A different place to different people

Conditional neighbourhood effects on residents' socio-economic status

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SOCIO-ECONOMIC CONSEQUENCES OF FORCED RELOCATION

Abstract Policymakers have actively pursued urban renewal and dispersal programmes in order to deconcentrate poverty in urban neighbourhoods. Relocation strategies may encourage employment opportunities for relocated residents if resourceful contacts and job information become more easily available after the move. This study provides an innovative evaluation of the early impacts of involuntary relocation programmes in The Netherlands on earnings and employment rates of forced relocatees. It establishes a quasi-experimental design by employing unique longitudinal individual-level population registry data from Statistics Netherlands: forced relocatees are tracked and matched to a control group consisting of similar residents that were not forced to move. A difference-in-difference design shows that forced relocatees are living in less deprived neighbourhoods after the move. However, the upgrade in housing does not in turn lead to more socio-economic opportunities: on average no improvement in economic prospects (earnings and employment) was found over time for the forced relocatees. These findings do not only challenge the neighbourhood effects literature, but also question the justification of the widespread area-based urban renewal policies.

INTRODUCTION

In several European and North American countries, dispersal programmes and urban renewal strategies are carried out to break the downward spiral of accumulating disadvantage in urban neighbourhoods. The rationale behind these policies is to counteract negative neighbourhood effects; the notion that residing in concentrations of disadvantage has a detrimental impact on an individual's economic self-sufficiency over and above the effect of their individual characteristics. Residents in a more upscale neighbourhood are assumed to be

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exposed to a more resourceful, work-oriented climate, enhancing its residents' economic well-being (de Souza Briggs, 1997, p. 217), while a relative absence of job information and prevalence of deviant work ethics in neighbourhoods with concentrations of poverty is hypothesised to hamper the socio-economic opportunities of its residents (Wilson, 1987, 1996; Galster et al., 1999).

Both European and American neighbourhood interventions aim to change the spatial distribution of the disadvantaged residents across cities to avoid negative impacts of residing in deprived neighbourhoods. In the United States, a dominant approach is to enable poor people to 'move to opportunity', that is, to neighbourhoods that could provide more opportunities because supposedly there are more resourceful social networks and role models available. In Europe, the implicit belief among policymakers is that creating mixed tenure neighbourhoods can maintain and attract middle- and higher income classes which act as adequate role models and network resources for more disadvantaged residents, thereby counteracting social exclusion and negative neighbourhood effects (Andersson and Musterd, 2005).

The Moving to Opportunity (MTO) experiment in the United States randomly allocated vouchers to voluntary applicants living in high-poverty neighbourhoods. Individuals that were assigned to the experimental group are required to move to a low-poverty neighbourhood (Leventhal and Brooks-Gunn, 2003). Overall, no impact of the MTO is found on adults' economic outcomes, although recently, Chetty et al. (2015) found that for children who moved to a lower-poverty neighbourhood when they were young as a result of the MTO experiment significantly improved their long-term economic outcomes as adults. While many researchers have used the MTO data to capture neighbourhood effects in an experimental setting, there has been a considerable debate on whether or not MTO can be employed to estimate neighbourhood effects due to selection bias: residents that were randomly offered a voucher are not forced to use it so compliance is highly selective (Clampet-Lundquist and Massey, 2008).

Moreover, the MTO studies do not help us with the evaluation of mixed tenure development programmes in which incumbent residents are forced to relocate. The aim of these programmes is to change homogeneous and disadvantaged neighbourhoods into mixed-tenure and socio-economically mixed neighbourhoods. The main strategy is the selling and selective demolition of low-rent social housing in neighbourhoods, which are replaced by upmarket dwellings (Kleinhans, 2003; Musterd, 2005). These neighbourhood interventions lead to involuntary moves of incumbent residents in and beyond the neighbourhood. While in the MTO experiment participants deliberately choose to sign up for the programme and could still decide not to comply — they voluntarily moved — relocatees in mixed tenure development programmes are forced to move due to demolition of their dwellings.

Mixed tenure programmes aim to improve the economic prospects of all residents, including those who are forced to move. While residents are obliged to move, they are not necessarily constrained in their choices. The current study is carried out in the Netherlands and this particular institutional context matters: Dutch forced relocatees are entitled to assistance in finding a new dwelling, they attain a priority status in the social housing allocation programme and often receive a financial compensation (Bolt and van Kempen, 2010). This suggests that relocatees should be able to improve their chances of achieving social mobility by moving to a less deprived neighbourhood (Posthumus, 2013). Unlike the MTO experimental group, however, forced relocatees are not required to move to a better-off neighbourhood. Also, there often is a possibility for the forced relocatees to move to newly built dwellings in the same neighbourhood, although in most cases only a subset of the newly constructed housing has been reserved for social rent (Tieskens and Musterd, 2013). This provides an interesting angle: will relocatees due to urban restructuring — who are forced to move but not required to move to low-poverty neighbourhoods — still on average take the opportunity to more affluent neighbourhoods? And will this in turn lead to more socio-economic opportunities?

Whereas the effects of mixing neighbourhoods' tenure in deprived neighbourhoods on its incumbent residents has been thoroughly evaluated (Bolt and van Kempen, 2010; Manley et al., 2012; Kleinhans et al., 2014), thus far socio-economic consequences for Dutch forced relocatees have been largely overlooked. In this study, involuntary relocation due to urban renewal policies serves as a natural experiment to evaluate whether relocatees end up in better-off neighbourhoods. In particular, it evaluates the early impacts of forced relocation on the earnings and employment rates of the relocatees.

Our analytical approach is innovative for three main reasons. First, we are able to track forced relocatees as we got access to data on the population of five urban renewal projects (from 2006-2011) within three neighbourhoods in Amsterdam, the Netherlands, provided directly by a housing association. Second, we have access to the full population of addresses of this particular housing association that were never targeted under the mixed tenure development programmes, that is, are not on the list for demolition or large-scale renovation. This list of non-targeted dwellings enables us to form an optimal control group — through exact matching — of other social housing residents that were never forced to move because of an urban renewal strategy, establishing a quasi-experimental design. Finally, the group of addresses from which residents were forced to move and the group of control addresses of this particular housing association are subsequently linked to a unique longitudinal individual-level population registry database, allowing us to follow the main tenants of the displaced households and the control group residents over time.

This design makes it the first study that is able to study individual socio-economic mobility and residential change for forced relocatees that moved within and beyond their neighbourhood, compared to their counterparts in comparable neighbourhoods that were not forced to move. In the end, we aim to answer the following research question: do forced relocatees live in more affluent neighbourhoods after the forced move, can an upward socio-economic mobility (income, employment) be observed after the move and, more specific, can an upward socio-economic mobility be observed once there has been a considerable improvement in the neighbourhood environment after the forced move?

A difference-in-difference design on the matched pairs of forced relocatees and the control group captures the impact of the forced move and shows that on average forced relocatees live in less socio-economically deprived neighbourhoods after the involuntary move. However, this upgrade in housing does not in turn lead to an improvement in economic prospects, even not for those who live in substantially better-off neighbourhoods after the forced move. These findings do not only challenge the neighbourhood effects literature, but also question the justification of the widespread area-based urban renewal policies.

THEORY

Intended policy effects of relocation strategies

In general, policymakers are concerned with the concentration of poor residents in neighbourhoods; urban relocation policies are essentially aimed at deconcentration of poverty in order to enhance the economic prospects of residents in disadvantaged areas. These policies come in two ways: individual anti-poverty dispersal strategies, which are voluntary mobility programmes — such the American housing mobility experiments Moving To Opportunity — and place-based renewal strategies in disadvantaged areas leading to involuntary relocation, e.g. the HOPE VI programme in the US, but also the demolition and upgrading of social rented housing in Europe (Goetz, 2002).

The argument that residing in concentrations of disadvantage has a detrimental impact on an individual's economic prospects finds its roots in the influential studies on disadvantaged communities in the United States (Lewis, 1997; Stack, 1975; Valentine, 1978; Wilson, 1987); the notion is that residents of disadvantaged neighbourhoods are isolated from the relevant institutions, role models and resourceful social contacts that can give them access to the mainstream culture and job information.

The logic of socialisation and social networks is at the core of this idea. Socialisation in the neighbourhood is a social process in which residents conform to work ethics that are prevalent in those neighbourhoods. A relative absence of positive role models in the neighbourhood and the existence of deviant work ethics of residents can hamper the socio-economic opportunities of a resident. This socialisation process in the neighbourhood is also often referred to as the 'contagion model' or 'epidemic theory' (Crane, 1991; Friedrichs and Blasius, 2003; Pinkster, 2007). The mechanism on social resources refers to the idea that social networks in the neighbourhoods are opportunity structures, containing job information, support and general resources that can enhance one's socio-economic status (Coleman, 1988; Granovetter, 1995; Lin, 1999; Lin et al., 2001).

Neighbourhood interventions aim to alter the spatial distribution of poor residents across cities to prevent negative socialisation and to enhance the quality of neighbour networks. This is assumed to facilitate disadvantaged residents in escaping their socially isolated position, either by an inflow of more affluent new neighbours (positive role models and potential job information) or by the opportunity to move to a more affluent neighbourhood. Although the notion that the neighbourhood can improve residents' economic self-sufficiency originates from the United States, it has also crossed the Atlantic; many European studies aim to confirm empirically that the neighbourhood influences individual socio-economic outcomes (e.g. Andersson et al., 2007; van Ham and Manley, 2010; Musterd et al., 2003).

Policy evaluation of Moving to Opportunity

The US Department of Housing and Urban Development has actively pursued dispersal programmes, such as the Moving to Opportunity experiment (MTO). The MTO programme randomly allocated voluntary applicants (who were thoroughly screened) living in high-poverty neighbourhoods to three groups: the experimental group (received a limited-term housing voucher together with counseling and assistance and are required to move to a low-poverty neighbourhood), a Section 8 group (vouchers without restrictions imposed on where to move) and the control group (Goetz, 2002; Leventhal and Brooks-Gunn, 2003). Movers were thus restricted in their neighbourhood choice as the deconcentration of poverty programme requires and assists them in moving to a low-poverty neighbourhood. MTO treatment has been found to have enhanced physical and mental health and perceptions of well-being and safety of dispersed residents (Chetty et al., 2015).

The compliance with the programme was, however, highly selective (Clampet-Lundquist and Massey, 2008). About one-quarter of families that were eligible (families with children, no criminal record, living in high-poverty neighbourhoods) also applied to the programme and once these families were randomly allocated vouchers, within the experimental group only 47 percent complied to the programme, and 60 percent for the Section 8 group (Goetz, 2002; Leventhal and Brooks-Gunn, 2003). Furthermore, in MTO, moving to a low-poverty neighbourhood does not necessarily mean an improvement in the neighbourhood quality. A recent study has shown that unemployment rates are not necessarily lower and educational levels not necessarily higher in the low-poverty neighbourhoods the participants moved towards (Aliprantis and Kolliner, 2015).

In addition, families were only required to stay in the low-poverty neighbourhood for at least one year but could then relocate back to their former neighbourhood of residence. It was also found that a large share of volunteers who moved to low-poverty neighbourhoods actually moved back to more disadvantaged neighbourhoods (Ludwig et al., 2008). This means that dispersed families are only exposed to low-poverty neighbourhoods for a short period of time. While Clampet-Lundquist and Massey (2008) find that exposure time to more affluent neighbourhoods is related to higher economic self-sufficiency (as expected under the neighbourhood effects hypothesis), Chetty et al. (2015) conclude the MTO experiment does not significantly influence the earnings and employment rates of adults (as also shown by Goetz, 2002; Sampson, 2008).

While many researchers have employed MTO data to attempt to capture the neighbourhood effects in an experimental setting, there has been a considerable debate on whether or not MTO can be employed to estimate neighbourhood effects. Many MTO studies assesed intent-to-treat (ITT) effects, comparing both compliers and non-compliers in the experimental group to all members of the control group (e.g. Kling et al., 2007). The ITT impact thus measures a policy treatment, but does not tell us much yet about the impact of moving as a result of the MTO program. Some scholars have aimed to establish this estimating the treatment of treated (TOT) effect, the impact of the intervention on a subset of the treated group who actually moved in line with the programme, which then "captures the impact of the entire bundle of changes in neighborhood attributes generated by MTO moves." (Ludwig et al., 2008, p. 153). In MTO, households in the Section 8 group were more likely to comply using the voucher, but they did not improve their neighbourhood as much as movers in the experimental group (Clark, 2008).

Whether TOT actually measures neighbourhood effects is heavily debated. Clampet-Lundquist and Massey (2008) warn that also the TOT impact is insufficient to measure neighbourhood effects because of selectivity. Families that were allocated housing vouchers are not forced to use them, so compliance with the experimental treatment was nonrandom. Also, even those who did move with the housing voucher did not always move to less disadvantaged areas and compliers could move back to their old neighbourhood after the initial relocation. The authors conclude: "Given that entry into neighbourhoods and compliance categories was highly selective and the length of stay quite variable, it is hardly surprising that comparisons made between experimental and control group members in MTO have failed to yield the robust and consistent evidence of neighborhood effects found in survey-based studies." (Clampet-Lundquist and Massey, 2008, p. 138) Ludwig et al. (2008, p. 153) disagree with this argument on selectivity under the assumption that receiving a voucher but not using it had no effect on noncompliers in the treatment group and that families in the control group are also not affected by not getting allocated a voucher.

Aliprantis (2011) aims to reconcile these opposing views and argues that actually neither ITT nor TOT are able to estimate neighbourhood effects, but are mostly programme effects. To assess neighbourhood effects, Aliprantis (2011) introduces Local Average Treatment Effect (LATE). These LATE estimates focus on a subpopulation that was induced by MTO to move to higher quality neighbourhoods (while ITT estimates focus on outcomes for entire MTO population and TOT on the subpopulation of compliers induced by the programme to move). Aliprantis (2011) notes that this is another subgroup of compliers: only about 10 percent of MTO participants actually moved to better-off neighbourhoods. In contrast to other MTO studies, which did not find that MTO treatments significantly improved adults' economic prospects, Aliprantis and Richter (2013) found through LATE that a move to a higher quality neighbourhood through the MTO programme has positive and substantial impact on, inter alia, employment and labour force participation.

Policy evaluation of involuntary moving due to urban renewal

It is important to realise that the voluntary anti-poverty dispersal programme MTO focuses on self-selected low-income households who expressed a desire to move. While this does not affect the internal validity of the randomized MTO experiment, it does restrict the MTO analyses to those who are actually interested in getting an opportunity. This contrasts with the involuntary moves of the incumbent residents due to mixed tenure development programmes. In these programmes, disadvantaged neighbourhoods are restructured into mixed-tenure and socio-economically mixed neighbourhoods forcing incumbent residents in targeted dwellings to move out (Kleinhans, 2003; Musterd, 2005).

While Ludwig et al. (2008) argue that ITT estimates are relevant for policy evaluations as non-compliance in these voluntary programmes is inevitable, in involuntary relocation programmes non-compliance is impossible. And while TOT and LATE estimates in MTO show the impact of moving itself and moving to higher-quality neighbourhoods, it does so only for the subset of individuals who have shown and acted upon a willingness to move. Hence, MTO designs have so far not helped us with evaluating forced relocation programmes.

The current study is carried out in the Netherlands, where the mixed tenure development programmes aim to improve the economic prospects of all residents, including those who are forced to move: forced relocatees receive relocation counselling, receive a financial compensation and priority in the social housing allocation programme (Bolt and van Kempen, 2010). The mixed tenure programmes aim for a place-based change in the socio-economic composition of the targeted neighbourhood, but thereby also have the goal to reduce concentrations of low-income groups in general. If relocatees move to another socio-economically disadvantaged neighbourhood, spatial concentrations of poor residents are simply transferred from one neighbourhood to another. In order to circumvent new concentrations of deprivation elsewhere in the city displaced households should therefore ideally move to a diverse range of neighbourhoods in less deprived and different areas and to a better-off neighbourhood than the one they leave behind in order to enhance their opportunities (Posthumus et al., 2013).

Until now, descriptive studies have shown that on average, displaced residents move to neighbourhoods with inexpensive housing and with a low socio-economic status, due to their preference to live among neighbours with similar problems which creates a feeling of safety, but also due to their restricted choice in housing market because of a lack of finances (Posthumus, 2013). Posthumus et al. (2013) also found that the new neighbourhood of forced relocatees often has a large share of ethnic minorities, because the relocatees prefer to live among co-ethnics, but also, again, because relocatees cannot afford to live in other neighbourhoods. Nevertheless, as relocatees often come from high-poverty neighbourhoods, descriptive research in the Netherlands has shown that on average relocatees still improved their living situation by moving to to neighbourhoods with lower concentrations of poverty (Kleinhans, 2003; Tieskens and Musterd, 2013).

The aforementioned studies focus on moving up in the housing career, but whether the urban renewal policies have actually decreased the individual deprivation and poor economic prospects of relocatees remains inconclusive. This is striking, as "[a]nother important aim of mixing is to improve the situation of all residents, including those who are displaced, who are considered to be relatively deprived. Displaced residents would have limited chances for social mobility in their old neighbourhoods because of the presence of high concentrations of (other) deprived residents. These chances would, theoretically, increase by moving to neighbourhoods with better perspectives" (Posthumus, 2013, p. 52).

Only one Dutch study has particularly devoted their attention to the evaluation of the individual socio-economic consequences for forced relocatees (Kleinhans et al., 2014). Kleinhans and colleagues (2014) found no upward socio-economic mobility for stayers and movers within the restructured neighbourhood in Rotterdam. Unfortunately, by focusing on within-neighbourhood movers the authors conflate voluntary movers and forced relocatees and pay no attention to forced relocatees that move beyond the neighbourhood, as their data does not allow for identifying and tracking the residents that are forced to move out their dwelling because of urban renewal strategies. Furthermore, the control neighbourhood was also subject to a urban renewal strategy which makes the comparison group suboptimal. The present study is able to track down forced relocatees (who move within and beyond the neighbourhood) and a control group of other social housing residents that were never forced to move because of an urban renewal strategy, which enables us to study individual socio-economic mobility and residential change for forced relocatees that moved within and beyond the neighbourhood, compared to their counterparts that are not forced to move.

DATA

The present study tracks down the forced relocatees of five demolition projects in Amsterdam. Amsterdam is an excellent case to study whether urban renewal strategies advanced the opportunities of the disadvantaged urban resident. First, in Amsterdam low-rent social housing dwellings can also be found in more affluent areas, not only in the more disadvantaged neighbourhoods. Forced relocatees could thus potentially move upwards in their housing career and possibly gain resourceful contacts in a more affluent neighbourhood. Also, the residential neighbourhoods in Amsterdam are socio-economically less strongly segregated than neighbourhoods most other European countries and are especially very heterogeneous compared to American neighbourhoods (de Vries, 2005; Musterd et al., 2006).

Data selection, anonymization and attrition

The housing association provided us with 1,047 addresses in disadvantaged areas in Amsterdam that are targeted for demolition due to mixed tenure development programmes and 23,104 addresses in Amsterdam that are not on the list for demolition or large-scale renovation. We will refer to this latter group of non-targeted dwellings as our 'control addresses'.[1] The total dataset containing 24,151 addresses was transferred to Statistics Netherlands, who anonymised the addresses with an address key that refers to a population register backbone, which made it possible to identify the individuals that were ever registered on these addresses, their dates of birth and their moving dates (the population register of habitation covers the period 1995-2013). Statistics Netherlands was unable to assign an anonymised address key to 16.6 percent of our 23,104 control addresses. Of our 1,047 urban restructurering addresses, over ten percent could not be assigned an anonymised address key and after resolving other minor issues we ended up with 928 urban renewal addresses.[2] The reason behind the attrition is a mismatch between the address administration of the housing association and the official computerised population register of Statistics Netherlands.

Tracking down relocatees

The first step is to track down the residents of the 928 addresses in the population registry that are subject to the urban renewal plans and forced to relocate. The administration of the housing association contains information on the date of birth and moving dates of most main tenants, as well as the reference date of the project, ranging from

1st of January 2006 (Project A, neighbourhood X, 38 addresses), 1st of February 2006 (Project B, neighbourhood Y, 192 addresses), 1st of January 2008 (Project C, neighbourhood X, 138 addresses), 1st of April 2008 (Project D, neighbourhood Y, 99 addresses) and 1st of December 2009 (Project E, neighbourhood Z, 461 addresses). From that date onwards, residents living in those dwellings targeted for demolition are forced to move in the near future and are entitled to assistance, priority status in the social housing allocation programme and financial compensation.[3]

We need to verify that these individuals in the housing association administration were also officially registered in the population registry, as this registration makes it feasible to track them over time in the administrative registers. We will only focus on one individual per address, namely the main tenant as registered in the housing association administration (these main tenants are entitled to assistance, a priority status and financial compensation).[4]

We can use both the birth dates/years and moving dates to verify that the persons that are registered on the urban renewal addresses in the population registry are the main tenants as registered in the administration of the housing association. Ideally, individuals match on both the birth dates and moving dates in the population and housing association administrations. However, we decide that being registered on the address on the reference date and having the same date of birth as known by the housing association is sufficient, as a mismatch in moving dates is often a result of delayed registration in the population register. Therefore, our main method to identify the main tenants is by using the date of birth. Using the month and year of birth, we were able to track down only half of the main tenants (459 out of 928). Using only year of birth we are able to track down another 85 tenants, resulting in 544 individuals.[5]

An alternative way of tracking down relocatees when the birth date is not available is through the move-in and move-out dates of the addresses.[6] To identify the main tenant of the urban renewal dwellings of which the move-in and move-out dates in the population registry resemble the ones in the housing association register — we decided that the difference between both registers cannot be more than 100 days — we selected the oldest tenant registered on the address. This resulted in identifying another 28 main tenants on urban renewal dwellings.[7] In the end, we were able to track down 572 forced relocatees living on the addresses that are targeted for demolition on the reference date.[8]

We continued with linking these forced relocatees to longitudinal administrative socio-economic, demographic and residential registers (Social Statistical Database, Statistics Netherlands). We allocated the address, neighbourhood of residence, income and employment information and household status on the 31st of December for each year from 2005-2011. This brought to light some missing cases, we checked this manually to prevent unnecessary loss in our treatment group. In addition, we excluded forced relocatees that were above 65 years old, who have an outlier on personal and primary income and those whom could not be followed up after the forced move.[9] This yields a group of 449 forced relocatees; 29 in Project A, 153 in Project B, 119 in Project C, 60 in Project D and 88 in Project E.[10]

Tracking down main tenants on control addresses

We start out with the control group of 17,918 addresses. From 2006 onwards many relocation projects were taking place in Amsterdam and housing associations joined forces in relocating social housing tenants. Although we know with certainty that the control addresses are not on the list for demolition or large-scale renovation, we cannot guarantee new residents from 2006 onwards are not displaced residents from elsewhere. Therefore, to select residents for our control group we make the restriction that individuals should already live on a control address on 1st of January 2006.[11] As multiple individuals can be registered to a control address, we select the reference person of the household as assigned by Statistics Netherlands as the control individual. The reference person is the member of the household which features are characteristic for the household and on which the household positions of the other household members are based.[12] We only selected those more conventional household situations with only one reference person. As with the forced relocatees, we then linked these individuals to the longitudinal administrative registers for each year from 2005-2011. Eventually, we employed an exact matching method that aims to find control observations that are very similar to the treated observations (the forced relocatees), thereby reducing the imbalance between the treated and control group. We will elaborate on this in the Methods section. For the matching procedure we need information for all years for the control group to find comparable observations to our treated individuals, so we only keep individuals that were registered in each year and excluded control individuals that have an outlier on personal or primary income, resulting in an initial control group of 13,610 individuals.

METHOD: DIFFERENCE-IN-DIFFERENCE WITH MATCHING

We confine our investigation to the residential and socio-economic consequences for forced relocatees, the main tenants of social housing dwellings in Amsterdam that are demolished. In the previous section, we identified the main tenants of the addresses targeted for demolition and control addresses. Next, we introduce how we identified the treatment and discuss our difference-in-difference estimation. Then, we discuss our selection of the control individuals through coarsened exact matching, followed by a discussion on our outcome measures.

Difference-in-Difference

The treatment is the forced move and the treatment package includes assistance in finding a new dwelling, priority status in the social housing allocation programme and financial compensation for moving costs. We examine the impact of the relocation treatment, although we cannot identify which of the elements of the package (financial compensation or the priority status in the housing allocation programme for instance) is effective. Concretely, the treatment is the first move that is observed after the reference date (start of project given by the housing association).[13] We follow up the forced relocatees from at least one year after the treatment. We focus solely on individual relocatees and not on the regeneration and improvement of the socio-economic composition of neighbourhoods after the urban renewal policy. Following this strategy means we do not take into account possible spill-over effects of the urban renewal for the area and the wider community, but sharpen the focus to what has not yet been investigated: the residential change and socio-economic consequences for the forced relocatee.

In the Moving to Opportunity programme participants are randomly allocated housing vouchers to move to lower-poverty neighbourhoods. To estimate the intent-to-treat impact, MTO researchers could suffice by simply comparing the average difference in the post-treatment outcome between the treatment and control group (as done by Kling et al., 2007). This simple comparison would lead to biased results in the involuntary relocation programmes because the non-random nature of the involuntary Dutch relocation strategies. The treatment effect will be confounded by differences in pre-treatment outcomes as residents are not randomly selected into the treatment and control group; forced relocatees are generally more disadvantaged than social housing residents in general. Indeed, their disadvantage is the main motive behind the urban renewal policy.

Ultimately, we want to estimate the difference between the outcomes of the forced relocatees and outcomes of the forced relocatees if they had not been forced to move out of their homes. The latter, the counterfactual, is not observable. This is referred to as the fundamental problem of causal inference: an individual cannot be observed in the treatment and control state at the same time (Holland, 1986). The difference-in-difference approach (DID) circumvents the biases and issues as described above. DID can be seen as a simple difference estimator between the actual outcome for the treated group and the outcome that would occur in the post-treatment period to the treated unit if there had been no treatment. In DID, the mean difference in the control group is subtracted from the mean difference in the treatment group as is shown in the formula below:

$$
\begin{aligned}
\tau^{DID} = & \big[E[Y_i|R_i = 1, t_i = 1] - E[Y_i|R_i = 1, t_i = 0]\big] \\
& - \big[E[Y_i|R_i = 0, t_i = 1] - E[Y_i|R_i = 0, t_i = 0]\big]
\end{aligned}
$$

In this equation $R_i = 1$ refers to the treated group that is forced to relocate and $R_i = 0$ to the control group and $t_i = 0$ to the pre-treatment period and $t_i = 1$ to the post-treatment period. DID thus estimates the average difference over time in the control group substracted from the average difference over time in the treatment group. This gets around the problem of not observing the counterfactual. The unobserved counterfactual non-treatment outcome for the treated can be identified as follows: the pre-treatment outcome of the treated is used to infer the level of the counterfactual outcome and the change of that outcome is inferred from the change over time (difference between post- and pre-treatment) that is observed for the control group.

The regression equation for this model — using the same terminology as above— is as follows:

$$Y_i = \beta_0 + \beta_1 \cdot R_i + \beta_2 \cdot t_i + \beta_3 \cdot R_i \cdot t_i + \epsilon_i$$

Here β_0 is a constant term, β_1 is the treatment group specific effect (accounts for difference between treated and control group on the base line), β_2 is a time trend common to the control and treatment group and β_3 is the DID estimator, the impact of the treatment. In our case this would be the impact of the forced move. In sum, DID removes biases in the post-treatment comparison between the treated and control groups coming from differences between these two groups, removes the influence of time-constant unobserved heterogeneity and it removes biases coming from time trends.

Triple Difference

The DID design measures the overall treatment effect of a forced move due to urban restructuring and does not take into account that forced relocatees might be differentially affected depending on the type of neighbourhood they move towards. We aim to further refine the definition of having received a treatment towards those that actually moved to a more affluent neighbourhood. In particular, we investigate whether the treatment effect is different for those that have made a considerable improvement in their neighbourhood conditions after the forced move; we particularly expect there to be a positive impact on socio-economic outcomes for those that relocated to a more affluent neighbourhood.

A dummy was created that indicates whether an individual moved to another and a more affluent neighbourhood (decrease of at least 0.5 points on our standardised neighbourhood deprivation index, both forced relocatees and control individuals could obtain a score on this dummy variable). In the Difference-in-Difference-in-Difference (Triple Difference) design, first, the difference at $t_i = 0, 1$ for individuals in the control group ($R_i = 0$) that *not* moved to a more affluent neighbourhood ($\mathtt{BNbh}_i = 0$) is subtracted from the difference for individuals of the treatment group ($R_i = 1$) at $t_i = 0, 1$ that had also *not* moved to a better-off, more affluent neighbourhood ($\mathtt{BNbh}_i = 0$). This first difference-in-difference is then substracted from the second difference-in-difference: difference at $t_i = 0, 1$ for individuals in the control group that did move to a more affluent neighbourhood ($BNbh_i = 1$) subtracted from the difference in the treatment group at $t_i = 0, 1$ for individuals that moved to a more affluent neighbourhood ($BNbh_i = 1$). This Triple Difference estimate captures how different the DID estimate is for those that moved to a more affluent neighbourhood. This is shown in the formula below:

$$\begin{aligned}\tau^{DDD} = \Big[&\big[E[Y_i|R_i = 1, BNbh_i = 1, t_i = 1] - E[Y_i|R_i = 1, BNbh_i = 1, t_i = 0]\big] - \\ &\big[E[Y_i|R_i = 0, BNbh_i = 1, t_i = 1] - E[Y_i|R_i = 0, BNbh_i = 1, t_i = 0]\big]\Big] \\ -\Big[&\big[E[Y_i|R_i = 1, BNbh_i = 0, t_i = 1] - E[Y_i|R_i = 1, BNbh_i = 0, t_i = 0]\big] - \\ &\big[E[Y_i|R_i = 0, BNbh_i = 0, t_i = 1] - E[Y_i|R_i = 0, BNbh_i = 0, t_i = 0]\big]\Big]\end{aligned}$$

Identifying control individuals through matching

A key assumption in DID is the parallel trends assumption. This means that we have to assume that the average change in the control group represents the counterfactual change in the treatment group if there were no treatment. In other words, DID only equals the treatment effect if both the treated and control group follow similar paths in the outcome variable before the treatment. This assumption is violated when pre-treatment covariates (including unobservables) that are associated with the outcome variable are unbalanced between the treated and the control group (Angrist and Pischke, 2009). Because of our natural experiment setting, in which the assignment to the treatment of the forced move is not random, this is something that we have to be especially aware of.

We will apply exact matching to circumvent the different parallel trends in the pre-treatment period. The matching method aims to find control observations that are very similar to the treated observations on observed characteristics and thereby reduces the imbalance in the pre-treatment characteristics — meaning more similarity in the distributions of the covariates — between the treated and control group. Consequently, the treatment variable is more independent from the other covariates and reduces the bias and model dependence in the analyses (Blackwell et al., 2009; Ho et al., 2007). In case of DID, matching makes sure that the pre- and post-treatment comparison is based on two groups that resemble each other before the treatment, thereby obeying to the parallel trends assumption.

We use coarsened exact matching (CEM) as a nonparametric method to create an optimal control group.[14] In CEM, the variables are coarsened according to our choice and an exact match is performed on these temporarily coarsened data (Blackwell et al., 2009). This transparant, ex ante user choice, imposes the same distribution of the covariates for the control and treatment groups and reduces the imbalance between treated and control observations. As the data is pruned from unmatched units, the treatment and control units are more similar before the treatment which is beneficial to our DID estimator.

We observe the group of forced relocatees and the control individuals over multiple years (2005-2011) and the year of treatment differs per relocatee. Matching on panel data is not straightforward as panel data involves dynamic covariates, while conventional matching periods are focused on matching observations rather than dynamic panels.[15] We match treated individuals to control individuals on the year prior to the forced move; thereby assuming that the forced move is exogenous and the year before the forced move is a good reflection of a relocatee's demographic, socio-economic and residential status. This pre-treatment information is assumed to provide sufficient information on the observables to match to a counterpart in the control group.[16] In the end, only those individuals in the control group are selected who resemble our relocatees in the year before the forced move on some key socio-demographic characteristics (age, gender, socio-economic category, immigrant background, household composition, neighbourhood deprivation). The forced relocatees are thus matched to the control group in the same calendar year. For our DID estimation we have then selected the year before treatment and one (and multiple) year(s) after the treatment for the forced relocatees and the same years for the control individuals.[17] In the Descriptives section we further elaborate on the practical implementation of this method.

Causality

Our research design combines the advantages of exact matching — improving the comparability of treated and control group — with the advantages of differencing in panel data that controls for timeinvariant individual effects. Thus, matching helps us to deal with selection on observables, while DID deals with unobserved heterogeneity. Our difference-in-difference estimation with matching is causal under a few assumptions: we have matched on observables and have to assume that we have included the most important characteristics (unfortunately, we could not match on educational attainment as education is not yet registered for the entire population) (Angrist and Pischke, 2009). We also assume that the trend in the treatment group would have been the same as in the control group had the treatment not been given (parallel trends assumption). While this assumption is sometimes tested by looking at the trend before the start of the observation window, our data do not allow for such an analysis because relocatees have their treatment in different years and we can only observe individuals for a limited time period. Nevertheless, the combination of DID and matching ensures that our results are strongly suggestive of the identification of a causal effect.

Outcome measures

We study individual socio-economic mobility and residential change for forced relocatees that moved within and beyond their neighbourhood, compared to their counterparts in comparable neighbourhoods that were not forced to move. We first estimate whether on average forced relocatees are living in less socio-economically deprived neighbourhoods after the forced move, measured by a standardised neighbourhood-level deprivation index. This index is constructed from several income and welfare benefits measures coming from the datafile *Key Figures Districts and Neighbourhoods* from Statistics Netherlands. The five items are (1) the average personal income per income recipient; (2) the average personal income per resident; (3) percentage of income recipients with income less than or equal to the 40th percentile of the national income distribution; (4) percentage of income recipients with income equal to or greater than the 80th percentile of the national income distribution (5) number of welfare benefits per 1,000 households. A high proportion of residents on welfare, a high share of low-income people and low share of high-income people, low average income in general and per income recipient captures the level of socio-economic deprivation in the neighbourhood. Following the research field's line of reasoning, a more deprived neighbourhood has a relative absence of positive role models, resources and job information in the neighbourhood. Using the neighbourhood definition by Statistics Netherlands, Amsterdam has neighbourhoods with an average size of about 8,000 residents. A standardised index of neighbourhood deprivation is created for each year based on the available information from all Dutch neighbourhoods.[18] The neighbourhood deprivation score is standardised per year because the data collection, number of neighbourhoods on which information is available, and measurement of (average) personal income has changed over the years. The internal consistency for the index was examined using Cronbach's alpha for standardised items and is high (mean alpha of standardised items of 0.90). The Kaiser-Meyer-Olkin postfactor measure of sampling adequacy is employed to assess the factorability and is also very adequate (mean KMO is 0.79). Only one component for each neighbourhood is estimated which explains on average over 72 percent of the variance (eigenvalue of this component is on average 3.61).[19]

Then we focus on two socio-economic consequences of the forced move: change in income and employment status. Income is available on a yearly basis and is adjusted for inflation until 2011 using the Consumer Price Index, and is measured by the natural logarithm. Incomes that were zero or negative are set to 1, resulting in a log of 0. We focus on two types of income: (1) logged primary income, consisting of income from work and income from a business. Income from work consists of wages and salaries (including the contributions to social insurance from employee and employer) and income from the own business is the profit made; (2) logged personal income, which goes beyond primary income and also includes transfers from social security benefits, fundings and alimonies minus the premiums for income insurance.

For employment status, we rely on an individual's socio-economic category as defined by Statistics Netherlands, which refers to the main source of income of a person measured over the entire year.[20] We create a dummy variable that denotes whether an individual is employed or not. We consider an individual to be employed when that individual is denoted by Statistics Netherlands as a employee, civil servant, self-employed, director/majority stakeholder or other active within that year. Those on benefits, (early) pension, students, other non-active or without income are considered as not employed.

Table 5.1: Pre-treatment characteristics on which main tenants are matched

Variable	Coarsened values
Age	18–25, 26–35, 36–45, 46–55, 56–65, over 65
Gender	male/female
Socioeconomic category	employee/civil servant; self-employed/director, majority stakeholder/other active; unemployment benefits; welfare benefits/social security/disability benefits; (early) pension/student/other non-active/without income
Ethnicity	native Dutch; first generation immigrant; second generation immigrant
Household composition	couple with children; single parents
Neighbourhood Deprivation Index	Automatic binning algorithm Sturge's Rule

MATCHING AND DESCRIPTIVES

Coarsened Exact Matching procedure: evaluation of unmatched data

In the Data section we described how we identified the treated and control group. This dataset with 449 treated and 13,610 control individuals is followed over the course 2005-2011 (7 years). We aim to select those residents in the control group who resemble treated units in the year before the forced move. We create a new dataset with the year prior to treatment for the forced relocatees (449 observations) and the full observation period (2005-2011, 95,270 control observations) for the control group as the relocatee should be matched to the control group in the same calendar year.

CEM is a trade-off between exactness (the definition of what a match is) and the number of matched observations. To attain a sizeable control group, we only matched on the (grouped) categories as reported in Table 5.1. In addition to these variables, we require the control group to be matched in the same year as the pre-treatment year for the treatment group, to live in Amsterdam and to be registered as living in a rental house in that same pre-treatment year. We create two matched datasets: (1) to estimate the treatment effects on income, all categories as mentioned in Table 5.1 are used to match

Table 5.2: Pre-treatment descriptives of forced relocatees and control group

	Forced relocatees		**Control individuals**	
	N	Percentage	*N*	Percentage
Gender				
Male	230	62.33%	230	62.33%
Female	139	37.67%	139	37.67%
Socioeconomic category				
Employee company	176	47.70%	175	47.43%
Civil servant	20	5.42%	21	5.69%
Director/majority stakeholder	0	0.00%	0	0.00%
Self-employed	23	6.23%	21	5.69%
Other active	0	0.00%	2	0.54%
Unemployment benefits	6	1.63%	6	1.63%
Welfare benefits	100	27.10%	88	23.85%
social security benefits	4	1.08%	3	0.81%
Disability benefits	28	7.59%	41	11.11%
Pension until 65	1	0.27%	3	0.81%
Pension above 65	0	0.00%	0	0.00%
Student	4	1.08%	4	1.08%
Other non-active	1	0.27%	0	0.00%
Without income	6	1.63%	5	1.36%
Household composition				
Living at parental home	0	0.00%	0	0.00%
Single	124	33.60%	115	31.17%
Couple without children	43	11.65%	46	12.47%
Couple with children	125	33.88%	125	33.88%
Single parent	74	20.05%	74	20.05%
Other	3	0.81%	9	2.44%

Source: authors' calculations using housing association administration merged with Social Statistical Database, Statistics Netherlands.

Table 5.2: Pre-treatment descriptives of forced relocatees and control group (continued)

	Forced relocatees		Control individuals	
	N	Percentage	N	Percentage
Ethnicity				
Native Dutch	50	13.55%	50	13.55%
Turkish (1st generation)	42	11.38%	55	14.91%
Turkish (2nd generation)	6	1.63%	4	1.08%
Moroccan (1st generation)	68	18.43%	86	23.31%
Moroccan (2nd generation)	8	2.17%	8	2.17%
Surinamese (1st generation)	69	18.70%	52	14.09%
Surinamese (2nd generation)	12	3.25%	10	2.71%
Antillean/ Aruban (1st generation)	18	4.88%	11	2.98%
Antillean/ Aruban (2nd generation)	1	0.27%	1	0.27%
Western (1st generation)	10	2.71%	17	4.61%
Western (2nd generation)	5	1.36%	10	2.71%
Other non-Western (1st generation)	78	21.14%	64	17.34%
Other non-Western (2nd generation)	2	0.54%	1	0.27%
Pre-treatment year				
2005	24	6.50%	24	6.50%
2006	34	9.21%	34	9.21%
2007	106	28.73%	106	28.73%
2008	39	20.57%	39	20.57%
2009	166	44.99%	166	44.99%

	Forced relocatees					Control individuals				
	N	Mean	SD	min	max	N	Mean	SD	min	max
Age	369	43.076	9.891	20.835	62.998	369	43.500	10.100	21.500	64.501
Neighbourhood Deprivation Index	369	1.844	0.424	1.414	2.429	369	1.868	0.410	1.404	2.429
Logged primary income	369	6.541	4.902	0	11.417	369	6.517	4.819	0	11.389
Logged personal income	369	9.816	1.382	0	11.310	369	9.753	1.373	0	11.389

Source: authors' calculations using housing association administration merged with Social Statistical Database, Statistics Netherlands.

the treated to control observations; (2) to estimate the treatment effects on employment, we match on the same characteristics except on the socio-economic category (neither on earnings, as this reflects the employment status).

Table A5.1 in the appendix shows the imbalance between the treated and control observations in this dataset for these categories. The L1 measure indicates the overall imbalance and is a baseline reference for unmatched observations, 0 refers to perfect global balance and the higher the value the larger the imbalance between the groups (with a maximum imbalance of 1, which means complete seperation) (Blackwell et al., 2009). Our L1 statistic is 0.995, indicating a very high disbalance. This high L1 statistic is not surprising, giving the set-up of our data: only 449 treated observations against 95,270 control observations.

We apply the *k*-to-*k* solution in CEM that randomly prunes the data until each strata has the same number of treated and control observations. This matching procedure provides us with 369 matches (coming from 336 unique individuals: 307 control individuals were used once, 25 individuals were used twice and 4 are used 3 times in different years). Our matching solution substantially reduced the imbalance to 0.629. We merge the matched observations in the pre-treatment year back to the original panel dataset and create a difference in difference dataset in which only the pre- and post-treatment year(s) for the treated and control group are preserved.

See Table 5.2 for the descriptives for the main group of 369 forced relocatees, showing that on average individuals in the treated and control group are very similar. We did not match exactly on living in the same neighbourhood, but while our treatment groups are based in three neighbourhoods, control observations are found in ten neighbourhoods of similar levels of socio-economic deprivation (and two out of three treated neighbourhoods are also represented here).

RESULTS

Residential change for forced relocatees

Before considering the improvement in economic prospects (earnings and employment) after the forced move, we focus on the resi-

Table 5.3: Difference-in-Difference on neighbourhood deprivation index

		Before treatment			After treatment		
		Cont.	Treat.	Diff.	Cont.	Treat.	Diff.
one year	Mean/Differences	1.868	1.844	-0.024	1.627	1.108	-0.524
	Standard Deviation	0.410	0.424		0.707	1.009	
	Difference-in-Difference	**-0.500*****					
	Standard Error	0.071					
	p-value	0.000					
	N observations per group	369	369		369	369	
	N observations	1,476					
two years	**Difference-in-Difference**	**-0.810*****					
	Standard Error	0.101					
	p-value	0.000					
	N observations	812					
three years	**Difference-in-Difference**	**-0.742*****					
	Standard Error	0.11					
	p-value	0.000					
	N observations	648					
four years	**Difference-in-Difference**	**-0.866*****					
	Standard Error	0.195					
	p-value	0.000					
	N observations	212					

Two-tailed test: † $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Source: authors' calculations using housing association administration merged with Social Statistical Database, Statistics Netherlands.

dential change of forced relocatees and aim to answer the question whether forced relocatees on average live in less socio-economically deprived neighbourhoods after the treatment than before the treatment. In our difference-in-difference design on the 369 individuals that were matched and that we could follow up to a year after the treatment, we found on average a very substantive improvement in their neighbourhood conditions. Table 5.3 shows that the year after the treatment, the standardised deprivation index of the neighbourhood of residence of forced relocatees is 0.500 points lower compared to their counterparts in the control group. This reflects a change of half a standard deviation in the standardised neighbourhood deprivation index. Being forced to relocate thus seems to have led to a substantive decrease in deprivation of the living environment compared to those individuals that were not forced to move (but could still voluntarily move).[21]

Table 5.3 also shows the change two, three and even four years after the treatment. This results in a smaller sample size and one should keep in mind that this also leads to another selection of projects as the year of treatment differs per project and individual. Our goal is to see if in general, forced relocatees live in better socio-economic neighbourhood conditions than before the treatment. Two years after the treatment, we can still observe 203 forced relocatees, who score 0.810 points lower compared to the control group. Three years after the treatment, the deprivation index is 0.742 points lower for the 162 forced relocatees we could still observe. And four year after the treatment we only observe 53 individuals and for them the deprivation index is 0.866 points lower.

This increasing improvement might be explained by improved neighbourhood conditions of the neighbourhoods in which the restructuring took place: individuals that were forced to move, but moved within the neighbourhood, then also experience an gradual upgrade in their living conditions.[22] Nevertheless, the largest improvement still comes from those moving out of the neighbourhood. Also, tracking individuals for multiple years means that we only focus on a subset of the forced relocatees which we can observe for a longer period of time. Those that moved later in our observation period are not included in this subset, while this group moved relatively more often within the neighbourhood and thus experienced less of an upgrade. A general trend is still very apparent though: also a few years after the treatment, on average, individuals that were forced to move end up in much less deprived neighbourhoods than if they were not forced to move. These findings are in line with earlier research (Kleinhans, 2003; Tieskens and Musterd, 2013).

Difference-in-Difference: Income change for forced relocatees the year after the forced move

We have shown that on average, forced relocatees end up in less deprived neighbourhoods compared to their counterparts that were not forced to move. We now focus on socio-economic consequences. More concretely, we investigate whether individuals increased their earnings after the forced move. First, we focus on logged primary income. Table 5.4 shows the difference-in-difference model for logged primary income. Individuals that were forced to move earn on average slightly

Table 5.4: Difference-in-Difference on logged primary income

	Year before treatment			One year after treatment		
	Cont.	Treat.	Diff.	Cont.	Treat.	Diff.
Mean/Differences	6.517	6.541	0.023	6.420	6.307	-0.114
Standard Deviation	4.819	4.902		4.862	4.948	
Difference-in-Difference	**-0.137**					
Standard Error	0.508					
p-value	0.787					
N observations per group	369	369		369	369	
N observations	1,476					

Two-tailed test: † $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Source: authors' calculations using housing association administration merged with Social Statistical Database, Statistics Netherlands.

less after the treatment compared to those that were not forced to move. This difference is, however, not significant.

Triple Difference: Income change for forced relocatees that had a considerable improvement in their neighbourhood environment the year after the forced move

On average forced relocatees are living in better-off neighbourhoods compared to those that were not forced to move. Furthermore, on average, there is no impact of forced relocation on logged primary income. So far, we only estimated the impact for residents that are forced to move, regardless of whether they moved to a less deprived neighbourhood or not. We now focus on those residents that actually moved to more affluent neighbourhoods (decrease of at least 0.5 points on our standardised neighbourhood deprivation index). Only 39 of the control individuals had moved to a better neighbourhood, while 150 forced relocatees moved beyond the original neighbourhood to a neighbourhood that is much less socio-economically deprived.

Table 5.5 shows our Triple difference model for log primary income. The first DID estimate (-0.396, n.s.) captures the average differential change in income for the forced relocatees that did not move to a more affluent neighbourhood relative to the change in income for those in the control group that also did not move a more affluent neighbourhood. The second DID estimate (0.485, n.s.), captures the

Table 5.5: Triple Difference on log primary income

	Year before treatment			One year after treatment			
Did not move to more affluent neighbourhood	Cont.	Treat.	Diff.	Cont.	Treat.	Diff.	First DID
Mean/Differences	6.262	6.406	0.144	6.200	5.948	-0.252	-0.396
Standard Deviation	4.895	4.959		4.908	5.052		
N observations per group	330	219		330	219		
Moved to more affluent neighbourhood	Cont.	Treat.	Diff.	Cont.	Treat.	Diff.	Second DID
Mean/Differences	8.673	6.737	-1.936	8.281	6.830	-1.451	0.485
Standard Deviation	3.483	4.828		4.054	4.761		
N observations per group	39	150		39	150		
Triple Difference	**0.881**						
Standard Error	1.373						
p-value	0.521						
N observations	1,476						

Two-tailed test: † $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.00$. Source: authors' calculations using housing association administration merged with Social Statistical Database, Statistics Netherlands.

average differential change in income for the forced relocatees that moved to a more affluent neighbourhood relative to those in the control group that also moved to a more affluent neighbourhood, which is positive but non-significant.

The Triple difference estimate (0.881) captures how different this DID estimate is for those that did move to a more affluent neighbourhood. The positive estimate suggests that the negative impact of being forced to move is less negative for individuals that are forced to move but move to a considerably better neighbourhood, but this estimate is — as the DID estimates — not significant. The total treatment effect for forced relocatees that moved to a more affluent neighbourhood is the sum of the first DID and Triple difference estimate, which would turn out positive, but is thus not significant.

It is interesting to point out though, that forced relocatees that moved to a more affluent neighbourhood have on average a much lower income before the treatment than those in the control group that also moved to a better-off neighbourhood. This seems to suggest that the treatment enables residents with a lower income to move beyond the neighbourhood of residence to a better-off neighbourhood.

Despite the non-significant findings the model thus seems to be indicative of an overall positive total treatment effect for forced relocatees that moved to a more affluent neighbourhood. We should, however, be careful with making a causal claim here; matched pairs of forced relocatees and the control group are based on an overall match on pre-treatment characteristics regardless of a move to a more affluent neighbourhood. It should also be noted that forced relocatees who did not move to a more affluent neighbourhood still moved, while the control group that did not move to a more affluent neighbourhood consists of both non-movers and movers (318 control individuals did not move, 4 moved within the neighbourhood and 8 moved beyond the neighbourhood but not to a more affluent neighbourhood).

DID and Triple Difference: Income and employment change for forced relocatees multiple years after the forced move

Table 5.6 shows the DID and Triple Difference estimates for logged primary income, logged personal income and employment. For primary and personal income, our estimates turn out to be statistically non-significant, also when we focus on the forced relocatees that we were able to track over multiple years. When we focus on employment, which consists of a different matched sample (not matched on socio-economic categories), we also do not find a significant improvement in the proportion of forced relocatees that are employed after the treatment.

Table 5.6: Difference-in-Difference and Triple Difference

	log primary income		log personal income		employment‡	
Difference-in-Difference		(SE)		(SE)		(SE)
one year	-0.137	(0.508)	0.057	(0.134)	0.002	(0.048)
N observations	1,476		1,476		1,700	
two years	-0.170	(0.678)	-0.125	(0.489)	0.020	(0.063)
N observations	812		812		1,008	
three years	-0.343	(0.768)	-0.022	(0.222)	0.005	(0.069)
N observations	648		648		820	
four years	-0.607	(1.381)	-0.447	(0.520)	-0.013	(0.115)
N observations	212		212		300	
Triple Difference						
one year	0.881	(1.373)	-0.025	(0.364)	0.121	(0.144)
N observations	1,476		1,476		1,700	
two years	0.768	(1.746)	0.002	(0.468)	-0.085	(0.159)
N observations	812		812		1,008	
three years	0.828	(1.954)	0.152	(0.568)	-0.043	(0.160)
N observations	648		648		820	
four years	0.369	(3.458)	0.911	(1.313)	0.088	(0.288)
N observations	212		212		300	

Two-tailed test: † $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Source: authors' calculations using housing association administration merged with Social Statistical Database, Statistics Netherlands.

‡ These models are based on a different number of observations than the models on earnings, as for the models on employment the data is not matched on the socio-economic categories (neither on earnings, as this reflects the employment status). We do match on age, gender, ethnicity, household composition and the neighbourhood deprivation index. As we do not require the treatment and control group to be balanced on the socio-economic categories, we obtain a slightly larger sample than in the models on earnings.

CONCLUSION

Alongside the academic debate on the existence and nature of neighbourhood effects, there is an increasing demand among policymakers to get more insight in whether relocation policies can reduce individual deprivation, because more economic opportunities are assumed to become available after the move. The place-based urban renewal strategy in the Netherlands, leading to involuntary relocation, served as a natural experiment in this study that allowed us to study this.

The main finding of this study is that on average forced relocatees have moved to a less deprived neighbourhood. The finding that relocatees generally improved their living situations suits the policy and supports earlier descriptive research (Kleinhans, 2003; Tieskens and Musterd, 2013), but this upgrade in housing career does not translate in socio-economic consequences, which was at the core of our study. We found no significant decrease in individual deprivation and improvement in economic prospects for the forced relocatees: on average, forced relocatees did not improve their economic prospects (earnings and employment). While our difference-in-difference estimates might have understated the impact of actually moving to a better-off neighbourhood, as not all residents move away from deprived neighbourhoods, also our Triple difference estimates do not show any significant improvements in socio-economic mobility. This is in line with recent work by Chetty et al. (2015), who finds no effects of MTO treatments on economic outcomes of adults.

While there are no significant differences between the treatment and control group in terms of economic outcomes, it might take longer for relocated residents to get connected to their new neighbourhoods. It could also be argued that relocation is not necessarily beneficial for residents: "forced to move away from their social support networks they lack the necessary contacts to find work and the employment opportunities through local social institutions" (Musterd and Pinkster, 2009, p.58). From the American literature, it follows that the relocatees that are found to move (back) to nearby socio-economically disadvantaged areas do not necessarily worsen their overall situation as they might be able to secure their existing social networks and live close to services and resources. From this it follows that there is not one clear scenario for improvements after relocation and that there are many challenges in pinpointing the actual treatment effect on economic prospects (Clampet-Lundquist and Massey, 2008; Kleinhans et al., 2014; Bond et al., 2013).

Furthermore, urban renewal policies are not only expected to be beneficial for relocatees, but also for other incumbent residents and the wider community in the area. Evaluation of this policy is challenged by the complex situations underlying the intervention and methodological challenges in quasi-experimental designs. The definition of an intervention is already a challenge, in words of Bond et al. (2013, p.942): " The intervention is difficult to define. It comprises multiple, interrelated activities (demolition, new builds aimed at tenure diversification, housing improvements, and social and community interventions), delivered in different ways to different people in different places and at different time points."

In general, it is difficult to identify the recipients of these neighbourhood interventions; the targeted dwellings are inhabited by a vulnerable and diverse group of residents living in a very deprived area, often characterized by instability in their household composition. Moreover, it is difficult to find out what can be attributed to the intervention and what not as it often takes many years before restructurings yield results. One should also keep in mind that in our matching approach, we pruned the observations so that the distribution of potential confounders is as similar as possible amongst the treated and control group. While this matching procedure increases internal validity — as we are able to obtain an estimate that is strongly suggestive of the identification of a causal treatment effect — it decreases the external validity. We forced the controls to be more similar to the treated group and less comparable to the general population and we thus cannot draw conclusions from those that are unmatched and pruned from the data.

Besides the methodological challenges, Kleinhans et al. (2014) mention some possible explanations why forced relocation has not lead to upward socio-economic mobility. First, they argue that improving individual socio-economic prospects by an area-based policy is an idealistic goal to begin with, as no studies so far showed higher incomes and more jobs in restructured neighbourhoods. In addition, the existence of neighbourhood effects, the supposed beneficial impact of middle-income neighbours (role models, resources and job information), is still very much debated. Social interaction between various social groups in mixed-tenure neighbourhoods is not self-evident, which undermines the potential of positive role models and resourceful contacts (Kleinhans, 2004). Second, the authors note that forced relocatees in deprived areas are among the most disadvantaged groups in society and they already have a limited upside potential and poor economic prospects. Third, they note that there have been cutbacks on social services that could assist forced relocatees both in finding a (new) job.

Finally, it simply takes time for effects of urban restructuring to show; it could be that in a decade we observe a positive impact for children and adolescents in the school-going age in the treatment group, as these individuals got the chance to grow up in a less deprived neighbourhood. Chetty et al. (2015) studied outcomes almost two decades after the random assignment in MTO and found a positive treatment effect for children who moved to lower-poverty areas while they where young; this move increased their future annual income substantially as well as their college attendance rates and quality of college attended. Future research should therefore focus on the long-term economic outcomes for children after a forced move who might benefit from better living situations, potentially better schools in better-off neighbourhoods and safer surroundings. Recent evidence from Sweden shows that living in a deprived area can be persistent and inherited; individuals tend to end up in neighbourhoods similar to where they grew up (van Ham et al., 2014). This suggest that inequality is partly inherited via neighbourhoods, which can potentially be reduced by offering low-income families with young children assistance in moving to lower-poverty neighbourhoods.

APPENDIX

Table A5.1: Measuring imbalance between treated and control observations

Univariate imbalance		mean	min	max
Gender		-0.022	0	0
Age		-9.919	0.496	-37.914
Neighbourhood Deprivation Index		1.056	5.694	-0.692
Socioeconomic category				
employee/civil servant;		0.080	0	0
self-employed/director, majority stakeholder/other active;		0.025	0	0
unemployment benefits;		0.008	0	0
welfare benefits/social security/disability benefits;		0.065	0	0
(early) pension/student/other non-active/without income		-0.177	0	0
Ethnicity				
native Dutch;		-0.284	0	0
first generation immigrant;		0.249	0	0
second generation immigrant		0.035	0	0
Position in the household				
Couple with children		0.168	0	0
Single parent		0.019	0	0
Number of treated observations/individuals (*N*)	449			
Number of control observations (*N*)	95,270			
Number of control individuals	13,610			
Multivariate L1 distance	0.995			

Source: authors' calculations using administration of housing association merged with Social Statistical Database, Statistics Netherlands. Note: The column 'mean' refers to the mean in the control group substracted from the mean in the treated group, a negative value thus refers to a lower mean in the treated group. The difference in means are especially large when it comes to age and neighbourhood deprivation index, the average in our standardised deprivation index is over one point higher in the treated group. On these variables, the differences in the distributions of the treated and control groups are rather large: the minimum of neighbourhood deprivation differs 5.69 points. This is as expected: the treated individuals live in rather deprived neighbourhoods. When it comes to the differences in distributions of the socio-economic, generation and household categories, there are on average less observations where an individual receiving (early) pension, students and that are other non-active or without income, there are on average less natives and there are more in a couple with children and single parents in the treated group.

ENDNOTES

[1] The housing association initially provided a list with around 40,000 addresses in the wider metropolitan area of Amsterdam that were in their records around 2006. In very close collaboration with the housing association we carefully checked all addresses to make sure they were not part of any other a demolition- or renovation project in the period 2006-2011. Following that strategy, we could be certain that residents that were living on these addresses in 2006 were never forced to move out of that dwelling

because of a urban renewal project and can therefore be assigned to our 'control group'. We also only selected addresses in Amsterdam.

[2] One individual moved from one urban renewal dwelling to another. As it is not clear which move is the treatment, we decided to omit this case. Also, on three urban renewal addresses nobody was registrered at the reference date (start of project as denoted by housing association), also these three cases are deleted and finally, two adresses with more than 70 individuals registrered are also omitted as this is clearly a particular case. In addition, the list of control addresses also contained non-dwellings such as shop-premises, elderly homes and parking garages. We only selected dwellings that are meant for residence for singles or households. The attrition and selection process amounts in 17,918 control addresses.

[3] In Project A and Project B all former residents had moved out in the course of 2009, in Project D the eviction was not completed until 2013, in Project E in 2012 and in Project C the end of 2009.

[4] In the population register we found 2,591 residents registered on our 928 addresses on the date of reference, this includes the other household members registered on that address. The housing association administration is missing the dates/years of births of 72 out of 928 main tenants, 27 moving-in dates and 473 moving-out dates — the latter is mostly Project E.

[5] For project A we could only use year of birth as month of birth was unavailable. Most problematic cases when it comes to matching by month/year of birth are based in Project E where illegal housing situations are rather common; for project E we could only detect about a quarter of the main tenants in the official population registry. For the 544 individuals that are matched on date or year of birth, 480 individuals also had the correct moving-in dates and 396 the correct moving-out dates (In 57 out of 544 cases there was a mismatch of more than 100 days on the move-in date and in 7 cases this information was missing. In 18 cases out of 544 cases there was a mismatch of more than 100 days on the move-out date and in 130 cases this information was missing.)

[6] Unfortunately, this information is missing from the housing association administration for most tenants in Project E. Also, it is important to realise that there can be a time gap between official registration with the municipality and the administration of the housing association. When this time gap is very large we cannot be sure that the person in our population register is the person that the housing association labelled as a forced relocatee. When we could not identify our person in the registry with the date of birth or move-in and move-out date we have decided to drop that case from our dataset.

[7] In 24 cases, the date of birth was not available so using the moving dates was the only available information, in 4 cases the date of birth did not match, but was very close and seemed to be a clerical error. We removed 3 cases where the moving dates in the housing association administration and population registry were close, but the birth dates of main tenants in housing association administration deviated largely from the population registry. It is too risky to identify these individuals as the forced relocatees.

[8] In Project E it was most difficult to detect forced relocatees, for Project A-D we were more successful: on average over 95 percent could be tracked for these projects. We also manually checked whether the neighbourhoods to which forced relocatees moved are the same match in the official population register and in the housing association administration. In about 10 percent of the cases, this did not match up but this could also be due to incorrect information that the housing association received.

[9] Out of the 572 forced relocatees that could be identified, there were 9 forced relocatees that had no record of moving in the population registry and some that have no proper registration in before or after the forced move, resulting in a loss of another 17 individuals. All these cases were checked manually to prevent unnecessary loss in our treatment group (for instance, not being registered in 2005, while the treatment is in 2008 does not have to be a problem so we did not omit this individual). There were also 40 individuals that moved in 2011, which can unfortunately not be followed up as our observation period runs till 2011. Another 56 persons that were omitted are above 65 years old during our observation period and one individual had an outlier on the income variable (above 100.000 euro).

[10] The 449 forced relocatees are all registered at least the year before and after the treatment. Another 6 forced relocatees are not registered a few years after the relocation, we only deal with these missings once we follow them over multiple years.

[11] There were 53 individuals that lived on a control address in 2006 and then moved to an urban renewal project. Obviously, as individuals cannot be included in both the forced relocatees and control group, we excluded these control addresses.

[12] The reference person is chosen as follows, if it is a couple the man is chosen, with a same-sex couple the oldest person, a single-parent household the parent and in other household the oldest adult man or oldest adult woman. This selection yields a group of 16,076 individuals living in on our control addresses on the 1st of January 2006. In almost 500 out of 572 cases, our forced relocatees were also registered as a reference person, so this is a good comparison group.

[13] We only registered addresses and neighbourhoods of residence once a year, on the 31st of December. A forced relocatee could potentially have moved more than once within one year - in that case we only observe the last move within that year (although multiple moves within one year are rare). Also, in some cases, the forced relocatees moved away from the first destination after treatment, this will become apparent in our models as we follow relocatees from at least one year after the treatment.

[14] King and Nielsen (2015) make a strong case against the often-employed propensity score matching (PSM) procedure, which depends heavily on the specification of the model predicting treatment and discards considerable information, making it an inefficient procedure that could actually increase imbalance between comparison and treated groups. CEM is independent from the logit score as employed in PSM and thereby reduces the bias.

[15] Nielsen and Sheffield (2009) refer to this as the "double-dimensionality" of panel data: the panels that are observed over time — in our case individuals that are eventually forced to move and their counterparts — are units of analysis, but each observation that is a panel-time period (in our case person-year) can also be considered as an individual unit of analysis. The observations of the same individuals are related at the different points in time. A solution to deal with the double-dimensionality is to collapse the time-element of the data by averaging on pre-treatment co-variates (Simmons and Hopkins, 2005), compressing the data to only one observation per panel. A possible downside of this approach is that this gets rid of trends over time and dynamic nuances in the pre-treatment period, which might even have led to the treatment. Disregarding these dynamics might in that case lead to biased matches. While in certain fields of study particular trends might lead up to a certain treatment, for instance reduced GDP leading to policy changes or declining economic success of a firm for a enterpreneur to resign, in the case of forced moves the treatment is rather independent from the individual-level trends in socio-economic mobility. Surely, the

individuals that are subject to urban renewal strategies and are forced to move are among the most disadvantaged groups in society, but their individual socio-economic development over time should be considered independently from the urban renewal policy. The relocatees are subjected to treatment because they live in low-quality housing and there is a concentration of poverty in the area, but their individual economic development the years before treatment are not relevant to the treatment assignment, as the urban renewal policies were already in the making many years before the actual demolishment. This approach, however, is not feasible for our data. In our case the treatment, the forced move, occurs at different years for different individuals. As a result, some individuals have more pre-treatment observations than others. Collapsing the time-element of the data by averaging on pre-treatment co-variates over several years as done by Simmons and Hopkins (2005), might be biased as some individuals have four pre-treatment years, and others only one. It is hereby also not clear over how many years we should collapse the covariates of the control group, which we observe for seven years. As explained above, we are also not concerned with diverging time-trends that lead up to the treatment, or endogeneity bias leading up the treatment, as the urban renewal strategy was a rather exogenous decision taken independently from the relocatees.

[16] An example of a study that combines matching on panel data with a DID design is by Becker and Hvide (2016), who investigate the individual contribution of entrepreneurs to firm performance. They use propensity score matching (PSM) to match firms where the entrepreneur dies to similar firms where the founder did not die, and make sure that the matched controls are most comparable in terms of their observable characteristics at the time of start-up and moreover, make the restriction that the treated and control firm are started up in the same calendar year. Their DID analysis proceeds on this matched sample. The authors use pre-treatment characterstics at the year of the firm foundation to match. While Becker and Hvide (2016, p. 18) warn that characteristics measured in the year before the treatment (in their case death of founder) "might already be subject to endogeneity bias because of the foreshadowing of (later) founder death", we believe this to be no or at least much less of an issue in our case. The forced move does not happen overnight, residents are informed about the plans already years before the actual intervention. Even if the upcoming treatment would have reduced earnings before the forced move, as a result of stress of the relocatee leading up to the move, this would be impossible to capture in a specific point in time.

[17] For example, a person for whom we observe the treatment in 2007, that individual is matched to a counterpart with similar characteristics in 2006. For the DID estimation we compare 2006 and 2008 (one year after the treatment).

[18] On average, we obtain the index from about 7,700 neighbourhoods in the Netherlands per year.

[19] Unfortunately, income information on neighbourhoods is not available for 2008. As neighbourhoods change relatively little over time, we took the average of the neighbourhood deprivation indices of 2007 and 2009.

[20] For example, an individual that was on welfare benefits till November and became employed again in December is registered as a welfare recipient.

[21] From the 369 forced relocatees, 197 (53%) lived outside the treatment neighbourhood the year after the treatment. On average, their neighhourhood deprivation after the treatment was a 1.350 point lower, pointing to a clear improvement in the socio-economic composition of their new neighbourhood of residence (the maximum was a 5.030 decrease in neighbourhood deprivation). Only 15 individuals entered a neigh-

bourhood that was more deprived than the neighbourhood from which they had to move (the maximum is a 0.623 increase in deprivation). For the 172 relocatees (47%) that live within the same neighbourhood the year after the treatment, there was also a slight difference in neighbourhood deprivation before and after the forced move. This difference comes from the restructured neighhbourhoods changing slowly over time (both an absolute change and relative to all the other neighbourhood in the Netherlands, but this was only a 0.043 improvement on average). The maximum change was a 0.226 improvement (decline in neighbourhood deprivation), and the maximum increase in neighbourhood deprivation was 0.249. The averages as mentioned in this endnote are only descriptive — they are not corrected for the change in the control group — so the difference-in-difference design provides a superior estimate of the impact of forced relocation on the change in neighbourhood conditions.

[22] For the forced relocatees that did not move beyond the neighbourhood and are living in the treatment neighbourhood, after two years there was on average an 0.284 improvement, after three years on average 0.261 and after four years 0.277 improvement.