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INSTITUTE OF RETAIL ECONOMICS

EFFECTS OF BUSINESS IMPROVEMENT DISTRICTS ON FIRM PERFORMANCE, PLACE ATTRACTIVENESS, AND URBAN SAFETY

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Effects of business improvement districts on firm performance, place attractiveness, and urban safety

Sven-Olov Daunfeldt, Oana Mihaescu and Niklas Rudholm

Abstract: Business improvement districts (BIDs) have emerged as possible solutions for the revitalization of urban areas characterized by economic decline. Using a difference-in-differences model, we investigate the effects of a voluntary Swedish BID programme in five cities on firm performance, urban safety, and place attractiveness – both within and outside the BID. We find that the BID programme increased labour productivity for incumbent firms within the BID by 7.62%, mainly through an increase in revenues. However, the positive effect of the BID programme on firm performance is largely transitory, decreasing sharply during the third year and then becoming insignificant. We find no statistically significant impacts on firm performance outside the geographical boundaries of the BIDs. The results also suggest that fewer crimes were committed in the BIDs, as the estimates for all years are negative, though they are significant only for the fourth year after BID implementation. Finally, we detect no statistically significant effects of the BID programme on property values either within or outside the designated BIDs.

Key words: Business improvement district, public–private partnerships, firm performance, labour productivity, property values, crime, difference-in-differences.

JEL codes: H44; L11; L25; R11; R12.

1 Introduction

Many city centres are struggling with long periods of economic decline. Urban sprawl and the decentralization of retail to the urban periphery are often discussed as particularly decisive for this development, resulting in the migration of customers, retail establishments, and other businesses from downtown areas to outlying suburban areas (Peel et al., 2009). Moreover, many local governments have not been able to meet the challenges facing their cities, providing insufficient basic services such as street cleaning and sanitation, maintenance for public spaces, and safety (Guy, 2001; Hoyt and Gopal-Agge, 2007; Jansen, 2017). To avoid extensive downturns, property and business owners have been compelled to provide these services themselves (Hogg et al., 2003).

One initiative that has received widespread attention is the implementation of business improvement districts (BIDs), which are privately managed and publicly sanctioned organizations that supplement public services in derelict urban neighbourhoods. BIDs are often grounded in the following three elements: (i) a well-defined geographical area; (ii) a public–private partnership between local property and business owners on the one side and the local government on the other; and (iii) the assessment of a compulsory tax on local property and business owners, which constitutes the base for the funding of place-based services and is regulated by law (Caruso and Weber, 2006; Hoyt and Gopal-Agge, 2007; Grossman, 2010).

By providing public services in the form of public–private partnerships, BID programmes translate centralized public services to meet special localized needs and connect business ingenuity to communal services (Grossman, 2010). This is often argued to be beneficial for the socioeconomic health and perceived attractiveness of the designated BIDs (Sutton, 2014), thereby increasing customer demand for businesses and services. This may in turn induce an improvement in firm performance, a decrease in crime levels, and, in the longer run, an appreciation of rental rates and real estate values (e.g., Elmedini et al., 2018; Bowen et al., 2019).

Although the positive effects of these public–private partnerships have been emphasized in numerous studies and reports (e.g., Ellen et al., 2007; MacDonald et al., 2010; Miller, 2013), BID programmes have also been criticized for being undemocratic, overregulating the public space, creating wealth-based inequalities in the delivery of public services, and contributing to urban gentrification (Hoyt and Gopal-Agge, 2007; Morçöl and Wolf, 2010). One concern is the lack of accountability in the BID process. Frederickson and Smith (2003) point out that because

the BID governance process is based on public–private cooperation and is thus less hierarchical and bureaucratic, accountability should be about measuring the performance of the BID programmes, which in turn requires the development of robust evaluation models.

A plethora of studies have taken up the task of investigating, measuring, and estimating the effectiveness of BID programmes. Many have examined the institutional aspects of BIDs as a novel form of governance (e.g., Mörçol and Wolf, 2010; Prentice and Porter, 2015; Stein et al., 2017) or have focused on the likelihood of a neighbourhood forming a BID (e.g., Meltzer, 2012). Few studies, however, have attempted to investigate the effects of BID programmes on firm performance (e.g., Sutton, 2014). Interest has instead been directed towards measuring the impact of BIDs on crime levels (e.g., MacDonald et al., 2010, 2013; Mello, 2018; Clutter et al., 2019) or residential and commercial property values (e.g., Ellen et al., 2007; Brooks and Strange, 2011; Miller, 2013; Hirao, 2020).

Most previous studies on the effects of BID programmes are also methodologically limited, being solely descriptive or including mostly anecdotal evidence about the effects on incumbent firms, crime levels and place attractiveness (e.g., Mitchell, 2001; Hogg et al., 2003; Radosavljević et al., 2015). Others have employed before-and-after analysis, trend analysis, or cost–benefit analysis to assess the effectiveness of BID programmes. However, the effects of BIDs cannot be isolated by comparing indicators from before and after the implementation of BID programmes (Reenstra-Bryant, 2010) or from simple comparisons between BIDs and control districts. Propensity score matching has previously been used to identify adequate controls (e.g., Sutton, 2014), but this approach has been criticized because many important characteristics of BID programmes are often unknown and unobserved by the researcher (Greenstone et al., 2010). Furthermore, many analyses of the effects of BIDs (e.g., MacDonald et al., 2013; Clutter et al., 2019; Piza et al., 2019) have been conducted at a highly aggregated level (e.g., market, town, census tract, municipality, county, state).

We contribute to the literature by analysing the impact of a BID programme implemented in 2015 in five Swedish cities on firm performance, urban safety, and place attractiveness. In contrast to most other countries, where tax contributions from business and property owners are compulsory for BID programmes, contributions in Sweden are voluntary, and the implementation process is not regulated in any way by law. This gives businesses and property owners the choice to be either yay- or nay-sayers. This opens, on the one hand, the possibility to opt out or free-ride, which means that the potential of these BID programmes is not fully realized (Ward, 2006; Reeve, 2007), thus generating legitimate questions about their

effectiveness. On the other hand, voluntary behaviour is often argued to be more effective than regulatory action because it is the result of a predisposition to contribute to a cooperative endeavour, contingent upon the cooperation of others. Contrary to common expectations, voluntary behaviour is more effective than regulatory action when it is grounded in intrinsic rewards and on a common understanding that the desired effects can be obtained through collective action (Ostrom et al., 1994). As Ostrom (2000) argues, the introduction of regulatory legislation leads, in these situations, to the ‘crowding out’ of voluntary cooperation (and thus to reduced effectiveness) because the focus on extrinsic motivation (i.e., the avoidance of punishment) destroys the cooperative/reciprocal nature of the task. Our study can thus be seen as the first attempt to evaluate the effects of BID programmes that are voluntary, thereby providing evidence on whether voluntary BID programmes are sufficient for improving firm performance, reducing crime and raising the attractiveness of the designated geographical area.

By using a difference-in-differences regression model that includes both spatial and time fixed effects, we ensure that our identification of the effects of BID programmes are more unbiased than those of most previous studies. We find that the implementation of the voluntary BID programme in Sweden increased the labour productivity of incumbent firms located within the BID by, on average, 7.62%. This effect is mostly due to an increase in firm revenues (11.65%), while we observe small changes in the number of employees. The effects on firm performance are, however, restricted in both time and space. The estimated effect of the BID programme on revenues is positive and highly significant shortly after the implementation of the programme (12.40% in 2015 and 13.74% in 2016), but decreases to 5.92% in 2017 (significant at the 10% level) and to zero in 2018. The effect of the BID programme on the number of employees is statistically significant only in the first year after the BID programme was implemented. We find no effects on firm performance outside the geographical boundaries of the BIDs. Hence, the voluntary BID programme was able to improve the performance of firms located within the BIDs, but the effects were only transitory.

Furthermore, we find few significant differences in crime levels between the BIDs and the control districts. The difference is, on average, negative for all three years (2016, 2017 and 2018) but statistically significant only in 2018 (-4.94%). Crime levels are also higher just outside the BIDs than in the control cities for all three years, with a statistically significant difference in the year after the BID programme was implemented (+3.43%). Finally, we find no statistically significant effects of the BID programme on property values within or outside the designated BIDs.

The rest of the study is organized as follows: in section 2, we discuss the theoretical arguments for the causes of urban decay and the mechanisms behind urban regeneration; in section 3, we provide a panoramic view of previous studies in the field; in section 4, we introduce the data and the research method; and in section 5, we present the results. In the last section, we summarize and discuss the findings of our study.

2 The role of BIDs in place attractiveness – a theoretical argument

BID programmes emerged from the view that local governments have failed to provide an appropriate level of public services, such as cleaning and sanitation, maintenance for public spaces, and security, in certain neighbourhoods (Jansen, 2017). It is believed that if not corrected, this failure may lead to a spiral of negative consequences for the affected areas, which may lead customers to avoid these districts.

Blighted neighbourhoods with low footfall send a signal that people do not care, which reduces resident commitment to participation in the informal surveillance of a neighbourhood and causes the environment to appear ‘uncontrolled and uncontrollable’. According to broken windows theory, introduced by Wilson and Kelling (1982), the probability of detecting and apprehending crime in such areas is low, and offenders feel less bound by social norms (Keizer et al., 2008). This kind of development decreases the attractiveness of a place (Cullen and Levitt, 1999; Brooks, 2008), thereby contributing to a potential contraction in property values.

BIDs have been used as a tool to break this negative spiral and trigger the revitalization of blighted areas. The expectation that BIDs would augment the performance of firms located within their boundaries is derived from both gravity models and random utility theory (Reilly, 1931; Christaller, 1933; Huff, 1962). Regional gravity models imply that an increase in the size and attractiveness of a district may result in an increase in its market reach and customer base. BIDs include measures to augment the safety and aesthetics of retail areas (e.g., improvements in the physical form of retail areas), create new meeting places (e.g., parks or playgrounds), support retail-related events (e.g., markets and showrooms), and widen the range of services provided in these districts (e.g., provide free WiFi in public spaces). Thus, BIDs offer strategies for improving the perceived attractiveness of the designated BIDs (Sutton, 2014) in the hope of increasing their customer range and thus, eventually, their customer base.

According to retail theory, the attractiveness of a district is a critical determinant of its customer base (Brown, 1993; Teller and Reutterer, 2008). More customers imply more sales and thus

increased revenues for firms located within an area. The increased customer base might also affect neighbouring firms through synergetic effects from co-location (Ghosh and McLafferty, 1987; Baker, 1996; Arentze and Timmermans, 2001; Arentze et al., 2005). Oppewal and Halyoake (2004) explain that this relationship is based on the potential for firm agglomerations to provide additional utilitarian and hedonic shopping values to the consumer by enabling both comparison and multipurpose shopping. Comparison and multipurpose shopping occur when the products sold are either imperfect substitutes or complements and generate so-called demand-side externalities (i.e., customer spillovers) for firms located within the same area. In other words, the revenues of one firm are positively affected by the location and revenues of other firms selling substitute or complement goods (Brown, 1989; van Handel, 1970; Wolinsky, 1983). Firm co-location may also reinforce supply-side externalities (Marshall, 1890), which reduces the costs of moving goods, people, and ideas between firms within the area (Ellison, 2010).

The combination of an increase in revenues due to demand-side linkages and a decrease in input costs due to supply-side linkages may lead to an increase in productivity for firms located within the BID area. However, firms within BID areas also compete for consumers' disposable income, with potentially negative effects on some of the incumbent firms' revenues and productivity (Saito and Gopinath, 2009). To define this dual relationship, Brandenburger and Nalebuff (1996) talk about 'co-opetition': while firms within BID areas cooperate to obtain demand- and supply-side advantages from an increased market, they also compete for a share of the customers' wallets and time (Teller and Reutterer, 2008). To minimize the negative effects of competition, any intensification in economic agglomerations should coincide with a commensurate expansion in the customer base.

In addition to the provision of increased utilitarian and hedonic shopping values, an essential contributor to the attractiveness of a district is a high level of perceived safety. By including measures meant to repair the signs of urban disorder, as well as improvements in security (e.g., 'parent walks', more security guards or police in the streets) and redesigning the architecture of public spaces (e.g., improved street lightning), BID programmes are expected to make neighbourhoods less attractive for motivated offenders (Hanish and Guerra, 2000) and have the potential to lead to a decrease in the level of urban crime, which in turn would make the BID area more attractive for customers to visit.

A different view, however, is that the higher density of customers caused by the implementation of a successful BID programme also increases the number of 'desirable targets' and,

consequently, the incidence of crime (Cohen and Felson, 1979). Disorder and crime are nevertheless largely conditional on the level of collective efficacy in a neighbourhood, that is, on the ability of the community members to control the behaviour of individuals or groups in their community (Sampson and Raudenbush, 1999). Routine activities theory explains that crime and victimization result from the convergence in time and place of a motivated offender, a desirable target, and the absence of an effective guardian (Cohen and Felson, 1979). The probability of the offender meeting the target in an area lacking social control is high in the blighted neighbourhoods that people tend to avoid but diminishes when the area becomes more attractive to customers.

If there are amenities in a particular area that deliver utility from living there, then, as explained by the theory of spatial equilibrium within the city and the Alonso-Muth-Mills model of location, property prices will rise to offset this higher level of utility (Glaeser, 2008). Measures included in the Swedish BID programme, such as the physical beautification of neighbourhoods or the equipping of urban areas with parks and playgrounds, are regarded as amenities that have the potential to make a location more attractive (Hanson, 2017). These amenities may contribute to consumer utility and thus have the potential to spill over into the prices of residential properties.

A significant body of research has debated the attenuation of both agglomeration and competition effects, in line with the classic argument of central place theory (Christaller, 1933; Lösch, 1954). These studies have explained that regardless of scale, agglomeration and competition effects are the strongest in market centres and decrease with distance from these central points (e.g., Ellickson and Grieco, 2013; Larsson, 2014; Andersson et al., 2016; Rosenthal and Strange, 2019; Arcidiacono et al., 2020). BID programmes meant to increase the attractiveness of the designated area thus might not only have direct effects within that area's boundaries but might also lead to spillover effects in neighbouring districts. The question is how strong these impacts on the adjacent areas, if present, are and whether they are in the same direction as any effects that may be detected within the BIDs.

3 Previous studies on BIDs and BID effectiveness

Researchers have begun to show interest in analysing the functional mechanisms within BID programmes and their effectiveness after the turn of the 21st century. Although this body of literature is quite vast, early studies have mostly focused on defining BIDs, describing the BID implementation process and proposed measures, and intuitively debating the potential benefits

and concerns related to BID implementation. For instance, Mitchell (2001) focuses on defining BIDs and providing an account of the types of services provided in BIDs throughout the United States. Morçöl and Wolf (2010) explain the dual public–private nature of BIDs in the United States and discuss the accountability and management challenges that arise from this organizational form. Michel and Stein (2015) compare different BID programmes in Hamburg, Germany, and contrast them with experiences from the United States and the United Kingdom.

Many studies have tried to identify the advantages of implementing BID programmes without succeeding in measuring them (or even attempting to)¹. For instance, Hogg et al. (2003) argue that small and medium-sized retailers could enhance their competitive position following improvements in the physical environment (e.g., new street furniture and tree planting), in the security of the area (e.g., installing surveillance cameras), in bus services, and in local training and employment schemes. They also maintain that involvement in BID programmes could create stronger economic links between businesses in the targeted area but provide no empirical evidence in support of these hypotheses.

Another segment of the previous literature has attempted to measure the effects of BIDs on the urban form and economy. Several studies have employed before-and-after or trend analysis, cost–benefit analysis, or simple comparisons between BIDs and non-BIDs to assess the effectiveness of BID programmes. These methodological approaches, however, fail to differentiate the effect of BIDs from the effects of other extraneous variables, simultaneous events, or local and regional economic trends (Mitchell, 2001; Caruso and Weber, 2006; Reenstra-Bryant, 2010). Causality cannot be implied solely from comparisons of the levels of various indicators before and after the implementation of BID programmes (Reenstra-Bryant, 2010). Furthermore, such evaluation methods may be inadequate, as nonfinancial costs and benefits, such as aesthetic improvements, are difficult to quantify. These studies are, however, valuable for reviewing the wide palette of indicators that could be used to assess BID success, such as customer flows, firm revenue and employment, the mix of stores and services, average rents and property values/prices, vacancy rates, and crime levels in the BIDs and their surrounding areas (Reenstra-Bryant, 2010; Radosavljević et al., 2015).

A few studies have used more robust quantitative methodologies to distinguish the impact of BID programmes from all other noise. These are summarized in Table 1 and are categorized

¹ See, for example, Pitkeathley, 2017; Lee, 2018; and Port, 2018.

based on whether they investigate the impact of BIDs on (i) firm performance, (ii) crime levels, or (iii) place attractiveness.

We can conclude from Table 1 that very few previous studies have attempted to robustly isolate the effects of BID programmes on firm performance. One rare exception is Sutton (2014), who investigates the impact of BIDs established between 2002 and 2008 in New York City on the economic performance of incumbent retailers. She finds that the BID programmes had no significant impact on retailers' revenues or on the number of their employees compared to those of retailers in statistically comparable non-BIDs in the city. However, she finds a decline in revenues (of -64%) and number of employees (of -58%) of incumbent firms located in small community BIDs that are often characterized by a narrow scope of services and a weak economic environment. Her results indicate that the effects in larger BIDs are positive, though only modestly significant for revenues and not statistically significant for employment. The author argues that the negative effects in small BIDs are due to stronger competition effects, while the slightly positive effects in larger BIDs are due to urbanization economies.

As is true of many other of the studies listed in Table 1, Sutton's (2014) research is focused on the effects of BIDs in New York City, which are often larger and benefit from resource levels surpassing those of many other BIDs established in the United States or internationally. These BID programmes are also legally binding in character, which contrasts with the BID model implemented in Sweden. New York City is also a geographic area with distinct features – it is the most populous city in the United States and the world's leading financial centre – and extensive access to private financing. This makes any attempt to generalize results from New York City outside its geographical scope unreliable. Methodologically, Sutton (2014) restricts her analysis to retail firms and builds her discussion on the effects generated from the increase in independent retailers in the BIDs. To tackle the endogeneity issues related to the non-random establishment of BIDs, she uses propensity score matching to find adequate control establishments. However, as pointed out by Greenstone et al. (2010), such a strategy implies that the adoption of BID programmes can be correctly modelled by observable characteristics, while in most cases, many important characteristics are generally unknown and unobserved by the researcher.

Research measuring the effects of BIDs on crime levels has also focused exclusively on urban spaces located within the boundaries of the United States, but the results are far from unanimous. Several studies have confirmed the expected negative effects. MacDonald et al. (2010), for example, find a decrease of 12% in the incidence of robberies and of 8% in the

incidence of violent crimes following the implementation of BID programmes in Los Angeles, CA. Based on a sample of 4 327 police departments spread across the entire United States, Mello (2018) finds that hiring one more police officer decreases violent crime by 1.3% and property crime by 0.8% and results in 1.9 fewer robberies, 5.1 fewer auto thefts, and 0.11 fewer murders in BID cities. This result is confirmed by Piza et al. (2019), who find that opening a police station in a BID leads to a decrease in burglary and motor vehicle theft in that district.

However, Clutter et al. (2019) find that the expected robberies per foot of street length increases by 59% in BIDs, in line with arguments that a higher density of firms and customers in an area may mean an increase in ‘desirable targets’ and thus in the incidence of crime (Cohen and Felson, 1979). Han et al. (2017) pinpoint that such effects may be heterogeneous over time, as they record stronger negative impacts (in absolute terms) directly after the point of implementation.

The estimated changes in property prices or values due to BID programmes are not undisputedly positive either, as one may expect. Ellen et al. (2007), for example, use a difference-in-differences approach with a hedonic price model to estimate the effects of 44 BIDs in New York City over a long period of time (1974–2003), finding that commercial property values within the BIDs appreciated by 15.7%–31.2%, while residential values appreciated by 12.4%, following the implementation of the BID programmes. They also find that only large BIDs have a significant impact, a result confirmed by other studies (Gross, 2005; Furman Center, 2007). The positive effects of BIDs are also identified by Jansen (2017), who estimates increases of £68 000–£105,000² for properties located both within the BIDs and in adjacent neighbourhoods.

However, Hanson (2017) finds that the price of properties located either within BIDs or within 500 m of their boundaries decreased by 17.18%. Using a difference-in-differences specification, Hirao (2020) also finds negative effects of 10%–42% for the value of residential properties after the implementation of BID programmes in Westminster, UK. Although these studies are generally more robust from a methodological standpoint, the results are far from convergent and lack external validity, as they also focus mostly on very large cities (e.g., New York, Los Angeles, Washington, D.C.). Some of the authors themselves acknowledge the limitations of their study – e.g., Miller (2013), who finds that the value of homes in BIDs appreciates more than that of homes in neighbouring districts but that this result is not robust to the inclusion of

² Approximately 76 000–118 000 EUR, exchange rate £1 = 1.12 EUR, 10 November 2020.

price trends. Furthermore, the use of aggregated units of analysis (as in, e.g., Bowen et al., 2019) does not allow for the proper identification of changes in the average property value.

Several studies have indicated that the impacts of BID programmes are concentrated within the BIDs, suggesting that there are no spillover effects outside the boundaries of the BIDs. Mello (2018), for example, finds that hiring one more police officer in a BID city results in little change in neighbouring cities. However, other studies have indicated that the effects of BID programmes do extend beyond the boundaries of the BID. Clutter et al. (2019), for example, find that the increase in expected robberies per foot of street length extends to neighbouring areas, although the effect exhibits a sharp distance decay of 3.5% per street block outside the boundaries of the BIDs. Crime thus seems to be ‘pushed’ into adjacent neighbourhoods, although it increases the most, contrary to common intuition, within the treated areas. In the case of property values, several studies (e.g., Ellen et al., 2007; Furman Center, 2007) have pinpointed the lack of spillovers or the presence of sharp distance-decaying effects (Miller, 2013; Hanson, 2017). Other studies have indicated that there are spillover effects on property values – both those that are in the same direction as the effects within the BIDs (Jansen, 2017) and those that are in the opposite direction after ‘crossing’ over the BID borders (Hirao, 2020).

To the best of our knowledge, no study has investigated the effects of voluntary BID programmes, such as those implemented in Sweden. Two earlier studies (Edlund and Westin, 2009, and Edholm, 2013) investigate possible alternatives to the implementation of BID programmes in Sweden. Edlund and Westin (2009) debate the Swedish legislative framework that would potentially steer the BID process, discussing the necessary changes in legislation needed for such schemes to be effectively implemented and juxtaposing BIDs with voluntary town centre cooperation alternatives. Edholm (2013) discusses the current Swedish legislation that could prevent BID programmes from working effectively and acknowledges that successful cooperation based on voluntary participation would require evidence that the measures included in the BID programmes benefit both the district as a whole and each of its members³.

³ More recently, Valli and Hammami (2020) have provided some anecdotal evidence on improved attractiveness, increased sense of security, and higher estate values in the BID Gamlestaden in Gothenburg, only to argue that these possible positive outcomes occur at the expense of socioeconomically vulnerable groups in the BID and thus negatively affect urban social justice based on democracy, diversity, and equality.

Table 1. Review of studies using quantitative methodologies to study the effects of BID

Study	Level of analysis	Geographical area	Treatment	Dependent variable	Data/period	Method	Summary of main results relevant to our study
Firm performance							
Sutton, 2014	Census tract	New York, NY, USA	N.A.	Retail revenues Retail employment	Panel 2000–2008	DiD with spatial and time FE	BID programmes in general have no impact on the revenues and employment of firms in the BIDs. The effects seem to differ, however, with the size and retail structure of the BIDs: a decline in revenues (-64%) and employment (-58%) among local firms is found for small community BIDs, while the effects in larger BIDs are positive but only marginally significant for revenues.
Crime							
MacDonald et al., 2010	Individual	Los Angeles, CA, USA	30 BIDs	Robberies Violent crime	Panel 1994–2005	Bayesian hierarchical model Spatial RE	The implementation of BID programmes leads to a 12% reduction in the incidence of robbery and an 8% reduction in the incidence of violent crime in the BIDs.
MacDonald et al., 2013	Individual Census tract	Los Angeles, CA, USA	30 BIDs	The odds of committing violent crime among adolescents	Panel	Multivariate regression, neighbourhood and interview-wave RE Interviews	There are no statistically significant effects of BID programmes on the odds of committing a violent crime among adolescents living within the BID boundaries, nor among those living in the surrounding neighbourhoods. National origin, immigration status, parental social control, and neighbourhood collective efficacy are strongly correlated with violent victimization.
Han et al., 2017		Philadelphia, PA, USA	N.A.	Number of nuisance crimes	Panel 1998–2009	System GMM estimation model with spatial and time FE	Negative effects are recorded during the first five years after the establishment of the BIDs. No significant effects are recorded after five years.
Mello, 2018	City	USA	4 327 police departments	Crimes/10 000 residents	Panel 2004–2014	DiD with spatial and time FE and city trends	Hiring 1 more police officer decreases violent crime by 1.3% and property crime by 0.8% and results in 1.9 fewer robberies, 5.1 fewer auto thefts, and 0.11 fewer murders. Little change is found in neighbouring cities.

							The impacts are largest in cities that had worse macroeconomic conditions during the Great Recession.
Clutter et al., 2019	Street block	Cincinnati, OH, USA	N.A.	Robberies/foot of street length	N.A.	Negative binomial regression	Expected robberies per foot of street length increase by 59% in the BIDs. This effect decreases by 3.5% per additional street block away from each BID.
Piza et al., 2019	Street segment Street intersection	Newark, NJ, USA	1 police station within a BID	N.A.	Panel 2012–2018	DiD	Opening a police station as part of a BID-programme leads to a decrease in the incidence of burglary and motor vehicle theft within the boundaries of the BID. However, the spatial displacement of robbery and theft from vehicles was also identified.
Property values							
Ellen et al., 2007	Property	New York, NY, USA	44 BIDs	Commercial property values Residential property values	Panel 1974–2003	DiD with hedonic price model, no spatial or time controls	Commercial property values within the BIDs increase by 15.7%–31.2% following the implementation of BID programmes; however, there is a significant impact among large BIDs only. Residential property values within the BIDs increase by 12.4% following the implementation of the BID programmes. The effects on properties located in areas adjacent to the BIDs are not significant.
Furman Center, 2007	Property	New York, NY, USA	44 BIDs	Commercial property values Residential property values	Panel	DiD with spatial controls	The value of commercial properties within the BID boundaries increase by +15% compared to that of other properties in the neighbourhood. Large BIDs have large positive effects on commercial property values, but smaller BIDs have no impact. The formation of a BID has little impact on the value of residential properties. There is no evidence of spillovers to commercial properties located just outside the BID's boundaries.
Brooks and Strange, 2011	Property	CA, USA	38 BIDs	Property values	Panel 1980–2005	DiD with spatial and time FE and trends	The properties of BID supporters experience larger post-BID changes than the properties of opponents. Small property owners are less supportive of BID formation than large property owners.
Miller, 2013	Property	Washington, D.C., USA	N.A.	Residential property values	Panel	DiD with time FE and trends	The values of homes within BID boundaries appreciate by 5.75%–10.5% more than the values of homes in the rest of the area, but the result is not robust to local price trends.

							Homes closer to BID boundaries appreciate more than those located further from the boundaries.
Jansen, 2017	Property	Liverpool, UK	2 BIDs	Commercial property values	Cross-section 2005, 2010, 2016	Hedonic regression	Increases of £68 000–£105 000 are found for properties located both within the BIDs and in areas adjacent to them. However, the author acknowledges that this method cannot accurately isolate the extent to which these effects are due to the BID programmes themselves.
Hanson, 2017	Property	Washington, D.C., USA	8 BIDs	Residential property prices	Panel 1997–2016	DiD with hedonic price model, spatial and time FE	The prices of properties located within the BIDs or within 500 m of their boundaries decrease by 17.18%. A marginal effect was also estimated and implies a nearly constant -2.05% decrease in the property's sale price for every additional 50 metres from the BID boundaries. Furthermore, the effects are significant in only 2 of the 8 analysed BIDs.
Bowen et al., 2019	Census tract	Denver, CO; Los Angeles, CA; Portland, OR; San Francisco, CA, USA	2 536 census tracts	Median rent Housing production	Panel 2010–2017	DiD	Increases of 1% per year in the median rent and of 0.1% per year in housing production are found in census tracts with BIDs. The effects in older BIDs are stronger than those in newer BIDs. No effect on the racial composition of the census tract was detected.
Hirao, 2020	Property	Westminster, UK	8 BIDs	Residential property values	Panel 2005–2015	DiD with hedonic price model, no spatial or time FE	Negative and significant effects (of 10%–42%) on the value of residential properties located within BID boundaries were identified. Positive and significant effects (of 3.6%–5.5%) on the value of residential properties located in the boundary areas were also found.
Other							
Gross, 2005	Census tract	New York, NY, USA	41 BIDs	BID functions	1999–2003	ANOVA Interviews Participant observations	The size of the resource base, the type of property, the balance of power among stakeholders, and the wealth of the community where the BID programmes are implemented influence BID functioning. Large corporate BIDs are more likely to be effective, as they have the resources to act beyond the physical maintenance of the urban environment and to finance capital improvements in the BID.

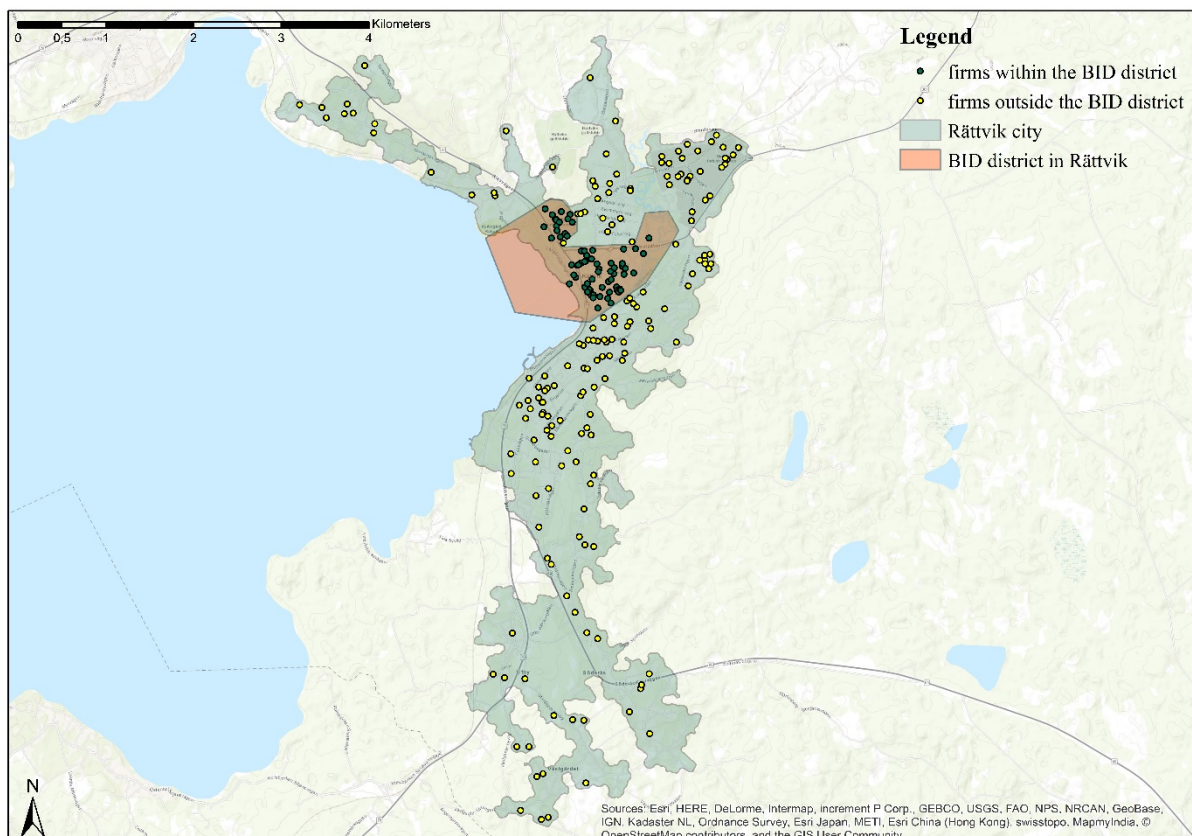
Meltzer, 2011	Subborough area	New York, NY, USA	N.A.	Local governmental service provision	N.A.	DiD	<p>A 1% increase in BID size leads to a decrease of -0.003% in public sanitation spending and of -0.003% in police spending. The BIDs also lead to a decrease of -4.5% in the annual number of police vehicles assigned to the district.</p> <p>After instrumenting for BID presence, any significant effects disappear.</p> <p>No spillover effects are identified.</p>
Meltzer, 2012	Property	New York, NY, USA	N.A.	The likelihood of a neighbourhood forming a BID	N.A.	Survival analysis	<p>BIDs are more likely to form in places where there is more commercial space over which the BID benefits can be capitalized and where there is homogeneity in service and spending preferences across properties.</p> <p>BIDs are more likely to form in neighbourhoods that possess signs of appreciation and growth.</p>

4 Empirical analysis

4.1 Selecting control group cities

We investigate the impact of a voluntary Swedish BID programme, which was implemented in five cities (Filipstad, Ludvika, Orsa, Rättvik, and Torsby; henceforth called ‘treated’ cities) in 2015 under the supervision of the Swedish Association of Town Centers, on firm performance, place attractiveness, and urban safety. Each of the five cities is a small monocentric city (with a population between 5 000 and 15 000 inhabitants); their economies are based on natural resources and focus mainly on the production of electrical power and electronics and on tourism. Although there are some differences in the revitalization measures adopted by these cities, they all follow the Swedish Association of Town Centers’ so-called ‘Swedish BID model’ with the same steps and timeline for implementation. The programme focuses on enhanced security, the removal of graffiti, street cleaning and sanitation, improvements in lighting, place-making (e.g., façade beautification, the maintenance of public sidewalks, streetscaping, and landscaping), the construction of new meeting spaces, and the facilitation of communication and cooperation between public and private stakeholders. The geography of the BID and the possible spillover areas is illustrated in Figure 1 for one of the treated cities, Rättvik.

Figure 1. Rättvik BID and its spillover area, with the location of incumbent firms.



Unbiased estimation of the impact of the BID programme would be possible if the use of randomized controlled experiments was feasible. However, randomization is not possible because the BID programme is adopted voluntarily by the district stakeholders (Sutton, 2014). Furthermore, the treated cities have typically experienced both physical and economic distress and are therefore likely to differ from randomly selected cities. We instead view the implementation of the BID programme as a natural experiment and investigate its effects on firm performance, crime levels, and place attractiveness in the treated cities. In an ideal setting, we would compare our outcome measures within the BID boundaries after the implementation of the BID programme with the same measures in the counterfactual state of no BID programme having been implemented. This is, of course, impossible since the treated cities cannot be observed in the counterfactual state in which no BID programme was implemented.

Several previous studies have used matching techniques to address the selection bias inherent in the adoption of BID programmes (Brooks, 2008; Brooks and Strage, 2011; Sutton, 2014; Piza et al., 2019). Matching techniques reduce selection bias by identifying counterfactuals based on control groups constructed on the basis of information observed prior to treatment – e.g., firm density, firm revenue, assessed property value, population levels, and/or population density (Dehejia and Wahba, 1999, 2002; Sutton, 2014; Daunfeldt et al., 2017; Hanson and Rohlin, 2018). However, as pointed out by Greenstone et al. (2010), such a strategy implies that the initial choice (i.e., the adoption of a BID programme) can be correctly modelled by the observable characteristics of the cities, even though many important characteristics are generally unknown and unobserved by the researcher. The formation of BIDs is not always a transparent process, and the decision to implement BID programmes is thus not always based on measurable indicators.

To minimize these empirical problems, we use seven cities that adopted the BID programme after the conclusion of our study period as controls for the five treated cities that implemented the same BID programme in 2015. The control cities are Borlänge, Enköping, Grängesberg, Lidköping, Simrishamn, Sollerön and Svärdsjö, each of which implemented the BID programme in either 2019 or 2020. This method has been previously used by, for example, Brooks and Strange (2011)⁴, who choose as controls areas that they refer to as ‘almost BIDs’, i.e., areas that adopt the BID programme at the end of the period chosen for analysis, had their BID programme revoked, or considered adopting the BID programme but never officially

⁴ This method for control group selection has also been used in other studies, e.g., Greenstone and Moretti (2010), Håkansson et al. (2019), and Daunfeldt et al. (2021).

started the process (see also Brooks, 2008). As both treated and control cities were deemed to be in need of revitalization strategies to increase the attractiveness of their central shopping districts within a five-year period, it is highly likely that these cities would be similar in terms of the determinants of BID adoption, which makes them plausibly valid controls.

4.2 Model specification

To obtain unbiased estimates of how the implementation of the BID programme impacted incumbent firms within the treated cities, we estimate a difference-in-differences model. That is, we compare the levels of the outcome variables within and outside the BIDs in the treated cities after treatment with those before treatment and with the levels of the outcome variables in the control cities during the whole period of analysis. Difference-in-differences analysis is one of the most frequently used tools in applied economics research for the evaluation of the effects of public interventions on relevant outcome variables (Abadie, 2005).

A standard difference-in-differences regression model in which we have two treated areas in the BID cities (one inside the BID, TR_{in} , and one outside, TR_{out} , which may possibly experience spillover effects) can be written:

$$\ln Y_{it} = \beta_0 + \beta_1 \times TR_{in} + \beta_2 \times TR_{out} + \beta_3 \times TP_t + \beta_4 \times (TR_{in} \times TP_t) + \beta_5 \times (TR_{out} \times TP_t) + \varepsilon_{it}. \quad (1)$$

where TR_{in} is an indicator equal to one for all observations within the BID in the treated cities, TR_{out} is an indicator variable equal to one for all observations located within the treated cities but outside the BID, TP_t is an indicator variable equal to one for the years after the introduction of the BIDs, and $(TR_{in} \times TP_t)$ is thus equal to one for observations located inside the BID in the treated cities in the years after the introduction of the BID programme, while $(TR_{out} \times TP_t)$ has a similar interpretation for observations outside the BID in the treated cities. The parameter estimates β_3 and β_4 thus measure the impact of the introduction of the BID programme for observations located within and outside the BID, respectively. Finally, ε_{it} is a random disturbance term assumed to have zero mean and constant variance.

A potential drawback of the specification in equation (1) is that it controls for heterogeneity only at the treatment group level via the TR_{in} and TR_{out} indicator variables and thus ignores the potential heterogeneity at lower levels. Recent applications of difference-in-differences analysis (e.g., Ailawadi et al., 2010; Gibbons et al., 2019; Arcidiacono et al., 2020) have often controlled for heterogeneity at the level of the observational units under study (i.e., firms, real

estate properties, etc.). We follow this approach when data are available⁵, and our preferred regression model can thus be written as follows:

$$\ln Y_{it} = \beta_0 + \beta_i + \beta_t + \beta_4 \times (TR_{in} \times TP_t) + \beta_5 \times (TR_{out} \times TP_t) + \varepsilon_{it}, (2)$$

where Y_{it} is the outcome variable in our analysis (i.e., labour productivity, real revenues, number of employees, number of crimes, real estate values), β_i is an observational-unit fixed effect, and β_t is a year-specific fixed effect. The terms $(TR_{out} \times TP_t)$, $(TR_{out} \times TP_t)$, and ε_{it} have the same interpretation as above. Note also that the log transformation of the outcome variable ($\ln Y_{it}$) has the benefit of making the parameter estimates related to the effect of the BID programme interpretable in percentage terms after using the formula $100 \times [\exp(\beta) - 1]$ (Wooldridge, 2010).

Finally, the effect of BID programmes is not necessarily linear over time, and to test the hypothesis of nonlinear effects, we also estimate our model using the following specification:

$$\ln Y_{it} = \beta_0 + \beta_i + \beta_t + \sum_{k=1}^4 \beta_k \times (TR_{in} \times \sum_{t=2015}^{2018} D_{YEAR_t}) + \sum_{l=1}^4 \beta_l \times (TR_{out} \times \sum_{t=2015}^{2018} D_{YEAR_t}) + \varepsilon_{it}, (3)$$

where D_{YEAR_t} refers to indicator variables for observations in the years 2015, 2016, 2017 and 2018. As indicated by $\sum_{k=1}^4 \beta_k$ and $\sum_{l=1}^4 \beta_l$, we thus estimate one effect parameter for each of the years 2015 to 2018 regarding both those observational units located within the BID and those located outside the BID but within the treated cities.

4.3 Data and descriptive statistics

Our main goal is to investigate how the implementation of the voluntary Swedish BID programme affected the performance of incumbent firms located in treated cities. Following Özçelik (2020), we measure changes in firm performance in terms of labour productivity and decompose this measure into changes in real revenues and changes in the number of employees. These variables are available to us because all limited liability firms in Sweden are required to report such information annually to the Swedish Companies Registration Office (CRO). We use a database compiled by Bisnode, which has gathered this information from the CRO. The dataset includes, among other variables, revenues, the number of employees, location, and

⁵ Data and the deviations from our preferred model due to data availability are discussed in section 4.3 below.

industry classification.⁶ The panel structure of the data enables the estimation of our preferred models as described in equations (2) and (3).

We also study how the implementation of the BID programme has affected urban safety as measured by the number of crime reports for 250 different offences under Swedish law. Data on the number of reported offences for the years 2016–2018 were provided by the Swedish Police for both the treated cities and the control cities. The first available year for the number of crimes is thus 2016, which implies that we are not able to use a difference-in-differences model in this case. Instead, we use a model similar to the one described in equation (1), with the addition of a crime-category fixed effect. This approach allows us to compare the number of crimes committed within the BIDs and in the potential spillover areas in the treated cities to crime rates in the control cities while controlling for possible heterogeneity in the types of crimes. Since data are available only for the period after the establishment of the BIDs, the comparison in this case is a straightforward analysis of cross-sectional differences in the number of crimes in the different areas (BID and spillover areas versus control cities), and the results should therefore not be interpreted as causal effects of the implementation of the BID programme.

We are also interested in how the introduction of BIDs affects place attractiveness, measured as the real estate valuations assessed for all Swedish properties for taxation purposes.⁷ Ideally, we would follow individual properties over time using market values, i.e., property sale prices. However, the BIDs (in fact, the entire cities in which the BIDs are located) are quite small, and the number of yearly sales is too low to analyse statistically; we therefore use data on real estate valuations. These valuations are set by the Swedish Tax Authority and equal 75% of the property's estimated market value two years before the taxation event. In addition to self-reported data from owners on restorations and other actions that could affect the market value of the properties, the tax authority uses data on actual sales in the area, as well as their own valuations, to obtain a final estimate of the property's market value. Assessments of the properties in our analysis took place in 2012 and 2018, implying that we have access, for each property, to only one observation before and one after the introduction of the BID programme.

⁶ As we link firm performance indicators to firm location, we are compelled to restrict our study to firms reporting performance indicators at the establishment level and to exclude multi-establishment firms that report performance indicators only at the headquarters level. However, approximately 91% of all firms in the Bisnode database are single-establishment firms.

⁷ Real estate properties are assessed every three or six years depending on the type of assessment (general, simplified, or special) and property (single-family residential owned, single-family residential rental, condominium, agricultural, or industrial).

This lack of observations makes the use of our preferred model difficult, and we instead rely on the standard model presented in equation (1) but with the addition of municipality-specific fixed effects to account for possible heterogeneity in local real estate markets.

Descriptive statistics for the five outcome variables at the beginning and the end of our study period (i.e., in 2012 and 2018) are presented in Table 2. The data show that average labour productivity increased by 18.26% for firms located within the BIDs, while there was a small decrease in labour productivity (-2.54%) for firms located outside the BIDs. This decrease in labour productivity outside the BIDs is associated with a small increase in revenues (+4.80%) combined with a simultaneous increase in the number of employees (+2.57%). The increase in labour productivity within the BIDs is related to an increase in revenues of 15.75% combined with a small reduction in the number of employees (-0.30%). This can be compared with an average increase in labour productivity of 6.72% for incumbent firms located in the control cities, which is due to a 9.32% increase in revenues combined with a much smaller increase in employment (+2.22%). We thus observe comparatively strong development within the BIDs along with weaker development outside them, which is a first indication that firm performance seems to have improved in the BIDs after the introduction of the BID programme.

Turning to the changes in real estate values, we see that those values increase more in BIDs (+25.77%) than in the areas outside the BIDs in the treated cities (+19.85%). However, the largest increase in property values is found in the control cities, with an increase of 29.66% over a six-year period. Finally, regarding crimes, we have no data for the period before the introduction of the BID programme. However, in 2018, crimes were least common in the areas outside the BIDs in the treated cities, with an average of 1.36 yearly crimes across the 250 crime categories; the average for the BIDs was 1.60 crimes, and for the control cities, it was 2.06 crimes.

The question is, how much of these changes or differences are due to the implementation of the BID programme in the treated cities? Although they provide us with interesting information, these comparisons of the outcome variables between 2012 and 2018 cannot accurately isolate the effect of the BID programmes from the possible effects of other events or of local, regional, or national economic trends.

Table 2. Changes in average labour productivity, real revenues, the number of employees, real estate valuations and the number of crimes in treated cities (both within and outside the BID) and control cities between 2012 and 2018

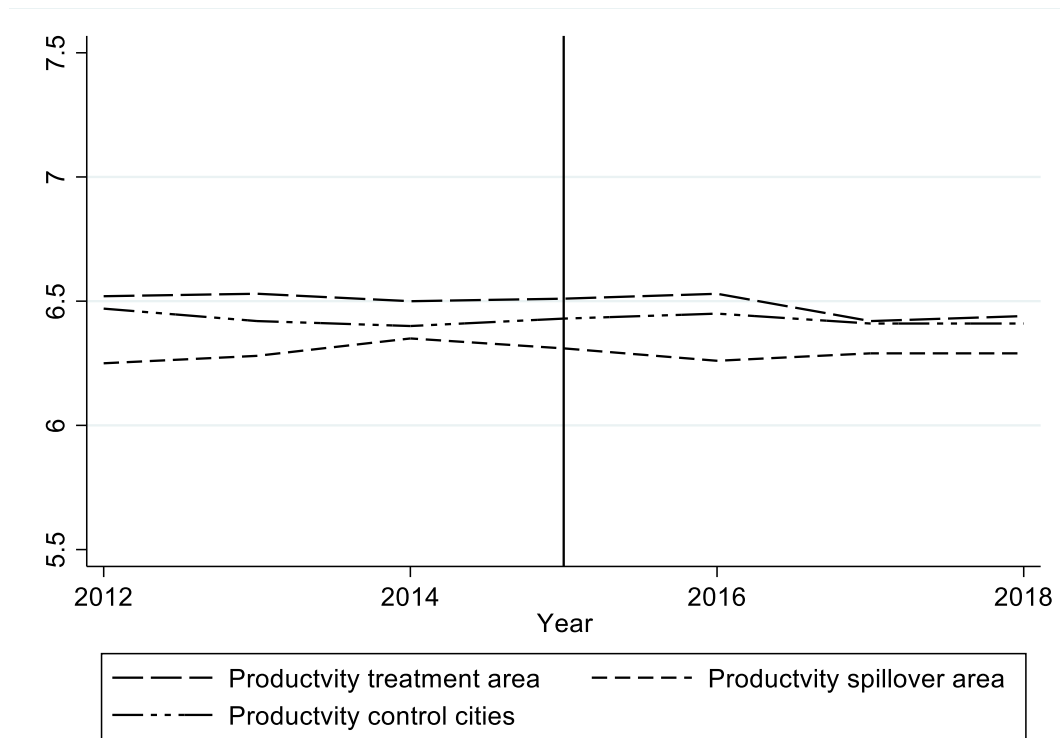
	Treated cities, within BID	Treated cities, outside BID	Control cities
Labour productivity			
2012	1 016.27	1 388.71	1 481.66
2018	1 201.86	1 353.45	1 581.18
Change from 2012–2018 in %	18.26	-2.54	6.72
Real revenues			
2012	10 302.80	7 863.21	9 626.12
2018	11 925.00	8 240.52	10 523 .05
Change from 2012–2018 in %	15.75	4.80	9.32
Number of employees			
2012	6.68	5.28	5.41
2018	6.66	5.42	5.53
Change from 2012–2018 in %	-0.30	2.57	2.22
Real estate valuations			
2012	1 848.63	889.04	2 147.53
2018	2 324.97	1 065.53	2 784.45
Change from 2012–2018 in %	25.77	19.85	29.66
Number of crimes			
2012	N.A.	N.A.	N.A.
2018	1.60	1.36	2.06
Change from 2012–2018 in %	N.A.	N.A.	N.A.

Note: Real revenues are measured in 1 000 SEK, labour productivity is defined as real revenues in 1 000 SEK divided by the number of employees, and real estate valuations are measured as the price per square metre in SEK.

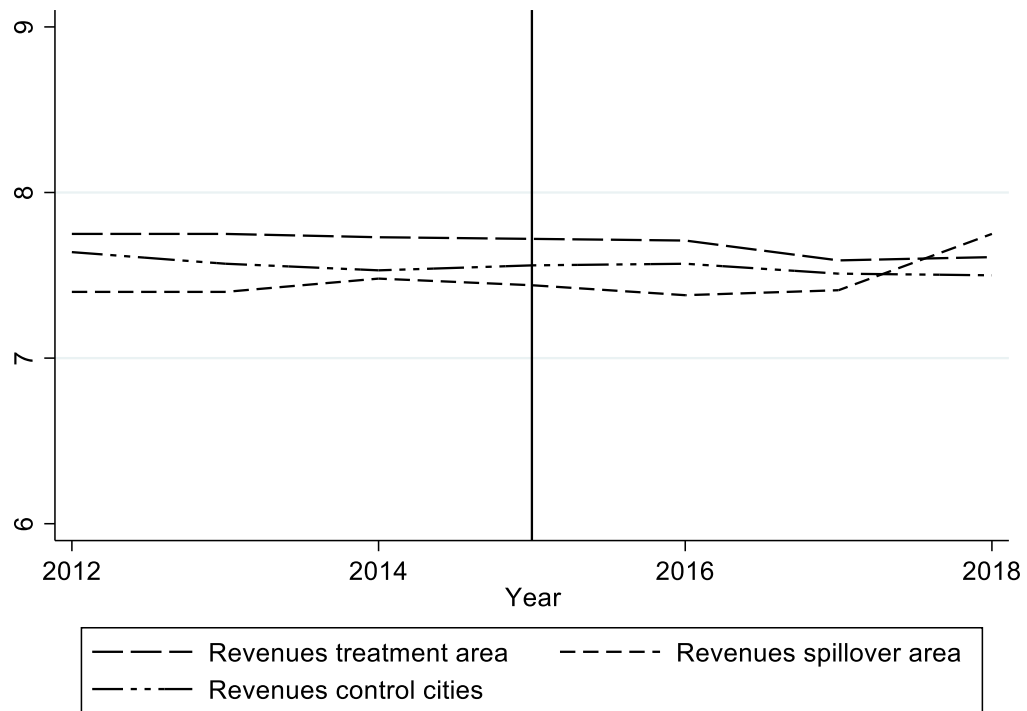
As described in section 4.2, we attempt to isolate the effects of the BID programme from the effects of other factors by means of a difference-in-differences analysis. This methodological approach assumes that the development of our outcome variables in the control cities accurately reflects the development of these variables in the treated cities in the absence of treatment. This is impossible to test, but we follow the standard approach to verifying this assumption: we check that the development of the outcome variables in the control cities is similar to that of the outcome variables in the treated cities in the period before treatment and assume that the trends in the treated cities would have continued unaltered after the treatment point if the BIDs had not been established (Hanson and Rohlin, 2018).

Figure 2 illustrates the pre- and posttreatment trends for the natural logarithm of labour productivity (measured as CPI-adjusted revenues per employee in 1 000 SEK), total CPI-adjusted revenues (in 1 000 SEK), and the number of employees in our treated and control cities. Since theory and previous studies (e.g., Hanson, 2017; Clutter et al., 2019; Hirao, 2020) imply that the introduction of BIDs is often associated with spillover effects into adjacent areas, we define the treated units to be the BID cities as a whole, but we estimate one effect for the BID and another for the spillover areas; the trends in both of these treated areas are then compared to the trends in the chosen control cities.

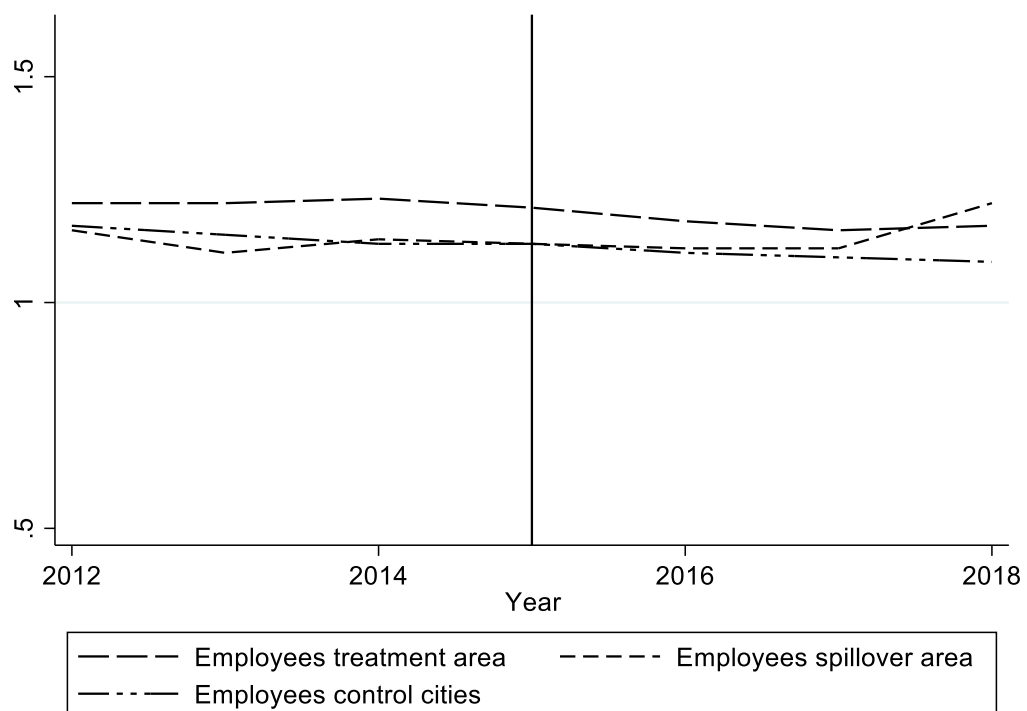
Figure 2a–c. Pre- and posttreatment trends in the BIDs and spillover areas in the treated cities and in the control cities as a whole, 2012-2018.



a) Productivity (ln of the CPI-adjusted revenue per employee in 1 000 SEK, firm averages).



b) Revenues (ln of the CPI-adjusted revenue in 1 000 SEK, firm averages).



c) Number of employees (ln of the number of employees, firm averages).

Figure 2 shows that the measured outcomes were approximately constant during the pretreatment period. This is confirmed by the data in Table A1 in Appendix A, which presents the dependent variables in the firm data regressions year by year together with their associated 95% confidence intervals. The data show no clear trends in any of the outcome variables before treatment, and the confidence intervals for the different areas overlap in most cases. This fact suggests that the outcome variables in the treated and control cities were developing similarly before treatment, indicating that the selected cities are likely to be valid counterfactuals for the treated cities. Moreover, our treated and control cities are located at a considerable geographical distance from each other, which suggests that the introduction of the BIDs affected only the treated cities and that there were no spillovers into the control cities (Hanson and Rohlin, 2018).

5 Estimation results

The results from estimating our most basic model are presented in Table 3 (Model 1), indicating that the implementation of the BID programme significantly increases the labour productivity of firms located within the boundaries of the BIDs by, on average, 7.62%. The increase in labour productivity for firms located in BIDs is due to the considerable increase in real revenues of 11.65%, concurrent with a much lower (but still significant at the 10% level) increase in the number of employees by 3.74%.

The year-by-year estimations show that the effect on productivity was lower in the first year after implementation (an increase of 6.23%, statistically significant at the 10% level) than in the second year (an increase in productivity of 11.04%, significant at the 1% level). Thereafter, the effect on productivity becomes insignificant, which suggests that the BID programme yields mainly transitory effects for a shorter period after it is implemented.

The estimation of the year-by-year treatment effects on real revenues after the implementation of the BID programme (Table 3, Model 2) suggests that the impact is positive and significant immediately after implementation – 12.40% in 2015 and 13.74% in 2016 – but that it decreases to 5.92% in 2017 (still significant at the 10% level). In 2018, however, we no longer find any statistically significant effect of the implementation of the BID programme on firms' real revenues.

Overall, we find no statistically significant effects of BIDs on the number of employees in the treated cities. In the year-by-year estimations, we find that the implementation of the BID programme increased the number of employees in 2015 by 5.64% in the BIDs, which might

indicate that firms temporarily hire additional personnel to implement the changes required by the BID programme.

Turning to the impact of the BID programme on crime, we find that BIDs have 2.66% fewer crimes than the control cities and that this difference is not statistically significant at conventional levels (Model 1 in Table 3). The year-by-year model (Model 2 in Table 3) reveals that the difference is negative for all three years (2016, 2017, and 2018), with a difference of -4.94%, which is statistically significant at the 10% level, in 2018. Note, however, that data on crimes are available only from 2016 onwards, which means that we are not able to perform a difference-in-differences analysis. Because of this, the results should not be interpreted as estimates of the causal effects of the BID programme but rather as an indication of how crime rates in BIDs and the areas adjacent to them differ from the crime rates in the control cities.

Finally, the estimation results regarding the effects of the BID programme on place attractiveness, measured with property values, show no statistically significant effects within the BIDs. Hence, we find no indication that voluntary BID programmes increase place attractiveness.

The results from analysing the impact of BIDs on adjacent areas are reported in Table 4. We find no statistically significant effects for any of our firm performance variables or for property values when estimating our most basic model (Model 1 in Table 4). This is also the case for the year-by-year results (Model 2, Table 4), except for a 3.87% increase in the number of employees in 2017 (significant at the 10% level). However, we find some indications that the number of crimes in the spillover areas is higher than that in our control cities when measuring the average effect over all study years (Model 1, Table 4: 3.43% higher, significant at the 10% level). The year-by-year estimation shows that although more crime is found across all years, the only statistically significant result is during the year when the BID was established (Model 1, Table 4: 3.43% higher, significant at the 5% level).

Last, we find no statistically significant effects on property values outside the BIDs. Thus, apart from crime rates, our results show no signs of spillover effects to adjacent areas in the treated cities. This seems to imply that the impact of the BID programme is mostly restricted to the implementation area, and any positive effects seem to be largely transitory in nature.

Table 3. Effects of the BID programme on labour productivity, real revenues, employment, real estate valuations and crime within the treated BIDs.

		Labour productivity		Real revenues		Employment		Real estate valuations		Crime	
		Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %
Model 1		7.35** (0.044)	7.62%	11.02*** (0.007)	11.65%	3.67* (0.068)	3.74%	-8.91 (0.274)	-8.51%	-2.72 (0.247)	-2.66%
Model 2	Year										
	2015	6.04* (0.079)	6.23%	11.69*** (0.002)	12.40%	5.64*** (0.003)	5.81%	N.A.	N.A.	N.A.	N.A.
	2016	10.47*** (0.005)	11.04%	12.87*** (0.002)	13.74%	2.40 (0.302)	2.43%	N.A.	N.A.	-1.86 (0.463)	-1.83%
	2017	7.20 (0.172)	7.46%	5.75* (0.076)	5.92%	3.00 (0.255)	3.05%	N.A.	N.A.	-1.54 (0.628)	-1.52%
	2018	5.65 (0.328)	5.81%	6.47 (0.163)	6.68%	3.39 (0.216)	3.44%	N.A.	N.A.	-5.07* (0.055)	-4.94%
No. obs.		23 482		23 482		23 482		48 274		38 799	

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. Coefficients scaled by 100.

Table 4. Spillover effects of the BID programme on labour productivity, real revenues, employment, real estate valuations and crime outside the BIDs in the treated cities.

Labour productivity		Real revenues		Employment		Real estate valuations		Crime			
	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	Coefficient (p-value)	Effect in %	
Model 1	-1.00 (0.764)	-0.99%	1.01 (0.799)	1.02%	2.01 (0.218)	2.03%	0.58 (0.878)	0.58%	3.37* (0.064)	3.43%	
Model 2	Year										
	2015	1.02 (0.760)	1.02%	2.04 (0.585)	2.06%	1.02 (0.478)	1.03%	N.A.	N.A.	N.A.	N.A.
	2016	-1.56 (0.681)	1.55%	0.47 (0.917)	0.47%	2.03 (0.273)	2.05%	N.A.	N.A.	3.37** (0.037)	3.43%
	2017	-0.94 (0.831)	0.93%	2.86 (0.575)	2.90%	3.79* (0.062)	3.87%*	N.A.	N.A.	2.93 (0.167)	2.97%
	2018	-2.89 (0.555)	2.85%	-1.59 (0.786)	1.58%	1.31 (0.555)	1.31%	N.A.	N.A.	3.76 (0.123)	3.83%
	No. obs.	23 482		23 482		23 482		48 274		38 799	

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. Coefficients scaled by 100.

6 Conclusions and discussion

BID programmes are increasingly being used to revitalize urban areas that are characterized by economic decline. Evaluations thus far have mainly been focused on compulsory programmes implemented in very large cities. We contribute to the literature by investigating the effects of a voluntary BID programme that was implemented in five Swedish cities in 2015 on firm performance, urban safety, and place attractiveness.

We found that the implementation of the BID programme increased the labour productivity of the incumbent firms located within the boundaries of the BID by 7.62%. This effect was driven by a considerable increase in real revenues, combined with a smaller increase in the number of employees. Our results thus imply that voluntary BID programmes can be effective for improving the performance of incumbent firms within the BID, suggesting that regulatory action might not be needed to achieve the desired effects on firm performance. The positive effects of the BID programme are, however, largely transitory, with significant positive impact on real revenues being observed immediately after the programme was implemented (+12.40% in 2015 and +13.74% in 2016). However, the effects decreased sharply in 2017 and became insignificant in 2018. The estimated effect of the voluntary BID programme on employment is smaller and statistically significant only in 2015. Measured as CPI-adjusted revenues per employee, productivity was found to increase by 6.23% and 11.04% in the first and second years after the BID programme was implemented, respectively. Thereafter, the effect on productivity is insignificant, suggesting that voluntary BIDs yield mainly transitory effects for a short period after their implementation.

There are numerous explanations for why the positive effect of the voluntary BID programme on firm performance seems to be transitory. The novelty of and thus the interest in BID projects might, for example, wear off over time. The decaying employment effect may reflect the (often small) short-term investments that are usually made during the first 18 months of a project when it is implemented under the management of the Swedish Association of Town Centers. The transitory effect might also reflect knowledge spillovers (Marshall, 1890; McCann, 2001; O'Sullivan, 2003), with the control cities adapting and starting to invest in measures aimed at attracting customers and increasing firm performance themselves. Another interpretation is that the voluntary element of the BID programme leads to free-riding behaviour, reducing the effectiveness of the programme. As we are not able to distinguish among these competing

hypotheses, the efficiency of voluntary BID programmes constitutes an interesting avenue for further research.

We furthermore found that the BIDs had 2.66% fewer crimes than the control cities, but this difference is not statistically significant at conventional levels. The model in which the year-by-year differences are studied shows that the differences are negative in all three years (2016, 2017, and 2018) but statistically significant only in 2018 (-4.94%). These results should be interpreted with caution due to the lack of pretreatment data, but it should be noted that previous literature (e.g., Cook and MacDonald, 2011) has also reported a lag between the implementation of BID programmes and reductions in crime rates. This supports the argument that the ‘age’ of the BID programme matters and that more mature BIDs may be more effective than early BIDs. The sign of the effect – although not significant for any of the years in the study period – is also in line with broken windows theory and the results of other previous studies that indicate a negative impact of BIDs on crime levels in the treated districts (MacDonald et al., 2010; Han et al., 2017; Mello, 2018; Piza et al., 2018). These differences may fuel further discussion about whether the smaller effects may possibly be due to the smaller size of the analysed cities, the smaller investments made under the umbrella of this BID programme, or the voluntary nature of the BID programme in Sweden.

Finally, we found no statistically significant effects of the BID programme on place attractiveness as measured by property values. This result is surprising since theoretical studies and the bulk of the empirical research (e.g., Ellen et al., 2017; Miller, 2013; Jansen, 2017; Hanson, 2017) indicate positive effects. There are, however, a series of explanations for these differences in outcomes. First, previous studies have focused on very large cities, e.g., New York, Los Angeles, and Washington, D.C., while we investigated the effect in much smaller cities. Second, the methodological approaches in many of these earlier studies exhibit a series of weaknesses, e.g., they do not control for underlying trends (Miller, 2013). Third, the properties analysed in our study are a mix of commercial and residential properties, and at least for the commercial real estate market, property values are closely connected to revenues. As we find that the introduction of BIDs had only short-term effects on revenues, these effects are likely too small to affect real estate values.

Regarding the impact of BIDs on areas adjacent to them, we find no statistically significant effects on firm performance or property values, with the exception of a 3.87% increase in firm employment in 2017. This is likely not an effect of the implementation of the BID programme two years earlier but rather due to some coincidence that affected the treated cities rather than

the controls at that point in time. Several previous studies have also indicated a lack of spillover effects (e.g., Gross, 2005; Ellen et al., 2007; Jansen, 2017) or the presence of sharp distance-decay effects (Miller, 2013; Hanson, 2017). This phenomenon is theoretically explained by the limited character of BID measures, which target only the BID (i.e., the improvement of façades, street sanitation, improved lighting), combined with the deterrent of travel distances and the possible competition from other areas that offer more/other types of amenities and thus a higher level of utility for the customer.

Turning to crime rates, we found that the level of crime is higher in the spillover areas than in the control cities when measured as the average over all years under study. The year-by-year estimation showed that although a higher level of crime was found for all years, the only statistically significant result was found during the year when the BID programme was implemented (+3.43%). This might indicate that crime is pushed outside the BID boundaries following the implementation of safety and security measures within the BID but also following the increase in the attractiveness of the district, which may draw more customers and thus more ‘eyes on the street’ (Jacobs, 1962). However, as this effect is not coupled with a simultaneous decrease in crime levels within the BID boundaries and because we lack pretreatment data, we advise caution when interpreting these results. It should also be noted that Cook and MacDonald (2010), MacDonald et al. (2013), and Mello (2018) do not identify any statistically significant effects on crime levels outside the BIDs.

Hence, the impact of the voluntary Swedish BID programme seems mostly restricted to the area of implementation, and the positive effects, when present, seem to be largely transitory in nature. We can thus conclude that voluntary BIDs implemented in small cities seem to be effective to an extent. On the one hand, the short-lived impact of these schemes leaves open discussions about the size of the investments, free-riding, and the limited capacity of these cities to work with a tool that requires extensive resources. On the other hand, the Swedish BID model, even if not based in legal measures, may allow for the use of a series of instruments aimed at increasing participation and minimizing free-riding – e.g., the possibility of making ‘in kind’ contributions if financial resources are not available.

Last, our study has some limitations due to data constraints, which need to be mentioned. Having data over a longer period before the implementation of the BID programme would help us apply more robust difference-in-differences models to, e.g., crime levels, while a longer period after the implementation of the BID programme would offer more insights into its long-run effects. Furthermore, the treated cities in our study are quite small. Including more (and

larger) cities in the treatment group would help us investigate whether larger investments generate different impacts and would also increase the external validity of our findings. Moreover, more exact knowledge of the investments made in the BIDs would enable the clearer identification of the relationship between ‘the money in’ and its effects, which could then be used as a meaningful and necessary argument in the policy debate about the future adoption of BID schemes.

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Appendix A. Mean and 95% confidence intervals for the dependent variables in the firm data regressions

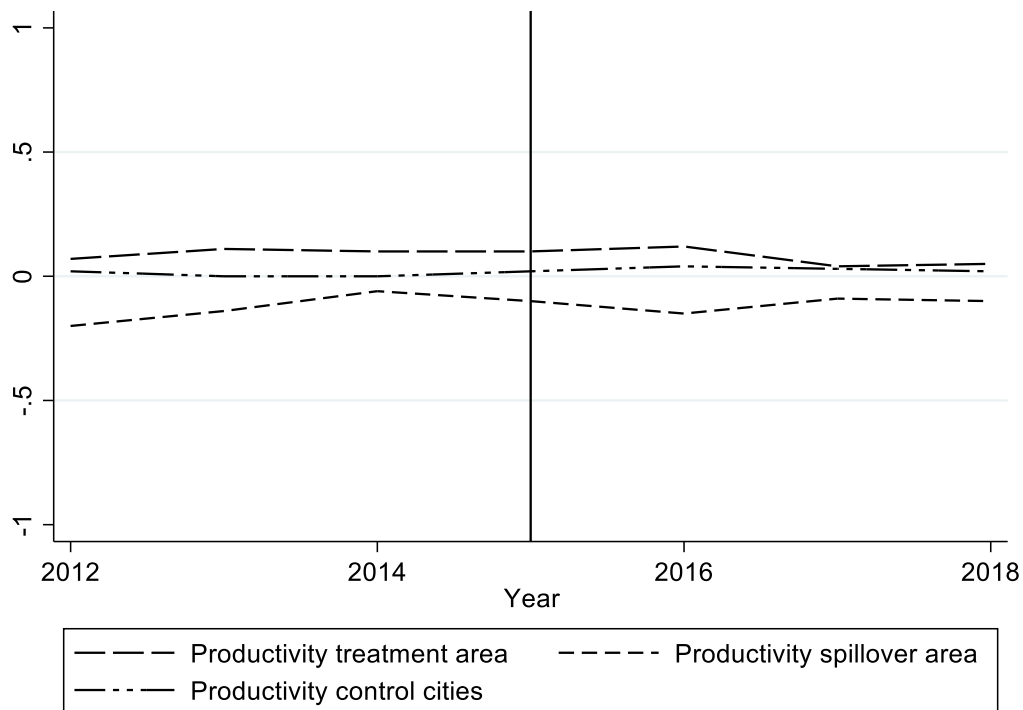
Table A1. Natural logarithm of labour productivity, CPI-adjusted revenues, and number of employees – mean and 95% confidence intervals

	Lower bound	Treated cities, within BID	Upper bound	Lower bound	Treated cities, outside BID	Upper bound	Lower bound	Control cities	Upper bound
Labour productivity									
2012	6.39	6.52	6.65	6.13	6.25	6.36	6.41	6.47	6.52
2013	6.41	6.53	6.66	6.17	6.28	6.39	6.37	6.42	6.48
2014	6.36	6.50	6.63	6.24	6.35	6.45	6.34	6.40	6.46
2015	6.39	6.51	6.63	6.21	6.31	6.41	6.37	6.43	6.48
2016	6.40	6.53	6.66	6.15	6.26	6.36	6.40	6.45	6.51
2017	6.29	6.42	6.56	6.19	6.29	6.39	6.35	6.41	6.46
2018	6.30	6.44	6.57	6.19	6.29	6.39	6.35	6.41	6.46
Revenues									
2012	7.55	7.46	7.94	7.24	7.40	7.57	7.56	7.64	7.71
2013	7.56	7.75	7.94	7.24	7.40	7.55	7.50	7.57	7.65
2014	7.53	7.73	7.93	7.33	7.48	7.63	7.45	7.53	7.61
2015	7.53	7.72	7.91	7.29	7.44	7.59	7.48	7.56	7.63
2016	7.52	7.71	7.90	7.23	7.38	7.53	7.49	7.57	7.64
2017	7.38	7.59	7.79	7.26	7.41	7.55	7.43	7.51	7.58
2018	7.41	7.61	7.81	7.25	7.39	7.54	7.42	7.50	7.58
Number of employees									
2012	1.12	1.22	1.33	1.08	1.16	1.23	1.13	1.17	1.21
2013	1.12	1.22	1.32	1.04	1.11	1.19	1.11	1.14	1.19
2014	1.13	1.23	1.33	1.06	1.14	1.21	1.09	1.13	1.16
2015	1.11	1.21	1.32	1.05	1.13	1.20	1.09	1.13	1.17
2016	1.07	1.18	1.29	1.05	1.12	1.20	1.07	1.11	1.15
2017	1.06	1.16	1.26	1.05	1.12	1.19	1.06	1.10	1.14
2018	1.17	1.17	1.27	1.03	1.10	1.18	1.06	1.09	1.13

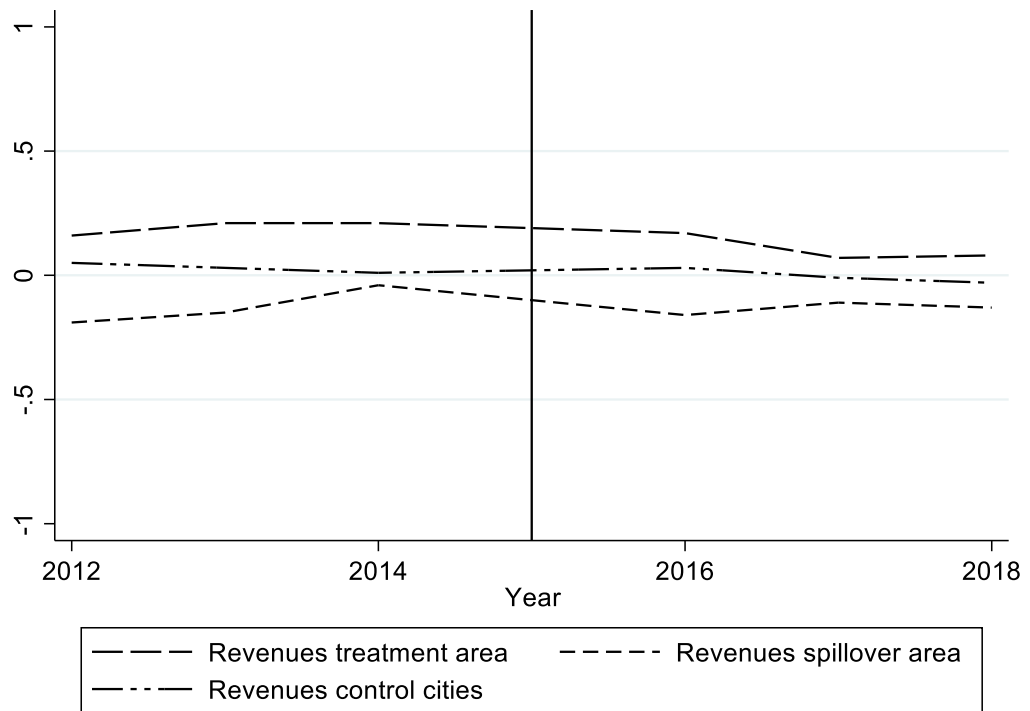
Appendix B. Pope and Pope (2015) trend estimations

Figure B1 illustrates the pre- and posttreatment trends of the type suggested by Pope and Pope (2015) for the natural logarithm of labour productivity (measured as the CPI-adjusted revenues per employee), the CPI-adjusted revenues, and the number of employees in our treated and control cities. To generate the type of trends suggested by Pope and Pope (2015), we run the regression presented in equation 2 without the treatment-effect variable for labour productivity, CPI-adjusted revenues, and the number of employees. The residuals from these regressions are presented in Figure B1. These residuals represent the underlying trend in the outcome variables after controlling for the impact of the other dependent variables in the regression. The trends in the treated areas in the BID cities and the trends in the selected control cities are parallel in the pretreatment period, which, together with the data presented in Appendix A and in Figure 2 in the main text, supports the argument that these cities are suitable controls for the BID cities.

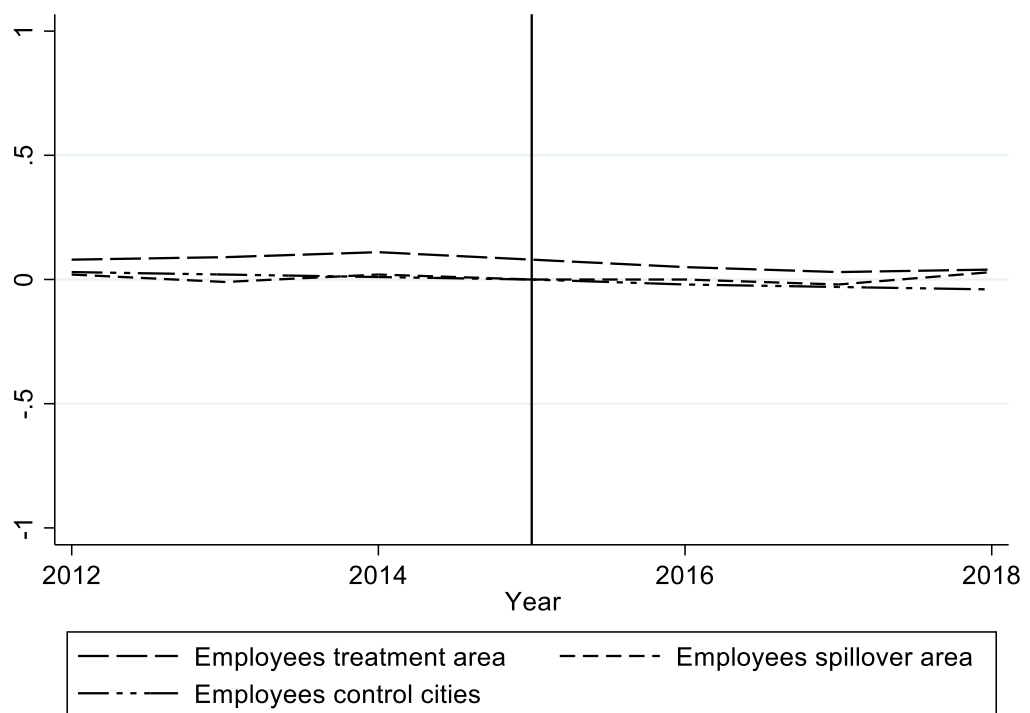
Figure B1a–c. Pope and Pope (2015) pre- and posttreatment trends in the BID and spillover areas in the treated cities and control cities as a whole, 2012–2018.



a) Productivity (residuals from Pope and Pope (2015) estimations).



b) Revenues (residuals from Pope and Pope (2015) estimations).



c) Number of employees (residuals from Pope and Pope (2015) estimations).