Residential self-selection in quasi-experimental and natural experimental studies: an extended conceptualisation of the relationship between the built environment and travel behaviour

Heinen, Eva; van Wee, Bert; Panter, Jenna

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Residential self-selection in quasi-experimental and natural experimental studies: an extended conceptualisation of the relationship between the built environment and travel behaviour

Eva Heinen
Bert van Wee
Jenna Panter

Abstract

Despite a large body of research suggesting that the built environment influences individual travel behaviour, uncertainty remains about the true nature and size of any causal relationships (strength) between the built environment and travel behaviour. Residential self-selection, the phenomenon whereby individuals or households select a residential area based on their transport attitudes, is a frequently proposed alternative explanation for the reported associations. To resolve the issue of residential self-selection, longitudinal studies are often recommended.

In this paper, we argue that intervention study designs are insufficient to fully resolve the problem and that intervention studies on the built environment and travel behaviour may still be biased by residential self-selection. The aim of this paper is to extend existing conceptualisations of the relationship between the built environment, travel behaviour, and attitudes and to provide suggestions for how a causal relationship between the built environment and travel behaviour may be determined with more accurate estimates of effect sizes. We discuss the complexities of determining causal effects in intervention studies with participants who relocate, and the biases that may occur. We illustrate the complexities by presenting extended conceptualisations. Based on these conceptualisations, we provide considerations for future research. We suggest repeating analyses with and without individuals who relocated during the study, and with and without statistical controls for residential relocation. Additional quantitative and qualitative analyses will be necessary to obtain more accurate effect size estimates and a better understanding of the causal relationships.

Keywords: *residential self-selection; built environment; travel behaviour; residential relocation; quasi-experimental studies; natural experimental studies*

1. Introduction

Over decades, transport policies have aimed to contribute to economically prosperous, attractive, healthful, and sustainable cities. In particular, the built environment has received much policy attention as a modifiable driver of travel behaviour, particularly mode choice. Much scientific effort has been invested into determining this relationship, with most studies suggesting that aspects of the built environment influence individual travel behaviour (e.g. Handy et al., 2005; Ewing and Cervero, 2010; Handy et al. 2002; Handy et al., 2002; Sealens et al., 2003). The main weakness of most studies is that they rely on cross-sectional data, which hampers the determination of causal relationships (Van de Coevering et al., 2015).

Causality is an important concept in many fields, but there is no single universally accepted definition, and the notion of causality in human behaviour is not always accepted (Naess, 2016; Parascandola and Weed, 2001). It is outside the scope of this paper to discuss all approaches and philosophies in detail. In the existing discussion of causality of the built environment on travel behaviour, many studies take the probalistic approach, and we therefore will follow Suppes' (1970) probabilistic theory of causality in this paper: '… one event is the cause of another if the appearance of the first event is followed with a high probability by the appearance of the second, and there is no third event that we can us to factor out the possibility relationship between the first and the second events' (Suppes, 1970, p.10). From a more practical perspective, we follow Bradford Hill, who provided a list of viewpoints to be considered when aiming to determine causal relationships between one condition and another (Hill, 1965):

- *strength:* the larger the association, the more likely it is causal, although a small effect size does not exclude this possibility;

• *consistency*: the relationship is repeatedly observed;

• *specificity*: one factor specifically affects one outcome or group;

• *temporality*: the cause precedes the effect;

• *plausibility*: the relationship between cause and effect must make sense in the light of current theories and results; if not, further testing and hypothesising is required;

• (*biological) gradient*: a greater dose of exposure should result in a larger effect;

• *coherence*: the finding should correspond with existing knowledge;

• *experiment*: (if possible) an experiment provides stronger evidence;

• *analogy*: a similar exposure may have a comparable effect, and therefore it is important to consider alternate explanations.

Some of these viewpoints are widely taken into account in existing research on the relationship between the built environment and travel behaviour, such as consistency, plausibility, and coherence. However, most studies do not use an experimental design, sufficiently describe temporal associations, or sufficiently consider alternative explanations. As a result, uncertainty remains about the true nature and size of any causal relationships (strength) between the built environment and travel behaviour.

One particularly important potential alternative explanation for an observed relationship between the built environment and travel behaviour is that of residential self-selection. This can be described as the phenomenon whereby individuals or households select their residential area based on their transport attitudes and preferences (hereafter referred to as attitudes) (Mokhtarian & Cao, 2008; Cao et al., 2009; Naess, 2009). For example, someone with positive attitudes towards travelling by public transport may choose to live in a location with good public transport access. In such a situation, attitudes are an antecedent to both travel behaviour and the residential built environment to which an individual is exposed, which complicates the attribution of a given travel behaviour to that built environment. Cao et al. (2009) have conceptualised the relationships between attitudes, the built environment, and travel behaviour using different causal structures. In this paper, we limit our discussion of attitudes to those relating specifically to transport, which are of direct relevance to the debate about residential self-selection.

To determine a causal effect of the built environment on travel behaviour, it is important to accurately estimate effect sizes for the independent association of the built environment with travel behaviour, following the strength consideration of Hill (1965). Accounting for residential self-selection enables one to improve the estimate of the effect and strengthen the case for causal inferences. For this, the potential confounding by residential self-selection needs to be reduced and many authors recommend longitudinal studies (e.g. Boone-Heinonen et al., 2011; Cao et al., 2009). These may explore the impact of changes in the built environment on changes in travel behaviour in an observational panel study, or—in the spirit of Hill's (1965) 'experimentation' viewpoint—by examining the impact of an intervention in the built environment on changes in travel behaviour in a quasi- or natural experimental study[1] (McCormack and Shiell, 2011; Van de Coevering et al., 2015). For the remainder of this paper, we will use the term 'intervention studies' to refer to quasi-experimental and natural experimental studies as most of our argument applies to both research

[1] It is important to note that whereas cross-sectional studies investigate the correlation between the built environment and travel behaviour, intervention studies generally investigate the association between an intervention (a deliberate change in the built environment) and changes in travel behaviour.

designs. Although intervention studies have the potential to offer stronger causal evidence, they may still be subject to bias and 'steps to rule out competing explanations and biases' are recommended (McCormack and Shiell, 2011, p.9).

In this paper, we argue that intervention studies on the built environment and travel behaviour may still be biased by residential self-selection. This paper focusses on residential self-selection, but it should be noted that similar reasoning could be applied to other forms of self-selection, such as workplace self-selection, and that other forms of bias may also be present, such as attrition bias and selection bias. We focus on residential relocation, which may introduce several threats to causal inference as it introduces the possibility of alternative causal structures. Uncertainty about the causal structure complicates the determination of the independent effect of the built environment on travel behaviour as different structures may require different analytical approaches, and thereby uncertainty on strength may increase, which is one of the viewpoints of Hill (1965) on causality. Therefore, an extended conceptualisation of residential self-selection is needed.

The aim of this paper is to extend existing conceptualisations of the relationship between the built environment, travel behaviour, and attitudes and to provide suggestions for how a causal relationship between the built environment and travel behaviour may be determined with more accurate estimates of effect sizes. Although this is most important for conceptual and methodological development, it has practical importance as it provides planners and decision makers with evidence on causality. Knowledge of the magnitude of the effects of different built environments on travel behaviour in different contexts contributes to more realistic expectations that can be achieved by different interventions (Naess, 2014b). This will assist them in determining the economic and wider societal impacts of future intervention strategies.

2. Background

2.1 Overview of residential self-selection literature and bias

Residential self-selection has received much scientific attention. Most papers have focussed on travel behaviour in general (e.g. Susilo, 2015; Cao, 2014; Scheiner, 2014; Wang and Lin, 2014; Chatman, 2014; Zhang, 2014; Cao and Ettema, 2014; Cao et al., 2010; Scheiner, 2007; Bhat and Eluru, 2009; Pinjari et al., 2009; Cao et al., 2009; Naess, 2009), with others examining active travel in particular (e.g. Cao, 2015; Yu & Zhu, 2015, Schoner and Cao, 2014; Cao et al., 2006), physical activity (e.g. Baar et al., 2015; Van Dyck et al., 2011; Boone-Heinonen et al., 2010, 2011), or other issues including residential choice, transport emissions, and car or bicycle ownership (e.g. He and Zhang, 2014; Bhat et al., 2013; Hong and Shen, 2013; Biying et al., 2012; Chen and Lin, 2011; Pinjari et al., 2008). It is beyond the scope of this paper to discuss all these studies in detail, so in this section we focus on how awareness of potential residential self-selection bias is handled. To date, most studies addressing attitudinal residential self-selection issue have used different (modelling) techniques (Mokhtarian and Cao, 2008; Cao et al., 2009): direct questioning, statistical control, instrumental variables models, sample selection models, joint discrete choice models, structural equations models (SEM), mutually dependent discrete choice models, and longitudinal designs. Both studies recommend the use of longitudinal structural equations modelling with a control group. However, most existing studies are cross-sectional, and control for residential self-selection statistically. More recently, the importance of intervention studies has been acknowledged, over other longitudinal studies.

The main difference between intervention studies and longitudinal studies without an intervention is, as the name suggests, that intervention studies have a specific (or multiple) intervention, whose effect is systematically determined. In contrast to other longitudinal studies, which can only determine the association between changes in the built environment on changes in travel behaviour, intervention studies allow stronger causal inference as the change precedes the change in travel behaviour.

The importance of intervention studies is shown by a systematic review on the relationship between the built environment and physical activity—including walking and cycling—by McCormack and Shiell (2011). They evaluated 33 studies, of which 20 used statistical approaches to control for self-selection and 13 were intervention studies. They concluded that whereas the built environment was significantly associated with physical activity in cross-sectional studies using statistical control for residential self-selection, more rigorous intervention studies provided less support for this relationship and sometimes showed insignificant or even counterintuitive results. For example, proximity to public transport was found to be associated with physical activity in cross-sectional studies such as that of Chatman (2009), which concluded that access to a light, but not a heavy, rail service within 800 meters of home was associated with a higher frequency of waking or cycling. In a quasi-experimental study, however, a new rail stop neither resulted in differences in ridership nor was associated with physical activity (Brown and Werner, 2009). Similarly, the development of a light rail transit corridor was not associated with the achievement of recommended levels of walking and physical activity in local residents (MacDonald et al., 2010). Although the evidence based on this is not conclusive, partly as the locations of these studies differ, this study showed that the studies that allow stronger causal inference showed different results than studies that have more limitations in reducing self-selection bias.

2.2 Existing conceptualisation of residential self-selection in cross-sectional studies

The finding that intervention studies have yielded more mixed evidence than cross-sectional studies, even those that have controlled for residential self-selection, suggests the importance of how residential self-selection is conceptualised[2]. Several papers on residential self-selection are (mostly) conceptual (e.g. Cao, 2015; Van Wee and Boarnet, 2014; Chatman, 2014; Zhang, 2014, Naess, 2014a&b, 2009; Chen and Lin, 2011; Bohte et al., 2009; Mokhtarian and Cao, 2008). Cao et al. (2009) provides a systematic set of conceptualisations, reproduced in Figure 1.[3] In the first conceptualisation (A), attitudes are an antecedent to travel behaviour as well as the built environment. This implies that any measured association between the built environment and travel behaviour is at least partly a result of sharing 'a parent', i.e. both are caused by a shared predictor (attitudes) and they are correlated as a

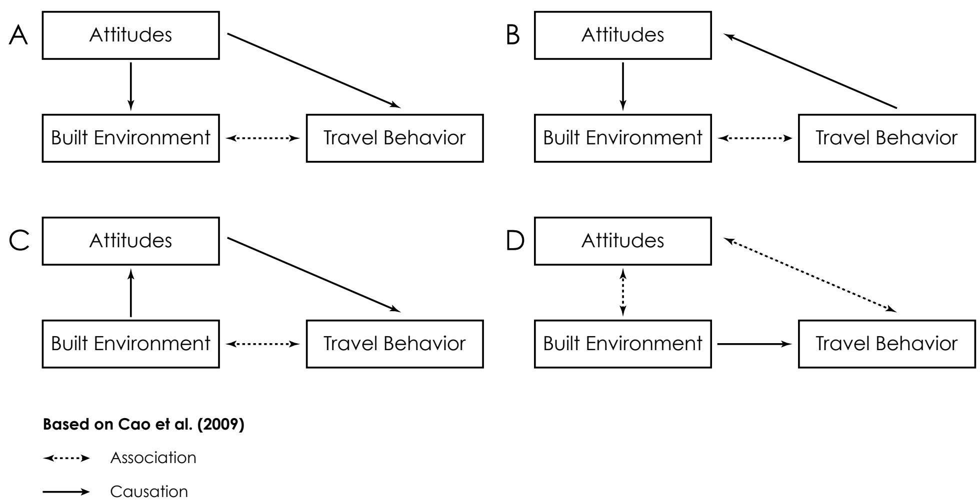
[2] The differences in results between cross-sectional and intervention studies may have several other explanations. One is that cross-sectional studies often use large data sets, whereas intervention studies generally involve smaller sample sizes. Cross-sectional studies are therefore more prone to have (too) much power and a hypothesis may be accepted, whilst not correct in reality, i.e. a type-1 error. Intervention studies may not have enough power to show a statistically significant effect, although present in reality, a type-2 error. Another is that changing the environment does not result in changes in behaviour, despite the fact that cross-sectional studies suggest an association.

[3] Some meanings differ depending on the relationship discussed. For example, in cases where attitudes influence the built environment, the term 'built environment' mainly covers residential choice or choice of built environment aspects. In cases where the built environment influences travel behaviour, the term 'built environment' mainly includes aspects such as density or distance to the central business district (CBD).

consequence. The second conceptualisation (B) shows that travel behaviour influences attitudes which in turn determine the residential location, i.e. built environment. The third conceptualisation (C) illustrates the reverse scenario in which the built environment influences attitudes that in turn affect travel behaviour. Only the fourth conceptualisation (D) shows the built environment influencing travel behaviour directly without attitudes affecting this relationship. Residential self-selection is present in the first conceptualisation (A), in which attitudes are antecedents for both travel behaviour and the built environment.

The conceptualisations of Cao et al. (2009) are the most conceivable, but in theory every line can be drawn in one of three ways (association, causation in one direction, or causation in the other direction) and there are therefore 27 (3^3) possible conceptualisations in total. In addition, bi-directional relationships are also a possibility (Naess, 2014).

Figure 1: Existing conceptualisation of the relationship between the built environment, travel behaviour and attitudes for cross-sectional studies



2.3 Concluding reflections

The continued uncertainty regarding the structure of these causal relationships complicates the determination of causal effects, as different causal structures require different statistical approaches to determine true estimates of effect sizes. For example, whether attitudes should be controlled for by being included as covariates in multivariable models depends on the conceptualisation adopted. If attitudes are 'only' a competing predictor of travel behaviour, adding them to statistical models is recommended because this would increase the predictive power of the model and would not change the effect size estimate for the built environment. However, if attitudes (partly) mediate the effect of the built environment on travel behaviour, although adjustment for attitudes would likewise increase the predictive power of the model it would also introduce bias. The effect size estimate for the built environment would be limited to the direct effect, and would not include the indirect effect now captured by the estimated effect of attitudes. In this situation, we do not recommend statistical adjustment for the effect of attitudes in a regression analysis or similar. Although indirect effects may be captured in the SEM, data collection should be aligned with the conceptualisation, i.e. the cause should precede the mediator, which, in turn, should precede the outcome.

3. Intervention studies

Many authors have come to the conclusion that longitudinal data are essential to determine causal relationships and 'true' effect size estimates (e.g. Boone-Heinonen et al., 2011; Cao et al., 2009). However, in the absence of randomised controlled trials, which in the case of the built environment may be impractical or unethical—for example, we cannot randomly assign individuals to a residential area—the strongest evidence for causal inference will come from quasi- and natural experimental studies (Craig et al., 2012). The main difference between these types of study is that in quasi-experimental studies, the researcher is in control of the intervention, whereas in natural experimental studies, they are not. Although most of our arguments apply to both research designs, in this section, we focus on natural experimental studies as these are more commonly encountered in this field.

3.1 Determining causal effects in natural experimental studies

There are multiple ways of strengthening causal inference in attributing outcomes to interventions in natural experimental studies (Craig et al., 2012). These include:

1. Matching: Comparing exposed and non-exposed groups matched on important characteristics;
2. Regression adjustment: Adjusting for measured differences in characteristics between those who did and did not receive an intervention in multivariable regression models;
3. Propensity scores: Using the likelihood of being exposed to an intervention given a set of covariates to match exposed and non-exposed cases in analysis.

Measured characteristics often vary between intervention and control groups in natural experimental studies and matching is therefore often an unavailable option. The use of propensity scores involves matching individuals with different propensities, but the covariates need to be balanced across treatment and comparison. This method often needs large sample sizes to achieve this, which is not always feasible. We therefore continue our argument with the assumption that studies are most likely to be handled using regression adjustment.

The effects of interventions in the built environment are usually multi-faceted. In the short term, individuals may change their travel choices in respect of certain destinations, the frequency of travel, and the mode and route selected. In the medium- and long-term, residential locations may change, as well as workplace location and the choice of destinations for other activities. We focus on changes in mode choice, because this is often the most important change in travel behaviour from a policy perspective.

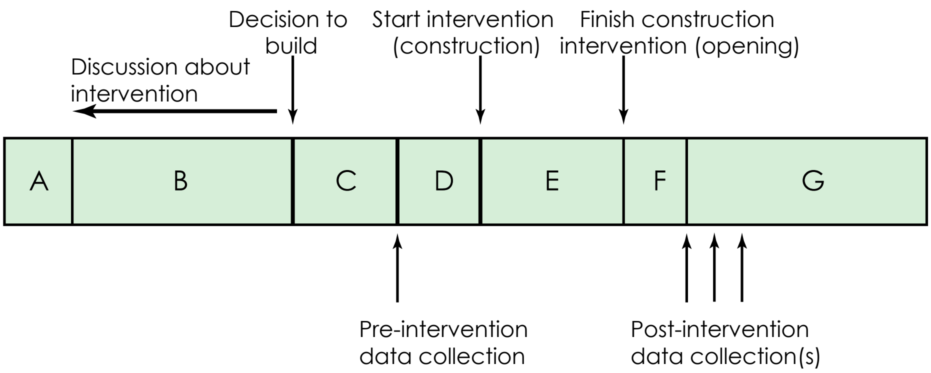
3.2 Timeline of intervention studies

Figure 2 shows a timeline of a typical intervention study. It starts with a 'baseline' situation (A). At a certain moment in time, which is often hard to pinpoint, ideas of an intervention begin to develop[4]. For the purposes of illustration, we focus on an intervention that consists of the construction of new physical transport infrastructure. The period between initial planning and the actual decision to build is period B. In natural experimental studies, the researcher is not in control of the intervention, or even (usually) of the start of the discussion of a possible intervention. Hence, the collection of research data most likely begins after the decision to build, indicated by period C. Data collection before the decision is possible, but unlikely in situations where this would entail significant cost and there is significant risk of the intervention ultimately not being implemented. Period D represents the interval between the start

[4] It should be noted that an intervention may not be random, and is also not necessarily exogenous. In other words, the location of the intervention may be a result of the existing local built environment or residents, or even evoked by its residents. The effect of the intervention may in the latter case be larger than in the entire population. However, for clarity of the argument, the paper assumes that the intervention location is random.

of the study (i.e. collection of 'pre-intervention' data) and the actual start of construction, whereas Period E represents the actual period of construction. It is possible that one or more intermediate waves of data collection may take place during this period. At the end of the construction period, the new infrastructure opens, and after some further interval (Period F), the post-intervention data are collected. Period G denotes the period after the final data have been collected. It is possible that two or more data collections take place after the intervention.

Figure 2: Timeline of an intervention study



4. Residential self-selection in intervention studies

As explained above, intervention studies are often proposed as a solution to the problems of causal inference in general and residential self-selection bias in particular. In this section, we argue that residential self-selection may still introduce bias in intervention studies. Residential relocation of participants is particularly problematic, but other changes, such as destination choices, could also introduce bias. We limit our discussion to residential relocation. We discuss residential relocation in intervention studies, followed by the relationships between the built environment, travel behaviour, and attitudes in these studies, before presenting an extended conceptualisation.

4.1 Residential relocation during intervention studies

Residential relocation may introduce bias in estimates of effect sizes for the relationship between the built environment and travel behaviour and thereby affect the causal inference from a study. Residential relocation may occur at any moment in the timeline of a study (Figure 2). The most obvious 'problematic' periods from a researcher's point of view are periods D, E, and F, as these occur between the pre- and post-intervention data collections. First, relocation in these periods complicates the assignment of a correct measure of exposure to the intervention. Second, although residential relocation may occur for various reasons, including change in employment or household structure, it may also be (partly) driven by attitudes whilst being aware of the likely post-intervention situation. Both considerations complicate the determination of the independent effects of changes in the built environment. Even moving before the intervention may be problematic because residential self-selection may still occur in period C and, to a lesser extent, period B. Some people may relocate in anticipation of the planned or potential changes, despite the uncertainty about whether those will take place or not. These individuals do not move home during the period of data collection, and thus a 'valid' measure of exposure can be determined for them. However, they may be more inclined than others to change their travel behaviour in response to the intervention because their residential location may have been influenced by the extent to which their attitudes are congruent with the built environment after the intervention. Moving in Periods A or G does not direct threaten the validity of a causal estimate, although moving in Period G may indicate a time-lagged intervention effect.

4.2 Moving in intervention studies and the relationships between the built environment, attitudes, and travel behaviour: an extended conceptualisation

This section discusses and extends the conceptualisation of the relationships between the built environment, travel behaviour, and attitudes in relation to residential relocation.

Figure 3 presents the new conceptualisations following from these observations. These extend the work of Cao et al. (2009) and have four central concepts: the outcome (labelled 'change in travel behaviour'), the exposure (labelled 'intervention: change in the built environment'), the alternative explanation of a possible effect (competing exposure) (labelled 'moving/change in built environment'), and 'attitudes'. They could be extended even further, but are limited in this paper to how attitudes may affect causal inference in intervention studies.

In intervention studies, the cause (an intervention in the built environment) precedes the anticipated effect (a change in travel behaviour) and therefore fulfils the requirement of temporality. Intervention studies are also designed (or, at least, analysed) as an experiment in which one or more characteristics of an exposure are purposefully altered to test the effect on the outcome. Potential bias due to residential self-selection is reduced because attitudes are not an antecedent (parent) to both the travel behaviour and the built environment. This assumes that the intervention is not targeted specifically at areas or individuals. However, some interventions may be undertaken in areas in which greater effects are anticipated (sometimes referred to as ‘selection bias’ or ‘confounding by indication’). For example, a new train station is more likely to be opened in an area where existing access to a train station is low and a large uptake of train use is anticipated. Some forms of natural experimental study are especially vulnerable in this respect, i.e. those where the intervention location is selected where most effects are anticipated. The effect of the intervention on changes in travel behaviour can therefore be determined relatively easily for non-movers (Figure 3, conceptualisation 1). For those who have relocated this is more complex, because relocation creates an opportunity to move to an area matching the mover’s attitudes whilst considering the anticipated or actual effects of the intervention.

There are two possible relationships between attitudes and residential relocation. The first is that neither the decision to relocate nor the choice of a new residential area are driven by attitudes. This implies that the new residential location is chosen independently of personal and household attitudes and the relocation is driven by other reasons, e.g. change in household composition, house prices, schools, etc. The second is that the relocation is at least partly driven by attitudes in that individuals decide to relocate to a new location that more closely matches their attitudes. If this relocation occurs at least partly because of the intervention, this is a form of residential self-selection during the study. These relationships and consideration will inform the new conceptualisations.

Conceptualisation 1 visualises the relationship between the built environment and travel behaviour for non-movers in intervention studies. As a result of the study design we can draw stronger causal inference from changes in the built environment to travel behaviour. Conceptualisation 2 captures the idea that attitudes drive the decision to relocate, and the relocation in turn influences travel behaviour.

Conceptualisation 3 captures another causal theory: that attitudes influence the choice of the new location, but the new environment does not result in changes in travel behaviour. For example, someone who holds positive attitudes towards cycling may move to a location with easy access to good cycling facilities, but does not change their mode choice either because they were already travelling by bicycle and continue to do so (but, now, more easily), or because they continue to use other modes of transport because they still do not consider cycling the best option.

In conceptualisations 4 and 5, attitudes do not influence the selection of the new residential location. In conceptualisation 4, there is also no causal relationship between the change in the built environment consequent on relocation and change in travel behaviour, whereas in conceptualisation 5 changes in the built environment consequent on relocation do affect travel behaviour.

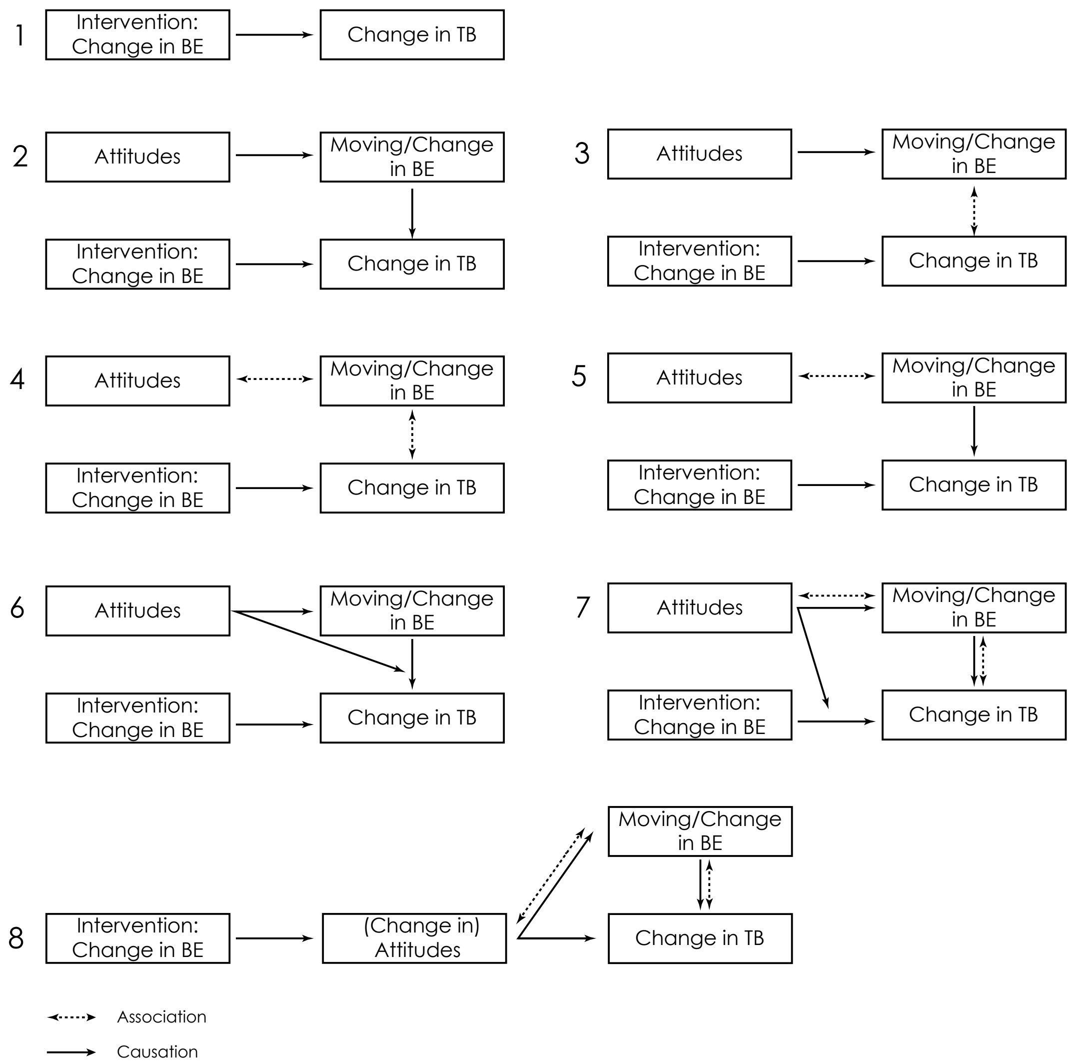
A further possibility is that of an interaction effect between attitudes and changes in the built environment consequent on relocation (conceptualisation 6). In other words, attitudes may strengthen or attenuate the effect on travel behaviour of the built environment change consequent on relocation. This relates to the concept of consonance and dissonance, i.e. the extent to which the built environment corresponds with one's attitudes (Festinger, 1957; Schwanen & Mokhtarian, 2004, 2005a&b, Kamruzzaman et al., 2015; Kamruzzaman et al., 2013a & b; Manaugh & El-Gneidy, 2015). The cited studies suggest that individuals whose attitudes correspond with their residential built environment tend to have different travel patterns from those who are mismatched. It is also possible that attitudes moderate the effect of the *intervention* in the built environment on travel behaviour (conceptualisation 7) through the same phenomena of consonance and dissonance.

Finally, (a change in) attitudes may mediate the effect of the change in the built environment on travel behaviour (conceptualisation 8). In this case, attitudes are a mediator of the intervention effect, and may or may not also be an antecedent of the competing exposure of relocation. In conceptualisations 7 and 8, the solid and dashed lines indicate that those relationships could be either causal or associative, respectively. The conceptualisations could be expanded to show all variations.

Two other considerations should be borne in mind. First, a change in the built environment due to residential relocation may or may not result in a change in travel behaviour. If moving is driven by attitudes (regarding transport), this can be seen as self-selection, but there is only a potential self-selection bias if this relocation also affects travel behaviour. Second, the effects of relocation on travel behaviour may not be restricted to those evoked by changes in the built environment. Moving, as a major life event in its own right or as a proxy of another life event such as childbirth, may evoke a reconsideration of travel options and changes in travel behaviour.

Conceptualisations 2, 3, 6, 7, and 8 are vulnerable to residential self-selection, and in conceptualisations 2, 6, 7, and 8, this potential residential self-selection may result in a residential self-selection bias.

*Figure 3: Conceptualisation of the relationship between the built environment, travel behaviour and attitudes in quasi- and natural experimental studies**



* In conceptualisations 7 and 8, the solid and dashed line indicated that those relationships may be either causal or associative, i.e. the number of conceptualisations could have been expanded to show this.

4.3 Concluding reflections

These conceptualisations depict different possible causal pathways.

Attitudes may act as an antecedent to a competing exposure (moving) (see conceptualisations 2, 3, 6, 7, and 8), a mediator of the intervention effect ((change in) attitudes) (see conceptualisation 8), a moderator of the intervention effect (see conceptualisation 7), a moderator of a competing exposure (see conceptualisation 6), or a combination of these functions. The uncertainty surrounding the causal structure complicates the determination of causal effects, as different causal structures may require different statistical approaches. For example, we wish to control for competing exposures (or proxies of competing exposures), but not for mediators.

5. Considerations and suggestions for future research

Longitudinal studies are often proposed as an effective method of limiting potential bias from residential self-selection. However, as the conceptualisations in Figure 3 show, residential self-selection may still be present in intervention studies.

The uncertainty about the causal structure complicates the determination of causal effects, as the different causal structures require different statistical approaches. In order to improve analyses of the impacts of interventions on travel behaviour, we suggest the following considerations.

Research design:

- Collect data on residential relocation before the intervention;
- Collect additional data, e.g. repeated cross-sectional recruitment, panel data, additional recruitment during later waves of data collection, and additional qualitative studies;
- Disentangle the causal relationships further using qualitative methods;

Statistical analysis:

- Repeat analyses with and without the inclusion of movers;
- Repeat analyses by controlling and not controlling for residential relocation.

We elaborate on these below. Although some of these suggestions may seem obvious, they are not widely and carefully considered.

5.1 Research design considerations

5.1.1 Collect data on residential relocation before the intervention

The moment of residential relocation has implications on its potential impact on the study. Moving between the two waves of data collection (Periods D, E, and F in Figure 2) directly threatens the validity of the effect size estimates and the possibility of determining a causal relationship. Minimising the durations of Periods C, D, and F may reduce the number of participants who relocate during the study and thereby limit this threat. We recommend keeping Period D as short as possible. Period C would ideally also be as short as possible to ensure that pre-intervention data collection truly represents a baseline and does not capture any anticipatory behaviour change. However, the optimal length of Period F is more difficult to pinpoint, as a shorter Period F also reduces the opportunity for participants to change their behaviour and consequently to detect an intervention effect. Therefore, multiple post-intervention measures are optimal, despite greater costs in time and money.

Although moving outside the period of data collection may, at a glance, appear to have little impact, it may still affect the results. For example, moving in Periods B or C may be a result of residential self-selection as individuals may act in anticipation of the intervention. If such relocation is (partly) driven by attitudes (such as in conceptualisations 2, 3, 6, 7, and 8), then those who have recently relocated to the intervention area may be more likely to adjust their travel behaviour as a result of the intervention than existing residents. This may pose threats to the accuracy of the effect size estimates. We therefore suggest collecting information on the date on which participants moved to their current residence and when they made the decision to move, as well as the location of their former residence, as some relocations may have been made over a only short distance for practical reasons, e.g. change in family size, whilst remaining in the same neighbourhood. This would allow researchers to compare residents who have recently moved into the area with longer-term residents controlling for potential confounding factors.

5.1.2 Collect additional data

Intervention studies generally recruit participants before the intervention is completed, and individuals are often assigned to exposed/intervention and control groups. The surface of exposed areas is often smaller than the surface of the control areas, which may also correspond with a larger population in the control area. The recruitment strategy may specifically target the exposed area to secure a large enough sample size, but moving may cause problems. Even if we assume that moving occurs at similar rates in the intervention and control areas (this is not a given because the housing tenure may differ in the areas, and it also assumes the construction of the intervention is not too burdensome), but given the differences in the size of the areas it is more likely that a given participant—independent of where they lived at the time of recruitment—will relocate into the control area than into the exposed area. Exposed and control areas could be similar in size. However, on a regional/national/global level, the exposed area is always smaller than the control area. Consequently, the exposed group is more likely to ‘lose members’ over time.

As a consequence, cohort (panel) data will capture moving into the exposed area to only a limited extent. This presents an interesting situation. The intervention is likely to attract 'lovers' (individuals whose attitudes match the intervention) and repel 'haters' (individuals whose attitudes do not correspond with the intervention). In a cohort without additional recruitment after the pre-intervention data collection, and with a larger control area than an exposed area, the sample will mostly include 'moving lovers' who moved in Periods B and C into the exposed area, and 'moving haters' who moved to the control area in Periods B–F. This may result in an underestimation of the effects of the intervention.

We therefore suggest additional data collection to ascertain that 'lovers' are included in studies corresponding with their prevalence in the population. One option is a repeated cross-sectional data collection to monitor whether the average characteristics and behaviour in the intervention area (and possibly the control area) change over time. A second option is additional recruitment, especially in the exposed area, during later waves of data collection. This may allow a case-control study comparing individuals who have relocated into the exposed area with those who lived in the area before the intervention, to determine whether 'lovers' have moved into the area. A third option is the use of qualitative research to understand the behaviour of 'lovers' and 'haters' of the intervention.

5.1.3 Disentangle the causal relationships further using qualitative methods

Whereas the determination of correct effect size estimates requires a quasi- or natural experimental quantitative approach, complementary qualitative research can help to further disentangle the relationships between moving, attitudes, the built environment, and travel behaviour. This will contribute to greater certainty about the nature of the relationships, which, in turn, will help in selecting the appropriate statistical approach.

It may also show other relationships between the constructs, which may inform future quantitative research.

A qualitative study of movers (see 5.1.2.) may also be useful to more fully capture the intervention effects, including medium- and long-term impacts, which will contribute to fully understanding the intervention effects which will contribute to disentangling the causal relationships and inform future research designs and statistical analyses.

5.2 Statistical considerations

5.2.1 Repeat analyses by controlling and not controlling for residential relocation

At first sight, an apparently simple solution may be to exclude individuals who relocated during the study and perform the analyses on non-movers alone and thereby eliminate conceptualisations 2, 3, 4, 5, 6, 7, and 8, and work with conceptualisation 1 alone. Indeed, the exclusion of individuals who have relocated removes most potential residential self-selection. However, this approach ignores the fact that residential relocation is generally not equally distributed over the (study) population. Relocation is restricted by micro-level restrictions (such as income) (Van Ham and Feijten, 2008) and may be more prevalent in younger people and those without a permanent job. This implies that excluding movers may reduce the likelihood of certain groups being included in the study, and therefore introduce a form of selection bias and a subtle change in the research question the study is addressing. It follows that including and excluding individuals who have relocated during the study may result in different inferences, with neither providing an accurate estimate of the true effect size for the intervention. We therefore suggest repeating analyses with and without individuals who relocated during the study. It is important to note that neither model is necessarily superior—as one may suffer from a residential self-selection bias, and the other from a recruitment bias—but they are complementary and together may strengthen the case for causal inference. If the two estimates are very different and the confidence intervals do not overlap, it is likely that either residential self-selection or another selection bias occurred.

5.2.2 Repeat analyses with and without controlling for residential relocation

Residential relocation may be a competing exposure (potential cause) for the outcome of a change in travel behaviour (conceptualisations 2, 5, and 6, and also conceptualisations 7 and 8). As such, we recommend statistical adjustment for residential relocation, by adding a covariate that either indicates whether a person has relocated or serves as a proxy for this (such as a variable that capture changes in the built environment consequent on relocation).

However, not all conceptualisations support adding a variable representing moving into statistical models. Conceptualisation 7 in Figure 3 illustrates that attitudes may be, in addition to being a predictor for moving, i.e. a competing exposure, a predictor of change in attitudes, i.e. a mediator. In this case, controlling for moving may confound the relationship between the intervention and changes in travel behaviour, and bias effect size estimates.

We therefore suggest estimating models with and without adjustment for moving, on a similar basis to the argument for estimating models with and without the inclusion of participants who have moved, as neither model is necessarily superior.

6. Conclusion

This paper has argued that intervention studies are not sufficient to control for the potential bias of residential self-selection in estimating the travel behaviour impacts of changes in the built environment. By extending the existing conceptualisation, we have shown how residential self-selection may still pose threats in quasi- and natural experimental studies. The conceptualisation presented may help in clarifying the causal relationships involved and selecting the most appropriate statistical methods for analysis.

We acknowledge that residential self-selection may only introduce one of the potential biases that need to be reduced, and reducing attrition bias and selection bias are at least as important. Nevertheless, to reduce the self-selection bias, we suggest repeating analyses with and without the inclusion of individuals who have relocated during the study, and with and without statistical control for relocation. Additional quantitative and qualitative data may be necessary to obtain more accurate effect size estimates and understanding of the causal structures. Some of these suggestions may seem trivial, but a more careful consideration of the potential conceptualisations is essential to improve our understanding of the determinants of travel behaviour. This will ultimately contribute to more accurate estimates of the quantifiable effects of interventions in the built environment, and thereby to more robust planning and policymaking.

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