Online Appendix: The Employment Effects of Countercyclical Public Investments

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A Appendix to Section II: 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.5 uses data on the subset of investment projects with project descriptions to show that the number of schools explains school related investments. Section A.4 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 historically predetermined 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

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Variable	Description	Source
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
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 10), 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 10), 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.4)	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.4 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.4)	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.3, 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)*
Hospitals p.c. (all the tables except Tables 3, A.2, A.3, B.1–B.5)	The number of hospitals within a county as of 2008.	German Statistical Office, hospital statistics

Variable	Description	Source
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 11 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
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

Variable	Description	Source
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

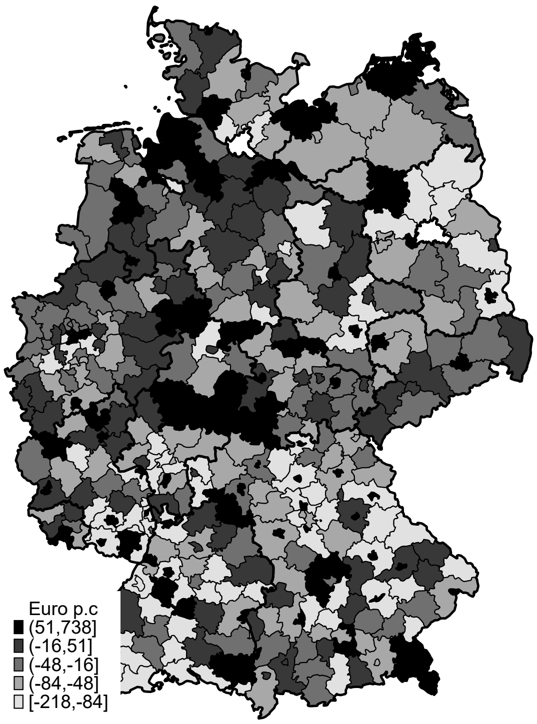
Wages in construction In the introduction and Section III.A we compare the cost per job-year to different wages in construction. The wage data have been retrieved from the following sources:

- 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.

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.

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 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

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
Academic High 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.

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 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 Complete System of First Stage Equations

Table A.3 reports the estimates of the complete system of first stage equations as described by Equation (2). More specifically, Table A.3 presents the first stage estimates of the baseline empirical results in columns (2) and (5) of Table 2.[1] 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 coefficients 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.3 and the cross-section are equal. These results are as expected, given that each of the first stage equations in Table A.3 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.3 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

[1] 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. Table A.3 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.3: 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	11.99	0.00	−0.00	−0.00	−0.01
	(2.76)	(0.01)	(0.00)	(0.01)	(0.02)
× 2009	−0.01	11.98	0.00	0.00	0.01
	(0.05)	(2.76)	(0.01)	(0.02)	(0.04)
× 2010	0.00	0.00	11.98	−0.00	−0.00
	(0.07)	(0.02)	(2.76)	(0.02)	(0.06)
× 2011	0.01	0.00	−0.00	11.98	−0.01
	(0.10)	(0.03)	(0.01)	(2.76)	(0.09)
× 2012–2013	0.19	0.05	−0.02	−0.07	11.80
	(0.23)	(0.07)	(0.04)	(0.08)	(2.78)
Primary & Secondary Schools p.c.					
× 2007–Q3 2008	2.35	−0.00	0.00	0.00	0.00
	(0.71)	(0.00)	(0.00)	(0.00)	(0.01)
× 2009	0.00	2.35	−0.00	−0.00	−0.00
	(0.01)	(0.71)	(0.00)	(0.00)	(0.01)
× 2010	0.01	0.00	2.35	−0.00	−0.01
	(0.02)	(0.01)	(0.71)	(0.01)	(0.02)
× 2011	0.01	0.00	−0.00	2.35	−0.01
	(0.03)	(0.01)	(0.00)	(0.71)	(0.03)
× 2012–2013	0.05	0.01	−0.01	−0.02	2.31
	(0.07)	(0.02)	(0.01)	(0.02)	(0.70)
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	3.55	3.55	3.55	3.55	3.55
Sanderson-Windmeijer F	7.31	7.65	7.62	7.57	7.48
Shea Partial R^2	0.11	0.11	0.11	0.11	0.11
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.

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.3 show that the instruments are equally informative for each investment-period interaction. We report the Kleibergen-Paap F-statistics instead of the effective F of Olea and Pflueger (2013), because the latter is not defined for multiple instruments. The Kleibergen-Paap F-statistics are below the commonly chosen critical value of ten, potentially indicating that the instruments are weak. However, the Sanderson–Windmeijer F-statistic, a test statistic for weak instruments suited for models with multiple endogenous variables, 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.3). 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 effective F-statistics at the same level of the ones reported 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.5 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.

Table A.4: 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.	10.26	4.54	2.86	1.59	1.95	1.08	0.17
	(3.01)	(1.75)	(1.81)	(0.72)	(1.25)	(0.43)	(0.39)
Primary & Second. Schools p.c.	1.59	1.25	0.88	−0.08	−0.23	0.45	0.28
	(0.66)	(0.43)	(0.34)	(0.21)	(0.31)	(0.16)	(0.10)
Empl. Share w College /100	0.76	0.26	0.87	−0.15	−0.11	0.10	0.02
	(0.36)	(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	0.06	0.50	−0.46	−0.12	−0.06	−0.04	−0.03
	(0.44)	(0.23)	(0.30)	(0.12)	(0.20)	(0.09)	(0.05)
Universities p.c.	86.78	8.72	77.99	−6.72	3.11	−0.55	−1.13
	(22.35)	(9.67)	(16.93)	(5.05)	(7.98)	(2.91)	(2.67)
Hospitals p.c.	4.28	0.19	0.21	0.77	1.78	0.37	−0.06
	(2.96)	(1.22)	(1.27)	(0.81)	(2.57)	(0.40)	(0.29)
State × UrbanIndex FE	yes	yes	yes	yes	yes	yes	yes
Shea Partial R^2	0.07	0.09	0.04	0.01	0.01	0.05	0.03
Effective F	10.57	8.18	4.32	2.35	1.45	8.22	3.71
Critical value 5% bias	13.21	12.49	18.26	6.04	15.93	14.74	10.52
Critical value 10% bias	8.89	8.47	11.87	4.68	10.50	9.80	7.31
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 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; 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.

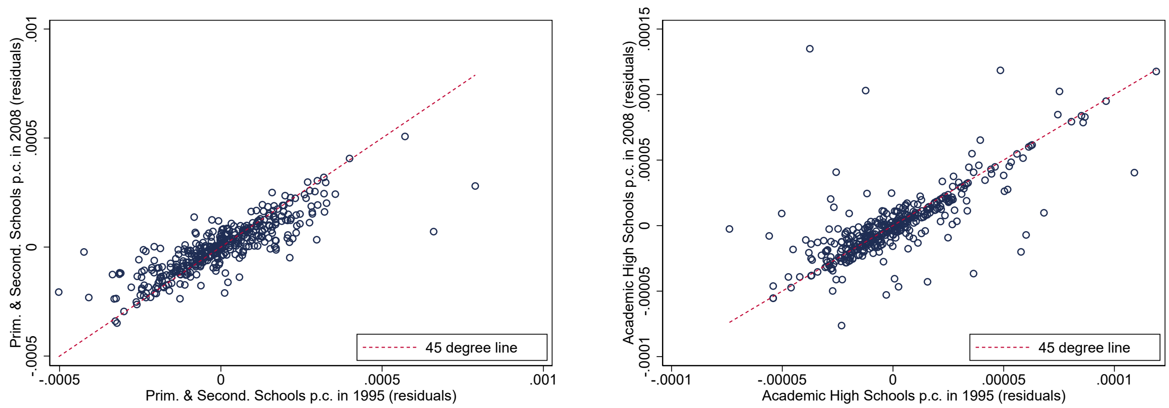
Of those projects that can be classified, 48 percent are school related projects. Among the school-related projects, we can classify 46 percent as projects for primary and secondary schools and 13 percent as projects for academic high schools. The project descriptions are not sufficiently detailed to assign the remaining 41 percent of school related projects to particular types of schools.[2] 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 per elementary and secondary school and more than one project for each academic high school.

Columns (1) to (5) of Table A.4 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.4 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).[3]

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

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



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.

A.6 The Stability of the Number of Schools over Time

This section further elaborates on the historical stability of the number of schools highlighted in Section II.B. Figure A.2 illustrates that the number of schools is predetermined 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.

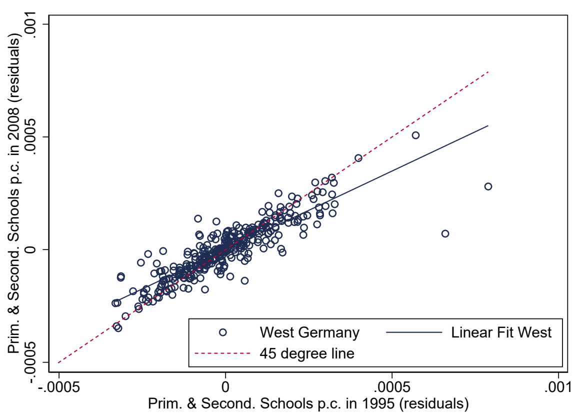
Figure A.3 plots the number of schools in 2008 against the number of schools in 1995, separately for counties in the former West German states (in Panels (a) and (b)) and in the former East German states (in Panels (c) and (d)). As above, schools are measured relative to their state averages. Clearly, the number of schools in the West is more stable 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 historical predetermination. In the East German counties, in contrast, the best linear fit of the observations is close to a horizontal line indicating a low stability over time. This result may be due to the significant restructuring of the administration in the East German states in the wake of reunification. The low stability 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 (10) of Table B.5), further amplifying the lack of statistical power for the

[2] 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.

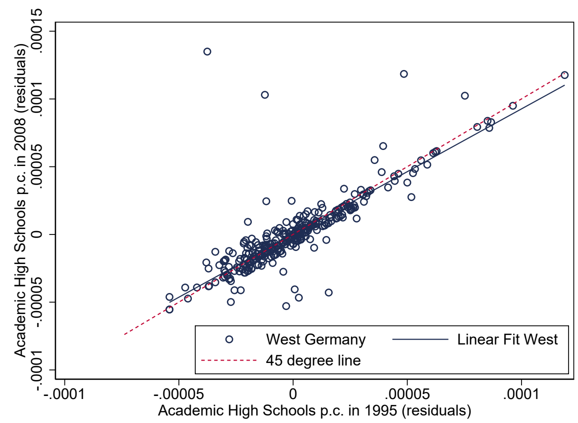
[3] Another share of this difference in total investments per school may be explained by the different sizes of academic high schools and primary and secondary schools pointed out in Appendix A.3.

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

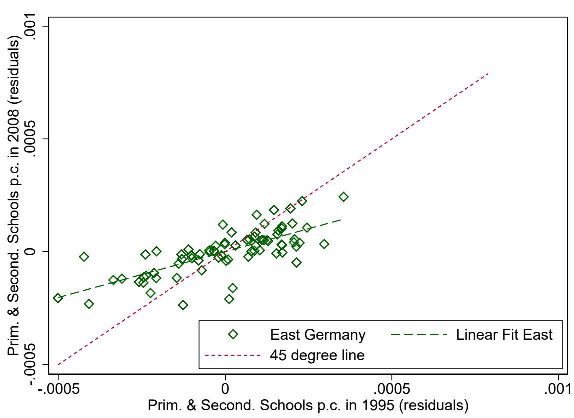
(a) Primary and Secondary Schools (West)



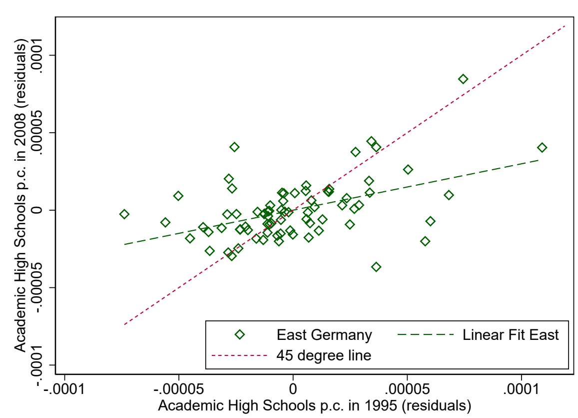
(b) Academic High Schools (West)



(c) Primary and Secondary Schools (East)



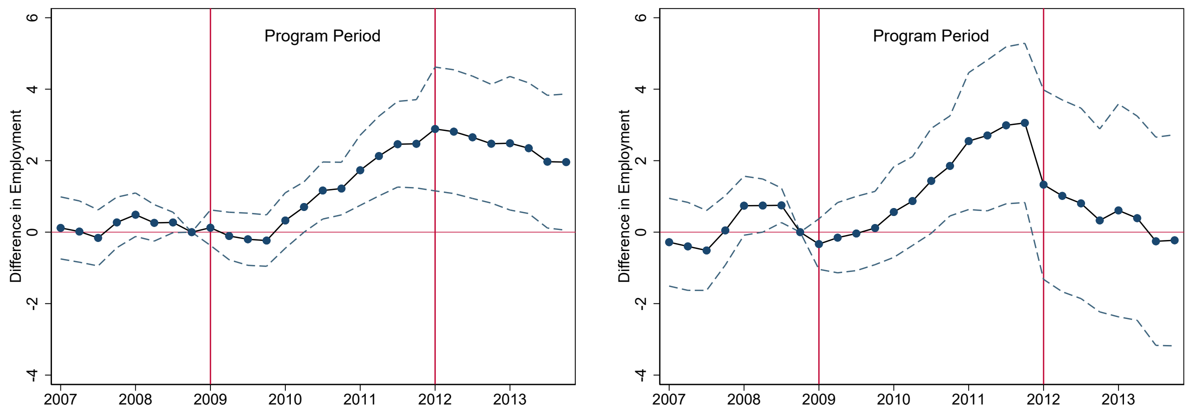
(d) Academic High Schools (East)



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.

Figure B.1: Employment Dynamics

(a) Parsimonious set of covariates (b) Comprehensive set of covariates



Notes. This figure shows the differences in employment per €100'000 invested, β_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.

sample of the 76 East German counties.

B Appendix to Section III: Results

This appendix presents a number of supporting results for the main analysis. Section B.1 complements Figure (3) 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 3 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 3 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) 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 3, 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 3.

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

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	0.38	0.32	0.34	0.36
	(0.36)	(0.35)	(0.39)	(0.37)
× 2009	0.09	0.40	0.29	0.34
	(0.41)	(0.50)	(0.47)	(0.49)
× 2010	1.52	2.06	1.93	1.95
	(0.60)	(0.69)	(0.70)	(0.69)
× 2011	2.49	3.37	2.93	2.91
	(0.88)	(0.94)	(0.99)	(0.98)
× 2012–2013	0.50	1.63	0.92	1.17
	(1.23)	(1.21)	(1.24)	(1.22)
Investments in Neighboring Counties p.c.				
× 2007–Q3 2008		0.01	0.01	0.01
		(0.01)	(0.01)	(0.01)
× 2009		−0.03	−0.04	−0.03
		(0.02)	(0.02)	(0.01)
× 2010		−0.03	−0.05	−0.03
		(0.03)	(0.03)	(0.02)
× 2011		0.00	−0.03	−0.01
		(0.04)	(0.05)	(0.02)
× 2012–2013		0.01	−0.01	−0.01
		(0.06)	(0.06)	(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	0.11	0.11
Cumulative Job Years	4.11	5.83	5.14	5.21
SE Cumulative Job Years	1.71	1.95	1.96	1.94
Costs per Job Year	24360	17141	19448	19210
SE Costs per Job Year	10136	5732	7401	7168
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.

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.[4]

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[5]—and a first check for spillovers, as the county aggregates of the satellite subregions should contain all spillovers within the aggregated counties.[6] 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 1.61 for the 2011 interaction in column (1) has a standard error of 1.33[7]).

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) (0.06 with 90% CI [-0.1, 0.3] and 0.35 with CI [-0.1, 0.5]), 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).

[4]In order to still be able to control for state × date fixed effects, we only consider regions that are located within one state.

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

[6]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.

[7]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.

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	0.53	0.39	1.21	1.43
	(0.53)	(0.53)	(0.55)	(0.56)
× 2009	0.48	0.77	−0.05	−0.00
	(0.53)	(0.58)	(0.83)	(0.80)
× 2010	1.75	1.92	1.87	2.22
	(0.78)	(0.77)	(1.06)	(1.06)
× 2011	0.88	1.23	2.34	3.45
	(1.00)	(0.99)	(1.49)	(1.59)
× 2012–2013	−1.81	−1.48	2.08	3.92
	(1.87)	(2.00)	(2.22)	(2.21)
Investments in Adjacent Region p.c.				
× 2007–Q3 2008		0.05		0.02
		(0.10)		(0.08)
× 2009		−0.18		−0.07
		(0.13)		(0.09)
× 2010		−0.06		0.06
		(0.18)		(0.12)
× 2011		−0.04		0.17
		(0.22)		(0.19)
× 2012–2013		−0.04		0.09
		(0.36)		(0.29)
Subregion Fixed Effects	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes
Date Fixed Effects ×				
State × Core Subregion	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	0.10	0.12
Cumulative Job Years	3.11	3.92	4.17	5.67
SE Cumulative Job Years	2.08	2.08	2.95	3.03
Costs per Job Year	32123	25501	24003	17622
SE Costs per Job Year	21489	13524	16997	9404
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.

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, 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.

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 η_y are indicative of crowding out, while positive values of η_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 €0.06 and €0.055 per Euro of *Investments* (with 90% CIs of [0.032, 0.088] and [0.030, 0.080], respectively). During the three years of the program period, these point estimates imply that counties and municipalities spent between €0.165 and €0.18 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

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	−0.099	−0.308	−0.087	0.027
	(0.021)	(0.220)	(0.017)	(0.050)
Investments p.c. × 2009–2011	0.060	−0.134	0.055	−0.002
	(0.017)	(0.140)	(0.015)	(0.048)
Investments p.c. × 2012–2013	0.036	−0.375	0.046	−0.009
	(0.024)	(0.201)	(0.024)	(0.073)
County Fixed Effects	yes	yes	yes	yes
Population Growth	no	yes	no	yes
Date Fixed Effects ×				
State × UrbanIndex	no	yes	no	yes
Emp. Shares by Educ.	no	yes	no	yes
School Age Population	no	yes	no	yes
Observations	2098	2098	2098	2098

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.* ×*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.

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.

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 [-0.365, 0.097] for the IV and [-0.081, 0.077] 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 III. 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 III.

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

In Section III.C, we ask whether the countercyclical investment program predominantly created employment in the treated (and non-tradable) industries. A related question is whether the in-

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	−0.0017	0.0005
	(0.0017)	(0.0038)	(0.0014)	(0.0039)
× 2009	0.0060	−0.0043	0.0006	−0.0023
	(0.0020)	(0.0049)	(0.0017)	(0.0052)
× 2010	0.0092	−0.0069	0.0019	−0.0043
	(0.0033)	(0.0094)	(0.0035)	(0.0098)
× 2011	0.0146	−0.0127	−0.0006	−0.0013
	(0.0070)	(0.0134)	(0.0045)	(0.0133)
× 2012–2013	0.0139	−0.0202	−0.0038	0.0102
	(0.0070)	(0.0169)	(0.0101)	(0.0190)
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.

vestment 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 III.C. 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 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 effective F-statistic of the first stage is 17.77, well above the critical value of 9.62 for 5% bias (Table 1). It is nevertheless reassuring that the LIML estimates are very close to their 2SLS counterparts. In row

Table B.5: Robustness

	β(2011)	SE	Shea R^2	Job-Years	SE(Job-Yrs)	Costs p. JY	N
(0) Baseline	2.49	0.88	0.11	4.11	1.71	24360	11200
Model Variants							
(1) LIML	2.62	0.94	0.11	4.31	1.82	23178	11200
(2) Weighted by labor force pop	2.17	0.70	0.16	3.55	1.44	28200	11200
(3) Cluster: labor market region	2.49	0.93	0.11	4.11	1.82	24360	11200
(4) West Germany only	2.65	0.91	0.13	4.38	1.75	22853	9072
(5) East Germany only	0.35	2.53	0.07	0.84	5.18	118609	2128
(6) Employment $\geq$ 25 years of age	1.68	0.76	0.11	2.90	1.54	34426	11200
Instruments							
(7) All school types separately	2.73	0.82	0.13	4.26	1.57	23483	11200
(8) All school types aggregated	1.96	0.89	0.08	3.86	1.88	25908	11200
(9) Instruments in 1995 (West Germany)	2.43	0.89	0.14	3.27	1.70	30570	9072
(10) Instruments in 1995 (East Germany)	−1.64	2.29	0.06	0.16	5.52	620458	2128
(11) Schools (2008) $\leq$ Schools (1995)	2.55	1.05	0.10	4.32	2.12	23151	9688
Controls							
(12) With short-time work	2.43	0.87	0.11	4.22	1.72	23706	11200
(13) With wage distribution (p25 p50 p75)	3.37	1.05	0.10	5.57	2.03	17965	11172
(14) With pop younger 18 (child support)	2.43	0.86	0.11	4.03	1.70	24818	11200
(15) With new cars	2.59	0.89	0.11	4.37	1.76	22863	11200
(16) With outcommuter	2.53	1.12	0.08	4.08	2.15	24528	11200
(17) With Bartik shocks (baseline: Q1 09)	2.56	0.65	0.11	4.87	1.29	20529	8000
(18) With industry structure	2.45	0.86	0.12	4.12	1.62	24272	9600
(19) With residential building construction	2.61	0.86	0.11	4.38	1.67	22855	11200
(20) With 2005 & 2009 election outcomes	2.42	1.07	0.09	3.68	2.01	27145	11200
(21) Additional age structure controls	4.06	1.34	0.07	6.97	2.55	14354	11200
(22) Additional age structure by gender	2.93	1.30	0.07	5.60	2.46	17863	11200
(23) Date $\times$ state FEs, area p.c.	2.91	0.79	0.13	5.40	1.56	18518	11200
(24) Date $\times$ state FEs, area p.c., area^2	2.92	0.79	0.13	5.48	1.56	18263	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 β(2011) reports the coefficient estimate of *Investments p.c.* $\times$ *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*.

(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.[8] 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 geographical and serial correlation of errors beyond county borders. This alternative of clustering leave the standard errors almost unchanged. Next, we split the sample according to whether the counties were part of the former West or East Germany. The results in rows (4) and (5) 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 (6) 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 (7) 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 (8), 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 are stable over time. We test this conjecture in rows (9) and (10) 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 historical stability of the number of schools (as shown in Figure A.3 in Appendix A.6) in

[8]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.

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 (11), 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 (12), 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.[9] 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 (13). The stimulus package also raised the tax and flat-rate bonuses for dependent children; row (14) 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 (15) we control for the number of newly purchased private cars per capita by county and year.[10] Finally, the government raised the commuting allowance. To account for this policy, row (16) 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 (17) includes quarterly "Bartik shocks" as an additional control variable (Bartik, 1991).[11] 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 needed to compute the shocks. The employment differences in this specification are thus estimated relative to employment in Q1 2009. Row (18)

[9] 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.

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

[11] 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).

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 (19) controls explicitly for private activity in the construction sector by controlling for the number of residential houses built in the respective years. Row (20) 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 (21) 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 (22) 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 (23). Row (24) 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

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

The only difference to the model underlying the main results 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

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

separately for each quarterly date $t \in \{\text{Q1 2007}, \text{Q2 2007}, \ldots, \text{Q3 2008}, \text{Q1 2009}, \ldots, \text{Q4 2013}\}$. 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 in Figures 3 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.

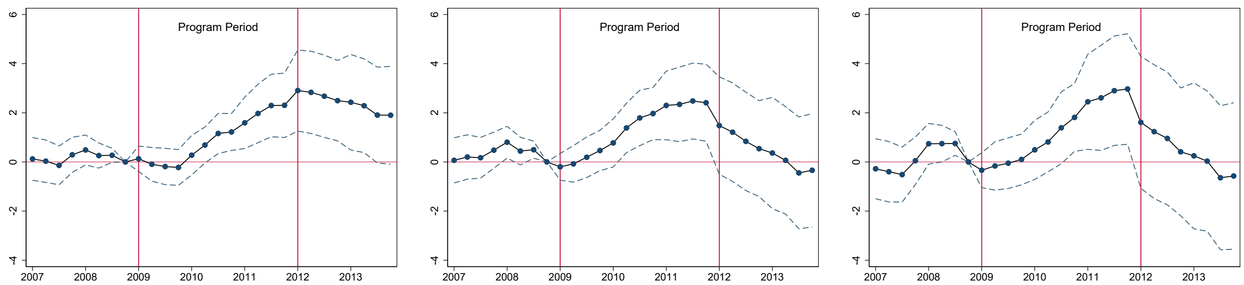
Table B.6: (Un)Employment Effects Relative to Pre-Program Averages

	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	0.44	0.13	0.18	0.11	−0.02
	(0.50)	(0.53)	(0.71)	(0.18)	(0.18)	(0.19)	(0.58)	(0.18)
× 2010	0.70	1.19	1.04	0.52	0.29	0.17	−0.59	−0.12
	(0.54)	(0.69)	(0.90)	(0.23)	(0.25)	(0.25)	(0.62)	(0.20)
× 2011	2.04	2.15	2.69	0.70	0.45	0.38	−1.43	−0.20
	(0.71)	(0.93)	(1.33)	(0.31)	(0.35)	(0.34)	(0.71)	(0.23)
× 2012–2013	2.30	0.17	0.36	0.73	0.08	0.18	−1.46	−0.16
	(0.98)	(1.22)	(1.63)	(0.45)	(0.48)	(0.48)	(0.81)	(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	2.48	3.10	3.49	1.66	0.87	0.73	1.92	0.34
SE Cumulative Job Years	1.56	1.96	2.67	0.66	0.72	0.72	1.75	0.55
Costs per Job Year	40292	32219	28624	60117	114778	136729	52191	297982
SE Costs per Job Year	25314	20385	21895	23915	94842	134935	47556	491849
Observations	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

(a) Parsimonious set of covariates (b) Intermediate set of covariates (c) Comprehensive set of covariates



Notes. This figure shows the differences in employment per €100'000 invested, β_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 3 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.

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.4), 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.3 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 $Employment\,p.c._{c,Q4\,2008}$ 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

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

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.[12] 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 effective F-statistics of the excluded instruments are above the critical values for 5% bias, indicating that the instruments are relevant.

References

Acconcia, Antonio, Giancarlo Corsetti, and Saverio Simonelli, "Mafia and Public Spending: Evidence on the Fiscal Multiplier from a Quasi-Experiment," *American Economic Review*,

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

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

	$1/4 \sum_{t=Q1\ 2009}^{Q4\ 2011}$ (Empl. Rate$_t$ − Empl. Rate$_{Q4\ 2008}$)						$1/4 \sum_{t=Q1\ 2009}^{Q4\ 2011}$ (UR$_t$ − UR$_{Q4\ 2008}$)	
	IV Estimates			OLS Estimates			IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Investments p.c. (in €100 Thd.)	2.77	3.96	3.73	0.64	0.15	−0.14	−1.86	0.05
	(1.31)	(1.69)	(2.22)	(0.60)	(0.62)	(0.60)	(1.61)	(0.60)
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. w. Univ.		10.56	13.20		13.41	14.87	−4.14	−5.57
		(3.72)	(3.54)		(3.00)	(3.06)	(3.27)	(2.95)
% Empl. w. Vocational T.		4.64	5.87		4.60	5.83	−6.63	−6.62
		(2.80)	(2.86)		(2.78)	(2.79)	(2.68)	(2.65)
Share School-Age Pop /100		11.16	7.93		4.73	4.95	5.18	8.41
		(6.73)	(6.51)		(5.70)	(5.86)	(5.02)	(4.62)
Universities p.c.			−413.34			−64.12		
			(299.75)			(193.14)		
Hospitals p.c.			51.27			84.63		
			(31.11)			(23.02)		
State × UrbanIndex FE	yes	yes	yes	yes	yes	yes	yes	yes
Shea Partial R^2	0.16	0.12	0.07	.	.	.	0.12	.
Effective F	27.04	18.43	10.70	.	.	.	18.43	.
Critical value 5% bias	13.54	9.39	13.84	.	.	.	10.13	.
Critical value 10% bias	9.10	6.65	9.28	.	.	.	7.08	.
Cumulative Job Years	2.77	3.96	3.73	0.64	0.15	−0.14	1.86	−0.05
SE Cumulative Job Years	1.31	1.69	2.22	0.60	0.62	0.60	1.61	0.60
Costs per Job Year	36135	25262	26842	155304	650265	-701864	53786	-1891431
SE Costs per Job Year	17103	10790	15973	144330	2619980	2953105	46450	21542189
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 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 *Investments p.c.* underlying 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.

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