More time, less crime? The effects of all-day school programs on juvenile delinquency*

****PRELIMINARY VERSION - PLEASE DO NOT CIRCULATE****

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Abstract

We examine the effect of a nationwide investment program to expand all-day school programs on juvenile delinquency. Using an instrumental variable approach and a combination of crime registry data, school funding records, and survey data, we find that the program reduced property crime, while it increased drug-related offenses, particularly among secondary school students. Our results therefore suggest that all-day school programs change the nature and shift the location of juvenile crime.

JEL classification: I2, J13, K42

Keywords: youth crime, all-day school, school attendance

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1 Introduction

Juvenile crime is a major political issue in many countries. Germany, where the number of minors and young suspects has been on the rise in recent years, is no exception. This results in significant social costs, such as costs in the judicial system, costs of imprisonment as well as material and immaterial costs to the victims of crime. For these reasons, the prevention of juvenile crime has the potential not only to improve young people's lifetime perspectives, but also to be an efficient method of reducing the social costs of crime.

It is often argued that one way to reduce juvenile crime is to increase school attendance. This argument is based on the belief that keeping children busy in a supervised environment will keep them out of trouble. However, previous literature suggests that this is only part of the truth, finding evidence that school incapacitation reduces property crime but increases violent crime in schools (see, e.g., Hansen et al., 2024; Jacob & Lefgren, 2003; Luallen, 2006). While this literature exploits various sources of variation in school attendance including school closure dates for teacher training (Jacob & Lefgren, 2003) or teacher strikes (Luallen, 2006), discontinuities at the beginning of a school year (Jones & Karger, 2023), changes in the number of school days per week (S. Fischer & Argyle, 2018), and changes in the minimum dropout age or compulsory school years (Anderson, 2014; Beatton et al., 2022; Lochner & Moretti, 2004; Machin et al., 2011), little attention has been paid to the impact of lengthening the school day on juvenile crime.

In this paper, we fill this gap by examining the impact of all-day school programs on juvenile crime in West Germany. Specifically, we analyze both the short- and long-term effects of all-day school programs on major crime groups such as property crime, violent crime and drug crime, as well as on more detailed types of crime. In addition to evaluating the average effects of all-day school programs, we distinguish between the effects of the programs on primary and secondary school-age children. To address problems arising from non-random selection into all-day programs, we exploit the exogenous nature of a national funding program in Germany in an instrumental variable (IV) approach. This program provided four billion euros for construction purposes over the period 2003-2009 and significantly increased the availability of all-day school programs.

Our empirical analysis is based on a registry data set of police-reported crime in two German federal states over the period 2005 to 2020. We link this data set to government reports containing lists of all schools that were funded as part of the investment program for each year of the program and information from the Federal Statistical Office of Germany on the number of all schools per county. Finally, we use the German Socio-Economic Panel (SOEP) for information on attendance in all-day school programs. This is important since participation is voluntary and schools do not necessarily offer a slot in their all-day program for every student.

Our preliminary results are as follows: First, using the SOEP data, we find that the instrument, i.e. the funding intensity within a county, is strongly correlated with attendance in all-day school programs. It, however, shows no significant association with other pre-reform county characteristics that might affect juvenile crime, which supports the validity of our empirical approach. Second, our reduced form results indicate that all-day school programs reduce the number of property crime, while they increase the number of drug crime. Specifically, under the assumption of linearity, our results suggest that the program led to a reduction in property crime of almost 10%, mainly driven by a reduction in theft, and an increase in drug crime of 33% compared to pre-reform levels. Although violent crime rates also show an upward trend, the effect is not statistically significant. Third, if we distinguish between primary and secondary school children, we find that this pattern is more pronounced for the latter group. In line with previous literature, our results therefore indicate that extending the school day may change the type of crime and shift the crime scene from the street to the school for this age group. In contrast, primary school students experience a general decline in criminal behavior, though not all coefficients reach statistical significance. In the coming weeks, we will shift the focus to a more comprehensive examination of the impact of these changes in crime, with particular attention to the effects on long-term criminal behavior and the situation of victims.

Our paper primarily contributes to the empirical literature on the short-run incapacitation effects of schooling on crime.[1] As noted above, this literature studies the relationship between school and juvenile delinquency using different sources of variation in school attendance. Yet, only few studies focus on the impact of lengthening the school day on juvenile crime. Berthelon

[1] For an overview of the extensive literature on schooling and crime, including the effect of education on crime, see for example Lochner & Moretti (2004).

& Kruger (2011) are among the few to investigate this margin of adjustment. They exploit a reform in Chile that lengthened the school day from half-day to all-day and find that keeping youths off the street for a longer time of the day decreases crime. Similarly, Dinarte-Diaz & Egana-delSol (2024) conducted a randomized experiment in El Salvador setting up an after-school program that combined a behavioral intervention with extracurricular activities. They find that this intervention promoted emotion regulation and thus reduced participants' violent behavior both in and out of school. We contribute to the scarce literature, which so far focuses on Southern American countries, by looking at a reform in Germany, a large European country, that expanded all-day schooling. Germany is an interesting setting for this research question since half-day schooling is still often the norm in this country. Moreover, the reform introduced extra time at school in the afternoon while the school curricula as such did not change. This setting allows us to rule out that any effects identified in our study might be driven by adjusted school curricula. In addition, we plan to go beyond the analysis of the suspects and focus on the victims of crime. Finally, we analyze the impact on children of primary school age, who in most cases do not receive much attention because crime rates are significantly lower in this age group than for adolescents. Nevertheless, we expect that adequate afternoon supervision will have a positive effect in the long term.

The remainder of the paper is organized as follows. Section 2 provides some background information and lays out the institutional setting. Section 3 describes our empirical approach. Section 4 describes the data. Section 5 and Section 6 present our main results and a variety of validity checks. Section 7 concludes.

2 Background and context

2.1 All-day school programs in Germany

In Germany, all-day school is organized under the responsibility of the school, with the minimum requirement that it offers an all-day program of at least seven hours a day for at least three days a week. Most schools, however, provide far more extended programs, both in terms of days and hours (N. Fischer et al., 2013; KMK, 2023).

A special feature of all-day programs in Germany is that schools can decide whether attendance is compulsory or not. In fully integrated schools, all children are required to attend school in the afternoon. In the partially integrated form, only some children (e.g. certain classes or cohorts) are obliged to attend in the afternoon. Schools organized in the most common form—the open form—offer a voluntary program in which all children attend regular classes in the morning, while only participants in the all-day program stay in school for lunch and activities. Enrollment for these non-integrated programs takes place at the beginning of the year. If accepted, parents usually have to pay for lunch and often a small participation fee (N. Fischer et al., 2013; KMK, 2023).

The programs are supposed to be linked to the syllabus. However, this can also be achieved through cooperation with extracurricular (cultural) partners (BKJ, 2006; KMK, 2023). All in all, most all-day schools, therefore, offer supervised leisure time and homework assistance rather than a structured program aimed at improving learning outcomes, which is also reflected in the fact that the majority of the staff in the afternoon has no teaching-related qualification (Seidlitz & Zierow, 2020; StEG, 2010). This makes all-day school programs in Germany an interesting setting to study as it allows us to rule out that the effect of lengthening a school-day on juvenile crime is driven by adjusted school curricula.

2.2 The federal investment program and the all-day school expansion

In the early 2000s, the German government decided to introduce a nationwide "Investment Program for the Future, Education and Care" (IZBB) to encourage the states to invest in all-day school programs. This introduction was based on three main objectives: First, in response to the 2001 PISA report, the program was intended to improve student performance. Second, given the low participation of mothers in the labor market, the program aimed at a better reconciliation of work and family life. Finally, the program should address inequalities in learning opportunities (BMBF, 2009).

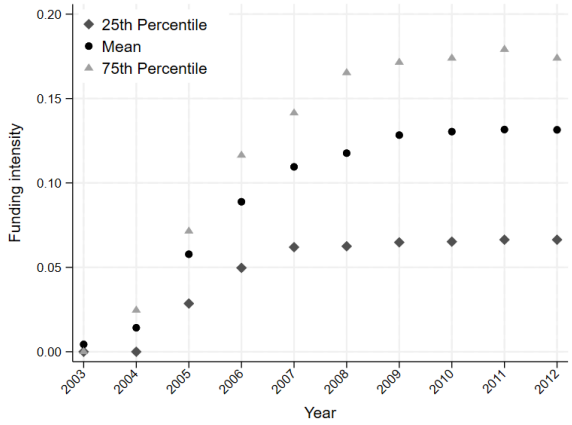
The total investment volume of around four billion euros was restricted to construction purposes, i.e., the creation of new and the restructuring of old all-day schools. The running costs of all-day school programs were covered by additional investments by the states and

communities. Specifically, the federal states invested a further €400 million in the expansion of all-day school programs (Blossfeld & von Maurice, 2019). The subsidies were allocated between 2003 and 2009 to more than 8,200 schools (including 542 in Baden-Württemberg and 442 in Hessen) on the basis of an application process (BMBF, 2009; SPI NRW, 2010). The program was successful in raising the share of students attending an all-day school programs: while only 8.2% of all children attended such a program in 2002, the proportion rose to 23.7% in 2012 (see Figure A1 in the Appendix).

While the subsidies were distributed proportionally to the number of students across states, the investment allocation and thus the expansion of all-day slots varied considerably from one county to another (BMBF, 2003). Looking at Figure 1, which shows the 25th percentile, the mean, and the 75th percentile of the funding intensity in our sample, we find few differences in funding at the beginning of the investment period. In the years after 2003, however, both the overall funding intensity and the disparities between counties increased sharply.

There are several reasons for these regional differences. On the one hand, this may be due to specific county characteristics, such as economic resources or demographic needs, which lead to a higher intensity of funding. For example, one might be expect that schools in counties with higher female employment rates would have a greater incentive to apply for a subsidy. There may also be an intention to allocate more funding to districts with higher proportions of disadvantaged children. On the other hand, the application process and the specific funding conditions also led to spatial differences: for example, in order to receive funding, the school director and the school committees first had to develop a concept for the use of the funds, which not only had to be approved by the school council, but often also by the parents' representatives. The local authorities then had to submit this concept to their respective federal states, which were responsible for selecting suitable all-day programs. Factors such as the motivation of an individual headteacher, space constraints, school conference dates or delays in coordination between the parties involved may therefore have affected the approval and the timing of the funding (BKJ, 2006; Nemitz, 2016).

Figure 1. The evolution of the funding intensity



Notes: The funding intensity is defined as the county-specific ratio of schools funded by the IZBB investment program. The 25th percentile, the sample mean, and the 75th percentile of the funding intensity are shown separately.

All in all, the regional variation is thus not only due to differences in county-specific characteristics, which may also influence changes in juvenile crime (and which we therefore need to control for), but also due to exogenous factors unrelated to the criminal behavior of school-age children. To further support this argument, which is the basis of our identification strategy, Table A3-A5 shows that there is no evidence of a general strategic provision of the subsidies.

3 Empirical approach

The aim of this study is to identify the causal effect of all-day school programs on juvenile crime. To overcome the problem of non-random selection into all-day school programs, we follow the idea of Dehos & Paul (2023) and exploit the regional and temporal variation in the provision of subsidies for the expansion of all-day school programs through the IZBB in an instrumental variable (IV) approach.[2] In this approach, the instrument Z is defined as the proportion of schools in a county funded by the investment program. Our main focus is on the reduced form, which is defined as follows:

$$C_{ct} = \phi_c + \tau_t + \rho Z_{ct} + \beta' X_{ct} + \varepsilon_{act} \tag{1}$$

[2]See Dehos & Paul (2023) fo a detailed description of the three forms of selection into all-day school programs.

where C_{ct} is the age-specific crime rate in county c in year t, ϕ_c are county fixed effects that capture unobserved time-constant heterogeneity across counties, and τ_t are year fixed effects that capture changes in crime over the years common to all counties.X_{ct} is a vector of time-varying county level control variables. In our preferred specification this vector includes unemployment rates to capture local labor market conditions and poverty, GDP per capita to account for a county's financial ability to cover the running costs of all-day programs, and the share of juvenile males since young men traditionally make up the majority of juvenile crime. Moreover, it includes the share of juvenile foreigners and the population density as these factors may influence the funding decisions. It is, however, reassuring that our results are robust to the inclusion of (different sets of) control variables.

Our IV approach relies on two identifying assumptions that must hold for a causal interpretation of the results. First, the instrument Z_{ct} must be relevant. That is, it must be correlated with the probability of attending an all-day school program. This assumption is testable and we detect a strong and significant relationship (see Table 1). Second, the instrument must be valid, and thus, conditional on the covariates included in the model, independent of unobservable factors that are correlated with age-specific crime rates. While this assumption cannot be tested directly, we are able to examine the extent to which funding is related to observable county characteristics that may also affect juvenile crime. We do this by regressing different measures of a county's funding intensity on covariates that are measured in 2002 and thus just before the start of the program. Looking at the results in Table A3-A3 in the Appendix, we find no evidence for a strategic provision of the funds. Consequently, there is also no evidence that our instrument is systematically correlated with specific county characteristics. Nevertheless, in Section 6 we present a number of robustness checks to further strengthen the validity of our empirical approach.

4 Data

4.1 Crime data

Our main analysis relies on an anonymised registry data set on police-reported crime in Germany. Specifically, the data set includes all police-reported cases from the German states of Baden-Württemberg and Hesse in the period 2005 to 2020, provided by the respective state criminal investigation offices (Landeskriminalämter). For all cases, we have detailed data on the type of crime, the date of the crime and the place where the crime occurred. The data set also contains information on the sex and age of the suspect and the victim (if applicable). Although we do not have data from all West German states, our data set is large enough to allow intensive empirical analysis, as the population of Baden-Württemberg is around eleven million (comparable to Sweden) and the population of Hessen is around six million (comparable to Denmark or Norway). Thus, our data set includes on average more than 900,000 criminal cases per year.

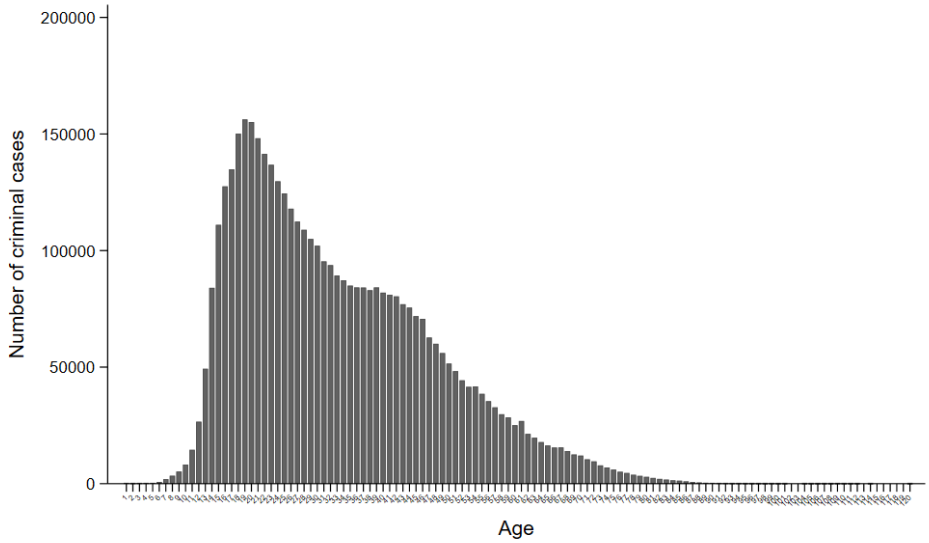
Since our analysis requires age-specific crime rates and is based on the county-level, we restrict our crime data to offenses in the specified age group (and thus to offenses for which we have information on the suspect's age). Moreover, we restrict our data to the years 2005-2012 to take into account that the last IZBB funds were not distributed until 2009, so that the corresponding all-day school program could only start after the end of the investment period.[3] For the age group of 6 to 16-year-olds, this leaves us with 432,367 criminal offenses (see Figure 2). We then collapse the data to county-year observations—taking into account the time structure of the German school year—and calculate county-age-specific crime rates using additional data on population by age and year at the county level provided by the German Federal Statistical Office.

4.2 Investment program data

For information on the nationwide investment program and thus the construction of the instrument, we use reports from the Federal Ministry of Education and Research (see BMBF

[3] We would have limited our sample to the years 2003-2012 if crime data had been available for more years.

Figure 2. Criminal cases by age



Notes: The figure includes all criminal cases in the German states of Hesse and Baden-Württemberg over the period from 2005 to 2012.

(2010a,b)). These reports obtain a list of all schools that were funded as part of the IZBB for each year of the program. We then use the information on the location of the school contained in the reports to identify the county in which the school is located and sum the number of schools that received funding over the course of the investment program up to year t for each county. This is based on the assumption that a school continues to offer an all-day program after receiving government funding (Dehos & Paul, 2023). Next, we divide the county-specific number of schools funded by the IZBB by the number of schools in the county. This ratio measures the program's county-specific funding intensity in a given year and serves as the instrument (see Figure 1 for the the evolution of this instrument over time). Finally, we merge the funding information and the instrument with the crime data to obtain the data set for our main analysis.

4.3 Attendance data

To test whether our first key identification assumption—namely that our instrument is correlated with full-day attendance—holds, we need data on attendance rates. Unfortunately, administrative data on attendance rates are only available at the state level and therefore cannot

be used. We thus build a second data set for our empirical analysis that is based on the German Socio-Economic Panel (SOEP). The SOEP is an annual representative longitudinal household survey that provides rich information on individual, socio-economic and household characteristics (see Wagner et al., 2007). Specifically, we use two questions on the type of school and the children's afternoon attendance that are asked in the household questionnaire. Moreover, we use the remote access in order to add county-specific information including the information on the IZBB funding and the instrument to the data.[4]

Our sample of interest contains school children who are 6 to 16 years old. Moreover, we use the years 2003-2012 based on the same reasoning as before. Due to low numbers of observations per county, we further keep all West German counties in this data set. We thus end up with a sample of 9,213 children between the ages of 6 and 16 years and 28,453 child-year observations in 225 counties over 10 years. Given the differences between the two data sets used for our empirical analysis, we refrain from estimating a two-sample two-stage least squares (TS2SLS) approach. Instead, we use the SOEP data to provide proof that our instrument affects all-day school participation and focus on the reduced form to identify the effects on juvenile crime.

5 Results

5.1 The effect of funding on program attendance

We start our analysis with estimating the effect of the instrument on the endogenous variable and thus with the effect of IZBB funding on all-day school program attendance. Table 1 shows the results based on the SOEP data set and indicates that the county-specific ratio of schools funded by the IZBB program is strongly related to all-day program participation. To be precise, if we only control for year and county fixed effects, the effect of the instrument Z on the probability of participation in an all-day school program is estimated to be 15 percentage points (see column (1)). This effect is highly significant and the F-statistic is sufficiently large to

[4]Socio-Economic Panel (SOEP), data for years 1984-2021, version 38.1 (with county identifiers), SOEP, 2023, doi:10.5684/soep.core.v38.1r.

Table 1. The effect of the investment program on all-day school attendance (SOEP)

	(1)	(2)
Ratio of schools funded by grants	0.15***	0.13***
(*Instrument*)	(0.04)	(0.04)
Observations	28,453	28,443
F-statistic	28.24	20.49
Year FE	Yes	Yes
County FE	Yes	Yes
County covariates	No	Yes

Notes: Regressions are run over the period from 2003-2012. The sample consists of children aged 6-16 years in the SOEP. County covariates include unemployment rate, GDP per capita, share of juvenile foreigners, share of juvenile males and the population density. Standard errors clustered at the county level are shown in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

suggest that our estimate does not suffer from a weak instrument problem. Moreover, the coefficient only slightly decreases if we add county-specific control variables in the model. For this preferred specification, we find that a one percentage point increase in the county-specific ratio of schools funded by the IZBB, increases attendance rates by 0.13 percentage points.

Overall, the results in Table 1 thus suggest that the investment program was an important driver of the expansion of all-day schools in West Germany. This finding underscores the relevance of our instrument. Furthermore, the finding contradicts the implicit assumption that schools which did not receive IZBB funding were systematically compensated by alternative funding sources.

5.2 Short-run effects of all-day school programs on juvenile crime

We continue with the reduced-form effects estimated using Equation 1 to answer the question whether all-day school programs affect juvenile crime. Table 2 shows the results for offenses committed by children between the ages of 6 and 16, grouped by main offense group. We have made a conscious decision not to include children over the age of 16 in the analysis, as all-day schooling programs are much less relevant to this age group.

In line with previous literature, we find that an increase in the proportion of schools funded by the IZBB leads to a significant reduction in property crime, while it leads to an increase in

Table 2. The effect of the investment program on juvenile crime by main offense group

	(1) Violent crimes	(2) Property crimes	(3) Drug crimes	(4) Minor crimes
Ratio of schools funded by grants	14.44	-123.35**	36.09**	-15.31
	(17.84)	(59.32)	(14.44)	(30.57)
Observations	560	560	560	560
R^2	0.28	0.35	0.43	0.09
Mean	40.28	167.79	14.43	45.22
Year-FE	Yes	Yes	Yes	Yes
County-FE	Yes	Yes	Yes	Yes
County covariates	Yes	Yes	Yes	Yes

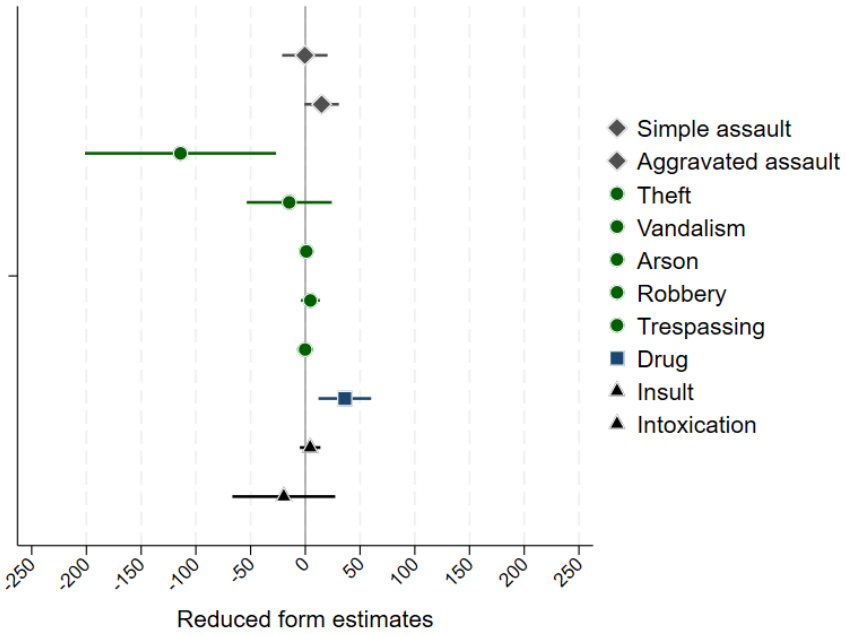
Notes: The table shows reduced form effects estimated using Equation 1. The dependent variables are the numbers of reported crimes per 10,000 residents aged 6-16. Regressions are run over the period from 2005-2012. County covariates include unemployment rate, GDP per capita, share of juvenile foreigners, share of juvenile males and the population density. Standard errors clustered at the county level are shown in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

the number of drug crimes. Our findings also support the finding in the literature that school incapacitation can increase violent crime through a 'concentration effect', which leads to an increase in the number of interactions between pupils and thus an increase in the likelihood of physical confrontations. The effect of the IZBB funding on violent crime is, however, not statistically significant. Assuming linearity, we can interpret our results in terms of magnitude as follows: given that on average almost 13% of all schools in a county received funding through the IZBB (see Figure 1), the program reduced the number of property crimes by 16,04 cases per 10,000 residents aged 6 to 16. The program increased the number of drug crimes by 4,69 cases per 10,000 residents aged 6 to 16. Compared to pre-reform means, the program therefore lead to a reduction in property crime by nearly 10% and an increase in drug crime by 33%.

To get a better understanding of these findings, we go one step further and examine the effect of the investment program on more detailed offenses. The results are plotted in Figure 3 and suggest the reduction in property crime is mainly driven by a reduction in theft. Moreover, as we assume that the effects differ for children in primary and secondary schools, we conduct the same analysis for children aged 6 to 10 years and 11 to 16 years. The results in Tables A1 and A2 support this hypothesis: while the coefficients of the effect of the IZBB program on the

crime rates of primary school children are negative for all offense groups, the pattern described above becomes even clearer for children of secondary school age.

Figure 3. The effect of the investment program on juvenile crime (detailed offenses)



Notes: The estimates plotted in the figure are based on reduced form regressions including year fixed effects, county fixed effects, and time-varying county covariates. The dependent variables are the numbers of reported crimes per 10,000 residents aged 6-16. Regressions are run over the period from 2005 to 2012. Standard errors clustered at the county level.

5.3 Long-run effects of all-day school programs on juvenile crime

In the previous section, we focused on the short-run effects of all-day school programs on juvenile crime. In the following, we want to go one step further and explore whether all-day school attendance affect criminal behavior later in life.

[Work in progress]

6 Validity checks

We plan to implement at least three further specification checks to test the validity of our empirical approach. First, we examine the effect of all-day schools on the crime rate of those slightly above school age. Second, we estimate the effect of all-day schools on weekends and

school holidays. If we find no effects, these two specification checks support the validity of our findings. Finally, as our empirical approach including county fixed effects resembles a difference-in-differences specification with a continuous treatment variable, we want to take into account recent advances in the two-way fixed effects literature. We therefore plan to follow the suggestions of Callaway et al. (2024) and Cook et al. (2022) to show that this is not a potential problem in our case.

[Work in progress]

7 Conclusion

This paper examines the impact of all-day school programs on juvenile crime in West Germany, leveraging the exogenous variation introduced by a national funding program that expanded all-day schooling between 2003 and 2009. Using an instrumental variable approach and a unique combination of registry crime data, school funding records, and survey data, we provide evidence on the effects of extended school hours on different types of crime.

Our results indicate that increasing school attendance through all-day programs leads to a substantial reduction in property crime, with theft accounting for most of this decline. However, we also find evidence of an increase in drug-related offenses, suggesting that extended school hours may change the nature of criminal activity rather than eliminate it altogether. In the coming weeks, we intend to extend our analyses and investigate whether these changes in the criminal behavior of young people have longer-term effects on their delinquency. As our findings also suggest that crime is shifting from the streets to schools, we also want to look at changes in victimization.

Our findings have important policy implications as they reinforce the view that structured, supervised environments play a crucial role in shaping juvenile behavior. The reduction in property crime suggests that additional school hours have an incapacitation effect, deterring students from engaging in criminal activity outside of school. However, the increase in drug-related offenses highlights the need for complementary policies or additional investments within all-day school programs, such as drug prevention programs, behavioral counseling, or mentorship

initiatives, to mitigate the unintended consequences of increased time spent in the school environment and thus to maximize the benefits of extending the school day.

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A Appendix

Table A1. The effect of the investment program on juvenile crime by main offense group
Children aged 6-10

	(1) Violent crimes	(2) Property crimes	(3) Drug crimes	(4) Minor crimes
Ratio of schools funded by grants	-7.47	-21.78*	-0.02	-11.55
	(5.01)	(12.99)	(0.03)	(9.71)
Observations	560	560	560	560
R^2	0.06	0.14	0.02	0.05
Mean	3.08	24.34	0.01	5.14
Year-FE	Yes	Yes	Yes	Yes
County-FE	Yes	Yes	Yes	Yes
County covariates	Yes	Yes	Yes	Yes

Notes: The table shows reduced form effects estimated using Equation 1. The dependent variables are the numbers of reported crimes per 10,000 residents aged 6-1o. Regressions are run over the period from 2005-2012. County covariates include unemployment rate, GDP per capita, share of juvenile foreigners, share of juvenile males and the population density. Standard errors clustered at the county level are shown in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A2. The effect of the investment program on juvenile crime by main offense group
Children aged 11-16

	(1) Violent crimes	(2) Property crimes	(3) Drug crimes	(4) Minor crimes
Ratio of schools funded by grants	32.55	-201.62*	64.71**	-14.23
	(31.63)	(103.68)	(26.23)	(52.96)
Observations	560	560	560	560
R^2	0.31	0.36	0.41	0.10
Mean	69.42	280.67	25.63	76.47
Year-FE	Yes	Yes	Yes	Yes
County-FE	Yes	Yes	Yes	Yes
County covariates	Yes	Yes	Yes	Yes

Notes: The table shows reduced form effects estimated using Equation 1. The dependent variables are the numbers of reported crimes per 10,000 residents aged 11-16. Regressions are run over the period from 2005-2012. County covariates include unemployment rate, GDP per capita, share of juvenile foreigners, share of juvenile males and the population density. Standard errors clustered at the county level are shown in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A3. Pre-reform correlations
Funding measure: Total cumulative funding

	(1) GDP per capita	(2) Median income	(3) Unemploy-ment	(4) Female employment	(5) Share of juv. foreigners	(6) Juvenile maleness	(7) Urban areas
Funding measure	14.36	-188.95	0.01	-0.03	0.05	0.00	0.06
	(11.88)	(207.98)	(0.02)	(0.04)	(0.06)	(0.01)	(0.43)
Observations	70	70	70	70	70	70	70
R^2	0.69	0.74	0.39	0.36	0.71	0.09	0.72

Notes: Following Pei et al. (2019), we regress pretreatment characteristics at the county level on different measures of the funding intensity. Each coefficient of the table stems from a separate regression, which includes all remaining pretreatment characteristics as further controls. Robust standard errors are given in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A4. Pre-reform correlations
Funding measure: Average yearly funding intensity

	(1) GDP per capita	(2) Median income	(3) Unemploy-ment	(4) Female employment	(5) Share of juv. foreigners	(6) Juvenile maleness	(7) Urban areas
Funding measure	102.06	-1166.79	0.10	-0.23	0.43	0.01	-0.19
	(86.79)	(1518.77)	(0.14)	(0.32)	(0.43)	(0.04)	(3.15)
Observations	69	69	69	69	69	69	69
R^2	0.69	0.74	0.38	0.36	0.71	0.08	0.72

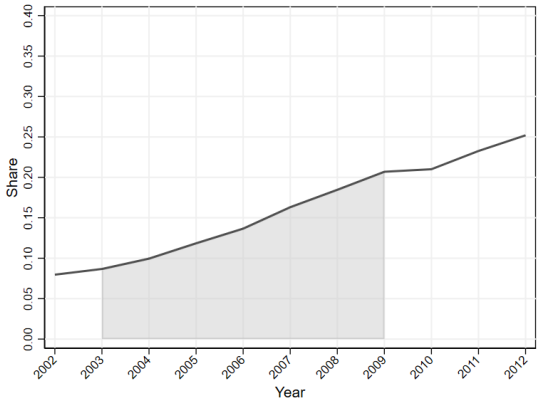
Notes: Following Pei et al. (2019), we regress pretreatment characteristics at the county level on different measures of the funding intensity. Each coefficient of the table stems from a separate regression, which includes all remaining pretreatment characteristics as further controls. Robust standard errors are given in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A5. Pre-reform correlations
Funding measure: Average funding per student (EUR)

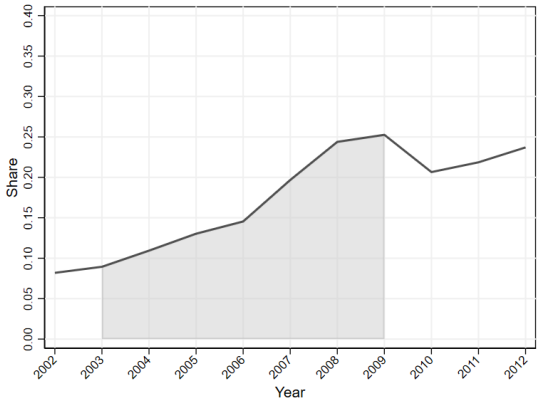
	(1) GDP per capita	(2) Median income	(3) Unemploy-ment	(4) Female employment	(5) Share of juv. foreigners	(6) Juvenile maleness	(7) Urban areas
Funding measure	882.11	-9832.26	-0.92	-2.46	-0.63	-0.19	-29.49
	(1559.61)	(27027.71)	(2.43)	(5.72)	(7.69)	(0.72)	(55.39)
Observations	69	69	69	69	69	69	69
R^2	0.68	0.74	0.38	0.35	0.70	0.08	0.72

Notes: Following Pei et al. (2019), we regress pretreatment characteristics at the county level on different measures of the funding intensity. Each coefficient of the table stems from a separate regression, which includes all remaining pretreatment characteristics as further controls. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

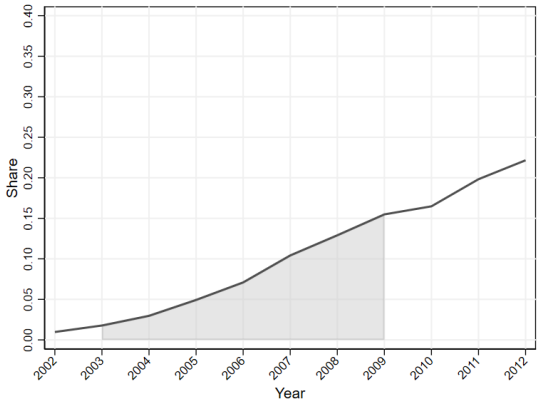
Figure A1. All-day school attendance by school type over time



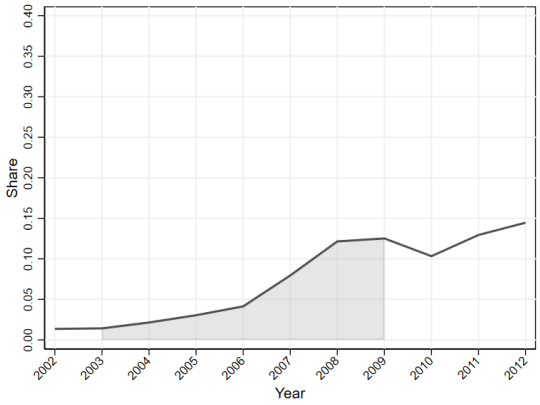
(a) All schools (West Germany)



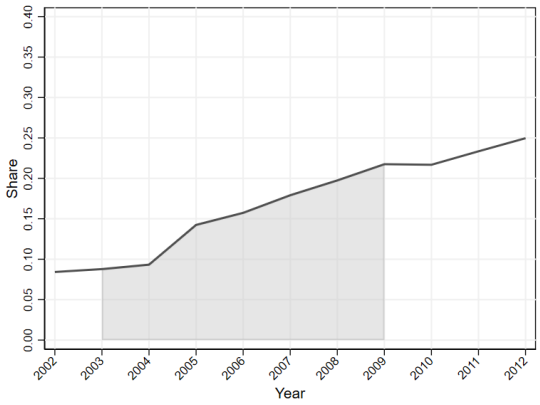
(b) All schools (BW & HE only)



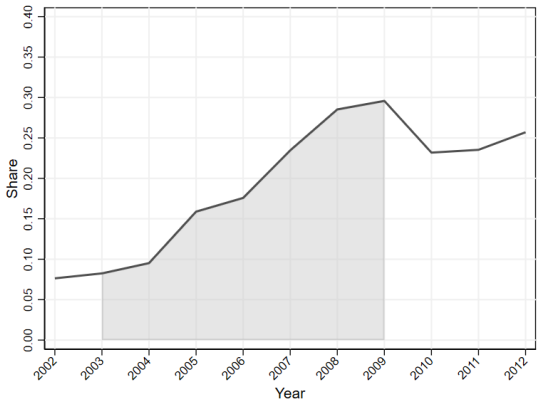
(c) Primary schools (West Germany)



(d) Primary schools (BW & HE only)



(e) Secondary schools (West Germany)



(f) Secondary schools (BW & HE only)

Notes: Own calculations based on data provided by the Konferenz der Kultusminister (KMK) and the Federal Statistical Offices.