countercyclical measures enacted during the Great Recession. A second set of controls accounts for potential confounds via the industry structure or general construction activity. A third set of controls implements alternative ways of accounting for the age structure and population density than our main specification.

Appendix B.6 estimates the employment dynamics relative to a different baseline, namely average (un)employment during the pre-crisis years. Appendix B.7 follows Chodorow-Reich et al. (2012), and estimates the employment dynamics via repeated cross-sections. And Appendix B.8 follows the approach of Dupor and McCrory (2018) and estimates the cumulative employment gains from single cross-section, that is, without dynamics. All of these approaches lead to similar results as our main specification.

IV. 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 alike. While there is new theoretical and empirical evidence concerning the macroeconomic conditions under which fiscal policy may be effective in general, evidence regarding which particular types of policies are successful in increasing output and jobs remains scarce. The contribution of this paper is to show that investment in public buildings can quickly and cost-effectively increase employment in the short run and are therefore a viable tool for counteracting 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 fiscal policy tools, like direct transfers to households or tax cuts. Given that job creation is a fundamental 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, namely countercyclical investment, this paper takes a first step towards answering this question. Further research is needed to inform policy makers about the employment effects of other policy tools at their disposal.

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