Adverse effects of place-based policies?

SUMMARY

We analyse the effects of the Dutch Act on Extraordinary Measures for Urban Problems. *This allows local governments to prohibit non-employed households from entering into public housing in targeted neighbourhoods to improve social mixing. We show that the Act is largely ineffective in changing the demographic composition of neighbourhoods. At the same time, due to prominent advertising of targeted deprived neighbourhoods, a stigma may have been created. We adopt a hedonic price approach and use a boundary-discontinuity (within 100 m of neighbourhood borders) to quantify the overall effect of the policy. We thus exploit spatio-temporal differences in house prices and find a sizeable price reduction of about 3–5%. The magnitude of this effect is confirmed for two other national place-based policy programmes, adding to the external validity of these findings. Our results suggest that neighbourhood stigma is important, which implies that individuals living in deprived neighbourhoods experience dis-utility from living in a place with a low status.*

JEL codes: H23, H31, J60, K25, R30, R38

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Neighbourhood stigma and place-based policies

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1. INTRODUCTION

Urban place-based policies are often implemented to reduce spatial disparities in income, unemployment and deprivation within and between cities. Many programmes explicitly aim to mix households with different incomes and education levels by improving the building stock and investing in public infrastructure (de Souza Briggs et al., 1999; Lee et al., 1999; Santiago et al., 2001; Rossi-Hansberg et al., 2010; Ahlfeldt et al., 2017; Koster and Van Ommeren, 2019). The effectiveness of these policies is heavily debated, as the effects on house prices – commonly used as a proxy for neighbourhood attractiveness – are sometimes positive and sometimes negative or statistically insignificant.

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Moreover, whether urban renewal programmes have a measurable impact on the demographic composition of neighbourhoods remains to be seen.

What is, to the best of our knowledge, overlooked by this literature is that place-based policies may not only generate positive amenity effects, but may also induce a *stigma* effect, that is, a negative reputation effect (Kelaher et al., 2010). Many place-based programmes are announced in the press and local governments explicitly post the names of the neighbourhoods that receive assistance.[1] Neighbourhood stigma then implies that individuals living in deprived neighbourhoods experience dis-utility from a low status of the street or neighbourhood, which in turn may lead to suspicion and mistrust in social interactions with others outside those areas (Besbris et al., 2015). There is a large literature that suggests that other economic actors that are relevant for residents (e.g., employers, mortgage providers and friends) may indeed not be indifferent about the reputation of the neighbourhood (Tootell, 1996; Zenou and Boccard, 2000; Carlsson et al., 2018).[2]

In a rational world with perfect and complete information it should not matter to residents what areas are identified as being deprived, as all residents are already aware of its reputation. However, there is ample evidence that residents neither have perfect information on local amenity and reputation levels, nor are fully rational (Genesove and Mayer, 2001; Piazzesi and Schneider, 2009; Han and Strange, 2016; Guren, 2018). Consequently, the announcement may lead to a stigma; that is, residents consider the new piece of information as leading to a negative reputation. Hence, the presence of stigma effects may lead to a downwards bias of the amenity effect of place-based policies because the overall policy effect on prices identified in these studies is the sum of the amenity and stigma effects. The presence of stigma effects may then explain why some studies evaluating place-based policies find counter-intuitive negative or statistically insignificant effects.

The first aim of this paper is to provide suggestive evidence of this stigma effect in the housing market. The main econometric challenge is that urban place-based policies typically improve physical amenities (e.g., the building stock) and indirectly induce changes in the demographic composition that typically are associated with house price increases (e.g., the share of rich households may increase). In an ideal setup, three conditions have to be fulfilled: (i) governments must announce what neighbourhoods are deprived, (ii)

1 Policies typically choose to-be-treated neighbourhoods based on poverty indicators that make explicitly clear which neighbourhoods are the worst of the city or even of the country (see, e.g., Wallace, 2001; Koster and Van Ommeren, 2019; González-Pampillón et al., 2020, for England, the Netherlands and Barcelona, respectively).

2 A theoretical contribution on redlining in the labour market, which focuses on the racial composition of neighbourhoods is Zenou and Boccard (2000). The empirical evidence of redlining by employers and mortgage providers, mainly focusing on the United States, is rather mixed. This literature struggles how to differentiate between neighbourhood and demographic composition effects (Tootell, 1996). Field experiments (see e.g., Carlsson et al., 2018) indicate that minorities from deprived neighbourhoods receive less invitations for job interviews. Finally, there is a large descriptive, qualitative, literature, which focuses on the importance of stigma effects for residents of public housing including the role of newspaper information (Kearns et al., 2013).

governments should not introduce any other (difficult-to-observe) investment policy and (iii) household sorting is absent. We will argue below that we come close to this ideal set-up by identifying stigma effects induced by place-based policies using a boundary-discontinuity design.

The second aim of the paper is to examine the effects of the *Act on Extraordinary Measures for Urban Problems*, a large-scale Dutch place-based regulation that allows local authorities to prevent specific deprived households from moving into designated streets or neighbourhoods (Van Gent et al., 2018). In designated neighbourhoods of eight cities, households with non-employed breadwinners, as well as those with a criminal record, are not allowed to move into public housing. In the Netherlands, 29% of all housing is public housing, while the share of non-employed breadwinners in public housing is about 25%. In targeted neighbourhoods, the share of public housing exceeds 50%. Hence, this regulation is potentially effective in changing the demographic composition of targeted neighbourhoods.

The Act we focus on may seem quite peculiar, but there are other countries with similar policies. For example, in Denmark, a similar regulation using a 'ghetto list' has been introduced, which has received a lot of attention in the international press (see O'Sullivan, 2020). In Sweden, there have been policy experiments to prohibit low-income households from locating in renovated rental housing (Baeten et al., 2017). Garroutse and Lafourcade (2022) show that district schools located in French urban neighbourhoods that were designated to benefit from placed-based subsidies experienced a significant drop in pupils' attendance relative to public schools located in neighbourhoods lying just above the poverty cut-off. They interpret this as evidence for the presence of a stigma effect. In the United States, individuals with a criminal record may experience insurmountable obstacles in applying for public housing or housing vouchers (Stone et al., 2015; Walter et al., 2017). Moreover, the Act is related to more common policies affecting the tenure mix of neighbourhoods in order to improve the status and amenity level of neighbourhoods (Hastings and Dean, 2003; Arthurson, 2013).

The Act was first implemented in Rotterdam in 2006, which is the second largest city of the Netherlands, followed by other cities about 10 years later. The Act has been controversial ever since, as opponents argue that the law implies (legal) redlining and fosters discrimination in the housing market. Proponents, on the other hand, argue that the law should be seen as a 'last resort' in order to improve neighbourhood quality and reduce segregation on basis of employment. It is important to note that the implementation of the Act was neither accompanied by investments in the designated neighbourhoods, nor was associated with improvements in the quantity or quality of public housing. Using a boundary-discontinuity design and employing micro-data on households moving into targeted neighbourhoods, we first demonstrate that the Act indeed leads to a reduction in non-employed households in public housing, that is, *the redlining effect*, but did not induce a change in the share of non-employed in private housing or a noticeable change in other demographic variables. The preferred specification shows that the share of non-employed households in targeted neighbourhood is reduced by about 2 percentage points (which is about 15% of the mean non-employment rate).

Our key idea is then to investigate the stigma effect of the Act by hypothesizing that reputation of neighbourhoods does not only vary continuously over space, but varies also *discontinuously over space* (e.g., in New York, residents may have a preference to live in Harlem or not in Harlem) and that *this reputation changes over time*. A number of studies from the criminology literature have shown that streets and local neighbourhoods capture most of the spatial variation in crime (Weisburd and Amram, 2014; Weisburd, 2015; Steenbeek and Weisburd, 2016; Schnell et al., 2017). Hence, reputation is plausibly street or neighbourhood specific. The sociology literature also provides evidence that reputation of neighbourhoods is discontinuous over space, typically labelled as 'postcode stigma' (Palmer et al., 2004; Arthurson, 2013; Denedo and Ejiogu, 2021) or 'territorial' stigmatization (Rhodes, 2012; Wacquant, 2014; Sisson, 2021). This literature contains examples of residents who avoid telling their acquaintances of where they exactly live and real-estate agents arguing that house prices are substantially lower for houses which are just in a certain postcode (see e.g., Palmer et al., 2004). We emphasize that we allow neighbourhood stigma to be continuous over space, but that at the boundary of the neighbourhood there is plausibly a discrete jump in this effect. This is particularly convincing in our context, because most of our neighbourhoods have a distinct name and are therefore well defined.

We then estimate the local effect of the Act on house prices applying a boundary-discontinuity design with property-fixed effects, implying that we focus on changes over time in prices for properties that are very close (within 100 m) to borders of designated neighbourhoods. We find that the announcement of the Act leads to price *decreases* of about 3–5%. Arguably, there are *three* possible interpretations of this negative effect: (i) this effect captures changes in neighbourhood quality or composition, (ii) it is an update of homeowners' information about the quality of the different neighbourhoods due to the announcement of the programme or (iii) it measures the inducement of a stigma effect, or to put it more precisely, it measures the inducement of a discrete change of stigma at the boundary of the neighbourhood.

We think the first interpretation is unlikely to explain the discrete price difference. Importantly, the redlining effect is rather small and can only explain a price *increase*, but not a decrease, as a higher share of non-employed workers is a positive amenity. Moreover, we will see that if we control for the share of non-employed workers (and many other control variables capturing changes in neighbourhood composition), then the effect of the Act on prices is not materially influenced. The latter makes sense, as neighbourhood quality and demography tend to be continuous over space in the Netherlands, whereas we focus on price differences of properties extremely close to borders of targeted areas. Hence, controlling for demographic composition is not expected to make a difference.

The second potential explanation for the discrete price effect is that the prominent announcement of designated neighbourhoods offers new, and correct, information on neighbourhood quality for prospective buyers so that the announcement implies a drop in prices of designated neighbourhoods. This implies that either local governments have knowledge about the quality of neighbourhoods, whereas potential homebuyers do not have this information, or that homeowners are misinformed about designated neighbourhoods, but are correctly informed about adjacent neighbourhoods. Both implications of this interpretation do not make much sense, we believe. Local governments sometimes have specific information not known to the public, because this information is collected by public authorities (e.g., about pollution or crime) or because the new information is related to future policy that is still unknown to the public (e.g., the opening of a new underground station). This is not the case in the current context. There is no good reason why homeowners would be misinformed about designated neighbourhoods, while not about other neighbourhoods. Finally, this interpretation misses the point that demographic neighbourhood effects tend to be continuous at the border, hence an update of information on the quality of targeted areas would not induce a statistically significant price jump at the border.

We think that the third interpretation of the negative price effect – the presence of a stigma effect – is the most convincing explanation. This is particularly so because the posting of the targeted neighbourhoods was widely covered by the press. Hence, posted neighbourhoods likely have received a negative stigma, while properties close to these neighbourhoods did not suffer from this. This conclusion is supported by a cross-sectional boundary-discontinuity design, where we show that *before* the policy there is no statistically significant discrete difference in prices between treated and adjacent neighbourhoods, suggesting that neighbourhood quality was about the same; however, after the policy we find a statistically significant price difference of about 3.5%. Consequently, before the policy these designated neighbourhoods seem identical to adjacent neighbourhoods *at the border* according to homeowners, but the policy created a stigma, which is locally noticeable. We subject this finding to a set of robustness checks and alternative identifying assumptions. For example, we use runner-up neighbourhoods as an alternative control group and we exclude portions of borders that intersect with rivers, main roads or municipal borders.

Furthermore, a recent set of papers has shown that in staggered difference-in-difference designs, the estimate may not be informative on the average treatment effect because of negative weights (see e.g., De Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). We address this issue by including nearest treatment group-by-year-fixed effects, implying that we compare price changes between treated properties and nearby never-treated properties. This way of addressing the issue of negative weights is novel and has more general applicability, we believe, and can be used in any context where a suitable nearby control group can be defined. In a spatial context, as in ours, it makes sense to define 'nearby' using geographical distance, but in other applications, nearby can be defined differently.

A concern may be that the stigma effect may be just a particularity of the Act on Extraordinary Measures for Urban Problems, but has otherwise no external validity.

We therefore also consider two other Dutch national place-based programmes that have been implemented: the *Krachtwijken* (KW) programme (also evaluated in Koster and Van Ommeren, 2019) as well as the *Nationaal Programma Rotterdam Zuid* (NPRZ). The former focused on improvements in public housing in 83 neighbourhoods. The latter took place only in a few neighbourhoods in Rotterdam and aimed to improve the building stock, schooling and employment opportunities for young individuals. Using a similar identification strategy based on spatio-temporal differences in prices close to borders of designated areas, we confirm price drops of about 3–5%, which points to the same stigma effect. These findings make it much more likely that our estimates have external validity.

The contribution of the current paper is then three-fold. First, to the best of our knowledge, we are the first to provide evidence, albeit suggestive, of sizeable neighbourhood stigma effects in the housing market due to the announcement of place-based policies. We emphasize that the evidence can be interpreted as suggestive because we do not have a direct quantitative measure for neighbourhood stigma so our evidence for neighbourhood stigma is based on a residual interpretation after having disproved other interpretations. The inducement of a stigma effect may explain why some studies find statistically insignificant or even negative price effects when evaluating place-based policies.

Second, we evaluate the effectiveness of a large programme that implies redlining by preventing unemployed individuals from moving into public housing. Programmes that explicitly aim to improve demographic mixing by redlining are rare, and effects of policy-induced mixing are unknown. Using micro-data on the Netherlands, we explicitly test whether the demographics of the neighbourhood are significantly affected. We find very little evidence for this, except for small reductions in the share of non-employed, which is the 'mechanical' effect induced by the policy. Hence, policies that aim to foster household mixing by limiting access to public housing do not seem to be very effective.

Third, there is a long tradition within economics to study the importance of the consumers' desire to signal high income or wealth, which may cause consumers to purchase *status goods*, as discussed in the theory of the leisure class by Veblen (1899). In this literature, the emphasis is on high status goods, that is, conspicuous consumption. Recently, Bursztyn et al. (2017) concludes that 'a promising avenue for future work [on status] is to focus on settings where self-esteem may be particularly low, such as in populations facing poverty, low social status or negative stereotypes.' Our study is exactly studying such a setting for the housing market. To study status in the housing market (using revealed preference) is not straightforward, in contrast to status of consumer goods such as expensive brand clothing. This is because reputation of a location is hard to distinguish from unobservable location characteristics and typically slowly changes over time. We believe that we have shown that status of neighbourhoods can be identified, as we exploit that it not only continuously varies over space, but discretely jumps over time and space, as demonstrated in the context of a place-based policy.

Our paper also relates to the literature on the effects of place-based policies. There is now a substantial literature on the effectiveness of place-based labour market programmes and enterprises zones (see e.g., Neumark and Kolko, 2010; Busso et al., 2013; Kline and Moretti, 2013; Mayer et al., 2017; Charnoz, 2018; Givord et al., 2018); for overviews, we refer to Neumark and Simpson (2015) and von Ehrlich and Overman (2020). However, the effects of place-based housing market policies on residents have been much less studied. Most studies show that place-based investments into public or subsidized housing have led to higher house prices (Santiago et al., 2001; Schwartz et al., 2006; Baum-Snow and Marion, 2009; Rossi-Hansberg et al., 2010; Ellen et al., 2016; Koster and Van Ommeren, 2019). However, the price effect may be an underestimate of the amenity improvement implied by the place-based policy if stigma associated with the announcement of the targeted neighbourhood plays a role. Hence, with stigma, place-based policies do not necessarily increase property values. For example, a number of studies find no statistically significant, or even small negative, effects of place-based policies that subsidize housing (see e.g., de Souza Briggs et al., 1999; Lee et al., 1999; Ahlfeldt et al., 2017).

One reason for these mixed findings might be that effects depend on the local context. In particular, whether the treated neighbourhood is poor or rich seems to be important (Dillman et al., 2017). For example, Diamond and McQuade (2019) find that the construction of subsidized housing decreases house prices in rich neighbourhoods, whereas by contrast, they increase in poor neighbourhoods. At the same time, it seems that these housing policies increase neighbourhood income diversity and reduce crime (Freedman and Owens, 2011; Dillman et al., 2017; Diamond and McQuade, 2019).

Our paper also relates to a literature that aims to examine the long-run effects of exposure of children and adults to better neighbourhoods exploiting the Moving to Opportunity experiment (Ludwig et al., 2013; Chetty et al., 2016), although these studies say little about the effectiveness of housing policies *per se*. We do not find that the Act has measurably improved outcomes of incumbent households, although the time-span of our data is likely too short to capture long-run effects.

The remainder of this paper is structured as follows. Section 2 discusses the data and context of the place-based programme. Section 3 outlines the methodology used in this study. Section 4 highlights our key regression results, including back-of-the-envelope welfare calculations. Section 5 considers stigma effects in other place-based programmes, while Section 6 concludes.

2. DATA AND CONTEXT

2.1. The institutional context

2.1.1. The WBMGP law. The Dutch government introduced the Act on Extraordinary Measures for Urban Problems (in Dutch: *Wet Bijzondere Maatregelen Grootstedelijke Problematiek*), henceforth WBMGP in 2005. The Act allowed local governments to prevent specific households to move into public housing. The main aim of the WBMGP is to improve liveability of distressed streets as well as neighbourhoods by increasing social mixing and thereby avoiding too high concentrations of disadvantaged households.[3]

The Netherlands has the highest share of public housing in the world. Public housing refers to 29% of all housing stock, with a higher concentration in cities. In cities where the WBMGP was implemented, public housing comprises 38% of the housing stock. In areas where the Act was implemented, public housing is even more common with a share of about 52%.

Public housing properties are owned by public housing associations and rents are below market level and controlled. Allocation of public housing units occurs predominantly using waiting lists that apply at the municipal (or metropolitan) level to households with incomes below a certain threshold (about €40,000 per year) (see, e.g., Van Ommeren and Van der Vlist, 2016).[4] Residential moving within the public housing sector is common.

The first version of the Act contained two conditions that must be fulfilled to allow local governments to refuse households moving into their public housing: (i) the newcomer condition, which implied that local governments could only refuse households when they had lived in the municipality/metropolitan region for less than 6 years and (ii) the employment condition, which meant that local governments could refuse households that did not receive income from labour, pensions or a student loan (Van Gent et al., 2018).[5] Later, the law was extended so that local governments could also refuse persons with a criminal record.[6] In principle, the Act is applied for 4 years, after which (in almost all cases) an extension is requested.

The WBMGP was, and still is, controversial because it is thought to induce 'redlining' and enhances discrimination on the basis of employment status and residential duration (Ouwehand and Doff, 2013; Uitermark et al., 2017). Moreover, the Act targets already disadvantaged households for which alternative housing options are limited (the private rental market share is small as it is crowded out by public housing and for households with a low income it is usually financially impossible to move into owner-occupied housing). Hence, Van Gent et al. (2018) argue that for excluded households the only remaining option may be to share a dwelling with other households.

3 In the current paper, we will frequently use the terms 'neighbourhoods' and 'streets' interchangeably, because in about 50% of the cases a street is affected by the Act, while for the other half a whole neighbourhood is affected. The main exception is in Section 4.3.2 where we will distinguish between the effects of targeted neighbourhoods and streets.

4 Individuals on a waiting list of a municipality can apply to any vacant public housing property within this municipality. A small share of public housing is allocated based on priority (only in Amsterdam this share is substantial, but this city is not included in our sample). For example, priority is given to households that are forced to move due to renovation of public housing.

5 For reasons of brevity, individuals without paid work, pension or student loan are labelled as 'non-employed'. As we will focus on individuals above 25 years, the alternative to non-employed is either having paid work or being retired.

6 We do not have any information about criminal records so our estimates of the stigma effect are potentially slight underestimates.

Soon after the law was designed in 2005, the municipality of Rotterdam was the first to implement the law in 2006. Because the law was initially only implemented in Rotterdam, the Act is commonly referred to as the 'Rotterdam-Act'.[7] An important prerequisite for a legitimate implementation of the law is a lengthy discussion on why those areas should be targeted. Not all areas that were shortlisted have been targeted. In Figure 1(a), we show the targeted areas in Rotterdam, but also the areas that were shortlisted, but not targeted.[8]

From 2013 onwards other cities followed (see Figure 1), such as Nijmegen in 2015, Capelle aan den IJssel and Vlaardingen in 2016, 's-Hertogenbosch and Tilburg in 2017 as well as Schiedam and Zaanstad in 2018. In Tilburg, only one small neighbourhood was targeted. In addition to Rotterdam, Nijmegen also shortlisted 'runner-up' neighbourhoods that were eventually not targeted.

The implementation of the WBMGP was widely considered as being a *last resort* to restore liveability of areas, after other interventions have failed. Importantly, the assignment of neighbourhoods and streets has been extensively discussed in the (local) press.[9] We list here just a small selection of press articles: ANP (2006), Brink (2016), Van der Velden (2016), Damen and Pan (2017), Eikenaar (2017), Oosterom (2019) and Don (2020). It seems therefore reasonable to believe that most people are aware which neighbourhoods or streets have been assigned. However, the press articles mostly paid attention to the targeted WBMGP *neighbourhoods*, while the targeted *streets* were less covered. Although the streets could still be found in several policy documents, one may hypothesize that this makes it plausible that the stigma effect will be stronger for targeted neighbourhoods than for streets. Hence, in Section 4.3.2, we will analyse the house prices effects of targeted WBMGP neighbourhoods and streets separately.

2.1.2. Rents, public housing and house prices. It is further important to understand the institutional context, as it is theoretically possible that the policy may not only create a negative stigma effect, but may also induce general equilibrium effects, including changes in rents and the supply of public housing, with possible spillovers on the private housing market. Furthermore, one can imagine that the policy changes the allocation method of public housing, or, potentially, the outcome of the allocation, as

7 Until 2018 the WBMGP was implemented in many areas of Rotterdam. In 2018, Rotterdam changed their policy and since then refuses people with a criminal record in 98 designated neighbourhoods, while dropping the employment condition. As this change occurred just before the end of our period of observation, it has hardly any influence on our results. We include those observations, but excluding those observations provides almost identical results.

8 In our preferred specifications, we only include observations within 100 m of WBMGP borders. We illustrate this by focusing on treated areas in Rotterdam-West in Appendix A.1.

9 The information about where the Act is introduced is public, because it is the local council that determines which locations are treated. Their decision is then covered in local media as the Act was considered to be controversial. Furthermore, after 4 years, when the Act is extended, it is again discussed by the local council, which again may have created media attention.

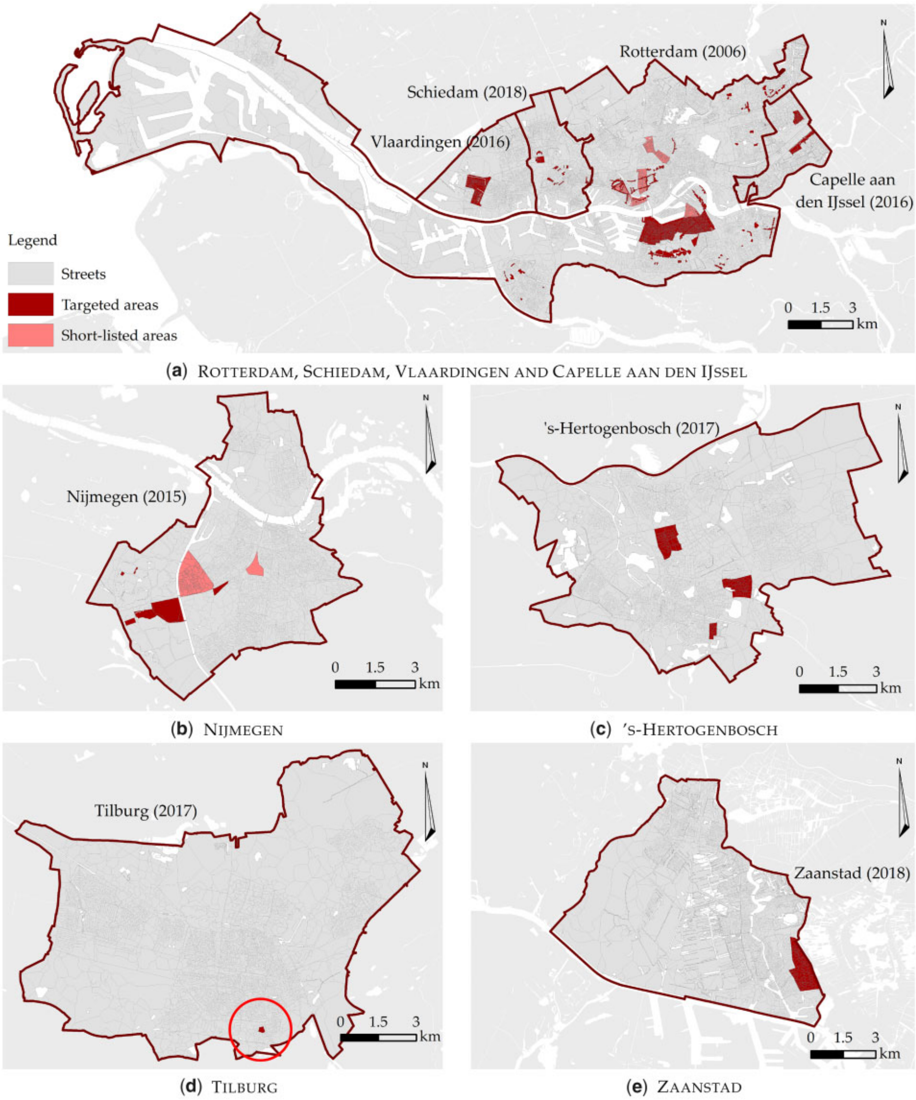

Figure 1. Targeted areas. (a) Rotterdam, Schiedam, Vlaardingen and Capelle aan den IJssel. (b) Nijmegen. (c) 's-Hertogenbosch. (d) Tilburg. (e) Zaanstad

households denied public housing will likely move somewhere else which may affect the estimates. Here, we will give a short summary of the institutional context, for more details, we refer to Van Ommeren and Van der Vlist (2016).

In cities under study, rents for public housing units are far below the free market rent and controlled in two ways. First, rents can be increased annually (rent decreases are also allowed but hardly occur), with a maximum rent increase (typically based on the inflation) determined by the national government. What is more important for the current study is that the rent is also subject to a maximum rent that is determined by the characteristics of the house and a nationally set scoring rule. Typically, housing associations do not charge this maximum rent, but ask a rent below that (typically about 10%), without differentiating between houses or households. Hence, there is no reason to believe that housing associations will change (or even have an incentive to change) rents for houses that are directly affected by the Act.

Public housing in the Netherlands is allocated to eligible households (those with low incomes) mainly by waiting lists, as rent control creates excess demand.[10] Waiting lists are organized at the metropolitan level, and in principle, each household can apply for each house (but restrictions on age or household size may apply). Public housing allocation is *choice based*, which means that each eligible household can infinitely reject housing offers until the household receives an offer that provides sufficient housing quality.

Because houses offer different quality, there is an equilibrium distribution of waiting times, where more attractive houses typically have longer waiting times (Van Ommeren and Van der Vlist, 2016). It is theoretically possible that the equilibrium distribution of waiting times changes due to the Act (as certain types of households do not have access to certain houses), and therefore this changes the incentives of households to move to the privately owned market, which in turn may affect local house prices. The latter is, however, very unlikely, because of two reasons. First, the Dutch housing market is highly segmented based on income, such that very few households choose between public housing and privately owned housing, because those with lower incomes typically do not gain access to privately owned housing (because of mortgage restrictions or income restrictions when aiming to rent a privately owned property), whereas those with higher incomes do not have access to public housing. Second, the Act applies to newcomers, that is, those who did not live before in the municipality in the last 6 years. Newcomers typically do not have strong preferences where to live, which is in contrast to incumbent households, who have a strong preference where to live (e.g., because their children go to a certain school). Hence, when refused access to a certain house, it is not so likely that these households will choose houses nearby (if they could) so the *local* general equilibrium effect of where the refused household will move to will be negligible.

In short, the institutional contexts make it very plausible that the general equilibrium effects from the policy on the privately owned housing market are essentially absent.

2.2. Data

Our analysis is based on several datasets for our study period of 2000–2019. We focus on the above mentioned eight cities where the WBMGP is implemented.

Our first source of data, the `Sociaal Statistisch Bestand`, is micro register-data from `Statistics Netherlands` and covers the whole population. In contrast to, for example, the United States, the Netherlands does not undertake censuses to register

10 Waiting lists are considerable in all cities under study and were at least two and a half years in 2019 (Kraniotis and De Jong, 2017), but were typically much longer.

their population, but the register is constantly updated. It provides basic information on demographic characteristics, such as age, country of birth, marital status and gender.

Information on yearly income and employment of household members are obtained from the `Integraal Huishoudens Inkomen` dataset from 2003 to 2010 and `Inhatab` from 2011 to 2019. These data are based on the tax register, which provides information on taxable income, tax paid as well as payments to or benefits from property rents or dividends. This dataset also provides information on whether household members are unemployed, whether households are homeowners or renters and whether they receive housing benefits.[11] We furthermore obtain information on the educational level of adults in the household from `Hoogsteopltab`. The latter data are based on various sources to determine the highest level of education for about 55% of the population.

The micro-data enable us to observe the Dutch population over time and track their location choices and associated housing characteristics. We link the micro-data to data on buildings from the `BAG` to have a yearly panel dataset of individuals and their characteristics. To determine whether a property is public housing, we exploit data from `Eigendomtab` with information on ownership on all Dutch residential properties. We focus on individuals aged above 25 years. In total, we have about 25 million observations.[12]

The treatment units are streets or neighbourhoods. We create streets by using information on the `BAG`, which is the Land Registry, containing all addresses and information on property characteristics such as size and construction year.[13] Neighbourhoods are defined by `Statistics Netherlands` and are small; on average the number of households is 822, while the median is just 290 households. We emphasize that the boundaries of streets of neighbourhoods are not overlapping with school districts or public good provision. However, even if they would overlap, then this would not invalidate our identification strategy that relies on changes over time around boundaries.

We use housing transactions data for the period between 2000 and 2019 from the Dutch Association of Real Estate Agents (`NVM`), which contains about 75% of all transactions. We focus on the above-mentioned cities where the WBMGP is implemented during this study period. We have information on the sales price, the exact location and a wide range of housing attributes such as size (in m^2), house type and construction year.[14] Our full sample contains 231,277 transactions. In our main analysis, we focus on

11 We exclude a few outliers of households with annual incomes below €1,200 and above €1 million. The methodology to determine income is slightly different between the two datasets, but the correlation between income, which can be calculated for overlapping years, exceeds 0.97 so any measurement error is expected to be small.

12 As we have individual data, but are interested in the effects at the property level, we weight each observation by the inverse of the number of individuals in the same property in the same year.

13 To create polygons for streets, we construct so-called *Voronoi*-polygons whose boundaries define the area that is closest to each property relative to all other properties. We then amalgamate property-specific polygons that are in the same street.

14 We exclude transactions with sales prices that are above €10 million or below €10,000 or a m^2 price below €185 or above €15,000 (referring to approximately the 0.01 and 99.99 percentiles, respectively). Furthermore, we exclude homes smaller than 25 m^2 or larger than 750 m^2.

repeated sales, so properties that are sold at least twice, which cover more than half of the number of transactions of the full sample.

2.3. Descriptive statistics

We report descriptive statistics for the characteristics of individuals in our data in Table 1. About 11% of individuals reside in a WBMGP street or neighbourhood, of which 41% after treatment. Outside WBMGP areas, 12% of individuals are non-employed, that is, they are unemployed or do not participate in the labour market. The majority of the non-employed are 'long-term' non-employed, defined as being non-employed for more than 1 year. The share of non-employed individuals is considerably higher (i.e., 23.1%) in WBMGP areas, confirming that these areas are deprived.

Average gross annual household income is €67,077 outside WBMGP areas. In WBMGP areas, it is about 30% lower. Approximately 50% of the Dutch population is 'low skilled', defined as having completed vocational, secondary or primary education. As mentioned above, the share of individuals in public housing is high in the Netherlands (i.e., 36%), with a higher share in WBMGP areas (52%).

Table 2 provides descriptive statistics for the house price data. About 6% of the housing transactions occur in WBMGP areas, while 2.3% of the total transactions occur after treatment. The average house price is €1,955 per m^2. House prices have strongly increased during the study period. We show this in Figure 2, where we focus on *non-treated* transactions in either (i) all non-treated areas, (ii) non-treated properties within 100 m of a border of a WBMGP area and (iii) WBMGP areas that are to-be-treated or for which treatment has been retracted. We observe that prices in and close to areas that are eventually treated have lower prices, which is in line with the notion that the policy targeted deprived areas. However, we find that price trends are very similar across to-be-treated and areas close to WBMGP areas.

3. METHODOLOGY

3.1. Measuring redlining effects

We first aim to measure the effect of the WBMGP on *neighbourhood composition* to see whether the policy was effective in preventing certain types of households of moving into the area, that is, whether redlining effects exist. We capture neighbourhood composition by employment status, income and several neighbourhood demographic variables.

We measure changes in neighbourhood composition using individual-level data, which avoid the aggregation of individuals at an arbitrarily chosen administrative neighbourhood level (Combes et al., 2008). One important issue is that the effect of the treatment is not immediate but dynamic as changes in neighbourhood composition fully depend on residential turnover (i.e., changes in employment composition depend on the probability that households without employed members move out and are replaced by

Table 1. Descriptive statistics of individual characteristics

	Inside WBMGP areas				Outside WBMGP areas			
	(1) Mean	(2) SD	(3) Min	(4) Max	(5) Mean	(6) SD	(7) Min	(8) Max
WBMGP implemented	0.411	0.492	0	1	0	0	0	0
Years of WBMGP treatment	4.589	3.698	0	14	0	0	0	0
Distance to WBMGP border (in m)	0.354	0.675	0	5.996	2.493	3.323	0.000135	16.39
Non-employed	0.231	0.418	0	1	0.119	0.322	0	1
Long-term non-employed	0.188	0.390	0	1	0.0949	0.293	0	1
Annual income (in €)	47,736	39,924	1,200	999,756	67,077	55,657	1,200	999,756
Low-skilled	0.625	0.484	0	1	0.504	0.500	0	1
Foreign-born	0.361	0.480	0	1	0.173	0.378	0	1
Pension receiver	0.121	0.325	0	1	0.170	0.375	0	1
Household – single	0.398	0.486	0	1	0.312	0.461	0	1
Public housing	0.523	0.499	0	1	0.362	0.481	0	1
Male	0.502	0.500	0	1	0.489	0.500	0	1
Year of observation	2011	4.906	2003	2019	2011	4.896	2003	2019

Notes: The number of observations is 2,593,064 for observations inside WBMGP areas and 22,935,938 outside WBMGP areas. Note that the number of observations may differ slightly per variable dependent on data availability. We remove the top and bottom 20 observations to ensure confidentiality.

Table 2. Descriptive statistics of house price data

	Inside WBMGP areas				Outside WBMGP areas			
	(1) Mean	(2) SD	(3) Min	(4) Max	(5) Mean	(6) SD	(7) Min	(8) Max
WBMGP implemented	0.373	0.484	0	1	0	0	0	0
WBMGP area boundary distance (in m)	259.5	474.9	0.251	5,996	2,410	2,133	0.218	15,270
Sales price (in euro per m^2)	1,604	533.5	185.6	14,250	1,976	637.1	189.7	15,000
Size of property (in m^2)	92.82	33.00	26	420	111.6	42.16	26	536
Apartment	0.650	0.477	0	1	0.447	0.497	0	1
Newly built property	0.00742	0.0858	0	1	0.0138	0.117	0	1
Central heating	0.860	0.347	0	1	0.918	0.274	0	1
Private parking space	0.134	0.341	0	1	0.253	0.435	0	1
Year of observation	2010	5.694	2000	2019	2010	5.900	2000	2019

Note: The number of observations is 13,477 inside WBMGP areas, while it is 217,750 outside WBMGP areas.

households with employed members). One consequence is that the immediate effect is a strong underestimate of the overall long-term effect. To deal with this, we include the *elapsed duration* of the treatment as the main explanatory variable of interest.

Let i be an individual living in property j in street s in year t. Then:

$$y_{ijst} = \beta D_{st} + \lambda_j + \mu_{s \in m,t} + \epsilon_{ijst}, \quad \text{if } d_{jst} < \overline{d}, \tag{1}$$

where y_{ijst} denotes either non-employment, the log of income, level of education, ethnic background or age. Here, D_{st} denotes the elapsed duration of treatment (in years) given

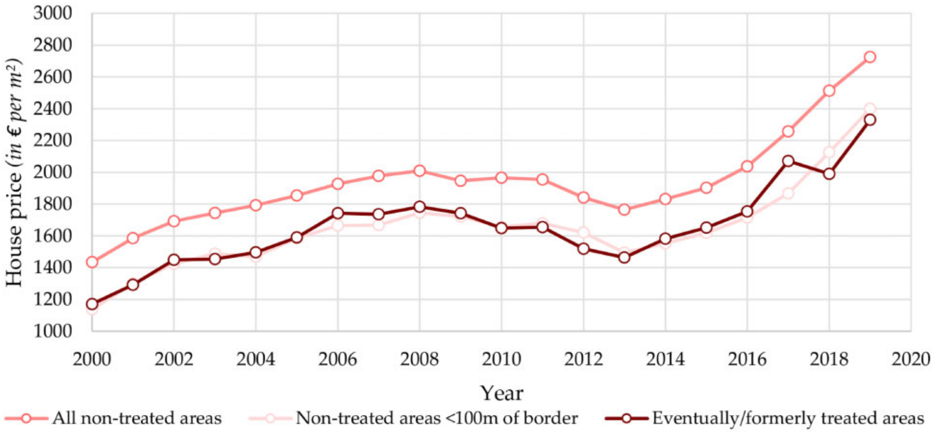

Figure 2. Price trends of non-treated observations

that a property is in a street in which the WBMGP is implemented.[15] We are particularly interested in the effect of the treatment duration captured by β. Furthermore, λ_j are property-fixed effects, $\mu_{s\in m,t}$ are municipality m by year time dummies that control for the overall trends in each municipality and ϵ_{ijst} refers to a random error.[16]

The main issue with the above specification is that the implementation of the WBMGP is not random over space. Despite the inclusion of property and municipality-by-year-fixed effects that absorb all time-invariant characteristics related to properties, one may argue that the WBMGP could have been implemented in places within municipalities where there were more negative developments in liveability, absent the policy, which may have disproportionally repelled advantaged households.

To address this issue, we only keep properties within a very small distance $\overline{d}$ of a border of a designated WBMGP-street or neighbourhood, in most specifications chosen to be 100 m, or even smaller. Hence, we focus on very local differences in demographic characteristics, where we expect that due to redlining, certain households (e.g., those who are non-employed) are less likely to move into targeted areas. Because the treatment is very local – that is, at the street level – we cluster standard errors at this level.

15 For some areas, the Act is implemented and then not renewed after 4 years, but re-implemented after a break. In this case, the elapsed duration remains constant during the break.

16 Alternatively, one may estimate the lagged specification: $y_{ijst} = \sum_{L=1}^{\mathcal{T}} \beta_\tau w_{st-\tau} + \gamma x_{jst} + \lambda_j + \mu_{s\in m,t} + \epsilon_{ijst}$, where w_{st} is an indicator that is 1 if a property is in a street in which the WBMGP is implemented, τ denotes the number of years since the first year of treatment and $\mathcal{T}$ denotes the overall duration of treatment. The main disadvantage of this specification is that we have an imbalanced panel of areas so for larger values of τ it appears that β_τ is only identified for specific areas. To solve this issue, one may assume that β_L does not vary over time, hence, $\beta_\tau = \beta$, $\forall\tau$. Given this restriction, one can derive Equation (1) where $D_{st} = \sum_{\tau=1}^{\mathcal{T}} w_{st-\tau}$. We also have estimated non-employment models at the household level, where non-employment is a dummy indicator which is one if *none* of the members of the household are employed. These results are almost identical and can be received upon request.

In spatial discontinuity designs, spatial spillover effects are potentially important. In the current context, this means that rejected households, that is, households that are constrained to move into locations at one side of the border, are more likely to move into nearby locations at the other side of the border, which may induce overestimates of effects of the WBMGP. In the context of public housing however, very local spillover effects must be negligible, because households cannot freely choose where to live. Rejected households are for a number of years on a waiting list for public housing at the municipal level (or even metropolitan level) and the probability that rejected households move into public housing *just at the other side of the border* is therefore negligible. This is particularly so for newcomers – households from outside the municipality – to which the Act applies.

We note that β captures two effects: the direct effect of the policy on y_{ijst} and a sorting effect. The first effect, which is usually the main focus of the literature on social interactions, captures, for example, whether improved social networks lead to a lower unemployment rate (Bayer et al., 2008). We expect the direct effect in our context to be small. We are mainly interested in the second effect: the sorting effect. To separate the direct effect from the sorting effect, we will also estimate regressions where we include individual-fixed effects that control for sorting effects (see Combes et al., 2008):

$$y_{ijst} = \beta D_{st} + \kappa_{ij} + \mu_{s\in m,t} + \epsilon_{ijst}, \quad \text{if } d_{jst} < \overline{d}, \tag{2}$$

where κ_{ij} captures individual-by-property-fixed effects.

Finally, one may wish to apply an 'event-study' methodology, which allows one to check for pre-trends and to check for the assumption that the treatment effect increases over time. To apply such a methodology, we essentially estimate:

$$y_{ijst} = \sum_{\tau=-4}^{3} \beta_\tau w_{st+\tau} + \lambda_j + \mu_{s\in m,t} + \epsilon_{ijst}, \quad \text{if } d_{jst} < \overline{d}, \tag{3}$$

where w_{st} is a time-varying indicator that is 1 if a property is in a street in which the WBMGP is implemented, τ denotes the number of years relative to year of treatment and 3 denotes that the treatment took place 4 or more years ago. In this setup, $\tau = 0$ is the year of the treatment and the period more than 3 years *before* the treatment is the reference category. In the absence of pre-trends, then β_τ is equal to 0 when $\tau < 0$ and β_τ measures the annual effect of the treatment for $\tau \geq 0$.

3.2. Measuring price effects

Our other aim is to measure the causal effect of the implementation of the WBMGP on house prices. In contrast to changes in demographic composition, one expects that the effect of the policy on prices is immediate (after implementation).[17] We will interpret a negative effect as providing suggestive evidence of the presence of a neighbourhood stigma effect induced by the policy. We allow neighbourhood stigma to vary continuously over space and time, but investigate whether it discretely changes over space and time because of the policy. We aim to identify the latter discrete effect by estimating:

$$\log p_{jst} = \beta w_{st} + \gamma x_{jst} + \lambda_j + \mu_{s\in m,t} + \epsilon_{jst}, \quad \text{if } d_{jst} < \overline{d}, \tag{4}$$

where p_{jst} is the transaction price of property j in street s in year t, and as above, w_{st} is an indicator that is 1 if a property is in a street in which the WBMGP is implemented. Here, β is supposed to capture the discrete stigma effect. β is an underestimate of the overall stigma effect if stigma varies monotonically over space within the vicinity of the border. This would imply that the difference in reputation for locations located at different sites of the border becomes larger if we focus on locations further away from the border.

One may, again, be concerned that the policy is mostly implemented in places where prices are declining. As with the analysis on redlining, we focus on properties close to a border of an area that is treated so within $\overline{d}$. As long as the distance to the border d_{jst} is small enough, we expect to control for the potentially non-random assignment of the WBMGP, as *locally* the borders of streets can be considered as random. Street and neighbourhood boundaries generally do not intersect with natural features of the landscape, nor with administrative borders, but we run additional regressions where we exclude portions of boundaries that interfere with rivers, main roads and municipal borders.

We consider various other identification strategies to identify the causal effect of the WBMGP. First, we only keep areas that are (eventually) treated and use temporal variation in the treatment to identify the effect of interest. The main identifying assumption is then that the *timing* of treatment is random, which implies that the first streets that have been assigned have similar price trends as streets that are assigned later, absent the policy. Second, the municipalities of Rotterdam and Nijmegen shortlisted a few neighbourhoods that were considered but eventually were not assigned. Similar as in Rossi-Hansberg et al. (2010), the identifying assumption is that price trends between assigned and considered neighbourhoods are the same. Third, we improve on the baseline identification strategy by including neighbourhood-by-year-fixed effects to absorb any price differentials between neighbourhoods.

17 We assume here that announcement and implementation dates coincide. As announcement preceded implementation, we will also estimate event studies to show that before implementation prices already decreased.

One may further argue that the price effect due to treatment of w_{st} estimated in Equation (4) is also capturing changes in neighbourhood composition. We expect that changes in neighbourhood composition are approximately continuous at the border; hence, our boundary design implicitly controls for changes in neighbourhood composition. However, to make sure that β does not pick up changes in neighbourhood composition, we also estimate

$$\log p_{jst} = \beta w_{st} + \gamma x_{jst} + \delta \bar{y}_{st} + \lambda_j + \mu_{s \in m,t} + \epsilon_{jst}, \quad \text{if } d_{jst} < \bar{d}, \tag{5}$$

where $\bar{y}_{st}$ refers to averages of demographics per street.

In the recent literature on difference-in-difference designs it is understood that with staggered adoption, difference-in-differences estimates may be not informative on the average treatment effect if average treatment effects are heterogeneous across streets or years (De Chaisemartin and D'Haultfœuille, 2018, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021). This is because the estimated coefficient $\hat{\beta}$ is a weighted average of several difference-in-differences comparing changes in prices between consecutive time periods across different pairs of properties. De Chaisemartin and D'Haultfœuille (2020) show that this may imply negative weights because treated observations in earlier periods may function as controls for observations that are treated later.

Among others, De Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) have proposed alternative estimators.[18] Here, we are able to overcome the issue of negative weights by exploiting only the identifying variation between treated properties and nearby never-treated properties.[19] We do so by including nearest treatment group-by-year-fixed effects. Hence, we estimate

$$\log p_{jst} = \beta w_{st} + \gamma x_{jst} + \lambda_j + \mu_{s \in n,t} + \epsilon_{jst}, \quad \text{if } d_{jst} < \bar{d}, \tag{6}$$

Hence, for each property, we define the nearest treatment group (i.e., the nearest street that received treatment sometime during the study period), which we denote by n. By including $\mu_{s \in n,t}$, we avoid using the variation in prices across neighbourhoods and instead only exploit the variation in price changes on both sides of a WBMGP border. By only using this identifying variation, there is no staggered adoption within groups and hence the issue of negative weights is addressed. The downside of this specification is that it may be slightly inefficient as it ignores potentially relevant identifying variation in prices between streets in different parts of a municipality. We therefore consider the results of Equation (6) as a robustness check.

18 Callaway and Sant'Anna (2021) assume irreversibility of treatment and that the panel data are balanced. Both conditions do not hold in our set up.

19 This approach is novel, we believe, and may be applied to other contexts in which 'nearby' never-treated control groups can be defined. This is particularly so for spatial settings where 'nearby' is based on geographical distance.

4. RESULTS

4.1. Effects on neighbourhood composition

First, we investigate whether the policy has the intended effects of limiting the share of non-employed individuals in designated streets and neighbourhoods. Panel A of Table 3 reports the baseline regressions, where we estimate Equation (1). We only include observations within 100 m of borders of WBMGP areas, but we reduce the maximum distance to 50 m in Appendix A.3.

Column (1) shows a sizeable reduction of the (elapsed) duration of the Act on the share of non-employed in areas where the Act was implemented, which can be interpreted as the 'mechanical' effect of the policy. The coefficient implies that the share of non-employed is reduced by 0.36 percentage points for each year of the WBMGP treatment. To put this estimate into perspective, after 4 years of treatment the reduction in non-employed individuals is 1.5 percentage points, which is 11.5% of the mean. Furthermore, in the remaining columns of Panel A, we find no effect on the share of low skilled, foreign-born or retired households and modest effects on two other neighbourhood composition variables, the income of households and the share of single households. However, in Appendix A.3, we show that this effect is not robust when we reduce the maximum distance to 50 m. In any case, the income effect is rather small. After 4 years, income of individuals in public housing is just 1.6% higher.

It is plausible that the policy predominantly, or even only, affects households who intend to live in public housing. We therefore re-estimate the same set of regressions, but now only include public housing in Panel B of Table 3. It appears that the effect on the share of non-employed becomes slightly stronger. Again we find small increases of income. The results suggest that the non-employed are not replaced by retired individuals, but by employed workers.

In Panel C of Table 3, we examine to what extent households residing in the private housing market are indirectly affected by the policy, because of the composition changes observed in public housing, that is, the effect of the policy beyond the mechanical effect. Now we do not find that the share of non-employed, or income, is affected by the Act, despite small standard errors. We find a small positive effect on the share of foreign-born, but this effect ceases to be statistically significant once we reduce the distance to the WBMGP border to 50 m.

In Figure 3, we report an event study to the effect of the WBMGP on the share of non-employed. We do not find evidence for pre-trends, as one expects given the boundary design. In the year of treatment the effect is –0.01, but increases in a (more or less) linear way to –0.02 after 3 years. Consequently, these results support the specifications in early analysis where we include the elapsed duration linearly. We extend the event study to 10 years before and 5 years after treatment in Appendix A.3, showing that after 5 years the effects increase to a reduction on non-employment of 2.5 percentage points.

Table 3. Baseline redlining regressions

Dependent variable:	Non-employed	Log of income	Low-skilled	Foreign-born	Retired	Single
Panel A: All properties	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0036***	0.0041***	0.0016	0.0019	−0.0016	−0.0019**
	(0.0011)	(0.0012)	(0.0012)	(0.0015)	(0.0011)	(0.0008)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	1,578,983	1,560,069	1,033,847	1,661,195	1,578,983	1,578,983
R^2	0.5652	0.6349	0.5385	0.5587	0.7134	0.6421
Panel B: Only public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0038***	0.0033***	−0.0014	−0.0007	0.0001	0.0020**
	(0.0014)	(0.0012)	(0.0013)	(0.0016)	(0.0018)	(0.0009)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	832,415	826,303	540,491	862,804	832,452	832,452
R^2	0.5651	0.6032	0.5375	0.5682	0.7262	0.6615
Panel C: No public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0013	0.0012	−0.0011	0.0050**	−0.0019	0.0006
	(0.0013)	(0.0020)	(0.0017)	(0.0021)	(0.0012)	(0.0010)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	724,200	711,904	477,831	774,257	724,200	774,257
R^2	0.4770	0.6117	0.4951	0.5513	0.6961	0.6210

Notes: We only include properties within 100 m of a WBMGP border. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

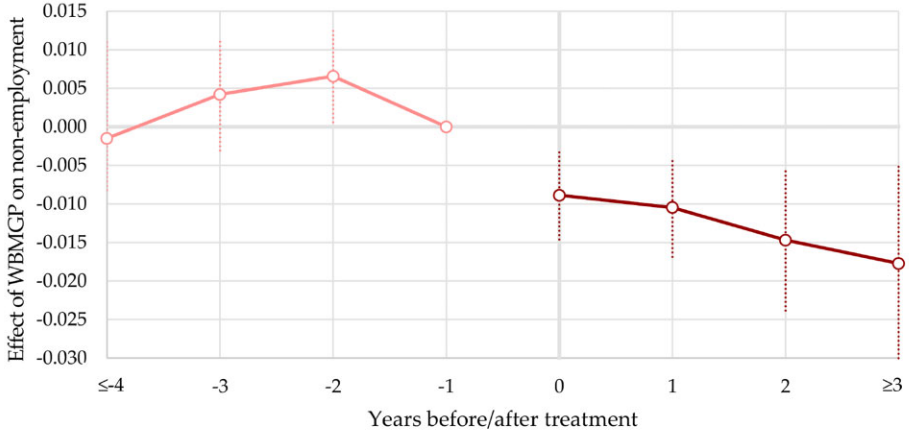

Figure 3. Effects on the share of non-employed: event study

Appendix A.3 further distinguishes between the effects of the WBMGP between individuals moving into and moving out of housing units. We show that the share of non-employed moving into public housing units in targeted areas is indeed considerably (i.e., 6 percentage points) lower. Because incumbent individuals, independently of their employment status, are not directly affected by the Act, one does not expect any effects on individuals moving out of public housing, which is indeed confirmed by the analysis.

In Appendix A.3, we further report regressions where we include individual-by-property-fixed effects. In this way, we fully control for sorting effects and focus on the causal effects of the policy on incumbent households. For example, it might be that the increase of employed individuals in the neighbourhood may have increased labour market opportunities of incumbent households (Bayer et al., 2008). We find that the effect of the programme on the probability to be non-employed for incumbent households is effectively zero. Hence, although the programme prevents the non-employed to move into public housing, it does not improve or worsen labour market opportunities of incumbent households independent of whether they are residing in public or private housing. We also do not find any effect on income or other (time-varying) demographic characteristics.

In sum, the results indicate that the Act indeed implies sizeable redlining effects in preventing non-employed from entering public housing, but it did not otherwise affect the demographic composition of the treated areas, nor did it improve outcomes of incumbent individuals.

4.2. House price effects: baseline results

Does the announcement of the policy also implies that households have changed their perception about areas after announcement of the designated areas? To study this question, we use house prices as dependent variable and estimate Equation (4). Table 4 reports the results.

We start in Column (1) with a standard specification with property characteristics, street and municipality-by-year-fixed effects. The coefficient indicates that prices change by $(\exp(-0.0258) - 1) \cdot 100\% = -2.5\%$. One may argue that this may be due to differences in spatial unobservable characteristics. To control for all time-invariant housing and location attributes, we include property-fixed effects in Column (2). The coefficient is somewhat stronger and all non-repeated sales are dropped from the estimation (about 50%). One may still argue that spatial unobservables may change differently over time in and close to WBMGP areas. In Column (3), we therefore further restrict the sample to transactions within 250 m of a WBMGP border, leading to a similar estimate.

Our preferred estimate is reported in Column (4), where we only keep observations within 100 m of the WBMGP border. The estimate implies that prices reduce by 4.2% in treated areas, but not outside those areas. We think it is very unlikely that changes in amenity values outside WBMGP areas can explain this result because local amenities are arguably continuous over space. To support our results, we just keep transactions within 50 m of a WBMGP border in Column (5). The coefficient now becomes even stronger, but less precise.

Table 4. Baseline price regressions

Dependent variable:	Log of house price per m2				
	Baseline	+ Property f.e.	<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)
WBMGP implemented	−0.0258***	−0.0329**	−0.0285**	−0.0428**	−0.0600**
	(0.0094)	(0.0138)	(0.0122)	(0.0176)	(0.0287)
Property characteristics	✓	✓	✓	✓	✓
Street-fixed effects	✓				
Property-fixed effects		✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓
Number of observations	230,425	120,449	11,986	4,729	2,414
R^2	0.7816	0.9233	0.9176	0.9238	0.9309

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

4.3. House price effects: alternative explanations and robustness

Hence, we find a consistent negative effect in WBMGP areas after announcement. This may be explained by the presence of stigma, which led prospective buyers to value properties in announced areas less. In this subsection, we consider a couple of alternative explanations for the price effects we find.

4.3.1. Pre-treatment differences in amenities and pre-trends. One may argue that the announcement of the Act may coincide with a declining price trend in WBMGP areas related to large differences in amenities *before* treatment. We believe that this argument is not so convincing, because we focus on very local price differences.

In order to investigate this further we estimate *cross-sectional* regressions in Table 5 where we control for a host of property characteristics and municipality-by-year-fixed effects. When keeping transactions within 250 m of a WBMGP border, we indeed find a negative effect: properties in WBMGP areas seem to be about 6% cheaper before treatment. However, when we concentrate on areas closer to the borders we do not find any price differential, in line with the idea that amenities are continuous over space.

In Columns (4)–(6), we investigate prices after implementation. There is a strong negative price differential within 250 m. This effect is considerably smaller and in line with earlier estimates if we narrow the sample to just 100 m or 50 m from the border (and the differences between the after and before implementation effects remain roughly −3%).

For difference-in-differences approaches, and related approaches such as the boundary discontinuity regression approach applied here, it is common to apply event studies to examine differences in pre-trends. However, in the context of house prices, they may

Table 5. House price regressions: before and after (Dependent variable: log of house price per m^2)

Dependent variable:	Log of house price per m2					
	Before implementation			After implementation		
	<250 m	<100 m	<50 m	<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP treatment group	−0.0644***	−0.0188	0.0026			
	(0.0141)	(0.0149)	(0.0176)			
WBMGP implemented				−0.0959***	−0.0369**	−0.0271
				(0.0168)	(0.0180)	(0.0217)
Property characteristics	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	15,715	6,329	3,138	9,455	3,989	2,429
R^2	0.5626	0.6417	0.6869	0.5970	0.6489	0.6510

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. We further include dummies with respect to house type (terraced, semi-detached and detached) and construction year decades. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

be less informative because the percolation of information about the Act, translating into lower house prices, may be slow, or because house sellers anchor their sales prices (Turnbull and Sirmans, 1993; Ihlanfeldt and Mayock, 2012). In other words, in contrast to, for example, stock market prices, it is uncommon that house prices jump discretely. More specifically, the salience of the stigma effect may increase over time as more home buyers may become aware of the negative stigma associated with the targeted areas.

These events studies still allow us to examine anticipation effects. These are thought to be important, because formal announcement of the Act occurs at least a couple of months *before* the actual treatment, after a local political debate which may have been reported in local media, it is plausible that prices already adjust downwards a year or so before treatment.

In Appendix A.4, we report event studies showing that there is no evidence for pretrends. Indeed, 1 year before the actual treatment prices are about 2.5% lower, in line with the results reported in Column (5) in Table 5. After 2 years, the price discount increases to about 7%. In other words, we do not find evidence that the stigma effect dissipates over time. We explore this further in Appendix A.4 where we show that the effect increases to about 10% after 7 years and does not become smaller.

4.3.2. WBMGP streets versus neighbourhoods. In the reporting on the targeted areas, the media mostly paid attention to the targeted neighbourhoods rather than the targeted streets. Moreover, neighbourhoods, which are clearly defined by `Statistics Netherlands`, are typically known to the public, which is much less

Table 6. Price regressions, WBMGP neighbourhoods versus streets

Dependent variable:	Log of house price per m2					
	WBMGP neighbourhoods			WBMGP streets		
	<250 m	<100 m	<50 m	<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0235**	−0.0208	−0.0560**	0.0013	0.0016	0.0070
	(0.0116)	(0.0166)	(0.0275)	(0.0202)	(0.0264)	(0.0317)
Property characteristics	✓	✓	✓	✓	✓	✓
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	6,492	2,381	958	5,494	2,333	1,445
R^2	0.9246	0.9265	0.9457	0.9139	0.9165	0.9201

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. We further include dummies with respect to house type (terraced, semi-detached and detached) and construction year decades. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

the case with streets. Consequently, a priori, one expects that the effects on targeted neighbourhoods would be more pronounced than on targeted streets. In Table 6, we report the results for neighbourhoods and streets separately.

Columns (1)–(3) focus on neighbourhoods and show that within 250 m we find a negative price effect of –2.4%. The effect is very similar once we reduce the distance to the nearest WBMGP neighbourhood to 100, but the coefficient ceases to be statistically significant at conventional levels. Similar to the baseline results, we find that the effect becomes stronger and statistically significant when we reduce the maximum distance to 50 m. All in all, the results are very comparable to the baseline results reported in Table 4.

In columns (4)–(6), we only keep transactions in and close to WBMGP streets. In line with the anecdotal evidence that the posting of targeted WBMGP streets was less prominent, we now find point estimates that are virtually zero across all specifications. One may therefore conclude that any discrete stigma effect only pertains to neighbourhoods rather than to streets. However, an important caveat is that the standard errors are too large to draw definite conclusions.

4.3.3. Induced changes in neighbourhood composition. Let us now consider the possibility that induced changes in neighbourhood composition may also affect prices. If this is the case, then this may even increase the magnitude of estimated stigma effects because, if anything, the neighbourhood composition has improved due to a decrease in the share of non-employed. To test this we match the housing transactions data to the micro-data from `Statistics Netherlands` and calculate the average of demographic characteristics in the street. Table 7 reports the results.

Table 7. Price regressions, controlling for neighbourhood composition

Dependent variable:	Log of house price per m2					
	Replication			Controlling for neighbourhood composition		
	<250 m	<100 m	<50 m	<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0289**	−0.0428**	−0.0708***	−0.0272**	−0.0417**	−0.0759***
	(0.0124)	(0.0177)	(0.0271)	(0.0114)	(0.0167)	(0.0243)
Share non-employed in street				−0.1615**	−0.2338**	−0.3439**
				(0.0683)	(0.1021)	(0.1405)
Average income in street (log)				0.0587*	0.0144	−0.0323
				(0.0315)	(0.0406)	(0.0568)
Share low-skilled in street				−0.1544***	−0.1395**	−0.2520***
				(0.0364)	(0.0574)	(0.0746)
Share foreign-born in street				−0.2740***	−0.3308***	−0.4810***
				(0.0701)	(0.1067)	(0.1379)
Share retired in street				−0.0643	−0.0545	−0.1637
				(0.0577)	(0.0919)	(0.1107)
Share single households in street				0.0011	−0.0657	−0.1629
				(0.0465)	(0.0802)	(0.1045)
Property characteristics	✓	✓	✓	✓	✓	✓
Year-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	8,619	3,414	1,733	8,616	3,412	1,731
R^2	0.9133	0.9156	0.9156	0.9158	0.9185	0.9220

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. We further include dummies with respect to house type (terraced, semi-detached and detached) and construction year decades. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Because we lose some observations when merging the `NVM` data to data from `Statistics Netherlands`, in Columns (1)–(3) we first replicate the regression as reported in Columns (3)–(5) in Table 4 for this slightly more selective sample. The coefficients are very similar.

Columns (4)–(6) then control for average neighbourhood composition in the street. It appears that the results of the policy are essentially identical. We find results for neighbourhood composition that are familiar in the literature. House prices are lower in streets with higher shares of (i) non-employed, (ii) low-skilled or (iii) foreign-born. However, because changes in neighbourhood composition induced by the policy are small, as we have seen above, the coefficient capturing the impact of the WBMGP is not much affected.

4.3.4. Robustness checks. In Table 8, we first aim to address the issue of negative weights in our staggered difference-in-difference design. We do so by estimating Equation (6), implying that we include nearest treatment group-by-year-fixed effects. In

Table 8. House price regressions: nearest treatment group-by-year-fixed effects

Dependent variable:	Log of house price per m2					
	Nearest treatment group× year-fixed effect			+ Irreversibility of treatment		
	<250 m	<100 m	<50 m	<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0241**	−0.0279*	−0.0538**	−0.0285**	−0.0290*	−0.0560**
	(0.0108)	(0.0164)	(0.0245)	(0.0112)	(0.0167)	(0.0244)
Property characteristics	✓	✓	✓	✓	✓	✓
Property-fixed effects	✓	✓	✓	✓	✓	✓
Nearest treatment group× year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	11,837	4,462	2,123	11,710	4,417	2,105
R^2	0.9321	0.9469	0.9627	0.9323	0.9468	0.9622

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

this way, we only exploit variation in prices across both sides of a WBMGP border and do not compare price changes across neighbourhoods within a municipality. In Columns (1)–(3), we show that the results are very similar. The point estimates in Columns (1) and (3) are virtually the same when compared with the baseline results reported in Table 4. The coefficient in Column (2) is somewhat lower, although the estimate is not statistically significantly lower. In Columns (4)–(6), we further exclude transactions in areas that have been treated before but are untreated later on. This excludes a small number of observations (about 1%). Unsurprisingly, this does not change the results.[20]

Table 9 reports other robustness checks. In Column (1), we include even more detailed fixed effects to control for spatially changing unobservables. More specifically, we include neighbourhood-by-year-fixed effects, which lead to very similar outcomes.

In Column (2), we control for other spatial programmes that were enacted during the study period. Because we focus on very local spatial price differentials, we do not think this is an issue. Indeed, when we include dummies for the NPRZ and whether a neighbourhood is part of the KW programme (both are discussed in the next section), the results are essentially unaffected.

In Column (3), we address the issue that borders of WBMGP areas may intersect with main roads, rivers and municipal borders. More specifically, we remove portions of borders that overlap with these features and recalculate for each property the

20 One may suspect that prices increase again once the WBMGP status is reversed. Unfortunately, we have too few observations to identify this effect.

Table 9. House price regressions: robustness

Dependent variable:	Log of house price per m2					
	+ Neighbourhood ×year f.e.	+ Other programs	Boundary selection	Only Rotterdam	Exclude Rotterdam	Exclude Rotterdam-South
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0487**	−0.0347**	−0.0481***	−0.0383	−0.0441**	−0.0325*
	(0.0232)	(0.0169)	(0.0178)	(0.0386)	(0.0183)	(0.0166)
Property characteristics	✓	✓	✓	✓	✓	✓
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects		✓	✓	✓	✓	✓
Neighbourhood×year-fixed effects	✓					
Number of observations	4,255	4,729	3,634	1,076	3,653	4,360
R^2	0.9549	0.9251	0.9262	0.9097	0.9285	0.9267

Notes: We only include properties that are within 100 m of WBMGP border. Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

distance to these adjusted borders. The coefficient becomes slightly stronger, but not significantly so.

The first city that implemented the Act was Rotterdam. In Column (4) in Table 9, we re-estimate our regressions where we only include observations in Rotterdam. Although this strongly reduces the number of observations (so the estimate is very imprecise), it does not materially affect the point estimate.[21] Column (5), instead, only includes observations outside of Rotterdam confirming the negative baseline estimate. Finally, because most targeted areas are in the southern part in Rotterdam (see Figure 1(a)), Column (6) shows that our results are robust if we exclude those neighbourhoods.

In Table 10, we investigate alternative identification strategies to identify the stigma effect. In Column (1), we include observations in treated and so-called runner-up neighbourhoods. Neighbourhoods that were considered but in the end not targeted were mentioned in policy documents in Rotterdam and Nijmegen. The runner-up neighbourhoods were not widely published in the press and it is therefore unlikely that these neighbourhoods also encountered stigma effects. If we compare price developments in treated and those runner-up neighbourhoods, we find again a negative price effect that is comparable to previous estimates.

21 We have also estimated models with less detailed location-fixed effects. When we use street-fixed effects rather than property-fixed effects, the size of the effect becomes again statistically significant at the 10% level.

Table 10. House price regressions: identification revisited and placebo

Dependent variable:	Log of house price per m2					
	Runner-up neighbourhoods	Neighbourhood rank	Time variation only	Placebo treatment		
				<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	-0.0365^{***}	-0.0597^{***}	-0.0488^{***}			
	(0.0119)	(0.0178)	(0.0103)			
WBMGP placebo neighbourhood				0.0211^{*}	0.0168	0.0108
				(0.0126)	(0.0151)	(0.0169)
Property characteristics	✓	✓	✓	✓	✓	✓
Property-fixed effects	✓	✓	✓	✓	✓	✓
Rank-by-year trends		✓				
Municipality×year-fixed effects	✓	✓		✓	✓	✓
Travel-to-work-area× year-fixed effects			✓			
Number of observations	5,928	7,017	6,320	6,465	2,647	1,534
R^2	0.9091	0.8914	0.9059	0.9244	0.9285	0.9336

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

In Column (2), we consider a list of neighbourhoods published by the municipality of Rotterdam that ranks neighbourhoods according to their degree of deprivation, with higher ranks being more likely to be treated. We find again a negative effect once we control for rank-by-year trends. In Column (3), we only use variation in timing of the treatment by only including observations in areas that are or will be treated in the future. Because within municipalities there is very little variation in the timing we do not include municipality-by-year-fixed effects, but instead include travel-to-work-area×year-fixed effects (where the municipalities Rotterdam, Schiedam, Vlaardingen and Capelle aan de IJssel are part of the same travel-to-work area, see Figure 1(a)). The coefficient once more confirms the negative effect we found earlier, even with smaller standard errors.

In the last three columns of Table 10, we undertake a 'placebo' analysis by considering the runner-up neighbourhoods as placebo neighbourhoods, while excluding treated neighbourhoods from the analysis. If anything, we find a small *positive* effect once we focus on areas within 250 m of a placebo neighbourhood in Column (4) of Table 10. However, this effect goes away if we reduce the threshold distance to the nearest placebo border in Columns (5) and (6).

Finally, in Table 11, we use ancillary information from the NVM data on time on the market and list prices. One may argue that the announcement of the policy may have

Table 11. Time on the market and list prices

Dependent variable:	Log of days on the market			Log of sales price over list price		
	<250 m	<100 m	<50 m	<250 m	<100 m	<50 m
	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	0.0547	−0.0481	0.0625	−0.0069	−0.0042	−0.0151
	(0.0816)	(0.1373)	(0.1868)	(0.0046)	(0.0075)	(0.0108)
Property characteristics	✓	✓	✓	✓	✓	✓
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	11,638	4,576	2,330	11,834	4,651	2,380
R^2	0.5821	0.6054	0.6174	0.6113	0.6392	0.6573

Notes: Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

affected sellers and prospective buyers differently. For example, if only sellers would respond to the announcement of the policy by listing their house on the market, this may affect local housing supply, which in turn could lead to longer sales times and reduced market power of sellers.

In Columns (1)–(3) of Table 11, we repeat the baseline regressions, but taking time on the market as dependent variable. We drop observations for which time on the market is zero or exceeds 6 years. We find effects that are statistically insignificant and quantitatively small, particularly given that the variance in time on the market substantially exceeds the variance in sales prices.

In Columns (4)–(6), we test whether the ratio of sales prices to list prices has changed due to the announcement of the Act, to investigate whether bargaining power of sellers or buyers may have changed. We remove sales-to-list-price ratios lower than 50% or larger than 150%. We do not find evidence that bargaining power is materially affected.

All in all, these results confirm the negative price effects we find in Table 4 and reinforce the conclusion that the reductions in sales prices are likely the result of stigma.

4.4. Overall house price effects induced by the policy

One of the attractive features of hedonic price analyses is the possibility to calculate overall house price effects of policies. This is particularly so for policies that have a small effect on prices and treat a small number of units, that is, have a marginal impact, so equilibrium effects can be ignored because they are second-order (Banzhaf, 2021). The policy is marginal because the estimated effect size of the Act is moderate, just –4%, and only a small percentage of houses, about 5%, are treated.

We start from the assumption that the (policy-intended) changes in the neighbourhood composition through household sorting have a negligible effect on overall housing market values. One justification for this assumption is that the change in the share of non-employment induced by the policy is limited. Another justification would be to assume that utility depends linearly on the share of employed nearby. In that case, at the city level, the average house price is not affected by the distribution of employment.

Furthermore, we will assume that the future is discounted at a given rate and that the stigma effect is believed to be permanent. For convenience, we will also assume that the same stigma effect, although calculated for the owner-occupied market, applies to households in the private rental market and to public housing. The last two assumptions are debatable, but can be easily adjusted. For that reason, we will also show the results by housing tenure. Only considering owner-occupied housing then provides an underestimate of the overall stigma effect.

We will treat the status of the residential location as a (continuous) neighbourhood attribute that determines the household utility with a, for the household, given price that depends on neighbourhood location, as is common in studies that focus on local air pollution or crime. Such an assumption may not be unreasonable in the light that human beings strongly care about reputation of the goods they consume, as is shown in the way they spend money on expensive brand clothes and other status consumer goods. Furthermore, human beings like to portrait themselves as successful.[22]

Given these, we believe plausible, assumptions, our preferred estimates, as provided in Column (4) of Table 4, imply that the negative house price effect induced by the policy is about 4% of the price of the directly affected housing units. We also calculate the effects per city using estimates by city shown in Appendix A.4.

In Table 12, we provide a back-of-the-envelope calculation of the house price effects per housing unit as well as the overall house price effects. Given an average house price in WBMGP areas of about €168,000 in our sample (in 2020 prices), the annualized housing market loss per household, given a discount rate of 3.5%, is about €200. Because house prices are somewhat higher for owner-occupied housing, the welfare loss is somewhat higher for households owning a property. The total annual loss in housing values due to the WBMGP is €11.5 million, which is substantial.

Arguably, households living in public housing have a lower willingness to pay to avoid stigma (as their household incomes are lower) so the average loss may be less. By only taking into account the owner-occupied market, we have a useful lower bound of the annual loss due to stigma effect of about €4 million annually.[23] On the other hand, recall

22 This is also recognized in defamation law, where the importance of reputation for persons, without being well-defined, is acknowledged (Post, 1986). In a similar way, households care about the reputation of their residential location, also because this reputation may have economic consequences (see Tootell, 1996; Zenou and Boccard, 2000; Besbris et al., 2015; Carlsson et al., 2018).

23 The above house price effects may be interpreted as welfare effects given certain assumptions (Banzhaf, 2021). The main criticism of such an interpretation is that a *negative* status is treated as a standard consumer good, whereas, in fact, it must be treated as a positional good. For positional goods, it is usually argued that their positive reputation imposes negative positional externalities which lead to wasteful spending in a consumption rat race (Frank, 1985). This begs the question

Table 12. Overall house price effects of the policy

		Annual effect per property (in €)			
		Average effect	Owner-occupied	Private rental	Public housing
Annual effect per property		−196*** (66)	−252*** (85)	−162*** (54)	−181*** (61)
		Total annual effects (in €)			
	Treated units	Total effect	Owner-occupied	Private rental	Public housing
Total effect	58,169	−11,409,232*** (3,839,895)	−3,943,288*** (1,327,154)	−2,005,429*** (674,948)	−5,460,515*** (1,837,793)

Notes: We assume a discount rate of 3.5% (see Koster et al., 2018) and calculate 2020 housing values using assessed housing prices and the consumer price index. Bootstrapped standard errors (250 replications) are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

we only capture the discrete jump in stigma at WBMGP borders, while at least part of the stigma effects may be continuous over space. To the extent the continuous stigma effect is important, the annual loss of €11.5 million per year may be an underestimate.

5. OTHER PLACE-BASED PROGRAMMES

The WBMGP is quite a particular programme and legal redlining is often not part of place-based programmes. Hence, in this section, we consider two alternative place-based programmes, which did not imply redlining but still may have induced negative house price effects. We consider the NPRZ as well as the KW programme.

5.1. Nationaal Programma Rotterdam Zuid

The NPRZ aimed to improve neighbourhoods in Rotterdam South since 2012. The aims are to improve school performance of children, labour market opportunities of young workers as well as the liveability of the neighbourhood. In Figure 4, we indicated 11 targeted neighbourhoods, for which there is substantial overlap with the WBMGP areas. To avoid overlap, we exclude observations in WBMGP neighbourhoods.

Like before we calculate the distance to the nearest border of a NPRZ neighbourhood, where we disregard borders between two NPRZ neighbourhoods. We then again

whether a negative reputation as identified in our paper creates a positive positional externality, which would imply that households who do not live in stigmatized areas derive utility from that other households are stigmatized. We believe that there is no evidence that human beings feel in this way, that is, that such a positive position externality is present (by contrast, it seems that most individuals feel sorry for others who are worse off). If we are wrong here (so a negative reputation is a positional good), then the welfare loss of the induced stigma would be less than indicated above and potentially zero.

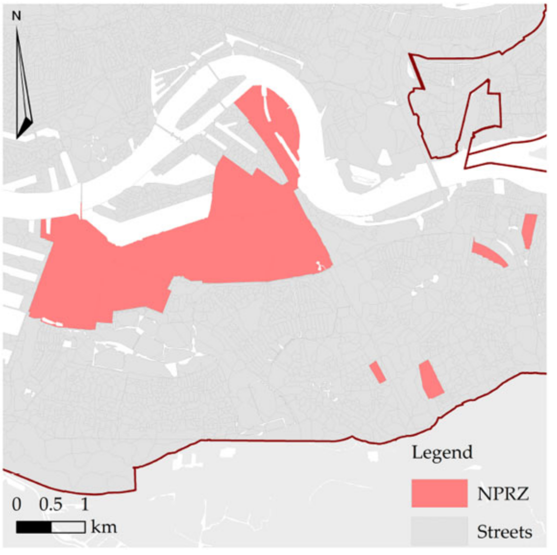

Figure 4. NPRZ programme in Rotterdam

compare price changes very close to borders of treated neighbourhoods. Column (1) in Table 13 shows that within 250 m, there is a negative effect although it is statistically insignificant. However, when we focus on areas within 100 m of an NPRZ border, the coefficient becomes statistically significant. Hence, the negative effect in NPRZ neighbourhoods again points at the presence of negative stigma effects. The magnitude of the point estimates is even somewhat larger than the estimates for the WBMGP, as the coefficient indicates that the programme reduced house prices by 5.7%. Note however that because of the larger standard error, the confidence interval of this estimate is quite wide and the null hypothesis that the stigma effect of this programme is identical to the WBMGP programme cannot be rejected.

5.2. KW programme

An alternative programme was the KW programme. The main aim of this programme was to improve quality of public housing units by demolition and renovation. About €1 billion was spend in 40 neighbourhoods over the course of 5 years, starting in 2007, which amounted to about €2800 per household per year. Neighbourhoods were treated when the deprivation score exceeded a certain threshold. For each neighbourhood in the Netherlands, deprivation z-scores were calculated based on social and physical deprivation and problems. The z-score ranges from –6 to 12. The cut-off to receive treatment is 7.3, although some neighbourhoods in the end were not selected although they had a score exceeding 7.3. Moreover, two neighbourhoods with z-scores below 7.3 were targeted.

Table 13. House price regressions: other programmes

Dependent variable:	Log of house price per m2					
	NPRZ program		KW program			
			All neighbourhoods		Rank ≤ 20	
	<250 m (1)	<100 m (2)	<250 m (3)	<100 m (4)	<250 m (5)	<100 m (6)
NPRZ implemented	−0.0205 (0.0286)	−0.0584** (0.0249)				
KW implemented			0.0143** (0.0057)	0.0246*** (0.0089)	0.0167 (0.0143)	0.0475 (0.0311)
KW ranking announced			−0.0177*** (0.0060)	−0.0082 (0.0095)	−0.0323** (0.0149)	−0.0534** (0.0253)
Property characteristics	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	1,187	570	49,904	18,315	6,737	2,096
R^2	0.9334	0.9518	0.9510	0.9500	0.9571	0.9524

Notes: We exclude observations in WBMGP neighbourhoods. Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. We further include dummies with respect to house type (terraced, semi-detached and detached) and construction year decades. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

In Koster and Van Ommeren (2019), this programme is studied in detail using a fuzzy regression-discontinuity design (RDD), and using the z-score as a running variable. They find a positive effect on house prices of about 3.5%, indicating that the programme was successful in improving targeted neighbourhoods. Neighbourhoods just outside KW neighbourhoods were excluded, because these neighbourhoods were indirectly treated due to spillovers. Here, we will exploit nationwide data on house prices, rather than only the eight aforementioned cities. Again, we exclude observations in WBMGP areas throughout. As we have a slightly different dataset with more years, and a slightly different methodology with more controls, we replicate the results of Koster and Van Ommeren (2019) in Appendix A.5, where we find effects of about 3–4.5%.

In this paper, we are interested in stigma effects of treated neighbourhoods. Although a list of the 40 worst neighbourhoods was published in September 2007, there was neither information published on the exact postcodes that were targeted, nor on the ranking of those neighbourhoods. After a successful appeal was made to the Freedom of Information Act, the government published the exact ranking of neighbourhoods in February 2009, which in turn received considerable attention in the press (see Het Parool, 2009; NU.nl, 2009; Trouw, 2009).

We then create two dummy variables whether a property is located within a targeted neighbourhood after September 2007 (when the programme started) and after February 2009 (when the ranking was made public). In Column (3) of Table 13, we include properties within 250 m of borders of KW neighbourhoods. We then find a small positive effect of the KW programme of 1.4%. This estimate makes sense, as it picks up the difference between treated neighbourhoods and nearby neighbourhoods. If spillover effects are important, this means that the effect should be smaller than the baseline effect of 3.5%. More importantly for the current paper, the announcement has a negative and statistically significant effect of about 1.8%. In Column (4), where we only include properties within 100 m of KW neighbourhood borders, this announcement effect turns statistically insignificant (whereas the effect of the KW implementation is slightly larger).

One may argue that stigma effects mainly applied to the most deprived KW neighbourhoods, as, out of 40 neighbourhoods, these were the most frequently discussed in the press. Hence, in Columns (5) and (6) of Table 13, we only include observations in the 20 most deprived neighbourhoods according to the ranking. In Column (5), unsurprisingly, we do not find a statistically significant effect of the KW programme, because of larger standard errors. However, the announcement dummy is strong, negative and statistically significant at the 5%. The coefficient indicates that the coefficient implied a price discount in KW neighbourhoods of 3.2%. The announcement effect becomes somewhat stronger if we reduce the threshold distance to a mere 100 m in Column (6).

Hence, for the KW programme, given the negative effects on prices, stigma effects also seem to be present and particularly apply to the most deprived neighbourhoods, as these neighbourhoods received the most (negative) attention in the press.

6. CONCLUSIONS

We provide causal evidence of a sizeable negative price effect in the housing market incurred by place-based policies that publicly announce which neighbourhoods are deprived. We thereby provide suggestive evidence that these policies induce a stigma effect. We emphasize here that the evidence can be interpreted as suggestive because we do not have a direct quantitative measure for neighbourhood stigma so our evidence for neighbourhood stigma is based on a residual interpretation after having disproved other interpretations. Annual housing market losses due to the policy are estimated to be about €200 for households residing in treated neighbourhoods, as reflected by house price drops of about 4%. Presumably because the prominent media reporting mostly applied to targeted neighbourhoods, rather than to streets, we do not find, in sharp contrast to targeted neighbourhoods, statistically significant price drops for targeted streets.

The presence of this negative price effect has been established for three different place-based policies in the Netherlands, which strongly adds to the external validity of our findings. The finding of a negative price effect in the housing market points towards a stigma effect. This complements a large literature that focuses on high status goods with little or no attention to low status goods (Bursztyn et al., 2017). The presence of a stigma effect addresses the puzzle of why some studies find statistically insignificant or even negative price effects of place-based policies that are thought to be beneficial for households.

Another contribution to the literature is that we evaluate the effectiveness of a large Dutch programme that implies redlining, by preventing the non-employed from moving into public housing. Such a policy is highly controversial, but, nevertheless, has come to the fore in the Netherlands, Denmark and Sweden. There appears to be very little evidence for changes in the demographic composition induced by the policy, except for reductions in the share of non-employed, which is the 'mechanical' effect induced by the policy. The policy reduced the share of non-employed persons by about 1.5 percentage points.

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APPENDIX

A.1 100M BUFFERS

In Figure A1, we show a sample map of Rotterdam to indicate the size of treated and control areas. We show the streets in Rotterdam-West that have been treated at least once in the study period and draw 100 m around those areas. It is easily observed that 100 m buffers are small and only include properties that are very close to targeted areas.

A.2 ADDITIONAL DESCRIPTIVE STATISTICS

Here, we provide additional descriptive statistics for the demographic data. In Table A1, we therefore show summary statistics within 100 m of WBMGP borders in- and outside WBMGP areas. We find a slightly higher share of non-employed and lower incomes inside WBMGP borders, but the differences are considerably smaller than when using the full extent of our data. Importantly, the share of public housing is comparable on both sides of the border.

A.3 ADDITIONAL RESULTS FOR REDLINING

In this Appendix section, we will provide some additional results with respect to the effects of the Act on the demographic composition of the targeted areas.

First, we extend the event study of the effect of the Act on non-employment to 10 years before and 5 years after the treatment. We find in Figure A2 that the effect

Figure A1. Rotterdam, sample map

Table A1. Descriptive statistics of individual characteristics, <100 m

	Inside WBMGP areas				Outside WBMGP areas			
	(1) Mean	(2) SD	(3) Min	(4) Max	(5) Mean	(6) SD	(7) Min	(8) Max
WBMGP implemented	0.397	0.489	0	1	0	0	0	0
Years of WBMGP treatment	0.5310	3.820	0	1	0	0	0	0
Distance to WBMGP border (in m)	0.0423	0.0288	0	0.1000	0.0514	0.0271	0.000135	0.1000
Non-employed	0.234	0.420	0	1	0.199	0.397	0	1
Long-term non-employed	0.193	0.395	0	1	0.167	0.373	0	1
Annual income (in €)	47,563	42,055	1,200	999,756	53,329	44,348	1,200	999,756
Low-skilled	0.604	0.489	0	1	0.592	0.491	0	1
Foreign-born	0.376	0.484	0	1	0.329	0.470	0	1
Pension receiver	0.113	0.316	0	1	0.164	0.370	0	1
Household – single	0.416	0.489	0	1	0.379	0.482	0	1
Public housing	0.530	0.499	0	1	0.513	0.500	0	1
Male	0.504	0.500	0	1	0.493	0.500	0	1
Year of observation	2012	5.349	2003	2019	2012	5.343	2003	2019

Notes: The number of observations is 896,165 for observations inside WBMGP areas and 852,225 outside WBMGP areas. Note that the number of observations may differ slightly per variable dependent on data availability. We remove the top and bottom 20 observations to ensure confidentiality.

generally increases somewhat and is -2.5% 6 years after the treatment. If we take the baseline specification in Column (1), Table 3, we would predict an effect of $-0.36 \times 6 = -2.2$ percentage points, which is very close to the effect displayed here.

Second, in Table A2, we replicate the results from Table 3 but only include properties within 50 m of a WBMGP border. It is shown that the effect on non-

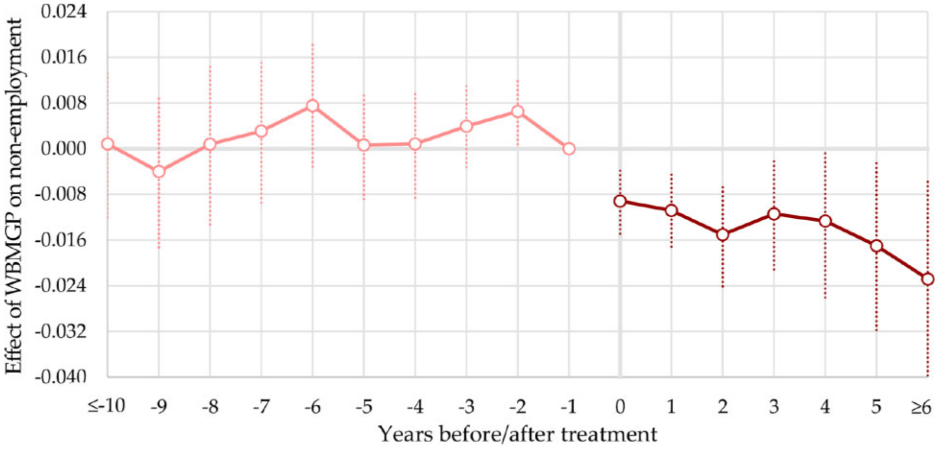

Figure A2. Effects on the share of non-employed: extended event study

Table A2. Redlining regressions, within 50 m of a WBMGP border

Dependent variable:	Non-employed	Log of income	Low-skilled	Foreign-born	Retired	Single
Panel A: All properties	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0045***	0.0028	0.0027*	0.0013	−0.0012	−0.0018*
	(0.0013)	(0.0018)	(0.0015)	(0.0018)	(0.0014)	(0.0010)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	876,357	864,076	588,945	929,136	876,357	876,357
R^2	0.5832	0.6330	0.5577	0.5610	0.7268	0.6447
Panel B: Only public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0046***	0.0026	0.0007	−0.0012	0.0011	0.0023*
	(0.0018)	(0.0018)	(0.0016)	(0.0021)	(0.0024)	(0.0014)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	445,411	441,908	296,381	462,992	445,441	445,441
R^2	0.5894	0.6162	0.5533	0.5668	0.7416	0.6692
Panel C: No public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0017	0.0006	−0.0007	0.0040	−0.0019	0.0002
	(0.0014)	(0.0028)	(0.0022)	(0.0025)	(0.0012)	(0.0013)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	416,454	408,032	282,384	450,418	416,454	416,454
R^2	0.5060	0.6131	0.5179	0.5594	0.7002	0.6238

Notes: We only include properties within 50 m of a WBMGP border. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

employment becomes even slightly stronger. We observe a reduction in non-employment of 0.45 percentage points for each year of treatment. This effect is essentially the same when only including public housing, but it turns statistically insignificant for the private rental and owner-occupied market, as anticipated.

Table A3. Redlining regressions, non-linear effects

Dependent variable:	Non-employed	Log of income	Low-skilled	Foreign-born	Retired	Single
Panel A: Only public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0090***	0.0009	−0.0025	−0.0010	0.0028	0.0058**
	(0.0031)	(0.0030)	(0.0031)	(0.0030)	(0.0034)	(0.0023)
(Years of WBMGP treatment)^2	0.0005**	0.0002	0.0001	0.0000	−0.0002	−0.0004*
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	832,452	826,303	540,491	862,804	832,452	832,452
R^2	0.5651	0.6032	0.5375	0.5682	0.7262	0.6615
Panel B: No public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0022	−0.0067	−0.0002	0.0055*	−0.0016	0.0059**
	(0.0026)	(0.0051)	(0.0038)	(0.0032)	(0.0024)	(0.0026)
(Years of WBMGP treatment)^2	0.0001	0.0006*	−0.0001	−0.0000	−0.0004**	0.0002
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0001)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	724,200	711,904	477,831	774,257	724,200	774,257
R^2	0.4770	0.6117	0.4951	0.5513	0.6961	0.6210

Notes: We only include properties that are within 100 m of WBMGP border. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

While we found a small positive effect on income within 100 m, this effect ceases to be statistically significant when restricting the distance to just 50 m of a WBMGP border. Apart from the reduction in non-employment we do not find strong and significant changes in the demographic composition of the affected areas. Hence, this confirms that apart from the 'mechanical' redlining effects the policy does not seem to be effective in considerably changing the demographic composition of the targeted areas.

Third, in Table A3, we test whether non-linearities are important in the treatment effect by adding a second-order effect of the years of treatment variable. We distinguish between public housing in Panel A and outside of public housing in Panel B. We find only very weak evidence that the effect becomes slightly less strong over the years. The coefficient implies that in the first year non-employment is reduced by about 0.65 percentage points, while this is 2.75 percentage points after 5 years.

Fourth, one may wonder what explains the negative effects on being non-employed. We expect that the effect entirely operates via fewer non-employed people moving into public housing in treated areas. Still, we also will test whether the outflow is affected, as well as incumbent people living in current housing. In Table A4, we only keep individuals who have lived in a different location in the previous year. Moreover, because we focus on the inflow of people into new housing it does not make sense to use elapsed duration so we use a dummy whether a property is in a

Table A4. Redlining regressions, effects of moving in

Dependent variable:	Non-employed	Log of income	Low-skilled	Foreign-born	Retired	Single
Panel A: Only public housing	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0600***	0.0547**	−0.0170	−0.0366**	0.0245*	0.0055
	(0.0164)	(0.0258)	(0.0243)	(0.0159)	(0.0128)	(0.0174)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	72,030	70,622	56,234	86,016	72,030	72,030
R^2	0.5373	0.5194	0.4346	0.4040	0.7297	0.5242
Panel B: No public housing	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0159	0.0456**	−0.0039	0.0034	−0.0007	0.0043
	(0.0107)	(0.0223)	(0.0146)	(0.0128)	(0.0044)	(0.0151)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	91,438	86,826	73,502	120,584	91,438	91,438
R^2	0.4377	0.5320	0.4498	0.4578	0.6035	0.4804

Notes: We only include properties that are within 100 m of WBMGP border. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table A5. Redlining regressions, effects of moving out

Dependent variable:	Non-employed	Log of income	Low-skilled	Foreign-born	Retired	Single
Panel A: Only public housing	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	0.0101	−0.0309	0.0390	0.0190	−0.0011	−0.0319
	(0.0629)	(0.0623)	(0.0627)	(0.0417)	(0.0263)	(0.0475)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	16,867	16,589	12,108	18,774	16,867	16,867
R^2	0.7029	0.6744	0.4785	0.4621	0.8061	0.7147
Panel B: No public housing	(1)	(2)	(3)	(4)	(5)	(6)
WBMGP implemented	−0.0063	0.0149	0.0304	0.0027	0.0005	−0.0311
	(0.0287)	(0.0706)	(0.0361)	0.0385)	(0.0117)	(10.0361)
Property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	17,299	16,331	12,015	20,581	17,299	17,299
R^2	0.6364	0.6280	0.5349	0.5240	0.7645	0.6365

Notes: We only include properties that are within 100 m of WBMGP border. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

treated area. In line with expectations, we find a strong and negative effect of the WBMGP designation on the probability to be non-employed. Unsurprisingly, the effect is considerably stronger because the WBMGP restricts inflow. Note that we also find positive effects on income for individuals outside of public housing. This appears

Table A6. Redlining regressions, with individual-fixed effects

Dependent variable:	Non-employed	Log of income	Low-skilled	Foreign-born	Retired	Single
Panel A: Only public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	0.0002	0.0018	0.0015	–	−0.0001	0.0014
	(0.0016)	(0.0015)	(0.0014)	(–)	(0.0016)	(0.0009)
Individual×property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	800,880	795,203	513,661	826,174	800,880	800,880
R^2	0.7565	0.8005	0.9202	1.0000	0.8721	0.8626
Panel B: No public housing	(1)	(2)	(3)	(4)	(5)	(6)
Years of WBMGP treatment	−0.0005	−0.0015	−0.0006	(–)	−0.0009	0.0006
	(0.0011)	(0.0018)	(0.0014)	(–)	(0.0012)	(0.0010)
Individual×property-fixed effects	✓	✓	✓	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	678,513	668,108	439,215	716,566	678,513	678,513
R^2	0.6963	0.8291	0.9336	1.0000	0.8509	0.8401

Notes: We only include properties that are within 100 m of WBMGP border. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

to be a Type I error as the coefficient is small and statistically insignificant when only including properties within 50 m of a WBMGP border.

In Table A5, we study the effects on people of moving out in the next year. We reiterate that the WBMGP did not force people who have become non-employed to move out of public housing. Indeed, we do not find any statistically significant effects of the implementation of the WBMGP on the non-employed rate of individuals moving out.

Finally, we test the impact of the Act on *incumbent individuals*. We investigate this by including individual-by-property-fixed effects as well as municipality-by-year-fixed effects. Table A6 shows the results, where we distinguish between individuals in and outside of public housing. We do not find economically and statistically significant effects of the Act on (i) incumbents' chance of being non-employed, (ii) their income or (iii) their skill level. The effect on the probability to be foreign-born cannot be identified because it does not change over time for a person. The probability of incumbents on being retired or single is, expectedly, also not impacted by the Act. Hence, we can conclude that the WBMGP did not improve outcomes of incumbent people living in targeted areas. We think it makes sense that the programme did not affect incumbents' outcomes because only the composition of neighbours slightly change, which is unlikely to significantly affect incumbents' outcomes. Still, the time-span of our data may be too short to capture long-run effects.

A.4 ADDITIONAL RESULTS FOR THE PRICE EFFECT

Here, we aim to test whether pre-trends and announcement effects are important. In order to do so, we will estimate event studies showing the effects before and after implementation of the WBMGP. If a stigma effect is important, we expect that there is a treatment effect already *1 year before* official designation, as the designated areas are almost always posted before the WBMGP is officially implemented. We take an event-study approach, where we generalize Equation (4):

$$\log p_{jst} = \sum_{\tau=-4}^{3} \beta_\tau w_{st\tau} + \gamma x_{jst} + \lambda_j + \mu_{s\in m,t} + \epsilon_{jst}, \quad \text{if } d_{jst} < \bar{d}, \tag{A.1}$$

so we estimate separate coefficients β_τ for each year to or after treatment, denoted by τ.

We report results in Figure A3. It is shown that 3 years and 2 years before the treatment there is no price effect. However, 1 year before the programme we observe a price drop, albeit imprecise. We think this makes sense as the announcement of the designated areas typically occurred in the year before implementation. After 2 years, the price discount increases to about 7%. In other words, we do not find evidence that the stigma effect dissipates over time.

One may be concerned about the relatively strong price drop from year 1 to 2 years after treatment from about 3% to 7.5%. We then replicate these results but instead slightly increase the threshold distance to 250 m in Figure A4. It is shown that the overall pattern remains similar (although still statistically imprecise). However, the drop in prices between years 1 and 2 after the implementation of the WBMGP appears not robust. More importantly, we do not find any evidence for pre-trends.

To further investigate the longer-run stigma effect, we extend the baseline specifications to include a second-order effects of years after the treatment. We then estimate:

$$\log p_{jst} = \beta_0 w_{st} + \beta_1 w_{st} \times D_{st} + \beta_2 w_{st} \times D_{st}^2 + \gamma x_{jst} + \lambda_j + \mu_{s\in m,t} + \epsilon_{jst}, \quad \text{if } d_{jst} < \bar{d}. \tag{A.2}$$

We report the results in Figure A5. It is shown that the stigma effect seems to increase over time and stabilizes around 10% after 7.5 years. Although the confidence bands prevent us from drawing strong conclusions, we do not find any evidence that the stigma effect is a temporary effect that quickly dissipates over time.

We further estimate city-specific estimates in Table A7. When we concentrate us on the preferred specification in Column (2) in which we only include observations within 100 m of WBMGP borders, we find negative estimates in all cities. They range from essentially 0 to about −13%. Unfortunately, because the number of observations per city is somewhat small, the coefficients are not particularly precisely estimated. Still, the results confirm that stigma effects due to place-based policies are not just a phenomenon that pertains to one or a few locations.

Based on the effects reported in Table A7, we can calculate the total effects per city. We report these results in Table A8. The largest total effects can be found in

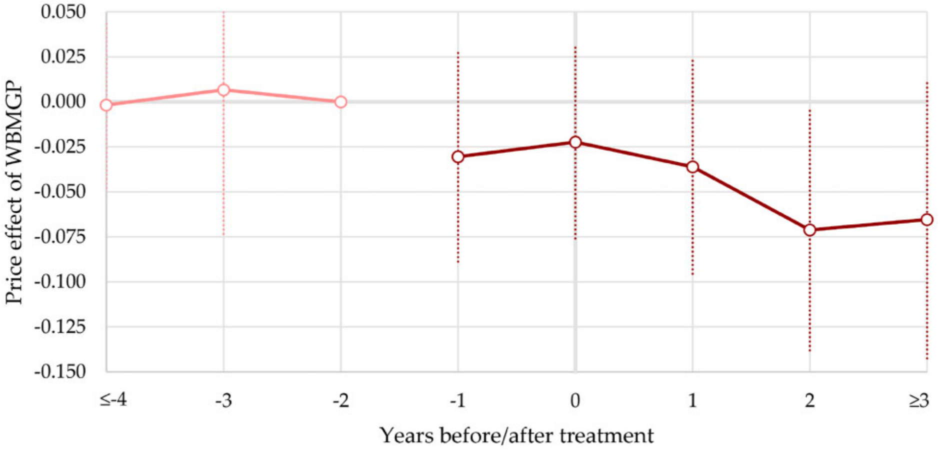

Figure A3. Price effects: event study within 100 m of a WBMGP border

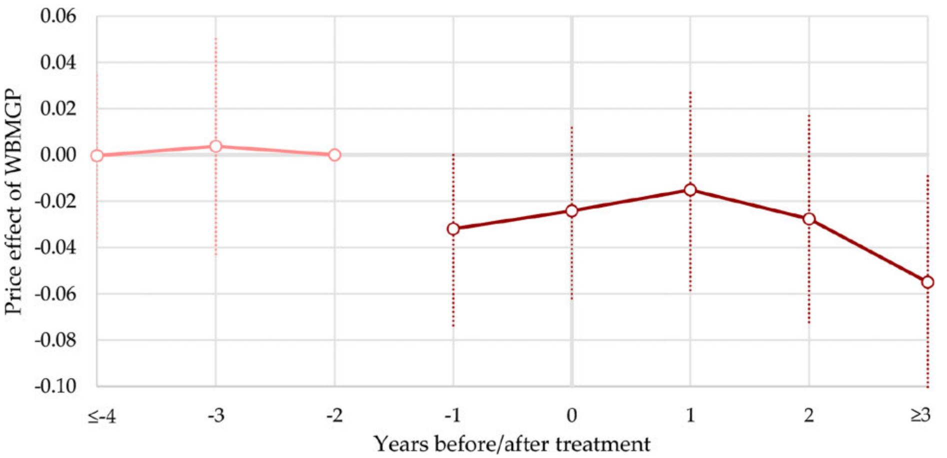

Figure A4. Price effects: event study within 250 m of a WBMGP border

Rotterdam (€5 million a year), which has the largest number of treated unites. Because the stigma effect seems to be more pronounced in Nijmegen, we also find large total effects in Nijmegen (€2.7 million a year). Note, however, that the city-specific effects are not particularly precise and differences between cities are likely much smaller than suggested by this table.

A.5 REPLICATION OF RESULTS FOR THE KW-PROGRAMME

This appendix section focuses on the replication of the effect of the KW-policy on house prices. We aim to measure external effects, by focusing on changes in prices of

Table A7. Price regressions: effects by city (dependent variable: log of house price per m^2)

	<250 m (1)	<100 m (2)	<50 m (3)
WBMGP implemented Capelle aan den IJssel	0.0134	−0.0031	−0.0190
	(0.0134)	(0.0175)	(0.0216)
WBMGP implemented 's-Hertogenbosch	−0.0191	−0.0525	−0.3415***
	(0.0330)	(0.0429)	(0.0727)
WBMGP implemented Nijmegen	−0.1038***	−0.1420***	−0.2129***
	(0.0243)	(0.0468)	(0.0597)
WBMGP implemented Rotterdam	−0.0177	−0.0407	−0.0365
	(0.0197)	(0.0391)	(0.0515)
WBMGP implemented Schiedam	−0.0397	−0.0457	−0.0166
	(0.0404)	(0.0451)	(0.0463)
WBMGP implemented Vlaardingen	−0.0197	−0.0004	0.0259
	(0.0255)	(0.0373)	(0.0622)
WBMGP implemented Zaanstad	0.0010	−0.0184	−0.0113
	(0.0205)	(0.0337)	(0.0604)
Property characteristics	✓	✓	✓
Property-fixed effects	✓	✓	✓
Municipality×year-fixed effects	✓	✓	✓
Number of observations	11,766	4,665	2,385
R^2	0.9199	0.9240	0.9320

Notes: We exclude Tilburg, which has only 164 treated units, from the analysis. Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

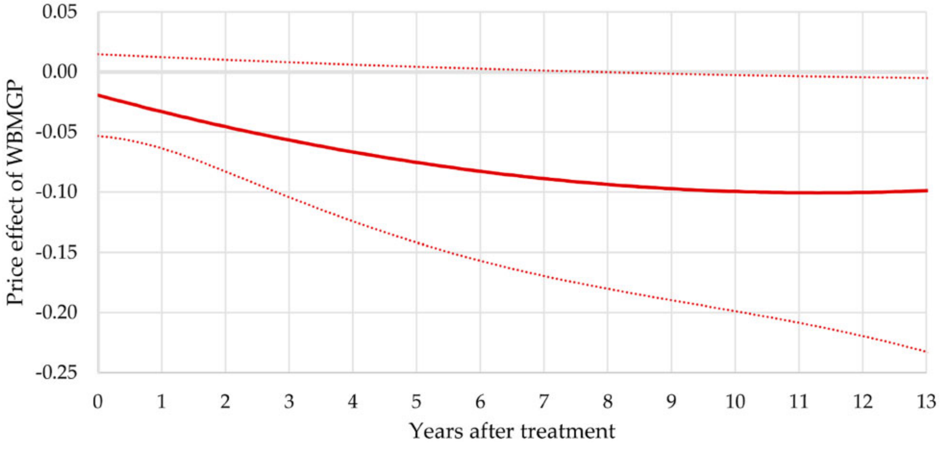

Figure A5. Price effects: long-term

Table A8. Overall house price effects of the policy, by city

	Treated housing units	Annual effect per property (in €)				Total annual effects (in €)			
		Average effect	Owner occupied	Private Private rental	Public housing	Total effect	Owner occupied	Private rental	Public housing
Capelle aan den Ijssel	4,331	−16	−24	−22	−14	−70,920	−20,672	−7,833	−42,415
		(124)	(181)	(164)	(104)	(538,877)	(157,073)	(59,521)	(322,283)
’s-Hertogenbosch	4,629	−362	−449	−393	−307	−1,676,526	−759,610	−70,675	−846,241
		(351)	(435)	(380)	(297)	(1,622,873)	(735,301)	(68,413)	(819,159)
Nijmegen	3,424	−780***	−924***	−803***	−687***	−2,669,522***	−1,070,690***	−292,721***	−1,306,111***
		(203)	(240)	(209)	(179)	(694,415)	(278,515)	(76,145)	(339,755)
Rotterdam	32,237	−164	−201	−143	−157	−5,299,837	−1,649,516	−1,346,557	−2,303,765
		(139)	(170)	(121)	(133)	(4,486,846)	(1,396,481)	(1,139,996)	(1,950,368)
Schiedam	2,927	−176	−236	−176	−168	−516,113	−75,265	−54,321	−386,528
		(194)	(260)	(193)	(185)	(566,810)	(82,658)	(59,657)	(424,495)
Vlaardingen	4,825	−2	−3	−2	−2	−10,473	−5,275	−0,912	−4,286
		(191)	(247)	(162)	(153)	(920,258)	(463,509)	(80,169)	(376,579)
Zaanstad	5,632	−107	−133	−113	−91	−603,142	−234,475	−30,293	−338,374
		(235)	(292)	(249)	(200)	(1,321,884)	(513,890)	(66,392)	(741,601)

Notes: We exclude Tilburg, which has only 164 treated units. We assume a discount rate of 3.5% and calculate 2020 housing values using assessed housing prices and the consumer price index. Bootstrapped standard errors (250 replications) are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

owner-occupied housing units that were not improved by the programme. About €1 billion was spent by public housing associations and the national governments in 83 deprived neighbourhoods. About 90% of the money was dedicated to improving the quality of public housing. The remainder was spent on green spaces and social empowerment programmes (Wittebrood and Permentier, 2011).

The main issue with identifying a causal price effect is that KW neighbourhoods were not randomly chosen. By contrast, deprivation scores calculated in 2007 based on the quality of the housing stock, perceived crime levels and moving behaviour, among others, were used to select 83 neighbourhoods.

The deprivation scores range from –6.6 to 12.98. In principle, only neighbourhoods with a score exceeding 7.3 were targeted. However, there are 14 non-complying neighbourhoods that had too low scores but were selected or had sufficiently high scores but did not receive treatment in the end. We therefore employ a fuzzy regression-discontinuity (FRD) design, for which it is necessary to observe a substantial jump in the probability to be treated. Indeed, as in Koster and Van Ommeren (2019), we observe a >90% increase in the probability to become treated when the deprivation score exceeds a certain threshold. Moreover, in the paper, it is shown that there is no bunching at the threshold confirming that deprivation scores could not be influenced by municipalities.

Using data from all of the Netherlands, we then estimate the following equation:

$$\log p_{jst} = \beta k_{st} + \gamma x_{jst} + \delta_{1t} z_s + \delta_{2t} z_s^2 + \delta_{3t} z_s^3 + \lambda_j + \mu_t + \epsilon_{jst}, \quad \text{if } |z_s - \bar{z}| < h, \quad (A.3)$$

where k_{st} is the treatment variable that equals one when a street s in a neighbourhood receives treatment and z_s is the (time-invariant) deprivation scores. The RDD implies that we only include neighbourhoods with deprivation scores that are sufficiently close, within h, of the cut-off $\bar{z}$. We also control for year-specific non-linear trends of the deprivation score z_s. Furthermore, because we have non-complying neighbourhoods, we instrument k_{st} with a dummy that equals one when the neighbourhood is above the cut-off value of the deprivation score after the programme was launched.

To avoid the issue of spatial effects that spill over across the borders of treated areas, we exclude observations within 2.5 km of targeted neighbourhoods (as in Koster and Van Ommeren, 2019). We report results in Table A9.

In Column (1), we estimate standard differences-in-differences specifications by including all observations and compare price changes between targeted and non-targeted neighbourhoods. The coefficient seems to suggest a large positive effect: the KW-policy is associated with a price increase of $(\exp(0.0753) - 1) \cdot 100\% = 7.8\%$. However, this may be an overestimate if price changes are particularly occurring in treated neighbourhoods, for example, because gentrification particularly occurs in these neighbourhoods.

Table A9. Replication of KW effects

Dependent variable:	Log of house price per m2			
	All Obs. (1)	Bandwidth = 2 (2)	Bandwidth = 1.5 (3)	Year ≤ 2014 (4)
KW implemented	0.0753***	**0.0690*****	**0.0317***	**0.0456*****
	(0.0048)	(**0.0174**)	(**0.0182**)	(**0.0158**)
Property characteristics	✓	✓	✓	✓
Property-fixed effects	✓	✓	✓	✓
Year-fixed effects	✓	✓	✓	✓
Deprivation score × year trends	✓	✓	✓	✓
Number of observations	954,755	53,610	36,113	20,447
Bandwidth	∞	2	1.5	1.5
R^2	0.9365			
Kleibergen–Paap F-statistic		371.2	386.3	388.9

Notes: Bold indicates instrumented. The instrument is a dummy whether the z-score is above 7.3 after March 2007. We only include properties that are outside WBMGP areas and further than 2.5 km from a KW border or inside KW areas. Property characteristics include the log of property size, the number of rooms, the number of insulation layers, the number of floors, number of kitchens, number of bathrooms and dummies indicating whether the property has a private parking space, a garage, a garden, whether it is well maintained, has a central heating, has a roof terrace, has a balcony, has internal office space, has a dormer window and is in a listed building. Standard errors are clustered at the street level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

We therefore employ the fuzzy RDD by controlling for linear trends of deprivation scores in each year and limit the number of observations to only include neighbourhoods that are within two points of the threshold (i.e., $h = 2$). This reduces the number of observations by almost 95%. The effect is somewhat lower, but still positive and highly statistically significant: the coefficient implies that prices have increased by 7.1%. When we further reduce the bandwidth in Column (3) to 1.5, we find an effect of 3.2%, which is almost identical to the preferred estimate in Koster and Van Ommeren (2019). In the final column (4), we limit the number observations to 2014, to be as close as possible to Koster and Van Ommeren (2019), who had data until 2014. We find a somewhat higher but more precise estimate, despite the reduction in the number of observations, suggesting that the effects are less heterogeneous in the first few years of the programme (the programme lasted until 2012).

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