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Urban regeneration projects and crime: evidence from Glasgow

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Abstract

This study investigates the effects of urban regeneration on crime, leveraging recent large-scale regeneration projects—called Transformational Regeneration Areas (TRAs)—in Glasgow, Scotland. We employ a difference-in-differences approach that makes use of variation in both the timing of TRA implementation, and in proximity to these areas to measure exposure to urban regeneration projects. We find a large and significant reduction in crime within 400 m of TRAs but this effect fades as we move further away. Simultaneously, we find no evidence of city-wide reductions in crime after urban regeneration.

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JEL classifications: I38, R20, K42
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1. Introduction

Large public housing estates became the dominant form of social housing in many of the UK's largest cities during the decades following WW2. These housing estates were built to tackle issues around affordable housing and growing populations, but over time developed a reputation as areas of deprivation and crime.[1]

In recent years, large-scale urban regeneration policies in multiple UK cities have attracted considerable attention.[2] These policies usually entail the replacement of decaying residential estates with mixed-income housing along with the redevelopment of surrounding areas. Advocates of urban regeneration projects often argue that deprivation becoming less concentrated as a result of such policies will also lead to less crime (Newman, 1996;

1 A recent summary of evidence reports that between 1990s and 2014 social renters in the UK experienced at least twice the national average of household crimes (Osborn and Tseloni, 1998; Tseloni et al., 2004; Tseloni, 2006; Hunter and Tseloni, 2016). In addition, relative to home owners, social renters are 40% more likely to fall victims of personal crimes with close distance to their homes (Tseloni and Pease, 2015). See also research and policy recommendations from the Quantitative and Spatial Criminology Research Group at Nottingham Trent University: https://gtr.ukri.org/publication/overview?outcomeid=5aa98d6e3ca5b3.97637440&projectref=ES/K003771/1

2 See for example, 'The real cost of regeneration', *The Guardian*, 21 July 2017.

Turner et al., 2007).[3] This line of reasoning is partly based on the theory of 'defensible spaces' outlined in Newman (1972), which states that large public housing estates provide a setting where disincentives to criminal activity are weak and criminals (and gangs) are particularly difficult to police—consequently, the elimination of these spaces should reduce crime on aggregate. Crime could also decrease if urban regeneration projects lead to better access to employment opportunities, making criminal activity less attractive (Aliprantis and Hartley, 2015). Critics, on the other hand, point out that regeneration projects simply lead to a relocation of crime to other areas, while the gentrification of neighbourhoods generates residential displacement which exacts a heavy psychological toll on former residents, many of whom are from a low-income background (Atkinson, 2000).[4] In this article, we examine the effects of urban regeneration projects on both local and city-wide crime levels using the city of Glasgow as a case study.

In general, public housing is a more prevalent form of tenure in the UK when compared with most other OECD countries (OECD, 2020). Even within this context, the city of Glasgow, Scotland's largest and most populous metropolitan area, has a particularly strong tradition in public housing. Under the leadership of the Glasgow City Council (GCC) and its predecessors, the city's history includes repeated large-scale state interventions aimed at increasing the supply of public housing, which by the end of the 1960s accounted for almost 40% of the city's housing stock (GHS, 2022). In the first round of large-scale demolitions in the 1960s and 1970s, a considerable share of the city's population was transferred from traditional tenement buildings—often overcrowded and in poor condition—to housing estates and high rises. These estates, often built near the city limits, tended to have poor access to amenities, which, paired with neglect, has resulted in deprivation and a concurrent increase in crime rates, drug abuse and health issues (Garnham, 2018; Davies, 2019). To tackle these issues, in 2009, the GCC, together with the Glasgow Housing Association (GHA), started implementing Transformational Regeneration Areas (TRAs). These projects involved the demolition and replacement of existing estates with new mixed tenure housing, and endowing these areas with green spaces and other amenities.

To examine how TRAs affected crime in local areas, and in areas nearby, we use georeferenced locations of the eight TRAs implemented in the last decade, alongside administrative data on neighbourhood-level crime numbers from 2007 to 2020. Since TRAs were implemented at different dates, we make use of a staggered difference-in-differences (DiD) approach with spatial spillovers. This method leverages two sources of variation in exposure to TRAs. First, by exploiting the staggered implementation of the regeneration projects we compare crime numbers across TRAs before and after their implementation. We show that the timing of the implementation was not driven by diverging trends in crime across areas. Second, to assess how crime effects change with proximity to TRAs, we implement a ring approach as common in the literature (Sandler, 2017; Blanco and Neri, 2021). This approach relies on the assumption that proximity to TRAs determines treatment intensity,

3 There is evidence showing that when poverty is spatially concentrated this could lead to a breakdown of informal social controls, leading to more crime (see Sampson and Raudenbush, 1999; Morenoff et al., 2001). Neighbourhood peer effects within social housing developments, whereby young residents are exposed to local criminals, can also lead to an increase in crime (Rotger and Galster, 2019).

4 See also 'Glasgow homes under the jackhammer—A photo essay', *The Guardian*, 18 February 2022 or 'Regeneration—or pushing out the poor? Labour divides in bitter housing battle'. *The Guardian*, 29 October 2017.

and compares crime numbers within short distances (inner rings) of a TRA to those in surrounding areas (outer rings). We estimate these models using a standard two-way fixed effects (TWFEs) method, but also complement them by a two-stage DiD DiD2S (Gardner, 2022) approach that accounts for treatment effect heterogeneity and spatial spillovers (Butts, 2023). Finally, following the time-series approach first outlined in Bruhn (2018) we also examine whether the TRAs led to changes in aggregate (city-wide) crime levels.

Our analysis presents three main results. First, we find that the implementation of TRAs is followed by a large (up to 36%) reduction in crime numbers within 400 m of TRA sites. In DiD2S specifications that account for potential bias from treatment effect heterogeneity, these effects are considerably smaller (a 19% reduction in our main specification) but still point in the same direction. We argue that these findings point to local effects on crime whereby TRAs eliminate the convenient physical setting created by public housing estates where crime can take place (Newman, 1972; Aliprantis and Hartley, 2015). This argument is supported by the large reduction in crimes such as theft and drug-related crime, which typically tended to occur in the environments created by large public housing estates. In robustness checks, we exclude areas that TRAs are nested in to alleviate concerns about our baseline effects being purely mechanical ones, and report similar findings, but only within close vicinity (400 m) of TRAs. Our baseline results are also robust to a battery of sensitivity checks, including changing the radii by which we define our distance rings; changing the way we calculate distance from TRAs; changing the outcome and model specifications and to delaying the timing of TRA implementations. Second, we report that TRAs are associated with lower deprivation across multiple dimensions in affected areas, but these effects are also mostly confined to the immediate location of the TRA site. These results are likely explained by the mechanical effect of the replacement of low-income housing with mixed-income units, and the resulting changes in neighbourhood composition. Improved employment and health outcomes in neighbourhoods can nonetheless act as a channel for further (local) crime reductions by making crime less attractive (Aliprantis and Hartley, 2015). Finally, we find no evidence of aggregate-level reductions in crime levels in response to TRA projects. Our aggregate-level effects are a bit smaller than what the size of the local reductions in crime would suggest at the city-level, implying that it might be a small positive crime displacement effect that leaves city-wide crime levels unchanged after TRA implementations.

The main contribution of our article is to the small literature on the effects of urban regeneration projects on crime. Our work builds on a small body of evidence from the USA that looks at the effects of public housing demolitions on crime, which mostly finds evidence of crime reductions at the local and aggregate level (Aliprantis and Hartley, 2015; Sandler, 2017). Contrasting findings by Bruhn (2018) suggest negative local effects but a city-wide increase in crime. Our article contributes to this literature by examining the local crime effects of urban regeneration projects using a version of the standard spatial DiD approach (Aliprantis and Hartley, 2015; Sandler, 2017) that accounts for potential treatment effect heterogeneity and spatial spillovers (Butts, 2023). We complement this approach to also examine city-wide crime changes in response to urban regeneration projects by following the time-series approach outlined in Bruhn (2018). Our results are consistent with the literature in that we also report negative crime effects in the vicinity of urban regeneration sites, but are novel in that we find no evidence of an aggregate-level crime effect.

Our study is also among the first ones (to our knowledge) to analyse the effects of urban regeneration projects on crime in a UK context, where, despite the country's strong tradition in public housing and related spatial concentration of crime, there is a relative lack of evidence on this topic. The only study looking at the link between urban regeneration projects and crime in the UK that we are aware of is a current working article by Blanco and Neri (2021). They find a negative effect from such projects in London, and also show positive effects on house prices and desirable neighbourhood amenities. We add to this literature by providing evidence on the crime effects of urban regeneration through the case study of Glasgow, where these effects are likely to be particularly pertinent given the city's peculiar history of public housing and high crime numbers concentrated near housing estates. We also contribute to the wider literature on the effects of urban regeneration, which examines the effects of these projects on a variety of outcomes such as house prices (Brown, 2009; Zielenbach and Voith, 2010; Blanco and Neri, 2021); neighbourhood socio-economic composition (Tach and Emory, 2017); employment (Gibbons et al., 2021; Zhang et al., 2021) and student achievement (Neri, 2020).

The rest of the article is structured as follows. Section 2 provides an overview of the historical and policy background; Section 3 describes the data; Section 4 outlines our empirical strategy and presents the results. Section 5 concludes.

2. Background

Scotland has a strong tradition of public housing. After WW1, the 1919 Housing Act paved the way for a shift from private landlords to council housing as the dominant form of tenure. By the end of the 1970s, public housing accounted for almost three-quarters of the entire Scottish housing stock, compared with hardly one-third in England (Robertson and Serpa, 2014).

Even within Scotland, the city of Glasgow provides a unique case study in public housing. In Glasgow, the GCC become the main builder of new housing after WW2, and was also in charge of large-scale urban planning policies that would shape the city for decades (Davies, 2019). Glasgow's post-industrial background required state intervention to accommodate the rising demand in housing of a fast-growing, predominantly working-class population. By the end of the 1960s, with a stock of about 126,500 dwellings, GCC owned (and managed) nearly 40% of the entire housing stock of the city (GHS, 2022). By the 1970s, only Russian cities had greater state involvement in the housing market than Glasgow (Davies, 2019). The city's ambitious programme also involved the development of large public housing estates, among them many high-rise buildings, that would house tens of thousands of residents across the city. Often times the people who occupied flats in these housing estates were rehoused from Victorian tenement buildings—tenements themselves were built to cope with Glasgow's extreme population growth in the 18th and 19th centuries[5]—fracturing the local community ties established in existing neighbourhoods (Davies, 2019). Housing estates often had poor access to amenities and, due to a lack of ongoing investment, their surrounding areas experienced high rates of deprivation,

5 Traditional Glaswegian tenements are a type of sandstone building, usually three or four stories high, with most facilities shared by tenants. Tenement housing was incredibly dense, and often times entire families occupied a single room in a tenement building. Despite overcrowding, tenements were important centres of social life, as tenants formed various clubs and societies and provided each other with community support (Davies, 2019).

with overcrowded housing, gang culture and growing crime, drug abuse, and low life expectancy among residents (Garnham, 2018).

Starting in the early 2000s, large-scale housing regeneration projects were implemented in Glasgow to address issues with its crumbling housing stock. In September 2003, the responsibility to manage Glasgow's housing stock transferred from the Scottish Government to the GCC, who in the same year delegated the management to the GHA (Zhang et al., 2021).[6] Since 2005, the GCC has been working in partnership with the GHA and the Scottish Government to establish a new approach to the regeneration of eight key areas in the city, known as TRAs. In 2009, the Scottish Parliament gave the go ahead for the programme to be initiated. The TRA programme aims to provide new sustainable mixed tenure communities through the provision of new housing, community facilities and local amenities, green space and commercial units.[7] The eight TRAs were selected at the same time (before any TRAs were implemented) by the GCC, GHA and the Scottish Government as areas 'that require major restructuring in order to create sustainable mixed tenure communities' (Glasgow City Council, 2011).[8] The timing and phases of TRA completion seem to be largely dependent on the way partnering private and social sector organizations deliver each project locally and on the specifics of building demolition and construction (GoWell, 2007). Across the TRA programme, approximately 600 homes for social housing are planned along with an estimated 6500 homes for mid-market rent. TRA implementation dates for the eight areas are summarized in Supplementary Appendix Table A.1. We consider each TRA to be 'active' when the first phase of the new building construction was completed. The locations of each TRA within Glasgow City are shown in Supplementary Appendix Figure A.1.

According to the GoWell Research and Learning Programme, a qualitative study that looks at the effects of housing regeneration on the well-being of local residents, TRAs mostly involved demolishing existing mass housing estates in the affected areas, and replacing them with mixed-income and social housing, leaving residents with the option to stay in the area in newly built homes (Kearns and Lawson, 2017). A before and after comparison of the housing built at the £250 million Sighthill TRA project is a good example of how TRAs led to the redevelopment of affected urban areas (see Figure 1). The Sighthill estate has been for decades one of the most deprived areas of Glasgow, with poor living conditions, high rates of unemployment, drug abuse and crime.[9] At Sighthill—and in fact in the case of most TRAs—large high-rise buildings in poor condition were demolished to give place to smaller, more densely packed housing units, and the surrounding residential areas were redeveloped.

6 GHA is the largest provider of social housing in Scotland with about 40,000 affordable properties throughout the city. Since its creation in 2003, it has invested more than £1.5 billion in improving current stock and building more than 2000 new properties (Black and Roy, 2019). In addition, GHA provides a wider range of support activities for the community, for example, financial advice, apprenticeships (Black and Roy, 2019).

7 Information on TRAs can be found on this website: https://www.gha.org.uk/about-us/regeneration/new-build-homes-transformational-regeneration-areas-tras. Additional information is available on the Glasgow City Council website: https://www.glasgow.gov.uk/article/19842/Transforming-Communities-Partnership.

8 Aside from this it is unclear from the available documentation what criteria were used to select TRAs. Generally, the regeneration strategy outlined by the GCC focuses on improving housing quality and access to housing and amenities, and makes no mention of crime either as a factor or as an outcome (Glasgow City Council, 2011).

9 Disappearing Glasgow, a photo documentary project by Chris Leslie, documents the demolition of public housing estates, and among them the Sighthill housing estate, while providing a qualitative account of the experience of living at these housing estates before the regeneration projects. See https://www.disappearing-glasgow.com/portfolio/sighthill-3/.

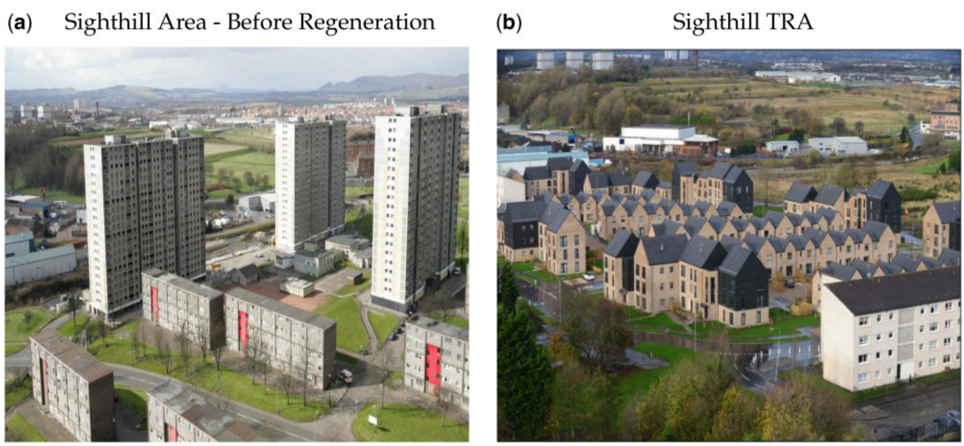

Figure 1 TRA—before and after regeneration.
Notes: The Sighthill area before and after (as of December 2015) TRA implementation. High-rise buildings are replaced by modern terraced estates for mixed-income tenure.

The report by Kearns and Lawson (2017) also states that despite the fact that old tenants of demolished housing estates had the option to remain, most of them sought housing in nearby areas instead of staying in newly built housing. This might be because the GHA's rehousing process—which was governed by GHA clearance and allocation policies along with legal requirements—generally required tenants to be moved into accommodation that was 'better than the ones they were leaving' (GoWell, 2011).[10] Those electing to stay were often moved around the area before being able to move into newly built social housing, which was more likely to be offered to tenants who lived at the housing estates for longer (GoWell, 2011; Kearns and Lawson, 2017). In many cases, this required longer waiting times than moving to the alternative accommodation offered, which could explain why many tenants opted to move away from the housing estate areas (Kearns and Lawson, 2017). The GHA also offered financial help of up to £2750 in home loss and disturbance payments to residents of demolished buildings (GHA, 2005a, 2005b).[11] According to survey evidence on the residents of the first few TRAs, those who moved out tended to stay on average 1.7 km away from their former home (with 80% staying within 2 km), and mostly moved to low-rise flats and to higher quality dwellings (GoWell, 2011).[12]

3. Data

We use crime data on the universe of recorded crimes in Scotland provided by Police Scotland through a Freedom of Information (FOI) request.[13] Our data consist of monthly

10 Specifically, the GHA's main aims were to 'provide alternative accommodation that is better than that which people occupied previously; to offer people choice; to minimise disruption to individuals and communities; and to avoid the effects of remaining for a long time in a condemned property' (GHA, 2005a, 2005b; GoWell, 2011).
11 We have not been able to find data on how many former residents moved away from TRAs and where they moved.
12 It is worth noting that these results are not based on a representative sample of housing estate residents.
13 FOI 22-1505.

Data Zone level crime counts for the time period 2007–2020. Data Zones are the second lowest level of territorial designation in Scotland (similar to US census blocks) and are composed of aggregates of the country's 46,351 Output Areas. They are designed to each include roughly between 500 and 1000 residents and to constitute socio-economically and geographically homogeneous areas.[14] There are 6976 Data Zones in Scotland, 746 of which are located within Glasgow City.

To avoid low cell sizes (few or zero crimes) in many Data Zones, we aggregate the crime data to the annual level.[15] We then construct a balanced panel of Data Zones over the 14-year period between 2007 and 2020. The data consist of all subcategories of crimes and offences.[16] We calculate the overall, Data Zone level, crime/offence numbers by aggregating all instances that fall within each category, based on Scottish Government classifications.[17] We also aggregate crime data across the five major crime subcategories used in Scotland: violent crimes (non-sexual), sexual crimes, crimes of dishonesty, fire-raising and vandalism and other crimes. These subcategories are described in more detail in Supplementary Appendix Table A.2. As base period controls, we use the Data Zone level scores for the different components (income, housing, access to services, health and employment) of the Scottish Index of Multiple Deprivation (SIMD) from 2006. The SIMD ranks all small areas in Scotland in terms of relative deprivation.[18]

We also use data on deprivation from SIMD waves 2006, 2009, 2012, 2016 and 2020 as additional outcome measures. We make use of the following outcomes: the income deprivation rate which is the percentage of population in receipt of the main forms of means-tested benefits; the employment deprivation rate which is the percentage of working age population who are not in employment and receive employment or disability-related benefits; the standardized mortality ratio; the standardized ratio of drug-related hospital stays and the overall SIMD rank deciles, ranging from 1 to 10, where 1 is the most deprived. Official definitions of SIMD components are summarized in Supplementary Appendix Table A.3.

Information on TRAs and their location coordinates is collected from the GHA. We use QGIS to calculate distance rings around each TRA using a set of (200, 400, 600, 800 and 1000 m) distance radii. If a Data Zone's area centroid falls within a specific distance radius, then the Data Zone is indicated to be part of the corresponding ring. For our main analytical sample, we limit our data to those Data Zones within 1 km of a TRA. This is so that our ring approach (see below) only relies on data for treated areas (the four rings within 800 m) and the control areas (the outer ring between 800 and 1000 m). When restricted this way our sample contains 119 Data Zones, and on average there are 60.3 crimes and 54.4 offences committed in each Data Zone each year. The average Data Zone in our sample stretches across 0.17 km^2 and, as per 2011, contains 763 residents. Summary statistics for the full sample are provided in Supplementary Appendix Table A.4, while Table 1 provides summary statistics for each distance ring for the pre-treatment

14 These are the equivalent to the English lower layer super output areas.

15 We also use monthly data for the time-series analysis in Section 4.2.

16 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.

17 For more detail on these classifications, see https://www.gov.scot/publications/user-guide-recorded-crime-statistics-scotland/pages/16/.

18 Note, that we do not use the Crime domain of SIMD as we use data from Police Scotland to measure crime numbers.

Table 1. Summary statistics—analytical sample—by distance ring (2007–2009)

	200 m			200–400 m			400–600 m			600–800 m			800–1000 m (outer ring)		
	Mean	SD	DZ—Years	Mean	SD	DZ—Years	Mean	SD	DZ—Years	Mean	SD	DZ—Years	Mean	SD	DZ—Years
Outcomes (pre-TRA)															
Crimes	79.12	45.62	24	89.56	81.40	45	72.67	60.09	54	58.03	48.39	96	89.74	117.06	138
Crime rate (per 1000)	104.94	46.67	24	94.62	79.22	45	97.06	84.30	54	79.84	75.44	96	109.00	140.65	138
Offences	63.58	49.54	24	80.40	86.08	45	74.59	114.49	54	49.01	53.33	96	71.64	87.27	138
Offence rate (per 1000)	83.85	60.10	24	82.72	78.43	45	97.26	138.63	54	65.20	74.93	96	86.44	105.77	138
Violent crime rate (per 1000)	73.09	39.27	24	52.83	39.26	45	46.52	40.38	54	39.43	40.57	96	57.48	73.88	138
Sexual crimes rate (per 1000)	2.01	1.60	24	1.91	2.43	45	1.76	2.91	54	1.59	1.92	96	3.38	7.21	138
Dishonesty and theft rate (per 1000)	51.55	33.20	24	54.77	65.10	45	68.67	81.81	54	44.62	64.70	96	55.52	94.72	138
Vandalism rate (per 1000)	38.45	14.68	24	29.81	19.85	45	31.30	23.20	54	27.03	17.99	96	31.82	18.42	138
Other crime rate (per 1000)	48.58	39.59	24	36.69	25.89	45	35.77	45.65	54	32.03	43.13	96	50.80	88.39	138
Base controls (2006)															
SIMD income (2006)	34.44	8.78	24	29.00	10.32	45	29.34	10.48	54	30.49	14.19	96	27.05	13.96	138
SIMD employment (2006)	27.88	7.90	24	23.83	9.36	45	24.49	9.81	54	25.92	12.39	96	23.24	12.80	138
SIMD health (2006)	1.32	0.52	24	1.27	0.72	45	1.19	0.64	54	1.11	0.89	96	0.89	0.98	138
SIMD housing (2006)	52.02	10.26	24	49.62	8.34	45	49.38	14.18	54	49.49	13.31	96	44.03	17.30	138
SIMD access (2006)	5.38	2.63	24	6.74	4.78	45	5.54	3.87	54	5.69	3.64	96	5.19	4.16	138

Notes: This table provides summary statistics for our main analytical sample, for each distance ring around TRAs. The sample considers data from the three years (2007–2009) before 2010, the treatment year for the first TRA. This sample is limited to include only those Data Zones whose centroid is within 1 km of a TRA site. Base controls are level scores for each domain of the SIMD 2006.

period.[19] There are 8, 15, 18, 32 and 46 Data Zones in rings 0–200, 200–400, 400–600, 600–800 and 800–1000 m, respectively. Generally, in the pre-treatment period, immediate TRA sites (within 200 m) were more deprived in terms of all SIMD measures when compared with Data Zones in other rings, but did not in all cases have more overall crime (or a higher crime rate). TRA areas did however have noticeably higher rates of violent crimes and vandalism, which is consistent with what was generally understood about crime in these areas (see Section 2).

4. Empirical evidence

The main goal of our study is to determine the average change in crime numbers in response to urban regeneration projects (TRAs). Our empirical strategy therefore aims to identify the appropriate counterfactual level of crime had an area not undergone urban regeneration through the TRA programme. To estimate this counterfactual we exploit two key sources of variation in: (i) treatment timing and (ii) spatial distance to TRA areas.

Relying on variation in treatment timing means comparing crime numbers across TRA areas before and after these programmes are implemented. Following this strategy prevents us from estimating spurious effects via comparison of 'treated' areas to areas without large public housing projects—where TRAs would not have been implemented in the first place—which would have likely followed different crime trajectories. On the other hand, simply estimating effects using variation in treatment timing would ignore potential spatial spillover effects from TRAs to neighbouring areas. To mitigate this, our analysis complements a simple model that relies on treatment timing with more complex strategies that incorporate this spatial dimension. Following the related literature (Aliprantis and Hartley, 2015; Sandler, 2017; Blanco and Neri, 2021), we estimate the following model:

$$Crime_{it} = \sum_{r \in R} \beta_r \times TRA_i \times Post_t \times D_{r \in R} + \gamma' X_{i,2006} \times \theta_t + \theta_i + \theta_t + \theta_c t + \epsilon_{it} \qquad (1)$$

where our dependent variable is the log of crime (and offence) numbers in each Data Zone i for each year t.[20] Our main parameters of interest are the β_r coefficients corresponding to each distance ring $r \in R$. These identify changes in crime in Data Zone i within ring r distance of each TRA. We operationalize this by constructing a set of indicators (represented by $D_{r \in R}$) switching to 1 if a Data Zone's centroid falls within ring radius $r \in \mathrm{R} = (200, 400, 600$ and $800\,\mathrm{m})$, following the implementation of the local TRA $(TRA_i \times Post_t)$. The timing indicator $Post_t$ varies by TRAs as these programmes were implemented at different dates in different areas. Our identification strategy relies on the fact that treatment status is determined by proximity to each TRA. TRA centroids in our case are nested within specific Data Zones, but these areas (and their effects on crime) likely extend Data Zone boundaries. We therefore also assign treated status to Data Zones

19 There are 2 and 3 years missing in the data for two data zones, while for the rest of the sample there are no gaps in the data.

20 There is one data zone–year combination that is dropped after the log transformation due to a zero value for crimes. The data zone is in the third ring for one TRA and outside the sample for the other ones. Retaining this observation using a log(y+1) or inverse hyperbolic sine transformation makes no difference to our results, but using these transformations could introduce other problems (see Section 4.1.3). In Section 4.1.3, we show that our results are robust to different ways of specifying the outcome variable.

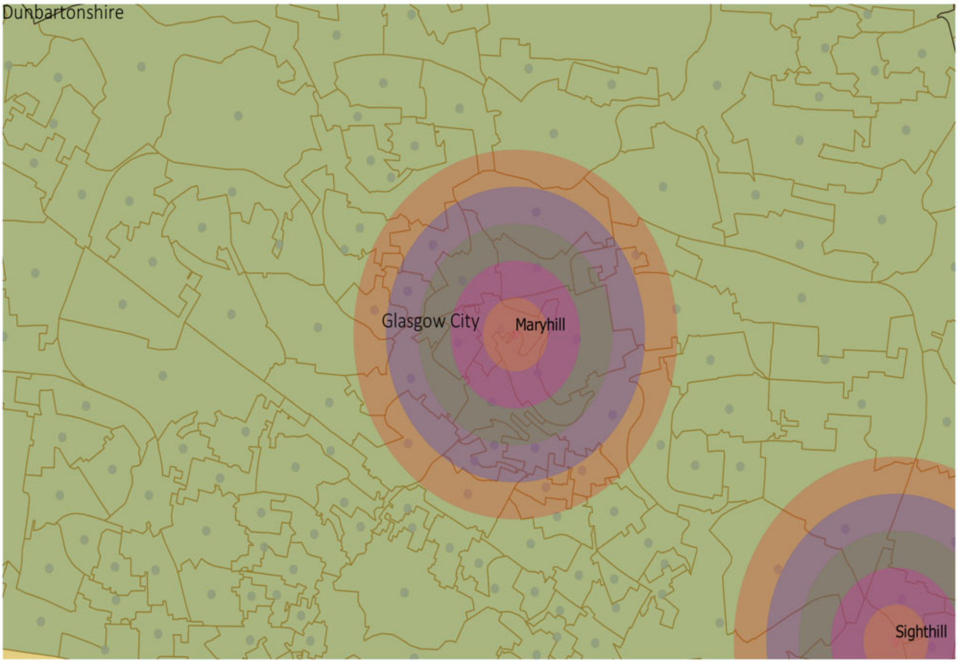

Figure 2 Distance rings—Maryhill TRA.
Notes: This figure reports the rings around Maryhill's TRA. The inner ring has a radius of 200 m, whereas the one immediately after (purple) is within 200–400 m from the TRA's centroid. The outer (orange) ring is instead within 800–1000 m from the TRA's centroid. The green polygons, and the dots within them, represent our statistical units, Data Zones, alongside their centroids. According to our mapping strategy, five Data Zones' centroids lie within 200–400 m from the TRA site. These five Data Zones are therefore 'treated' by that ring. One Data Zone centroid lies within 200 m while the red centroid around which the circles are drawn indicates the TRA's centroid.

whose centroids are within a wider set of radii of the centroid of the TRA Data Zone.[21] Data Zones with centroids located between 800 and 1000 m from the TRA are designated to be part of the 'outer ring', which serves as our control group. The approach we use is illustrated in Figure 2, and relies on the stipulation that, conditional on observables and Data Zone fixed effects, the only difference between rings will be distance to the TRA.

Any difference in crime *levels* across Data Zones is accounted for by estimating Equation (1) with a TWFE approach using θ_i and θ_t. The term θ_i controls for Data Zone-specific effects, whereas θ_t takes into account any time-specific variation which is common to all Data Zones. We want to rule out the possibility that Data Zone-specific and time-varying shocks would bias our results by simultaneously driving the timing of the implementation of the TRA and changes in crime rates, for example, city-level population shocks that

21 Data zone centroids being within a given ring need not mean that the whole data zone is, as these areas sometimes span across different treatment rings when we define these based on short distances. This can potentially lead to some crimes occurring at distances technically outside each ring to be 'reassigned' into a given ring if the data zone where they occur has its centroid within that ring. This lack of precision in our measurement of distances is a feature of data zone level (as opposed to geocoded) crime data and is a limitation of our study. We nonetheless include robustness checks below to check the sensitivity of our results to changes in the way we define rings based on different methods for calculating distances.

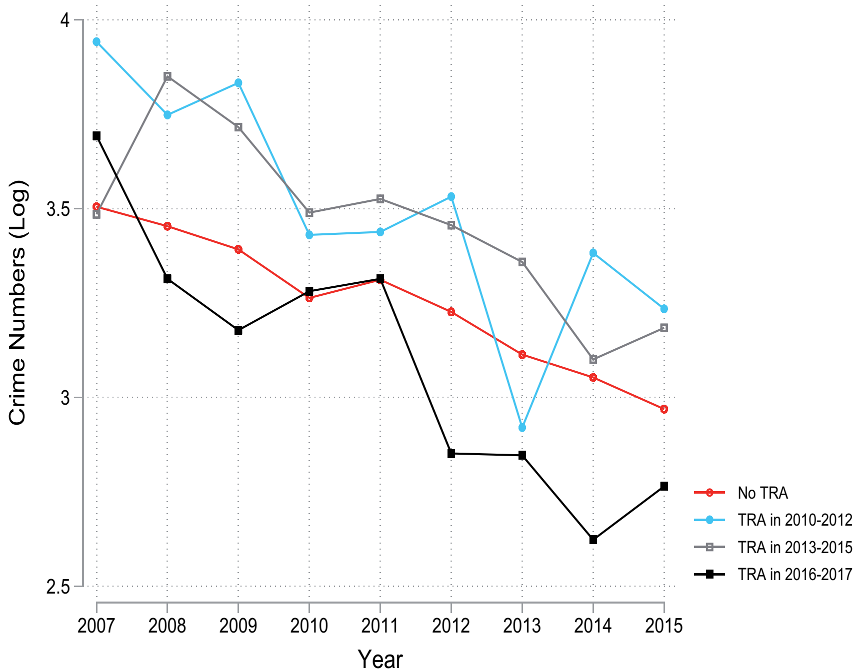

Figure 3 Trends in crime numbers—TRA timing groups.
Notes: This figure reports (IHS) crime numbers' trends, broken down by time of implementation of the TRAs. Each line is a trend for all Data Zones' affected by TRAs' implementation within a specific time window. For instance, the red line is the average crime numbers for all Data Zones whose centroid did not fall within a (at most) 1000 m radius from a TRA. The turquoise line instead is the average crime numbers for all Data Zones matched to any TRA whose implementation occurred between 2010 and 2012 and so forth.

heterogeneously affect neighbourhoods, resulting in higher population density, simultaneously urging urban redevelopment and providing a larger pool of victims for criminals.[22] Unfortunately, limited variation at the Data Zone level means we need to use a higher level of aggregation, and therefore we include time trends specific to each Intermediate Data Zone ($\theta_c t$). An Intermediate Zone includes two to nine Data Zones. This approach was previously employed by Sandler (2017). Finally, we control for $X_{i,2006}$, namely our base period set of Data Zone level controls. These include SIMD scores for income, employment, health, housing and access to services. By using these values in 2006, we control for neighbourhood characteristics which are pre-determined relative to the treatment, and thus are not potential outcomes of the regeneration.[23] Naturally, since these covariates are fixed across Data Zones, estimation of γ is only possible by interacting them with year indicators.

We combine this spatial approach with a staggered DiD strategy, whereby crime numbers of the inner rings are compared with those of the outer ring, before and after the implementation of each TRA. A crucial assumption for our identification strategy is that

22 We only observe population in 2011 as a result of the latest census, thus year-to-year population changes are unobserved to us.

23 Our sample starts in 2007 and the first TRA was implemented in 2010.

trends in crime numbers did not influence TRA implementation dates in affected neighbourhoods. In other words, neighbourhoods that adopted TRAs early did not do so in response to increased crime numbers in the surrounding area. Following Aliprantis and Hartley (2015), we test this by plotting time trends in crime numbers for groups of Data Zones where TRAs were implemented at different points in time (Figure 3). We can see from Figure 3 that time trends are mostly very similar across the different groups, and that for all groups the overall negative trend in crime numbers tends to precipitate TRA implementations.

A characteristic of our set up is that TRA implementations occur at different points in time (see Supplementary Appendix Table A.1). A burgeoning literature discusses how the standard TWFE approach might not be suitable in the context of staggered timing due to the possibility of heterogeneous treatment effects (see Roth et al. (2023) for a review). This is simply because if treatment effects are not distributed identically across treatment groups and time periods, the weights for each of these effects in the average treatment effect on the treated (ATT) are not correctly specified, and the ATT is therefore not identified. To overcome this issue, alternatively to our TWFE model we also estimate our baseline model using a DiD2S approach, as first proposed by Gardner (2022).[24]

The intuition behind the DiD2S approach is simple: it is designed to recover the average difference in outcomes between treated and control units, after removing group and time period-specific effects (Gardner, 2022). In the simplest version of the two-stage procedure, the first stage contains a regression of the outcomes on group and time period fixed effects, estimated on the sample of untreated observations, namely never-treated and not-yet-treated units. In the second stage, the first stage estimates for group and time period effects are subtracted from actual outcomes (i.e., outcomes for both treated and untreated units), and these adjusted outcomes are then regressed on a treatment indicator. Simply put, we can infer the never-treated (potential) outcome for each treated unit using the predicted values from the first-stage regression (Roth et al., 2023). Gardner (2022) shows that under the standard parallel trends assumption the second stage identifies the ATT effect even when average treatment effects are heterogeneous across groups and time periods.[25]

A particular advantage of the DiD2S approach in our case is that we can further modify it to allow for the incorporation of spatial spillovers (see above), as suggested by Butts (2023). In the context of our study, this is operationalized by estimating Equation (1) in two stages.

1. First, we estimate

$$Crime_{it} = \gamma' X_{i,2006} \times \theta_t + \theta_i + \theta_t + \theta_c t + \eta_{it} \tag{2}$$

for observations where both the treatment indicator ($TRA_i \times Post_t$) and the distance ring dummy (for all rings $r \in R$) are equal to zero. This equation contains all of our controls and fixed effects from Equation (1) on the right hand side, and is used to remove the

24 The method developed by Gardner (2022) is part of a wider group of approaches that use imputation techniques to overcome the limitations of the standard TWFE approach (Wooldridge, 2021; Borusyak et al., 2022; Liu et al., 2022). According to Roth et al. (2023), these approaches tend to yield valid causal estimates of average treatment effects when parallel trends hold for all timing groups (and periods) and there are no anticipation effects. Out of this group of approaches, DiD2S is particularly suitable for our analysis as it allows for the incorporation of spatial spillover effects following the work of Butts (2023).

25 In our case, the parallel trends assumption takes the form: $E[Y_{it}|i, t, T^k_{it}] = \alpha_i + \gamma_t + T^k_{it}\delta^k_{it}$, where T_{it} is the treatment indicator equivalent to $TRA_i \times Post_t$ in Equation (1).

fixed and common trend component. From this regression, we retain the estimated effects $\widetilde{\gamma}, \widetilde{\theta_t}, \widetilde{\theta_i}, \widetilde{\theta_c t}$.

2. Regress the adjusted outcome $\widetilde{Crime_{it}} = Crime_{it} - \widetilde{\gamma} - \widetilde{\theta_i} - \widetilde{\theta_t} - \widetilde{\theta_c t}$ for all observations (both treated and untreated) on treatment and spillover dummies corresponding to treated areas and distance rings the treatment effect could spill over into.[26] The second-stage equation is then estimated as:

$$\widetilde{Crime_{it}} = \sum_{r \in R} \beta_r \times TRA_i \times Post_t \times D_{r \in R} + \mu_{it}. \tag{3}$$

The spillover dummies are created as the interaction term between the control group ($\text{TRA}_i = 0$) and rings $r \in R$.

From Equation (3), we report the same β_r coefficients as we do for our TWFE specifications in Equation (1). These estimate the effects of TRAs on crime for each distance ring $r \in R$. The results for our baseline TWFE and DiD2S specifications, for both crimes and offences, are summarized in Table 2. Our preferred specification is the DiD2S specification (Columns (6) and (12) in Table 2) incorporating both base controls (interacted with year fixed-effects) and area-specific time trends as these should account for potential treatment effect heterogeneity and also reduce the likelihood that time-varying shocks specific to local areas drive our results.

Our data structure also allow us to estimate event study specifications, where we interact treated distance rings with event time indicators to estimate treatment effects over time. The event study specification takes the following form:

$$Crime_{it} = \sum_{\tau=-M}^{L} \sum_{r \in R} \beta_{\tau,r} \times TRA_i \times 1(t - E_i = \tau) \times D_{r \in R} + \gamma' X_{i,2006} \times \theta_t + \theta_i + \theta_t + \theta_c t + \epsilon_{it}, \tag{4}$$

where $\beta_{\tau,r}$ are the changes in crime across treated rings in years before (–M) and after (L) the local TRA implementation. This allows us to formally test for the absence of pre-trends in crime rates and examine how crime effects change over time. Event studies for our DiD2S specifications are plotted in Figure 4.

4.1. Results

4.1.1. Baseline results

Table 2 helps us shed light on the aggregate size of TRAs' effect on local crime. We split our results by crimes and offences, based on the classifications of the Scottish Government. For crimes, Columns (1)–(3) present results from our TWFE specifications, whereas (4)–(6) report coefficients from our DiD2S estimations. Columns (7)–(12) present the same results for offences. The standard errors in every specification are clustered at

26 As $\widetilde{\gamma}$, $\widetilde{theta_i}$, $\widetilde{theta_t}$ and $\widetilde{theta_{ct}}$ are estimated from the sub-sample of never and/or not-yet-treated observations, $\widetilde{crime_{it}}$ are based on out-of-sample observations to obtain predicted values for treated units.

Table 2. Baseline results

	Crimes						Offences					
	TWFE			DiD2S			TWFE			DiD2S		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TRA within 200 m	−0.40	−0.24***	−0.28***	−0.28	−0.19***	−0.19***	−0.18	0.02	−0.02	−0.07	0.07	0.04
	(0.30)	(0.07)	(0.07)	(0.24)	(0.05)	(0.05)	(0.28)	(0.12)	(0.13)	(0.23)	(0.16)	(0.17)
TRA within 200–400 m	−0.12	−0.30***	−0.36***	0.03	−0.17	−0.19*	−0.00	−0.05	−0.11	−0.02	−0.04	−0.06
	(0.13)	(0.10)	(0.11)	(0.12)	(0.10)	(0.11)	(0.17)	(0.14)	(0.16)	(0.14)	(0.15)	(0.18)
TRA within 400–600 m	0.00	0.02	0.01	0.11	0.10	0.11	−0.02	−0.03	−0.04	−0.02	−0.05	−0.06
	(0.12)	(0.08)	(0.08)	(0.10)	(0.07)	(0.07)	(0.11)	(0.09)	(0.10)	(0.09)	(0.09)	(0.10)
TRA within 600–800 m	−0.06	−0.05	−0.09	0.01	−0.04	−0.06	0.14	0.13	0.12	0.17*	−0.07	−0.12
	(0.06)	(0.06)	(0.06)	(0.07)	(0.09)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)	(0.16)	(0.18)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Data Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Int Data Zone linear trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE X Base controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports estimated coefficients β_r from Equation (1). Columns (1)–(6) contain specifications whose dependent variable is the log of crime numbers in each Data Zone area, whereas Columns (7)–(12) repeat the same exercise but using (the log of) offence numbers. Columns (1)–(3) and (7)–(9) report estimates from a TWFE model, whereas Columns (4)–(6) and (10)–(12) refer to the DiD2S model. Base controls include income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis pertains to years 2007–2020. The number of observations in all specifications is 1660 Data Zone-years. Standard errors are clustered at the Data Zone level.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

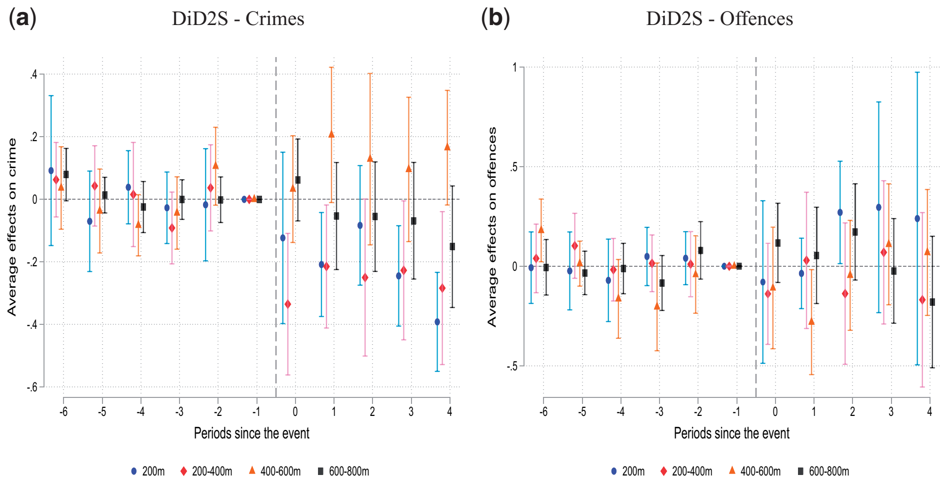

Figure 4 Event studies—baseline model.

Notes: This figure reports estimated coefficients $\beta_{\tau,r}$ from Equation (4) using DiD2S model. Outcomes are the log of crime—panel (a)—and offence numbers—panel (b)—in each Data Zone area. The models' specification is equivalent to those in Columns (6) and (12) of Table 2, whereby we interact year FE with base controls. Base controls include income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis pertains to years 2007–2020. The whiskers are 95% confidence intervals, built using standard errors clustered at the Data Zone level.

the Data Zone level. The TWFE results that account for area level linear trends indicate a 24–28% reduction in crime in the immediate vicinity of the site (200 m), relative to outer ring areas within 800—1000 m from the TRAs, following their implementation. The same DiD2S specifications indicate a 19% reduction in crime. Specifications that account for area-specific time trends tend to be more precisely estimated, which highlights the importance of accounting for variation in crime trends over time specific to each area (see Sandler, 2017). The negative crime effects in the TWFE models within the inner ring are somewhat larger than the 12% reduction found by Blanco and Neri (2021) for large regeneration projects (more similar to TRAs) in London. A reduction in crime of 19% from our full DiD2S specification would correspond to roughly 15 fewer crimes, on average, for each immediate TRA site each year. Considering that 763 people live in the average Data Zone, this is roughly equal to a decrease in the crime rate of 19.7 crimes per 1000 people (0.23SD in our sample), for these local areas. Conversely, we find no evidence of a reduction in offences following TRA implementation.

As we move further away from the first ring, the negative crime effect mostly remains for the 200–400 m ring—although in our full specification the DiD2S estimate is only marginally significant—then becomes positive but mostly small and insignificant in the 400–600 m ring, and becomes a moderately sized but mostly statistically insignificant negative effect in the outermost (600–800 m) treated ring.

Our event study estimates confirm the negative effects on crime within close proximity of TRAs (see Panel (a), Figure 4).[27] The post-treatment negative deviation in trends is

27 The event study estimates for TWFE models are shown in Figure A.3 and suggest largely similar results.

Table 3. TRA Effects by crime subcategory

	Violent crimes		Sexual crimes		Dishonesty and theft		Vandalism		Other	
	TWFE	DiD2S	TWFE	DiD2S	TWFE	DiD2S	TWFE	DiD2S	TWFE	DiD2S
Crime (TRA within 200 m)	−0.15	−0.09	0.14	0.13	−0.31***	−0.20***	−0.26**	−0.14	−0.20*	−0.11
	(0.13)	(0.14)	(0.19)	(0.18)	(0.06)	(0.07)	(0.10)	(0.11)	(0.11)	(0.09)
Crime (TRA within 200–400 m)	−0.26	−0.19	0.01	0.04	−0.33***	−0.19	−0.26**	−0.08	−0.45**	−0.29**
	(0.16)	(0.13)	(0.12)	(0.12)	(0.08)	(0.12)	(0.11)	(0.13)	(0.19)	(0.14)
Crime (TRA within 400–600 m)	0.02	0.07	−0.01	0.03	−0.12	0.04	−0.03	0.03	−0.05	0.03
	(0.09)	(0.08)	(0.13)	(0.16)	(0.08)	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)
Crime (TRA within 600–800 m)	0.01	0.04	−0.18*	−0.26*	−0.08	−0.11	−0.14**	−0.10	0.07	0.21
	(0.08)	(0.11)	(0.10)	(0.13)	(0.06)	(0.09)	(0.07)	(0.10)	(0.10)	(0.16)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Data Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE × Base controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Int Data Zone linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimated coefficients β_r from Equation (1). Outcome variables are the IHS of crime numbers, by crime subcategory, in each Data Zone area. Base controls include income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis pertains to years 2007–2020. The number of observations in all specifications is 1661 Data Zone-years. Standard errors are clustered at the Data Zone level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

particularly notable within 200 m and within 200–400 m of TRAs. For these two rings, the negative effects seem persistent although not always significant in every time period. Moreover, in line with Columns (4)–(6) of Table 2, point estimates are positive (but not significant) in the 400–600 m ring following regeneration. The post-treatment point estimates for offences are mostly close to zero and not significant. Finally, Figure 4 documents the absence of pre-trends for both outcomes, within any distance from each TRA. Point estimates are close to zero and not significant at any reasonable level. The negative crime effects we observe post-treatment are therefore predicated on the absence of pre-existing deviations in crime trends across treated and control units. Overall, evidence from the event studies suggests a persistent reduction in crime in close proximity of the regeneration sites (within 400 m).

4.1.2. Heterogeneity

Table 3 summarizes our main results by different subcategories of crime, such as violent crimes (murder, assault, etc.), sexual crimes, dishonesty and theft (theft, attempted housebreaking and housebreaking), vandalism and other crimes.[28] To retain zero values in different crime categories, for this specification, we use an inverse hyperbolic sine (IHS) transformation of crime numbers.[29] Using the IHS provides a helpful transformation of

28 For a detailed breakdown of these categories, see Table A.2.
29 While the IHS transformation is useful in our case to retain both zero crime values and also the interpretation of our baseline effects, there is an emerging literature on scaling issues associated with this transformation which

right-skewed data which preserves the log-interpretation, that is, $\beta_r \times 100$ change in crime following TRAs implementation, while still accommodating null values.[30] Overall, the negative effects for the innermost ring, though not always significant, are consistent across all subcategories, with the exception of sexual crimes (although there also seems to be a reduction for this category albeit a bit further away). The most striking (and significant) result is the reduction in thefts by 20–31% in close proximity (within 400 m) to TRA sites, and the reduction in 'other crimes' within 400 m of urban regeneration projects. As we move to the outermost distance ring, our estimates for this category are also suggestive of a positive spillover effect, although these effects are not statistically significant. The 'other' category consists mainly of weapon and drugs possession, with drug-related crimes accounting for about 70% of the overall category. Therefore, this result is consistent with the idea that regeneration projects remove the physical setting where certain types of crimes (e.g. theft or drug-related crimes) could take place (Newman, 1972; Aliprantis and Hartley, 2015). It is also in line with the 'broken windows' theory of crime, whereby decaying urban spaces (such as housing estates) can create an atmosphere of lawlessness that encourages criminal activities, while the removal or improvement of these spaces would have the opposite effect (Kelling and Wilson, 1982). Conversely, urban regeneration could in theory also encourage certain types of crimes, such as theft and burglary, by making these more lucrative due to higher property values and wealthier residents. This can be compensated by new-built housing developments being more secure compared with old housing estates due to security measures or alarm technologies (Disney et al., 2020). As we find a negative effect for thefts, it is likely that the latter is the case or alternatively that criminals engaging in these activities simply moved into new areas to target.

While the outer treated rings generally show no evidence of spillover effects, these null effects could mask heterogeneity across different types of housing areas within each ring.[31] If regeneration projects remove the physical setting for certain types of crimes, could criminal activities relocate to nearby (non-TRA) housing estates that provide similarly convenient settings for crime?[32] We examine this by including interaction terms in our baseline specifications where we interact the post-TRA treatment ring indicator with a dummy for whether there is a public housing estate located in each Data Zone. We estimate our models using wider (400 m) distance rings compared with our baseline specification to increase the sample size of public housing estates that we can include.[33] The results are summarized in Table 4. Generally, we find negative crime effects for housing estates at closer distances (400–800 m) and positive effects a bit further away, but none of these effects are significant, and the large negative effects in the first ring can be due to

can make estimates unreliable and difficult to interpret under specific circumstances (Bellemare and Wichman, 2020; Chen and Roth, 2022; Mullahy and Norton, 2022). For this reason, we check the robustness of our IHS estimates in Section 4.1.3.

30 For the number of crimes c, IHS transformation is $\sin h^{-1}(c) = ln(c + \sqrt{c^2 + 1})$.

31 We further check this with wider distance rings below and find no evidence of crime effects outside 400 m of TRAs. Generally, as shown below, our results are robust to the choice of distance radii used.

32 As mentioned in Section 2, qualitative evidence suggests that most former tenants did not relocate to other public housing estates or high-rises (GoWell, 2011). Thus, this channel is likely unrelated to residential replacement and is more related to criminals finding new locations for crime.

33 There are only two public housing estates within 800 m of TRAs and there are naturally no (non-TRA) housing estates within 400 m of regeneration areas. In the wider distance ring sample, there are also 6 between 800 and 1200 m, and a further 10 within 1200 1600 m of TRAs. The list and locations of public housing estates in Glasgow are from the website of the Tower Block project conducted by the University of Edinburgh, see https://www.towerblock.eca.ed.ac.uk/search.

Table 4. TRA effects in nearby public housing estates

	Crimes		Offences	
	DiD2S			
	(1)	(2)	(3)	(4)
TRA within 400–800 m × Public housing estate	−0.49	−0.39	−0.64	−0.51
	(0.31)	(0.27)	(0.59)	(0.53)
TRA within 800–1200 m × Public housing estate	0.11	0.07	0.18	0.19
	(0.15)	(0.11)	(0.17)	(0.17)
TRA within 1200–1600 m × Public housing estate	0.03	0.05	−0.08	−0.13
	(0.12)	(0.11)	(0.13)	(0.15)
Year FE	Yes	Yes	Yes	Yes
Data Zone FE	Yes	Yes	Yes	Yes
Int Data Zone linear trend	Yes	Yes	Yes	Yes
Year FE × Base controls	No	Yes	No	Yes

Notes: This table reports estimated coefficients for interaction terms between our treatment ring variables from Equation (1) and indicators for whether a Data Zone has a non-TRA public housing estate located within it. Columns (1) and (2) contain DiD2S specifications whose dependent variable is the log of crime numbers in each Data Zone area, whereas Columns (3) and (4) repeat the same exercise but using (the log of) offence numbers. Base controls include income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis pertains to years 2007–2020. The number of observations in all specifications is 6993 Data Zone-years. Standard errors are clustered at the Data Zone level.
$^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

noisy estimates as only two Data Zones with public housing estates are included there. Looking at the crime trends for different categories for non-TRA housing estates in Figure 5, we can also see that only the first TRA implementation in 2010 can be associated with a subsequent increase in crime at other estates, and the trend is unchanged after other TRAs are implemented. Overall, there is little evidence that crime relocates to other public housing estates after regeneration projects are implemented nearby. Nonetheless, our estimates here are under-powered due to the small sample size of Data Zones with non-TRA housing estates in treated rings, and thus we would hesitate to rule out such effects completely.

4.1.3 Robustness checks

4.1.3.1. Sensitivity to distance ring radii In this section, we address some residual concerns in relation to our baseline model. First, one could be concerned that our results are sensitive to the choice of distance radii used to specify treated areas. If this was true, we might see the effects disappear within a larger radius. Alternatively, it could be a concern that in our baseline analysis, we use a control ring (800–1000 m) that contains a small cluster of observations leading to less precise estimates. For this reason, we re-run the exercise in Section 4.1 but use a wider set of radii, for example, $R = \{400, 800, 1200, 1600\}$ and a control ring of 1600–2000 m.[34] Results from this robustness check are

34 Similarly to the set-up in Blanco and Neri (2021), some of the rings around TRAs might overlap using the wider distance rings, that is, a data zone may appear twice in the data set, for example, as the second ring of

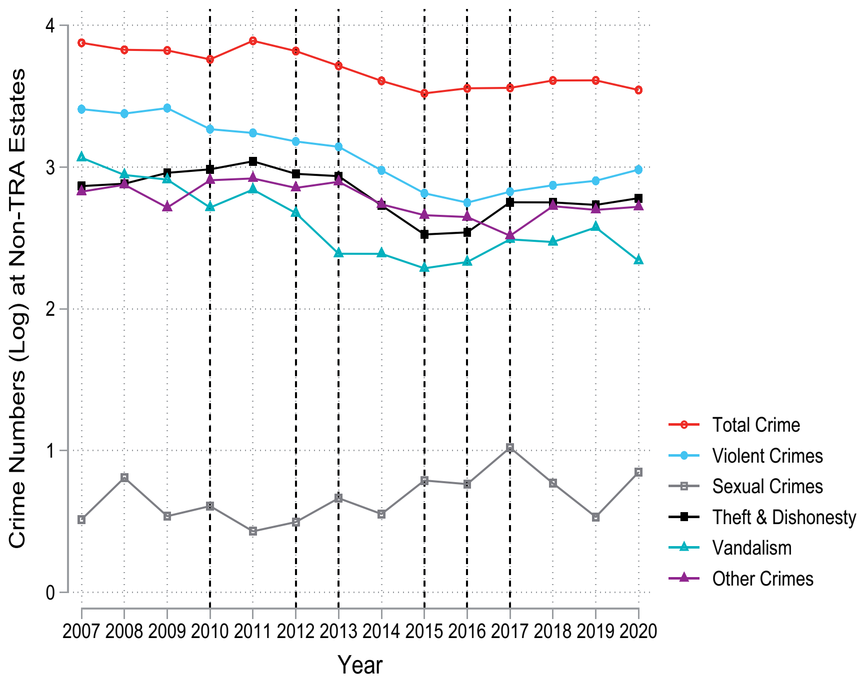

Figure 5 Trends in crime numbers—non-TRA housing estates.
Notes: This figure reports (log) crime numbers' trends for public housing estates in the City of Glasgow that are not part of a TRA during our sample period. Vertical dashed lines indicate the years of TRA implementations as per Supplementary Appendix Table A.1.

summarized in Table 5. Overall, our results remain very similar to our baseline estimates, with clear evidence of a negative crime effect within 400 m of TRA sites. Effects for rings further away are close to zero and not significant at any reasonable level. This is important, because, combined with our results in Table 2, these findings suggest no crime effects within the areas former TRA tenants likely moved into (see Section 2). This could imply that residential displacement is not one of the main channels driving crime effects. Alternatively, it is possible that if former tenants were dispersed across wider areas, any associated crime effects would be too small to detect, even at the aggregate (400 m ring) level. For offences, this specification suggests negative effects, although these are still only marginally significant.

4.1.3.2. Sensitivity to inclusion of immediate TRA site Another concern is that the large reduction in crime observed within close distance of TRA centroids is purely a mechanical one. As old estates are demolished, and large parts of the surrounding areas are turned into work sites, the setting where crime could happen becomes unavailable. Even when newer buildings are occupied, criminal activity may have already spilled over to nearby areas. This is likely not due to residents of old housing estates being engaged in criminal activity themselves, but that the estates served as a centralized location for crime where

TRA 'A' and also as the third ring of TRA 'B'. Such 'stacked' designs are understood to be robust to potential treatment effect heterogeneity (Blanco and Neri, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2022).

Table 5. Robustness check—wider distance rings

	Crimes						Offences					
	TWFE			DiD2S			TWFE			DiD2S		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TRA within 400 m	−0.23	−0.40***	−0.40***	−0.11	−0.31***	−0.31***	−0.11	−0.27*	−0.26*	−0.06	−0.10	−0.09
	(0.14)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.15)	(0.16)	(0.14)	(0.12)	(0.14)	(0.14)
TRA within 400–800 m	−0.04	−0.05	−0.05	−0.01	0.01	0.00	0.03	−0.03	−0.01	0.06	0.02	0.03
	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)	(0.07)
TRA within 800–1200 m	−0.01	0.02	0.02	0.00	0.05	0.04	−0.03	−0.03	−0.02	−0.01	−0.03	−0.01
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)
TRA within 1200–1600 m	0.00	0.02	0.02	−0.01	0.00	0.00	−0.03	−0.02	−0.01	−0.01	0.01	0.03
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Data Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Int Data Zone linear trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE × Base controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports estimated coefficients β_r from Equation (1). Unlike Table 2, we consider wider radii, starting with a 400 m radius, and moving up in 400 m increments. Columns (1)–(6) contain specifications whose dependent variable is the log of crime numbers in each Data Zone area, whereas Columns (7)–(12) repeat the same exercise but using (the log of) offence numbers. Columns (1)–(3) and (7)–(9) report estimates from a TWFE model, whereas Columns (4)–(6) and (10–(12) refer to the DiD2S model. Base controls include income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis pertains to years 2007–2020. The number of observations in all specifications is 6993 Data Zone-years. Standard errors are clustered at the Data Zone level.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 6. Robustness check—TRA Data Zone excluded

	Crimes						Offences					
	TWFE			DiD2S			TWFE			DiD2S		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TRA within 200–400 m	−0.12	−0.30***	−0.37***	0.02	−0.21*	−0.21*	0.00	−0.03	−0.07	−0.03	0.07	0.06
	(0.13)	(0.10)	(0.11)	(0.12)	(0.12)	(0.12)	(0.17)	(0.15)	(0.17)	(0.14)	(0.13)	(0.17)
TRA within 400–600 m	0.01	0.04	0.03	0.11	0.09	0.11	−0.01	−0.00	−0.00	−0.01	−0.02	−0.02
	(0.12)	(0.08)	(0.08)	(0.11)	(0.08)	(0.07)	(0.12)	(0.09)	(0.09)	(0.09)	(0.10)	(0.11)
TRA within 600–800 m	−0.06	−0.06	−0.09	−0.01	−0.04	−0.08	0.14	0.14	0.12	0.16	−0.07	−0.14
	(0.06)	(0.06)	(0.06)	(0.07)	(0.09)	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)	(0.16)	(0.18)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Data Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Int Data Zone linear trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE × Base controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports estimated coefficients β_r from Equation (1). Unlike Table 2, we exclude the 200 m-radius ring from the sample. Columns (1)–(6) contain specifications whose dependent variable is the log of crime numbers in each Data Zone area, whereas Columns (7)–(12) repeat the same exercise but using (the log of) offence numbers. Columns (1)–(3) and (7)–(9) report estimates from a TWFE model, whereas Columns (4)–(6) and (10)–(12) refer to the DiD2S model. Base controls include income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis pertains to years 2007–2020. Standard errors are clustered at the Data Zone level.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

both victims and perpetrators were present (Sandler, 2017). Therefore, we run an additional robustness check, in the same fashion as Sandler (2017), whereby we exclude the innermost ring from our estimations, to assess whether there are crime effects once the immediate regeneration sites are not considered. Results are reported in Table 6. We can see that now the ring closest to the TRA site is the one spanning within 200–400 m. Our preferred DiD2S estimates are suggestive of a 21% reduction in crime, which is only marginally significant, while TWFE estimates continue to suggest a large negative effect. Overall, these findings suggest that local crime reductions remain even if we exclude the immediate (central) TRA area. It is possible that this is due to the wider effects of TRAs on local neighbourhoods, where amenities are improved within a wider area. Equally, since TRAs tend to cover large areas that may span multiple Data Zones, it is possible that in some cases effects concentrate within 400 m simply because the TRA extends this radius. Once again, none of the results for offences are statistically significant.

4.1.3.3. Sensitivity to imprecise treatment timing Another concern is that results might be sensitive to changing the treatment date, in case some TRAs were implemented with effective delays whereby residents could only move in much later than the indicated implementation date. Supplementary Appendix Table A.5 implements our baseline regression but pushing treatment dates 1 year later. Results are unchanged relative to our baseline estimates.

4.1.3.4. Sensitivity to model specification For our baseline estimates we mainly rely on the DiD2S approach which allows us to deal with potential treatment effect heterogeneity while also incorporating spatial spillovers (Butts, 2023; Gardner, 2022). Nonetheless, these approaches are as of yet relatively untested, and thus we turn to the alternative CSDiD estimator developed by Callaway and Sant'Anna (2021). This approach relies on the estimation of group-time treatment effects at every treatment timing group relative to either a never treated or not yet treated group. In our case, to keep results comparable to DiD2S ones, we include both of these in our control group. The ATT is calculated for each group-time combination and is then aggregated into an overall ATT estimate. We report these for each treated distance ring in Supplementary Appendix Table A.6. Since in CSDiD we can only include one treatment at a time, we effectively restrict our sample to one treatment ring and the outer ring for each regression, so that the Data Zones of the outer ring can form the never-treated control group. This further reduces our sample size when using this approach compared with our baseline TWFE and DiD2S specifications. Our overall treatment effect estimates remain similar to our baseline ones but are no longer significant using this approach—this is possibly due to CSDiD estimates being underpowered due to the sample size issues explained above.

4.1.3.5. Sensitivity to outcome specification For our analysis of the TRA effects on different types of crime (Table 3), we rely on an IHS transformation of crime numbers as our outcome. An emerging literature highlights that the IHS transformation can lead to estimates that are difficult to interpret or unreliable due to scaling issues (Bellemare and Wichman, 2020; Chen and Roth, 2022; Mullahy and Norton, 2022). These studies provide a number of potential solutions or robustness checks for these issues, such as using a normalized outcome variable (Chen and Roth, 2022) or using Poisson regressions of the count outcome variable to model the data generating process more directly (Mullahy and Norton, 2022). We thus replicate Table 3 using crime rates (per 1,000 population) as outcomes and also using Poisson regressions with crime numbers as the outcome variable. Our results are largely robust to changes in the outcome specification, see Supplementary Appendix Tables A.7 and A.8. Our findings also remain the same when we replicate our baseline results from Table 2 using these outcome specifications, see Supplementary Appendix Tables A.9 and A.10.[35]

4.1.3.6. Distance from TRAs The way we specify the distance rings around TRAs could lead to some imprecision due to the locations of the Data Zone centroids we use to measure distance from each TRA (see Section 4). For this reason, we might have Data Zones that belong to a specific ring based on the location of their centroid but whose area extends into other ring(s). In Table 5, we show that our results hold when we use wider distance rings. Here, we further check the sensitivity of our results to the way we calculate distances from TRAs in two ways. First, in Supplementary Appendix Table A.11, we show that our results remain similar when we specify distance rings without relying on distance from Data Zone centroids. In this specification, the innermost ring is the Data

35 Our results in Table 2 rely on a log transformation of crime numbers. Log transformations do not have the same known issues associated with them as IHS or $\log(y + 1)$ transformations (Mullahy and Norton, 2022) although there is evidence that under specific circumstances they are also not entirely reliable (Silva and Tenreyro, 2006).

Zones containing the TRAs, the next treated ring contains the Data Zones neighbouring the TRA Data Zones, while the outer ring are those outside these areas. Our results show that the negative crime effect for the TRA Data Zones remains, but there is a null effect for neighbouring Data Zones, suggesting once again that crime reductions are highly localized and specific to TRA locations. Additionally, in Supplementary Appendix B, we check the robustness of our results to an alternative way of calculating distances from TRAs, whereby we map Data Zone level data into symmetric hexagonal grids with equal areas and re-estimate baseline models. This approach is summarized in Supplementary Appendix B and results are reported in Supplementary Appendix Table B.1 and Supplementary Appendix Figure B.2. Overall, our results are robust to using this alternative approach.

4.1.4. Additional outcomes

Here, we examine how TRAs affected various types of deprivation in their own Data Zone and in areas nearby. The main aspects of deprivation we focus on are income, employment, mortality, drug-related hospitalizations, where lower numbers indicate lower deprivation, and overall SIMD rank, where a higher rank indicates lower deprivation. Section 3 describes our outcome variables in more detail, while Table 7 summarizes our results for these outcomes when estimating our baseline DiD2S specification. The mean and standard deviation for each dimension of deprivation are reported at the bottom of the table.

Overall, the results summarized in Table 7 suggest that TRAs reduced neighbourhood deprivation across several dimensions. All types of deprivation are reduced in the immediate vicinity of TRAs and the overall SIMD rank of the main Data Zones affected improves substantially. Nonetheless, these effects are much less clear when we assess them even as much as 400 m away and the effects disappear (or change sign) further away. While this evidence is only suggestive, taken together with the effects observed for crime numbers, it does imply that TRA effects are mostly confined to the areas they contain. Our results are consistent with a mechanical effect on neighbourhood composition whereby the change from low-income to mixed-income housing leads to gentrification, as new TRA residents are less likely to struggle with unemployment, have higher incomes and better expected health outcomes. It is possible that all of these changes are in turn making crime less attractive for new and existing residents (Aliprantis and Hartley, 2015). Overall, our findings suggest that local crime reductions in and near TRAs could materialize through (i) the removal of physical spaces (high-rises) where criminal activity was taking place and (ii) through improved neighbourhoods with stronger disincentives to crime. Nonetheless, both of these channels are in a large part mechanical—they are a result of replacing one type of housing with another in a specific local area—and do not imply crime reducing effects on the aggregate. The next section deals with this issue in more detail.

4.2. Aggregate-level evidence

The evidence presented in the previous sections point towards a reduction in crime in close proximity of urban regeneration sites. However, the question remains as to how appropriate our empirical approach is to detect aggregate-level (city-wide) changes in crime in response to urban regeneration projects. While we find strong evidence of highly localized reductions in crime, criminals could simply relocate to other parts of the city, leaving

Table 7. DiD2S results—SIMD outcomes

	DiD2S				
	Deprivation in				Overall SIMD
	Income	Employment	Mortality	Drugs	SIMD rank
TRA within 200 m	−0.27***	−0.18***	−42.11***	−315.64***	2.79***
	(0.01)	(0.01)	(7.42)	(34.49)	(0.13)
TRA within 200–400 m	−0.02	−0.02***	24.07*	−105.72**	0.72
	(0.02)	(0.01)	(14.47)	(52.01)	(0.47)
TRA within 400–600 m	−0.01	−0.01	14.55*	53.16	0.22
	(0.01)	(0.01)	(8.05)	(55.86)	(0.33)
TRA within 600–800 m	0.00	0.00	14.55*	18.63	0.37*
	(0.01)	(0.01)	(7.67)	(35.63)	(0.19)
Mean DV	0.24	0.20	129.06	216.86	3.27
SD DV	0.12	0.11	47.60	265.89	2.64
Observations	459	459	459	459	460
Wave FE	Yes	Yes	Yes	Yes	Yes
Data Zone FE	Yes	Yes	Yes	Yes	Yes
Wave FE × Base controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimated coefficients β_r from Equation (1). All columns present results from a DiD2S model. Outcome variables are the rate of income deprived people by Data Zone, the rate of employment deprived people, the standardized mortality ratio, standardized drug-related hospital visits and overall SIMD rank in deciles. Base controls include overall income, employment, health, housing and access to services scores from the 2006 edition of the SIMD. This analysis includes SIMD waves 2006, 2009, 2012, 2016 and 2020. Standard errors are clustered at the Data Zone level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

overall crime numbers unchanged. Following Bruhn (2018), we therefore implement a time-series approach to examine the aggregate effect of TRAs on crime in the city of Glasgow. To do this, we make use of monthly crime data for the whole of the city of Glasgow (see Section 3).

Figure 6 shows monthly trends in all of our indicators from January 2007 to December 2020. We can notice an overall decreasing trend in most of our measures of crime.[36] We want to investigate whether city-wide crime has experienced a similar reduction to the localized one we observe in the micro-data after the implementation of TRAs. Following Bruhn (2018), we estimate the following vector autoregressive model (VAR):

$$Crime_t = \delta_{(m)t} + \alpha t + \sum_{j=1}^{J} \beta_j Crime_{t-j} + \sum_{j=1}^{J} \gamma_j TRA_{t-j} + \epsilon_t \tag{5}$$

$$TRA_t = \delta'_{(m)t} + \alpha' t + \sum_{j=1}^{J} \beta'_j Crime_{t-j} + \sum_{j=1}^{J} \gamma'_j TRA_{t-j} + u_t, \tag{6}$$

where $Crime_t$ is the city-wide, monthly time series of crime rate, which is modelled as a function of monthly dummy variables ($\delta_{(m)t}$) a flexible time trend t as well as its own

36 With the exception of sexual crimes, which exhibit a positive trend.

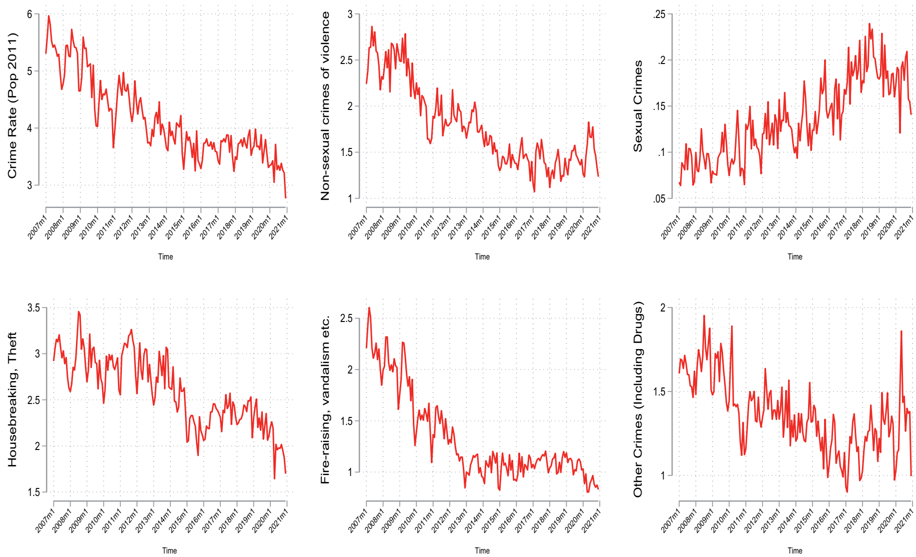

Figure 6 Crime trends—relative to 2011 population.
Notes: This figure shows trends for our six measures of crimes. We calculated mean crime rates at the month level for all Data Zones within the City of Glasgow.

lagged values $Crime_{t-j}$ and finally TRA implementation. This is operationalized through TRA_{t-j}, which switches to one every j months after any TRA is implemented.

While we allow for TRA implementation to be predicted by past values of crime and past implementation by mean of the second equation, our main focus is on the first equation, which tells us how crime rates respond to TRAs. We estimate the above model for the overall crime rate as well as for crime subcategories and select lag length based on information criteria. For instance, for the overall crime rate we have FPE and AIC, suggesting three lags, while HQIC and SBIC suggest two and one lag, respectively. We therefore pick the number of lags suggested by the majority of information criteria. Figure 7 plots the impulse response functions for all of our outcomes. For most of the crimes, we notice a small decrease up to 3 months after TRA implementation, followed by a reversion towards zero. None of these effects are, however, economically significant. For instance, the observed drop in crime following 3 months from the implementation would correspond to roughly 0.24 cases per 1000 inhabitants, per year. Assuming no crime effects elsewhere, and considering that, according to our data, 2.73% of Glasgow's population lives within 400 m of a TRA, our baseline crime effect of 19.7 fewer crimes per 1000 inhabitants for local areas would translate into roughly 0.54 cases per 1000 inhabitants on the aggregate level. The fact that we find aggregate-level crime effects that are generally smaller than this could suggest spillover effects away from TRA areas or that the local crime reductions near TRAs are simply too small to lead to a noticeable city-wide effect. Regardless, we can conclude that while we find evidence of localized (negative) crime effects from TRAs, we find no evidence of a corresponding aggregate-level reduction in crime, and the general equilibrium effect of urban regeneration on crime seems to be a null one.

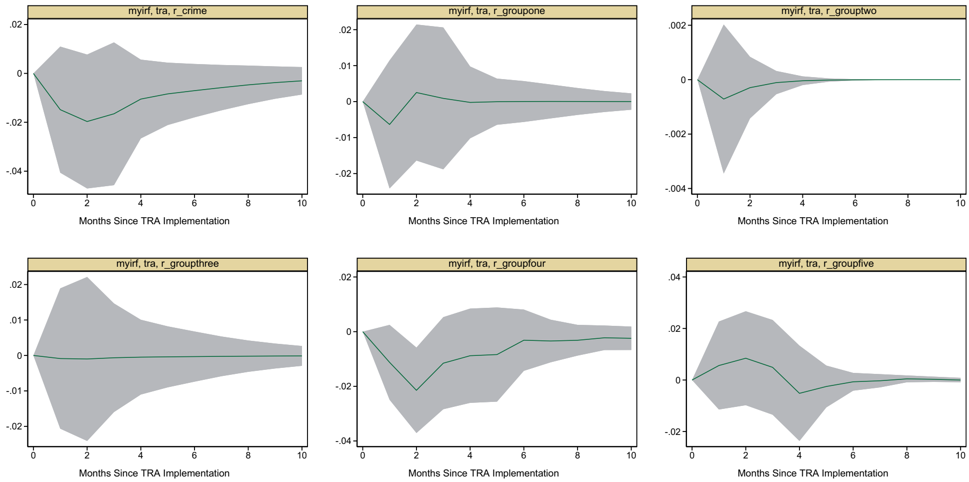

Figure 7 Impulse response function.
Notes: This figure shows impulse responses to TRA implementation of our six measures of crimes. These include overall crime (the top left panel) and five subcategories, from group 1 (violent crimes) to group 5 (other crimes, bottom right panel). We averaged crime rates at the month level and estimate six different VARs between each single crime variable and a TRA dummy variable.

5. Conclusions

Urban regenerations involving large-scale demolitions of public housing estates have often been endorsed on account of their alleged crime-reducing outcomes. In this article, we test this by examining the effects of recent urban regeneration projects on crime in the city of Glasgow, in Scotland. These projects—called TRAs—included the demolition of old public housing estates and their replacement with mixed-income housing, along with the redevelopment of surrounding public spaces. We match a rich panel data set of block-level crime numbers to the location of these projects, and exploit variation in both the timing of TRA implementation, and in proximity to these areas as a way to measure treatment intensity. We document a large reduction in crime in close vicinity (within 400 m) of TRAs but these effects get smaller (and insignificant) as we move further away from TRA locations. We argue that the large reductions in crime within the immediate TRA locations are likely driven by the fact that urban regeneration removed (or replaced) the physical setting where crime could take place. We further find reductions in neighbourhood deprivation following urban regeneration, but once again these findings are confined to immediate TRA locations, and are therefore likely driven by changes in neighbourhood composition as local housing units are replaced by mixed-income housing. Nonetheless such neighbourhood changes could act as a channel for local crime reductions as the incentives to engage in crime get weaker. Finally, we find no evidence of aggregate-level reductions in crime for the city of Glasgow, suggesting that the crime reducing effects of TRAs are confined to their immediate locations.

Our work carries a number of policy implications. While our study finds that any crime spillovers are generally offset by the large reduction experienced near the demolished estates, public authorities need to carefully contemplate the potential spillover and general equilibrium effects of these interventions. In other words, the fact that crime reducing effects are so spatially concentrated to the TRA area implies that it is simply the setting for crime that changes, and general equilibrium effects (an overall reduction in crime) are limited. Simply put, we find no evidence that urban regeneration projects are successful in reducing crime at the aggregate (city-wide) level. Our findings can also advise urban planners on the benefits of mixed-income communities, as opposed to models facilitating segregation. These communities seem to be characterized by lower levels of deprivation compared with the ones they replaced across a variety of domains (crime, employment, health), but it is unclear whether it is long-term residents of these areas who enjoy these benefits or whether they accrue to (and are driven by) new residents.

Further areas remain for future research. First, one main limitation of this article is that it does not provide insights on criminal behaviour. While we find some evidence of local crime reductions specific to certain types of crime, future research could focus on substitution effects between different criminal activities. Second, research can shed light on the mechanisms through which regeneration improves the lives of local residents, for example, better housing conditions or peer effects within mixed-income communities. Finally, researchers could explore which other domains of deprivation are affected by regeneration, if any. Future work, potentially using micro data on residents, can investigate how these projects affect a wider range of outcomes such as public health, social cohesion, or neighbourhood segregation.

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Supplementary material

Supplementary data for this paper are available at *Journal of Economic Geography* online.

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