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Money changes everything? Education and regional deprivation revisited

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1. Introduction

A long line of research has sought to explain the relationship between regional deprivation and educational outcomes, with high levels of adversity often shown to have a deflationary impact on achievement. However, a questionable leitmotif of this literature, stretching back to, e.g., Tolley and Olsen (1971), is the use of economic variables to reflect deprivation. Whilst understanding of the relationship has developed significantly in recent years, with causality isolated through experimental and quasi-experimental methods, measurement of deprivation by economic proxies remains prevalent. In a large percentage of cases, a negative impact is confirmed, but transfer mechanisms and policy recommendations remain under-explained. It is, for example, theoretically ambiguous why exogenously increasing income should have a singular and direct effect on improved educational performance.

In this vein, this article challenges the wisdom of a causal link stemming from financial deprivation. We propose that deprivation is a much wider phenomenon, implying that the real relationships have, so far, only been approximated. Instead of a direct effect, we suggest that the real impact stems from an array of adverse social conditions that are often, although not always, highly correlated with financial deprivation. To this end, we ask three research questions. First, fitting with the literature to date, we ask whether or not there is a causal relationship between deprivation and schooling outcomes. Second, we ask whether or not financial poverty acts as a valid proxy for neighbourhood deprivation. Finally, we ask whether there is a unique impact of financial deprivation, by conducting an analysis that includes both financial deprivation metrics and an aggregate measure of other deprivation indicators.

To do so, we employ the Northern Ireland Multiple Deprivation Measure (NIMDM), which captures variations in

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neighbourhood deprivation through seven domains. In addition to those expected, such as financial or employment deprivation, the NIMDM includes five subsequent indicators. The first is education deprivation, which looks at the prevalence of skills and achievements in the local population. Next is health deprivation, which, amongst other things, includes metrics for disabilities, expected life span and prevalence of mental health issues. The next two domains measure various aspects of local neighbourhood quality through living environment indicators, such as house over-crowding, quality of heating, etc., and through a neighbourhood's proximity to important services, such as healthcare facilities, post offices, food stores and so on. Crime is the final sub-component of the NIMDM and measures, amongst other things, the prevalence of various forms of theft and property damage.

By using this measure, this paper constitutes one of the first major attempts to employ a wider deprivation measure and is certainly the first to use such data to question the use of economic proxies in studies to date. We focus on Northern Ireland, rather than the United Kingdom as a whole, as we infer causality through a unique instrumental variables approach, which links historical conflict to current levels of deprivation. Within the developed world, Northern Ireland is one of the few countries to have experienced such a conflict in recent years. Perhaps more importantly, it is also the only case where the conflict can be argued to be truly exogenous to current primary school outcomes. Violence was low-intensity, most commonly involving small arms. As a result, comparatively little property damage occurred and, to the best of the authors' knowledge, no schools were destroyed. Similarly, the relatively abrupt end of violence in 1994 provides a strict measurement point for the cessation of violence, which is not always available in other conflicts. As violence ended over 20 years ago, we study a large cohort who were educated after the violence ended. In other conflicts in the developed world, such as that in the Basque Country for example, multiple cohorts are not available. Despite this uniqueness in the history of Northern Ireland, however, the state remains highly comparable to the United Kingdom as a whole.[1]

We use a bespoke dataset drawing on Sutton (1994) that links each conflict fatality to one of Northern Ireland's 582 electoral wards – the highest geographic disaggregation of violence data ever produced for Northern Ireland. The spatial variation of this violence (Fig. 1) is highly correlated with contemporary deprivation but can reasonably be assumed to be uncorrelated with our outcome of interest. We match this data to school attainment using the performance of each primary school in Northern Ireland in the standardised Key Stage II exams for the academic years 2000/2001–2010/2011.

We show a negative causal relationship between deprivation and school performance in our baseline analyses, where we use the net NIMDM. Once we disaggregate the NIMDM into its sub-components, we find that neither financial, nor employment, deprivation are significant drivers of school performance when we include an aggregated measure of the other domains. However, both exhibit a negative and significant coefficient when included in isolation. Thus, financial and employment deprivation act as valid proxies for wider neighbourhood deprivation in this case but cannot alone explain the causal mechanisms at play. This implies that the findings of the literature to date are conditional on the strength of the correlation between actual neighbourhood deprivation and the economic proxies employed. Similarly, they also imply that policy interventions that aim only to redistribute income will prove less successful in mitigating the negative impacts of deprivation than policies that tackle a wider array of sources of deprivation.

The remainder of this article is set out as follows: in the next section, we review the literature relevant to this study. In Section 3, we describe the data we use. In Sections 4 and 5, we describe our methodology and present our results. We provide a discussion of the Northern Ireland-specific context and the external validity of the findings in Section 6 and conclude in Section 7.

2. Literature review

The positive role played by good primary school performance on the formation of human capital and of individuals' future labour market outcomes is uncontroversial (Sparkes, 1999). When coupled with the long history of literature showing the relationship between regional deprivation and education outcomes (e.g. Tolley and Olsen, 1971), however, two important questions arise. First, if there is a link between deprivation and primary school outcomes then concern must arise about cycles of poverty. Those children who grow up and are educated in the most deprived regions are those most likely to spend their adulthood living in poverty. Second, should such a cycle exist, it is important to understand the causal mechanisms by which the effect is transmitted, in order to allow the development of suitable policy interventions. If, for example, financial poverty can explain the relationship, then income redistribution could play a joint role in alleviating both current and future deprivation. Alternatively, if, as Tolley and Olsen (1971) suggest, the effects are less direct, then more specific and nuanced policies would be required.

Tolley and Olsen (1971) argue that the effect is based on richer regions simply having the greatest ability to invest in education infrastructure, suggesting richer parents gain access to the best schools. In such a setting, deprivation only plays an indirect role and suitable policy interventions would require greater investment in schools in impoverished areas. Although the notions this raises of endogeneity and reverse causality are important considerations in subsequent literature, these findings still imply that there is a causal relationship that runs between deprivation and education outcomes.

Measuring deprivation at either the household or neighbourhood level is notoriously complex, however. To this end, a series of proxies have been developed in the economic literature. These include parental education (Cremer, Pestieau, & Rochet, 2003), family income (Blau, 1999) and parental occupation (Oreopoulos, Page, & Stevens, 2008) in studies looking at individual outcomes. At the regional level, the proportion of social housing (Weinhardt, 2010), number of house demolitions (Jacob, 2004) and the proportion of

[1] Dorsett (2013), for example, simulate Northern Ireland's economy using a composite of other UK regions and although there are small differences in the education and health systems, they are broadly similar in organisation, quality and availability.

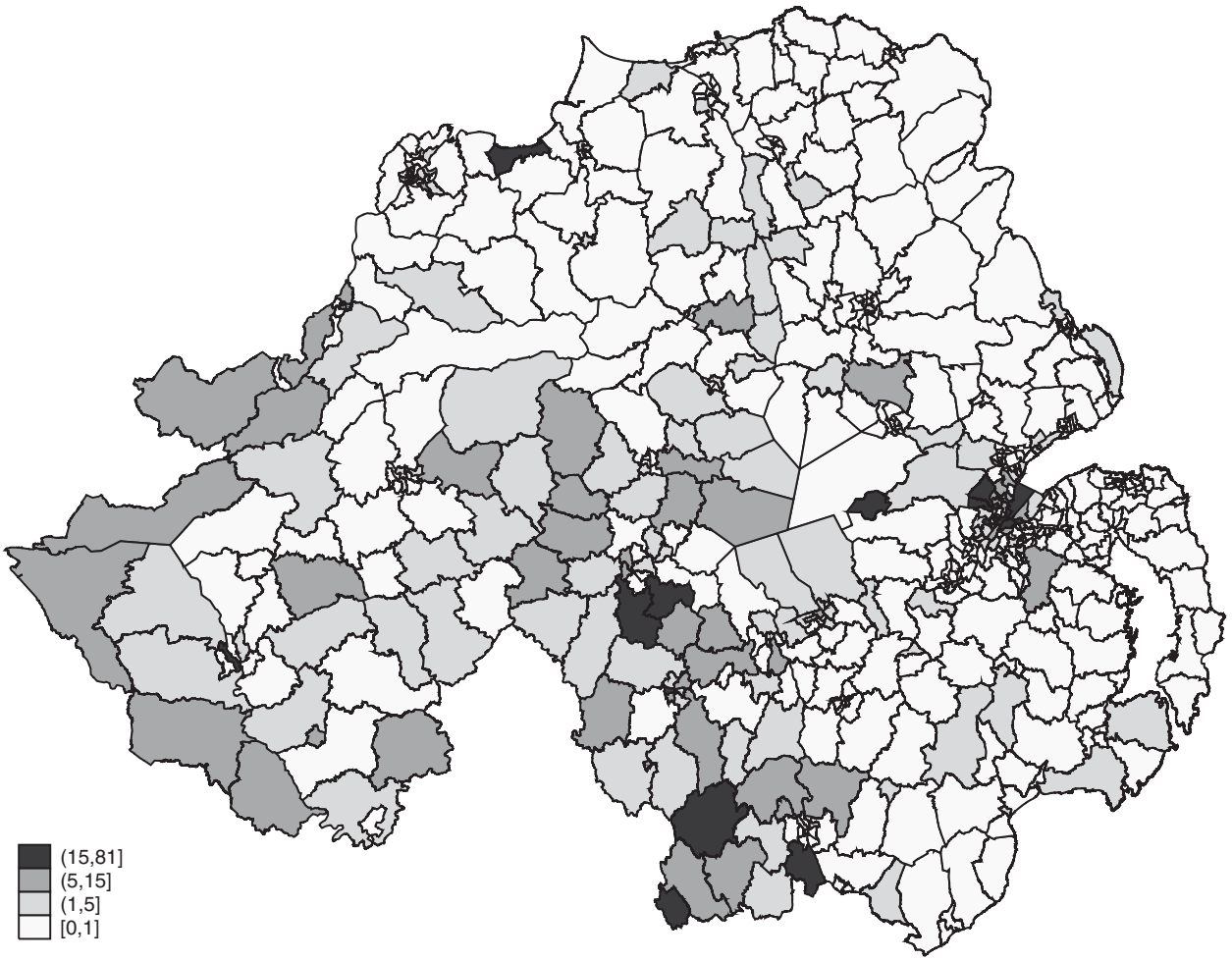

Fig. 1. Number of conflict-related deaths by wards between 1969 and 1994 in Northern Ireland.
Source: Authors' construction using data by Sutton (1994).

children receiving free school meal entitlement (FSME) (Harwell & LeBeau, 2010) have been used. Such proxies remain imperfect, however, as they are all more or less measures of regional or individual financial well-being. At the same time, however, parental education, income or occupation could easily be related to unobservable characteristics like latent ability. Should such unobservables be the only drivers of the measured outcomes, then neither exogenous increases in income nor investment in schools will yield observable improvements in schooling outcomes.

Noting such potentially convoluting factors, recent literature has used experimental or quasi-experimental approaches. Milligan and Stabile (2008) and Duncan, Morris, and Rodrigues (2011), for example, exploit the introduction of government income transfers. Both establish a positive relationship between such exogenous increases in income and educational attainment. Løken (2010) and Løken, Mogstad, and Wiswall (2012), alternatively, use the uneven geographic dispersion of gains from the Norwegian oil boom as a source of exogenous variation in wealth. Both articles show a pronounced positive impact on school attainment amongst low-income families. Bayer, Ross, and Topa (2008), Davis-Kean (2005) and Cutler and Glaeser (1997) further support such findings, although the likes of Jacob (2004) present contrary evidence. Using demolitions of high-rise public housing in Chicago and the movement of those affected as a source of exogenous variation, Jacob (2004) shows that those who moved as a result of demolitions have neither better nor worse educational outcomes than their peers.

Therefore, despite the relative uniformity of the findings throughout the literature, there remains a lack of robust or detailed explanation of the transfer mechanisms at play. Particularly at the neighbourhood level, it is not immediately obvious why living in an impoverished area, regardless of a household's own characteristics, should lower educational attainment. Financial poverty, therefore, appears to be only one of a number of potential mechanisms through which the negative effects of deprivation can be transmitted. Individual health, and particularly parental health, has frequently been linked to adverse education outcomes, for example (see McLoyd, 1990; Sun & Yao, 2010; Choi, 2011; Bratti & Mendola, 2014). Similarly, a wealth of literature has focused on the relationship between social capital and education outcomes. Coleman (1988), for example, argues that social

capital, both within the family and at the level of the neighbourhood, aid the formation of human capital. Subsequent empirical studies confirm Coleman's theory using hirachical models that account for individual-, family- and neighbourhood-level unobservables (e.g. Sun, 1999; Gradstein & Justman, 2000; Misra, Grimes, & Rogers, 2013).

Both health and social capital are, in turn, likely to be correlated with financial deprivation and, indeed, are likely to be important components of wider neighbourhood deprivation measures. The transfer mechanisms from both are also, if anything, more intuitive than those from poverty. Parents with poor health or mental health, for example, may be less physically capable of aiding children's studies than those in better health. Similarly, areas with high social capital are likely to create better opportunities for human capital accumulation than areas with lower social capital. At the same time, however, it seems probable that these indicators will be correlated with other sources of deprivation. Parents with poor health, for example, may also be less able to work, simultaneously influencing children's education outcomes and household income. Such correlations, in turn, suggest a need to focus more broadly on the manifold sources of regional deprivation through the use of so-called multiple deprivation indices, rather than on single measures of deprivation. Although some studies have used such indices, such as Garner and Raudenbush (1991), Andersson and Subramanian (2006) and Barnes, Belsky, Broomfield, Melhuish et al. (2006), these approaches, along with the studies focusing on social capital, typically do not account for endogeneity or selection issues, nor do they attempt to disentangle the effects of the different correlated neighbourhood characteristics.

To date, therefore, despite relatively uniform findings in the literature, a holistic approach has been lacking. Hitherto, a number of potentially correlated variables have all been shown to negatively influence outcomes, yet the literature has largely failed to show if each has a unique causal effect. This is particularly apparent with the use of financial deprivation measures, where the literature has insufficiently explained why making individuals, families or neighbourhoods exogenously richer should have a direct impact on the educational attainment of those whose wealth increases. In the cases of Løken (2010) and Løken et al. (2012), for example, it seems unlikely that the uneven spatial distribution of the gains during the earlier years of the Norwegian oil boom affected only family income. Rather more likely, a whole array of neighbourhood services, including access to healthcare and investment in education and infrastructure, are likely to have simultaneously improved. This, in turn, is likely to be suggestive of concurrent improvements to the financial, health, education, proximity to services and living standards deprivation domains present in our multiple deprivation index. Any effect transmitted through such improvements in the neighbourhood as a whole, in turn, has vastly different policy implications than one transmitted through income alone.

A final limiting feature of this strand of literature is that potential selection issues are not always addressed. It is likely, for example, that families select into neighbourhoods. Particularly if such selection is based on a number of desirable local features, such as reputation of nearby schools, biases may arise. Poor families, however, are likely to have less choice over where to live and, therefore, may lack access to the best schools. This complexity is addressed in the likes of Goux and Maurin (2007), Gibbons, Machin, and Silva (2008) and Bayer and Ross (2006), who use panel approaches to overcome these selection effects and to confirm the negative relationship between deprivation and education outcomes. In this respect, and given the continuing debate in the literature about outcomes and methodologies, knowledge gaps remain. It is important to understand not just the size or direction of effects but also the causal mechanisms through which these effects are transmitted. To this end, we employ an index of deprivation as in Lupton (2001) and Barnes et al. (2006) but use the availability of the multiple domains in an attempt to disentangle the unique effects of each single domain. Similarly, we use a unique instrumental variables approach and panel data models to overcome the endogeneity and selection biases commonly mentioned in the literature.

Table 1
Descriptive statistics of violence data.

	Mean	Std. dev.	Min	Max
Deaths	3.67	7.88	0	81
Deaths per 1000 inhabitants	1.21	2.40	0	25
Deaths $\geq$ 1 (median)	0.61	0.49	0	1
Deaths $\geq$ 4 (mean)	0.27	0.44	0	1
Deaths $\geq$ 9 (top decile)	0.11	0.31	0	1
N	5937			

Note: Authors' calculations based on Sutton (1994). "Deaths" measure the number of deaths in the ward of the respective schools. Deaths per 1000 inhabitants divides the number of deaths occurring during The Troubles by population in year 2010, and is, hence, only an imperfect proxy for the real death rate.

3. Data

This article brings together a number of substantial data sources that, hitherto, have not been used in an empirical study of education outcomes, including a bespoke and unique construction of Sutton's (1994) index of deaths from the conflict in Northern Ireland. To date, the highest available geographic disaggregation of Northern Irish violence data could be found in McKeown (2009), which links fatalities to one of Northern Ireland's 18 Westminster parliamentary constituencies. The presentation of Sutton (1994) in this article links each fatality to one of Northern Ireland's 582 electoral wards, significantly increasing the available variation of this data. We construct a variable *Deaths*, which counts the total number of fatalities that occurred in each ward between the death of Francis McCloskey in June 1969 and the PIRA ceasefire of August 1994. Fig. 1 displays the spatial variation of this variable. In total, the conflict caused almost 3600 fatalities, 95% of which are included in our dataset (Table 1).[2]

Some 200 wards experienced no fatal violence whilst others suffered in excess of 75 fatalities. Some notable incidents have occurred since the paramilitary ceasefires in 1994, but fatal violence has been extremely rare. To account for such violence, we include an annual measure of the number of bombing and shooting incidents that have taken place in our

[2] A significant proportion of the 5% of fatalities not included in our sample took place outside of Northern Ireland.

sample period. This data is directly sourced from the Police Service of Northern Ireland's (PSNI) statistical office and allows us to provide a complete picture of both historical violence and incidents that have occurred since 1994.

To measure educational outcomes, we employ a panel of primary school data that runs from the 2000/01 academic year until 2010/11, with the exception of 2003/04, which is unavailable due to a teachers' strike. We begin our analysis in 2000/01 as this is the first year in which the entire cohort entered primary education after the 1994 ceasefires.[3] We exclude private preparatory schools[4] and the few schools that did not provide data for Key Stage II achievements or other important school-level controls. This school-level data is sourced from the Department of Education for Northern Ireland's (DENI) annual school census. We include information on the teaching language of the school (English or Gaelic), the number of pupils per school, the class-size of the examined cohort and the teacher:pupil ratio in each school. This results in a large, unbalanced panel of $n \times T = 5937$ school-years. We construct a variable *Average Pass Rate* (APR), which is the mean of the proportion of students achieving at least a Level 4 pass in English and Mathematics, respectively. Key Stage II is a formal national exam taken by all children at approximately age 10 and is assessed against a number of "Levels" running from Level 1 (lowest) to Level 8 (highest). Level 4 is designated as the difference between "pass" and "fail", with those attaining below Level 4 deemed to have fallen behind.

Because of our estimation strategy, it is infeasible to use educational attainment data for children who started education before the ceasefires of 1994. Focus on post-primary education outcomes, therefore, is rendered impossible. Similarly, the links between schools and local neighbourhoods are significantly weaker when using post-primary school-level attainment as our intervening variable of interest, yet these ties are incredibly strong for primary schools. Given the absence of individual-level explanatory variables such as hours spent doing homework or number of absentee days, however, little is lost by looking at average achievement at school rather than individual level (see e.g. Gibbons, 2002). Additionally, given that we focus on a standardised procedure sat by all pupils in Northern Ireland, we can create a nationally comparable measure of achievement for a number of cohorts. Development of this panel, which would be infeasible using individual level data, allows modelling of both school- and neighbourhood-level unobservables.

We use the NIMDM index from 2005, which was collected by the Northern Ireland Statistical Research Agency (NISRA) in 2003 and which was obtained from the Social Disadvantage Research Centre at the University of Oxford. The NIMDM is a five-yearly measure that aims to identify small-area concentrations of deprivation in Northern Ireland and provides information on seven domains, each of which is designed to measure a distinct type of deprivation. These domains and their weights as suggested by NISRA are: income deprivation (25%), employment deprivation (25%), health deprivation and disability (15%), education, skills and training deprivation (15%), proximity to services (10%), living environment (5%) and crime and disorder (5%).

Each domain is built from a series of sub-indicators. Income deprivation, for example, is based on a number of metrics that have been, or could be, used as proxies for regional deprivation in the literature. This includes the proportion of households on income support, the proportion of households with state pensions, the proportion receiving jobseekers' allowance, etc. Ideally, this measure would simply measure the proportion of households living under the UK's commonly used poverty-threshold, which is 60% of median income.[5] Survey data, however, is unreliable at the small-area level of the NIMDM's analysis, precluding use of such a metric. As shown in Fenton (2013), however, there is a close correlation between the "households below average income rate" and a so-called "unadjusted means-tested benefits rate", which is "[…]similar to the income domains of the various UK indices of deprivation" (Fenton, 2013, p. 30). In this regard, although there is some imperfection in the use of data on benefits, as discussed in the NIMDM 2005 Final Report,[6] such an approach provides a reliable proxy for true average financial deprivation in small-areas. A full list of each domain's indicators can be found in Table A.9. We construct an overall multiple deprivation measure, which consists of a weighted average of all domains. Following the concerns about weighting of such indices influencing outcomes (see e.g. Haisken-DeNew & Sinning, 2010), we apply a series of different weights as robustness checks.

Our preferred identification strategy links annual variation in school-level attainment to a single iteration of this index. Although measures are available in 2001, 2005 and 2010, it is not possible to use all three in combination. The 2001 measure is geographically disaggregated to 1984 wards, rather than those in current use, meaning that the small-area boundaries between 2001 and later measures are not directly comparable. Such boundary changes also pose significant issues for important ward-level control variables, which are not available at 1984 area boundaries. Although geographically comparable, the 2005 and 2010 measures are constructed from different sub-domains, suggesting they are also not directly equivalent. This is particularly problematic with the education domain, where the 2010 measure includes sub-domains referring to primary school performance, creating an obvious endogeneity problem. The education domain of the 2005 measure, comparatively, only includes sub-domains that focus on education deprivation of individuals older than fourteen years of age.

In this context, therefore, we include only the 2005 measure, due to the availability of ward-level control data and the exogeneity of the education domain. As this data was

[3] We estimate our models on a subsample of the cohorts (2004/05–2010/11) who were born after the conflict, to ensure that the results are not driven by early child exposure to violence. Results from this subset are not materially different from those from our baseline analysis and can be found in Table A.8.

[4] Whilst most primary schools in Northern Ireland service local communities, most pupils attending private schools do not live in the region surrounding the school. As this link between school and community is important for our empirical strategy, we exclude these schools.

[5] Source: https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/325416/households-below-average-income-1994-1995-2012-2013.pdf.

[6] See: http://www.nisra.gov.uk/archive/deprivation/nimdm2005fullreport.pdf.

Table 2
Descriptive statistics of school data.

	Low violence area		High violence area		Difference	
	Mean	Std. dev.	Mean	Std. dev.	Diff.	SE
Average pass rate	79.88	(12.64)	77.09	(14.29)	2.78***	(0.38)
Number of pupils	31.93	(24.34)	27.56	(19.57)	4.38***	(0.68)
Total enrolment	225.00	(159.75)	193.70	(132.06)	31.30***	(4.50)
Pupil–teacher ratio	20.21	(2.95)	19.37	(2.88)	0.84***	(0.09)
Gaelic exams	0.02	(0.13)	0.02	(0.13)	−0.00	(0.00)
Catholic school	0.44	(0.50)	0.57	(0.50)	−0.13***	(0.01)
Catholic inhabitants (%)	41.99	(30.38)	55.61	(27.03)	−13.62***	(0.86)
All persons 0–15	689	(341.76)	735	(272.99)	−46***	(9)
All persons 16–39	1018	(472.40)	1137	(680.53)	−119***	(16)
All persons 40–59/64	827	(308.46)	848	(306.32)	−21**	(9)
All persons 60/65+	464	(169.04)	526	(233.84)	−62***	(6)
Bombing incidents	2.90	(5.45)	5.61	(17.95)	−2.70***	(0.30)
Shooting incidents	4.61	(7.18)	8.29	(17.96)	−3.68***	(0.33)
Multiple DM	0.00	(0.00)	0.00	(0.00)	−1.95***	(0.10)
Income DM	0.20	(0.11)	0.29	(0.14)	−0.09***	(0.00)
Employment DM	0.15	(0.05)	0.19	(0.07)	−0.04***	(0.00)
Education DM	20.23	(13.00)	28.37	(19.86)	−8.13***	(0.44)
Health DM	−0.16	(0.58)	0.14	(0.89)	−0.30***	(0.02)
Services DM	−0.31	(0.75)	−0.29	(1.02)	−0.02	(0.02)
Living env. DM	21.53	(11.07)	28.45	(13.21)	−6.92***	(0.34)
Crime DM	17.22	(16.09)	23.26	(23.09)	−6.04***	(0.53)
N	4337		1600		5937	

Note: Authors' calculations based on primary school data from DENI, NIMDM scores from NISRA (2005), conflict data from Sutton (1994) and contemporary violence data from PSNI. In a high violence area, at least 4 deaths occurred (3.77 is the mean number of deaths). SE means standard error.
*Significance level of 10%.
** Significance level of 5%.
*** Significance level of 1%.

collected in 2003, no child within our sample has yet reached an age where their own performance could be included within any of the sub-domains. We accept that such an approach may introduce a small amount of bias into our results, as the 2005 NIMDM may be an imperfect proxy for actual deprivation in each of the other years. Any effect here, however, is likely to be small, due to the high unconditional correlations between the NIMDM measures. Between 2005 and 2010, for example, this correlation is 93%, suggesting very low year-to-year variation. Although the 2001 MDM is not directly comparable, a significant proportion of 1984 wards share names and have a large amount of geographic overlap with those used in the 2005 and 2010 MDMs. By matching these wards, we link 397 of the 449 wards in our sample with a ward in 2001. The unconditional correlation of the MDM scores for these linked wards is 95% between 2001 and 2005 and 92% between 2001 and 2010, further supporting the assertion of very low year-to-year variation. Any variations that do arise, therefore, are likely to be akin to measurement error, which our IV approach also accounts for. This, in turn, suggests that the output from any single iteration of the NIMDM is a valid proxy for the true level of annual deprivation.

The panel data analysis facilitated by such an approach also accrues a number of benefits, which significantly outweigh these concerns. Most importantly, it allows us to control for school-level and neighbourhood-level unobservables and selection effects through the inclusion of random effects.

Table 2 provides summary statistics on the overall NIMDM score and the single domains as well as school level and ward level variables. We provide the statistics for two samples; schools located in areas of high violence, i.e. areas where the number of deaths is higher than the mean (see Table 1), and schools located in areas of low violence, i.e. areas where the number of deaths was below the mean.[7] Table 2 shows that children meeting or exceeding the minimum expected Level 4 pass is significantly higher in low-violence than high-violence areas. High violence areas also exhibit certain demographic differences, typically having larger populations and higher levels of deprivation. The distribution of average pass rates and the NIMDM score are displayed in Fig. 2. In the estimations, the NIMDM scores are standardised with mean zero and a standard deviation of one to facilitate comparison between the domains. Due to the high unconditional correlations between some of the domains,[8] our analyses may give rise to the potential for multicollinearity between the deprivation indicators, when included jointly. To ensure this issue does not influence our results, we conduct a series of subsequent analyses where we drop each domain in turn from the index of remaining domains and test the robustness of the results.

4. Estimation methods

While the unconditional relationship between multiple deprivation and school performance is negative, questions

[7] We also divide the sample by median deaths (1) and by the threshold that divides the top decile of schools from the rest (9). These summary statistics suggest that differences in means are smaller when using the median and larger when using the top decile threshold. Tables are available on request from the authors.

[8] The unconditional correlations between the NIMDM domains are shown in Table A.1.

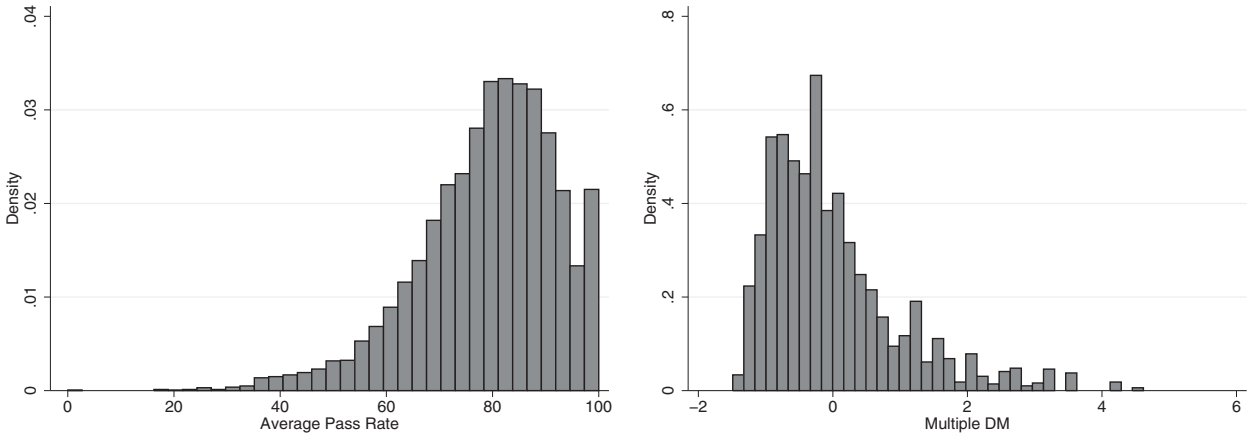

Fig. 2. Average pass rates and MDM score density.
Source: Authors' construction.

remain about causality. It is, for example, likely that high ability parents will give birth to high ability children or raise them in a way that they develop their full ability potential (e.g. Becker, 1993; Griliches & Mason, 1972; Mincer, 1958). Therefore if, say, only lower ability parents live in deprived areas, children in these areas may perform more poorly in schools for reasons correlated with, but not caused by, local deprivation. Similarly, parental health or education, as discussed in a long history of intergenerational literature (e.g. Black, Devereux, & Salvanes, 2005; Choi, 2011; Haveman & Wolfe, 1995; Holmlund, Lindahl, & Plug, 2011; Gertler, Levine, & Ames, 2004) are likely to be related to deprivation but not necessarily caused by it.

At the school-level, another array of concerns about attributing causality arise. Teacher quality or school ethos, for example, are likely to be correlated with both deprivation and school performance. As these differences are unobservable and consequently omitted from the regression, identification of the true effect of deprivation is unlikely in simple OLS approaches. To overcome such biases, we follow an instrumental variables approach as described in, for example, Angrist and Pischke (2008). In this setting, we require one or more variables that are correlated with each endogenous deprivation variable but which are also uncorrelated with the regression error term. To this end, we construct two instruments. The first is total number of fatalities that occurred within an electoral ward during The Troubles, which is the *Spatial Lag Deaths* variable we describe in the data section. The second instrument is the spatially lagged value of violence.[9] This is a non-truncated inverse distance weighted value of deaths that occurred in the neighbourhoods surrounding the area in question but not within the neighbourhood itself. We call this instrument *Spatial Lag Deaths*.[10]

Due to potential collinearity between the two instruments, we use a third order polynomial of the *Deaths* variable and the first order polynomial of *Spatial Lag Deaths*. Model statistics, such as the Angrist–Pischke multivariate F-test of excluded instruments, show this to be the optimal valid transformation. Including two instruments in this manner allows us to account for up to two potentially endogenous variables within our analyses.

Therefore, we can write the basic relationship as a hierarchical model as follows:

$$APR_{ijt} = \beta_0 + \beta_1 MDM_j + \beta_2 X_{it} + \beta_3 Z_{jt} + u_i + v_j + \epsilon_{ijt}, \tag{1}$$

where i is the subscript for individual schools, j for electoral wards and t for years. *APR* is the average pass rate, *MDM* is the endogenous multiple deprivation measure. X and Z are vectors of exogenous regressors at school- and ward-level, respectively. u is school-level unobserved heterogeneity, and v represents ward-level unobserved heterogeneity. β_0 is a constant and the other β's are (vectors of) regression coefficients and ε is the error term. As u and v are unobservable, the OLS coefficient of MDM will be biased, because $COV(u_i, \varepsilon_i) \neq 0$. We therefore follow an instrumental variables approach where we regress the endogenous variable, *MDM* on all exogenous regressors X and Z and the instruments *IV*

$$MDM_j = \alpha_0 + \alpha_1 X_{it} + \alpha_2 Z_{jt} + \alpha_3 IV_j + \eta_j. \tag{2}$$

Here, α_0 is the constant, the other α's are regression coefficients and η is the error term. For *IV* to be valid instruments, they must be partially correlated with *MDM*, i.e. $\alpha_3 \neq 0$ and they must be uncorrelated with the error term ε, i.e. $COV(IV, \varepsilon) = 0$ (Wooldridge, 2002). Whilst we can show, statistically, that our instruments fulfil the first condition, instrument exogeneity can only be argued qualitatively.

Our careful sample selection and focus on primary school data ensures that all children in our study began primary education after the ceasefires of 1994, whilst a robustness check focuses only on children born after the end of violence. This precludes any direct effects of violence on education as noted in a long line of literature (e.g. Chamarbagwala & Moran, 2011; Swee, 2009; Kahanec & Yuksel, 2010; Brueck, Maio, & Miaari, 2013; Kibris, 2014). Accordingly, any relationship between the instrument and the error term would rely

[9] Given recent literature (e.g. Besely & Mueller, 2012), we know that violence in nearby regions is likely to correlate with local socio-economic conditions.

[10] More technical information on the construction and use of spatial lags can be found in, e.g., Ward and Gleditsch (2007).

on long-term or more indirect relationships – for example, long-term under-investment in schools in the most socially disadvantaged areas. Our comparative statistics in Table 2 immediately refute some of these possibilities, however. Typical indicators of school quality, such as pupil:teacher ratio or class size, are actually significantly better in the areas that experienced the worst violence. Other unobservables, such as low teacher quality or motivation in conflict areas could also cause identification issues. This should, however, also be unproblematic due to active central government funding and incentive schemes designed to send the best teachers into the most socially disadvantaged schools.

Other effects, such as intergenerational transmission of distress from parents exposed to the conflict to their children may play a role.[11] However, our deprivation measures, particularly the health deprivation and disability domain, are likely to account for such issues as they include measures of regional mental health levels. Thus, the indirect long-term effects of conflict are already accounted for in the analysis, precluding problematic correlation between the instrument and the error term. Similarly, other control variables are likely to account for any unobservable effects arising from unobserved parental capability. Finally, there is no evidence of long-term relationships between the occurrence of conflict and the education outcomes of those not exposed to conflict. Chen, Chen, and Liu (2009), for example, use a panel of forty countries over forty years and find that, once violence ends, recovery in economic performance, health and education occurs rapidly. Thus, given the controls included, the nature of school funding in the United Kingdom and the breadth of our deprivation measure, we offer evidence that refutes the conceivable sources of instrument invalidity. To further eliminate such effects, we control for contemporaneous violence.

A second potential bias arises due to the non-random selection of families into neighbourhoods. Families, particularly those with the greatest financial resources, have large degree of choice of where they live. This is, perhaps, uniquely important in the context of Northern Ireland, with Besely and Mueller (2012) showing that house prices in non-violent areas increased as a result of violence in nearby areas. Selection is likely to be determined, therefore, by financial means, resulting in artificial increases in some of our deprivation domains in conflict-affected regions. If this selection varies by, say, school catchment areas, denoted u_i in Eq. (1), our instruments will account for this kind of selection bias.

School catchment areas, however, are seldom enforced and children can be enrolled into any primary school, as long as it is not over-subscribed (BBC Learning, 2013). Therefore, it is likely that families will not directly select into these catchment areas but into larger geographic areas, such as electoral wards, which exhibit a wider array of desirable features. Such effects are not accounted for by our instruments alone and would introduce biases into our findings if left unchecked. To account for this, we propose that selection into wards can be seen as a random effect, represented by v_j in Eq. (1). The performance of each school, then, should depend on two independent factors: unobservable school-level qualities and unobservable neighbourhood-level qualities. Accordingly, each school will have some distribution of exam success based, in part, upon these two unobservable effects. The neighbourhood random effect should be identical for each school within a region, whilst the school random effect is unique to each.

To overcome these potential biases, we estimate a random effects two-stage least squares model (RE2SLS) as in Balestra and Varadharajan-Krishnakumar (1987), which allows for a more general error structure than is required for consistency in 2SLS estimations. The usefulness of random effects models in education research is discussed in Clarke, Crawford, Steele, and Vignoles (2010), who show how they can account for multiple effects. Although the smallest unit of observation in Clarke et al. (2010) is pupils and the second level of analysis is schools, this can easily be 'up-scaled' in our case to account for school and electoral ward heterogeneities. To further increase our modelling efficiency, we also estimate an error-component two-stage least squares model (EC2SLS) (Baltagi, 1981). The EC2SLS model is essentially an extension of the Balestra and Varadharajan-Krishnakumar (1987) model, using a larger set of instruments.

While RE2SLS uses the GLS transformed instruments X and Z, in EC2SLS these GLS transformed instruments are, themselves, treated as endogenous and are instrumented with both the within-transformed instruments $\widetilde{Z}$ and the between transformed instruments $\overline{Z}$. Accordingly, EC2SLS uses up to double the number of instruments as RE2SLS and is shown to be more efficient in small samples (Baltagi & Liu, 2009). Baltagi (2008) provides more technical details of the methods.[12]

We run three main versions of our analyses. In the first, we include the composite MDM measure and both instruments. In the extended analyses, we estimate the effect of each single domain on school APR by running a separate regression for each. In the first set of these models, we do not include a control for the omitted domains of our MDM. In the second set of analyses, we include an unweighted total of the omitted domains, raising the concern of two endogenous variables. In the first and second specifications, we therefore present an over-identified analysis and a just-identified analysis in the final case, using *Deaths* and *Spatial Lag Deaths* as our instruments.

5. Results

Our results link contemporaneous spatial differences in deprivation to the intensity of historical conflict. In Column 1 of Table 3, the results from the 'first stage' regressions[13] show that the number of historical deaths in a region is a positive and accurate predictor of current deprivation. Although these effects are not large in magnitude, they are strong enough to identify a causal relationship in the second stage of the regression, as the regression satistics show. The Cragg–Donald F Statistic identifies that the instrument set is strong,

[11] For example, Bratti and Mendola (2014) show that war-experienced mother's mental health affects children's education outcomes in Bosnia and Herzegovina.

[12] We compute the RE2SLS and EC2SLS models using the Stata command 'xtivereg3' (Schaffer, 2013).

[13] This 'first stage' output is obtained from simple OLS regressions of the MDM score on our instruments and other control variables. The full set of coefficients can be found in Table A.2.

Table 3
Base results.

	First stage MDM score[a]	Second stage average pass rate[a]				
	1 OLS	2 OLS	3 RE	4 2SLS	5 RE2SLS	6 EC2SLS
Deaths[3]	0.004**	–	–	–	–	–
	(0.002)					
Spatial lag deaths	0.095***	–	–	–	–	–
	(0.020)					
Multiple DM	–	− 4.089***	− 4.437***	− 6.983***	− 6.963***	− 6.013***
		(0.429)	(0.327)	(0.828)	(0.745)	(0.547)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937	5937	5937
R^2	0.451	0.197		0.165	0.924	0.925
R^2 overall			0.191			
χ^2			764			
F	14	23		21	2881	2899
p	0.000	0.000	0.000	0.000	0.000	0.000
CD Wald *F*				559	778	80
Sargan statistic				1	1	51
Sargan χ^2–*p*				0.301	0.466	0.000

[a] *Note*: Dependent variable. OLS = Ordinary Least Squares, RE = Random Effects, 2SLS = two-stage least squares, RE2SLS = Random Effects Two-Stage Least Squares, EC2SLS = Error Component Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses. Standard errors are clustered at Ward level in OLS, RE and 2SLS models and based on the GLS variance estimator in the RE2SLS and EC2SLS models.
*Significance level of 10%.
** Significance level of 5%.
*** Significance level of 1%.

passing the Stock–Yogo threshold at 5%, the traditional test of the first stage F-statistic exceeding 10 is, with a value of 14, passed, and the instruments are jointly significant at the 1% level. Moreover, in the models where we instrument one endogenous variable with the two instruments, the overidentification restrictions are valid, as indicated by the Sargan χ^2 *p*-value in the last row of Table 3.

The models presented in Columns 2 (OLS) and 3 (RE) indicate that schools in areas with a higher index of multiple deprivation are those with the poorest school performance. In these models, a one standard deviation increase in deprivation is associated with a reduction of pupils achieving the minimum pass at Key Stage II by 4.1–4.4 percentage points.

Results from the structural analyses are shown in Columns 4–6. These results suggest a bias towards zero in the previous estimates. This finding appears to contradict the traditional theory of "ability" as a source of bias, yet remains intuitive given the context of education funding in Northern Ireland. The UK government has engaged a number of policies that aim to provide active funding streams and top-quality teachers to schools in deprived areas, for example.[14] In addition to observable phenomena, such as significantly better pupil:teacher ratios in deprived regions, school funding and other policies like the Early Years Entitlement, are unobserved in our analysis but should benefit schools in deprived regions. We propose the direction and scale of this bias, therefore, suggest that school-level interventions are successful in mitigating the effects of growing up in deprived neighbourhoods but are not strong enough to eradicate the negative outcomes completely.

The results in Columns 4–6 show that a one standard deviation increase in deprivation is associated with a six-percentage point drop in pupils achieving at least a Level 4 pass. This means, *ceteris paribus*, that the average school in each performance decile would drop at least one decile should the level of deprivation in the area exogenously worsen by one standard deviation. This suggests a large economic effect from our findings. For example, a school ranked in the top 75% would just scrape into the top 60% of schools as a result of the direct effects of worsening deprivation. A school ranked in the bottom 20%, alternatively, could see its ranking improve by almost fifteen-percentage points, should the level of deprivation in its neighbourhood improve by one standard deviation.

Such a causal link between deprivation and schooling outcomes is relatively uncontroversial, suggesting that the results of our baseline analyses are well embedded in the literature discussed above. In the literature to date, and in our analysis this far, the transfer mechanisms in this relationship remain unclear and under-explained, however. In order to shed light on such mechanisms, we conduct two subsequent sets of analyses that look at the unique role of each of the domains of our multiple deprivation measure. In the first, we replace the NIMDM measure with each single deprivation domain defined within the wider measure. In the second, we repeat this analysis but include both each individual domain

[14] http://www.education.gov.uk/schools/adminand_nance/_nancialmanagement/schoolsrevenuefunding/archive/a0014385/school-funding-deprivation-indicator.

Table 4
Single deprivation measures w/o MDM: EC2SLS results.

	Deprivation measure						
	Income	Empl.	Education	Health	Liv. env.	Prox. serv.	Crime
Single indicator	− 4.673***	− 5.282***	− 3.817***	− 7.256***	− 4.332***	− 7.800***	− 9.133***
	(0.636)	(0.670)	(0.537)	(0.781)	(0.828)	(0.841)	(1.054)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937	5937	5937	5937
R^2 centered	0.322	0.317	0.325	0.310	0.303	0.319	0.257
F	2981	2783	2934	2591	2601	2913	1377
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000
CD Wald *F*	77	62	69	40	26	39	43

NOTE: *Deaths*[3] and *Spatial Lag Deaths* are the instruments used. EC2SLS = Error Component Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses.
*Significance level of 10%.
**Significance level of 5%. First stage results can be found in Table A.4.
*** Significance level of 1%.

Table 5
Single deprivation measure: EC2SLS results.

	Deprivation measure						
	Income	Empl.	Education	Health	Liv. env.	Prox. serv.	Crime
Single indicator	− 0.072	0.209	− 3.011***	0.192	− 0.021	0.116	− 3.783***
	(1.928)	(2.096)	(1.043)	(2.684)	(1.525)	(1.404)	(1.229)
MDM w/o indicator	− 6.342***	− 6.580***	− 3.981***	− 6.553***	− 6.544***	− 6.474***	− 4.051***
	(1.738)	(1.877)	(1.289)	(2.197)	(1.183)	(1.026)	(0.888)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937	5937	5937	5937
R^2 centered	0.305	0.305	0.306	0.304	0.305	0.304	0.304
F	2640	2638	2643	2638	2639	2634	2626
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: EC2SLS = Error Component Two-Stage Least Squares. Standard errors in parentheses.
*Significance level of 10%.
**Significance level of 5%.
*** Significance level of 1%.

and an unweighted average of the other six domains. The EC2SLS results from these analyses are presented in Tables 4 and 5. EC2SLS is our preferred methodology due to the treatment of instruments and increased efficiency of outputs, but these results are robust across a number of models as shown in the appendix (Table A.3).

Table 4 shows that each single domain predicts, as expected, a negative causal relationship running from high deprivation to reduced schooling outcomes. Here, a one standard deviation increase in each domain is associated with a 3.8–9.1 percentage point reduction in the average number of students attaining a Level 4 pass. Once the index of other omitted domains is included in the analyses (Table 5), neither financial nor employment deprivation are shown to have a significant impact on outcomes. The index of omitted domains is statistically significant and of proportionate scale in all specifications. Only the crime and education domains are shown to have significant effects over and above the composite of other MDM domains.

Thus, the results presented in Table 5 support our hypothesis that regional deprivation plays a significant and causal role in lowering school performance, although financial deprivation alone does not. Not only does this stand in direct contrast to suggestions pertaining to the role of financial deprivation, it also provides a rational for the mixed results to date – when the net effect of wider deprivation measures is not accounted for, financial deprivation appears to play a significant role. This suggests that suitable measures of financial deprivation act as valid proxies for the effects of deprivation but that one must be careful in drawing active policy advice from such findings. More concerning, however, is the suggestion that the success of analyses that look only at financial deprivation are entirely contingent on the relationship between actual regional deprivation and the proxy of financial deprivation used.

In all specifications, the main driver of the impact of deprivation is the composite indicator of other deprivation domains. This finding fits our main hypothesis that the effects of deprivation are transmitted not merely through financial poverty but a wide array of “social bads”, each of which can reduce the life opportunities of those exposed to them. In many cases, the transfer mechanisms are easily understandable, as they closely relate to the literature on social capital and its role in the formation of human capital. Children

growing up in areas that collectively suffer poorer environments or live in areas with a higher proportion of long-term illnesses are more likely to have a parent suffer such adversity. Fitting with the literature (e.g. Coleman, 1988; McLoyd, 1990; Misra et al., 2013; Bratti & Mendola, 2014), this may limit the psychological and physical capacity of parents to engage fully in their child's education.

Similarly, in the living environment domain, issues such as household overcrowding may affect the capacity of a child to have a suitable study space in the home, with associated impacts on, for example, time spent doing homework. The proximity of services domain includes the distance from an urban settlement with a population in excess of 10,000 people. Thus, individuals attending schools in regions with high values of this domain are likely to live in more rural settings. This, in turn, could affect school attendance due to quality of road access, distance to school premises and so on. The children attending schools in more financially deprived areas may lack access to important learning resources, such as the Internet, which may have logical ties to deflated achievements.

In addition to the impact of this wide array of sources of deprivation, the results presented in Table 5 indicate that the education and crime deprivation domains have unique effects not accounted for by the aggregated measure of other domains. *Prima facie*, a major endogeneity issue appears to arise by the inclusion of the education deprivation domain. In the 2005 NIMDM, however, this domain focuses only on adverse outcomes in the education indicators of those aged 14 and over but includes a number of metrics that have strong causal links to the educational outcomes of primary school children. It includes, for example, information on the skills of the adult population, as well as those of secondary school age. This domain, therefore, is likely to capture an intergenerational transfer effect. Children living in these areas are likely to have parents with lower skills and educational attainment, who, in turn, may be less likely to be able to engage actively and fully in all aspects of their child's education.

It seems more obvious why a high prevalence of crime should have such negative impacts on education outcome. Whilst the crime domain focuses on crimes most likely to be committed by individuals older than primary school age, issues like criminal damage, public disorder and arson, for example, increase the risk of damage to school property and associated lost school days. Alternatively, however, other correlates of such effects, which may include access to solvents or other substances, are likely to have impacts on school attendance and / or the quality of the school work pupils are capable of doing.

To verify our results, we conduct a series of robustness checks. We variously alter the form of our instrument, for example, to use different degrees of polynomial, deaths per 1000 inhabitants and three different dummy variable specifications, which compare violent and non-violent areas. These dummy variables take the value of one if violence is above a threshold of 1 (median), 4 (mean) or 9 (top decile) deaths and zero otherwise (see Table A.5). We also repeat our baseline analyses using only a single instrument – the third degree polynomial of deaths. These results (Table A.6) do not differ substantially from those in Table 3. Moreover, because of potential concerns about the use of the cross-sectional MDM measure used in a panel context, we estimate two-stage least square regressions on each cross-section in our sample and control for random effects at the neighbourhood level. These results can be found in Table A.7 and confirm the robustness of our findings obtained from our panel regressions. Further analyses show that our results are robust to different weightings of our MDM variable, the inclusion or exclusion of various single NIMDM domains in the composite measure, and the inclusion or exclusion of a number of control variables.[15]

6. Discussion: the Northern Irish context

Our results demonstrate the role of multiple deprivation in the determination of primary school performance, not least on the particular aspects of deprivation likely to result in harmful outcomes. Whilst we believe that these results are readily generalisable to a wide number of other cases, given the relative similarity between Northern Ireland and the United Kingdom as a whole, our findings must also be understood in the context of Northern Ireland. In this setting, it is possible that certain features of the country's recent past and present may play a role, both in influencing the results we find and in determining the importance of these findings.

Whilst we treat the violence of the conflict itself as exogenous, the intercommunity tensions, broadly between Northern Ireland's Protestants and Catholics, that led to the conflict remain. Such tensions frequently manifest themselves as so-called "street disturbances" – most commonly riots featuring one, and often, both communities. These disturbances are included as one of the indicators that make up the crime deprivation domain, suggesting a possible link between a relatively unique feature of Northern Ireland and our findings. Given the long-term political climate in the country, such disturbances occur significantly more frequently in Northern Ireland than in most other countries, particularly those in the developed world. In addition, they do so more frequently in more deprived regions of Northern Ireland than in less deprived ones. Thus, the role of crime, whilst important in this particular setting, may be overstated in a more general sense. This is not to say that the causal mechanisms themselves are unique to the Northern Irish case, however. Rather, a potential driver of the effects is likely to be a more common occurrence in this case than in a variety of others.

Similarly, the importance of policies designed to minimise the effects of deprivation on education and other outcomes and, by extension, possible persistence of regional poverty, may be singularly important in Northern Ireland. The persisting correlation between historical conflict intensity and contemporaneous deprivation indicates significant regional horizontal inequalities, which have frequently been cited as a cause of the violence (see Fitzduff & O'Hagan, 2009; Honaker, 2010). In Northern Ireland, therefore, counter-deprivation policies are likely to play a double role; firstly, as in other scenarios, they should help to increase social mobility and reduce the disadvantages associated with growing up in deprived regions. Second, they become an important instrument for the continuing peace process in the state. Although we find no differences in achievement between Catholic and

[15] Results can be obtained from the authors on request.

non-Catholic schools,[16] even perceptions of inequality along such lines could be a driver of future tensions. Given the nature of the links we find in our analysis, this in turn creates a further risk of educational adversities in the most deprived regions in Northern Ireland.

Such risks are also important in the context of Northern Ireland's education system, which sees children transfer from primary to post-primary school at age 11. Until 2008, Northern Ireland formally operated a transfer exam, which took place one year after Key Stage II exams are sat. Those "passing" this transfer exam typically move on to grammar schools, with those "failing" typically moving to other secondary schools. Despite the abolition of an official transfer exam in 2008, pupil selection at 11 remains prevalent, with grammar schools organising and operating their own selection exams. A long line of evidence suggests that the probability of enrolling in university, having attended grammar school, is significantly higher than for those attending secondary schools.[17] Similarly, 94% of pupils at grammar schools in Northern Ireland achieved the minimum expectation of 5 A*– C grades at GCSE-level in 2013/14 compared to only 37.7% at non-grammar schools.[18]

Given the short time between the Key Stage II exams, typically sat at age 10 and the transfer exam, sat at age 11, there is likely a high correlation between those who do well at Key Stage II and those who transfer to grammar schools. Given our results, it is easy to see the longer-term issues that could be associated with the negative educational impact of growing up in a deprived region in Northern Ireland, therefore. Not only is it a reflection of the direct effect on early-life achievement but also an encumbrance to social mobility and later life opportunity at all subsequent stages of education and life.

7. Conclusion

The link between human capital accumulation and future labour market outcomes is well-established in the literature. By extension, human capital accumulation itself can become an important anti-poverty device. Over and above the link between innate (parental) abilities and exposure to deprivation, however, a substantial body of literature has documented the negative role exposure can play. At both individual and neighbourhood levels, childhood exposure to deprivation is frequently shown to have a negative and significant impact on education outcomes (e.g. Weinhardt, 2010; Oreopoulos, 2003; Jacob, 2004; Durlauf, 1996; Gibbons, 2002; Bauer, Fertig, and Vorell, 2011).

Despite this body of work, finding a suitable measurement metric of deprivation remains difficult and the role of neighbourhood effects is not well understood, suggesting potential issues with the identification of causal relationships. In this paper, we overcome a number of these outstanding issues and show how exposure to deprivation can influence educational performance. Following Gibbons (2002), we employ a metric that focuses on several domains of deprivation. This metric, Northern Ireland's multiple deprivation indicator, includes seven domains of deprivation, each constructed from a series of sub-domains. This, in turn, allows us to measure the impact of a wide array of "social bads". Using a weighted average of these domains, we estimate the effect of deprivation on the proportion of primary school pupils attaining at least the minimum acceptable pass in the Key Stage II standardised tests. Using a unique feature of Northern Ireland's history – a long-term, low-intensity conflict – as an instrument for the endogenous deprivation score, we show a negative causal relationship between the level of neighbourhood deprivation and the outcomes of primary schools located in those neighbourhoods.

By disaggregating our deprivation measure into its constituent domains, our results show that the major component of the impact is accounted for by the full array of deprivations, rather than being driven by any single metric. In addition to the general impacts of the MDM, only education deprivation and crime deprivation have unique impacts. Particularly in Northern Ireland, the role of crime is important and is likely to be a supply-side effect resulting from, for example, lost school days. The impact of education deprivation, which looks only at the quality of education of post-14, is likely to be reflective of the findings of the intergenerational transfer literature (e.g. Currie & Moretti, 2003; Black et al., 2005; Kahanec & Yuksel, 2010), with parents with poorer educational outcomes more likely to have children who also suffer poorer outcomes.

Furthermore, we show that when we include the impact of other deprivation domains, it is impossible to separate out the effects of either financial or employment deprivation, raising concerns about the frequent use of such measures in the bulk of the literature. Whilst in many cases, including this article, financial deprivation is highly correlated with net regional deprivation, this may not always be the case. In turn, findings based only on financial deprivation measures may be unreliable and it follows that any attempts to improve social mobility and to reduce long-term persistence of deprivation may fail if the focus is only on individual or local economic factors. Rather, policies need to focus on improving a host of more general neighbourhood characteristics. More specifically, these findings also suggest a requirement for more specific focus on improving the capacity of parents with lower qualifications to ensure better outcomes for their own children and in reducing both the demand and supply-side effects of high local crime rates.

Such findings are particularly pertinent in the case of Northern Ireland as regional and horizontal inequalities have been prevalent since the creation of the state in 1921. Whilst the history of the country serves as an important feature in facilitating this study, it also suggests the unique importance of our findings to the region. Real and perceived inequalities between the Catholic and Protestant communities are frequently mentioned as causes of the conflict (e.g. Fitzduff & O'Hagan, 2009). Similarly, reductions in individual opportunity are cited as drivers of the intensity of violence (e.g. Honaker, 2010). Persisting regional inequalities in Northern Ireland, therefore, are not only a barrier to social mobility, as they are in more general cases, but also a potential obstacle for the continuing peace process.

[16] Descriptive statistics by denomination are available upon request.

[17] See e.g. the Sutton Trust: http://www.suttontrust.com/public/documents/sutton-trust-he-destination-report-final.pdf.

[18] See http://www.deni.gov.uk/year_12_and_year_14__examination_performance_at_post-primary_schools_in_northern_ireland_2012-13__revised_.pdf (page 5).

The endurance of detrimental deprivation effects suggest that governments, hitherto, have failed to address issues that, in the context of Northern Ireland, appear to have two mutually reinforcing effects. First, it reduces the expected life outcomes of those in deprived regions, as we would expect to find elsewhere. Second, it facilitates the ongoing, low-level street disturbances and other manifestations of violence that, in turn, are a contributing factor to regional deprivation and potentially a major cause of the reduced attainment of those exposed to it. This suggests an urgent need, particularly in the Northern Ireland case, to address the core causes of deprivation through a set of policy interventions that target the full array of deprivation domains, rather than those that only aim to increase individual or regional income.

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Appendix

Table A.1
Correlation between deprivation domains.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Multiple DM	1.000							
(2) Income DM	0.819	1.000						
	(0.000)							
(3) Employment DM	0.825	0.931	1.000					
	(0.000)	(0.000)						
(4) Health DM	0.765	0.712	0.728	1.000				
	(0.000)	(0.000)	(0.000)					
(5) Education DM	0.949	0.792	0.802	0.649	1.000			
	(0.000)	(0.000)	(0.000)	(0.000)				
(6) Living Env. DM	0.725	0.664	0.651	0.562	0.651	1.000		
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)			
(7) Crime DM	0.627	0.413	0.415	0.605	0.389	0.252	1.000	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		
(8) Services DM	0.492	0.316	0.348	0.550	0.318	0.136	0.748	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
N				5937				

Table A.2
Base results – all coefficients.

	First stage MDM score[a]	Second stage Average pass rate[a]				
	1 OLS	2 OLS	3 RE	4 2SLS	5 RE2SLS	6 EC2SLS
Deaths[3]	0.004**	–	–	–	–	–
	(0.002)					
Spatial lag deaths	0.095***	–	–	–	–	–
	(0.020)					
Multiple DM	–	−4.089***	−4.437***	−6.983***	−6.963***	−6.013***
		(0.429)	(0.327)	(0.828)	(0.745)	(0.547)
Total enrolment	0.001	0.020***	0.014***	0.022***	0.016***	0.015***
	(0.000)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)
Number of pupils	0.001	−0.277***	−0.227***	−0.260***	−0.218***	−0.220***
	(0.004)	(0.046)	(0.033)	(0.046)	(0.034)	(0.034)
Number of pupils sqrd.	−0.000	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pupil–teacher ratio	−0.041	−1.190*	−0.393	−1.352*	−0.454	−0.236
	(0.075)	(0.675)	(0.442)	(0.722)	(0.445)	(0.466)
Pupil-teacher ratio sqrd.	−0.000	0.042**	0.014	0.041**	0.014	0.009
	(0.002)	(0.017)	(0.011)	(0.018)	(0.011)	(0.012)
Gaelic exams	0.043	5.077***	3.028*	5.695**	3.379**	3.400**
	(0.229)	(1.962)	(1.548)	(2.278)	(1.576)	(1.575)
Catholic school	−0.014	1.073*	1.045	1.030	1.065	1.053
	(0.070)	(0.615)	(0.675)	(0.670)	(0.704)	(0.710)

(*continued on next page*)

Table A.2 (*continued*)

	First stage MDM score[a]	Second stage Average pass rate[a]				
	1 OLS	2 OLS	3 RE	4 2SLS	5 RE2SLS	6 EC2SLS
Catholic inhabitants (%)	0.000	−0.005	0.002	−0.003	0.010	0.005
	(0.002)	(0.013)	(0.012)	(0.015)	(0.013)	(0.013)
All persons 0–15	0.002***	−0.001	−0.002	0.005	0.000	−0.001
	(0.000)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
All persons 16–39	0.000	−0.001	−0.001	0.000	0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
All persons 40–59/64	−0.004***	0.001	0.002	−0.011**	−0.004	−0.002
	(0.000)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)
All persons 60/65+	0.002**	−0.003	−0.005**	0.005	0.001	−0.001
	(0.001)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)
Bombing incidents	−0.004*	0.044*	0.026	0.015	0.021	0.023
	(0.002)	(0.025)	(0.020)	(0.026)	(0.020)	(0.020)
Shooting incidents	0.010**	−0.040	−0.005	0.029	0.009	0.006
	(0.004)	(0.028)	(0.023)	(0.029)	(0.023)	(0.023)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937	5937	5937
R^2	0.451	0.197		0.165	0.924	0.925
R^2 overall			0.191			
χ^2			764			
F	14	23		21	2881	2899
p	0.000	0.000	0.000	0.000	0.000	0.000
CD Wald *F*				559	778	80
Sargan statistic				1	1	51
Sargan χ^2–*p*				0.301	0.466	0.000

[a] NOTE: Dependent variable. OLS = Ordinary Least Squares, RE = Random Effects, 2SLS = Two-Stage Least Squares, RE2SLS = Random Effects Two-Stage Least Squares, EC2SLS = Error Component Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses. Standard errors are clustered at Ward level in OLS, RE and 2SLS models and based on the GLS variance estimator in the RE2SLS and EC2SLS models.

* denotes significance level of 10%.

** denotes significance level of 5%.

*** denotes significance level of 1%.

Table A.3
Single deprivation measures

	Dependent variable: average pass rate			
	OLS 1	RE 2	2SLS 3	RE2SLS 4
Income DM	1.114*	0.328	0.412	−0.166
	(0.649)	(0.594)	(2.269)	(2.718)
MDM w/o income	−2.153***	−2.601***	−5.869**	−5.614**
	(0.589)	(0.534)	(2.410)	(2.335)
Controls	Yes	Yes	Yes	Yes
R-squared	0.253	0.245	0.208	0.320
CD Wald-*F*			175	172
Employment DM	0.639	−0.010	0.564	−0.207
	(0.573)	(0.529)	(2.989)	(3.669)
MDM w/o empl.	−1.880***	−2.375***	−5.974**	−5.582*
	(0.594)	(0.514)	(2.847)	(3.029)
Controls	Yes	Yes	Yes	Yes
R-squared	0.252	0.245	0.208	0.320
CD Wald-*F*			82	74
Education DM	−1.041**	−1.626***	−1.885	−2.542
	(0.438)	(0.387)	(2.225)	(2.628)
MDM w/o educ.	−0.632	−1.054***	−4.320	−3.786
	(0.443)	(0.409)	(3.050)	(2.902)

(*continued on next page*)

Table A.3 (*continued*)

	Dependent variable: average pass rate			
	OLS 1	RE 2	2SLS 3	RE2SLS 4
Controls	Yes	Yes	Yes	Yes
R-squared	0.252	0.245	0.204	0.320
CD Wald-*F*			50	60
Health DM	−0.327	−0.576	0.780	−0.483
	(0.447)	(0.449)	(4.675)	(6.879)
MDM w/o health	−1.237**	−1.977***	−6.202	−5.339
	(0.499)	(0.467)	(4.363)	(5.965)
Controls	Yes	Yes	Yes	Yes
R-squared	0.252	0.245	0.203	0.320
CD Wald-*F*			23	15
Liv. env. DM	−0.347	−0.381	−2.422	−2.422
	(0.392)	(0.347)	(3.396)	(4.084)
MDM w/o liv. env.	−1.265**	−2.219***	−3.901	−4.710
	(0.495)	(0.393)	(2.658)	(3.036)
Controls	Yes	Yes	Yes	Yes
R-squared	0.252	0.245	0.206	0.321
CD Wald-*F*			27	24
Prox. serv. DM	−0.485	−0.668	−1.720	0.243
	(0.438)	(0.437)	(7.524)	(7.658)
MDM w/o prox. serv.	−1.265***	−2.088***	−4.017	−5.991
	(0.469)	(0.394)	(6.692)	(6.899)
Controls	Yes	Yes	Yes	Yes
R-squared	0.252	0.245	0.222	0.321
CD Wald-*F*			5	8
Crime DM	−0.522	−1.005***	−4.572	−2.248
	(0.369)	(0.357)	(10.449)	(9.034)
MDM w/o crime	−1.184***	−1.797***	−2.782	−4.469
	(0.384)	(0.348)	(5.819)	(6.642)
Controls	Yes	Yes	Yes	Yes
R-squared	0.252	0.245	0.186	0.309
CD Wald-*F*			3	5

NOTE: OLS = Ordinary Least Squares, RE = Random Effects, 2SLS = Two-Stage Least Squares, RE2SLS = Random Effects Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses. Standard errors are clustered at Ward level in OLS, RE and 2SLS models and based on the the GLS variance estimator in the RE2SLS.

* denotes significance level of 10%.

** denotes significance level of 5%.

*** denotes significance level of 1%.

Table A.4
"First stage".

	Deprivation measures								
	Multiple	Multiple	Income	Empl.	education	Health	Liv. env.	Prox. serv.	Crime
Deaths[3]	0.005***	0.004**	0.006**	0.005**	0.005***	0.004***	0.000	0.001**	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Spatial lag deaths	–	0.095***	0.067***	0.068***	0.087***	0.070***	0.075***	0.079***	0.061***
		(0.020)	(0.016)	(0.018)	(0.022)	(0.014)	(0.017)	(0.011)	(0.017)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937	5937	5937	5937	5937	5937
R^2	0.370	0.451	0.537	0.458	0.394	0.406	0.349	0.595	0.418
F	12	14	20	15	11	17	12	38	15
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: Ordinary Least Squares regressons with clustered standard errors (Ward level). Standard errors in parentheses.

*Significance level of 10%.

** Significance level of 5%.

*** Significance level of 1%.

Table A.5
Robustness checks: varying instrument.

	EC2SLS			
	D/pop	D ≥ 1	D ≥ 5	D ≥ 10
Multiple DM	−5.513***	−5.577***	−5.600***	−5.632***
	(0.547)	(0.814)	(0.636)	(0.546)
Controls	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937
F	3181	1235	2273	3233
p	0.000	0.000	0.000	0.000
CD Wald *F*	71	81	72	70

Note: EC2SLS = Error Component Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses.
*Significance level of 10%.
**Significance level of 5%.
*** Significance level of 1%.

Table A.6
Robustness checks: one instrument.

	First stage MDM score[a]	Second stage average pass rate[a]				
	1 OLS	2 OLS	3 RE	4 2SLS	5 RE2SLS	6 EC2SLS
Deaths[3]	0.005***	–	–	–	–	–
	(0.002)					
Multiple DM	–	−4.089***	−4.437***	−5.648***	−5.949***	−5.771***
		(0.429)	(0.327)	(1.223)	(1.537)	(0.562)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
N	5937	5937	5937	5937	5937	5937
R^2	0.370	0.197		0.188	0.928	0.928
R^2 centered				0.188	0.313	0.313
R^2 overall			0.191			
χ^2			764			
F	12	23		23	3052	3058
p	0.000	0.000	0.000	.	0.000	0.000
CD Wald *F*				212	288	69

[a] *Note*: Dependent variable. OLS = Ordinary Least Squares, RE = Random Effects, 2SLS = Two-Stage Least Squares, RE2SLS = Random Effects Two-Stage Least Squares, EC2SLS = Error Component Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses. Standard errors are clustered at Ward level in OLS, RE and 2SLS models and based on the GLS variance estimator in the RE2SLS and EC2SLS models.
*Significance level of 10%.
**Significance level of 5%.
*** Significance level of 1%.

Table A.7
Robustness checks: RE2SLS cross section regressions.

	Second stage average pass rate[a]									
	2000/01	2001/02	2002/03	2004/05	2005/06	2006/07	2007/08	2008/09	2009/10	2010/11
Multiple DM	−5.489***	−9.366***	−5.981***	−5.552***	−10.144***	−8.781***	−7.636***	−6.956***	−8.788***	−6.202***
	(1.624)	(2.126)	(2.048)	(2.084)	(2.955)	(2.015)	(1.778)	(1.595)	(1.422)	(1.289)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	588	536	588	592	593	593	585	601	601	660
R^2										
F										
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
CD Wald *F*										

[a] *Note*: Dependent variable. Deaths and Spatial Lag Deaths are used as instruments. RE2SLS = Two-Stage Least Squares with random effects at ward level and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses. Standard errors are clustered at Ward level.
*Significance level of 10%.
**Significance level of 5%.
*** Significance level of 1%.

Table A.8
Robustness check: subsample 2004–2010.

	First stage MDM score[a]	Second stage average pass rate[a]				
	1 OLS	2 OLS	3 RE	4 2SLS	5 RE2SLS	6 EC2SLS
Deaths[3]	0.004**	–	–	–	–	–
	(0.002)					
Spatial Lag Deaths	0.087***	–	–	–	–	–
	(0.019)					
Multiple DM	–	−3.841***	−4.145***	−7.171***	−6.873***	−7.362***
		(0.408)	(0.356)	(0.933)	(0.835)	(2.042)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
N	4225	4225	4225	4225	4225	4225
R^2	0.466	0.177		0.133	0.941	0.832
R^2 overall			0.172			
χ^2			435			
F	14	19		16	3071	1068
p	0.000	0.000	0.000	.	0.000	0.000
CD Wald *F*				370	515	0
Sargan statistic				1	0	8
Sargan χ^2–*p*				0.425	0.664	0.368

[a] *Note*: Dependent variable. OLS = Ordinary Least Squares, RE = Random Effects, 2SLS = Two-Stage Least Squares, RE2SLS = Random Effects Two-Stage Least Squares, EC2SLS = Error Component Two-Stage Least Squares and CD = Cragg–Donald Wald *F* statistic for weak instrument identification test. Standard errors in parentheses. Standard errors are clustered at Ward level in OLS, RE and 2SLS models and based on the the GLS variance estimator in the RE2SLS and EC2SLS models.
*Significance level of 10%.
** Significance level of 5%.
*** Significance level of 1%.

Table A.9
Northern Ireland multiple deprivation measure 2005.

Sub-domain	Indicators
Income deprivation	Income support households; job seeker's allowance households; working families' tax credit households; disabled person's tax credit households
Employment deprivation	Unemployment claimant count of women aged 18–59 and men aged 18–64 averaged over 4 quarters; Incapacity Benefit claimants women aged 18–59 and men aged 18–64; severe disablement allowance claimants women aged 18–59 and men aged 18–64; participants in new deal for young people (18–24 years) who are not included in the claimant count; participants in new deal for 25+ who are not included in the claimant count; invalid care Allowance claimants women aged 18–59 and men aged 18–64
Education deprivation	General Certificate of Secondary Education (GCSE)/General National Vocational Qualification (GNVQ) points score; key stage 3 data; proportions of those leaving school aged 16 and not entering further education; absenteeism at secondary level (all absences); proportions of 17–20 year olds who have not successfully applied for higher education; proportions of years 11 and 12 pupils not in a grammar school; proportions of post-primary pupils with special educational needs in mainstream schools
Health deprivation	Years of potential life lost; comparative illness and disability ratio; a combined measure of two indicators (i) individuals suffering from mood or anxiety disorders, based on prescribing and (ii) suicides; people registered as having cancer (excluding non-melanoma skin cancers)
Living environment deprivation	Super Output Area (SOA) level housing stress; houses without central heating; household overcrowding; Local Government District (LGD) level rate of acceptances under the homelessness provisions of the housing (Northern Ireland) order 1988 and the housing (Northern Ireland) order 2003, assigned to the constituent SOAs; SOA level local area problem score
Proximity to services deprivation	Road distance to a family doctory premises; road distance to an accident and emergency hospital; road distance to a dentist; road distance to an optician; road distance to a pharmacist; road distance to a job centre or jobs and benefit office; road distance to a Post Office; road distance to a food shop; road distance to the centre of a settlement of 10,000 or more people
Crime and disorder deprivation	Violence, robbery and public order; burglary; vehicle theft; criminal damage; malicious and deliberate primary fires; disturbances

NOTE: A more detailed description can be found at NISRA (2005).

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