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The effect of low emission zones on firm employment

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Abstract

This thesis tries to identify impacts of the introduction of LEZ's on firm employment. Literature and societal concerns on the implementations of these zones are discussed, however with limited arguments and non-scientifically backed concerns for a possible effect no clear answer can be provided on this question. To identify this effect, the implementation of LEZ's in the five major Dutch cities are analyzed. Treatment and control groups are created around the borders of the individual LEZ's. A difference-in-difference model is then utilized to answer the research question. Both for each city individually, and for different heterogeneity analyses based on firm size; industry; and specific locations, no causal effect can be determined on the effect on the number of jobs for firms inside the LEZ. It is however noted that this thesis was first aimed to identify the effect of LEZ implementation on housing prices. Due to late issues with data availability, this was not possible, and the shift was made to the effect on firm employment. For this reason, the exact approach used in this thesis is not ideal for the research question.

Introduction

Low emission zones (LEZ from here on) are specific zones, often located inside inner city areas, that prohibit certain polluting cars or other motorized vehicles from entering these areas (Green-Zones EU, 2021). The purpose of these zones is increasing the quality of life within city centers by reducing localized air pollution from the vehicles that pollute most and by reducing road traffic by trucks, who are often targeted by these zones (Eurocities, 2021; Ministerie van Algemene Zaken, 2022). The Netherlands currently hosts fifteen LEZ's, out of which four target person cars and vans (Amsterdam, Arnhem, Den Haag, and Utrecht) (Locaties Milieuzones | Milieuzones in Nederland, n.d.). It is important to note that each specific LEZ in the Netherlands has its own rules and regulations, some criteria are stricter than others and some zones change their regulations over time to become more or less strict (anwb, 2021; Locaties Milieuzones | Milieuzones in Nederland, n.d.).

This thesis will focus on the effects of implementing a LEZ on the number of jobs per company within these zones in the five major cities of the Netherlands (Amsterdam, Den Haag, Eindhoven, Rotterdam, and Utrecht). This as a LEZ could potentially alter the development of employment. Due to the high amount of LEZ's in the Netherlands, the seemingly long-term implementation of these zones, and the current non-existing literature on this effect, it is of both social as academic relevance for this subject to be researched. For these reasons, the research question that this thesis will therefore try to answer is as following:

Does the implementation of low emission zones in one of the five major cities in the Netherland alter the number of jobs per company within this zone compared to similar companies outside this zone?

Before analyzing the effects of LEZ's on the number of jobs, it is important to review what the effects of environmental zones on its imposed policy goals are, as these could be possible explanations for the effect of the LEZ on the number of jobs. Noted is however that these explanations will not be proxied for in the model, and only serve as an understanding in what ways the LEZ might materialize on the number of jobs. They are used for argumentation purposes on why an effect of the LEZ on the number of jobs might exist and so the importance to investigate this possible effect. To investigate this possible effect, a difference in difference model will be implemented with treatment and control groups being created around the borders of the LEZ's of the five cities. It can however be argued that the effect of a LEZ differs per city, with strictness of the regulations, or within different subgroups within a city. Therefore, all five individual cities are analyzed separately. Additionally, heterogeneity between firms within cities is considered. For this, separate analyses based on firm size, industry, and certain specific areas are performed. Finally, variation in the size of buffers for the treatment and control group creation will be taken into account resulting in two verification checks.

Literature review

Determinants of business success and location choice

When it comes to business performance the current literature looks at organizational characteristics, locational characteristics, and individual characteristics of executives as potential determinants of successful companies. The following section will provide a brief overview of some of these studies and what the literature classifies as determinants of success. Sarwoko & Frisdiantara (2016) provide a model on the determinants of growth of small and medium enterprises (SME's). Here, they consider three main elements under subject of research and identify their effect on SME growth. The first are individual factors, which considers both so called "attributes" characteristics, that are often pre-determined (for example sex, age, gender, family upbringing) and "attained" characteristics that are often influenced by internal and external factors as education and previous work experience. Secondly, they mention organizational characteristics which is described as the effectiveness of using capabilities and resources. This can be influenced by firm attributes, strategies, resources, organizational strategies, and dynamic capabilities. Lastly, they take environmental factors, such as the market conditions into account. Results seem to suggest that for individual factors, motivation, and previous experience play an important role in SME growth, but also how individual characteristics play a role in increasing organizational effectiveness. Additionally, the effective use and access to raw materials and skilled labor seems important for SME growth. Finally, the effect that the environment has on organizational characteristics is deemed important, in for example how businesses use technology and product variation to manage the possible competition. Islam et al (2011) finds how success for SMEs in Bangladesh is impacted by for example, the duration of the business operations, gender, individual characteristics such as personal involvedness, attention to detail, social networks, inter-firm cooperation, and communication. Kalleberg & Leicht (1991) however describe how there are no differences in chance of business survival between genders. They also do not find that older companies are more profitable, but they do find that older companies are less likely to go out of business. Finally, Dehning & Stratopoulos (2003) find how managerial IT skills can be an origin of a sustainable competitive advantage.

Not just organizational or individual characteristics can impact business success or location choice. So does Gordon (1991) discuss how certain industries can influence the culture of companies, and so their success. This is due to different competitive environments, customer requirements, and societal expectations between industries. Bartik (1985) discusses the effect of union sympathies and tax policies in US States to influence location choice of companies. Additionally, it is found in Bartik (1988) that for the most polluting industries environmental regulation can affect locational choice. Schmenner (1994) find how infrastructure, proximity to customers, and how effectively labor can be attracted impact firm location. Additionally, it is found that less local companies are more affected by governmental policies than more locally tied firms.

From the brief literature overview, it has become clear that different types of characteristics can affect the performance of a businesses. It can be argued that a LEZ can be one of these characteristics, or can bring unintended effects, that influence the performance of companies. For this reason, the following sections will discuss the goals and effects of LEZ's and their possible effect on firm performances.

Main policy of LEZ's: improving air quality

The Dutch government explains that the need for LEZ's in the Netherlands is caused by the bad air quality in city regions, allowing municipalities to implement a LEZ to improve this air quality (Uitleg Milieuzones | Milieuzones in Nederland, n.d.). The German government explains that LEZ's are implemented to reduce emissions that negatively affect health (Auswärtiges Amt, n.d.). Similarly, the European Commission acknowledges the environmental objectives of implementing LEZ's in the European Union (European Comission, n.d.) and in a portal developed by the European Commission it is stated that LEZ's are implemented in areas where the air quality is harmful to the health of inhabitants, as it is argued that LEZs can decrease the emissions of fine particles, nitrogen dioxide and ozone (Urban Access Regulation in Europe, n.d.-b). For the five LEZ's that are discussed in this thesis, similar argumentation can be applied, which will be further discussed in the section "Selection of LEZ's".

Now that it is determined what the main policy goal of implementing a LEZ is, it is important to verify whether this goal is achieved by different LEZ's around the world.

Panteliadis et al. (2014) compares the change in levels of different measurements in air pollution in Amsterdam before and after the introduction of the LEZ. By comparing these changes between two stations, one alongside a busy road (which was beforehand often used by heavy traffic) and one in a less frequently used road in the zone. They were able to find a significant reduction in pollution levels after the introduction of the LEZ. Internationally similar effects have been found, with a larger decrease in particulate matter pollution inside the LEZ of London compared to outside of its zone (Ellision et al., 2013). During the 2009 and 2016 period, decreasing levels of PM_{10} and NO_2 were found after the installation of the LEZ in Lisbon (Santos et al., 2019). Holman et al. (2015) reviews literature on pollution changes and conclude that in several German zones, which are often stricter than other zones, decreasing PM10 and NO2 levels are found (even if this is a small percentage). They also note that these zones might be more effective in decreasing carbon particles emitted by traffic. Cyrys et al. (2014) show how PM10 levels decrease by up to 10% after the introduction of the Berlin LEZ and highlight that the most toxic diesel soot in these particulate matters decreased by up to 63%. Wolff (2014) shows how PM10 levels decrease by 9% for German LEZ's and additionally finds that this effect is lower or non-existent outside the LEZ. Additionally, it is found that there are heterogenous effects for LEZ's, with larger LEZ's being more successful in reducing PM10 levels. Additionally, Urban Access Regulations in Europe (n.d.-a) provides an overview of results from various European LEZ's. It mentions how the different LEZ's reduce nitrogen (dioxide), carbon dioxide, soot, and particulate matters. Positive results are mentioned for the LEZ's in London, Berlin, Milan, Rotterdam, Leipzig, Cologne, Stockholm, and Copenhagen.

With this information, it can be concluded that LEZ's are able to decrease several forms of air pollution. It is noted however that the exact impact of a LEZ on air quality is dependent on several factors such as the enforcement of the LEZ, the banned vehicle types, the area of the LEZ, how vehicle owners react to the LEZ, the vehicle fleet before implementation, the weight of different emission types, and how severe the air quality problems were before implementation of the LEZ (Urban Access Regulations in Europe, n.d.-a).

The increasing air quality in cities caused by LEZ's has several impacts on, for example health of the residents and visitors of the city. So does a study in Rome show how the implementation of a LEZ and its consequential improvement of the air quality resulted in a gain of 3.4 lived days per person for over 260.000 residents (Cesaroni et al., 2012). In Cyrys et al. (2014) it is calculated how the measured decrease in PM10 emissions for the Berlin LEZ is calculated to have prevented 144 premature deaths. It is argued that the actual health benefits would be way larger if more health relevant components of PM10 would be measured, for example diesel soot, which is argued to be one of the main harmful components of fine particulate matter. This is due to diesel having less unburned hydrocarbon and carbon monoxide than petrol (Prasad and Bella, 2011).

De Kok et al., (2006) and Harrison & Yin (2000) further elaborate on this in highlighting the different impacts on health of different components of fine particulate matter and the difficulties in measuring these components, with de Kok et al. (2006) also highlighting that some of the most harmful components do arise from traffic intensity and predominantly diesel engines. There are however no actual measurements of these direct effects and compositions of PM in specific areas. Health effects of particulate matter furthermore depend on local conditions such as weather, size of particles (as smaller particles further enter the respiratory system), social-economic factors, and respiratory habits (Davidson et al., 2005; De Kok et al, 2006) complicating investigating direct effects of PM emissions.

In more general literature, Kim et al. (2015) provides an overview on the effects of fine particulate matter (PM2.5 and PM10). It is estimated that over 2.000.000 deaths occur around the globe annually due to air pollution, most of those caused by PM emissions. Health effects of PM can be fetal and infant deaths, shortness of breath, coughing, diabetes, lung development for children, asthma, lung cancer, etc. People with already present health conditions are experiencing even further health problems. All these effects lead to millions of pre-mature deaths and billions of healthcare costs.

It is however unlikely that the difference in air quality and its health effects between areas with or without a LEZ will influence business performances in a significant amount. It is possible that business owners would rather locate in areas with better air quality or that customers prefer better air quality, but there is no literature finding a causal effect of this preference being strong enough to impact business performances.

With the examined literature, it is hypothesized that the main policy goal of LEZ's, namely improving the air quality will be seen as an increase in value of the area, by either perceived benefits or actual health benefits. However, this increase in value of the area is not strong enough to influence business performances. Additionally, another cause of concern is the research of Lebrusán and Toutouh (2020), who find decreasing levels of pollution levels due to the implementation of the Madrid LEZ. However, they did not find a so called "border" effect, indicating that the improved air quality was not just limited to inside the LEZ but also other parts of the city, indicating that there is no benefit in terms of air quality of being located on one side of the border of the LEZ compared to the other side.

Side effects of the LEZ

Low emission zones can have several side effects besides the main policy goal of improving air quality. Some of the most prominent side effects and their possible effects on employment are therefore examined and described in the following sections.

Accessibility and social exclusion

A possible effect of the implementation of a LEZ is the decreasing accessibility of firms in the LEZ and forms social exclusion, potentially affecting firm success and location choice. Targa et al (2006) for example find how different forms of accessibility (highway access, road capacity) affect the location choice of firms.

Social exclusion can be seen as the multidimensional development of sidelining of individuals in their daily lives, affecting indicators like income inequality, homelessness, and health (Shaw et al., 1997). The change in accessibility for firms inside the LEZ can cause changes in the provided labor pool, as for some workers it is no longer possible to commute to work with an older vehicle as these vehicles are now banned from entering inside the LEZ. It is also noted that most of the cars being banned for being polluting are often older and cheaper, possibly targeting poorer residents who are already more at risk of social exclusion and less likely to be able to afford a new car. This argumentation shows how firms inside the LEZ can be less attractive as employers for employees as the lower accessibility of firms creates a new form of social exclusion. This unattractive characteristic can lead to a decrease in labor supply, an increase in wage costs, and so, possible changes in firm employment. Additionally, firm owners experience lower accessibility for customers and suppliers, negatively affecting the performance of these companies. With customers not being available to enter to LEZ when cars are regulated, this could decrease revenues for shops.

The larger concern however is urban freight deliveries not being able to enter the LEZs. Dablanc and Montenon (2015) show how the introduction of the LEZ's in Berlin and London caused a decrease in firms making urban deliveries and a 15-30% decrease in transport and logistics firms operating in cities with a LEZ was observed. This decrease in urban deliveries and transport firms operating inside LEZs could cause potential increases in costs for the deliveries of goods, or less frequent deliveries of goods. This increase in cost and / or time associated with urban deliveries could potentially result in firms becoming less profitable and so in turn the employment of these firms could potentially decrease.

Additionally, small businesses in the Amsterdam region raised concerns about the new expansion of the LEZ as they were no longer allowed inside the LEZ for businesses opportunities and jobs. This was supported by nationwide concern, voiced by the Dutch SME organization, that the growth of LEZ's and the decrease in access for companies would be hurtful for these companies (Kleijn & Weel, 2020; NH Nieuws, 2009; Het Parool, 2018). This voice of concern additionally shows that even in the case where the effects of lower accessibility do not actually materialize, firms "feel" threatened by the introduction of a LEZ. This feeling

alone could already cause firms to relocate or make changes in their operations, leading to changes in their firm employment.
To conclude, the implementation of a LEZ decreases the accessibility of companies inside the LEZ. It can therefore be hypothesized as being a mechanism for a decrease in the number of jobs in this region. There is however no current literature available on this effect.

Noise reduction

With the banning of heavy traffic (and possibly older diesel and petrol cars) an additional side effect of the implementation of a LEZ is a decrease in noise. This would be the case as diesel engines are way noisier than petrol engines, which is mainly caused due to the higher compression and temperature inside a diesel engine causing uneven burning of the fuel and a so-called knock (Vartabedian, 1990). Lebrusán and Toutouh (2020) find how the implementation of the LEZ in Madrid caused decreasing noise levels, mainly those of road traffic. Browne et al. (2005) briefly note that one of the benefits of the LEZ in London is noise reduction. Stansfeld et al. (2000) provide an overview how different environmental noise factors (including traffic noise) negatively affect the quality of life, but more recent studies suggest a relationship with traffic induced noise and different health issues such as sleeping problems (Bluhm et al., 2004; Fyhri and Aasvang, 2010), hypertension and coronary heart diseases (Clark and Stansfeld, 2007).
There is however no literature on its effect on firm employment, and it is unreasonable to expect that a slight noise reduction will affect the success of firms in the region.

Decreasing and changing fleet

Another goal of LEZ's is decreasing the amount of traffic and congestion within city centers. The idea behind this is relatively simple, by banning a certain amount of the vehicle fleet to enter, it reduces the traffic in this area. Tassinari (2022) investigates this effect and notes several important mechanisms in reducing traffic as people can change behavior in several ways. People can take public transport, use different routes around the zone (depending on the size of the specific zone, which in the Madrid case is relatively small with only 1% of total city area) or change from older to newer vehicles that are allowed in the zone. Overall, a decrease in traffic is found within the zone, however this traffic is moved to other areas in the city. This is also supported by de Bok et al (2021) who find decreasing vehicle miles travelled inside LEZ's by the freight sector and increasing miles travelled outside the LEZ's. The decreasing traffic of both daily commuters and the freight sector could potentially hurt firms inside the LEZ, as suppliers and other forms of urban deliveries might enter the LEZ on a less frequent basis or charge extra for deliveries inside these zones due to the costs associated with renewing their fleet. Customers could be entering the zone in lower volumes due to their cars being banned resulting in a loss of income for firms. Overall, the banning of a certain number of vehicles (specific to the regulations of the LEZ) will decrease the utility of companies inside the LEZ. This could possibly work as a mechanism in decrease the labor demand and so a decrease of the number of jobs per company inside the LEZ.

Besides possibly decreasing fleet size, the vehicle fleet inside the city changes. Ferreira et al. (2015) highlights how there was a relative decrease in pre-EURO 2 vehicles and an increase in EURO 4 and 5 cars, causing a significant higher vehicle turnover rate in for example Berlin and London after introducing the LEZ. An important notion is however that the change in fleet only really took off after the 2nd phase of the implementation of the Lisbon LEZ and Cyrys et al. (2014) states that the LEZ motivated people to increasingly replace their cars. Changing the vehicle fleet is the main mechanism in improving air quality. It is argued that an LEZ speeds up the process of replacing old polluting cars to newer and less polluting cars and therefore, that this is only a temporary effect with the turnover rate coming back to national averages in subsequent years (Holman et al., 2015; Ellison et al., 2013). On average, the car fleet is renewed within 7 months, however with several environmental zones already showing increasing regulations over time, this process would thus then also be repeated over time (Shaw et al., 1997; Tassaniri, 2022).

However, besides this main goal, changing this fleet also comes at a cost, namely the costs of buying new cars for residents and firms. Settey et al (2019) raise concern of the readiness of road freight transport due to the increasing amount of LEZ's in cities. Conway et al (2012) describe access regulations for different emission types as a clear competitive advantage for cleaner vehicle owners. It is however not uncommon for cities and municipalities to have certain benefits by providing subsidies for residents as for example in Rotterdam (Rubio, 2015), but for companies these benefits do not cover all costs that are made for renewing the vehicle fleet as they are relatively small. With this reasoning, the decreasing fleet through city centers, with addition of the cost made for the change in fleets of companies could negatively influence the success of businesses inside LEZ's, and so decrease the number of jobs inside the LEZ.

Selection of LEZ's

After the above literature review on the potential effects of LEZ's on the success of businesses, it is important to analyze each of the LEZ's that will be subject to research in this thesis. Currently, there are four cities in the Netherlands with a LEZ for personal vehicles: Amsterdam, Arnhem, Den Haag, and Utrecht (Locaties Milieuzones | Milieuzones in Nederland, n.d.). The Rotterdam LEZ offers another distinct possibility as this LEZ changed regulations on what type of cars and heavy traffic were allowed. The Eindhoven LEZ is only applicable for heavy traffic and not for personal cars.

The first LEZ's in the Netherlands were introduced after a covenant was signed in 2006 by ten cities (Tilburg, Delft, Eindhoven, Den Haag, Haarlem, Helmond, Den Bosch, Nijmegen, Rotterdam, Utrecht). This covenant was followed by the introduction of LEZ's for heavy traffic in 2007 in these cities, with the aim of only allowing the cleanest heavy traffic in 2010 inside city centers (De Volkskrant, 2006). The first LEZ's were introduced in July 2007.

For this research, the 5 major cities in the Netherlands will be investigated. This allows to compare possible effects of different type of LEZ's and the timing of possible effects as some zones are longer in place than others. The next sections offer an overview of the history, regulations, and type of enforcement of the zones.

Amsterdam

The LEZ in Amsterdam was first introduced on 1 October 2008 for heavy traffic (Promovendum, 2022b). At first, this LEZ was planned to not just ban heavy traffic but also for vans and personal vehicles. This plan also incorporated increasing parking tariffs and benefits in parking or public transport prices for owners of less polluting vehicles (De Volkskrant, 2008). However, in 2009 it was announced that the introduction of the LEZ for personal vehicles was delayed by a year due to legal issues, as the matching of number plates and emission classes for cars was not yet legally possible. Besides this issue it was also mentioned that the introduction of the LEZ for personal cars was already to be re-evaluated, with this being dependent on more recent air quality measurements. The introduction of the LEZ for heavy traffic went according to plan, with enforcement starting in the second week of January of 2009. This enforcement would first be performed by officer of the municipality, with later in the year (September) number plate cameras taking over this task. Heavy traffic with emission class 0 and 1 were completely banned, trucks with emission classes 2 and 3 had to install a specific filter. If they would still enter the zone, a fine of €150 would be given. It was reported that the introduction of these cameras raised compliance from 65% to 90-95% and a daily number of 150 trucks were fined each day (De Volkskrant, 2009a; De Volkskrant, 2009b; De Volkskrant, 2009d). Later in 2009 a TNO report stated that the LEZ for personal cars would have less effect than proposed. This resulted that the LEZ would remain to only apply for heavy traffic and the implementation of a LEZ for personal vehicles was no longer under considerations (De Volkskrant, 2009c). Since 2018 it is no longer possible to enter residential areas with mopeds built in 2010 and before (Promovendum, 2022b; Gemeente Amsterdam, 2022a). This increase in regulations was followed in 2020, as diesel cars manufactured before 2005 and petrol cars manufactured before 1992 were banned since November 2020 with fines started being handed out since April 2022 (Gemeente Amsterdam, 2022b). This increase in regulation was also followed by an increase of size of the LEZ, with now the north of the city also being incorporated in the LEZ. This is illustrated in Figure 2, with the black line representing the old borders and the yellow surface the expanded LEZ. Future further restrictions are already planned for 2025 and 2030 (Promovendum, 2022b).

To conclude, with expansion of the LEZ in 2020 it is important to create the two different LEZ sizes for each specific period, this to prevent false assignment into the treatment and control groups.

Figure 1. Current Amsterdam LEZ (Gemeente Amsterdam, n.d.)

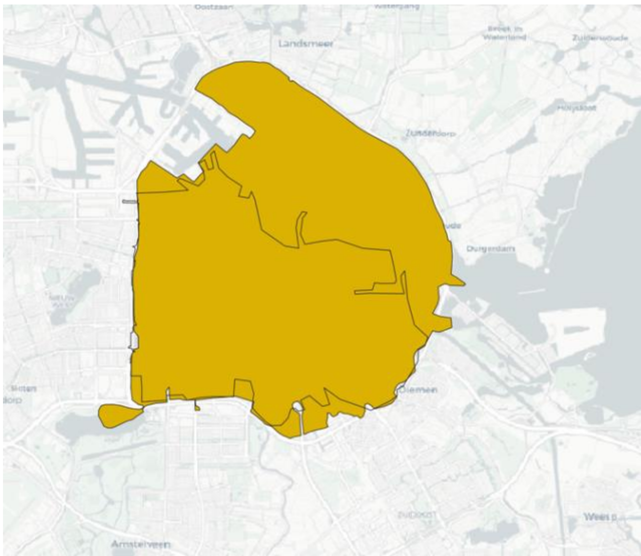


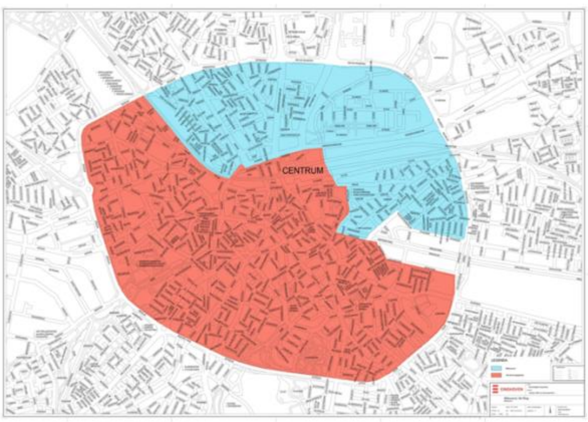
Figure 2. QGIS created figure of LEZ's in Amsterdam

Eindhoven

Similar to other zones, the LEZ in Eindhoven was implemented after signing the covenant in 2006 (De Volkskrant, 2006) and together with Utrecht it was the first municipality to open the LEZ. This was done on the first of July 2007 (Green-Zones EU, n.d.). The first months of the LEZ were not enforced, only from 1 January 2008 the municipality started enforcing the LEZ.

The LEZ was located around the highway ring around the city and banned heavy traffic with emission class EURO 0 and 1, and classes 2 and 3 were only allowed to enter after installing a soot filter (Vliegenberg, 2007). In February of that year, it was reported most trucks in the city were still “dirty” and fining would start from April 1, with enforcement intensifying from that point onwards. Enforcement was done by random controls in the LEZ, fines for entering the LEZ with a banned truck were €150 (AD, 2008a; AD, 2008b). The percentage of heavy traffic inside the LEZ that according to the regulations was not allowed to enter the zone was 27% (AD, 2009). In 2010 this was reported to still be at around 20% (AD, 2010). In 2016, the green party advocated the use of cameras to further enforce the LEZ (AD, 2016c). On 1 July 2013 the regulations of the LEZ became stricter, as now heavy traffic with emission classes 2 and 3 now also being completely banned on the south side of the LEZ (Molle, 2013). Plans for a LEZ targeting personal vehicles were not pushed through as the gains of such zone would be too small (AD, 2016a). The residents of the city however were not opposed to further restrict vehicles in the LEZ, and more importantly advocated for stronger enforcement of the LEZ (Vermeeren, 2018). Stricter rules of the LEZ are planned, with banning of heavy traffic with emission class 4 or lower for the entire LEZ from 1 January 2021 and emission class 5 from 2022 onwards (Gemeente Eindhoven, n.d.), with possibilities of enforcement with cameras in the entire zone. Plans are made to ban personal vehicles in 2025 and to have an entire emission free zone in 2030 (Theeuwen, 2020).

It is important to note that the LEZ in Eindhoven was divided in two different sections, with until 2021 only the south part of the zone being enforced (Oerlemans, 2020). For this reason, only the southern part of the LEZ will be examined, and it will be taken into account that there was weak enforcement of this zone. The location of the zone, and the exact zone used in the analysis are provided in the figures below.



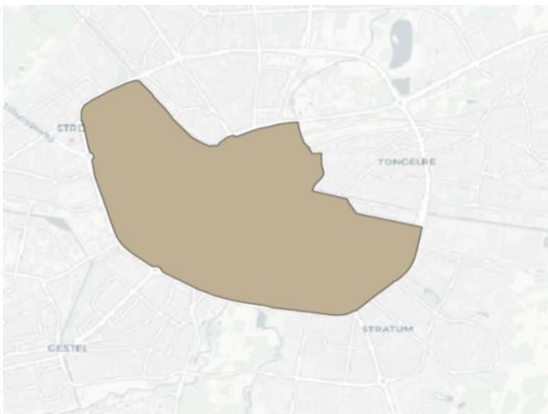


Figure 3. LEZ with red part being enforced (Oerlemans, 2020) *Figure 4. QGIS created figure of LEZ in Eindhoven*

Den Haag

The LEZ in Den Haag (The Hague) was implemented on 16 April 2008 (Verkeersnet, 2009; Gemeente Den Haag, 2008). This LEZ was implemented after the municipality signed the covenant together with the other participating cities and banned heavy traffic. In 2010 it was reported that only 80% of the heavy traffic in Den Haag fulfilled the requirements, randomized checks inside were put in place since 1 January 2010 to further battle the banned trucks from entering the LEZ with planning of starting enforcement with cameras (Dijkhuizen, 2010; Smit, 2009; Omroep West, 2009). Even with some political parties advocating for further restrictions and expansions of the LEZ, the city council stated that this was not the plan, and no personal vehicles would be banned from the LEZ. It was stated that the results of the Utrecht LEZ were not convincing enough to implement a similar LEZ in den Haag (Van Putten, 2016). This however changed in 2020 and 2021, as several expansions of the LEZ were announced and implemented. Increasing regulations were mostly supported by the residents, with results from a survey suggesting a majority of the Den Haag residents would welcome more action against the bad air quality, with the same survey noting that the air quality was already perceived to be increasing by these residents (Klippus, 2018).

Initially, the size of the LEZ got expanded in 2021, both in the north and in the south additional parts of the city center were now under the new regulations of the LEZ, the increased size of the LEZ is provided in Figure 5 below.

The first change in these regulations happened on 1 December 2020, with mopeds built before 2011 being banned inside the LEZ (Gemeente Den Haag, 2022c). This plan however could not start immediately due to technical issues, so the enforcement and fining started from 1 March 2021 (Rubio, 2020b). Additionally, as of July 2021 diesel cars with emission class 3 and lower were banned to enter the LEZ (Gemeente Den Haag, 2022b; Promovendum, 2022a), for owners of these diesel vehicles a €1000 grant was provided by the municipality (Dollen, 2021). However, this was barely used in the city of Den Haag resulting in a low vehicle turnover (Rubio, 2020a). The new regulations got even more strict in 2022 as heavy traffic with emission classes 5 and lower were now being banned from the city center (Gemeente Den Haag, 2022a). With the information on the LEZ in Den Haag in mind, it is important to make clear distinctions in the two different sizes of the LEZ in the analysis of this research. Both LEZ's are therefore created in QGIS which can be seen in Figure 6.

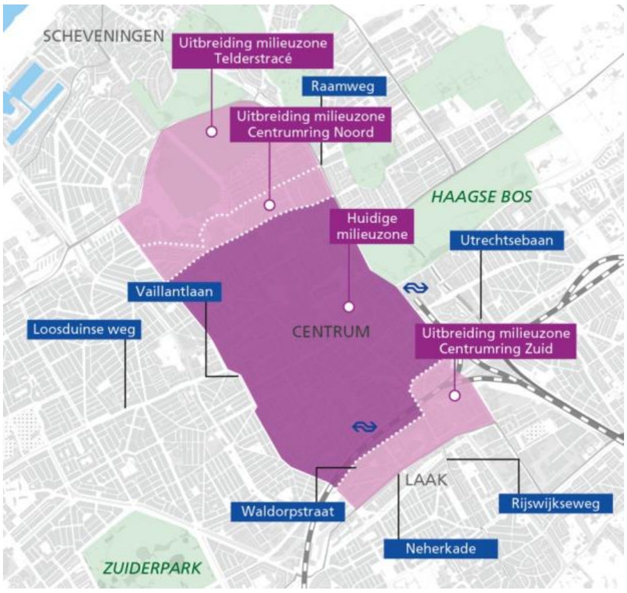


Figure 5. Expansion of LEZ in Den Haag (Hoogland, 2020)

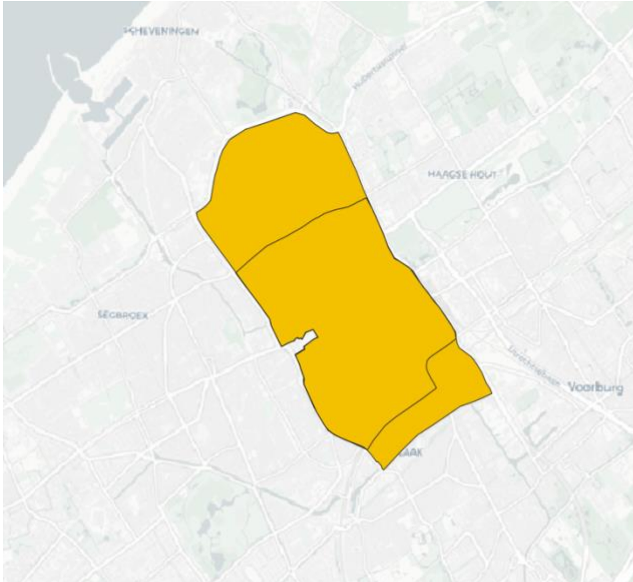


Figure 6. QGIS created figure of LEZs in Den Haag

Rotterdam

The LEZ in Rotterdam is one of a large and troubled history. The introduction of the LEZ in the city center of Rotterdam took place on 1 September of 2007 and was only focused on heavy traffic (RTL Nieuws, 2015; de Volkskrant, 2015). The city of Rotterdam was, as earlier mentioned, one of the ten participating cities in the covenant signed in 2006. Heavy traffic could only enter the city center if they satisfied European regulations on particulate matter and nitrogen dioxide, or if they had specific soot filters installed. The enforcement of the regulations was done by supervision of the municipality’s enforcement agents (De Volkskrant, 2007a), with for example routine checks by these agents leading to 15 and 37 fines after monitoring trucks in the city center (Rijnmond, 2010). The original environmental zone was located inside the areas: CS Kwartier, Stadsdriehoek, Oude Westen and Cool and caused concerns by local workers and storeowners on the accessibility of these areas (Vriend, 2008). At the end of 2008 it was announced by the municipality that the LEZ would remain in the city center after first positive signs of the effect on the air quality in the area (Rijnmond, 2008).

In 2015, the new administration of the Rotterdam municipality announced the expansion of the previous LEZ into the, at that time, largest LEZ of the Netherlands. The new LEZ expanded heavily in size, now being almost being the entire north of the city. Besides heavy traffic, also personal vehicles as cars and vans were no longer allowed inside this new LEZ. Diesel cars manufactured before 2001 and petrol cars manufactured before 1992 were no longer allowed to enter the city. These new regulations would start on 1 January of 2016, with several regulations such as an acclimatization period, exemptions for local entrepreneurs and

inhabitants and demolition arrangements put in place to battle negative side effects for the residents. (AD, 2015b; Rubio, 2015).
The first results of this newly expanded LEZ and its regulations around the expansions seemed positive, with reports of a 50% reduction of polluting cars in the city which was the highest decrease in the country (AD, 2017c), demolition arrangements causing lower registration numbers of polluting cars, and a 20-30% decrease in pollution, benefiting 68.000 residents (Rubio, 2016). After the acclimatization period ended, the first month of enforcing resulted in 1300 fines of €90 each, the enforcement was based on cameras around the edges of the LEZ (AD, 2016b). However, cautions had to be made with the municipality acknowledging a 36% decrease in particulate matters level emitted by cars (which would have been achieved a couple of years later without the LEZ) but no measurements on the actual air quality were performed (Beek, 2018). The total amount of fines over the period 2016-2018 surpassed 30.000, with a total revenue of over €4.000.000 (Kooyman, 2018).
The expanded LEZ in Rotterdam was however not shy from controversies. In 2017, the Rotterdam court decided that the ban of petrol cars manufactured before 1992 was not allowed as the municipality missed sufficient argumentations with according to the court the applied measures of the LEZ being disproportionate to the small group of vehicle owners (AD, 2017b). This judicial decision was followed by an appeal of the municipality (van Vliet, 2017) and a new calculation method, which according to the municipality was enough foundation to ban petrol cars again (Onnink, 2017). This decision, however, was again overruled by the Rotterdam court (De Koning, 2017), which later got overruled by the national court, meaning that after all court cases and periods of delay the firstly proposed LEZ was now finally completely in place again (Keunen, 2018).
After all the court cases and the over €20.000.000 estimated costs of implementing the LEZ, the new municipality coalition decided to abolish the LEZ for personal traffic from 1 January 2020 onwards, this was done after the right liberal party VVD demanded this in the coalition negotiations. The expanded LEZ would however remain in place for heavy traffic (Beek & Keunen, 2020). Since 1 January 2022 stricter regulations apply for heavy traffic, with the LEZ now banning heavy traffic with emission class 5 and lower (Gemeente Rotterdam, n.d.) and with the municipality aiming to introduce a LEZ for company vehicles and vans (as the likes of delivery vehicles) (Keunen, 2020). The large number of changes in regulations and size of the Rotterdam LEZ make it difficult to estimate a causal effect on the employment of firms. It is therefore important to take the timing of regulations into account into the analysis, as it is important to create the two different zones in QGIS. This can be seen in Figure 8.

Figure 7. Original and expanded LEZ in Rotterdam (Rijnmond, 2015)

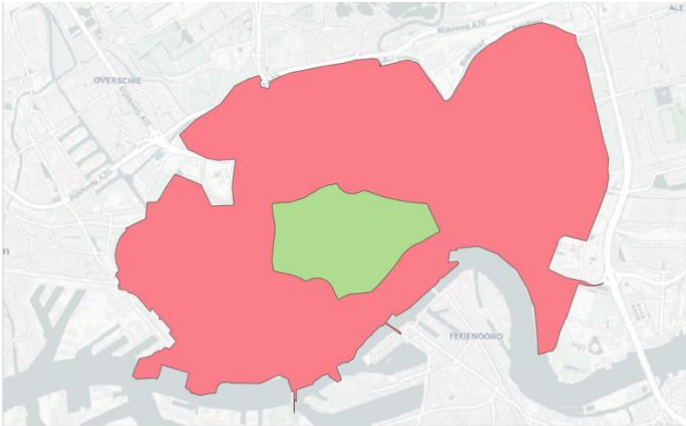


Figure 8. QGIS created figure of LEZ's in Rotterdam

Utrecht

Utrecht was (one of) the first municipalities which implemented a LEZ in the Netherlands (FEHAC, 2015). It was one of the participating municipalities in the previously described covenant, signed in 2006 (De Volkskrant, 2006). The LEZ was opened on 1 July 2007 by the Dutch minister of environment. The LEZ would cover the entire city center of Utrecht (see Figure 9 and 10) and strategically placed cameras would enforce the banned heavy traffic from not entering the LEZ (De Volkskrant, 2007b).

In 2012, the municipality of Utrecht announced plans for expanding the LEZ from not only heavy traffic but also adding the banning of personal vehicles to further improve the air quality within the city center. The plan for the expansion of the LEZ would start at the beginning of 2013 and would ban diesel cars older than 8 years and ban petrol cars older than 12 years (AD, 2012b; De Volkskrant, 2012; FEHAC, 2015; Zuithof, 2017). On 1 January 2015 the first (of total 20) cameras were put in place to enforce personal vehicles for entering the LEZ, up until 1 May banned cars entering to LEZ received a warning. From 1 May onwards the municipality started handing out fines of €90, with this fine increasing later in the year to €160 (AD, 2012a; AD, 2012b; AD, 2015a). The 4-month period was both used to acclimatize drivers as to put the enforcement cameras in place as this took longer than expected and was not ready on 1 January 2015 (Franck, 2014). On this day, it was also announced that the fleet size of polluting cars in the inner-city of Utrecht decreased from 4350 to 2000 after the LEZ was announced (Franck, 2015). Within the first four months, the municipality fined over 7500 people, which most of those being in the first two months (De Kruijff, 2015) with over €1.000.000 of revenue after almost a year of enforcement and over a total of €2.000.000 in 2018 (Remmers & Steinberger, 2016; Steinberger, 2018). The first results of the LEZ provided conflicting reports. The first report on the air quality inside the city were positive, with decreases of nitrogen-dioxide. However, this decrease was larger outside the LEZ than inside the LEZ. It is however noted that these measurements were not official or academic, and that the larger decrease outside the LEZ does not mean the LEZ is not successful in increasing air quality. It can for example be argued that the city center has significant different traffic patterns which are harder to further increase the air quality with comparison to the outside of the city (Cazandar, 2015). A later report showed increasing air quality in the city, but again notes were made that there was no direct evidence of the effectiveness of the LEZ, but only for all air quality improving measures (Franck, 2016a). The researchers themselves argued however that with their research no causal effect could be found, but with the decreasing polluting traffic in the city, calculations could prove the effectiveness of the LEZ in reducing particulate matter levels in the city (Franck, 2016b). Later, it was again reported that the air quality in the city had improved from the 2017 to 2018 (Hoving, 2020).

Just like the Rotterdam LEZ, the Utrecht LEZ was not shy from controversies. A court case on the removal of the LEZ was started but did not lead to removal of the LEZ (van Wijk, 2015). This court case was appealed by a pro-car organization (Remmers, 2016a). This appeal was however again not justified by the highest level of court in the Netherlands, meaning the LEZ for personal cars would stay (Remmers, 2017). Arguments against the efficiency of the zone were that most fines came from non-Utrecht inhabitants (van Unen, 2015), resulting in hundreds of non-justified fines and several court cases on the unclearness on the location of the LEZ (Remmers, 2016b; Remmers, 2016c). This resulted in the placement of additional signs at the entry of the LEZ (AD, 2017a). In 2021, further restrictions on cars were applied and now petrol cars manufactured between 2000 and 2005 with emission classes 3 and lower were banned from the city center (Promovendum, 2022a; Steinberger, 2021a). Causing an increase in the number of fines after enforcement started on 1 April of 2021 (Steinberger, 2021b). The municipality of Utrecht announced increasing measures for the future, with concrete plans of further restricting cars and increasing the size of the LEZ (van Rossum du Chattel, 2020) and plans are being made to make the LEZ completely emission free in 2030, also targeting mopeds. It is however at this time unclear how large this zone will be, and how this plan is progressing (Hoekstra, 2018).

These new plans however do indicate the LEZ in Utrecht is there to stay, and unlike Rotterdam will be abolished.

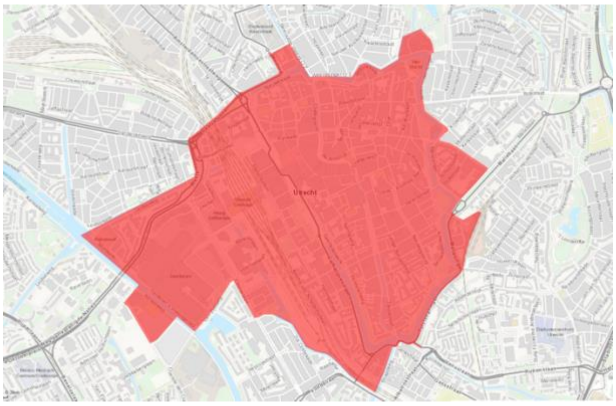


Figure 9. LEZ of Utrecht (Gemeente Utrecht, 2023)

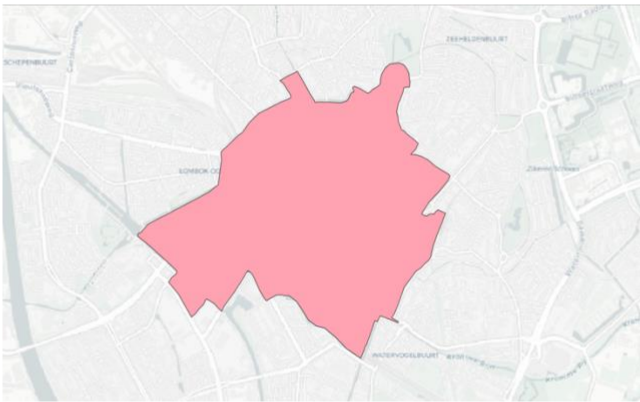


Figure 10. QGIS created figure of LEZ in Utrecht

Overview & hypothesis

The literature does not provide a clear hypothesis to the stated research question due to their not being any similar study performed yet. The main goal of the LEZ is improving air quality inside city centers. It is however unlikely that this will directly affect firm employment. Besides this main effect the side effect of noise reduction is also deemed unlikely to affect firm employment. Side effects such as decreasing accessibility due to arising social exclusion and the costs of changing the fleet after LEZ implementation are argued to negatively impact the performance of firms inside the LEZ, which in their turn can cause a decrease of the number of jobs that reside within these firms. Therefore, the following hypothesis is provided.

The implementation of a LEZ will negatively affect firm performances due to decreasing accessibility and costs of changing fleets.

No clear answer however can be provided whether the negative effect on profitability is sufficient to affect the firm employment. Due to there not being any similar studies performed before, the possibilities of changes in firm employment due to negative impacts of LEZ on firm success, the concerns of firms and SME organizations, the wide array of different LEZs in the Netherlands, and the extensive future plans of expanding LEZs this question is of high scientific and societal relevance to study.

Data

Defining treatment and control

To investigate the effects of implementing LEZ's on firm employment two main sources of data are needed: the location of the LEZ's and firm employment data in the proximity of this zone. With this data, the treatment and control group can be created. However, before this process is explained it is important to define these groups. The created treatment group can be defined as the companies laying in a buffer from a certain distance at the border of the LEZ, inside the LEZ. These are the firms that do experience the policy of the LEZ. The control group can be defined as the companies laying in a buffer from a certain distance at the border of the LEZ, outside the LEZ. These are the firms that do not experience the policy of the LEZ. This is done for several reasons. Firstly, as the LEZ borders provide a natural experiment, where certain firms are under regulation of the policy and other firms are not. By not simply comparing firms outside and inside the zone, but by creating these buffers of a certain distance around the border the difference in company characteristics (that as in the literature explained can affect the success of companies) can be limited. For example, firms on the outskirts of the city could be very different than firms in the city center. Additionally, it is harder to compare cities with a LEZ to cities without a LEZ, as these cities are often way smaller than the cities subject to research or can have different measures against bad air quality, causing possible issues in causal identification. The creation of a treatment and control group based on distance is often used in so called border discontinuity designs, which are used to investigate the effects of certain amenities on housing prices. So does Gibbons et al. (2013) investigate the effect of schooling quality on housing prices by making use of local authorities' boundaries. Bosker et al (2019) investigates the effect of flood risk on housing prices by, simply summarized, creating treatment and control groups based on a 100-meter distance buffer from the flood line. The approach of this thesis is inspired by these methods and applies it to a difference-in-difference model.

For the main results, a buffer distance of 250 meters will be used. This is done for two main reasons. If the distance becomes too small, the sample size decreases and causes issues in causal identification. This can for example lead to problems in the later proposed heterogeneity analyses. If the distance becomes too large, firms will become too different from each other, which as explained above causes issues. For the exact 250-meter distance there is however no argumentation found in the literature. For this reason, a check for the main results will be provided. Buffers of 100 meters and 500 meters will be created and the analyses will be similarly performed to verify the main results. In Appendix A, a full table of all used buffers for each of the created LEZ's is presented.

LEZ-data and creation of control and treatment groups

The first data is the location data of the LEZs for creating a treatment and control group around the borders of the LEZs. Postal code data on the location of LEZ's in the Netherlands is available from the Dutch government website milieuzones (*Locaties Milieuzones | Milieuzones in Nederland*, n.d.). This data provides the postal codes and addresses of all current LEZ's. A barrier around this border will be created on both sides to create the treatment and control group. Postal code data of the Netherlands was used as geometric reference and was the first data to be downloaded into QGIS. With this now referenced data the LEZ data described in the above section was downloaded into QGIS. The two data types were joined together, allowing the LEZ's to be georeferenced in QGIS. This had the result that the data layer now existed of all the LEZ's of the five cities. Each individual city was filtered by name, making sure only this city was visible in the layer, allowing for individual adjusting of the data. The first step of adjusting the layers was that the different postal codes of each city were dissolved into one polygon. This was needed, as otherwise buffers would be created around each individual postal code and not the outer range of the LEZ.

For all five cities, the website of each corresponding municipality was used to check the LEZ's and compare them with the attained dissolved polygon. Based on this comparison adjustments were made. These adjustments were needed, as slight mistakes in the determination in the zones could result into wrong placement of firms in either the control or treatment group, falsifying the results of the research. As the original data source was completely based on postal codes, and not on a map with borders, the provided maps from each city's municipality were deemed more reliable, as for example a postal code can be partially located inside and outside the LEZ. The correction of the LEZ was done manually by deleting, adding, and adjusting border points of the attained dissolved polygon for each of the five cities. The three main fixed are described below.

- Missing parts of a LEZ inside the provided borders of the zone were added to the zone. Missing parts existed due to either errors in the handling of the data by the used program causing certain areas within the LEZ to be deleted from the attained dissolved polygon, or due to certain postal code areas not being present in the original data source. Without the removal of these empty zones, a buffer inside the LEZ would be created resulting in the wrong placement of firms inside the LEZ into the treatment and control groups. All the border points of these spots were deleted and so these empty parts were integrated into the LEZ, preventing the wrong placement of firms in either of these groups.

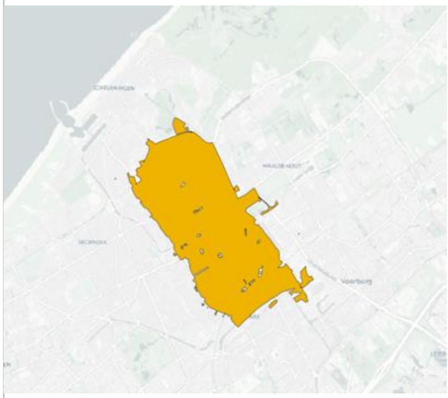
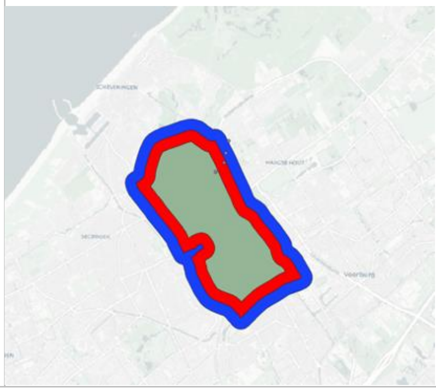
- Deleting of areas which were not into the actual LEZ. A LEZ is not determined exclusively by postal codes but is mostly determined by the highway ring around the city center. This caused several sections of postal codes that laid both inside as outside this ring to be completely in the LEZ, while in reality this was only partially the case. These border points of these sections had to be deleted and new points were added to create the correct border, and so correct placement of firms.
- Small (seemingly) random parts of streets or postal codes outside the zone were attributed to be inside the environmental zone. If these spots would not have been deleted, buffers around them would be created for firms way outside the area of interest to be placed into the treatment and control groups. For this reason, these areas were deleted from the data. An example of these errors in the data for the city of Den Haag are provided in the figures in Table 1, where the arrows point to random (small) dots that were deleted to the LEZ and the (small) empty white areas in the LEZ were added into the LEZ. Similar adjustments were made for all the five cities.
- Precise adjustments of the borders were made to exactly recreate the provided maps by each of the five municipalities. Some borders of sections of the zones were correctly in their form however were slightly skewed in their placement. Examples for this were borders being at the opposite side of the river in Rotterdam causing the control group to be in the south of the city, logically causing significant difference in the treatment and the control group. Another example of incorrect borders is the case where the border is being located across the highways in Den Haag. Without adjusting these small differences, the treatment and control group would be moved with a small but significant number of meters. This would have caused incorrect placement of firms in the treatment or control groups. The exact line of the provided maps by the municipalities was followed and all border points were laid exactly on this line.

After all the data manipulation, the then edited and correct dissolved version of the LEZ was used to create two different buffers. The first buffer was one with a positive range outside the dissolved LEZ. The second buffer was one with a negative range inside the dissolved LEZ. The difference between the outside buffer and the dissolved LEZ was taken to obtain the "outside barrier", which functions as the control group. The difference between the inside buffer and the dissolved LEZ was taken to obtain the “inside” barrier, which functions as the treatment group. The two now obtained barriers are joined together to form one layer, this gave this layer two categories, which were in the case of the current LEZ of. Den Haag “Den Haag current 250m control” and “Den Haag 250m current treatment”.

All five cities used similar processes, and in the case for Amsterdam, Rotterdam , and Den Haag two zones (current and original) were created. The first LEZ, the so called “current” LEZ was obtained as explained above. The, from now on called, “original” LEZ had to be determined based on old online newspaper articles that provided maps for these original zones. For Rotterdam maps provided by Promovendum (2015), Dijkhuizen (2009), Rijnmond (2015) and NKC (2015) were used. For Den Haag the previous mentioned article of Hoogland (2020) that provided the official map of the municipality was used. For Amsterdam no newspaper articles with a map of the LEZ were found, therefore this LEZ was based on multiple alternative online articles (Amsterdam Logistics, 2016; Public Space Info, 2015; Partij voor de Dieren Noord-Holland, 2018; Sustainable Amsterdam, 2018). Borders of these zones had to be drawn by hand in QGIS as none of these maps provided georeferenced data or postal codes. The current LEZ’s were therefore duplicated and with the vertex tools border points were manipulated into the original borders. All zones were merged into one final layer. With this layer, it is possible to merge the firm location file and identify which of these firms lay in the control or treatment group.

The process above is illustrated in Table 1 below. The Northwest quadrant provides the raw data of the LEZ provided by the data source after filtering for the city of Den Haag, the arrows point at the seemingly random spots added to the LEZ. The Northeast quadrant provides the unedited dissolved polygon of the LEZ, here it can be seen how this unedited version had seemingly missing parts inside the LEZ and several parts outside the LEZ which were not in the actual LEZ. The Southwest quadrant provides the edited dissolved polygon for the LEZ of Den Haag; the layer below shows which parts of the LEZ were deleted and where the borders had to be adjusted. The Southeast quadrant provides the final version, with the treatment group being in the red buffer, the control group in the blue buffer, the inner city in the green section and four example firm points.

Table 1. Process of creating the control and treatment groups of the LEZ

Step 1: Load in postal code date and filter on specific LEZ. 	Step 2: Dissolve data into a single polygon 
Step 3: Edit dissolved version into correct polygon according to municipalities website 	Step 4: create 2 buffers, one inside the LEZ and one outside of the LEZ 

Company specific data

The obtained LEZ's can be matched with the second source of data that contains the number of jobs and exact location of companies in these zones. The number of jobs will be considered the variable of interest, the locational data is needed to identify whether the specific firm is located in the treatment or control group. The data is obtained from LISA (National Information System of Jobs), which is an association that maintains both geographical data as social economical characteristics of companies in the Netherlands that offer paid work to either employees or paid owners. The data is obtained from regional collaboration between both public as private parties, combining the regionally obtained data to a countrywide dataset (Stichting LISA, n.d.-a; Stichting LISA, n.d.-b).

Data is needed for the five selected cities (Amsterdam, Den Haag, Eindhoven, Rotterdam, and Utrecht). The period for which the data is preferred is 2000-2022. With this period, it is first made possible to run pre-treatment checks, follow the movement in firm employment in the direct period the LEZ's were installed, the period after the installation and possible strengthening / loosening of regulations. So is it seen that in some LEZ's described above the first regulations started in 2007/2008 and were strengthened around 2015 and 2021. However, the data that was made available ranges from 2000-2017. This limits the scope of the research, as this means that for three of the LEZ's (Amsterdam, Den Haag, and Eindhoven) only the heavy traffic LEZ is subject to research. Rotterdam is the only LEZ that expanded its LEZ both by size and by regulations in this period, and for 2016 and 2017 also had a LEZ banning some personal vehicles inside the LEZ. Utrecht only expanded in its regulations, also banning personal cars from 2013 onwards. The data contained several variables for each of the individual companies for each year they were available in the dataset. As earlier mentioned, the variable of interest is the number of jobs at the company. Other economic variables available where the amount of full-time and part-time workers, the different industries of each company (on all four levels, as determined by the KVK) and a description of the industry the company is active in. Additionally, to locate the different companies geographic data was provided. It contained the official municipality name and code, postal codes, coordinates, and a LISA registration number which is unique to each company. The data was adjusted in several ways. First, the data was transformed to panel form. The year served as time variable. A company ID number was created based on the unique LISA numbers of the companies. This however caused one problem, duplicate company IDs in the dataset. This resulted from two different problems. The first problem arose from missing years for certain companies. As there was no way to identify these years, these companies got removed from the dataset. Secondly, a small overlap between the current and original zones caused companies to be listed in both zones for the same years. Here, the values of the company were kept in the zone they received their first treatment of the LEZ from, this was done by manually checking these firms. These adjustments resulted in panel data without duplicates.

Additionally, a binary variable for being in the treatment group was created based on the layer the observation was located in. Based on the, in the literature provided, implementation date of each LEZ a variable indicating whether an observation received treatment or not (receiving treatment if being in the treatment group after the implementation date) was created. For this variable it was important to both know the exact implementation date of the LEZ and the reference date of the survey of the LISA dataset. These dates are provided in the LISA manual (Stichting LISA, 2018). The first year of treatment is considered as the first year of measurement after the implementation date of the LEZ. This is provided in Table 2 below.

Table 2. Implementation dates, reference date for number of jobs and the subsequent first year of treatment for each LEZ

LEZ	Implementation date	Reference date number of jobs	First year of treatment
Amsterdam original	1-10-2008	Continuous	2008
Amsterdam current		Continuous	
Den Haag original	16-4-2008	1 January	2009
Den Haag current		1 January	
Eindhoven	1-7-2007	1 April	2008
Rotterdam original	1-9-2007	1 January	2008
Rotterdam current	1-1-2016	1 January	2016
Utrecht	1-7-2008	1 April	2009

Heterogeneity in effects on different type of companies could occur, so could it be argued that firms in the transport industry are more affected by the LEZ implementation than for example firms in the financial institutions category. For this reason, two additional categories are created. Firstly, the industry indicators provided by the dataset are further combined into the main 21 company industries as provided by the Dutch chamber of commerce and the Dutch central bureau of statistics (KVK & CBS, 2022). A list of these different categories is provided in Table 3 below. Secondly, categories for company size are created based on the number of jobs they provide (MKB), 2003). This categorization is provided in Table 4 below.

Table 3. Industry descriptions of each industry used in the heterogeneity analyses.

Industry ID	Industry descriptions	Industry ID	Industry descriptions
A	Agriculture, forestry, and fishing	L	Rental and trade of real estate
B	Mineral extraction	M	Consultancy, research, and other peculiar business services
C	Manufacturing	N	Rental of goods and other business services
D	Production and distribution of electricity, gas, steam, and cooled air	O	Public governance, governmental services, and required social insurances
E	Water, waste, wastewater management and sanitation	P	Education
F	Construction	Q	Healthcare and wellbeing
G	Retail and car reparations	R	Culture, sports, and media
H	Transport and storage	S	Other business services
I	Hospitality, food, and liquor	T	Households as employees; production of goods for own use
J	Information and communication	U	Extraterritorial organizations and bodies
K	Financial institutions		

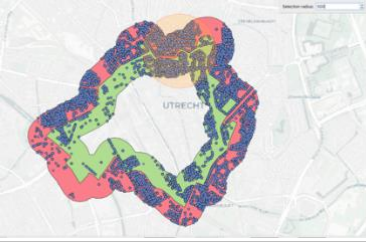
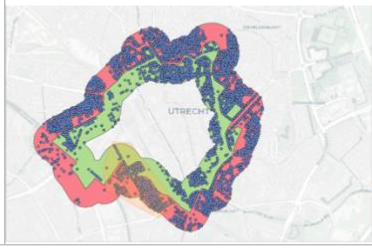
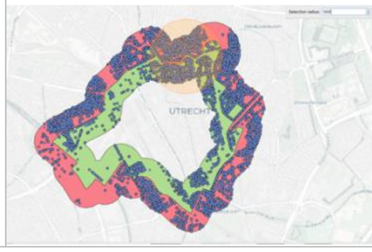
Table 4. Definition of company sizes

Size category	Number of jobs
1: micro businesses	Less than 10 employees
2: small businesses	Between 10 and 49 employees
3: medium sized businesses	Between 49 and 250 employees
4: large businesses	More than 250 employees

Finally, specific areas in each of the LEZ's were selected. Here, areas with similar patterns of companies in the treatment and control group were identified and selected. This was done by either using a certain radius around a point, or by hand drawing a polygon in a certain neighborhood. These areas can be identified as the yellow areas in the figures. The list of these zones and figures are provided in Table 5 below.

Table 5. Overview of the locations of the 12 different specific areas used in the heterogeneity analyses.

Amsterdam Northwest	Amsterdam Southwest	Den Haag Southeast
		
Den Haag Southwest	Eindhoven South-central	Eindhoven Southeast
		
Rotterdam current North	Rotterdam original Northeast	Rotterdam original Southwest
		

Utrecht Buiten Wittevrouwen	Utrecht Dichterswijk	Utrecht North-central
		

Descriptive statistics

With the data that has been described, it is important to look at some of the descriptive statistics of the variable of interest: jobs. Table 6 below provides an overview of the number of observations, the mean, the standard deviation, the minimum and maximum values for the total dataset of the 250-meter analyses and each of the different categories of the specifications. The complete dataset has close to 450.000 observations for approximately 95.000 unique companies. The average number of workers is 11 with a maximum number of workers being 5755. It is important to note that there is a difference in means between the control and treatment groups. This difference is important to be accounted for in the used method.

When looking at individual LEZ's, it can be noted that most observations are in Amsterdam, followed by Den Haag, Utrecht, Rotterdam original, Rotterdam current and Eindhoven. The LEZ of Eindhoven has the highest mean and the highest maximum, Amsterdam has the lowest mean, and Den Haag has the lowest maximum.

When looking at the different size categories it can be noted that most observations are in the micro size category, followed by the small, medium, and large categories. When looking at the different industries, most observations are in industry categories M, G, and R. The lowest number of jobs are in industry category T (with no jobs), B, and D. The highest average amount of jobs are in the industry categories O, D, and E. The lowest number of jobs are in the industry categories A, R, and G. The specific area's subject to research show that the specific areas of Utrecht North Central is double as large as the second, third and fourth largest areas that all have around 11.500 observations. The smallest area is Eindhoven South Central, with 2.771 observations. The means of the number of jobs are, largely looking, lower than those of the entire LEZ's. This could mean that the specific areas react differently to treatment than the wider city average. Table 7 and 8 provide more detailed looks at the total amount of jobs and observations for each year and industry category per city.

Table 6. Descriptive variables for the number of jobs in each different category used in the different analyses.

Subset of group in the data	Observations	Mean	Standard deviation	Min	Max
Total	447.042	10,77	78,92	0	5.755
Treatment group	269.257	9,20	61,27	0	4.161
Control group	177.785	13,16	99,83	0	5.755
Amsterdam	149.822	9,23	66,24	1	3.084
Den Haag	80.533	10,97	84,52	0	2.748
Eindhoven	34.491	15,19	127,77	0	5.755
Rotterdam current	43.054	14,92	102,49	0	3.589
Rotterdam original	61.916	13,20	75,81	0	4.161
Utrecht	77.226	7,33	47,87	0	2.619
Size: micro	393.698	1,96	1,75	0	9
Size: small	39.475	20,30	10,10	10	49
Size: medium	10.806	102,70	51,53	50	250
Size: large	3.063	696,42	622,05	251	5.755
Industry A	2.092	1,60	2,71	1	66
Industry B	296	7,71	14,91	1	126
Industry C	12.271	17,36	193,31	0	5.755
Industry D	321	123,71	331,32	0	2.067
Industry E	477	62,17	195,31	1	1.383
Industry F	19.389	7,42	54,80	0	1.674
Industry G	68.228	5,222	17,87	0	823
Industry H	12.208	23,38	124,51	0	2.687
Industry I	24.295	7,37	15,64	0	471
Industry J	37.658	8,23	51,42	0	1.641
Industry K	11.719	34,86	162,85	0	2.934
Industry L	9.379	6,93	20,72	0	485
Industry M	115200	6,00	38,79	0	2.495
Industry N	23.106	12,85	84,69	0	4.161
Industry O	3.000	252,49	429,98	0	3.589
Industry P	17.587	17,96	105,29	0	2.879
Industry Q	30.772	15,08	93,50	0	2.748
Industry R	39.997	3,47	20,64	0	820
Industry S	18.167	5,63	20,58	0	474
Industry T	0	-	-	-	-
Industry U	921	16,25	33,48	1	246
Amsterdam Northwest	9.099	5,35	45,44	1	1.865
Amsterdam Southwest	5.848	13,52	52,92	1	979
Den Haag Southeast	11.650	2,88	6,57	0	140
Den Haag Southwest	9.864	3,48	13,59	0	397
Eindhoven South-central	2.771	6,78	24,09	0	303
Eindhoven Southeast	7.047	3,17	7,93	0	195
Rotterdam current North	4.835	10,00	111,79	0	2.687
Rotterdam original Northeast	11.499	6,23	25,83	1	1.184
Rotterdam original Southwest	11.413	6,40	38,89	0	1.354
Utrecht Buiten Wittevrouwen	5.961	4,99	12,42	0	250
Utrecht Dichterswijk	4.696	4,623	22,25	0	502
Utrecht North-central	22.363	3,32	9,41	0	318

Table 7. Number of jobs and companies per year for each individual LEZ

	Amsterdam		Den Haag		Eindhoven		Rotterdam current		Rotterdam original		Utrecht	
Year	Jobs	Companies	Jobs	Companies	Jobs	Companies	Jobs	Companies	Jobs	Companies	Jobs	Companies
2000	65753	4825	44425	3242	27256	1359	32962	1599	48445	2805	29761	2945
2001	68512	5205	44569	3287	26672	1328	33731	1603	50803	2806	29470	3026
2002	69068	5273	46389	3386	26177	1353	35689	1644	49543	2818	31286	3096
2003	70654	5413	48637	3398	26368	1352	35678	1682	49559	2840	29687	3131
2004	69541	5406	48179	3427	28708	1349	33739	1713	49520	2810	29659	3168
2005	74439	5990	48091	3602	28892	1423	34484	1749	42146	2743	28721	3316
2006	71168	6396	48410	3887	29014	1465	35095	1761	42969	2757	28665	3461
2007	72253	6664	49657	4195	29755	1567	36148	1892	43595	2806	30161	3719
2008	75818	7048	50470	4541	30394	1598	37870	2045	42661	2972	30675	3902
2009	80015	8089	50222	4847	29212	1797	33240	2294	41149	3235	31027	4234
2010	79291	9003	49799	5013	28538	1974	35020	2623	43202	3641	30876	4545
2011	79594	9606	49594	4988	29929	1998	35917	2785	43292	3830	31226	4941
2012	79836	9825	48439	4973	29986	2225	37070	2937	43474	3969	32463	5087
2013	83381	10380	48830	5124	29481	2416	35863	2993	43608	3979	32283	5268
2014	82554	11089	50456	5413	29413	2598	35792	3142	44625	4148	33009	5548
2015	83830	12067	51502	5611	30251	2729	36690	3194	45265	4228	36046	5776
2016	87663	13438	51636	5693	31663	2908	38043	3528	45288	4589	35510	5987
2017	89683	14105	54074	5906	32338	3052	39413	3870	48136	4940	35892	6076
Total	1383053		883379		524047		642444		817280		566417	

Table 8. Number of jobs and companies per industry for each individual LEZ

	Amsterdam		**Den Haag**		**Eindhoven**		**Rotterdam current**		**Rotterdam original**		**Utrecht**	
Industry	Jobs	Companies	Jobs	Companies	Jobs	Companies	Jobs	Companies	Jobs	Companies	Jobs	Companies
A	41	39	2988	1846	37	25	147	111	113	47	27	24
B	733	180	456	45	0	0	19	9	1073	62	0	0
C	37150	4510	7926	1793	123108	1300	28878	1346	7676	1505	8298	1817
D	24936	142	486	32	5889	63	1252	38	6638	40	508	6
E	22912	155	2100	93	276	19	2588	95	1471	98	306	17
F	39230	6168	12868	5265	16415	1987	52396	2366	8559	1302	14486	2301
G	107608	22227	50958	13469	49722	5827	36356	6354	72080	11230	39481	9121
H	83706	5471	31654	1197	24980	542	77866	2191	58771	2218	8502	589
I	46588	4444	28484	5733	9653	1170	14094	2223	50847	6279	29285	4446
J	118108	15318	56773	4067	25953	3000	42297	3443	38084	4404	28697	7426
K	86564	5728	48344	1530	83479	982	19180	940	97496	1650	73438	889
L	15145	2916	14956	2349	5208	790	7153	741	15679	1576	6842	1007
M	252932	41146	86172	16180	51223	9795	89599	10683	120266	14363	90798	23033
N	113849	7492	28890	4082	31660	2011	25937	2079	65271	3988	31239	3454
O	188604	665	275297	896	27641	136	117555	446	66559	357	81807	500
P	54461	5361	60310	2575	18811	1282	80931	2102	52575	1964	48758	4303
Q	138388	8844	103382	6078	35110	2064	26322	3197	103295	4064	56911	6475
R	29610	14421	18963	7627	10032	2230	14940	3364	38060	3696	27133	8659
S	22444	4583	37421	4758	4850	1268	4934	1326	12767	3073	19901	3159
T	0	0	0	0	0	0	0	0	0	0	0	0
U	12	3	14951	918	0	0	0	0	0	0	0	0
Total	1383021		83379		524047		642444		817280		566417	

Methodology

Main idea of Difference-in-Difference

For the evaluation of the effect of implementing a LEZ inside a city, the effect of moving the treatment group from pre-treatment to post-treatment is needed. More traditional econometric methods fall short in identifying the causal effect of the treatment. A simple before-after comparison is not possible due to the time trends in the data and so does not account for time varying characteristics. An OLS regression is not possible due to the missing of control variables in the dataset. An RDD/ BDD as proposed in Bosker et al. (2019) also faces issues. This method does not account for time invariant characteristics that differ between the control and treatment group, as is the case with the difference in means of the amount of jobs in the 250-meter analysis. In this setting with a clear treatment and control group, and a clear pre-and post-treatment before and after the implementation of the LEZ's, a difference-in-difference model would be best suited. The main idea of a DiD model is that the difference of outcomes between the treatment and control groups after the treatment, subtracted by the original difference between these groups provide a causal treatment effect (under, later described, assumptions). By controlling for the original difference between the treatment and control, time invariant differences are accounted for. By controlling for the time trend (under, later described, parallel trends assumption), time varying characteristics are also accounted for.

This method was first used to identify whether Cholera was transmitted through water or air in 19th century in London and moved in the early 20th century to the economics. This shows that the method is highly intuitive and does not need any complicated computations.

Under a potential outcomes framework, the Average Treatment Effect of the Treated (ATET) can be defined as the below expression, with Y being the outcome and D being in the treatment group (1) or not (0). The following expressions come directly from Lechner (2010).

$$ATET_t = E(Y_t^1 - Y_t^0 \mid D = 1) = E[E(\, Y_t^1 - \, Y_t^0 \mid X = x, D = 1) \mid D = 1]$$

$$ATET_t = \, E_{X \mid D=1}\, \delta_t(x)$$

Assumptions of Difference-in-Difference

The DiD method needs several assumptions to provide an unbiased estimate of the ATET. The first assumption is the Stable Unit Treatment Value Assumption (SUTVA). This assumption implies that there are no relevant interactions between members of the control and treatment groups are observed, meaning that one of the potential outcomes is observed. Whether this assumption is likely to hold will be discussed in the results section. The second assumption of the method is the exogeneity assumption, assuming that components of the control variables are not influenced by the treatment variables. This assumption holds as there is no use of control variables in the model. Thirdly, it is assumed that the treatment did not have any effect on the pre-treatment population. Finally, the common trend (or otherwise called parallel trends) assumption assumes that without any treatment, both the control and the treatment groups would have the same trend (conditional on the control variables). This also means that the trends of both groups are similar in the pre-treatment period(s). Whether the last assumption will hold is discussed in each of the specific results section.

Regression formulas

Lechner (2010) uses the mathematical expressions of the above provided assumptions to provide the desired treatment effect, notated within the potential outcome framework. This is provided in the below expression. Here δ represents the treatment effect, Y the outcome, X the covariates and D whether being in the treatment group or not.

$$\delta_1(x) = [E(Y_1 | X = x, D = 1) - E(Y_0 | X = x, D = 1)]$$
$$-[E(Y_1 | X = x, D = 0) - E(Y_0 | X = x, D = 0)]$$

These expressions, with help of the assumptions, can then be transformed into Ordinary Least Square regressions. A simplified version of the 2x2 model (2 groups, 2 time periods) of this regression function is provided below (Albouy, n.d.). Here, α represents the const term, β the fixed effect of being in the treatment group (compared to being in the control group), γ the time fixed effect, δ the treatment effect, and ε the error term. So, this regression contains group and time dummies for main effects and an interaction variable of being in the treatment group and being in the post-treatment period capturing the main effect.

$$Y_i = \alpha + \beta D_i + \gamma t_i + \delta(D_i * t_i) + \varepsilon_i$$

The main advantage of this formulation is that it is relatively easy to obtain and compute. Another advantage is that it is relatively easy to extend to multiple time periods. Additional years before the intervention can help verify the common trends assumption and additional years after the intervention can take dynamics of potential treatment effects into account. The formulation of this so called TWFE (Two Way Fixed Effects) event study specification is provided below (Library of Statistical Techniques, n.d). Here Y represents the outcome, α represents the constant, y the effect of the leads, δ the treatment effect, β the parameter for the covariates, θ and ϕ the time and group fixed effects, and ε the error term. The leads (number of years before intervention) are indicated by q and m indicates the lags (number of years after intervention.

$$Y_{it} = \alpha + \sum_{t=-q}^{-1} y_t D_i + \sum_{t=0}^{m} \delta_t D_i + \beta X_{ti} + \phi_t + \theta_i + \varepsilon_{i,t}$$

The above form of specification will be used in the further section of this thesis. An aggregate regression of all 6 zones will be performed. However, due to heterogeneity between effects for different cities and different rules of the LEZ, this result is prone to bias. Therefore, each city will be analyzed individually. As previously mentioned in the data section, heterogeneity issues between different areas of the city might still bias the result, further specification analyses are performed. These are based on company size, company industry and several specific areas inside the cities.

Approach of time periods

There are several ways to approach this regression in statistical software. The first way is to divide the time periods in the pre-and post-treatment periods, with the then time variable (t=0,1) having either the value 0 or 1 for being in the pre-and post-treatment group respectively, resembling the 2x2 framework. By interacting this value with the value for being in the treatment group or not (D_i=0,1) a regression can be created with the estimated coefficient being the treatment effect of the entire post-treatment period. This way of performing the regression however has several downsides. The first being not being able to identify any heterogeneity in effects over time. It could for example be possible that the treatment only has an effect after a few years, or that it fades out after time. Additionally, changing guidelines can cause different effects over time. An example is the LEZ of Utrecht that, throughout the period of analyses, expands its LEZ to personal vehicles. With dividing the sample in two periods, it becomes no longer possible to identify any differentiation in effects. The second downside is the testing of the parallel trends assumption. To test this assumption, the regression must obtain the leads, as is provided in the formula. To add this to the regression, the future value of the interaction term between the time and treatment group must be created. Adding one future value only tests whether the assumption holds for the last year before treatment. By adding multiple future values, multiple years back can be checked solidifying the testing of this assumption. However, from adding multiple future values, the most recent years from the post-treatment value gets lost from the regression, this as for the first future value the value of the final year cannot be determined, and for the second future value the latest two values cannot be determined. This creates a trade-off between a further tested parallel trends assumption and the loss of data in the post-treatment period.

The second way of performing this regression is instead of creating a pre-and post-treatment dummy indicator, is by interacting the variable identifying the control and treatment group (D_i=0,1) directly by the individual time periods (t=2000, 2001, 2002…….. 2017). The performed regression in the statistical software will thus include the effect of being in the treatment group, the yearly effects, and the interaction effects of each individual year and being a part of the treatment group. The estimated coefficients of the interaction term of being in the treatment group with the years before treatment indicate whether the parallel trends assumption holds, as here this interaction effect should not influence the outcome. The estimated coefficients of the interaction of the years after the treatment indicate a year-by-year estimate of the possible causal effects. With this method, both problems of the first method are solved, as now year-by-year effects can be identified, and the full sample is utilized in testing the parallel trends assumption and with identifying causal effects.

For this reason, it is opted to perform the last approach for the main results of this thesis. However, as the two approached should not inherently differ, the first approach will be performed to solidify the main results. In this check, for five out of the six LEZ's it is chosen to use three future values, allowing for a strong check of the parallel trends assumption without losing too much data in the post-treatment period. For the current Rotterdam LEZ this is not possible, with only two periods in the post-treatment period. For this reason, this LEZ only applies one future value to check the parallel trends assumption.

Issues with difference-in-difference

Recent literature however presents several issues with this form of performing a DiD design for multiple time periods. The first main issue is an inferencing issue. Lechner (2010) describes this as an issue regarding correlation not just over time but also between groups, a so-called group-time specific error term. This issue was first raised in Bertrand et al. (2004), who state how using standard OLS standard errors cause severe overestimation of the standard error and in result higher amounts of rejection of the null hypothesis. This is however only valid under the assumption that in absence of the treatment, both the treatment and control group would experience the exact same change in outcome. This is however very unlikely. The authors found that after extensive placebo testing, 45% of the null hypothesis tests were rejected, while under a 5% confidence interval this could only be 5%. Reasons for DiD designs to be subject to this autocorrelation problems could be the 1) reliance on long time series, 2) serially correlated dependent variables, and 3) rarely changing treatment variable over time. Three main solutions to this problem are available, the first being the use of so-called block bootstrapping, the second option aggregating the time series into pre-and post-treatment periods and lastly the use of clustered standard errors. In the case of this analysis, this is only possible for the overall regression and not for all other regressions which are for a specific city as these only have two groups (treatment and control). As there already is a specification check with a 2x2 framework, this can also be seen as a check for this issue for the main results.

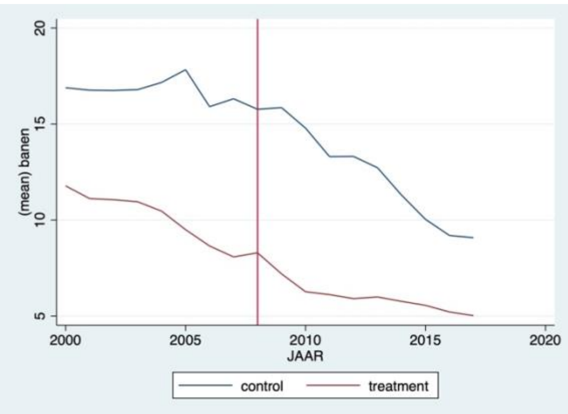
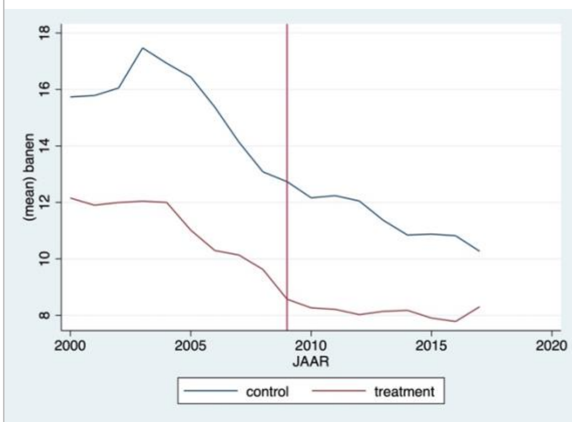
A secondary, even more recent, issue with the performed method is the TWFE event study specification of the DiD model being biased under staggered circumstances (meaning when there are different moments in the implementation of treatment across groups). Literature such as de Chaisemartin & D'Haultfoeuille (2022) explain how effects are misleading if treatment effects are heterogenous over time and between groups. Goodman-Bacon (2018) mention how the estimate averages heterogeneity and if effects change over time this can lead to bias. Sun and Abraham (2021) mention how in a staggered setting the coefficients of leads and lags can be affected by effects of other periods and even a pre-trend can arise due to this heterogeneity. Callaway and Sant'Anne (2021) even go as far as developing a new estimator because of this issue. However, in the already proposed setting above, the only analysis with staggered data is in the combined analyses of all the LEZ's. As this analysis is already known to have heterogeneity issues due to difference in regulations between LEZ's and only serves as starting point of further analyses, it is not within the scope of this thesis to develop a further model to solve the bias within this analysis. The individual city specific analyses all have one timing of treatment, and so are not staggered, this issue of bias is no problem and therefore needs no additional change in model. The only way the sample differs in treatment is with differentiating levels of treatment through time with the LEZ's become stricter. There is however no mention of this in the literature, making it not needed (let alone possible) to check for this difference in level of treatment.

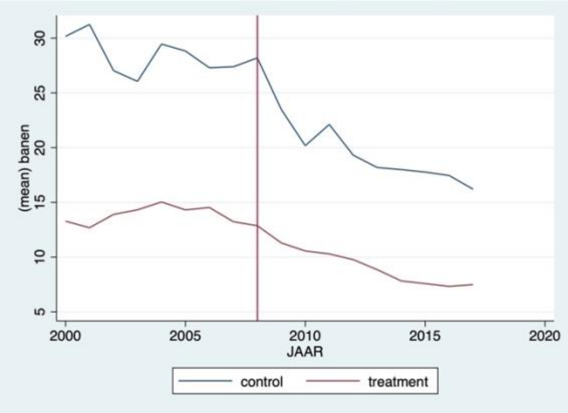
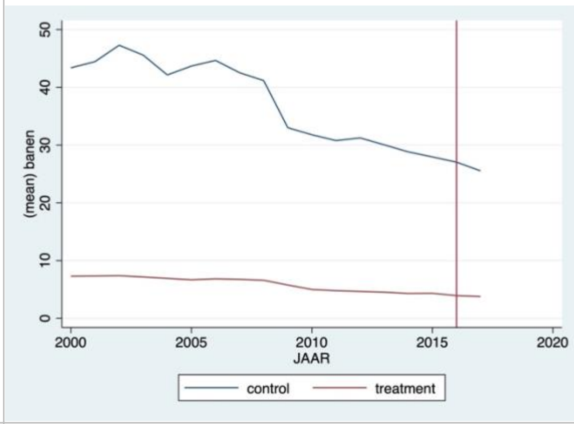
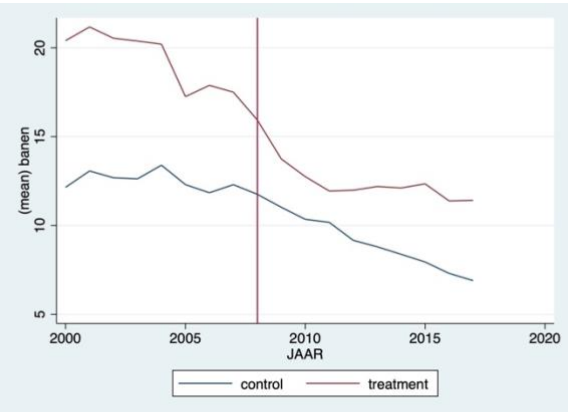
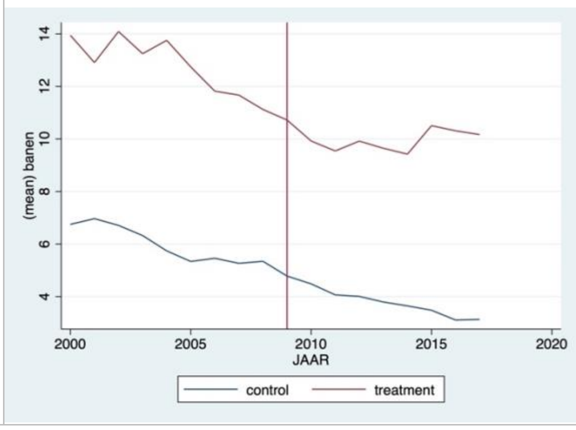
Results

Full sample and individual Low Emission Zones

In Table 9, figures are provided for each of the six individual cities. These figures show the average amount of jobs of a firm in the city over time from the complete data period. The two lines depict the treatment and control group, the red vertical line shows the first year of treatment. Left from this line the pre-treatment period can be seen, in which the parallel trends assumption must hold. Right from this line, the post-treatment period is can be seen, in which any treatment effects must be visible in case of a causal effect. A brief description of each graph is provided in the table.

Table 9. Difference-in-difference graphs for each individual LEZ.

Amsterdam: no clear causal effect can be seen in the graph. The control group has an initial higher number of average jobs per firm, with a decrease of the number of jobs that is observed over time. The parallel trends assumption looks like it is likely to hold. The difference between the two groups seem to become smaller over time and after treatment took place.	Den Haag: no clear causal effect can be seen in the graph. The control group has an initial higher number of average jobs per firm, with a decrease of the of the number of jobs that is observed over time. The parallel trends assumption looks like it is likely to hold. The difference between the two groups seem to become smaller over time and after treatment took place.
	
Eindhoven: no clear causal effect can be seen in the graph. The control group has an initial higher number of average jobs per firm, with a decrease of the number of jobs that is observed over time. The parallel trends assumption looks like it is likely to hold. The	Rotterdam current: no clear causal effect can be seen in the graph. The control group has an initial higher number of average jobs per firm, with a decrease of the number of jobs observed over time for the control group. The parallel trends assumption does not look

difference between the two groups seem to become smaller over time and after treatment took place	like it is likely to hold. The difference between the two groups seem to become smaller over time.
	
<u>Rotterdam original:</u> no clear causal effect can be seen in the graph. The treatment group has an initial higher number of average jobs per firm, with a decrease of the number of jobs observed over time. The parallel trends assumption does not look like it is likely to hold. The difference between the two groups seem to become smaller over time but is increasing in the last 5 years.	<u>Utrecht</u>: no clear causal effect can be seen in the graph. The treatment group has an initial higher number of average jobs per firm, with a decrease of the number of jobs observed over time. The parallel trends assumption looks like it is likely to hold. The difference between the two groups does not seem to become smaller over time and is increasing in the last 5 years.
	

However, from these graphs no causal effects can be determined, nor can the parallel trends assumption be statistically verified. Additionally, due to the high number of regressions the next sections will not provide any similar graphs. For these next sections the results will be provided in regression table form. These regression tables allow for statistical analysis of the parallel trend assumption, and year by year effects of the interventions with standard errors allowing for hypothesis testing. In Table 10, the results of the regressions are partly provided. It is noted that none of the year fixed effects are presented, as this is not subject to research. The full tables, with full results of all the regressions run in this thesis (including checks) can be found in the additionally provided log file. In Table 10, column 1 provides the DiD results for the full data sample of all 6 cities. Columns 2 through 7 provide the results for the 6 individual cities. It can be seen that for some of the cities significant differences are present between the treatment and control group (Amsterdam, Eindhoven, Rotterdam current and original, and Utrecht). This indicates that the treatment and control groups are not similar, and a simple before-after comparison between these groups does not provide estimates of a causal effect. However, as clearly visible in the table, only twice a significant effect is found of the interaction effect of being in the treatment group and the corresponding year. The first is found for the overall regression in column 1 for the year 2001. This is however not a causal effect as this is before the implementation and can be seen as a violation of the parallel trends assumption. The other significant effect is for original Rotterdam zone in 2011 in column 6. As this is the only significant effect found, it is likely that this is spurious instead of an indication of a causal effect. Therefore, it can be concluded that for these analyses no causal effect for the implementation of a LEZ on the number of jobs within specific distance of the LEZ border can be found. However, as previously mentioned, heterogeneity issues can play a part in this analysis. For this reason, a heterogeneity analyses on size, industry, and specific areas of the cities are assessed in the next sections.

The results also further prove why the choice for a RDD / BDD as in Bosker et al (2019) was rightfully not made, but instead the adaptation to a difference-in-difference was correct. As it can be seen in Table 9 and 10, for most cities the treatment and control group do differ significantly in the average amount of jobs, and as seen in the graphs this difference existed far before the LEZs were implemented. This initial difference biases the RDD/BDD.

Table 10. Regression results for all zones combined and for each individual LEZ.

VARIABLES	(1) All Zones	(2) Amsterdam	(3) Den Haag	(4) Eindhove n	(5) Rotterdam current	(6) Rotterdam original	(7) Utrecht
T	-3.587	-5.110*	-3.579	-16.89*	-36.07***	8.255**	7.198***
	(4.809)	(2.692)	(3.237)	(9.116)	(7.814)	(3.387)	(2.121)
T#2001	-0.608***	-0.533	-0.307	-1.677	-1.046	-0.150	-1.256
	(0.126)	(3.619)	(4.606)	(12.99)	(10.95)	(4.733)	(2.954)
T#2002	-0.297	-0.579	-0.475	3.750	-3.826	-0.403	0.182
	(0.470)	(3.581)	(4.593)	(12.52)	(11.23)	(4.613)	(3.050)
T#2003	-0.541	-0.726	-1.851	5.156	-2.337	-0.499	-0.276
	(0.591)	(3.604)	(4.699)	(11.89)	(11.24)	(4.548)	(2.963)

T#2004	-0.585	-1.591	-1.350	2.469	0.859	-1.441	0.816
	(0.704)	(3.632)	(4.737)	(12.26)	(11.10)	(4.740)	(2.961)
T#2005	-1.680	-3.216	-1.846	2.371	-0.926	-3.304	0.208
	(1.042)	(3.807)	(4.553)	(12.44)	(11.31)	(4.303)	(2.847)
T#2006	-1.230	-2.143	-1.500	4.123	-1.741	-2.212	-0.836
	(0.900)	(3.610)	(4.437)	(12.37)	(11.35)	(4.339)	(2.788)
T#2007	-1.381	-3.124	-0.422	2.737	0.302	-3.050	-0.793
	(1.164)	(3.584)	(4.310)	(12.85)	(11.02)	(4.325)	(2.758)
T#2008	-1.260	-2.363	0.122	1.559	1.509	-4.077	-1.418
	(1.162)	(3.529)	(4.151)	(13.31)	(10.64)	(4.227)	(2.735)
T#2009	-1.179	-3.547	-0.585	4.700	8.835	-5.537	-1.258
	(1.877)	(3.518)	(4.067)	(11.69)	(9.657)	(4.085)	(2.672)
T#2010	-1.119	-3.408	-0.320	7.272	9.293	-5.854	-1.759
	(1.937)	(3.439)	(4.026)	(10.92)	(9.558)	(3.974)	(2.602)
T#2011	-0.838	-2.074	-0.448	5.065	10.09	-6.493*	-1.719
	(1.873)	(3.312)	(4.054)	(11.63)	(9.404)	(3.913)	(2.591)
T#2012	-0.574	-2.293	-0.447	7.347	9.492	-5.436	-1.286
	(1.848)	(3.318)	(4.030)	(11.29)	(9.513)	(3.873)	(2.554)
T#2013	-0.000590	-1.617	0.355	7.559	10.57	-4.863	-1.345
	(1.933)	(3.265)	(3.987)	(11.21)	(9.441)	(3.809)	(2.469)
T#2014	0.455	-0.423	0.909	6.718	11.56	-4.521	-1.421
	(1.925)	(3.177)	(3.937)	(10.93)	(9.349)	(3.791)	(2.449)
T#2015	0.925	0.632	0.605	6.700	12.46	-3.858	-0.173
	(1.933)	(3.059)	(3.934)	(10.84)	(9.506)	(3.782)	(2.615)
T#2016	0.990	1.130	0.539	6.757	12.96	-4.176	0.00527
	(1.982)	(2.965)	(3.918)	(10.88)	(9.403)	(3.722)	(2.633)
T#2017	1.225	1.052	1.615	8.169	14.32	-3.737	-0.164
	(2.039)	(2.947)	(3.874)	(10.77)	(9.225)	(3.698)	(2.605)
Constant	16.93***	16.89***	15.74***	30.17***	43.38***	12.15***	6.750***
	(3.816)	(2.407)	(2.519)	(8.963)	(7.723)	(1.343)	(0.786)
Observations	447,042	149,822	80,533	34,491	43,054	61,916	77,226
R-squared	0.002	0.003	0.001	0.003	0.020	0.003	0.005

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Clustered standard errors are used for column 1), standard errors are presented in parentheses with robust standard errors are used for columns 2)-7). Significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Size of companies

For the size heterogeneity analysis, per LEZ four regressions are ran. One for the micro, small, medium, and large company size subsample. Appendix B provides the results of all interaction effects for each of these regressions per individual LEZ (note that here the year fixed effects are left out and can be found in the separately provided log file). In this section, only the regressions with significant results will be provided and discussed. These regression results are provided in Table 11.

In column 1 of Table 11 it can be seen that from 2014 onwards, receiving treatment has a positive and a significant effect on the number of jobs for micro-companies in Amsterdam.

These effects indicate a possible lagged effect of the implementation of the LEZ. As none of the leads are significant, it is to be assumed that the parallel trends assumption holds.

In column 2 of Table 11 it can be seen that for 2010-2014 and 2017 receiving treatment has a negative and significant effect is found on the number of jobs for medium-sized companies in Amsterdam. These effects indicate that the effects of the LEZ implementation are lagged one year for medium sized companies in Amsterdam. As none of the leads are significant, it is to be assumed that the parallel trends assumption holds. For small and large companies, no significant effects are found in Amsterdam.

In column 3 of Table 11 it can be seen that from 2007-2015, receiving treatment has a positive and a significant effect on the number of jobs for micro-companies in Den Haag. These effects indicate a possible lagged effect of the implementation of the LEZ. However, as determined earlier the first year of treatment of the data for Den Haag is 2009. This means that due to the significant leads of 2007 and 2008 the parallel trends assumption does not hold, and no causal effects can be determined from these results. It could very well be possible that the signing of the covenant in 2006 caused an anticipation effect, however there is no way to causally determine this. For small, medium, and large companies, no significant effects are found in Den Haag.

For the city of Eindhoven, none of the different size categories provide significant results. This indicates that there are no causal effects of the implementation of the Eindhoven LEZ on the number of jobs within the city.

In column 4 of Table 11 it can be seen that for 2015 and 2017, receiving treatment has a positive and significant effect on the number of jobs of micro-companies in the current Rotterdam LEZ. These effects indicate a possible effect of the implementation of the LEZ, however the first year of treatment of the current Rotterdam LEZ is 2016. This means that due the significant lead of 2015, the parallel trends assumption does not hold. Similar to the result for micro-companies in Den Haag, it could be possible that an anticipation in the year prior of implementation caused an effect. This makes it not possible to determine a causal effect for micro-companies in the current Rotterdam LEZ.

In column 5 of Table 11 it can be seen that for 2004-2006 and 2011-2017 the interaction effect of being in the treatment group with the year has a negative and significant effect on the number of jobs for micro-sized companies in the original Rotterdam LEZ. As the parallel trends assumption does not hold (due to significant leads in 2004-2006), no causal effects of the implementation of the LEZ on the number of jobs can be determined. Further, it is seen that even after the removal of the original LEZ, significant effects can be seen in 2017.

In column 6 it can be seen that for small companies in the original Rotterdam LEZ positive and significant effects on the number of jobs are present in 2014 and 2016. These effects are however seriously lagged, with no effects being present after implementation in 2008. For this reason, it is not reasonable that these effects can be a causal effect of the implementation of the LEZ. For small, medium, and large companies, no significant effects are found in the current Rotterdam LEZ. For medium and large companies, no significant effects are found in the original Rotterdam LEZ.

In column 7 of Table 11 it can be seen that for small companies in the Utrecht LEZ positive and significant effects are found for the 2008-2011 period. The first year of treatment for this zone was 2009, indicating that the parallel trends assumption does not hold. Similarly, to the case of micro companies in Den Haag, it could very well be possible that the signing of the covenant in 2006 caused an anticipation effect, however there is no way to causally determine this.

In column 8 of Table 11 it can be seen that for medium companies in Utrecht a positive and significant effect of the interaction term for 2015 is found. As this is the only value with a significant effect is found, with this effect also being seriously lagged after the implementation of the LEZ in 2009, it is unlikely this is a causal effect. For micro and large companies, no significant effects are found in the Utrecht LEZ.

To conclude, for the heterogeneity in company sizes per individual LEZ, no strong causal effects of the implementation of a LEZ on the number of jobs can be determined. Most regressions performed provide insignificant results, and when significant effects are found these are either troubled by the violation of the parallel trends assumption, or by seemingly random timing of effects. The following sections will provide further heterogeneity analyses in industries and within specific smaller areas inside cities.

Table 11. Regression results for the size heterogeneity analyses.

VARIABLES	(1) Amsterdam Micro	(2) Amsterdam Medium	(3) Den Haag Micro	(4) Rotterdam Current Micro	(5) Rotterdam Original Micro	(6) Rotterdam Original Small	(7) Utrecht Small	(8) Utrecht Medium
T	-0.505***	14.62**	-0.240***	-0.947***	0.487***	-1.489	-0.301	-3.426
	(0.0651)	(7.101)	(0.0778)	(0.130)	(0.0887)	(1.026)	(1.268)	(13.13)
T#2001	-0.0468	-2.266	0.0765	0.0590	-0.141	-1.185	0.142	-4.633
	(0.0894)	(9.882)	(0.111)	(0.181)	(0.130)	(1.461)	(1.732)	(18.65)
T#2002	-0.0349	3.240	0.0896	-0.0780	-0.143	0.193	-0.361	-13.27
	(0.0888)	(9.921)	(0.108)	(0.182)	(0.129)	(1.444)	(1.753)	(18.55)
T#2003	0.00925	1.737	0.0591	-0.136	-0.193	0.898	1.448	-13.59
	(0.0881)	(10.42)	(0.108)	(0.182)	(0.129)	(1.463)	(1.811)	(18.02)
T#2004	-0.0622	-3.436	0.0665	0.00450	-0.304**	0.194	1.681	-20.77

	(0.0879)	(10.46)	(0.106)	(0.180)	(0.129)	(1.471)	(1.800)	(17.98)
T#2005	0.0302	-3.145	0.116	0.171	-0.261**	-1.019	1.356	-8.673
	(0.0855)	(10.22)	(0.104)	(0.180)	(0.129)	(1.511)	(1.810)	(18.80)
T#2006	-0.000621	-0.880	0.143	0.0853	-0.238*	-0.0996	0.131	-9.759
	(0.0841)	(10.04)	(0.101)	(0.183)	(0.128)	(1.477)	(1.793)	(18.23)
T#2007	0.0227	-0.725	0.198**	0.0744	-0.170	1.687	1.977	-17.63
	(0.0839)	(10.38)	(0.0993)	(0.177)	(0.128)	(1.435)	(1.780)	(18.05)
T#2008	0.0709	-13.84	0.266***	0.210	-0.138	0.827	3.205*	-1.687
	(0.0824)	(10.40)	(0.0973)	(0.173)	(0.124)	(1.432)	(1.771)	(18.18)
T#2009	0.0290	-16.44	0.216**	0.106	-0.184	0.289	4.137**	2.602
	(0.0800)	(10.27)	(0.0947)	(0.170)	(0.119)	(1.478)	(1.760)	(18.05)
T#2010	0.0678	-17.17*	0.237**	0.126	-0.172	1.341	3.191*	5.103
	(0.0781)	(10.23)	(0.0932)	(0.164)	(0.115)	(1.432)	(1.768)	(18.89)
T#2011	0.0782	-22.98**	0.194**	0.229	-0.199*	1.448	3.072*	2.278
	(0.0769)	(10.11)	(0.0928)	(0.159)	(0.113)	(1.422)	(1.716)	(18.75)
T#2012	0.0758	-22.18**	0.232**	0.189	-0.251**	1.312	2.310	1.445
	(0.0765)	(10.03)	(0.0925)	(0.159)	(0.111)	(1.451)	(1.651)	(18.91)
T#2013	0.118	-18.80*	0.252***	0.210	-0.306***	1.701	2.041	-21.97
	(0.0757)	(9.947)	(0.0915)	(0.158)	(0.112)	(1.464)	(1.731)	(18.71)
T#2014	0.214***	-19.75*	0.225**	0.215	-0.304***	3.301**	1.533	-24.05
	(0.0742)	(10.58)	(0.0903)	(0.157)	(0.110)	(1.416)	(1.639)	(18.72)
T#2015	0.285***	-16.88	0.157*	0.267*	-0.315***	2.174	0.500	-30.76*
	(0.0728)	(10.61)	(0.0902)	(0.155)	(0.109)	(1.466)	(1.715)	(17.99)
T#2016	0.270***	-16.28	0.147	0.236	-0.333***	2.433*	0.709	-20.65
	(0.0718)	(10.03)	(0.0899)	(0.153)	(0.108)	(1.469)	(1.680)	(18.29)
T#2017	0.295***	-16.71*	0.222**	0.296**	-0.365***	1.908	2.724*	-12.45
	(0.0712)	(9.763)	(0.0885)	(0.149)	(0.105)	(1.437)	(1.635)	(18.52)
Constant	2.612***	90.13***	2.598***	3.115***	2.647***	21.31***	19.95***	111.8***
	(0.0552)	(4.806)	(0.0611)	(0.117)	(0.0649)	(0.859)	(0.962)	(10.36)
Observations	133,830	3,203	71,280	36,875	51,127	8,140	4,741	1,419
R-squared	0.028	0.012	0.025	0.060	0.033	0.006	0.013	0.030

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-8). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Industries

For each of the six LEZ's, analyses are made for the 21 different industries. This section will briefly go over the results of these analyses. For the industries A,B, C, D, E, K, L, P, Q, S, T ,and U no (significant) results were found. This either meant that there were insufficient observations of a certain industry in a specific LEZ, or that there were no causal effects to be found. It is noted that not for all industries in each city an analysis was possible. This is due to limited data on these specific industry in a specific city.

For industries I, J, M, and R significant effects are found in one of the six LEZ's are found. These results are presented in Table 12 below, in columns 1-4. For all these industries it can be seen that the parallel trends assumption holds. Additionally, effects only arise after 2010 and 2014, indicating that any potential effects are severely lagged.

For industries F, G, and N two of the six LEZ's have significant effects. However, in both cases for industries F and G the parallel trends assumption does not hold, similar to the result of industry N in Den Haag. These regressions can be seen in Appendix C. As presented in column 5 of Table 12 below, for industry N in Eindhoven, both significant effects are found, and the parallel trends assumption holds. This indicates that there is a negative effect on the number of jobs in this industry for the LEZ in Eindhoven.

For industry O, three of the six LEZ's provide significant results. For the current Rotterdam LEZ the parallel trends assumption does not hold, as can be seen in Appendix C. For Eindhoven a positive, and later a negative effect is found of the number of jobs within this industry. For Utrecht this effect does not change and remains positive over the years.

To conclude, no clear pattern can be witnesses in the results of the analyses of specific industries in the six different LEZs. Out of the total 120 different analyses, only seven result into significant results without a violation of the parallel trends assumption. This low amount falls within a 10% confidence interval, therefore suggesting that there are no causal effects of the implementation of a LEZ on the number of jobs in specific industries and the found significant effects are deemed to be spurious. All other results of the insignificant results are provided in a separate log file.

Table 12. Regression results for the industry heterogeneity analyses.

VARIABLES	(1) Rotterdam Original-I	(2) Utrecht-J	(3) Amsterdam-M	(4) Den Haag-R	(5) Eindhoven-N	(6) Eindhoven-O	(7) Utrecht-O
T	1.879	11.27*	-13.45**	2.982**	15.57	-79.12	-69.77
	(1.488)	(6.357)	(5.813)	(1.339)	(9.967)	(139.0)	(64.53)
T#2001	0.588	-5.151	6.010	-0.189	-9.242	41.12	-18.38
	(2.148)	(6.781)	(7.200)	(1.874)	(10.63)	(204.2)	(95.44)
T#2002	0.809	-5.525	3.379	-0.297	-13.45	232.4	-16.07
	(2.145)	(6.665)	(7.246)	(1.853)	(10.42)	(155.7)	(98.98)
T#2003	0.704	-6.921	4.808	-0.340	-14.13	248.6	-27.51
	(2.149)	(6.646)	(6.877)	(1.803)	(10.27)	(157.3)	(101.1)
T#2004	0.630	-6.730	5.647	-0.289	-45.96	244.5	96.23
	(2.128)	(6.644)	(6.744)	(1.761)	(34.87)	(158.7)	(94.53)
T#2005	-0.360	-8.463	0.884	-0.660	-44.49	278.3	134.1
	(2.214)	(6.921)	(8.337)	(1.754)	(31.43)	(179.7)	(94.31)
T#2006	-0.466	-10.02	0.996	-1.214	-47.87	296.3	52.83
	(2.220)	(6.951)	(8.432)	(1.679)	(33.29)	(196.0)	(99.88)
T#2007	0.237	-10.46	0.994	-1.232	-57.18	395.1	59.86
	(2.404)	(6.888)	(8.038)	(1.663)	(41.04)	(259.3)	(96.12)
T#2008	0.986	-9.346	0.425	-1.437	-49.30	396.3	0.747
	(2.465)	(7.402)	(8.262)	(1.648)	(31.86)	(258.8)	(134.3)
T#2009	0.518	-8.865	2.254	-1.429	-53.23	458.6	-6.556

	(2.343)	(7.285)	(7.668)	(1.624)	(34.82)	(296.0)	(148.2)
T#2010	0.696	-10.91	6.660	-1.703	-39.71*	387.5**	30.09
	(2.319)	(6.648)	(6.340)	(1.593)	(22.39)	(187.2)	(157.5)
T#2011	1.278	-11.55*	8.954	-1.789	-41.38*	326.4**	27.22
	(2.351)	(6.507)	(6.046)	(1.539)	(23.13)	(160.1)	(152.0)
T#2012	1.967	-11.84*	8.762	-2.006	-42.12*	321.6*	24.87
	(2.348)	(6.500)	(6.056)	(1.508)	(23.39)	(165.2)	(144.8)
T#2013	2.200	-11.21*	9.716	-2.287	-40.23*	328.9**	180.1**
	(2.331)	(6.463)	(5.949)	(1.436)	(22.57)	(165.5)	(82.73)
T#2014	3.634*	-11.83*	10.72*	-2.690*	-40.80*	1.125	170.0**
	(2.050)	(6.468)	(5.913)	(1.394)	(23.39)	(258.7)	(82.25)
T#2015	3.401*	-11.91*	11.44*	-2.700*	-41.32*	-51.95	205.0**
	(2.040)	(6.470)	(5.924)	(1.379)	(22.85)	(300.7)	(82.94)
T#2016	4.076**	-11.78*	11.18*	-2.631*	-38.12*	-392.1**	220.3**
	(2.078)	(6.488)	(5.906)	(1.394)	(19.64)	(176.4)	(86.12)
T#2017	3.285	-11.63*	11.93**	-2.670*	-31.54**	-398.9**	221.7**
	(2.004)	(6.503)	(5.878)	(1.392)	(15.94)	(177.9)	(86.84)
Constant	6.217***	1.495***	20.47***	1.468***	6.538***	170.5	149.3**
	(1.072)	(0.155)	(5.709)	(0.109)	(2.120)	(134.8)	(58.44)
Observations	6,279	7,426	41,146	7,627	2,011	136	500
R-squared	0.014	0.006	0.008	0.009	0.019	0.193	0.044

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-7). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Specific areas

For this heterogeneity analysis, a look is taken at 12 separate areas in the 6 LEZ’s. These specific areas are determined on how well the treatment and control group match with each other in geographic terms, as explained in the data section. Each specific area has an own regression. In Table 13 below it can be seen that for the specific area Utrecht Dichterswijk negative and significant effects are found for the interaction effects of being in the treatment group and the corresponding year. This result would suggest that for this area, the implementation of the LEZ caused a decrease in the number of jobs. However, this effect is also found before the implementation, as is seen for 2006, 2007, and 2008. As this indicates a violation of the parallel trends assumption the results found cannot be deemed causal.

For all other specific areas, the results are provided in the two tables in Appendix D. Here it can be seen that there are no significant outcomes, indicating that in none of the specific areas an effect of the implementation of the LEZ on the number of jobs is found.

To conclude, even when differentiating small zones that are crossed by the LEZ, no causal effects can be determined as only one out of twelve of these specific areas finds significant results, with this not fulfilling the parallel trends assumption. Full results including year fixed effects are provided in the separate log file.

Table 13. Regression results for the specific areas heterogeneity analyses

	(1)		(1)
VARIABLES	Utrecht Dichterswijk	VARIABLES	Utrecht Dichterswijk
T	15.32	T#2010	-17.82*
	(9.545)		(9.653)
T#2001	-8.111	T#2011	-17.56*
	(11.25)		(9.638)
T#2002	-10.35	T#2012	-17.73*
	(11.00)		(9.624)
T#2003	-10.81	T#2013	-17.99*
	(11.64)		(9.632)
T#2004	-14.85	T#2014	-17.42*
	(10.77)		(9.604)
T#2005	-16.20	T#2015	-17.21*
	(10.50)		(9.593)
T#2006	-18.05*	T#2016	-17.27*
	(9.912)		(9.593)
T#2007	-18.23*	T#2017	-17.28*
	(9.808)		(9.601)
T#2008	-18.15*	Constant	6.241**
	(9.732)		(2.819)
T#2009	-17.89*	Observations	4,696
	(9.653)	R-squared	0.018

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Check for Stable Unit Treatment Value Assumption

One important assumption of the difference-in-difference model is the Stable Unit Treatment Assumption (SUTVA). One way this assumption could be violated is by firms in the control group being affected by spillover effects of the treatment group. As the results seem to suggest there is no causal effect on the treatment group, it is unlikely that any spillover effect will exist. This would only happen when for example the displaced workers of the treatment group will move to the control group. As no effect of loss of jobs is found, it is highly unlikely that the control group would experience job growth. Another way however of violating this assumption and causing any bias in the results is by firms moving from the treatment group to the control group (or vice versa) as a response to the treatment of implementing a LEZ. To check whether this influenced the results, the data was adjusted and removed any firms that experienced a movement between these zones after 2007, this cut-off date was determined with any possible anticipation effects of the first signing of the covenant was made. Similar regressions were run as described above. Results provided similar conclusions as in the above sections. No clear deviations in size of the coefficients were found, with only minor changes occurring seemingly randomly. Additionally, only minor deviations in significance were found. So did some regressions provide additional significant results (10 extra in total), but no clear patterns were observed. It is therefore likely to assume that even with a possible violation of the SUTVA assumption, no biases will influence the results and the conclusion of this thesis. For this reason, the results presented remain the original results without the removal of these firms, as the small changes in the results can also be attributed due to the removal of a specific type of firm that is more prone to move. The full results of this check are provided in the separate log file.

Specification check with two period analysis

As described in the method section, for all analyses above a specification check will be performed with a different approach of a difference-in-difference design where the analyses are based on only the pre-and post-treatment periods. As argued, this method is less powerful ass it only controls for limited periods for the parallel trends assumption, as it would otherwise disregard too many data periods in the post-treatment period. Secondly, it is also less insightful whether possible effects only occur after time or directly after implementing the LEZ's. It does however provide a way to check whether similar results. For every regression provided in the above sections, a respective regression was performed with the form of the regression described in the method section. Results of these regressions further solidify the results as described as above. Only in six instances (Eindhoven Medium, Rotterdam Original Micro, Amsterdam industries D, K, M, and Eindhoven industry P) significant effects were found for the treated group. In all these instances the parallel trends assumption held. However, with this small number of significant results, it is not likely that these effects are causal and only arise as spurious results falling within the margin of error. Therefore, concluding that with this analysis no causal effects can be found, similar to the main results. The full regression results of this specification check are provided in the separate log file.

Check with 100- and 500-meter zones

The same regression as in the main results are ran for zones with 100-and 500-meter buffers. The full results are provided in the separate log file. Due to issues with overlapping zones for the Rotterdam current and original LEZ in the 500m analyses, the current Rotterdam LEZ is withdrawn from this check.

For the 100-meter analyses, a total of 29 significant regressions are found. Out of these, 15 results are either with a very small sample size and therefore not valid as causal effects or fail to verify the parallel trends assumption. 3 of the remaining significant results are also found in the main results (Amsterdam micro, Utrecht small, and Eindhoven industry N). The other 11 significant results are not found in the main results. However, similarly as the significant effects found in the main results it is likely that these effects are spurious and do not suggest a causal effect as no clear pattern arises, results seem seemingly random in terms of timing. The number of significant results also fall within a 10% confidence interval.

For the 500-meter analyses, a total of 21 significant regression results are found. Out of these, 9 either fall victim to small sample size or failure to accept the parallel trends assumptions. From the other regressions, 6 provide similar results as in the main results analyses (Amsterdam micro, Amsterdam small, Den Haag micro, Rotterdam original micro, Utrecht small, Eindhoven industry N). Only slight differences in the exact years are found for these results (for example, in the main results for the micro-Amsterdam companies the interaction effects of the years 2014-2017 were significant while for these results this was for the 2013-2017 period). The other 5 results provide different results, these are for the Rotterdam micro, Amsterdam industry F and J, Eindhoven industry R, Den Haag industry M and Utrecht industry O. Besides this, some of the significant effects in the main results are not found in the 500-meter analyses. It is argued that these effects are therefore spurious and do not provide any causal estimates due to the missing of clear patterns and seemingly random terms of timing similar to the conclusion of the 100-meter analysis.

Overview

In each of the performed analyses, whether performed on aggregate city level, categorized by company size, industry or within specific areas within a LEZ, no clear causal effect comes forward. This as the small number of significant effects do not have a clear pattern or strong or concise timing of effects. This also indicates that any heterogeneity in regulations, size or enforcement of LEZ were not strong enough to cause any heterogeneity in results. The results therefore suggest that there is no effect of the implementation of a LEZ on the number of jobs for similar firms just outside, and inside this LEZ. This result is backed by the effects found when removing firms that move between the treatment and control group into account, and by using a 2-time period approach. Here again, no clear pattern in effects is found and all significant effects seem spurious instead of causal estimates. The results of the 100-meter and 500-meter analyses do not identify any clear causal effects either. The few significant effects found in the main results are not supported in the two analyses indicating lack of a clear causal effect, instead the significant effects that are found seem spurious.

Conclusion & Discussion

Low emission zones are specific zones in cities that restrict access of certain motorized vehicles from entering based on for example their emission classes. The Netherlands hosts 15 of these zones. This thesis answers the research question whether the implementation of LEZ's in the 5 major cities in the Netherlands affected firm employment. Detailed description of these 5 cities is provided to identify possible differences between the zones in terms of regulations, implementation dates of the LEZ's, and possible expansion of the LEZ's in both increasing regulations and size. Literature on LEZ's find positive effects for the main goal of implementing a LEZ, namely improving the air quality inside city centers. It is however not likely that this increase in air quality will influence the number of jobs from firms. However, besides this main effect other externalities are present that possibly can alter the number of jobs. So could the implementation of a LEZ impact the accessibility of companies. This decreases the labor supply for firms in the LEZ with increased wage costs as a result, increases costs for urban freight deliveries, and decreases the number of customers in these areas. It could also become a cost due to changing the fleet composition for companies that operate within these zones causing concerns among corporate organizations.

To answer the research question, a treatment and control group were created by making buffers inside and outside the borders of all different LEZs. These groups were matched with LISA data that contains the number of jobs within each company and their geographic location for the 2000-2017 period. A difference-in-difference model was used to identify any causal effects of the implementation of the LEZ's. This difference-in-difference model provided results with interactions of each year in the sample data together with a variable indicating if a firm was belonging to the treatment group. This allowed for verification of the parallel trends assumption and identifying any possible causal effects. Besides analyses for each individual LEZ, different heterogeneity analyses were performed due to possible heterogeneity in effect between different type of firms. However, even with these different heterogeneity analyses based on size, industries, and areas, no clear causal effect on the number of jobs can be found. This result was backed by a 2x2 approach that did not result in different result, and by a partial verification of the SUTVA assumption. Not finding any causal effects of implementing a LEZ on the number of jobs within this LEZ goes against concerns of corporate organizations and further strengthens the decision-making process of implementing LEZ for municipalities. To answer the stated research question, there is no causal evidence in this thesis that finds any changes in firm employment of firms inside a LEZ compared to firms outside a LEZ after the implementation of a LEZ.

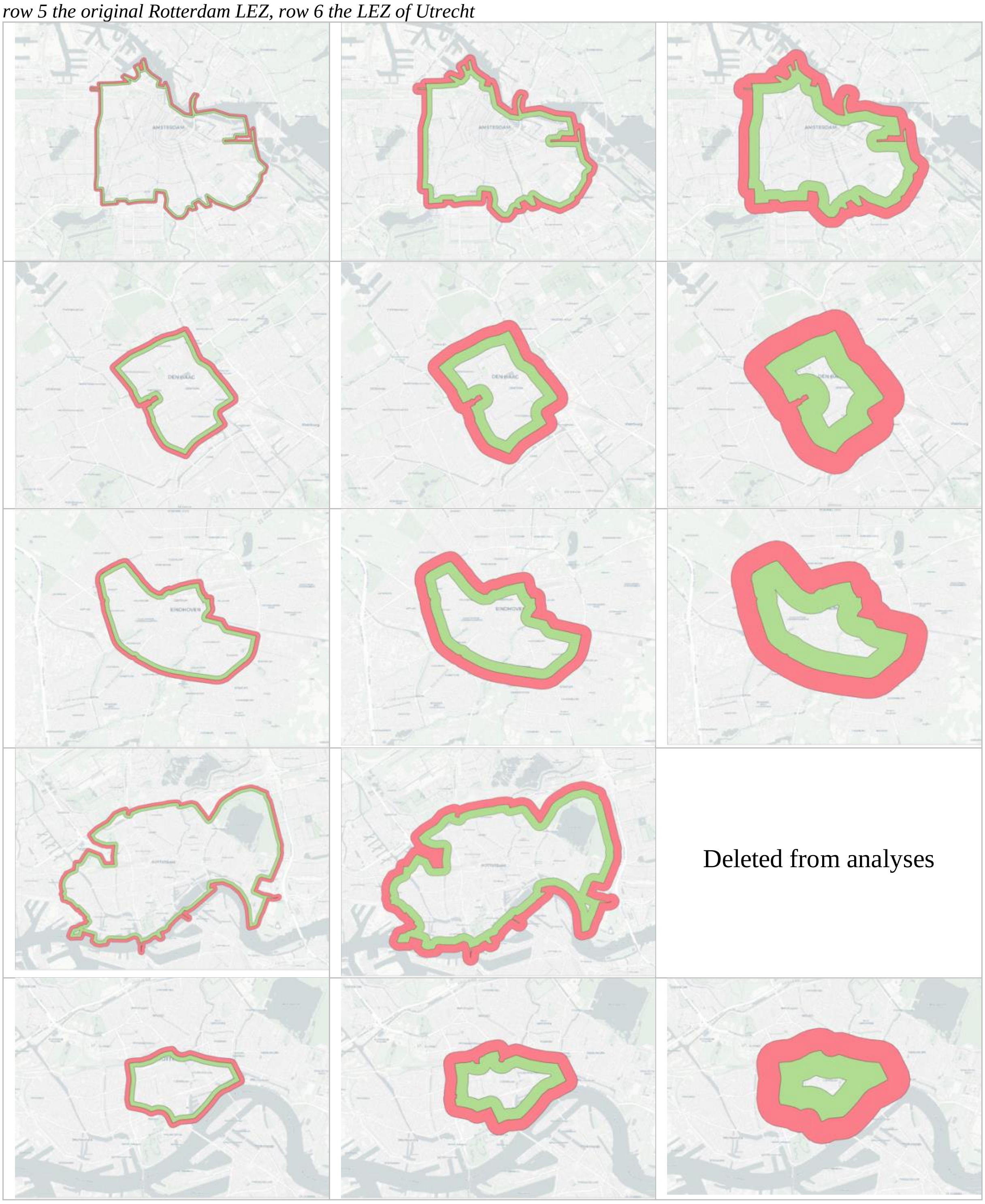
There are however several concerns and issue with the used data and method. First and foremost, this research design was based on analyzing the effect of the implementation on housing prices, which the literature finds a stronger argumentation for. This also means that the identifying of control and treatment groups are not optimal. The way businesses are potentially harmed most is in the access restriction of city centers for potential business opportunities. This is not only applicable for firms just inside the LEZ, but also for firms outside the LEZ that have customers inside the LEZ. Secondly, the large heterogeneity within firms causes issues in answering the research question. Even with the different heterogeneity analyses, this method still answers the question in a "macro" way, where effects may only arise on "micro" levels and therefore cannot be identified. It can therefore be that no causal effects are found, but individual firms can still be harmed by the measures. Thirdly, the use of number of jobs as measure of businesses success might be extreme, it could very well be that the implementation of the LEZ's had negative effects on costs, or other ways of measuring success but that these were not sufficient to translate into job loss. This data was however not available and could therefore not be used instead of the now used number of jobs. Lastly, the available type of data, with its downsides or not, was not as recent as would be ideal for this approach. The data only ran until 2017. As mentioned, this prevented analyses on some LEZs with bans of personal vehicles.

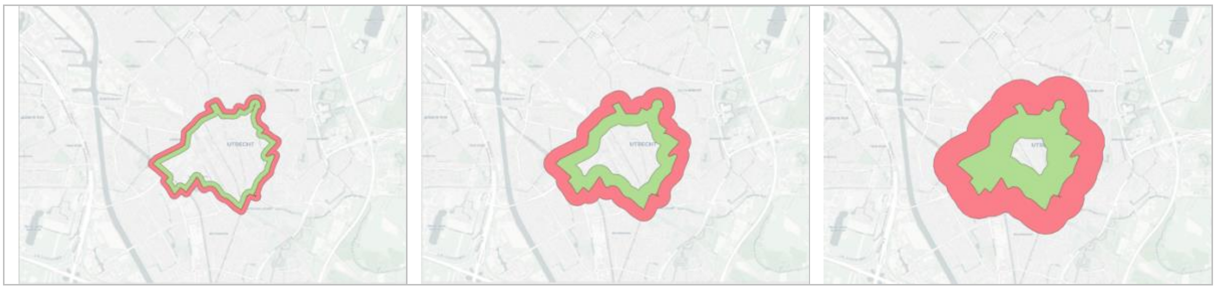
A possible different research method on identifying effects of implementing the LEZ on business success could be by surveying different companies in a specific potentially heavily harmed industry, as for example the trucking or delivery industries on whether they made increasing expenses due the implementation of the LEZ.

Appendix

Appendix A

Table 14. Column 1 provides the 100-meter buffers, column 2 the 250-meter buffers, column 3 the 500-meter buffers. Row 1 provides the LEZ of Amsterdam, row 2 the LEZ of Den Haag, row 3 the LEZ of Eindhoven, row 4 the current Rotterdam LEZ, row 5 the original Rotterdam LEZ, row 6 the LEZ of Utrecht





Appendix B

Table 15. Amsterdam regression results sizes

VARIABLES	(1) Micro	(2) Small	(3) Medium	(4) Large
T	-0.505***	1.156	14.62**	-181.7
	(0.0651)	(0.873)	(7.101)	(160.0)
T#2001	-0.0468	0.297	-2.266	107.1
	(0.0894)	(1.207)	(9.882)	(211.1)
T#2002	-0.0349	-0.339	3.240	145.5
	(0.0888)	(1.210)	(9.921)	(207.8)
T#2003	0.00925	-1.489	1.737	113.0
	(0.0881)	(1.229)	(10.42)	(203.3)
T#2004	-0.0622	-2.087*	-3.436	63.07
	(0.0879)	(1.262)	(10.46)	(203.6)
T#2005	0.0302	-1.029	-3.145	-291.5
	(0.0855)	(1.230)	(10.22)	(227.4)
T#2006	-0.000621	-1.958	-0.880	-247.2
	(0.0841)	(1.197)	(10.04)	(238.1)
T#2007	0.0227	-1.232	-0.725	-186.0
	(0.0839)	(1.194)	(10.38)	(226.2)
T#2008	0.0709	-1.447	-13.84	-254.7
	(0.0824)	(1.196)	(10.40)	(230.8)
T#2009	0.0290	-1.250	-16.44	-264.7
	(0.0800)	(1.175)	(10.27)	(228.3)
T#2010	0.0678	-1.479	-17.17*	-216.5
	(0.0781)	(1.175)	(10.23)	(226.0)
T#2011	0.0782	-1.187	-22.98**	-183.1
	(0.0769)	(1.164)	(10.11)	(229.4)
T#2012	0.0758	-1.717	-22.18**	-240.4
	(0.0765)	(1.179)	(10.03)	(225.0)
T#2013	0.118	-1.924	-18.80*	-121.1
	(0.0757)	(1.181)	(9.947)	(217.8)
T#2014	0.214***	-1.626	-19.75*	-69.48
	(0.0742)	(1.178)	(10.58)	(219.3)
T#2015	0.285***	-1.333	-16.88	-57.26
	(0.0728)	(1.153)	(10.61)	(219.7)
T#2016	0.270***	-1.156	-16.28	18.98

	(0.0718)	(1.126)	(10.03)	(216.0)
T#2017	0.295***	-1.572	-16.71*	13.62
	(0.0712)	(1.114)	(9.763)	(216.7)
Constant	2.612***	20.38***	90.13***	804.8***
	(0.0552)	(0.629)	(4.806)	(134.8)
Observations	133,830	11,925	3,203	864
R-squared	0.028	0.002	0.012	0.088

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-4). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Table 16. Den Haag regression results sizes

VARIABLES	(1) Micro	(2) Small	(3) Medium	(4) Large
T	-0.240***	-0.0832	1.569	-16.01
	(0.0778)	(0.941)	(11.94)	(212.2)
T#2001	0.0765	0.478	2.133	-28.95
	(0.111)	(1.315)	(16.96)	(311.1)
T#2002	0.0896	0.135	-14.65	-119.6
	(0.108)	(1.323)	(17.42)	(312.6)
T#2003	0.0591	-0.549	-16.87	-82.85
	(0.108)	(1.330)	(16.81)	(300.6)
T#2004	0.0665	-0.0407	-18.82	65.81
	(0.106)	(1.291)	(16.30)	(336.6)
T#2005	0.116	0.411	-2.492	136.2
	(0.104)	(1.300)	(16.25)	(331.9)
T#2006	0.143	-0.595	-10.86	-73.89
	(0.101)	(1.359)	(16.81)	(314.3)
T#2007	0.198**	0.191	-10.09	29.80
	(0.0993)	(1.338)	(16.07)	(313.5)
T#2008	0.266***	-0.710	-8.159	49.90
	(0.0973)	(1.325)	(16.02)	(314.7)
T#2009	0.216**	0.526	-3.947	36.70
	(0.0947)	(1.354)	(16.58)	(319.7)
T#2010	0.237**	1.477	-4.060	-25.44
	(0.0932)	(1.366)	(16.43)	(311.7)
T#2011	0.194**	0.993	-0.556	-76.93
	(0.0928)	(1.357)	(16.60)	(308.1)
T#2012	0.232**	1.181	-1.794	-94.03
	(0.0925)	(1.356)	(16.63)	(309.1)
T#2013	0.252***	1.222	4.296	53.06
	(0.0915)	(1.394)	(16.88)	(320.2)
T#2014	0.225**	0.559	-5.731	208.3
	(0.0903)	(1.384)	(17.43)	(322.3)
T#2015	0.157*	0.427	-10.77	184.6
	(0.0902)	(1.399)	(17.41)	(328.2)
T#2016	0.147	-0.312	-12.40	210.6

	(0.0899)	(1.420)	(18.46)	(323.6)
T#2017	0.222**	-0.587	-16.96	79.59
	(0.0885)	(1.448)	(18.25)	(300.8)
Constant	2.598***	19.65***	100.9***	670.9***
	(0.0611)	(0.663)	(8.343)	(129.8)
Observations	71,280	7,128	1,565	560
R-squared	0.025	0.002	0.016	0.013

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-4). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Table 17. Eindhoven regression results sizes

VARIABLES	(1) Micro	(2) Small	(3) Medium	(4) Large
T	-0.0966	0.726	1.413	-578.9
	(0.126)	(1.634)	(12.77)	(353.8)
T#2001	-0.0209	-0.426	0.910	40.80
	(0.180)	(2.344)	(19.24)	(495.8)
T#2002	-0.141	-2.493	0.137	-204.1
	(0.180)	(2.468)	(18.49)	(609.4)
T#2003	-0.114	-0.459	0.793	-25.05
	(0.173)	(2.500)	(17.99)	(557.1)
T#2004	-0.0751	-0.235	-5.017	144.7
	(0.175)	(2.476)	(17.56)	(487.7)
T#2005	0.0444	0.292	-11.69	148.5
	(0.172)	(2.447)	(18.25)	(505.3)
T#2006	0.158	0.738	-19.32	42.82
	(0.171)	(2.417)	(17.23)	(566.9)
T#2007	0.102	-2.251	-15.40	-33.67
	(0.168)	(2.430)	(17.66)	(634.2)
T#2008	0.0142	-1.893	4.025	34.53
	(0.168)	(2.421)	(18.19)	(639.4)
T#2009	0.111	-1.913	-1.876	118.2
	(0.159)	(2.362)	(19.03)	(562.0)
T#2010	0.0916	-3.453	0.0201	245.8
	(0.153)	(2.416)	(19.49)	(501.4)
T#2011	0.129	-3.106	9.082	181.8
	(0.151)	(2.366)	(18.86)	(542.4)
T#2012	0.155	-3.206	12.10	204.7
	(0.148)	(2.343)	(19.13)	(578.4)
T#2013	0.218	-2.255	8.486	187.9
	(0.145)	(2.327)	(18.40)	(608.5)
T#2014	0.234	-1.310	21.26	380.6
	(0.144)	(2.310)	(17.64)	(542.8)
T#2015	0.166	-0.906	3.388	359.7
	(0.143)	(2.221)	(18.11)	(546.3)
T#2016	0.191	-0.834	-6.953	165.6

	(0.142)	(2.122)	(18.70)	(636.1)
T#2017	0.177	-0.813	3.208	171.0
	(0.140)	(2.124)	(18.37)	(613.4)
Constant	2.235***	22.18***	102.8***	970.8***
	(0.100)	(1.187)	(9.667)	(352.7)
Observations	29,858	3,104	1,173	356
R-squared	0.019	0.009	0.012	0.062

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-4). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Table 18. Rotterdam current regression results sizes

VARIABLES	(1) Micro	(2) Small	(3) Medium	(4) Large
T	-0.947***	-0.582	-10.24	-270.1
	(0.130)	(1.441)	(14.40)	(216.9)
T#2001	0.0590	1.144	-12.52	12.30
	(0.181)	(2.104)	(19.55)	(281.9)
T#2002	-0.0780	-0.170	4.038	102.8
	(0.182)	(2.087)	(19.70)	(335.9)
T#2003	-0.136	1.651	17.08	239.8
	(0.182)	(2.114)	(21.15)	(377.3)
T#2004	0.00450	1.013	3.089	183.0
	(0.180)	(2.086)	(21.15)	(382.5)
T#2005	0.171	-0.960	9.286	-156.3
	(0.180)	(2.059)	(20.50)	(313.3)
T#2006	0.0853	-1.220	4.100	-166.6
	(0.183)	(2.016)	(18.60)	(277.7)
T#2007	0.0744	-1.255	13.22	-142.6
	(0.177)	(2.056)	(19.14)	(279.0)
T#2008	0.210	-0.973	10.48	-106.3
	(0.173)	(2.007)	(17.98)	(276.2)
T#2009	0.106	-2.030	1.863	-117.3
	(0.170)	(1.954)	(18.19)	(267.7)
T#2010	0.126	-1.801	4.328	-126.6
	(0.164)	(1.888)	(18.19)	(280.5)
T#2011	0.229	-0.580	1.834	-140.7
	(0.159)	(1.913)	(19.02)	(279.5)
T#2012	0.189	-0.853	0.671	-306.1
	(0.159)	(1.894)	(18.78)	(268.1)
T#2013	0.210	-1.824	-4.217	-258.7
	(0.158)	(1.870)	(18.31)	(268.4)
T#2014	0.215	-0.828	-5.053	-315.5
	(0.157)	(1.856)	(18.33)	(271.7)
T#2015	0.267*	-0.649	2.349	-325.6
	(0.155)	(1.846)	(17.96)	(295.1)
T#2016	0.236	-0.704	5.452	-290.9

	(0.153)	(1.853)	(18.42)	(293.7)
T#2017	0.296**	-0.901	0.530	-306.5
	(0.149)	(1.870)	(18.16)	(296.2)
Constant	3.115***	21.70***	105.6***	841.8***
	(0.117)	(0.915)	(8.670)	(137.9)
Observations	36,875	4,437	1,314	428
R-squared	0.060	0.012	0.015	0.068

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-4). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Table 19. Rotterdam original regression results sizes

VARIABLES	(1) Micro	(2) Small	(3) Medium	(4) Large
T	0.487***	-1.489	-0.945	365.5*
	(0.0887)	(1.026)	(10.93)	(209.8)
T#2001	-0.141	-1.185	6.019	-3.044
	(0.130)	(1.461)	(14.81)	(265.7)
T#2002	-0.143	0.193	-2.134	-47.63
	(0.129)	(1.444)	(14.71)	(260.0)
T#2003	-0.193	0.898	-4.923	-197.7
	(0.129)	(1.463)	(15.30)	(265.4)
T#2004	-0.304**	0.194	2.768	-320.2
	(0.129)	(1.471)	(14.50)	(259.4)
T#2005	-0.261**	-1.019	-1.751	-264.6
	(0.129)	(1.511)	(13.61)	(252.3)
T#2006	-0.238*	-0.0996	-6.514	-307.2
	(0.128)	(1.477)	(13.45)	(260.4)
T#2007	-0.170	1.687	-9.160	-372.6
	(0.128)	(1.435)	(13.23)	(263.9)
T#2008	-0.138	0.827	-3.872	-549.7*
	(0.124)	(1.432)	(13.90)	(323.1)
T#2009	-0.184	0.289	-1.283	-416.8
	(0.119)	(1.478)	(13.62)	(305.5)
T#2010	-0.172	1.341	-2.732	-373.9
	(0.115)	(1.432)	(13.81)	(284.6)
T#2011	-0.199*	1.448	-2.571	-400.8
	(0.113)	(1.422)	(13.93)	(264.3)
T#2012	-0.251**	1.312	-7.173	-361.2
	(0.111)	(1.451)	(14.13)	(271.2)
T#2013	-0.306***	1.701	-4.173	-229.9
	(0.112)	(1.464)	(14.44)	(250.4)
T#2014	-0.304***	3.301**	11.08	-248.2
	(0.110)	(1.416)	(13.83)	(240.6)
T#2015	-0.315***	2.174	5.459	-234.1
	(0.109)	(1.466)	(14.36)	(251.2)
T#2016	-0.333***	2.433*	3.011	-254.6

	(0.108)	(1.469)	(14.93)	(259.2)
T#2017	-0.365***	1.908	4.605	-255.4
	(0.105)	(1.437)	(15.06)	(254.4)
Constant	2.647***	21.31***	111.2***	417.4***
	(0.0649)	(0.859)	(8.963)	(76.07)
Observations	51,127	8,140	2,132	517
R-squared	0.033	0.006	0.009	0.026

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-4). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Table 20. Utrecht regression results sizes

VARIABLES	(1) Micro	(2) Small	(3) Medium	(4) Large
T	0.338***	-0.301	-3.426	173.1
	(0.0763)	(1.268)	(13.13)	(120.3)
T#2001	-0.0289	0.142	-4.633	-0.793
	(0.107)	(1.732)	(18.65)	(199.6)
T#2002	0.0191	-0.361	-13.27	118.1
	(0.104)	(1.753)	(18.55)	(185.8)
T#2003	0.0112	1.448	-13.59	39.71
	(0.102)	(1.811)	(18.02)	(187.1)
T#2004	0.0249	1.681	-20.77	227.4
	(0.102)	(1.800)	(17.98)	(163.5)
T#2005	-0.0304	1.356	-8.673	214.0
	(0.101)	(1.810)	(18.80)	(162.0)
T#2006	-0.0953	0.131	-9.759	110.0
	(0.0997)	(1.793)	(18.23)	(171.0)
T#2007	-0.0523	1.977	-17.63	85.05
	(0.0971)	(1.780)	(18.05)	(170.7)
T#2008	-0.00915	3.205*	-1.687	-24.89
	(0.0955)	(1.771)	(18.18)	(229.8)
T#2009	-0.0102	4.137**	2.602	-108.2
	(0.0943)	(1.760)	(18.05)	(223.9)
T#2010	-0.0546	3.191*	5.103	-63.26
	(0.0915)	(1.768)	(18.89)	(225.2)
T#2011	-0.0463	3.072*	2.278	13.43
	(0.0895)	(1.716)	(18.75)	(214.1)
T#2012	-0.0722	2.310	1.445	-79.94
	(0.0884)	(1.651)	(18.91)	(193.2)
T#2013	-0.0465	2.041	-21.97	8.617
	(0.0886)	(1.731)	(18.71)	(138.7)
T#2014	-0.0740	1.533	-24.05	38.72
	(0.0876)	(1.639)	(18.72)	(142.9)
T#2015	-0.0700	0.500	-30.76*	87.66
	(0.0868)	(1.715)	(17.99)	(173.5)
T#2016	-0.0520	0.709	-20.65	131.2

	(0.0861)	(1.680)	(18.29)	(185.7)
T#2017	0.00189	2.724*	-12.45	94.99
	(0.0867)	(1.635)	(18.52)	(188.6)
Constant	1.987***	19.95***	111.8***	435***
	(0.0458)	(0.962)	(10.36)	(72.03)
Observations	70,728	4,741	1,419	338
R-squared	0.023	0.013	0.030	0.091

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-4). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Appendix C

Table 21. Significant regression results industry analyses

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Amsterdam-F	Eindhoven-F	Den Haag-G	Rotterdam Original-G	Den Haag-N	Rotterdam current-O
T	-11.58***	4.286	-1.894***	3.220***	-7.010*	-89.18*
	(3.331)	(7.855)	(0.671)	(1.103)	(3.888)	(47.59)
T#2001	0.490	-8.489	-0.577	-0.855	5.418	-49.63
	(4.808)	(11.90)	(1.003)	(1.586)	(4.252)	(74.93)
T#2002	6.398	-6.847	-0.584	-1.702	0.353	-70.87
	(4.607)	(9.252)	(0.989)	(1.628)	(5.445)	(78.61)
T#2003	8.678**	-14.24	-0.295	-2.306	3.708	-150.4
	(4.337)	(11.19)	(1.003)	(1.740)	(4.568)	(113.5)
T#2004	7.600*	-20.41*	0.291	-3.416*	5.201	-187.6
	(4.331)	(10.86)	(0.926)	(1.776)	(4.745)	(116.1)
T#2005	8.130*	-20.32*	0.718	-4.402***	7.826*	-205.7*
	(4.276)	(10.63)	(0.874)	(1.678)	(4.648)	(118.9)
T#2006	7.675*	-16.55*	0.676	-4.483***	7.276	-159.5
	(4.053)	(9.982)	(0.854)	(1.730)	(4.738)	(109.9)
T#2007	8.584**	-14.73	0.738	-4.409**	10.45*	-160.8
	(4.027)	(9.744)	(0.861)	(1.754)	(5.435)	(116.6)
T#2008	8.277**	-14.12	0.884	-4.468**	9.674*	-170.2
	(3.837)	(9.312)	(0.838)	(1.776)	(5.179)	(124.7)
T#2009	8.231**	-13.82	0.562	-4.914***	9.594*	-194.2
	(3.951)	(9.123)	(0.851)	(1.742)	(4.902)	(128.1)
T#2010	7.220*	-13.06	0.435	-5.082***	9.750**	-209.6
	(4.008)	(9.027)	(0.851)	(1.682)	(4.963)	(130.0)
T#20011	9.444**	-12.65	0.648	-4.962***	9.561*	-176.9
	(3.857)	(8.889)	(0.851)	(1.707)	(5.112)	(122.8)
T#2012	11.02***	-11.24	0.670	-3.694*	8.942*	-188.9
	(3.669)	(8.692)	(0.880)	(1.993)	(5.047)	(134.2)
T#2013	10.81***	-8.531	1.126	-1.909	8.832*	-156.2
	(3.742)	(8.230)	(0.869)	(2.470)	(5.136)	(125.0)
T#2014	12.18***	-14.15	1.273	-0.483	10.82**	-174.4
	(4.117)	(9.467)	(0.857)	(2.600)	(5.052)	(124.7)
T#2015	11.86***	-15.09	1.374	0.505	9.562**	-283.9

	(4.038)	(9.748)	(0.868)	(2.883)	(4.826)	(189.2)
T#2016	10.19**	-14.20	1.138	1.160	8.804*	-309.7
	(4.178)	(9.075)	(0.924)	(2.870)	(4.742)	(195.6)
T#20017	8.873**	-15.00	1.626*	0.786	9.472**	-359.5*
	(3.866)	(9.348)	(0.853)	(2.822)	(4.700)	(208.3)
Constant	17.70***	13.35**	5.206***	4.483***	12.03***	155.9***
	(3.124)	(5.337)	(0.573)	(0.576)	(3.782)	(39.63)
Observations	6,168	1,987	13,469	11,230	4,082	446
R-squared	0.016	0.038	0.010	0.004	0.006	0.113

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-6). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Appendix D

This appendix provides the results for specific area analyses.

Table 22. Regression results first half specific areas

VARIABLES	(1) Amsterdam North West	(2) Amsterdam South West	(3) Den Haag South West	(4) Den Haag South East	(5) Eindhoven South Central	(6) Eindhoven South East
T	26.84*	2.130	1.813	0.595	2.000	-0.685
	(16.20)	(10.86)	(2.242)	(0.642)	(2.790)	(1.506)
T#2001	-2.987	-0.755	-2.093	-0.0982	0.731	-0.0617
	(22.45)	(15.70)	(2.524)	(0.862)	(4.619)	(2.055)
T#2002	-8.353	0.487	-1.372	-0.557	1.086	0.538
	(19.80)	(16.83)	(2.579)	(0.831)	(5.256)	(1.901)
T#2003	-6.993	2.125	-1.305	-0.190	-1.327	-0.0612
	(20.08)	(16.45)	(2.506)	(0.832)	(5.609)	(1.877)
T#2004	-3.323	3.875	-1.363	0.211	-0.663	1.123
	(22.17)	(16.99)	(2.477)	(0.859)	(5.447)	(2.085)
T#2005	-14.48	7.518	-2.201	0.316	-0.719	0.572
	(18.11)	(15.66)	(2.490)	(0.808)	(5.204)	(1.812)
T#2006	-22.77	6.435	-2.129	0.916	-2.555	1.183
	(16.33)	(15.34)	(2.399)	(0.944)	(5.644)	(1.819)
T#2007	-22.16	7.292	0.175	0.539	-1.658	1.431
	(16.33)	(15.15)	(3.015)	(0.868)	(5.277)	(1.839)
T#2008	-22.96	-0.903	-1.158	0.457	-2.529	1.567
	(16.29)	(14.43)	(2.661)	(0.850)	(4.918)	(1.856)
T#2009	-24.66	-11.23	-1.305	0.269	0.277	1.388
	(16.25)	(11.34)	(2.491)	(0.842)	(4.154)	(1.781)
T#2010	-24.85	-8.760	-1.409	0.130	0.201	1.674
	(16.25)	(11.30)	(2.470)	(0.801)	(4.018)	(1.618)
T#2011	-24.27	-4.829	-1.147	0.0137	1.149	1.563
	(16.24)	(11.20)	(2.504)	(0.807)	(4.380)	(1.621)
T#2012	-24.28	-4.244	-0.874	-0.202	0.301	1.585
	(16.24)	(11.19)	(2.518)	(0.806)	(4.097)	(1.611)
T#2013	-23.86	-5.416	-0.696	-0.0344	0.133	1.609
	(16.22)	(11.14)	(2.488)	(0.768)	(4.087)	(1.576)
T#2014	-24.67	-4.615	-0.766	-0.0532	-0.279	1.378

	(16.22)	(11.10)	(2.490)	(0.743)	(4.144)	(1.565)
T#2015	-25.10	-7.194	-0.967	0.0379	0.661	0.826
	(16.22)	(11.38)	(2.463)	(0.724)	(3.959)	(1.636)
T#2016	-25.51	-6.770	-1.126	-0.0248	-5.719	1.058
	(16.21)	(11.24)	(2.434)	(0.719)	(3.858)	(1.587)
T#2017	-25.69	-6.528	-0.853	0.215	-6.468	0.957
	(16.21)	(11.19)	(2.419)	(0.724)	(3.966)	(1.586)
Constant	3.119***	18.74***	5***	3.565***	5.675***	4.888***
	(0.819)	(4.485)	(0.798)	(0.351)	(1.478)	(1.371)
Observations	9,099	5,848	9,864	11,650	2,771	7,047
R-squared	0.016	0.009	0.004	0.010	0.005	0.012

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-6). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

Table 23. Regression results second half specific areas

VARIABLES	(1) Rotterdam Current North	(2) Rotterdam Original North East	(3) Rotterdam Original South West	(4) Utrecht Buiten Wittevrouwen	(5) Utrecht Central North
T	-49.42	2.627**	-1.485	2.571*	1.585**
	(32.32)	(1.181)	(1.893)	(1.453)	(0.784)
T#2001	-1.711	-0.366	0.551	-0.154	-0.649
	(45.72)	(1.721)	(2.594)	(2.210)	(0.984)
T#2002	0.967	0.0666	-0.151	-0.523	-0.170
	(44.76)	(1.764)	(2.432)	(2.218)	(1.007)
T#2003	-3.389	-1.025	-0.797	0.452	0.124
	(45.43)	(1.857)	(2.621)	(1.913)	(1.013)
T#2004	10.20	-0.526	-2.900	-0.121	0.330
	(44.42)	(2.200)	(3.295)	(1.987)	(1.002)
T#2005	5.459	-1.270	-2.851	-0.0746	-0.144
	(47.83)	(1.724)	(3.408)	(1.897)	(0.905)
T#2006	7.924	-1.161	-3.082	-0.460	-0.236
	(46.29)	(1.699)	(3.547)	(1.993)	(0.896)
T#2007	8.949	-0.976	-3.579	-0.663	0.342
	(45.47)	(1.679)	(3.834)	(1.946)	(0.926)
T#2008	16.40	-0.572	-4.880	-1.446	0.675
	(41.88)	(1.654)	(4.666)	(2.006)	(0.994)
T#2009	44.43	-0.719	-4.604	-1.668	0.474
	(32.40)	(1.542)	(4.696)	(1.957)	(0.971)
T#2010	45.07	0.353	-4.183	-1.897	0.170
	(32.38)	(1.852)	(4.320)	(1.905)	(0.893)
T#2011	45.19	0.584	-2.068	-1.392	0.353
	(32.37)	(2.117)	(3.864)	(1.881)	(0.872)
T#2012	45.64	0.491	-2.512	-0.921	0.130
	(32.36)	(2.146)	(3.765)	(1.894)	(0.849)
T#2013	45.81	0.487	-2.260	-1.569	0.225
	(32.35)	(2.034)	(3.036)	(1.918)	(0.853)

T#2014	46.17	0.791	-1.510	-1.593	0.377
	(32.35)	(2.027)	(2.937)	(1.875)	(0.846)
T#2015	46.59	1.874	-1.201	-1.541	0.344
	(32.34)	(2.607)	(2.952)	(1.912)	(0.845)
T#2016	46.98	1.143	-1.315	-1.784	0.156
	(32.34)	(1.818)	(2.837)	(1.997)	(0.840)
T#2017	47.42	1.645	-1.109	-0.841	0.151
	(32.33)	(2.146)	(2.652)	(2.174)	(0.836)
Constant	52.47	5.128***	7.270***	4.406***	3.171***
	(32.32)	(0.703)	(1.578)	(1.098)	(0.284)
Observations	4,835	11,499	11,413	5,961	22,363
R-squared	0.020	0.004	0.003	0.008	0.012

*Note: T represents being in the treatment group for value 1, and 0 for being in the control group. T#2001 represents the observation being in the treatment group in year 2001, similar goes for other years. The interaction term of T with the first year of treatment represents the start of treatment. Year fixed effects are used in the regression but not being presented in this table. Robust standard errors are used for columns 1)-5). Standard errors are presented in parentheses with significance level of 10%, 5%, and 1% are used, denoted by *, **, and *** respectively.*

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