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Permanent School Closures and Crime: Evidence from Scotland*

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Abstract

In this article we study the effects of permanent school closures on crime. We leverage the closure of over 300 schools in Scotland between the school years 2006/07 and 2018/19, and employ a staggered difference-in-differences design on a matched sample. We find that neighbourhoods affected by school closures experience a reduction in crime of about 9% of a standard deviation, relative to areas where schools remained open. This effect is mainly driven by a reduction in violent and property crimes. We provide evidence on several mechanisms explaining the negative crime effect, such as changes in neighbourhood composition and reductions in school-level segregation.

JEL classification numbers: I38, R20, K42
Keywords: Crime, School Closures, Neighbourhoods

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1 Introduction

School closure, or consolidation, is a widely adopted practice in developed (Berry and West, 2010, Engberg et al., 2012, Beuchert et al., 2018) and developing countries alike (Dongre and Tewary, 2020).[1] For example, in the US, on average 2% of schools closed every year between 2003 and 2013, disrupting over 200,000 pupils every year (Gallagher and Gold, 2017). Faced with declining school performance and depopulation, local governments consolidate by closing under-enrolled schools and relocating pupils elsewhere, or by merging two or more schools.[2] In other cases, such as the recent RAAC scandal in the UK, schools are forced to close due extraneous factors, such as their buildings not meeting safety regulations.[3] Proponents of the practice argue that permanent closures boost school efficiency, while opponents claim that since closures are unevenly distributed across space, it is the most deprived communities who bear most of the burden (Tieken and Auldridge-Reveles, 2019).

Pupils switching schools as a result of a school closure may experience a change of educational environment (Holmlund and Böhlmark, 2019), for example due to moving to bigger schools (De Haan et al., 2016). An important strand of the academic literature investigates the effects that permanent school closures have on the educational outcomes of both displaced pupils and those in receiving schools (see Berry and West, 2010, Engberg et al., 2012, Brummet, 2014, Beuchert et al., 2018, Steinberg and MacDonald, 2019, Taghizadeh, 2020*a*,*b*, Bifulco and Schwegman, 2020). It is widely understood that school closures tend to benefit displaced pupils when these are relocated to better performing schools. It is less established whether pupils in receiving schools are negatively affected, and if such effects persist over time. To date, only Steinberg and MacDonald (2019) provide evidence of detrimental effects on school behaviour, both for displaced pupils and those in receiving schools. Despite extensive evidence linking school exclusions and criminality later in life (see, e.g. Bacher-Hicks et al., 2019), only a small literature focuses on how school closures affect

[1]As permanent school closures are often paired with two or more schools merging into a single school, we use the terms 'consolidation' and 'closure' interchangeably.

[2]See for example, 'More than 90 English primary schools to close or face closure for lack of pupils', The Guardian, 29-05-2023.

[3]See for example, 'UK public buildings feared to be at risk of collapse as concrete crumbles', The Guardian, 14-06-2023.

local crime rates (Steinberg et al., 2019, Brazil, 2020).

The theoretical channels linking schools and crime yield ambiguous predictions on the effects of closures on local crime rates. A number of theories from sociology and criminology stipulate a positive association between the presence of schools and crime, and thus would predict reductions in crime rates after school closures. For example, social disorganisation theory posits that the presence of multiple agents (residents, pupils, teachers, etc) around schools makes it more difficult to establish informal systems of control that prevent crime, especially among young people (Shaw and McKay, 1942). Similarly, routine activity theory suggests that schools increase criminal opportunities by concentrating potential targets (Cohen and Felson, 1979). On the other hand, if school closures leave former school buildings vacant, the resulting perception of decay can attract more crime to the area, in line with the broken windows theory of crime (Kelling et al., 1982). Displacement of pupils (and residents) can also lead to increased crime in other areas, due to, for example, pupils having to cross rival gang boundaries (Hagedorn, 2017). The small empirical literature on the topic is mostly focused on case studies from large U.S. cities, and finds either negative (Steinberg et al., 2019) or ambiguous (Brazil, 2020) crime effects, whereby the sign of the crime effect depends on the type of land use after a closure.

In this paper, we leverage a large school consolidation effort that took place in Scotland between 2006 and 2020, which resulted in the closure of over 350 schools, circa 10% of the entire stock. We build a panel data set of all 6,976 neighbourhoods (Data Zones) in Scotland, spanning from the school year 2006/07 to 2018/19. We combine recorded crime data with the geo-referenced episodes of school closures that occurred during our thirteen year sample period. As the decision of which schools to close is not random, a simple comparison of neighbourhoods with closed and open schools will most likely return biased estimates. For identification, we thus leverage variation in the timing of school closures within a difference-in-differences (DiD) framework.

Recent work has stressed the limitations of using the standard two-way fixed effects (TWFE) approach when estimating DiD models with variation in treatment timing (see Roth et al., 2022 for a review). These arise from the possibility that treatment effect heterogeneity leads to the inaccurate, and at times even negative, weights being assigned to group and period specific effects in the overall treatment effect (the Average Treatment Effect on the Treated, or ATT). To overcome this issue, we adopt Callaway and Sant'Anna's 2021 approach (CSDiD), as well as the two-stage difference-in-differences (DiD2s) approach developed by Gardner (2022). These allow us to estimate ATT effects that are robust to potential treatment effect heterogeneity under standard DiD assumptions (parallel trends and no anticipation effects). Moreover, as neighbourhoods with closed schools are systematically different from other neighbourhoods, we estimate these models on a matched sample. Specifically, we use Mahalanobis nearest neighbour matching, based on baseline covariates from the 2001 census, to find neighbourhoods that could act as suitable control units to compare neighbourhoods with school closures to.

We find that neighbourhoods affected by school closures experience a reduction in overall crime of about 4 crimes per 1,000 people annually, relative to areas where schools remained open. This reduction corresponds to roughly 9% of a standard deviation, and pertains to violent and property crimes alike. We find no evidence of crime spillovers in contiguous areas, or in the areas to where pupils are displaced.

Our work carries a number of contributions. To the best of our knowledge the only comparable studies on the effects of school closures on crime are those by Steinberg et al. (2019) and Brazil (2020).[4] The former find large reductions in violent crimes in Philadelphia following school closures, while the latter finds that the sign of the crime effect is sensitive to the type of land use on the school site following closures in the city of Chicago. In Steinberg et al. (2019)'s case, closures were specifically targeting what the authors define as '*chronically underperforming*' schools. Moreover, the authors specify how their context is characterised not only by lower academic achievement, but also higher levels of youth violence relative to statewide averages. Our context is different for two main reasons. First, school closures are largely prompted by shifts in population, as well as the need to provide better buildings, rather than by school outcomes (Scottish Government, 2023*a*). Second, the two contexts differ considerably in terms of average crime rates, where in the Scottish

[4]We acknowledge the work by Brinig and Garnett (2009), who look at the effect of closing parochial schools in Chicago, and MacDonald et al. (2018) who investigate the effect of opening public chartered schools in Philadelphia. While valuable contributions, we believe their work deals with a context and policy that differs considerably from ours. Specifically, they explore closures of very specific types of schools, which only involve a minority of the school population.

case crime rates are lower and closer to national averages.[5] The Chicago closures examined in Brazil (2020) instead are closer to ours as these do not specifically target underperforming schools. However, unlike his context where closed school buildings either remained vacant, hosted a merger or were repurposed, to the best of our knowledge in Scotland most of the former school buildings remained vacant and were demolished at a later time.

Moreover, to disentangle potential mechanisms, we look at the effects of school closures beyond crime, and find that they are associated with changes in the socio-economic composition of the original neighbourhood. We therefore provide an alternative mechanism to the pupils-as-offenders argument outlined in Steinberg et al. (2019), whereby the main channel behind crime reductions is pupil offenders leaving a particular area after a closure. We also show that where school closures are adopted more frequently over the sample period, school-level segregation –as measured by the number of pupils eligible for free school meals– decreases by as much as 5% of a standard deviation. While this finding does not have a causal interpretation, it is consistent with the literature that documents a positive, causal, link between school-level segregation and crime (Weiner et al., 2009, Billings et al., 2014, 2019). Also, by using national (as opposed to city-level) data, we are able to capture a variety of contexts, and contribute more generalisable evidence to the small literature on this topic. Finally, this is the first study, to our knowledge, to examine school closures and crime in a UK (and European) context. The only other study on closures in a European context we are aware of is Di Cataldo and Romani (2023), from Italy, looking at the effect of closing undersized schools on population and income.

The article is structured as follows. Section 2 outlines the conceptual and institutional background. Section 3 presents the data and describes the sample construction. Section 4 describes the empirical strategy. Section 5 presents the results. Section 6 concludes.

[5]According to the statistics presented by the authors, within the period 2006-2012, Philadelphia's annual total crime rate is about 128 per 1,000 people (Steinberg et al., 2019). In Scotland instead, the same measure amounts to circa 30 crimes per 1,000 people. Even where crime is more prevalent, for example in the city of Glasgow, the rate is 'only' about 60 crimes per 1,000 people.

2 Background

2.1 Conceptual Background

School closures have attracted considerable attention among scholars, especially in terms of its effects on pupil achievement (Berry and West, 2010, Engberg et al., 2012, Brummet, 2014, Beuchert et al., 2018, Steinberg and MacDonald, 2019, Taghizadeh, 2020*a*,*b*, Bifulco and Schwegman, 2020). In addition, schools are a form of land use. An extensive literature studies the link between changes in land use and crime, i.e. on how crime changes near vacant commercial premises or homes (Chang and Jacobson, 2017, Ellen et al., 2013, Cui and Walsh, 2015), regenerated or demolished public housing estates (Aliprantis and Hartley, 2015, Spader et al., 2016, Sandler, 2017, Borbely and Rossi, 2023), closed police stations (Facchetti, 2021, Blesse and Diegmann, 2022) and commercial land use, especially alcohol outlets (Han et al., 2016, Twinam, 2017). Yet, little is known about how crime is affected when schools close, and are converted into other buildings or demolished.

Through what channels does the presence of schools affect local crime rates? According to social disorganisation theory (Shaw and McKay, 1942), the intersection between deprivation, social fragmentation and residential mobility prevents the formation of social ties and trust, thus affecting informal social control, such as, for example, the supervision of young people.[6] Relatedly, a recent study by Braakmann (2023) finds that high residential turnover is a strong determinant of crime. Within this context, the presence of a school can potentially fragment a neighbourhood, leading to more crime. The convergence in the same place of a wide range of agents, e.g. teachers, school staff, parents of pupils and residents makes it difficult to establish and enforce an informal system of control. Wo and Park (2020) find support for this explanation in their study of crime around Chicago public schools.

Another potential channel is given by routine activity theory, first proposed by Cohen and Felson (1979), according to which crime results from the spatial and temporal convergence of victims, offenders, and the absence of guardians. Neighbourhoods with schools are characterised by a high ratio of non-residents to residents (Brantingham and Brantingham, 1995, Roman, 2004), affecting

[6] Some empirical evidence on this is provided by Sampson and Groves (1989) and Sampson et al. (1997).

traffic density, thus providing a wide pool of potential victims (Murray and Swatt, 2013, Willits et al., 2013, Groff and Lockwood, 2014). Guardianship might also be limited: school guardians' jurisdictions are confined to school grounds, while residents might be willing to guard only within limited distance from their homes. Furthermore, a reason why the presence of schools is associated with higher crime rates is because criminal (and anti-social) behaviour typically peaks in teenage years (Farrington, 1986). Schools might gather victims and perpetrators in the same place, thus reducing the opportunity cost of crime (MacDonald, 2015, Cook, 2017). Schools also facilitate interactions, so that offenders can influence their peers (Glaeser et al., 1996). A handful of studies in the economics literature leverage quasi-random variation in school attendance and find that violent crimes increase on days when school is in session (Jacob and Lefgren, 2003, Luallen, 2006, Akee et al., 2014). This link is even stronger in highly segregated schools (Akee et al., 2014, Billings et al., 2014, 2019).

Given the established, positive, link between schools and crime, one might posit that closing down schools, especially highly segregated and deprived ones, might reduce crime. However, there are also channels through which school closures could increase crime. First, schools represent just one form of land use (Weisburd et al., 2012), and their closure – and consequently their empty estates – might contribute to an atmosphere of decay and lawlessness which encourages crime, in line with the broken window theory (Kelling et al. 1982). In addition, displaced pupils coming from a variety of neighbourhoods (Nerenberg, 2021) have to commute to new schools and thereby become targets of crimes (Stults and Hasbrouck, 2015, Hagedorn, 2017).

2.2 Institutional Background

Scotland is one of the devolved nations of the United Kingdom and is divided into 32 Local Authorities (LAs). These are responsible for the provision of a range of services, including education. School funding is provided by the Scottish Government (SG) to each LA mostly based on the size of the school population and level of deprivation. Since 2011, the SG employ an executive agency named Education Scotland (ES) with a portfolio of roles, from delivery of educational policy to school inspections. It is within the discretion of each LA to request the closure of one or more of the schools within their jurisdiction.

School closures, or more generally, consolidation, can take place due to a variety of factors, such as change in population or the necessity to improve school estates. If an LA proposes some sort of consolidation it must comply with a detailed procedure described by the Schools (Consultation) (Scotland) Act 2010. This procedure is based on a consultation process with a specific timeline, where the LA is meant to prepare a proposal indicating the reason for the closure, as well as its financial and educational implications. In Appendix B we provide a summary of the timeline of school closure procedures, as outlined in the Schools (Consultations) (Scotland) Act 2010.

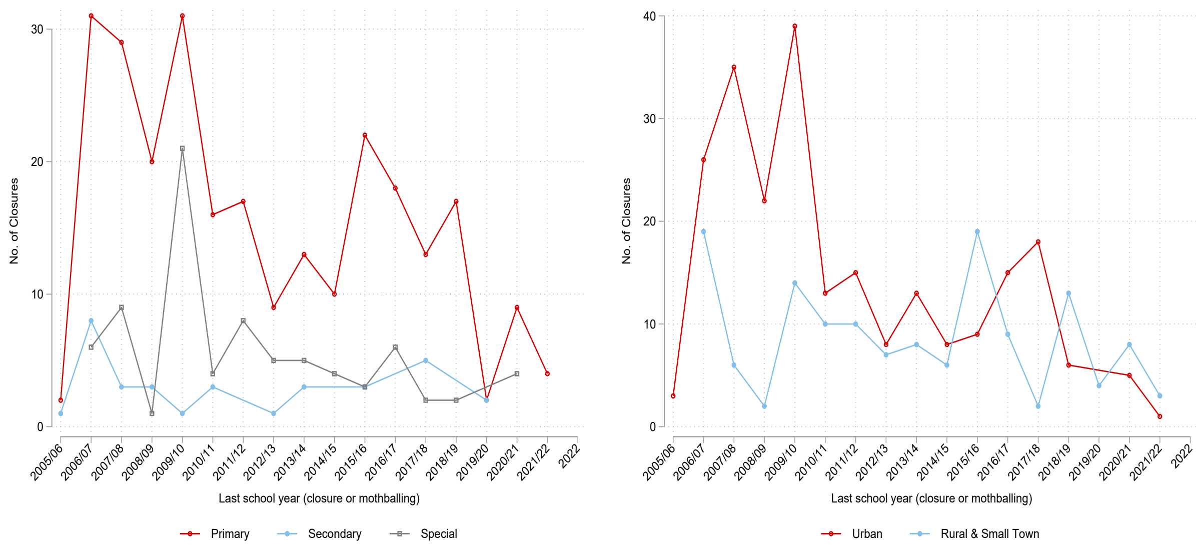
Pupils typically enter primary school in August of the year they turn five and stay in primary schools for seven years (stages P1 to P7), until they then move to secondary schools for additional six years (S1 to S6). The Scottish educational system also includes special schools, i.e. those specialised in helping pupils with emotional, behavioural and/or social difficulties, among other additional needs. These schools typically include all year groups from primary to secondary. The school year begins on the second week of August, and concludes at the end of June, for a total of 180 school days, at most.

Between school years 2005/06 and 2021/22, 376 schools closed down. Figure 1 shows trends by school type, i.e. primary, secondary and special as well as by type of area. The left-hand panel of Figure 1 shows how most closures affected primary schools (about 70% of all closures), followed by special (about 21%) and secondary schools. In addition, as shown in the right-hand panel of Figure 1, especially in the earlier part of the sample period, circa 63% of closures occurred in urban areas.

3 Data

We use data on crime and school closures that we aggregate at the Scottish Data Zone level. Data Zones are the second lowest level of territorial designation in Scotland (similar to U.S. census blocks and English Lower Layer Super Output Areas) and are composed of aggregates of the country's

Figure 1. Trends in Closures



Notes: Left-hand panel presents the total number of closed schools by sector, i.e. primary, secondary or special schools. Right-hand panel presents the total number of closed schools by area classification.

46,351 Output Areas. They are designed to include roughly between 500 and 1,000 residents each, and are meant to constitute socio-economically and geographically homogeneous areas.

Our final sample is a panel of all 6,976 Scottish Data Zones observed every year from the 2006/07 school year through to 2018/19 (see below). We combine four sources of data: *i)* crime data from Police Scotland; *ii)* data on school closures and consolidation; *iii)* school-level data on various outcomes and school characteristics and *iv)* Data Zone-level demographic data from the Scotland Census 2001 and the Scottish Index of Multiple Deprivation (SIMD) 2004, which we use as pre-treatment, baseline controls or matching variables. We also use SIMD data from waves 2009, 2012, 2016 and 2020 as additional outcomes. Our data sources are described in more detail in the following sub-sections.

3.1 Crime Data

We use monthly data at the Data Zone level on reported crimes and offences from January 2007 to December 2020, provided by Police Scotland through a Freedom of Information (FOI) request.[7] Our data consist of monthly Data Zone level crime counts (for different crime categories) for the time period 2007 to 2020. To avoid low cell sizes (few or zero crimes) in many Data Zones, we aggregate the crime data to the annual (school year) level. We construct a balanced panel of Data Zones over the thirteen year period covering the period between school years 2006/07 and 2018/19 (see more detail on this below). The data consist of all available subcategories of crimes and offences.[8] We calculate the overall, Data Zone level, crime/offence/total numbers by aggregating all instances that fall within each category, based on the classifications of the Scottish Government. We also aggregate crime data across five major crime subcategories: violent crimes (non-sexual), sexual crimes, crimes of dishonesty, fire-raising and vandalism, and other crimes. These subcategories are described in more detail in Table A.2.

3.2 School Closures and Mergers

Information on schools and their location is collected from the 'School education statistics' (Scottish Government, 2023*b*) data set published by the Scottish Government. This is a collection of surveys on pupil and schools characteristics, and includes an updated register of all opened schools, alongside information on school mergers. In particular, we observe the month and year of a closure, as well as whether it was part of a merger. This means that we can track where pupils from a closed school were displaced to, at least for the first school year following the closure. In addition, we merge a set of other data sources: *i)* the Scottish Healthy Living Survey, which contains school-level information on free school meals registration and uptake; *ii)* the Attendance and Absence Survey, which provides attendance rates as well as information on exclusions as disciplinary measures; and *iii)* other school information on size of the school population, pupil-to-teacher ratios, and school

[7] FOI 22-1505.

[8] Offences are classified under a separate crime category in Scottish Criminal Law and include more minor crimes such as speeding, dangerous and careless driving, drunkenness and other disorderly conduct, breach of the peace, et cetera.

location. Unfortunately, we do not have consistent information on academic attainment. This is not available for special schools, whereas for primary and secondary schools it only became publicly available starting from the school year 2015/2016.[9]

3.3 Neighbourhood-level Covariates and Outcomes

Measures of neighbourhood quality are important both for our matching approach (see Section 4), as covariates, and for our heterogeneity analysis. We therefore use finely grained data from the data zone level census 2001 and the Scottish Index of Multiple Deprivation (SIMD) 2004 to create variables for a detailed neighbourhood profile. The SIMD is a relative measure of poverty and deprivation. It ranks all 6,976 data zones in Scotland separately by income, housing quality, access to services, health profile, and employment outcomes.[10] We obtain the 2004 ranks for each of these components where the most deprived neighbourhood is ranked 1 and the least deprived neighbourhood is ranked 6,976. Along the demographic composition of a neighbourhood, these measures form our set of baseline control variables.

To explore potential mechanisms, we employ SIMD data from waves 2009, 2012, 2016 and 2020 as additional outcomes. We make use of eight variables: *i)* the income deprivation rate which is the percentage of people in receipt of the main forms of means-tested benefits; *ii)* the employment deprivation rate which is the percentage of working age population who are not in employment and receive employment or disability-related benefits; *iii)* the education deprivation rank ; *iv)* the percentage of people living in overcrowded dwellings; *v)* age/sex standardised rate of alcohol-related hospitalisations per person; *vi)* age/sex standardised rate of drug-related hospitalisations per person; *vii)* age/sex standardise measure of mortality and morbidity; *viii)* the overall SIMD rank deciles, ranging from 1 to 10, where 1 is the most deprived. Official definitions of SIMD

[9] It is also worth noting that the way academic attainment is assessed differs substantially across primary and secondary schools. In primary school, assessments are teacher-based. By the end of the school year, teachers establish whether each pupil meets the expected level in literacy and numeracy for a specific school stage. Such information is released as the school-level percentage of all pupils meeting the expected level but, for confidentiality reasons, values are binned into categories, i.e. $\geq 90\%$, $\leq 90\%$ *and* $\geq 80\%$ etc. Nation-wide statistics, however, reveal little variation in the data. In school year 2018/2019, about 80% of all pupils met the expect level in reading, 86% in listening and talking, 79% in numeracy and 75% in writing.

[10] Note that there is also a crime component of the SIMD which we do not use. For our crime outcome measures, we use data from Police Scotland.

components are summarised in Table A.1.

3.4 Analytical Sample

We build a balanced panel data set of all 6,976 Data Zones in Scotland, observed over a thirteen year period. Since school closures only become effective at the end of each school year (which last from August to July), this is our time unit of interest. Therefore, our sample spans from school year 2006/07 through to 2018/19, with the first closure occurring in March 2007 and the last one in November 2018. This leaves us with 342 of the original 376 closures, in 315 Data Zones. As we only observe crime from January 2007, the outcomes for the first school year will be the sum of all crimes in a given Data Zone from January to July 2007. For all subsequent school years, we calculate the total crime count per Data Zone as the sum of crime from August to the following July.

As 24 Data Zones (7.6% of those with closures) experienced more than one closure, often a few years apart, we set up our sample in a 'stacked' way.[11] In other words, if a Data Zone experiences the first closure in 2007 and another one in 2010, this Data Zone will appear in our data twice, but under different panel identifiers, one per closure episode, and with different treatment timing. Our panel unit is therefore each Data Zone - Closure level observation.

When using SIMD variables as outcomes, we apply the following matching strategy. SIMD data are collected at least one year before their publication, therefore waves 2009, 2012, 2016 and 2020 actually pertain to calendar years 2008, 2011, 2015 and 2019. These are going to be relevant for each school year starting the calendar year before. Therefore, school year 2007/08 is matched to SIMD wave 2009, whose data were collected in 2008, school year 2010/11 is matched to SIMD wave 2012 and so forth. This leaves us with a four-year panel dataset, as well as fewer Data Zones. The reason for this is that SIMD versions 2009 and 2012 use the old Data Zone boundaries (6,505 Data Zones) and only 5,157 of the new 6,976 Data Zones fully overlap with the old ones.

[11] 21 Data Zones had two closures, and 3 Data Zones had 3 closures. This means that 51 closures (about 16%) occur within a Data Zone with at least one other closure.

4 Empirical Strategy

4.1 Treatment and Control Comparisons

Our aim is to examine whether school closures lead to changes in crime rates at the neighbourhood (Data Zone) level. This carries a number of methodological challenges. A simple comparison between areas affected by the closures and areas with operational schools (or no schools at all) will likely pick up spurious correlations. After all, schools are not closed randomly. For example, local authorities might decide to close underperforming schools, or those with falling enrolment. Table 1 corroborates this prior, by showing that closed schools are typically smaller, have larger shares of pupils on free school meals, lower attendance, as well as higher exclusion rates.[12]

Table 1. Summary Statistics - Schools

	(1) All Schools		(2) Open Schools		(3) Closed Schools		(4) Balancing: $x_i = \beta_0 + \beta_1 D_i + e_i$	
	Mean	SD	Mean	SD	Mean	SD	$\widehat{\beta_1}$	SE
School Roll in September	253.28	282.26	269.18	289.25	134.21	183.88	-134.97***	(11.46)
No. of FTE Teachers	18.61	22.05	19.52	22.65	11.90	15.34	-7.62***	(0.94)
Pupil-Teacher Ratio (FTE)	13.54	4.72	14.00	4.42	10.05	5.36	-3.95***	(0.30)
Religious School (1/0)	0.15	0.36	0.15	0.36	0.14	0.34	-0.02	(0.02)
Attendance Rate	94.11	3.39	94.24	3.09	93.13	5.06	-1.11***	(0.28)
Unauthorised Absence Rate	1.37	1.92	1.38	1.89	1.36	2.09	-0.02	(0.12)
Exclusion Rate	0.61	3.27	0.52	2.78	1.30	5.74	0.78**	(0.32)
% Free School Meals	0.31	0.18	0.31	0.17	0.35	0.25	0.04***	(0.01)
Gross Internal Area (sq-metres)	3104.33	3716.66	3221.54	3791.78	2216.58	2945.68	-1004.96***	(176.67)
Site Curtilage (sq-metres)	19673.83	24909.53	20274.85	25273.97	15121.80	21445.83	-5153.05***	(1268.33)
Schools	2,902		2,560		342		2,902	

Notes: These are averages from school years 2006/2007 (first closure in our sample) to 2018/2019 (last closure in our sample). Attendance, absence and exclusion rates are measured as a fraction of all episodes and the total number of possible attendance (No. of Pupils × School Days). The percentage of pupils registered (and thus eligible) for free school meals is based on the entire school population and refers to the number of pupils whose families are in receipt of the main forms of means-tested benefits. Gross internal area is the measure (in squared metres) of the indoors space of the school estate, whereas Site Curtilage also includes outdoor spaces.

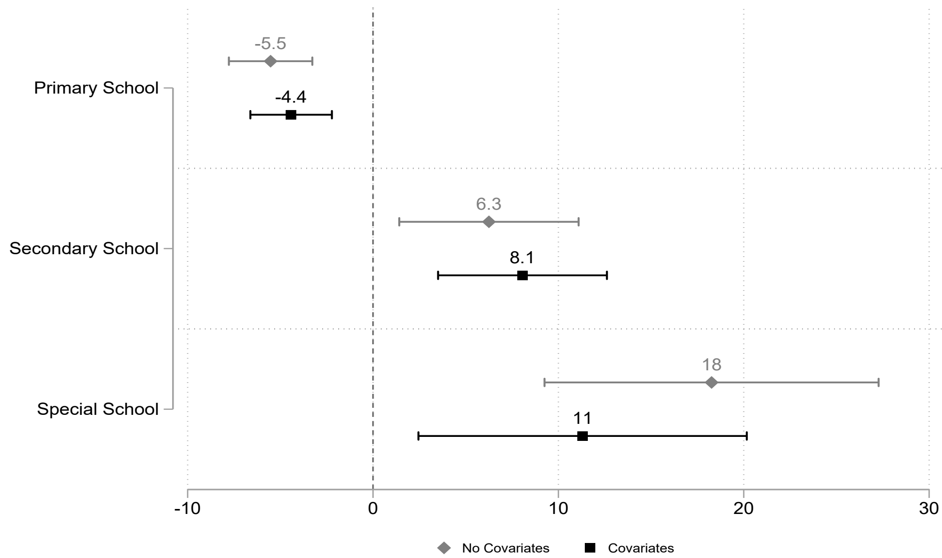
The characteristics of closed schools might be correlated with neighbourhood characteristics, therefore we would expect Data Zones with closed schools to be systematically different from those

[12] Table A.3 shows means and standard deviations, similar to Table 1, broken down by school sector. Unfortunately, we cannot use school-level ethnicity data for this analysis. Scotland is much less diverse than the rest of the UK, and to avoid statistical disclosure risk from low cell values, most information on ethnicity has not been provided as exact values in school-level data sets. According to Census 2011, only 8% of the population in Scotland is from an ethnic minority background (Scotland's Census, 2011), compared to about 19% in England and Wales (Office for National Statistics, 2011), and in most schools this share will be even lower than the national average.

where schools remain open.

Table 2 shows the results of a descriptive analysis of Data Zone characteristics, based on whether they contained schools, and whether they experienced at least one closure between 2007 and 2019. This analysis is done using all 6,976 Data Zones from school year 2006/07 to 2018/19. Our results in column (6) show that Data Zones which contained at least one school during 2007-2019 are characterised by a slightly different demographic structure. For example, the share of people with elementary occupations is slightly larger in such neighbourhoods (.9pp), with fewer managers or professionals living in these areas (.3pp and 1pp, respectively). However, we do not observe different levels of deprivation across these areas. For example, there are no significant differences in crime, income deprivation or in the presence of council estates. Surprisingly, areas with schools are not characterised by higher crime rates, in spite of what the sociology and criminology literatures suggest (see Section 2.1). In fact, areas with at least one school within the period under analysis experience on average 1.8 fewer crimes per 1,000 people, and this difference is not statistically significant. This coefficient, however, masks some heterogeneity across different types of schools. Figure 2 sheds some light on this. The point estimates are from regressions of the crime rate per 1,000 people on three dummy variables, indicating the presence of at least one primary, at least one secondary and at least one special school in a given Data Zone - school year combination. Therefore, the comparison group are Data Zone - school year combinations with no schools at all. The results show that while the presence of at least one primary school in a given Data Zone - school year is associated with lower crime rates (about 4.4 to 5.5 fewer crimes per 1,000 people) relative to areas with no schools at all, secondary school areas have 6.3 to 8 more crimes per 1,000 people on average. The gap is even wider for special schools (11-18 more crimes per 1,000 people). The negative (and not statistically significant) correlation in Table 2 is in fact driven by the larger weight of primary schools, making up about 78% of the entire school stock, compared to 14% for secondary and the remaining 8% for special schools. Taken together, these correlations are consistent with the literature reviewed in Section 2.1 which stipulates that areas with schools (especially secondary) have more crimes, potentially due to pupils in their teenage years being the offenders themselves (Steinberg and MacDonald, 2019). Moreover, when we look at the results for Data Zones with schools (column 7), we see that those experiencing at least one closure have higher crime rates (8.9 crimes per 1,000 people), a larger fraction of households living in social housing (7pp) and a more marked difference in the neighbourhood's demographic structure.

Figure 2. Difference in Crime Rates by Presence of School



Notes: The point estimates are from regressions of the crime rates on three dummy variables, indicating the presence of at least one primary, at least one secondary and at least one special school in a given Data Zone/School Year. The comparison group are Data Zones/School Years with no schools at all. The analysis was conducted on all 6,976 Data Zones from school year 2006/07 to 2019/20. We use year fixed effects, but due to the little variation in the presence of schools within the panel we do not add block fixed effects. Covariates are the fraction of people who are income deprived, population density as per Census 2001, and fraction of people aged 16 to 74 without qualifications. Standard errors are clustered at the Data Zone level and the whiskers are 95% confidence intervals.

Table 2. Summary Statistics - Blocks

					Ever School						Balancing: $x_i = \beta_0 + \beta_1 D_i + e_i$					
	(1) All Data Zones		(2) Never School		(3) All Data Zones		(4) No Closures		(5) Closures		(6) School vs Non-School		(7) Closed vs Non-Closed		(8) Matched Sample	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	$\widehat{\beta_1}$	SE	$\widehat{\beta_1}$	SE	$\widehat{\beta_1}$	SE
Crime Rate (per 1,000)	32.51	54.23	33.08	58.75	31.32	43.22	30.07	42.76	38.97	45.21	-1.760	(1.17)	8.899***	(2.53)	6.623**	(3.20)
Crime & Offence Rate (per 1,000)	58.35	106.89	58.21	114.26	58.64	89.53	56.02	85.88	74.74	107.99	0.422	(2.27)	18.722***	(5.50)	15.346**	(6.61)
Offence Rate (per 1,000)	25.84	64.63	25.14	65.59	27.32	62.57	25.95	58.20	35.77	84.13	2.182	(1.35)	9.823**	(3.93)	8.723**	(4.41)
Violent Crimes Rate (per 1,000)	12.60	27.01	12.73	29.90	12.31	19.65	11.84	19.79	15.20	18.51	-0.420	(0.55)	3.359***	(1.01)	2.821**	(1.26)
Sexual Crimes Rate (per 1,000)	1.22	2.56	1.20	2.46	1.27	2.75	1.25	2.82	1.41	2.31	0.076	(0.05)	0.159	(0.10)	0.105	(0.11)
Dishonesty Crimes Rate (per 1,000)	21.24	49.85	21.92	55.55	19.82	34.98	19.20	34.93	23.62	35.00	-2.100**	(1.05)	4.419**	(2.00)	3.889	(2.53)
Vandalism Crimes Rate (per 1,000)	11.40	12.82	11.18	12.55	11.86	13.34	11.45	13.04	14.34	14.81	0.680**	(0.27)	2.888***	(0.71)	2.026**	(0.97)
Other Crimes Rate (per 1,000)	8.61	19.11	8.72	20.31	8.38	16.30	7.87	15.83	11.46	18.68	-0.350	(0.39)	3.588***	(0.99)	2.303*	(1.21)
Population Density	43.45	42.08	50.53	45.80	28.63	27.60	28.21	27.21	31.19	29.74	-21.899***	(0.88)	2.982*	(1.79)	2.588	(2.36)
Income Deprivation	14.78	11.55	14.78	11.76	14.79	11.08	14.04	10.35	19.39	13.92	0.013	(0.29)	5.345***	(0.82)	1.459	(1.16)
Geographic Access	0.02	0.78	-0.07	0.69	0.21	0.92	0.22	0.91	0.19	0.98	0.286***	(0.02)	-0.029	(0.06)	-0.037	(0.08)
Households w/o Central Heating	5.76	7.22	5.57	7.25	6.15	7.16	5.88	6.89	7.84	8.42	0.581***	(0.18)	1.957***	(0.50)	0.959	(0.70)
Households Overcrowded	13.84	9.41	14.26	9.66	12.94	8.79	12.40	8.29	16.25	10.83	-1.324***	(0.23)	3.847***	(0.64)	1.388	(0.89)
Households in Social Housing	0.26	0.20	0.25	0.21	0.26	0.19	0.25	0.19	0.32	0.21	0.004	(0.01)	0.066***	(0.01)	0.013	(0.02)
Managers And Senior Officials	0.12	0.05	0.12	0.05	0.12	0.05	0.12	0.05	0.10	0.05	-0.003**	(0.00)	-0.015***	(0.00)	-0.003	(0.00)
Professional Occupations	0.10	0.07	0.10	0.07	0.09	0.06	0.10	0.06	0.08	0.06	-0.010***	(0.00)	-0.013***	(0.00)	-0.001	(0.01)
Associate Professional And Technical Occupations	0.13	0.04	0.14	0.05	0.13	0.04	0.13	0.04	0.12	0.04	-0.011***	(0.00)	-0.009***	(0.00)	-0.001	(0.00)
Adminstrative And Secretarial Occupations	0.13	0.03	0.13	0.03	0.12	0.03	0.12	0.03	0.12	0.03	-0.010***	(0.00)	-0.001	(0.00)	-0.002	(0.00)
Skilled Trades Occupations	0.12	0.05	0.12	0.04	0.14	0.05	0.14	0.05	0.14	0.05	0.024***	(0.00)	-0.001	(0.00)	-0.001	(0.00)
Personal Service Occupations	0.07	0.02	0.07	0.02	0.08	0.02	0.08	0.02	0.08	0.02	0.004***	(0.00)	0.006***	(0.00)	0.001	(0.00)
Sales and Customer Service Occupations	0.09	0.03	0.09	0.03	0.08	0.03	0.08	0.03	0.09	0.03	-0.008***	(0.00)	0.005***	(0.00)	0.001	(0.00)
Process; Plant And Machine Operatives	0.10	0.05	0.10	0.05	0.10	0.05	0.10	0.05	0.11	0.05	0.005***	(0.00)	0.006**	(0.00)	-0.000	(0.00)
Elementary Occupations	0.13	0.06	0.13	0.06	0.14	0.05	0.14	0.05	0.16	0.06	0.009***	(0.00)	0.021***	(0.00)	0.006	(0.00)
Population < 16	0.19	0.05	0.19	0.05	0.19	0.04	0.19	0.04	0.19	0.05	0.002*	(0.00)	0.001	(0.00)	-0.000	(0.00)
Population > 65	0.16	0.07	0.16	0.07	0.16	0.06	0.16	0.06	0.17	0.06	0.010***	(0.00)	0.000	(0.00)	0.002	(0.00)
Males b/w 15 and 20	0.07	0.03	0.08	0.03	0.07	0.03	0.07	0.03	0.08	0.02	-0.002***	(0.00)	0.003*	(0.00)	0.001	(0.00)
Females	0.52	0.02	0.52	0.02	0.52	0.02	0.52	0.02	0.52	0.02	-0.002***	(0.00)	0.004**	(0.00)	-0.000	(0.00)
Ethnic Minority (Incl. non-british white)	0.03	0.05	0.04	0.05	0.03	0.03	0.03	0.03	0.03	0.04	-0.009***	(0.00)	0.000	(0.00)	0.004	(0.00)
Unemployment rate	0.07	0.05	0.06	0.05	0.07	0.04	0.06	0.04	0.08	0.05	0.002	(0.00)	0.018***	(0.00)	0.004	(0.00)
No Qualification	0.33	0.13	0.33	0.13	0.35	0.12	0.34	0.12	0.38	0.12	0.021***	(0.00)	0.042***	(0.01)	0.009	(0.01)
Highest Qualification: Group 1	0.25	0.06	0.25	0.06	0.25	0.05	0.25	0.05	0.25	0.05	0.004***	(0.00)	0.001	(0.00)	-0.003	(0.00)
Highest Qualification: Group 2	0.15	0.05	0.16	0.05	0.15	0.04	0.15	0.04	0.14	0.04	-0.008***	(0.00)	-0.012***	(0.00)	-0.003	(0.00)
Highest Qualification: Group 3	0.07	0.02	0.07	0.02	0.07	0.02	0.07	0.02	0.06	0.02	-0.004***	(0.00)	-0.003**	(0.00)	-0.001	(0.00)
Highest Qualification: Group 4	0.19	0.12	0.20	0.13	0.18	0.11	0.19	0.11	0.16	0.11	-0.013***	(0.00)	-0.027***	(0.01)	-0.003	(0.01)
Data Zones	6,976		4,722		2,254		1,939		315		6,976		2,254		564	

Notes: Neighbourhood-level variables are from the Census 2011 and from the Scottish Index of Multiple Deprivation (SIMD) 2004. 'Income Deprivation' is the fraction of population who are income deprived, i.e. in receipt of main income-support benefits (see Table A.1). Employment, Geographic Access, Housing, Education and Health are Scottish Index of Multiple Deprivation scores. Higher scores correspond to higher levels of deprivation across each domain. See Table A.1 for a more complete description.

4.2 Identification Strategy

To identify the effects of school closures on crime, we employ three distinct difference-in-differences (DiD) approaches. The idea is to compare crime rates across Data Zones before and after school closures take place. That is the identifying variation comes from differences in the timing of school closures, where both never treated (no school closures) and not-yet-treated (schools not closed yet) areas could serve as our comparison group.

4.2.1 Matched Differences-in-Differences

Our analysis above illustrates that areas without schools or closures (the never treated group) may be structurally different from our treated group, and with non-random timing of treatment these differences can further be correlated with trends in our outcomes across these groups. In order to minimise structural differences across treated and comparison areas, all our DiD analyses will rely on a matching approach. Specifically, we use Mahalanobis nearest neighbour matching similar to Blesse and Diegmann (2022), to identify Data Zones that could act as suitable control units to compare to Data Zones with school closures. The matching algorithm minimises the standard Euclidean distance of all matching variables, which in our case are the Data Zone demographic variables summarised in Table 2. We restrict the potential control units to only those Data Zones with at least one school at any point within the sample period. Column (8) of Table 2 shows that following the matching, all the variables (except for crime, which as outcomes of interest are excluded from the matching) are balanced. Point estimates for the differences are now much smaller, and not statistically significant. We will rely on this matched sample for all subsequent analysis, where we will rely on three different DiD estimators.

4.2.2 TWFE Estimator

In our identification we rely on the fact that school closures occur at different points in time. In this setup, the simplest way to estimate the effects of closures on crime rates is by using the standard two-way fixed effects (TWFE) estimator, which can be formalised as:

$$Y_{it} = \theta_i + \theta_t + D_{it}\delta + \eta_{it} \tag{1}$$

where our outcome is the crime rate per 1,000 people in Data Zone - Closure unit 'i' at year 't', θ_i and θ_t are unit and year fixed effects, respectively, while D_{it} is a treatment indicator for school closures in unit 'i' at time 't'. The coefficient of interest is δ which measures the effect of closures on crime rates.

4.2.3 Callaway and Sant'Anna Estimator (CSDiD)

However, an emerging literature discusses how the standard TWFE approach might not be suitable to estimate difference-in-differences models in this context due to the possibility of heterogeneous treatment effects (see Roth et al. (2022) for a review). To overcome this issue, we combine our matching approach with the DiD approach developed by Callaway and Sant'Anna (2021). This "CSDiD" approach estimates group-time treatment effects for each treatment timing group relative to either a never treated or not yet treated group (Callaway and Sant'Anna, 2021). In our case, both of these are included in the matched comparison group. We can then calculate the average treatment effect on the treated (ATT) for each group-time combination which is then aggregated into an overall ATT estimate. This is done formally by estimating $ATT(g,t) = E[Y^1_{it}(g) - Y^0_{it}(0)|G_g = 1]$ at every (g,t) point, where 'g' are treatment timing group identifiers, 't' are our time units, and Y_{it} are outcomes for unit 'i' at time 't'.[13] We estimate these ATTs using the regression method presented in Callaway and Sant'Anna, 2021. Our results are reported in Table 4.

Moreover, with CSDiD, we can use first difference results for each timing group's pre-treatment period to represent placebo estimates, which can then be aggregated across groups to create 'event study' style plots where we can test for the presence of pre-trends. These are plotted in Figure 3.

[13] In its simplest version, the estimated $ATT(g,t)$'s are a series of all possible 2×2 DiD estimates at every possible point in time (period) and for each group compared against the comparison group.

4.2.4 Two-Stage Differences-in-differences (DiD2S)

CSDiD event-study are not directly comparable to traditional TWFE specifications or event-study results from other DiD estimators. For our event-study results, we therefore include estimates estimates from two alternative approaches. First, we conduct a traditional TWFE event-study analysis (see Figure 3) which would be unbiased even with heterogeneous treatment effects as long as the treatment effects are not dynamic. Because non-dynamic treatment effects is a strong assumption, we also estimate our baseline event studies using a two-stage DiD (DiD2S) approach, as proposed by Gardner (2022). The intuition behind this approach is that it allows us to recover the average difference in outcomes between treated and control units, after removing the group and time period specific effects that could lead to treatment effect heterogeneity (Gardner, 2022). Note that our control group, again, only consists of matched units.

Formally, in the first-stage we estimate

$$Y_{it} = \theta_g + \theta_t + \theta_c t + \eta_{it} \tag{2}$$

where Y_{it} is the crime rate per 1,000 people as in Equation 1 above, θ_g are timing group fixed effects, θ_t are year fixed effects, and $\theta_c t$ is a local authority level linear trend.[14] The latter term is included to account for time-variant trends in crime rates specific to wider local areas. The first-stage is estimated using observations where the treatment indicator is equal to zero. From this regression, we retain the estimated effects $\widetilde{\theta_t}, \widetilde{\theta_i}, \widetilde{\theta_c t}$. Then, in the second stage, we regress the adjusted outcome $\widetilde{Y_{it}} = Y_{it} - \widetilde{\theta_i} - \widetilde{\theta_t} - \widetilde{\theta_c t}$ for all observations on our treatment variable D_{it}, or, in the event study specifications, on leads and lags of the treatment indicator. We plot the point estimates corresponding to each lead/lag in Figure 3.

[14]We use group fixed effects instead of unit fixed effects (as in the TWFE model) to make the estimation of DiD2S models more computationally efficient. Using group or unit fixed effects does not make a difference in this context, as shown by Wooldridge (2021).

5 Results

Table 3 reports our ATT estimates for different DiD specifications. The dependent variable in all cases is the crime rate per 1,000 people. Table 4 reports results for our preferred CSDiD and DiD2S specifications for different aggregate crime categories, along with offences and overall crime rate.[15] These specifications are the ones from Columns (2) and (4) of Table 3 for CSDiD and DiD2S, respectively. TWFE specifications for Table 4 are reported in Table A.4. In general, our results in Table 3 suggest that closures reduce the overall crime rate by roughly 4 crimes per 1,000 people (per year), which corresponds to around 9% of the standard deviation in our sample. These results are consistent across different specification although they do get smaller (and are no longer significant) when we omit the never treated areas from the control group in Column (5).

Our event study estimates plotted in Figure 3 suggest similarly sized negative effects for crimes that are persistent over time and consistent across different specifications. Reductions in crime materialise within one year of a school closure. Point estimates suggests that they at best grow moderately over time. The figure also supports the parallel trends assumption as all lead coefficients are small and none of them is statistically significant at the 5% level.

The results shown in Table 4 highlight that baseline results seem to be driven mostly by violent crimes, dishonesty and theft, and vandalism. Notably, there is no significant effect for the 'other' crime category, which contains mostly drugs and weapons related crimes, while the large negative effect on offences is only marginally significant for our CSDiD specification. In addition, Figure A.1 shows that the results for crime are driven by closures occurring in urban areas. On the other hand, the drop in offences seem to be driven by rural school closures. While this estimate is statistically significant at the 5% level, we think it needs to be taken with caution given the large confidence interval. Generally, our baseline results suggest that closures are associated with a persistent negative effect on the local crime rate. These results are quite similar to those reported in Steinberg et al. (2019), who also find a negative crime effect after closures for school blocks in Philadelphia.

[15] Offences are a distinct crime category in Scottish Criminal Law and include more minor crimes such as speeding, dangerous and careless driving, drunkenness and other disorderly conduct, etc. See Table A.2 for more detail on crime classifications.

In their case this reduction seems to be mostly driven by a reduction in violent crimes, which is something we also observe in our setting. Unlike in their study however, we also observe a reduction in property crimes. The former effect was mostly explained (Steinberg et al., 2019, Brazil, 2020) by the relocation of pupils (especially in secondary schools) with problem behaviours no longer engaging in crime around former school locations. Nonetheless, the mechanisms underlying our results here deserve further attention, which we will provide in the next sections.

Table 3. Baseline Results - Overall Crime Rate

	CSDiD		DiD2S		
	(1)	(2)	(3)	(4)	(5)
School Closure	-4.01***	-4.02***	-3.32**	-3.33**	-2.06
	(1.43)	(1.46)	(1.50)	(1.38)	(1.92)
Observations	8,541	6,624	8,541	8,541	4,446
Data Zones	564	564	564	564	315
Mean DV	36.24	35.87	36.24	36.24	38.97
SD DV	42.31	40.99	42.31	42.31	45.21
LA Linear Trends	No	No	No	Yes	No
Not Yet Treated	Yes	No	Yes	Yes	Yes
Never Treated	Yes	Yes	Yes	Yes	No

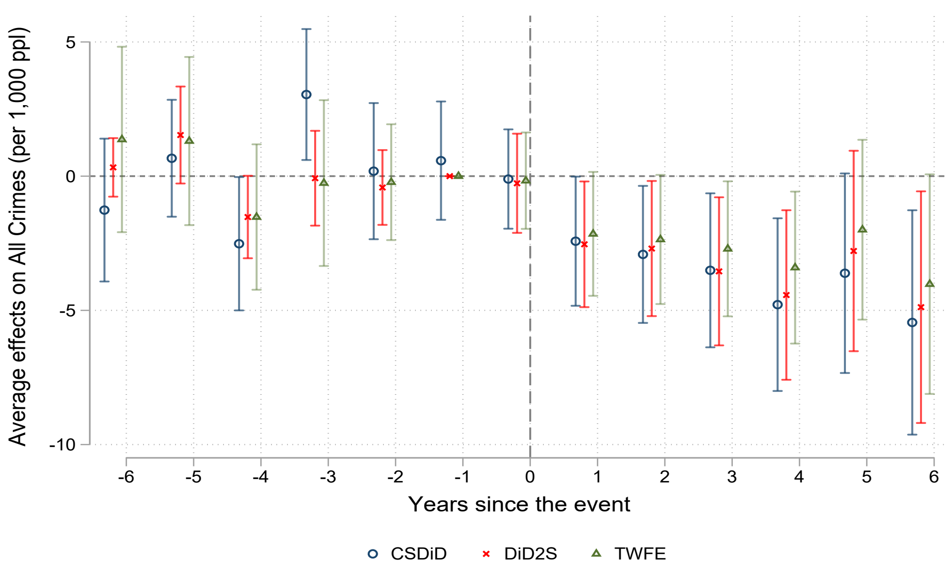
Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models in columns (1)-(2) are estimated using the CSDiD (Callaway and Sant'Anna, 2021)approach whilst in columns (3)-(5) we use a DiD2s (Gardner, 2022) estimator. Columns (1), (3) and (4) use never-treated plus not-yet-treated as the comparison group. Column (2) drops not-yet-treated units from the sample, whilst column (5) drops never-treated units. In column (4) we control for local authority-specific linear time trends. The dependent variable in all cases is the crime rate per 1,000 people. Standard errors are clustered at the Data Zone-by-Closure level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Baseline Results - Crime Categories

	All Crimes	Offences	Violent Crimes	Sexual Crimes	Dishonesty & Theft	Vandalism	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: CSDiD							
School Closure	-4.01***	-11.32*	-2.31***	-0.10	-3.07***	-1.49**	-1.08
	(1.43)	(6.00)	(0.82)	(0.11)	(0.97)	(0.73)	(0.79)
Panel B: DiD2S							
School Closure	-3.33**	-7.20	-1.74**	-0.15	-2.36**	-1.21*	-0.75
	(1.38)	(4.58)	(0.72)	(0.10)	(0.93)	(0.69)	(0.77)
Observations	8,541	8,541	8,541	8,541	8,541	8,541	8,541
Data Zones	564	564	564	564	564	564	564
Mean DV	36.24	32.53	13.98	1.37	22.21	13.41	10.51
SD DV	42.31	70.79	17.18	2.20	33.28	14.01	18.21

Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models in Panel A are estimated using the CSDiD (Callaway and Sant'Anna, 2021) approach, whilst models in Panel B use a DiD2s (Gardner, 2022) estimator. All dependent variables are crime rates per 1,000 people. Standard errors are clustered at the Data Zone-by-Closure level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3. Event Study



Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models are estimated using the CSDiD (Callaway and Sant'Anna, 2021), DiD2s (Gardner, 2022) and TWFE approaches. Standard errors are clustered at the Data Zone-by-Closure level. Whiskers indicate 95% confidence intervals.

5.1 Robustness Checks

In this section, we address some residual concerns regarding the validity and causal interpretation of our main findings. One threat to identification is a violation of the Stable Unit Treatment Value Assumption (SUTVA). In particular, if the reduction in crime we observe was to be due to pupil perpetrators leaving the (former) school's area, we risk that the outcomes in the control group are also affected by the closures. In other words, if school closure merely shifted crime from treatment to control areas, our DiD results would not reflect actual crime reductions. To mitigate concerns about SUTVA violations, we re-run the Mahalanobis matching procedure but impose two alternative restrictions: first, we omit Data Zones where no closures have ever happened but that received displaced pupils from closing schools in column (4) of Table 5. Second, we impose the control unit not to be bordering a treated one. Results based in this matching strategy are reported in column (5) of Table 5. Our baseline estimates are unaffected by these changes.

Second, as a neighbourhood can contain more than one school, one could expect the crime-reducing effect of school closures to be stronger where no schools remain open. In column (2) of Table 5, we focus on those closures which led to no school remaining in the Data Zone. We observe that while the point estimate is unchanged, this is much less precisely estimated, and not statistically significant at any conventional level.

Finally, we want to ensure that the effect is not underestimated due to the fact that a neighbourhood with closed schools might have received displaced pupils within our time window. In column (3) of Table 5, we drop from the sample those Data Zones with closures which at some point also received displaced pupils from other school closures.

Table 5. Robustness Checks - CSDiD

	Baseline	No School Left	No Displacement: Treated	No Displacement: Control	No Neighbours
	(1)	(2)	(3)	(4)	(5)
School Closure	-4.01***	-4.01	-4.60***	-4.00***	-4.28***
	(1.43)	(2.85)	(1.56)	(1.42)	(1.46)
Observations	8,541	2,483	7,605	8,541	8,541
Data Zones	564	180	505	560	561
Closures	342	191	270	342	342

Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models are estimated using the CSDiD (Callaway and Sant'Anna, 2021) approach. Standard errors are clustered at the Data Zone-by-Closure level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Displacement Effects

Our baseline findings provide evidence of local reductions in crime rates following school closures. However, if schools are indeed places of crime, closing a school in one neighbourhood might lead to crime moving somewhere else, potentially following where the victim pool, and/or where the possible perpetrators (if these are pupils) relocate. We investigate this possibility in Table 6. In columns (1) and (2) we repeat the same exercise as column (1) of Table 4, but we add a binary variable for neighbouring Data Zones that 'mimic' the treatment indicators for Data Zones containing a closure. For example, if Data Zone A is treated in 2008, the *neighbour* variable for all its neighbouring Data Zones will switch to one in 2008. We define *neighbour* in two ways. First, a Data Zone neighbours a treated one if it shares some of its boundaries. Second, a Data Zone neighbours a treated one if they are part of the same Intermediate Zone.[16] Results using these two different definitions are shown in columns (1) and (2) of Table 6, respectively. Columns (3)-(6) instead define the treatment based on whether a Data Zone has received pupils displaced from closed schools. Columns (3) and (4) use a binary treatment, whereas (5) and (6) interact this binary treatment with the number of displaced pupils, the absolute number and the fraction, respectively. Finally, column (4) uses only those receiving Data Zones which have not experienced a closure themselves.

Overall, the results presented in Table 6 provide no clear evidence of displacement effects.

[16]Intermediate Zones are groups of Data Zones. There are 1,279 Intermediate Zones, containing between 2 and 9 Data Zones. On average, each has about 5 Data Zones.

Our baseline estimates generally hold when we add the 'neighbour' dummies in columns (1) and (2) while the coefficients for these terms are not significant themselves. In columns (3) to (6) we also find no significant evidence that displaced pupils drive crime displacement effects. This finding would not be in line with a 'pupils-as-perpetrators' mechanism at play to drive crime effects, although it nonetheless comes with the caveat that in these specifications we include all – including primary – school closures, and young pupils switching primary schools are very unlikely to be perpetrators of crimes. We therefore check the heterogeneity of our results by school type in Section 5.3 below.

Table 6. Displacement Effects

	Same Border	Same Int. Zone	*d* - Binary Treatment		$d \times$ *No. Displaced*	$d \times$ *Fraction Displaced*
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-4.40**	-3.38**	-1.27	-2.56	0.00	-1.71
	(1.74)	(1.69)	(2.85)	(4.02)	(0.01)	(2.59)
Neighbour	2.63	0.13				
	(2.31)	(2.25)				
Treatment	Closure	Closure	Displacement	Displacement	Displacement	Displacement
Observations	8,541	8,541	4,446	3,679	4,446	4,446
Data Zones	564	564	323	264	323	323

Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Columns (1)-(2) are based on the same matched sample as Table 4, columns (3)-(6) are based on a similar matching exercise whereby the treatment is defined by whether a neighbourhood received displaced pupils. In column (1) we add a control for contiguous Data Zones, whereas in column (2) we add a control for Data Zones within the same (wider) Intermediate Zone, whether contiguous or not. Columns (3)-(6) define the treatment based on whether a Data Zone has received pupils displaced from closed schools. Columns (3) and (4) use a binary treatment, whereas (5) and (6) interact this binary treatment with the number of displaced pupils, the absolute number and the fraction, respectively. Finally, column (4) uses only those receiving Data Zones which have not experienced a closure themselves. Models are estimated using the DiD2s (Gardner, 2022) approach. Standard errors are clustered at the Data Zone-by-Closure level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Mechanisms

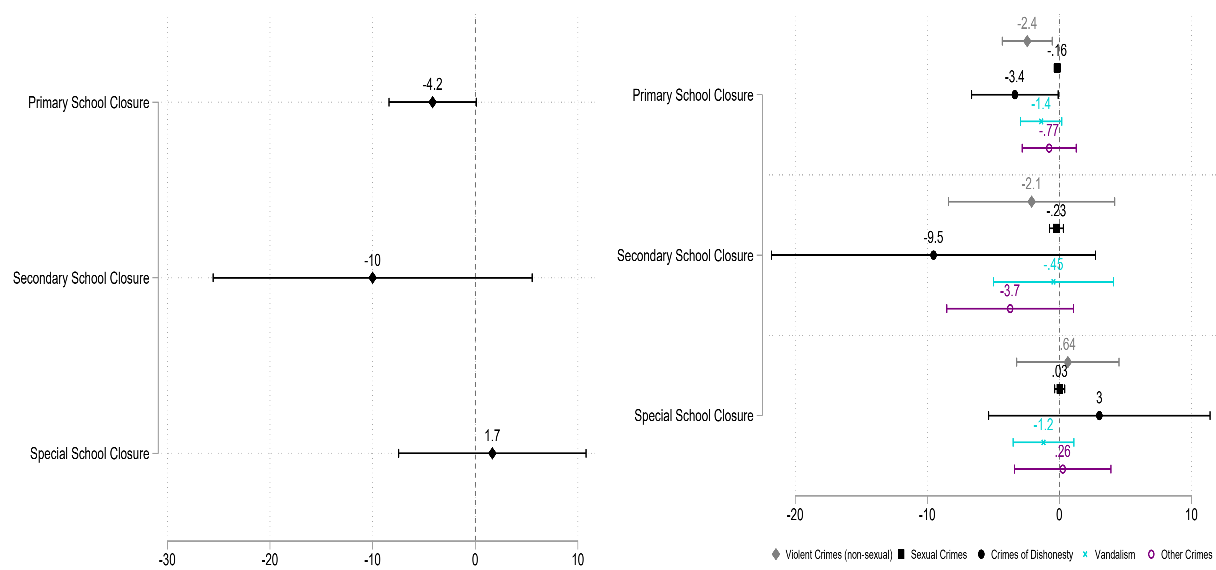
So far, we have established that neighbourhoods experiencing the permanent closure of one or more schools experience a reduction in crimes. In this section, we want to explore the potential mechanisms underpinning this effect. To do this, we draw on the criminology literature to explain how the presence of schools is linked to local crime rates in the first place, and what is to be expected when schools close. Furthermore, we look at additional socio-economic channels through which closures might affect crime.

Schools as places of crime. As criminal behaviour peaks during teenage years (Farrington, 1986), areas with schools might be characterised by higher crime rates relative to areas without schools. This might be amplified by the presence of a large pool of victims and the lack of suitable guardians (routine activity theory) which is generated by weak informal control (social disorganisation theory). If this was the case, it is plausible that the negative effect on crime would be driven by closures of secondary or special schools.

Figure 4. Heterogeneity Analysis

(a) Crimes

(b) Types of Crime



Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. The results reported are based on the DiD2s specification from column (4) of Table 3. Standard errors are clustered at the Data Zone-by-Closure level. Whiskers indicate 95% confidence intervals.

Figure 4 breaks down the main effect by school type. Panel (a) shows that while the point estimate for secondary schools is more than twice as large as the one for primary schools, (-10 crimes per 1,000 inhabitants versus -4.2) it is non statistically significant. Similarly, the effect is not driven by special schools, whose coefficient is positive, small and not statistically significant. Panel (b) breaks down the effect by type of crime. Again, we do not observe heterogeneous effects. This is likely due to lack of power. Of the 342 closures we investigate, only 30 are secondary schools, compared to 74 special and 238 primary schools. This means we might not have enough instances to obtain meaningful estimates by type of school.

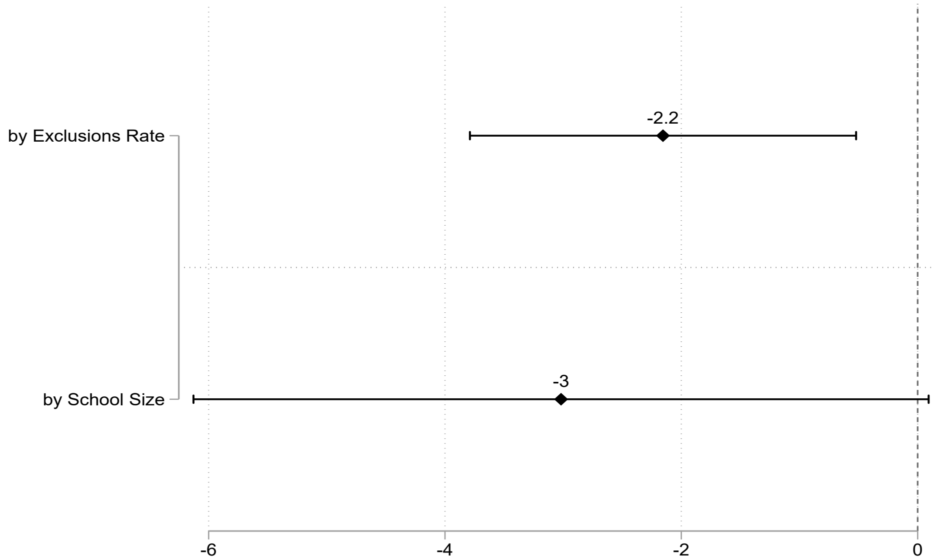
For this reason, we look at the same question from a slightly different angle. If pupils from closed schools are the perpetrators, we would expect the crime reduction to be driven by the size of the closed school's population, and/or the amount of disciplinary issues experienced by the school prior to closure (assuming that disciplinary issues are correlated with the likelihood of engaging in crime). In Figure 5 we interact our treatment with the pre-closure size (in terms of school population) and exclusion rate and report marginal effects. Our results show that while the effect is driven by schools with higher exclusion rates (extra reduction by 2.2 crimes for 1σ), the main effect does not mask any heterogeneity by school size (or at least this is not statistically significant).

Overall, we do not find strong evidence that the reduction in crime is driven by the former pupils being moved from the original neighbourhood. This is consistent with the finding in Table 6, where we find no evidence of spillover effects in neighbouring areas or those where the pupils are displaced.

Residential Turnover. Another channel through which school closures might affect crime is residential turnover. Scotland's school assignment system is purely residence-based. In other words, every school has an attendance ('*catchment*') area, which automatically determines its pupil pool. As a consequence, there is an incentive for parents to relocate into the 'desired' attendance area.[17] When a school shuts down, we thus would expect fewer people to move in and out of the neighbourhood,

[17] Rossi (2020) documents a 3% house price premium for a one-standard deviation increase in school 'quality' in Scotland.

Figure 5. Heterogeneity by School Characteristics

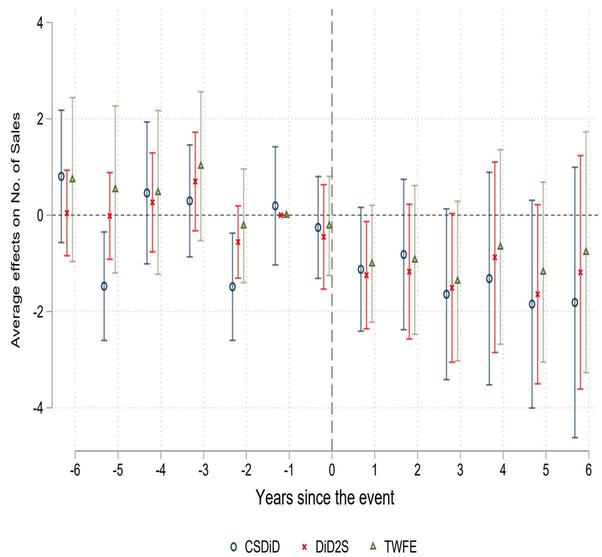


Notes: Exclusion rate and school size refer to the last year before the school closed and are standardised to have mean 0 and standard deviation of one. Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models are DiD2s (Gardner, 2022) approach. Standard errors are clustered at the Data Zone-by-Closure level. Whiskers indicate 95% confidence intervals.

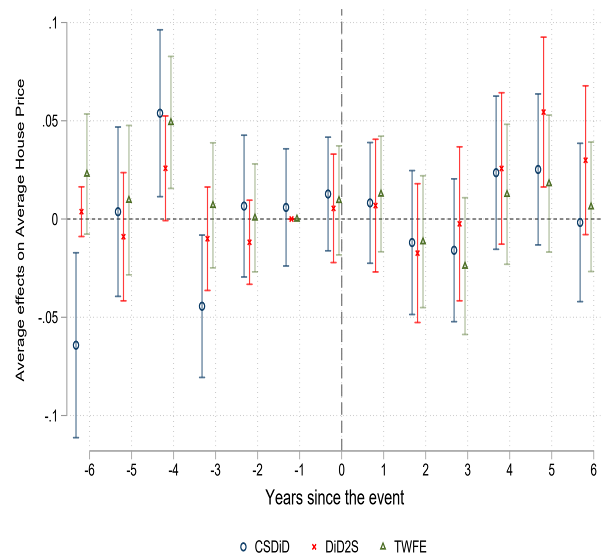
ceteris paribus. Equally, as closing schools are under-subscribed and, plausibly, underperforming, their neighbourhoods might not be sought-after in the first place, but yet experience depopulation after a closure. Either way, a reduction in residential turnover might translate into a decrease in crime (Braakmann, 2023). This is because inflows and outflows of residents might disrupt local community ties thus weakening social networks (social disorganisation theory). Likewise, inflows of new residents might simultaneously increase the pool of victims and reduce the availability of guardians (routine activity theory). Figure 6 shows that residential turnover, measured by the number of sales occurring one year after the closure (Panel (a)), reduces slightly, but this is not statistically significant, and neither is the effect on average house prices (Panel (b)). Finally, we look at the average effect on the value of the housing stock, measured by the product between the number of sales times the average price. Here, we see a small reduction, marginally significant three and five years after the closure. In general, there is very little evidence that our baseline crime effect is driven by changes in residential turnover.

Figure 6. Residential Mobility

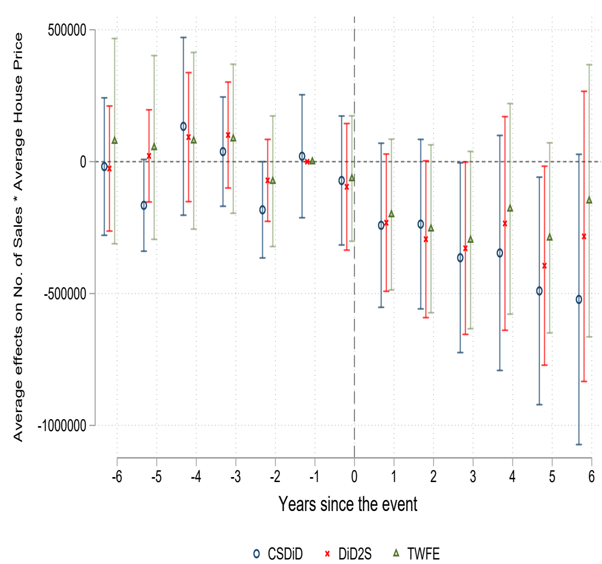
(a) Turnover



(b) House Prices



(c) Sales * House Prices



Notes: We use data on residential transactions and prices from year 2007 to 2019. Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Event studies are estimated using the CSDiD (Callaway and Sant'Anna, 2021), DiD2s (Gardner, 2022) and TWFE approaches, in line with our baseline specifications in Figure 3. Standard errors are clustered at the Data Zone-by-Closure level. Whiskers indicate 95% confidence intervals.

Gentrification. We see from Table 2 that neighbourhoods with closed schools have higher levels of deprivation than those with open schools, as well as a different socio-economic composition. Yet, school closures might also affect neighbourhoods beyond their effects on crime. For example, previous residents (families of pupils) might relocate closer to the receiving schools after a closure, with new residents moving in to replace them. The resulting changes in neighbourhood composition could, in turn, affect local crime rates, either by changing the local pool of victims or perpetrators. To investigate closures' effects on neighbourhood composition, we estimate our baseline specifications from Table 3 but using SIMD variables as outcomes.[18] In particular, we use SIMD waves 2009, 2012, 2016 and 2020, and observe outcomes such as the income, employment, and education deprivation rates, variables measuring the extent of overcrowding, alcohol use, drug use, mortality, and the overall SIMD decile of the neighbourhood. The results are summarised in Table 7.

Table 7. Gentrification

	Panel A: CSDiD							
	Deprivation in							Overall SIMD
	Income	Employment	Education	Overcrowding	Alcohol	Drug	Mortality	Decile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School Closure	-1.94**	-0.93	-108.81	-2.42***	5.34	-33.26	16.94	-0.15
	(0.87)	(0.69)	(132.71)	(0.91)	(19.89)	(34.26)	(14.93)	(0.12)
	Panel B: DiD2S							
School Closure	-1.55***	-0.59***	-72.66*	-1.01***	-2.12	-1.09	4.60	-0.00
	(0.31)	(0.23)	(40.65)	(0.30)	(6.88)	(10.74)	(3.66)	(0.05)
Observations	1,460	1,460	1,460	1,460	1,460	1,460	1,460	1,460
Data Zones	330	330	330	330	330	330	330	330
Mean DV	16.86	13.92	2907.60	14.30	131.21	138.05	109.64	4.39
SD DV	11.13	9.06	1873.86	9.79	131.44	194.27	48.99	2.53

Notes: The outcomes are variables from the Scottish Index of Multiple Deprivation (SIMD) waves 2009, 2012, 2016 and 2020, which we match to school years 2007/08, 2010/11, 2014/15 and 2018/19 respectively. The sample is matched based on the pre-treatment variables presented in Table 2. Models in Panel A are estimated using the CSDiD (Callaway and Sant'Anna, 2021), whilst models in Panel B use DiD2s (Gardner, 2022). Standard errors are clustered at the Data Zone-by-Closure level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 7 we see that following school closures, the fraction of people who are income deprived decreases by about 1.5-1.9 percentage points, which is roughly 14 to 17% of a standard deviation. Employment deprivation only experiences a .6-.9 percentage points reduction (approximately

[18]Unfortunately, we are not able to use demographic variables such as age composition or number of new dwellings built. These are normally census variables whose most recent version dates back to 2011.

10% of one standard deviation). However, this result is only statistically significant in the DiD2S specification. Moreover, we observe a reduction in the percentage of people living in overcrowded dwellings. This corresponds to circa 1-2.4 percentage points (10 to 24% of a standard deviation). These findings are suggestive of a (positive) shift in the socio-economic composition of the affected areas. In Figure 6, we do not observe a significant change in property sales post-closures, which might point towards a change in the composition of those on (social) rent. On the other hand, we do not observe a significant change in health-related outcomes, or overall deprivation.

School segregation. Another potential mechanism through which school closures could affect crime is school segregation, whose positive link to crime is well established (Weiner et al., 2009, Billings et al., 2014, 2019). As closures and mergers result in fewer schools, this might have an effect on reducing school segregation (see Chin, 2023) and in turn crime. We measure school segregation based on school-level free school meal (FSM) registrations. For every local authority, we calculate Duncan and Duncan (1955)'s index of dissimilarity from school year 2004/05 (well before the first closure) to 2018/19.[19] This index, which is widely used in literature as a measure of segregation (see for example Greaves 2023), measures the extent to which members of a group should be reallocated across units to achieve full integration, i.e. the fraction of individuals from each group is the same across units. It varies between 0 and 1. When it takes a zero value, it means that non-FSM pupils are equally distributed across schools (complete integration). Conversely, when the index takes a value of one it indicates that FSM and non-FSM pupils never share the same school (complete segregation). If the index for a local authority is .7, it implies that about 70% of FSM pupils within that local authority would need to change school, in order for the fraction of FSM pupils to be the same across schools. There are 32 local authorities in Scotland. According to the School Meals and Healthy Living Survey, the total number of schools in school year 2004/05 was 2,428, with the average local authority hosting 75 schools, compared to 2,200 in school year 2018/19, with the

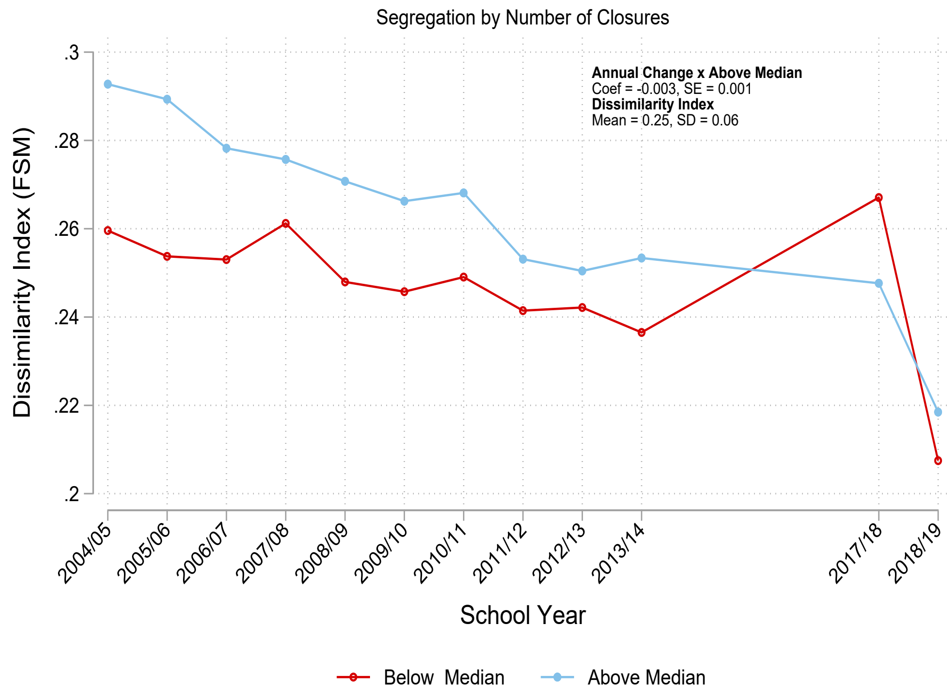
[19]For each local authority, we calculate the index of dissimilarity across S schools, based on FSM registration, i.e. FSM∈{0,1} as follows:

$$D = \frac{1}{2}\sum_{s=1}^{S}\left|\frac{n_{s,non-FSM}}{N_{non-FSM}} - \frac{n_{s,FSM}}{N_{FSM}}\right|$$

where $n_{s,non-FSM}$ is the number of non-FSM registered pupils in school s, while $N_{non-FSM}$ is the sum of all non-FSM pupils across schools, within the same local authority.

average local authority hosting 69.[20]

Figure 7. Trends in Segregation



Notes: This figure presents trends in Duncan and Duncan (1955)'s index of dissimilarity based on free school meals eligibility. We exclude school years 2014/15 to 2016/17 as we are not able to distinguish eligible pupils from those registered as a result of universal free school meals (first three years of primary school).

Figure 7 shows the trends in the dissimilarity index across two groups of local authorities: those which experience a total number of closures above the median (10 or more) between 2004/05 and 2018/19, and those with a number of closures below the median.[21] We observe a negative trend in segregation for both groups. To further examine this, we also estimate the average annual change

[20]In school year 2005/05, the average local authority hosts 75 schools, and the median 53. The maximum number is 216 (Glasgow), followed by 212 (Highland) and the minimum is 22 (Clackmannanshire). There were in total 2,428 schools. In school year 2018/19, the average local authority hosts 69 schools, and the median 49. The maximum number is 200 (Highland), followed by 168 (Glasgow) and the minimum is 18 (Dumfries and Galloway). There were in total 2,200 schools.

[21]The gap between school year 2013/14 and 2017/18 is due to the lack of reliable data on FSM eligibility. In January 2015, FSM became universal among pupils in the first three years of primary school. Between school years 2014/15 and 2016/17 our school level data only report the total number of FSM registrations without distinguishing by school stage. Therefore, for those years we are not able to distinguish those pupils universally eligible from those who are instead eligible due to their families' financial circumstances.

in segregation across the two groups. We estimate the model

$$D_{lt} = \gamma t \times \boldsymbol{I}(Above\ Median) + \alpha_l + \varepsilon_{lt} \tag{3}$$

where D_{lt} is the dissimilarity index for local authority l in school year t, $\boldsymbol{I}(Above\ Median)$ is a dummy variable for whether a local authority has enforced a number of closures above the median (above 10) throughout the entire period, t is a linear trend and α_l are local authority fixed effects. The estimated γ, alongside its standard error, is reported in the top-right corner of Figure 7. Local authorities above the median experience an annual reduction in segregation which is .3 percentage points lower than for below median (4% of one standard deviation). While this relationship is not causal in nature, we believe it might still point towards a potential mechanism, i.e. school consolidation, which results from closures, leading to a reduction in school-level segregation. In light of the positive association between school segregation and local crime, this might be a channel driving the reduction in crime rates (Akee et al., 2014, Billings et al., 2014, 2019).

6 Conclusion

In recent years, permanent school closures have been followed by considerable discontent among parents and teachers on account of their disruptive nature and unequal impact across communities. Others argue that closing down under-performing, under-enrolled schools can be a useful cost-cutting measure that also benefits pupils' academic outcomes.

In this work, we investigated whether closing schools permanently affects local crime rates. We focused on Scotland, where between school years 2006/07 and 2018/19 local governments permanently closed about 350 schools. We employed a staggered difference-in-differences design using a matched sample and found a reduction of about 9% of a standard deviation in the local crime rate following closures. Looking at effect by crime categories, we found reductions for both violent and property crimes. Unlike previous work, whose findings suggest a 'pupils-as-offenders' mechanism whereby pupil perpetrators of crimes moving away from areas drive crime reductions, in our context most closures are those of primary schools, whose pupils are typically too young to commit crimes. Instead, we showed how school closures led to gentrification of directly affected neighbourhoods, as well as an overall reduction in school-level segregation.

Our study comes with some limitations. First, we are not able to identify displaced pupils for all of the closures, and when we do, this is only for the first post closure year. Second, the most recent census wave we are able to observe is the 2011 one, as the Scotland Census 2022 data collection is still underway. For this reason, at present our study fails to provide a full understanding of the long-term consequences of school closures in terms of neighbourhoods' demographic composition.

Despite its limitations, our work carries a number of policy implications. When considering whether to close a school, policy makers need to balance the interests of a number of agents, i.e. displaced pupils, as well as those in receiving schools, along with parents and teachers. Not only do our findings contribute to the understanding of the negative effects of segregating pupils from poor backgrounds in one area or school (Weiner et al., 2009, Billings et al., 2014, 2019) but also that the effects of school closures extend beyond attainment and policy makers should also consider the interests of neighbourhood residents, regardless of whether they are part of the wider school community or not.

Further areas remain for future research. For example, scholars should investigate the longer term crime effects of closures, and also consider a variety of other related outcomes, such as employment, earnings, and, of course, criminal behaviour. In addition, future work, using more detailed data on neighbourhoods and their residents, should explore in more detail how school closures affect neighbourhood composition.

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Appendix

A Additional Tables and Figures

Table A.1. SIMD Domains Description

SIMD Domain	Components
Income	Income Deprivation Rate: Percentage of population in receipt of the main forms of means-tested benefits
Employment	Employment Deprivation Rate: Percentage of working age population who are not in employment and receive employment or disability-related benefits
Health	Comparative Illness Factor: standardised ratio[1] Hospital stays related to alcohol misuse: standardised ratio[1] Hospital stays related to drug misuse: standardised ratio[1] Standardised mortality ratio[2] Percentage of population being prescribed drugs for anxiety, depression or psychosis Percentage of live singleton births of low birth weight Emergency stays in hospital: standardised ratio[1]
Education, Skills and Training	School pupil attendance[3] Attainment of school leavers[4] Working age people with no qualifications: standardised ratio Percentage of people aged 16-19 not in full time education, employment or training Percentage of 17-21 year olds entering in to full time higher education
Access to Services	Average drive time to a petrol station in minutes Average drive time to a GP surgery in minutes Average drive time to a post office in minutes Average drive time to a primary school in minutes Average drive time to a retail centre in minutes Average drive time to a secondary school in minutes Average drive time to a secondary school in minutes Public transport travel time to a GP surgery in minutes Public transport travel time to a post office in minutes Public transport travel time to a retail centre in minutes Percentage of premises without access to superfast broadband (at least 30Mb/s download speed)[6]
Crime	Crime Rate per 1,000 inhabitants
Housing	Percentage of people in households that are overcrowded Percentage of people in households without central heating

Notes: This table provides a breakdown of the Scottish Index of Multiple Deprivation (SIMD) domains. We use 2004, 2006, 2009, 2012, 2016 and 2020 editions. As base period controls we use income, employment, health, education, access to services and housing scores. For access to services, employment, health and education, the scores are calculated by ranking the indicators, standardising them to a standard normal distribution and combining them using weights generated by Factor Analysis. Income score is simply its own rate as reported in the ***Components*** column. The housing score is the sum of its own components. **Source:** Scottish Index of Multiple Deprivation. © Crown copyright 2004, 2006, 2009, 2012, 2016 and 2020.

[1] This indicator is an indirectly age/sex standardised rate of episodes per person.
[2] This indicator is a directly age/sex standardised measure of mortality and morbidity.
[3] Previously collected as *School Pupil Absence*
[4] Previously collected as *Pupil Performance on SQA at Stage 4.*
[5] This indicator is an indirectly age/sex standardised rate.
[6] Only available in 2020.

Table A.2. Crime Classifications

Crime and Office Group Name	Crime Description
Overall Crime	Attempted Murder, Fireraising, Fraud, Housebreaking (houses and other premises), Murder, Possession of drugs, Other drugs offences (incl. importation), Possession of offensive weapon (incl. restriction), Robbery and assault with intent to rob, Theft by shoplifting, Theft from a Motor Vehicle, Insecure etc, Theft of a motor vehicle, Vandalism (incl. reckless damage, etc.), Other Group 1, 2, 3 and 5 crime.
Non-Sexual Crimes of Violence	Attempted Murder, Cruel & Unnatural treatment of children, Culpable Homicide, common law, Culpable Homicide, (others), Domestic Abuse (of female), Domestic Abuse (of male), Murder, Offensive weapon (used in other criminal activity), Possession of offensive weapon (incl. restriction), Robbery and assault with intent to rob, Reckless conduct (with firearms), Serious Assault (incl. culpable & reckless conduct - causing injury), Threatening and abusive behaviour, Other Group 1 Crime.
Sexual Crimes	All Group 2 crimes.
Crimes of dishonesty	Attempt theft of motor vehicle, Common Theft, Fraud, Failure to insure against third party risks, Housebreaking (houses and other premises), Theft by shoplifting, Theft of a motor vehicle, Threats and extortion, Other Group 3 crimes.
Fire-raising, vandalism	Culpable & reckless conduct (not firearms), Fireraising, Vandalism (incl. reckless damage, etc.).
Other Crimes	Bladed/pointed instrument (used in other criminal activity), Carrying of knives/bladed instruments, Offensive weapon (used in other criminal activity), Other drugs offences (incl. importation), Possession of drugs, Production, manufacture or cultivation of drugs, Supply of drugs (incl. possession with intent), Possession of offensive weapon (incl. restriction), Other 5 crimes.
Offences	Dangerous driving, Driving Carelessly, Driving without a licence, Driving while disqualified, Drivers neglect of traffic directions (NOT pedestrian crossings), Speeding, Drink, Drug driving offences incl. Failure to provide a specimen, Other alcohol related offences, Minor Assault, Other Group 6 and Group 7 offences.

Notes: This table provides a breakdown of the six measures of crime used in this paper.

Table A.3. Summary Statistics - Schools

	All Schools		Closed Schools		All Primary		Closed Primary		All Secondary		Closed Secondary		All Special		Closed Special	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
School Roll in September	253.28	282.26	134.21	183.88	179.63	129.03	107.97	86.59	775.82	380.59	602.06	269.51	41.44	41.08	28.93	36.32
No. of FTE Teachers	18.61	22.05	11.90	15.34	11.10	6.83	7.71	5.04	64.72	26.61	53.87	20.05	11.66	9.36	8.35	7.94
Pupil-Teacher Ratio (FTE)	13.54	4.72	10.05	5.36	14.89	3.69	12.14	4.39	11.33	2.27	10.50	2.27	3.34	3.69	2.76	1.71
Religious School (1/0)	0.15	0.36	0.14	0.34	0.16	0.36	0.13	0.33	0.16	0.36	0.23	0.43	0.10	0.30	0.14	0.34
Attendance Rate	94.11	3.39	93.13	5.06	95.03	1.86	94.30	3.94	91.21	2.47	90.15	2.36	89.87	8.10	90.42	7.30
Unauthorised Absence Rate	1.37	1.92	1.36	2.09	1.08	0.89	1.08	1.33	2.28	1.65	1.96	1.62	2.83	5.67	2.05	3.63
Exclusion Rate	0.61	3.27	1.30	5.74	0.17	0.37	0.37	0.73	1.33	1.24	2.13	1.38	3.97	11.34	4.08	12.13
% Free School Meals	0.31	0.18	0.35	0.25	0.32	0.14	0.29	0.19	0.15	0.09	0.18	0.08	0.63	0.28	0.62	0.30
Gross Internal Area (sq-metres)	3104.33	3716.66	2216.58	2945.68	1856.02	1184.18	1517.85	1014.78	10932.01	4132.16	10350.61	3790.66	1414.53	1741.61	1106.23	1049.71
Site Curtilage (sq-metres)	19673.83	24909.53	15121.80	21445.83	12864.73	10355.33	11214.29	8640.30	57768.16	34913.16	45470.02	32655.44	19253.94	40958.41	15400.95	32992.37
Schools	2,902		342		2,275		238		409		30		218		74	

Notes: These are averages from school year 2006/2007 (first closure in our sample) to 2018/2019 (last closure in our sample). Attendance, absence and exclusion rates are measures as a fraction of all episodes and the total number of possible attendance (No. of Pupils × School Days). The percentage of pupils registered (and thus eligible) for free school meals is based on the entire school population and refers to the number of pupils whose families are in receipt of the main forms of means-tested benefits. Gross internal area is the measure (in squared metres) of the indoors space of school building estate, whereas Site Curtilage also includes outdoor spaces.

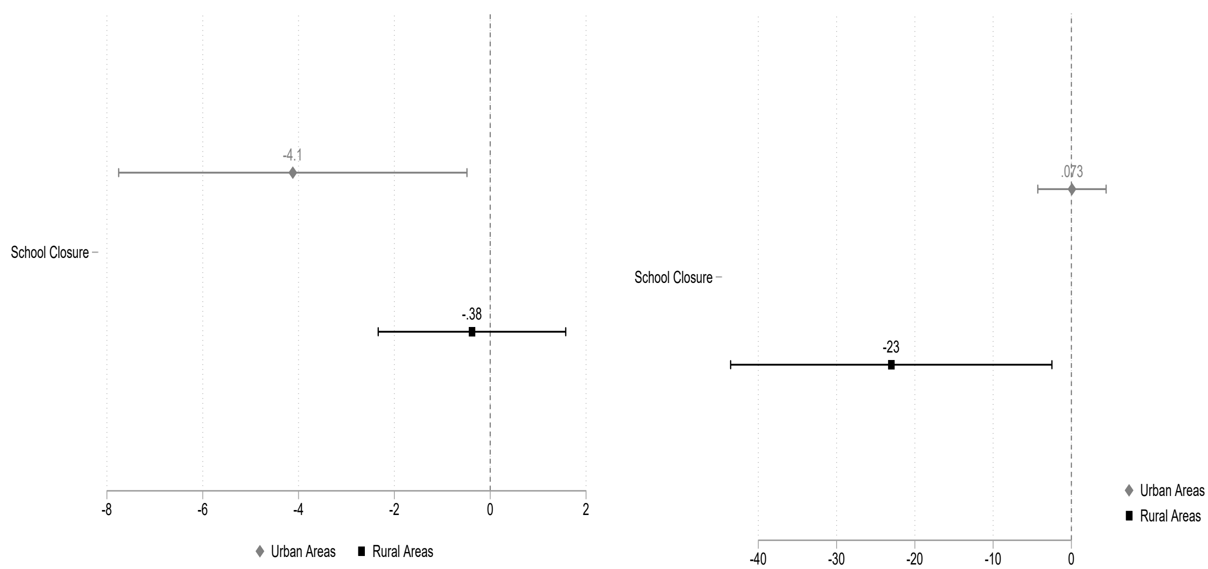
Table A.4. Baseline Results - TWFE

	All Crimes	Offences	Violent Crimes	Sexual Crimes	Dishonesty & Theft	Vandalism	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School Closure	-2.67**	-8.91	-1.36**	-0.13	-1.43*	-1.11	-0.66
	(1.31)	(9.40)	(0.64)	(0.08)	(0.73)	(0.76)	(0.65)
Observations	7,215	7,215	7,215	7,215	7,215	7,215	7,215
Data Zones	533	533	533	533	533	533	533
Mean DV	35.84	32.94	14.22	1.43	21.64	13.08	10.84
SD DV	41.10	70.85	17.63	2.14	31.06	13.69	18.55

Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models are estimated using Two-Way Fixed Effects (TWFE) estimators. All dependent variables are crime rates per 1,000 people. Standard errors are clustered at the Data Zone-by-Closure level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1. Heterogeneity Analysis

(a) Crimes **(b)** Offences



Notes: Sample spans from school year 2006/07 to 2018/2019 and is matched based on the pre-treatment variables presented in Table 2. Models are estimated using the DiD2s (Gardner, 2022) approach. Standard errors are clustered at the Data Zone-by-Closure level. Whiskers indicate 95% confidence intervals.

B Timeline of School Closures (Schools Consultations Scotland Act 2010)

1. *For rural school closure proposals only, the education authority must meet the preliminary requirements before publishing such a proposal paper*. In particular, the LA needs to identify the reason for the proposal, alongside a consideration of the possible alternatives to closure and an assessment of the educational benefits and the effects on the wider community. If closure results to be the most appropriate response, then the LA would proceed to Phase 2.

2. *An education authority consults on a proposal – for a minimum of six weeks, including at least 30 school days*. This includes a statement on the educational benefits of the closure, as well as financial information. For rural schools the authority must provide a detailed assessment of all possible alternatives to closure. This procedure concludes with the publication of the proposal paper.

3. *Preparation of Education Scotland's report on the educational aspects of the proposal - to be completed within a maximum of three weeks*. After ES receives the proposal paper, an inspector provide the LA with a report including the feedback on the educational benefits originally stated in the proposal paper.

4. *Consultation report - within no specified timescale, the authority prepares and publishes a consultation report*. This constitutes a response to the ES report, and how the LA intends to address any concern raised by ES.

5. *Authority decision - a minimum of three weeks after the publication of the consultation report the authority publishes its final decision*. If the LA makes a closure decision, it must notify the Scottish Ministers and, in case of a rural school's closure also publish a notice on the website on the intention to close the school.

6. *Ministerial call in, only where the authority makes a closure decision - a maximum of eight weeks*. This can occur in the event the Ministers believe the LA did not comply with the Act (2010)'s requirements. This phase consists of two parts: First, three weeks from the date of the

authority's decision, during which anyone can make representations to Ministers on whether the decision should be called in. Second, a maximum of further five weeks for Ministers to decide whether or not to issue a call-in notice. Ministers may require information from the authority during this period. On 30 March 2015 the School Closure Review Panels, independent statutory bodies created *ad hoc*, took over the responsibility from the Scottish Ministers

7. *School Closure Review Panel Determination, a maximum of nine or 17 weeks*. The panel might: *i* refuse consent to the proposal; *ii* refuse consent and remit it to the education authority for a fresh decision; *iii* grant consent to the proposal, either subject to conditions, or unconditionally.

8. *Restriction on school closure consultation for five years*. This only applies in case the School Closure Review Panel refuses consent to the closure proposal or if at the and of Phase Five, the LA decides not to proceed with a closure.