DISCUSSION PAPER SERIES

DP17132

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INTERNATIONAL TRADE AND REGIONAL ECONOMICS

PUBLIC ECONOMICS

ISSN 0265-8003

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Discussion Paper DP17132
Published 24 March 2022
Submitted 14 January 2022

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www.cepr.org

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Abstract

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JEL Classification: H23, H31, J60, R30, R38

Keywords: Place-Based Policies, redlining, stigma effects, amenities, House Prices

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Neighbourhood stigma and place-based policies*

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January 13, 2022

Abstract — 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 100m 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.

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*We thank the editor, Isabella Mejean, and two anonymous referees for very useful comments. Stuart Rosenthal is also thanked for useful insights in the early stages of this project.

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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 (Santiago et al. 2001, De Souza Briggs 1999, Lee et al. 1999, Rossi-Hansberg et al. 2010, Ahlfeldt et al. 2017, Koster & 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. 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 typically also induce a *stigma* effect, *i.e.* 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, friends) may indeed not be indifferent about the reputation of the neighbourhood (Tootell 1996, Zenou & 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

[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, for example, Wallace 2001, Koster & Van Ommeren 2019, González-Pampillón et al. 2020, for England, the Netherlands and Barcelona respectively).

[2] For a theoretical contribution on redlining in the labour market, see Zenou & Boccard (2000). They focus on the racial composition of neighbourhoods. The empirical evidence of redlining by employers and mortgage providers, mainly focusing on the U.S., 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).

amenity and reputation levels, nor are fully rational (Genesove & Mayer 2001, Piazzesi & Schneider 2009, Han & Strange 2016, Guren 2018). Consequently, the announcement may lead to a stigma; that is, residents consider the new piece of information as inducing a negative reputation. Hence, the presence of stigma effects may lead to a downward 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 identify these stigma effects 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*) 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 8 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). In the U.S., 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 & Dean 2003, Arthurson 2013).

The Act was first implemented in Rotterdam in 2006, which is the 2nd 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, *i.e. 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 hypothesising 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 small street segments explain most of the spatial variation in crime (Weisburd & Amram 2014, Weisburd 2015, Steenbeek & Weisburd 2016, Schnell et al. 2017). Hence, reputation is plausibly street 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 & Ejiogu 2021) or 'territorial' stigmatisation (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 emphasise 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 are essentially streets with a distinct name and are therefore well defined, which is in contrast to large neighbourhoods with fuzzy boundaries.

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 100m) to borders of designated neighbourhoods. We find that the announcement of the Act leads to price *decreases* of about 3-5%.[3] 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 or street.

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

[3] As neighbourhood stigma may also vary *continuously* over space, this implies that the estimated effect is an underestimate of the stigma effect given non-restrictive assumptions discussed later on.

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 tends 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 streets 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 another 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 & D'Haultfœuille 2020, Callaway & 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*-programme (also evaluated in Koster & Van Ommeren 2019), as well as the *Nationaal Programma Rotterdam Zuid*. The former focused on improvements in public housing in 83 neighbourhoods, while the latter took place only in a few neighbourhoods in Rotterdam while aiming 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 threefold. First, to the best of our knowledge, we are the first to provide evidence of sizeable neighbourhood stigma effects in the housing market due to the announcement of place-based policies. We emphasise 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. 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, *i.e.* 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 & Kolko 2010, Busso et al. 2013, Kline & Moretti 2013, Mayer et al. 2017, Givord et al. 2018, Charnoz 2018); for overviews, we refer to Neumark & Simpson (2015) and Von Ehrlich & 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 subsidised housing have led to higher house prices (Santiago et al. 2001, Schwartz et al. 2006, Baum-Snow & Marion 2009, Rossi-Hansberg et al. 2010, Ellen et al. 2016, Koster & 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 subsidise 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 & McQuade (2019) find that the construction of subsidised 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 & Owens 2011, Dillman et al. 2017, Diamond & 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 discuss the data and context of the place-based programme. Section 3 outlines the and methodology used in this study. Section 4 highlights our key regression results, including back-of-the-envelop welfare calculations. Section 5 considers stigma effects in other place-based programmes, while Section 6 concludes.

2 Data and context

2.1 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.[4]

The Netherlands has the highest share of rental public housing sector 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 neighbourhoods where the Act was implemented, public housing is even more common with a share of about 52%.

[4] In the current paper, we will frequently use the terms 'neighbourhoods' and 'streets' interchangeably, because in many cases a street is affected by the Act, but sometimes a whole neighbourhood is affected.

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 thousand per year) (see, for example, Van Ommeren & Van der Vlist 2016).[5] 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 six 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).[6] Later, the law was extended so that local governments could also refuse persons with a criminal record.[7] 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 basis of employment status and residential duration (Ouwehand & 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.

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'.[8] An important prerequisite for a legitimate

[5]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.

[6]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.

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

[8]Until 2018 the WBMGP was implemented in many neighbourhoods of Rotterdam. In 2018, Rotterdam changed their policy and since then refuses people with a criminal record in 98 designated streets, while dropping the

implementation of the law is a lengthy discussion on why those neighbourhoods should be targeted. Not all neighbourhoods that were shortlisted have been targeted. In Figure 1a we show the targeted neighbourhoods in Rotterdam, but also the neighbourhoods that were shortlisted, but not targeted.[9]

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 neighbourhoods, after other interventions have failed. Importantly, the assignment of neighbourhoods and streets has been extensively discussed in the (local) press. We list here just a small selection of press articles: ANP (2006), Brink (2016), Van der Velden (2016), Damen & Pan (2017), Eikenaar (2017), Oosterom (2019) and Don (2020). In other words, it is reasonable to believe that most people are aware which neighbourhoods and/or streets have been assigned.

2.2 Data

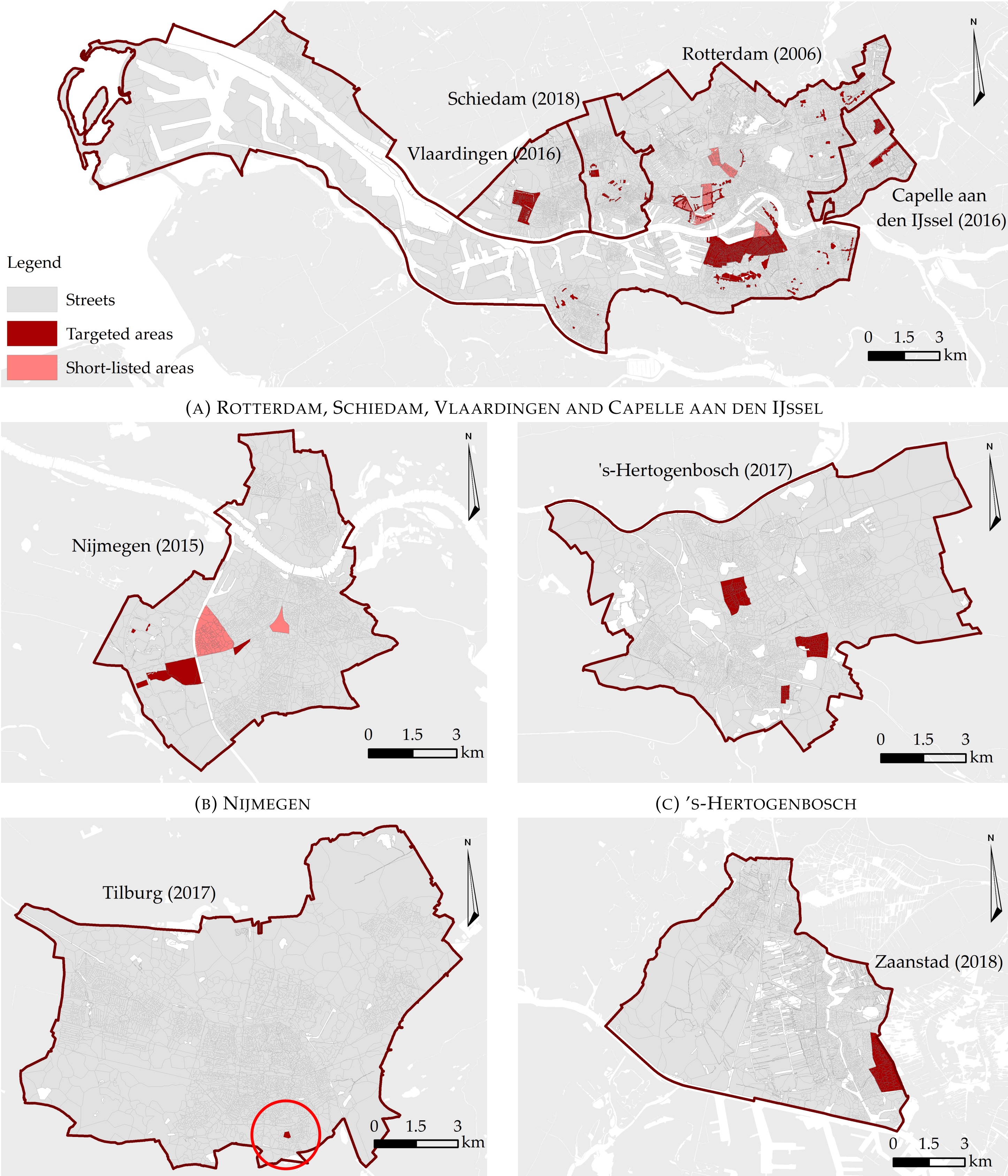
Our analysis is based on several datasets for our study period of 2000 until 2019. We focus on the above-mentioned 8 cities where the WBMGP is implemented.

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 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 2003-2010 and `Inhatab` from 2011-2019. These data are based on the tax register, which provides information on taxable income, tax

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.

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



(A) Rotterdam, Schiedam, Vlaardingen and Capelle aan den IJssel

(B) Nijmegen

(C) ’s-Hertogenbosch

(D) Tilburg

(E) Zaanstad

Figure 1 – Targeted neighbourhoods

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.[10] 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.[11]

The treatment unit are streets or, sometimes, 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.[12] 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 use housing transactions data for the period between 2000 and 2019 from the Dutch Association of Real Estate Agents, 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.[13] Our full sample contains 231,277 transactions. In our main analysis, we focus on repeated sales, so properties that are sold at least twice, which cover more than half of the number of transactions of the full sample.

[10]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.

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

[12]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.

[13]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 25m^2 or larger than 750m^2.

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

Note: 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.

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, *i.e.* 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 one 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 neighbourhoods (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 about €$1,955$ per m^2.

House prices have strongly increased during the study period. We show this in Figure 2,

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.

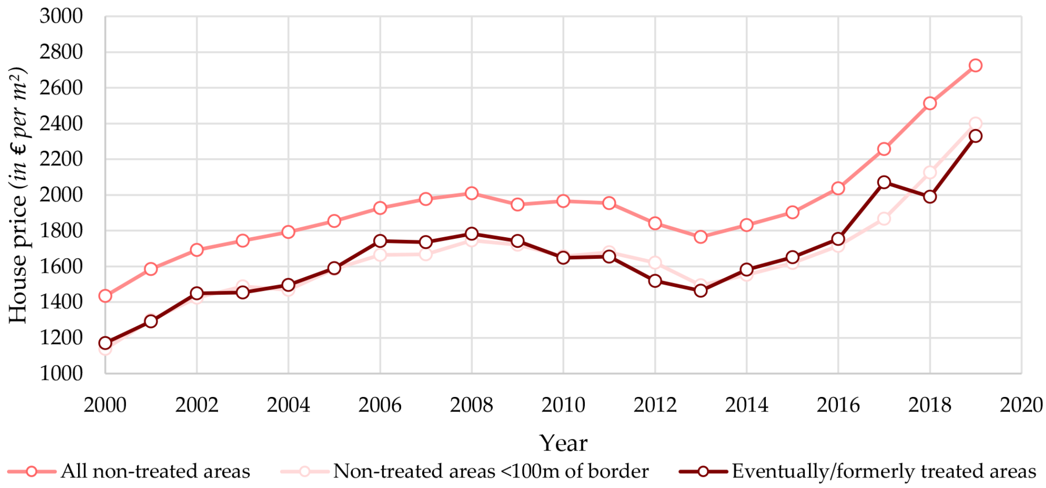


FIGURE 2 – PRICE TRENDS OF NON-TREATED OBSERVATIONS

where we focus on *non-treated* transactions in either *(i)* all non-treated neighbourhoods, *(ii)* non-treated properties within 100m of a border of a WBMGP neighbourhood, and *(iii)* WBMGP neighbourhoods that are to-be-treated or for which treatment has been retracted. We observe that prices in and close to neighbourhoods that are eventually treated have lower prices, which is in line with the notion that the policy targeted deprived neighbourhoods. However, we find that price trends are very similar across to-be-treated and neighbourhoods close to WBMGP neighbourhoods.

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 neighbourhood, *i.e.* whether redlining effects exist. We capture neighbourhood composition by employment status, income and several neighbourhood demographic variables.

We measure neighbourhood composition at the individual level, which avoids 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 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 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:

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

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 that a property is in a street in which the WBMGP is implemented.[14] 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, ϵ_{jst} refers to a random error.[15]

[14]For some neighbourhoods, 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.

[15]Alternatively, one may estimate the lagged specification: $y_{ijst} = \sum_{L=1}^{M} \beta_L w_{st-L} + \gamma x_{jst} + \lambda_j + \mu_{s \in m,t} + \epsilon_{ijst}$, where w_{st} is an indicator that is 1 if a property is in a street in which the WBMGP is implemented, L denotes the number of years since the first year of treatment and M denotes the overall duration of treatment. The main disadvantage of this specification is that we have an imbalanced panel of neighbourhoods so for larger values of L it appears that β_L is only identified for specific neighbourhoods. To solve this issue, one may assume that β_L does not vary over time, hence $\beta_L = \beta, \forall L$. Given this restriction, one can derive (1) where $D_{st} = \sum_{L=1}^{M} w_{st-L}$. We also have estimated non-employment models at the household level, where non-employment is a dummy indicator which is

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 $\bar{d}$ of a border of a designated WBMGP-street or neighbourhood, in most specifications chosen to be 100m, 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 neighbourhoods. Because the treatment is very local – *i.e.* at the street level – we cluster standard errors at this level.

In spatial discontinuity designs, spatial spillover effects are potentially important. In the current context, this means that rejected households, *i.e.* 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.

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 place-based policies studies, captures, for example, better social networks leading to a lower unemployment rate (Bayer et al. 2008). Although we expect the direct effect to be small, and while we are mainly interested in the sorting effect, to separate the direct effect from the sorting effect, we will also estimate regressions where we

one if *none* of the members of the household are employed. These results are almost identical, and can be received upon request.

include individual fixed effects that control for sorting effects (see Combes et al. 2008):

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

where $\kappa_i j$ captures individual-by-property fixed effects.

Finally, one may wish to apply an 'event-study' methodology, which allows ones 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:

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

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 stigma effects

One of our aims is to measure the causal effect of the implementation of the WBMGP on house prices to identify neighbourhood stigma. In contrast to changes in demographic composition, one expects that the effect of the policy on prices is immediate (after implementation).[16] 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:

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

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,

[16] 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.

β captures the discrete stigma effect, which is an underestimate of the overall stigma effect. This is given the plausible assumption that stigma varies monotonically over space within the vicinity of the border (note that vicinity may exceed $\bar{d}$). For example, in the context of London, this assumption implies that because the neighbourhood of Acton had a worse reputation than Chiswick, the difference in reputation for locations located at different sites of the border becomes larger if we focus on locations further away.

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 $\bar{d}$. 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 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.

One may further argue that the price effect due to treatment of w_{st} estimated in (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:

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

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

In the recent literature on difference-in-difference designs it is well 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 street or years (De Chaisemartin & D'Haultfœuille 2018, 2020, Borusyak et al. 2021, Callaway & 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 & 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 & D'Haultfœuille (2020) and Callaway & Sant'Anna (2021) have proposed alternative estimators for panel data that are balanced. Moreover, the latter paper assumes the irreversibility of treatment. Because our data of properties are unbalanced and some streets may become untreated later, we cannot apply those estimators. Still, we are able to overcome the issue of negative weights by only exploiting the identifying variation between treated properties and nearby never-treated properties.[17] We do so by including nearest treatment group-by-year fixed effects. Hence, we estimate:

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

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

[17]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.

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	Yes	Yes	Yes	Yes	Yes	Yes
Municipality×year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
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	Yes	Yes	Yes	Yes	Yes	Yes
Municipality×year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
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 100m of a WBMGP border. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.

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 100m of borders of WBMGP areas, but we reduce the maximum distance to 50m 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 50m. 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, *i.e.* 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 50m.

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 three 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 increases to a reduction on non-employment of 2.5 percentage points.

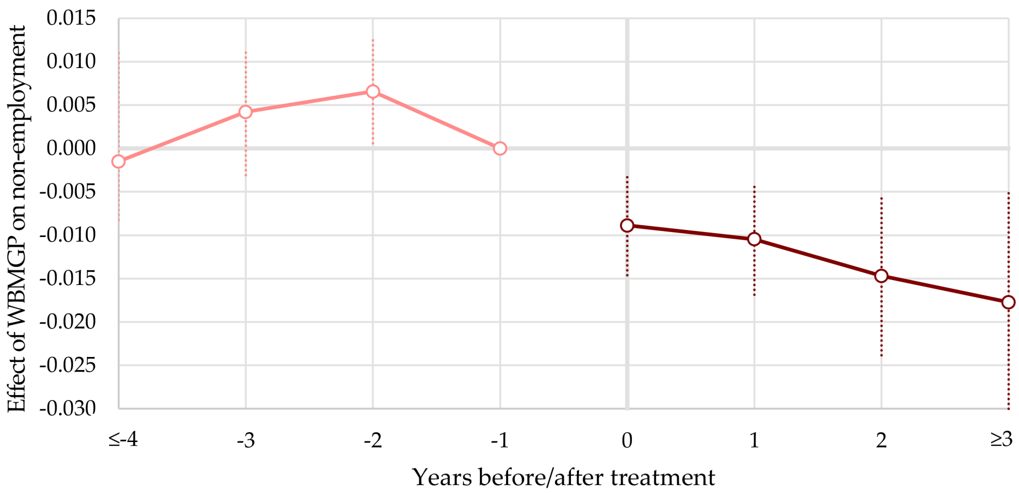


FIGURE 3 – EFFECTS ON THE SHARE OF NON-EMPLOYED: EVENT STUDY

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 neighbourhoods 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 neighbourhoods, nor did it improve outcomes of incumbent individ-

TABLE 4 – BASELINE PRICE REGRESSIONS
(Dependent variable: log of house price per m^2)

	Baseline	*+ Property f.e.*	*<250m*	*<100m*	*<50m*
	(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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

uals.

4.2 *House price effects: baseline results*

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

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 250m of a WBMGP border, leading to a similar estimate.

Our preferred estimate is reported in column (4), where we only keep observations within 100m 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

TABLE 5 – HOUSE PRICE REGRESSIONS: BEFORE AND AFTER

(Dependent variable: log of house price per m^2)

	Before implementation			*After implementation*		
	<250m	*<100m*	*<50m*	*<250m*	*<100m*	*<50m*
	(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, detached), and construction year decades. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

space. To support our results, we just keep transactions within 50m of a WBMGP border in column (5). The coefficient now becomes even stronger, but less precise.

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 neighbourhoods 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, while controlling for a host of property characteristics and municipality-by-year fixed effects, see Table 5. When keeping transactions within 250m 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 250m. This effect is considerably smaller and in line with earlier estimates if we narrow the sample to just 100m or 50m 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 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 & Sirmans 1993, Ihlanfeldt & 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 neighbourhoods.

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 pre-trends. Indeed, one year before the actual treatment prices are about 2.5% lower, in line with the results reported in column (5) in Table 5. After two 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 Induced changes in neighbourhood composition

First, we 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

TABLE 6 – PRICE REGRESSIONS, CONTROLLING FOR NEIGHBOURHOOD COMPOSITION

(Dependent variable: log of house price per m^2)

	Replication			Controlling for neighbourhood composition		
	<250m	*<100m*	*<50m*	*<250m*	*<100m*	*<50m*
	(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, detached), and construction year decades. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

characteristics in the street. Table 6 reports the results.

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 further 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.

TABLE 7 – HOUSE PRICE REGRESSIONS: NEAREST TREATMENT GROUP-BY-YEAR FIXED EFFECTS
(Dependent variable: log of house price per m^2)

	Nearest treatment group×year fixed effect			*+ Irreversibility of treatment*		
	<250m	*<100m*	*<50m*	*<250m*	*<100m*	*<50m*
	(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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4.3.3 Robustness checks

In Table 7 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 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 column (1) and (3) are virtually the same as compared to the baseline results reported in Table 4. The coefficient in column (2) is somewhat lower, although the estimate is not statistically significantly lower. In column (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.[18]

Table 8 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 *Nationaal Programma Rotterdam Zuid* and whether a

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

TABLE 8 – HOUSE PRICE REGRESSIONS: ROBUSTNESS
(Dependent variable: log of house price per m^2)

	+ Neighborhood ×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 100m 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

neighbourhood is part of the *Krachtwijken* 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 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 8 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.[19] 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 1a), column (6) shows that our results are robust if we exclude those neighbourhoods.

In Table 9 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 docu-

[19] 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 9 – HOUSE PRICE REGRESSIONS: IDENTIFICATION REVISITED AND PLACEBO
(Dependent variable: log of house price per m^2)

	Runner-up neighbourhoods	Neighbourhood rank	Time variation only	Placebo treatment		
				<250m	<100m	<50m
	(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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

ments 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.

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 1a). The coefficient once more confirms the negative effect we found earlier, even with smaller standard errors.

In the last three columns of Table 9 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 250m of a placebo neighbourhood in column (4) of Table 9. However, this effect goes away if we reduce the threshold distance to the nearest placebo border in columns (5) and (6).

All in all, these results confirm the negative price effects we find in Table 4 and reinforce the conclusion that the reductions in 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 analysis is a 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, *i.e.* 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 which 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

TABLE 10 – 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***	-252***	-162***	-181***
		(66)	(85)	(54)	(61)
		Total annual effects (in €)			
	Treated units	*Total effect*	*Owner-occupied*	*Private rental*	*Public housing*
Total effect	58,169	-11,409,232***	-3,943,288***	-2,005,429***	-5,460,515***
		(3,839,895)	(1,327,154)	(674,948)	(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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

brand clothes and other consumer goods (with a preference for expensive brands). Furthermore, human beings like to portrait themselves as successful.[20]

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 10 we provide a back-of-the-envelop 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 annualised 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.[21] On the other hand, recall we only capture the

[20] This is also recognised 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 & Boccard 2000, Besbris et al. 2015, Carlsson et al. 2018).

[21] 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 leads to wasteful spending in a consumption rat race (Frank

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 *Nationaal Programma Rotterdam Zuid* as well as the *Krachtwijken* programme.

5.1 *Nationaal Programma Rotterdam Zuid*

The *Nationaal Programma Rotterdam Zuid* (henceforth: 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 indicate eleven 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 compare price changes very close to borders of treated neighbourhoods. Column (1) in Table 11 shows that within 250m, there is a negative effect although it is statistically insignificant. However, when we focus on areas within 100m of an NPRZ border, the coefficient becomes statistically significant. Hence, the negative effect in NPRZ neighbourhoods again indicates 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.

1985). This begs the question whether a negative reputation as identified in our paper creates a positive positional externality, which would imply that households who do not live in stigmatised neighbourhoods derive utility from that other households are stigmatised. We believe that there is no evidence that human beings feel in this way, *i.e.* 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.

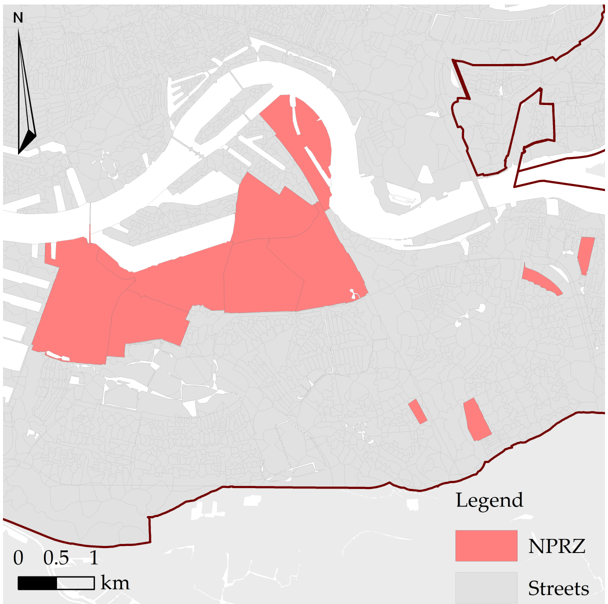


FIGURE 4 – NPRZ PROGRAMME IN ROTTERDAM

TABLE 11 – HOUSE PRICE REGRESSIONS: OTHER PROGRAMMES
(Dependent variable: log of house price per m^2)

	NPRZ program		*KW program*			
			All neighborhoods		*Rank* $\leq$*20*	
	<250m	*<100m*	*<250m*	*<100m*	*<250m*	*<100m*
	(1)	(2)	(3)	(4)	(5)	(6)
NPRZ implemented	-0.0205	-0.0584**				
	(0.0286)	(0.0249)				
KW implemented			0.0143**	0.0246***	0.0167	0.0475
			(0.0057)	(0.0089)	(0.0143)	(0.0311)
KW ranking announced			-0.0177***	-0.0082	-0.0323**	-0.0534**
			(0.0060)	(0.0095)	(0.0149)	(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, detached), and construction year decades. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5.2 *Krachtwijken programme*

An alternative programme was the *Krachtwijken*, henceforth 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 there 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.

In Koster & Van Ommeren (2019), this programme is studied in detail using a fuzzy regression-discontinuity design (RDD), 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 8 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 & 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 11 we include properties within 250m of borders of KW neighbourhoods. We then find a small positive effect of the KW programme of 1.4%. This estimate makes sense, as this estimate 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 100m of KW neighbourhood borders, this announcement effect turns statistically insignificant (whereas the effect of the KW implementation is slightly larger).

One may argue that any stigma effects likely applied to the most deprived KW neighbourhoods, as, out of 40 neighbourhoods, these were frequently discussed in the press. Hence, in columns (5) and (6) of Table 11 we only include observations in the 20 most deprived neighbourhoods according to its 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 100m in column (6).

Hence, in the KW programme, 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 evidence of a sizeable negative price effect in the housing market incurred by place-based policies that publicly announce which neighbourhoods are deprived, and therefore appear to induce a stigma effect. 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%.

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 100m around those areas. It is easily observed that 100m 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 100m of WBMGP borders in- and outside WBMGP areas. We find slightly higher 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.

FIGURE A1 – ROTTERDAM, SAMPLE MAP

TABLE A1 – DESCRIPTIVE STATISTICS OF INDIVIDUAL CHARACTERISTICS, <100M

	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

Note: 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.

FIGURE A2 – EFFECTS ON THE SHARE OF NON-EMPLOYED: EXTENDED EVENT STUDY

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 that the effect 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 50m of a WBMGP border. It is shown that the effect on non-employment becomes even slightly stronger. We observe a reduction in non-employment of 0.45 percentage points for each year of

TABLE A2 – REDLINING REGRESSIONS, WITHIN 50M 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 50m of a WBMGP border. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.

While we found a small positive effect on income within 100m, this effect ceases to be statistically significant when restricting the distance to just 50m 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 neighbourhoods. 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 neighbourhoods.

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

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 100m of WBMGP border. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 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 to be a Type I error as

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 100m of WBMGP border. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

the coefficient is small and statistically insignificant when only including properties within 50m of a WBMGP border.

In Table A5 we study the effects on people of moving out in the next year. We re-emphasise 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 are, expectedly, also not impacted by the Act. Hence, we can conclude that the WBMGP did not improve outcomes of incumbent people living in targeted neighbourhoods. We think it makes sense that the programme did not affect incumbents' outcomes because only the composition of neighbours

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 100m of WBMGP border. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 100m of WBMGP border. Standard errors are clustered at the street level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.

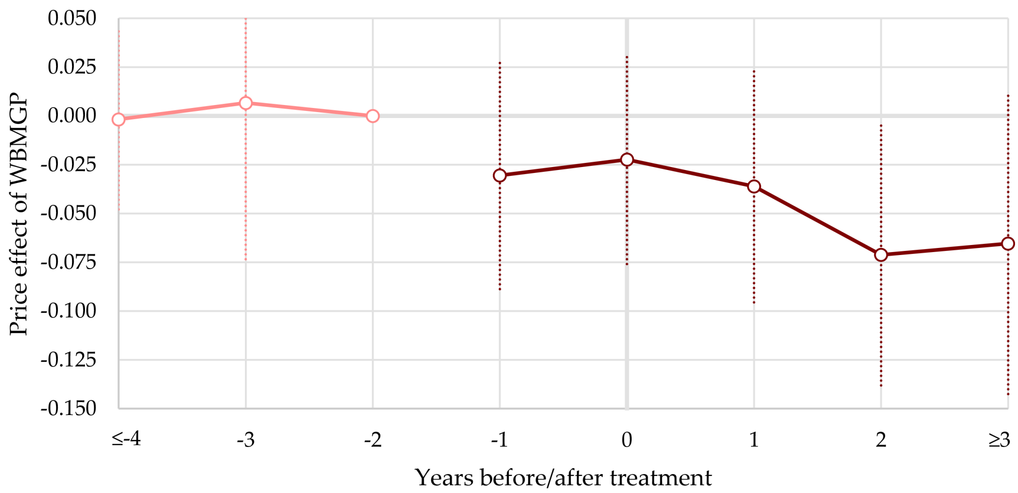

FIGURE A3 – PRICE EFFECTS: EVENT STUDY WITHIN 100M OF A WBMGP BORDER

A.4 *Additional results for the stigma 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 *one 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 generalise equation (4):

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

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, one 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 two 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 250m in Figure A4. It is shown that the overall pattern remains similar

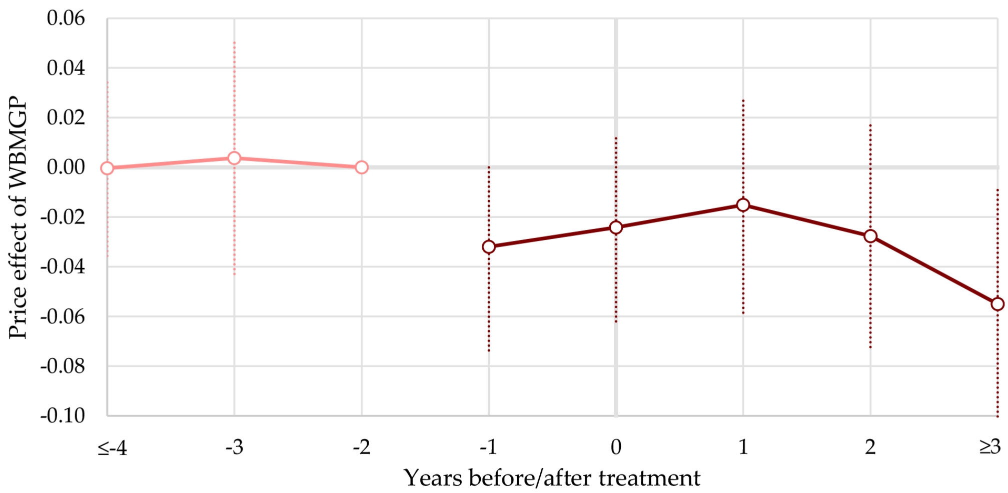

FIGURE A4 – PRICE EFFECTS: EVENT STUDY WITHIN 250M OF A WBMGP BORDER

(although still statistically imprecise). However, the drop in prices between year 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:

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

We report the results in Figure A5. It is shown that the stigma effect seems to increase over time and stabilises 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 100m 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 A7 we can calculate the total effects per city. We report these

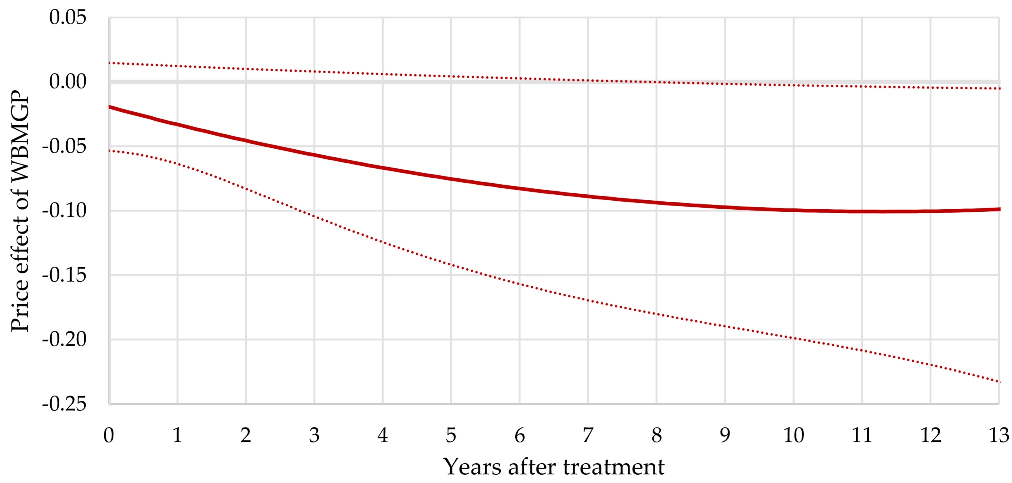

FIGURE A5 – PRICE EFFECTS: LONG-TERM

TABLE A7 – PRICE REGRESSIONS: EFFECTS BY CITY
(Dependent variable: log of house price per m^2)

	<250m	*<100m*	*<50m*
	(1)	(2)	(3)
WBMGP implemented	0.0134	-0.0031	-0.0190
Capelle aan den IJssel	(0.0134)	(0.0175)	(0.0216)
WBMGP implemented	-0.0191	-0.0525	-0.3415***
's-Hertogenbosch	(0.0330)	(0.0429)	(0.0727)
WBMGP implemented	-0.1038***	-0.1420***	-0.2129***
Nijmegen	(0.0243)	(0.0468)	(0.0597)
WBMGP implemented	-0.0177	-0.0407	-0.0365
Rotterdam	(0.0197)	(0.0391)	(0.0515)
WBMGP implemented	-0.0397	-0.0457	-0.0166
Schiedam	(0.0404)	(0.0451)	(0.0463)
WBMGP implemented	-0.0197	-0.0004	0.0259
Vlaardingen	(0.0255)	(0.0373)	(0.0622)
WBMGP implemented	0.0010	-0.0184	-0.0113
Zaanstad	(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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

results in Table A8. The largest total effects can be found in 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).

TABLE A8 – OVERALL HOUSE PRICE EFFECTS OF THE POLICY, BY CITY

		Annual effect per property (in €)				*Total annual effects (in €)*			
	Treated housing units	*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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 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 programs (Wittebrood & 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 design (FRD), for which it is necessary to observe a substantial jump in the probability to be treated. Indeed, as in Koster & Van Ommeren (2019), we observe a more than 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:

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

where k_{st} is the treatment variable that equals one when a neighbourhood s receives treatment, and z_s is the (time-invariant) deprivation scores. The regression-discontinuity design 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 2.5km of targeted neighbourhoods (as in Koster & Van Ommeren 2019). We report results in Table A9.

In column (1) we estimate a 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 $\exp(0.0753) - 1 = 7.8\%$. However, this may be an overestimate if price changes are particularly occurring in treated neighbourhoods, for example because gentrification particularly occurs in these neighbourhoods.

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 2 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 to 1.5 in

TABLE A9 – REPLICATION OF KW EFFECTS
(Dependent variable: log of house price per m^2)

	All obs	*Bandwidth* $= 2$	*Bandwidth* $= 1.5$	*Year* ≤ 2014
	(1)	(2)	(3)	(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	No	✓	✓	✓
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 2.5km 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

column (3) we find an effect of 3.2%, which is almost identical to the preferred estimate in Koster & Van Ommeren (2019). In the final column (4), we limit the number observations to 2014, to be as close as possible to Koster & Van Ommeren (2019), who had data until 2014. We find a somewhat higher but more precise estimate, despite the reduction in number of observations, suggesting that the effects are less heterogeneous in the first few years of the programme (the programme lasted until 2012).