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Examining the impacts of residential self-selection on travel behavior: A focus on methodologies

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Abstract

Numerous studies have found that suburban residents drive more and walk less than residents in traditional neighborhoods. What is less well understood is the extent to which the observed patterns of travel behavior can be attributed to the residential built environment itself, as opposed to the prior self-selection of residents into a built environment that is consistent with their predispositions toward certain travel modes and land use configurations. To date, most studies addressing this attitudinal self-selection issue fall into seven categories: direct questioning, statistical control, instrumental variables models, sample selection models, joint discrete choice models, structural equations models, and longitudinal designs. This paper reviews and evaluates these alternative approaches with respect to this particular application (a companion paper focuses on the empirical findings of 28 studies using these approaches). We identify some advantages and disadvantages of each approach, and note the difficulties in actually quantifying the absolute and/or relative extent of the true influence of the built environment on travel behavior. Although time and resource limitations are recognized, we recommend usage of longitudinal structural equations modeling with control groups, a design which is strong with respect to all causality requisites.

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1. Introduction

Numerous studies have observed that residents of higher-density, mixed-use (“traditional”, “neo-traditional”, or “new urbanist”) neighborhoods tend to walk more and drive less than do inhabitants of lower-density, single-use residential (“suburban”) areas (e.g., Cervero and Duncan, 2003; Crane and Crepeau, 1998; Frank et al., 2006). What is less well understood is the extent to which the observed patterns of travel behavior can be attributed to the residential built environment itself, as opposed to the prior self-selection of residents

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into a built environment that is consistent with their predispositions toward certain travel modes and land use configurations. For example, residents who prefer walking may consciously choose to live in neighborhoods conducive to walking, and thus walk more (as found by Handy and Clifton, 2001). Therefore, the observed differences in pedestrian behavior in those two types of neighborhoods may be more a matter of residential choice than travel choice. In other words, residential self-selection may be at work. If so, we are likely to overestimate the influence of built environment elements on travel behavior when we use land use policies to try to reduce travel, fuel consumption, and emissions (e.g., Kitamura et al., 1997).

In the past few years, this complex issue has been addressed in a variety of ways. This paper describes and critiques the various methodological approaches adopted to date to assess the causal impact of the built environment on travel behavior. A companion paper (Cao et al., submitted for publication) focuses more heavily on the empirical studies employing each approach, describing and evaluating their key findings. A companion report (Cao et al., 2006) contains the essential content of both papers, plus considerable additional detail that could not be incorporated into journal-length articles. In particular, it includes a table summarizing the 28 empirical studies reviewed for this work.

The organization of this paper is as follows: Section 2 describes the self-selection problem, reviewing the prerequisites of causality inference and placing the issue in the context of the built environment and travel behavior. Section 3 analyzes the various methodologies that have been used to address this issue, while Section 4 discusses numerous ways of posing the research question(s) of interest, and highlights the difficulties in actually quantifying the absolute and/or relative extent of the true influence of the built environment on travel behavior. The last section summarizes the review and makes some recommendations for future research.

2. The self-selection problem

As indicated above, previous studies have consistently found a significant association between the built environment and travel behavior. However, association itself is insufficient to establish causality. To *robustly* infer causality, scientific research generally requires at least four kinds of evidence (Schutt, 2004; Singleton and Straits, 2005; for a more extensive discussion of these in this context, see Cao et al., 2006): *association* (a statistically significant relationship), *nonspuriousness* (a relationship that cannot be attributed to another variable), *time precedence* (cause precedes effect), and *causal mechanism* (a logical explanation for why the alleged cause should produce the observed effect).

In attempting to ascertain the extent to which the built environment (BE) *causes* travel behavior (TB), therefore, the goal is to use a methodology that is as robust as circumstances will permit with respect to these types of evidence. It is particularly important to ensure that an observed association between BE and TB is not the spurious result of the fact that unmeasured variables (such as attitudes) are causing both. As shown in Fig. 1, there are in fact a number of plausible relationships among attitudes (AT), BE, and TB, and the chosen methodology will ideally be capable of distinguishing among the various possibilities.

Self-selection in this context refers to “the tendency of people to choose locations based on their travel abilities, needs and preferences” (Litman, 2005 p. 6). Residential self-selection generally results from two sources: attitudes and sociodemographic traits. With respect to the latter, an example of self-selection is that low-income and zero-vehicle households may choose to live in neighborhoods with ample transit service and hence use transit more. In this case, it is not good transit facilities but households’ economic constraints that have a true and direct influence on their choice of transit mode. However, since most previous studies have employed multivariate analysis and accounted for the sorting effect of sociodemographic characteristics (e.g., Abreu e Silva et al., 2006; Kitamura et al., 2001), we focus this review on the issue of attitude-induced self-selection. Unless explicitly indicated, residential self-selection in the remainder of the paper refers only to that resulting from attitudinal factors.

In simple mathematical terms, the often-observed relationship between the built environment and travel behavior is generally modeled as taking the form:

$$TB = f_1(BE, X) + \varepsilon, \quad (1)$$

where X denotes other observed variables such as sociodemographics, and ε represents the collective influence on TB of all unobserved variables. The problem is that the standard estimation of such functional forms,

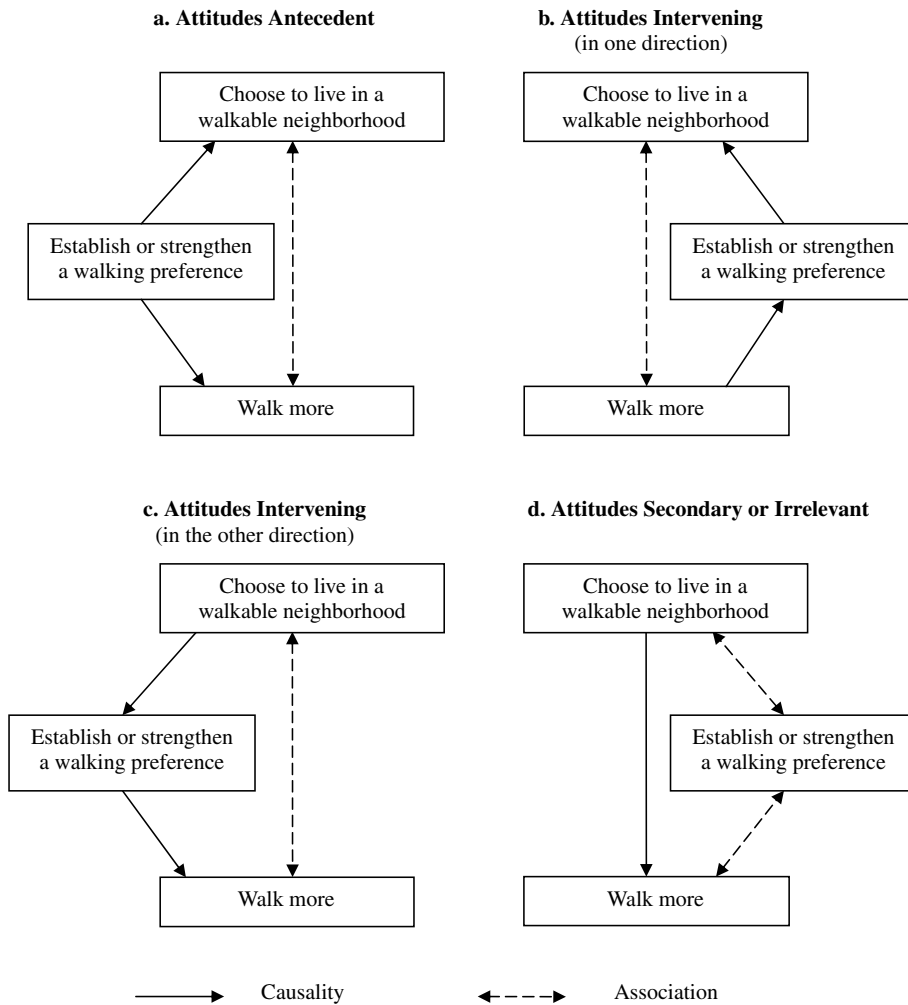


Fig. 1. Some potential relationships among travel attitudes, built environment, and travel behavior.

whether the dependent variable is continuous and observed (as in linear regression models) or representing a discrete choice (as in logit or probit models), requires that observed explanatory variables (BE, X) be uncorrelated with unobserved explanatory variables (ε). Failure to meet this important condition is broadly referred to as *endogeneity bias*, and produces coefficients for BE and X that are biased and inconsistent estimators of the true values. Furthermore, the conventionally-estimated standard errors of the estimated coefficients will also be biased, which renders invalid the usual hypothesis-testing on the significance of variables (Ramathan, 2002).

Endogeneity bias can occur in two conceptually distinct ways, either of which could arise in our context. *Simultaneity bias* is produced when an “explanatory” variable is simultaneously a function of the “dependent” variable it is supposed to explain – that is, when one variable is both a cause and an effect of another. In the present context, this would mean:

$$\begin{aligned}
 TB &= f_1(BE, X, Y) + \varepsilon_1, \\
 BE &= f_2(TB, X, Z) + \varepsilon_2,
 \end{aligned}
 \tag{2}$$

where X denotes observed explanatory variables common to both TB and BE , and Y and Z denote observed variables distinctive to TB and BE , respectively. In this formulation, travel behavior is assumed to exert a direct influence on residential choice and thus the built environment (as well as the more conventionally assumed

converse direction of causality, from BE to TB), separate from the influence of attitudes. This could occur if travel behavior were largely determined by constraints such as income (X) – e.g. making it impractical to own a car – and then residential location were influenced by the resulting travel behavior, e.g. a reliance on public transportation (as well as separately by income also). In models such as these, it is easy to see that BE is likely to be correlated with ε_1 , because of its correlation (through f_2) with

$$TB = f_1 + \varepsilon_1.$$

The second type of endogeneity bias is *omitted variables bias*. This occurs whenever observed and unobserved explanatory variables are directly correlated, either because one causes the other or because both are functions of the same antecedent variables. The most frequently discussed form of the residential self-selection problem is of this type, and can be expressed as:

$$TB = f_1(\text{BE}(\text{AT}), X) + \varepsilon(\text{AT}), \quad (3)$$

in which the attitude (AT) portion of ε partly explains or causes BE. However, as illustrated by Fig. 1c, the opposite direction of causality between BE and AT is also plausible:

$$TB = f_1(\text{BE}, X) + \varepsilon(\text{AT}(\text{BE})), \quad (4)$$

in which travel attitudes are influenced by the built environment.

3. Methodologies for treating the self-selection problem

A number of methodological approaches have been applied to test and control for these endogeneity biases in previous studies; we discuss seven such approaches in this section. Generic forms of the statistical control, instrumental variables, sample selection, and longitudinal approaches are discussed in the excellent review article by [Winship and Morgan \(1999\)](#), a highly recommended gateway into the more complex econometric literature on the estimation of causal effects in the presence of selection bias. An archetype of the latter is [Heckman and Vytlacil \(2005\)](#). [Bhat and Guo \(2007\)](#) provide a useful discussion of the statistical control, instrumental variables, and longitudinal approaches in the specific context of residential self-selection.

[Table 1](#) offers a brief overview of each approach discussed below, including a window into the empirical findings discussed more fully in the companion references cited in the Introduction. In the present paper, the details of empirical studies are omitted for space reasons, with the exception of the [Salon \(2006\)](#) study in Section 3.5.1. Since it constituted a unique (to date) application of a particular technique to this context (specifically, the use of nested logit elasticities to quantify the self-selection effect), it seemed more appropriate to discuss it in specific terms rather than in the purely generic terms used with the other, more often applied, methods.

3.1. Direct questioning

To assess whether people's travel and land use predispositions influenced their choice of residential neighborhood, why not just ask them? Although this approach may appear primitive next to more complex quantitative approaches, it requires considerable ingenuity to execute well. Done well, it can provide very useful insights, sometimes beyond what multivariate analyses can do. It can also be used effectively in conjunction with quantitative approaches, for example in the development of survey instruments, the identification of appropriate model specifications and/or market segments having different decision-making processes, and the validation of multivariate analyses ([Clifton and Handy, 2003](#); [Pendyala, 1998](#)). Nevertheless, used on its own it has several limitations. To begin with, the sample size is generally small and may not be representative of the population of interest. Moreover, direct questioning is likely to suffer from a number of biases, including:

- *memory*: For even very recent moves and certainly for longer-ago ones, attempts to recall one's attitudes and preferences prior to the move will be unreliable, as those beliefs are likely to be altered by the realities of the new residential environment and other intervening events and changes;

Table 1
Overview of methodologies for dealing with self-selection

Method	Special Data Requirements	Causal Inference Capacity	Limitations	Example Application
<i>Direct questioning</i>	Focus group, personal interviews	Can offer strong qualitative evidence, sometimes beyond what quantitative approaches can do.	<ul style="list-style-type: none"> a. Uses small samples, which may not be representative; b. Is more vulnerable to some biases including memory, consistency, saliency, and social desirability; c. Is unable to quantify the influence of BE. 	Handy and Clifton (2001) found that individuals who prefer walking chose to live in a neighborhood that facilitates walking, and also walk more.
<i>Statistical control</i>	Explicit measures of as complete as possible a set of attitudes toward residential and travel choices.	Association: strong Nonspuriousness: strong Time precedence: weak ^a Causal mechanism: yes	<ul style="list-style-type: none"> a. Attitudes are not straightforward to measure and analyze; b. Temporal mismatch can be a concern for cross-sectional data; c. Considers only a single causal direction. 	Kitamura et al. (1997) found that although neighborhood characteristics have a separate influence on travel behavior, attitudes explain it better than neighborhood characteristics. Schwanen and Mokhtarian (2005) found that the built environment has a stronger influence on the distance traveled of dissonant suburban dwellers than on that of dissonant urban residents.
<i>Instrumental variables models</i>	Data for instrumental variables, which are highly correlated with the built environment but uncorrelated with the error term of travel behavior.	Association: strong Nonspuriousness: moderate ^b Time precedence: weak ^a Causal mechanism: yes	<ul style="list-style-type: none"> a. Is difficult to find suitable instruments; b. Weak instruments may fail to reject the null hypothesis; c. Weak instruments can yield biased and inconsistent estimates; d. Is unable to quantify the relative influence of BE and RSS; e. Only the BE → TB direction of causality is modeled. 	After performing instrumental variable regressions, Greenwald and Boarnet (2001) found that the built environment influences the generation of nonwork walking trips.
<i>Sample selection models</i>	Discrete types of residential location such as urban vs. suburban.	Association: strong Nonspuriousness: moderate ^c Time precedence: weak Causal mechanism: yes	<ul style="list-style-type: none"> a. Discrete residential location is a simplistic representation of complex residential choices; b. Only the BE → TB direction of causality is modeled. 	No study was identified that exactly fit this model. ^g

<i>Joint discrete choice models</i>	Discrete measures for both residential location and travel behavior.	Association: strong Nonspuriousness: weak to moderate ^d Time precedence: weak Causal mechanism: yes	a. Residential choice and travel choice do not directly influence each other; b. The direction(s) of causality between residential choice and travel behavior cannot be statistically tested.	Salon (2006) concluded that self-selection accounted for 1/3 to 1/2 of the effect of a change in population density on walking level.
<i>Cross-sectional structural equations models</i>	Explicit measures of as many relevant attitudes as possible. All endogenous variables must be continuous, binary, or ordinal.	Association: strong Nonspuriousness: strong Time precedence: moderate ^e Causal mechanism: yes	a. Attitudes are not straightforward to measure and analyze; b. Temporal mismatch among variables can be a concern; c. Dynamic processes must be stable and at equilibrium; d. Identifiability requirements may impose conceptually undesirable constraints; e. Alternate model structures may fit the data about equally well; f. Cannot treat multinomial endogenous variables.	Bagley and Mokhtarian (2002) found that attitudinal and lifestyle variables had the greatest impact on travel demand, while residential location type had little separate influence on travel behavior.
<i>Longitudinal models – single equation</i>	Measurements of variables before and after treatments (or the change in variables).	Association: strong Nonspuriousness: moderate ^f Time precedence: moderate ^f Causal mechanism: yes	a. It is expensive and time-consuming; b. Neither the treatment nor its assignment to a subsample is completely random; c. It can be difficult to determine the optimal times of measurements; d. Processes generally should be stable; e. Attitudes are not straightforward to measure and analyze.	Krizek (2003a) found that a change in neighborhood accessibility influences a change in travel behavior.

(continued on next page)

Table 1 (continued)

Method	Special Data Requirements	Causal Inference Capacity	Limitations	Example Application
<i>Longitudinal models – structural equations</i>	Same as above.	Association: strong Nonspuriousness: very strong Time precedence: very strong Causal mechanism: yes	Same as for single-equation longitudinal models, plus f. Cannot treat multinomial endogenous variables.	Using a quasi-longitudinal approach, Cao et al. (2007) found that although self-selection matters, changes in built environment elements have separate influences on changes in travel behaviors.

Notes: Except direct questioning, all approaches require measurements of the built environment and travel behavior.

^a The statistical control and instrumental variables approaches implicitly assume that attitudes are a cause rather than an effect of residential choice and travel behavior, which is open to debate.

^b Reflects the limited ability of the IV approach to find instruments for BE that are both uncorrelated with ε and explain BE well enough to be useful.

^c Depending on how well RC is modeled.

^d The nested logit model is relatively weak with respect to nonspuriousness. The joint simultaneous discrete choice approach is an improvement over the nested logit approach in that it parameterizes the error terms of the RC and TB equations as functions of BE (and potentially other observed variables), thus reducing the potential for their correlations to be due to a spurious third-party variable.

^e Even with cross-sectional data, a structural equations model can provide some evidence for the direction of influence if attitudes are explicitly controlled for.

^f Strong if attitudes are measured at each wave; otherwise, a Δ BE that precedes a Δ TB may itself be preceded and caused by a Δ AT.

^g However, a very recent article (Vance and Hedel, 2007) applies a related technique, the two-part model (2PM), to control for the endogeneity of urban form measures in a model of

- *consistency*: If we ask about participants' behavior first, they may later (consciously or subconsciously) express attitudes to be consistent with that behavior;
- *saliency (recency)*: Participants will tend to focus on the aspects of the situation to which their attention is drawn, and thus their responses will be very much flavored by the specific content and tone of the interview questions; and
- *social desirability*: As the conversation goes along, participants may anticipate the objective of the study and hence conform their expressed attitudes and choices either to what they think the researcher wants to hear, or to established social norms.

Of course, these biases are also possible with the design of the self-administered questionnaires from which the data for quantitative analyses are often collected. However, some scholars (e.g. Dillman, 1978) suggest that all else equal, the extent of at least the latter three biases could be more severe in the case of direct questioning, where the body language and tone of the interviewer can offer additional cues to the participants, and where (even in the case of a prepared script or set of questions) the interviewer generally has a certain amount of discretion over the spontaneous "sidetracks" that the interview might take.

Equally importantly, direct questioning does not allow us to quantify the respective influences of the built environment and residential self-selection, and determine which is more important. In addition, this approach is vulnerable to most of the limitations discussed in the following sections.

3.2. Statistical control

The method of statistical control explicitly accounts for the influences of attitudinal factors in analyzing travel behavior, by measuring them and including them in the TB equation (thereby moving them from unobserved to observed). This approach has been operationalized in two different ways in the literature, one incorporating attitudes directly (e.g. Kitamura et al., 1997), and the other incorporating an attitudinal-based measure of dissonance between one's preferred and actual neighborhood types (e.g. Schwanen and Mokhtarian, 2005).

In the first case, TB is simply modeled as a function of AT as well as BE:

$$TB = f_3(BE, AT, X) + \xi, \quad (5)$$

which removes AT from the ε of Eqs. (3) and (4), and thereby presumably eliminates any correlation between BE and ξ . If the inclusion of AT drives the influence of BE into insignificance, the natural conclusion is that the influence of BE was entirely due to predispositional attitudes. If BE is still significant, the conclusion is that the BE exerts some influence of its own, separate from the predisposition that led an individual to locate there in the first place.

In the second case, in addition to incorporating travel-related attitudes into the equation for travel behavior, attitudes toward residential location type are used to classify survey respondents as consonant (well-matched) or dissonant (poorly matched) with respect to their current residential location. The travel behavior of dissonant residents is then compared to that of consonant residents in the type of neighborhood in which they would rather live, and in their current neighborhood. If the travel behavior of dissonant residents is more similar to that of the consonant residents in their *desired* type of neighborhood, it suggests that their predispositions dominate their travel behavior. If their travel behavior is more similar to that of the consonant residents in their *current* neighborhood, it suggests that the built environment exerts a separate influence that outweighs a contrary predisposition. Alternatively, a continuous measure of the degree of dissonance, as well as measures of the built environment, can be incorporated into the travel behavior equation, and tests performed to see whether the built environment remains significant after dissonance is accounted for.

Although the statistical control approach can offer insightful evidence of residential self-selection, it is vulnerable to several intrinsic limitations. First, attitudes are not straightforward to measure and analyze, and are often not measured, e.g. not available in standard travel/activity diary data sets, and hence pose significant difficulty in the context of regionwide travel demand forecasting. Even when they *are* measured, they are measured with error, and may not comprehensively capture all the relevant attitudes. Second, when data are cross-sectional, there can be a temporal mismatch: the attitudes measured in the present may differ from those

leading to the prior choice of the built environment. Third, these studies model only a single causal direction, from the built environment to travel behavior. As illustrated in Fig. 1, this is too simplistic a representation of the potential interactions among these variables.

3.3. Instrumental variables models

Another approach to address residential self-selection is to use instrumental variables (IVs) to purge BE of its correlation with ε . A time-honored econometric technique, it involves (as applied in this context) first modeling BE as a function of relevant instrumental variables (or “instruments”), z , that are not correlated with ε , and then replacing the observed BE in Eq. (1) with its predicted value \widehat{BE} from that model:

$$\begin{aligned} BE &= b(z) + \eta(AT), \\ TB &= f_4(\widehat{BE}, X) + \varepsilon(AT), \end{aligned} \quad (6)$$

where $\widehat{BE} = \hat{b}(z)$. The predicted \widehat{BE} will then, by construction, be uncorrelated with ε . The implication is that the entire influence of AT on TB will lie in ε ; if \widehat{BE} is significant in the equation for TB, it represents an influence of the BE that is purged of the self-selection attitudinal component. Thus, the statistical control and IV methods represent opposite strategies in dealing with the endogeneity problem (Winship and Morgan, 1999): whereas the object of the former method is to identify variables that are *maximally* correlated with ε to use as controls, the object of the latter method is to find variables that are *minimally* correlated with ε to use as instruments.

The intrinsic limitations of the IV technique are well-recognized in the literature (as stated by Winship and Morgan, 1999 p. 683, “the perfect instrument [is] an apparent contradiction”). Generally, instrumental variables should satisfy two criteria: they must be highly correlated with the endogenous explanatory variable (BE) they are predicting (“relevance”), but not be significantly correlated with the error term (ε) of the original equation (“exogeneity”; Cameron and Trivedi, 2005; Hall et al., 1996). The problem is that BE must be *substantially* correlated with ε in order for endogeneity bias to be a problem; small correlations between observed and unobserved variables are tolerated all the time, without remedial measures being required or taken. But in that case, first of all, finding suitably uncorrelated variables with which to model BE in the first place (i.e., meeting the exogeneity criterion) can be difficult. Second, modeling BE as a function of variables *uncorrelated* with ε will therefore necessarily leave a sizable portion of the variance in BE unexplained (thereby falling short on the relevance criterion).

The problem of low relevance or “weak instruments”² occurs quite often with this technique, and has a number of (related) potential deleterious consequences:

- (1) The standard error of the coefficient of \widehat{BE} in Eq. (6) is likely to be high (Bound et al., 1995; Shea, 1997), reflecting the imprecision with which the true effect of BE on TB is being captured. In that case, finding \widehat{BE} to be insignificant may not reflect a true lack of influence after controlling for self-selection, but rather the inability of the poor \widehat{BE} to capture that influence.

² There does not appear to be a consensus in the literature on the definition of “weak”. However, in a linear regression context, the relevant indicators are the R^2 of the first-stage equation estimating BE, and the F -test for the significance of the block of instruments in the same equation. In the special case of one instrument (z) and one regressor (BE), Nelson and Startz (1990) indicate that $\{\text{sample size}\} \times R^2 \ll 2$ is problematic, and Shea (1997) provides a partial- R^2 -based test for the case of multiple instruments and multiple regressors. Bound et al. (1995, p. 446) indicate that an F -statistic near 1 or lower is “cause for concern”, and Staiger and Stock (1997) point to an F -statistic less than 5 as problematic, even with a sample size of several hundred thousand. Hall et al. (1996) generalize these tests of instrument relevance to one based on the statistical significance of the smallest canonical correlation between instruments and endogenous explanatory variables, with the R^2 - and F -based tests appearing as special cases. In the one-instrument/one-regressor case, their relevance test statistic, $-\{\text{sample size}\} \times \ln(1 - [\text{corr}(z, BE)]^2)$, is asymptotically χ^2 -distributed with 1 d.f. However, they caution against using such measures as an a priori screening device to identify a suitable set of instruments, finding that higher relevance is likely to be achieved at the expense of lower exogeneity. Nevertheless, several authors (e.g. Bound et al., 1995; Staiger and Stock, 1997) urge that R^2 s and/or F -statistics from the first-stage regression of instruments against endogenous explanatory variables be routinely reported, as a basis for judging instrument relevance.

- (2) Having a poor \widehat{BE} can be viewed as an instance of measurement error in the original variable (the true BE), which is known to result in coefficient estimates for that variable that are inconsistent and biased toward zero, and coefficient estimates for the other variables in the equation that are also biased (Greene, 1997).
- (3) The asymptotic properties of IV estimators no longer hold, even for very large samples. The resulting coefficient estimators can be extremely biased and statistically inconsistent, i.e. differing considerably from the true value with nonnegligible probability (Bound et al., 1995; Hall et al., 1996; Staiger and Stock, 1997).
- (4) In small samples, the coefficients in Eq. (6) are biased in the same direction as the ordinary least squares (OLS) coefficients, which, as mentioned in Section 2, are themselves biased and inconsistent estimators of the true values. The weaker the instruments, the more closely the bias of IV estimation approaches that of OLS (Bound et al., 1995).
- (5) When the correlation between BE and ε is high, it is possible for \widehat{BE} to appear to be strongly significant in Eq. (6), even when the true effect of BE is zero, and “[t]hus it is in the cases where least squares is a poor estimator that instrumental variables with a poor instrument will be even worse” (Nelson and Startz, 1990 p. S125; also see Hall et al., 1996).

Finally, independently of the weak instruments problem, special account needs to be taken of the sampling variance in \widehat{BE} , or else incorrect statistical inferences on the significance of its coefficient in the TB model may result. The corrections needed are especially tedious when the TB variable is discrete (Bhat and Guo, 2007).

3.4. Sample selection models

The basic idea behind this approach is to explicitly model the prior selection into (or participation in) different discrete states (residential location types here), and model the outcome of interest (TB) as conditional on that prior selection. Probably the most common form of the sample selection model has a participation equation and a single outcome equation, where the outcome is observed only if participation in a particular state occurs. That typical form of the selectivity model is not quite appropriate in our context, however: although residential choice (RC) is often treated as binary in the literature (e.g., representing a stereotypical “urban” (U) or “suburban” (S) neighborhood), we observe TB in either case, not only if RC = 1. A more general form of the sample selection model, referred to as a switching regression model with endogenous switching, is needed (Lee, 1983; Maddala, 1983; Heckman, 1990):

$$\begin{aligned}
 RC^* &= f_R(BE, X, Z) + \varepsilon_R, \\
 RC &= 1 \text{ (urban neighborhood chosen) if and only if } RC^* \geq 0; \text{ otherwise } RC = 0 \text{ (suburban neighborhood chosen),} \\
 TB_U &= f_U(BE, X, Y) + \varepsilon_U, \quad \text{and} \\
 TB_S &= f_S(BE, X, Y) + \varepsilon_S,
 \end{aligned} \tag{7}$$

where TB_U is observed if $RC^* \geq 0$, and TB_S is observed if $RC^* < 0$.³ (More specifically, we could define the outcome equations in terms of latent variables TB_U^* and TB_S^* , with an observed variable TB equal to TB_U^* if $RC^* \geq 0$ and TB_S^* if $RC^* < 0$, respectively). ε_R is allowed to be correlated with ε_U and ε_S , and their correlations are generally simultaneously estimated together with all the other parameters of the joint system. The selection bias arises because the equation for TB_U is not estimated over the entire population, but only for people living in urban neighborhoods, so it is not a reliable indicator of how a randomly selected member of the population as a whole would behave in an urban neighborhood (and similarly for TB_S). If the selection bias is ignored and the equations for TB_U and TB_S are estimated using ordinary least squares regression, the resulting coefficients will be inconsistent and inefficient. Doing so leads to a form of omitted variables bias, where the omitted variable is one that corrects for the sample selectivity (see the equations in, e.g., Vance and Geoghegan, 2004).

³ See Train (1986, Chapter 5) for an informative discussion of this model when the RC variable is multinomial rather than binary.

Where is AT in this model? If observed, it is represented by X , Y , and/or Z , depending on its expected relationships with RC and TB (generally expected to influence both, i.e. to be represented by X). Most commonly, however, AT is unobserved, and the implicit assumption is that the influence of AT on BE is controlled for by the presence of BE in the RC* equation. In reality, since the measurement of BE will not be perfect and its relationship to RC* will not be perfectly captured, some (perhaps much) influence of AT will remain in ε_R and possibly be correlated with counterparts in ε_U and ε_S . The model formulation allows for this eventuality.

The fundamental unity between sample selection models and those using the IV method has been formally explored, e.g. by Heckman and Vytlačil (2005). At a superficial level, both involve multiple equations: one or more “outcome” equations (for TB, in our context) and one or more equations modeling the “troublesome” (endogenous explanatory) variable (RC or BE, in our case), referred to in the sample selection context as the “participation” or “selection” equation(s). The latter equation is typically a discrete choice model in the sample selection context, but the same can be true for an IV model (e.g. Khattak and Rodriguez, 2005), although they are more often linear regressions on continuous dependent variables. However, whereas in a classic IV model the instrumental predictors of BE should be variables that are not expected to have a direct impact on TB (e.g. Bound et al., 1995), in sample selection models it is not only permissible but customary (though not essential) for the participation and outcome equations to share some explanatory variables (X and BE in Eq. (7)), and permissible for the observed explanatory variables in the participation equation to be correlated with the unobserved variables in the outcome equation. It is generally assumed, though, that a sample selection model contains at least one explanatory variable (Z above) that influences participation but has no direct effect on the outcome; such variables are instrumental variables for participation. When all the explanatory variables in the participation equation fit that description, the sample selection model is essentially an IV model.⁴

In our context the two approaches differ in the forms of the structure they place on the relationships among the key endogenous variables. The IV approach explicitly incorporates BE into the TB equation, whereas the sample selection approach need not – the effect of the BE could be completely captured by the RC and RC* variables (although that would represent the special case in which the BE has no separate influence on TB, beyond its influence on RC). On the other hand, the concept of discrete observed participation (RC) controlling the entire outcome (TB) equation is integral to the sample selection approach, whereas in the IV approach, BE (or RC) is just one of many potential explanatory variables that enters the TB equation in the usual linear compensatory fashion.

3.5. Other joint models

The quantitative approaches discussed thus far have progressed from single-equation models of TB that explicitly control for attitudes, to multi-equation models where selection into discrete conditions (residential neighborhood types) is modeled jointly with an outcome variable (TB) having a specification that will differ by condition. In terms of application to the problem at hand, two other types of models that jointly account for multiple endogenous choices appear in the literature: joint discrete choice models involving nominal and/or ordinal endogenous variables, and structural equations models involving continuous endogenous variables. Although in principle a combination of these two types of simultaneous models is possible – that is, a system of structural equations with discrete (binary or ordinal) endogenous variables (see, e.g., Lee, 1981; Lewbel, 2004; Louviere et al., 2005; Muthén, 1983) – applications involving such systems are still relatively rare and we are not aware of any in the present context.

3.5.1. Joint discrete choice models

In *joint discrete choice models*, the observed endogenous variables measuring residential choice (RC) and travel behavior (TB) are both discrete, whether nominal or ordinal, and the joint probability of an (RC, TB) bundle being chosen is modeled. Such models have been recognized as an approach to dealing with res-

⁴ At the other extreme, for the first formulation of the selection model above, if the selection and participation equations are identically specified, the Tobit model results (e.g., Sigelman and Zeng, 1999).

idential self-selection for several decades. For example, Horowitz (1986) reported on a 1976 multinomial logit estimation of joint probabilities of residential location (census tract) and commute mode choices (auto or bus) in Washington, DC using data collected in 1968. In justifying the approach, he commented (pp. 207–208) that preferences for travel by certain modes could affect one’s choice of residential location as well as the converse, and “[t]hus, there is not a clear direction of causality from one choice to the other. Causality may run in both directions simultaneously, thereby making the choices interdependent.” He explained that a model of commute mode choice *conditional on residential location* could account for short-term impacts of (for example) policy changes on mode choice, whereas modeling the two choices *jointly* could account for long-term impacts of the policy on residential location, as well as on mode choice.⁵

This category can be further subdivided into two: “sequential” and simultaneous models. The “*sequential*” approach is represented by the multidimensional nested logit model (Ben-Akiva and Lerman, 1985), where both choices are treated as nominal, and in which one choice (most naturally, TB) is conditioned on the other (RC) so that the joint probability of an (RC, TB) bundle being chosen is modeled as $\Pr[\text{RC}] \Pr[\text{TB}|\text{RC}]$. The latent utility for a generic (r, t) combination is formulated as an additive function of observed variables common to both the residential choice and travel behavior alternatives (X_{rt}) and unique to each type of choice (Y_r and Z_r , respectively), unobserved variables common to both (ε_{rt}), and unobserved variables unique to the “upper” choice (ε_r , independent of ε_{rt}):

$$U(r, t) = f_5(X_{rt}, Y_r, Z_r) + \varepsilon_r + \varepsilon_{rt}. \quad (8)$$

The conditional probability of the lower choice, $\Pr[\text{TB} = t | \text{RC} = r]$, will be a function of X_{rt} and Y_r . The marginal probability of the upper choice, $\Pr[\text{RC} = r]$, will be a function of the expected maximum utility (referred to as the “inclusive value”) across all alternatives available for the lower choice (conditioned on the upper choice), as well as Z_r . It can be shown that the correlation θ of the error terms for the utility functions of choices within the same nest (i.e. the utilities of all (r, TB) choices for a specific residential choice r) is

$$\theta = \frac{\text{Var}(\varepsilon_r)}{\text{Var}(\varepsilon_r) + \text{Var}(\varepsilon_{rt})}.$$

This quantity measures the proportion of total variation in the unobserved portion of utility that is due to unmeasured variables (such as attitudes) common to all (r, TB) alternatives (ε_r) as opposed to variables unique to each individual (r, t) combination (ε_{rt}). As such, it could be viewed as an indicator of the extent to which selection into a particular residential neighborhood has not been controlled for by observed variables: the more completely variables that are related to residential choice in a systematic way are accounted for (observed), the more the variation in *unobserved* utility will be due to idiosyncratic features of specific (RC, TB) combinations, which vary randomly from one combination to the next.

Alternatively, Salon (2006) suggested that the influence of self-selection can be quantified by taking the difference between unconditional elasticities of TB and those computed to be conditional on RC. Specifically, using travel diary data from the Regional Travel – Household Interview Survey, she estimated a three-tiered nested logit model of residential choice (census tract: the chosen tract plus 10 randomly selected alternatives), auto ownership (AO: 0, 1, or 2+ cars), and walking level (WL: zero = no trips that were walk-only, low = 1–49% of trips walk-only, and high = 50% or more trips walk-only) for 4,382 residents of New York City. Given the available variables, she used population density as an indicator of neighborhood walkability. Using the full joint model, she then computed various elasticities of WL with respect to population density (BE, for the sake of argument). Specifically, she computed the self-selection effect as the difference between (1) the elasticity of walking level with respect to population density calculated from the marginal $\Pr[\text{WL}]$ (obtained from the unconditional probabilities $\Pr[\text{WL}, \text{RC}, \text{AO}]$ by $\Pr[\text{WL}] = \sum_a \sum_r \Pr[\text{WL}, \text{RC}, \text{AO}]$), and (2) that calculated from the conditional $\Pr[\text{WL}|\text{RC}]$ (obtained from $\Pr[\text{WL}, \text{AO}|\text{RC}]$ by $\Pr[\text{WL}|\text{RC}] = \sum_a \Pr[\text{WL}, \text{AO}|\text{RC}]$).

It is important to realize that although such a model *can* represent a temporal sequence of choices, it need not do so (Sobel, 1980). Mathematically, the nested logit model simply represents a particular structure for the correlations of unobserved variables across sets of alternatives; the choices themselves theoretically could take

⁵ A simplified and hypothetical, but instructive, example illustrating this point is presented in Cao et al. (2006).

place in any order or simultaneously. However, although nested logit models do not *impose* a sequential structure on multiple decisions, they can certainly *reflect* one when it exists (in other words, sequentially dependent decisions are a sufficient, though not necessary, condition for nested logit to be a potentially appropriate model structure). The present context is one such natural application, since residential choice has long and widely been held (e.g. Salomon and Ben-Akiva, 1983) to be a longer-term choice which is antecedent to short-term choices related to individual trips. But it must be emphasized that finding such a structure to fit the data well cannot be taken as *confirmation* of a sequential decision process, only as being *consistent* with it. Further, analysts should not let a presumed temporal sequence of decisions blind them to alternative possibilities such as those shown in Fig. 1.

In the *simultaneous* joint discrete choice model, latent utilities for each choice, RC* and TB* (where, in the two applications to date, RC is nominal and TB is respectively ordinal or nominal), are formulated in separate equations, with the probability of a particular (RC, TB) bundle being estimated jointly. The separate utility equations may have overlapping sets of explanatory variables, but (together with the other joint models discussed so far) do not include one endogenous variable directly in the equation for the other. Bhat and Guo (2007) pioneered the theoretical development and empirical application of such a joint structure modeling discrete residential choice and ordinal car ownership, parameterizing the error terms as follows:

$$\begin{aligned} \text{RC}^* &= b(\text{BE}, Z, X) + u\text{BE} \pm w\text{BE} + \zeta, \\ \text{TB}^* &= t(\text{BE}, Y, X) + v\text{BE} + w\text{BE} + \delta, \end{aligned} \quad (9)$$

where u and v are unobserved (individual-specific) factors (such as attitudes) impacting households' sensitivity to built environment traits in residential choice alone and travel choice alone, respectively; w stands for unobserved individual factors impacting both residential and travel choices; and ζ and δ are idiosyncratic terms. By including the common error term $w\text{BE}$, Bhat and Guo's model simultaneously corrects for the endogeneity of the built environment. Pinjari et al. (2007) extended this approach to incorporate a multinomial mode choice representation of TB.

3.5.2. Structural equations models

The second category of joint models is *structural equations models*. By contrast to the joint discrete choice models of the previous subsection, here the endogenous variables are typically continuous, and they are usually modeled as directly influencing other endogenous variables. Recognizing that AT influences both BE and TB, and therefore including it in a single-equation model for TB, as in Eq. (5), constitutes a useful improvement in the realism of a model of TB. In fact, however, the influence between attitudes and behavior is probably not entirely unidirectional, as Fig. 1 illustrates. It is quite possible that over time, both the BE and TB may affect AT as well, and AT and TB could affect BE (bringing about a residential relocation). There is a sizable literature in transportation (and other fields) on the mutual causality between attitudes and behavior, with ample evidence for impacts in both directions (e.g., Tardiff, 1977; Golob, 2001). Thus, improving the realism of the model even further suggests the need for multiple interrelated equations, reflecting the multiple likely directions of causality. Specifically, one could postulate the following Structural Equations Model (SEM):

$$\begin{aligned} \text{TB} &= t(\text{AT}, \text{BE}, W, X, Y, Z) + \omega_1 \\ \text{BE} &= b(\text{AT}, \text{TB}, W, X, U, V) + \omega_2 \\ \text{AT} &= a(\text{TB}, \text{BE}, W, Y, U, S) + \omega_3, \end{aligned} \quad (10)$$

where W = observed variables common to all three equations, X = observed variables influencing both TB and BE but not AT; similarly for Y and U ; Z , V , and S are observed variables whose influences are unique to TB, BE, and AT respectively; and the ω s represent the net impacts of the unobserved variables relevant to each left-hand side.

3.5.3. Discussion

As indicated above, in joint discrete choice models, the RC and TB choices are not directly modeled as affecting each other. A dependent relationship or *correlation* of unobserved factors influencing those choices

can be ascertained through statistical tests of certain parameters in the formulation, and in the Bhat and Guo formulation, the correlation is even modeled as a function of specific observed explanatory variables common to both equations (in this respect, it goes one step beyond an approach such as seemingly unrelated regressions for continuous dependent variables, or a sample selection model where the outcome variable as well as the participation variable is discrete, where error terms are allowed to be correlated across equations but are not parameterized). Although their initial application did not have observed attitudinal variables, the inclusion of such variables in future applications could provide additional insight into the sources of the relationship between the two choices: the estimates of the variance of w in their system were found to be insignificant, and this could be because the correlation of the error terms for the two choices was due to unmeasured variables such as attitudes toward walking and/or driving rather than the BE variables that *were* measured.

In general, then, one limitation of this approach is that unobserved portions of the RC and TB equations are assumed to be correlated only through their relationship to observed variables (though not necessarily BE variables alone, as was assumed in their initial application for simplicity only); the remaining error terms in Eq. (9), ζ and δ , are assumed to be uncorrelated. Thus, if the available observed variables fail to largely capture the effects of attitudes common to both choices, a key assumption of the model might not be met. It would be interesting to test the extent to which this assumption holds, in a future study which obtains attitudinal measures as well as typical “objective” measures of the BE and sociodemographic traits, and then examines the extent to which parameterizing the error terms with only the objective measures serves to capture the effects of attitudes common to both equations. Of course, it is fair to say that *all* techniques are limited by the variables for which observations are available, and can be improved by the measurement and inclusion of additional relevant variables. Our point is precisely that although this method uses to the utmost the information embedded in commonly available measures, we can only learn so much about behavior without moving more explanatory variables from unobserved to observed.

In any case, it may also be considered a limitation in some respects that the direction(s) of *causality* (if any) between RC and TB cannot be statistically tested. Rather, the two choices are modeled as if they occur simultaneously, potentially jointly influenced by common antecedent variables, but not by each other directly. (This is formally true even of the nested logit approach: while a sequential interpretation may be placed on the model as mentioned above, the essence of the model is the estimation of the joint probability $\Pr[\text{RC}, \text{TB}]$, and the assumption of direct causality between those two choice dimensions, in either direction, is neither required nor implied. However, see, e.g., [Tringides et al. \(2004\)](#) for a transportation application of the recursive bivariate probit model, involving two binary endogenous variables with a unidirectional relationship between them; and [Ye et al. \(2007\)](#) for an application of a simultaneous equation system with two mutually dependent discrete endogenous variables. The latter paper also has a useful discussion of the nature of causal inferences that can be made from such systems). This is a reasonable approach when two choices are made close together in time, but such a model may not reflect a situation in which the choices are in fact temporally decoupled, as RC and TB often are.

In structural equations models, by contrast, the fact that endogenous variables are modeled as directly influencing other endogenous variables provides the ability to conduct tests to ascertain which directions of causality (if any) are statistically supported by the data. This ability can enrich our insight into the behavioral processes of interest: beyond learning that RC and TB are correlated through having the BE in common, it can be valuable to determine whether that correlation is due to TB influencing the choice of the bundle of BE attributes that constitutes RC, or to the direct influence of the BE on TB, or both. The difference could be important to properly predicting the reaction to a change in the BE: potentially little reaction in the first instance (especially in the short run), and considerable reaction in the second.

However, although allowing multiple directions of causality arguably constitutes a conceptual improvement over the single-equation and joint (simultaneous) model methodologies, the use of cross-sectional data is still a practical drawback to this approach. The same temporal mismatch described in connection with model (5) of Section 3.2 may occur here. Further, properly estimating the parameters of a dynamic process using a static snapshot of data requires that the process be stable (not trending over time) and have achieved an equilibrium – conditions that not only may be unrealistic, but for which there is not a good statistical test ([Kline, 2005](#)).

Structural equations models have other limitations as well. For example, identifiability requirements may limit the specifications that can be empirically tested, and it is possible for several different

specifications – representing substantively distinct behavioral processes, with different policy implications – to fit the data roughly equally well (MacCallum, 1995; although the same can be true for nested logit models as well – see, e.g., Forinash and Koppelman, 1993). Further, structural equations models are not well-suited to situations where one or more endogenous variables are multinomial, a case of considerable interest in travel behavior research (particularly, in the present context, mode choice, but destination and route choices are potentially also measures of travel behavior that could be relevant in a residential self-selection study).

3.6. Longitudinal designs

In classical experimental design (e.g., Shadish et al., 2002), “before” measurements are taken, then participants are randomly assigned to either the experimental or the control group, the experiment is performed on the first group, and then “after” measurements are taken and changes are compared between the two groups. Such a longitudinal design can be used to control for attitudes that do not vary over time: if AT does not change across time, then $\Delta AT = 0$, and in the model

$$\Delta TB = f_6(\Delta BE, \Delta X) + \eta, \quad (11)$$

ΔBE and η ($=\Delta \varepsilon$) will be uncorrelated (if BE and ε were only correlated through AT). This formulation also controls for any other important variables that are either observed (ΔX) or remain constant (0 change) over the same time period. For these reasons, conventional wisdom holds that modeling the change in a given dependent variable is easier (produces better-fitting models, all else equal) than modeling its absolute level. Such an approach is very strong on the nonspuriousness and time precedence causality requisites mentioned in Section 2 (Singleton and Straits, 2005).

Thus, longitudinal designs can offer substantial improvement over cross-sectional designs, providing a more robust causal inference on the relationship between the built environment and travel behavior. These designs still have a number of limitations, however, both inherently and in the way they are likely to be applied in the present context. For example, even here, to be able to use the temporal sequence of observations to sort out multiple potential directions of causality, it is generally necessary to assume that the process is stable over time, which is not always realistic. Another intrinsic issue is that it can be difficult to determine the optimal time(s) at which to take each measurement, especially since the optimal spacing between measurements may differ by individual: too short, and the changes of interest will not have had time to occur; too long, and measurement of variables and relationships will be unreliable due to memory lapses and other noise in the system (Kline, 2005).

With respect to limitations specific to the likely application of longitudinal methods in the current context, an important issue is that neither the treatment nor the assignment to experimental or control group is completely random. Residential relocation, for example, is not a treatment randomly assigned by experimenters, but is a more or less voluntary result of individuals' changes in employment location, lifecycle, and, importantly, potentially attitudes toward travel modes and residential neighborhood environments. By contrast, an intervention (such as a traffic calming program) is to some extent an experimental manipulation. However, intervention programs are implemented at specific locations, which themselves are generally not random but rather (often) chosen on the basis of being more deficient on the dimension that the intervention is expected to improve. Further, participants are automatically classified into the treatment or control group based on their residential locations, not randomly assigned. These self-selection effects can jeopardize the generalizability of a study's findings. A further practical difficulty of true longitudinal studies is that they can be more expensive and are certainly more time-consuming than cross-sectional ones. Finally, although not intrinsic limitations of the approach, applications to date have been hampered by not measuring attitudes across time (when in point of fact, it may be precisely a change in attitudes that prompted a residential relocation in the first place), and by not including feedback loops from the built environment to attitudes.

4. Exactly what question(s) are we trying to answer, again?

Given the preceding immersion in technical detail, it is worthwhile at this juncture to step back and ask ourselves specifically what it is we are interested in knowing! The purpose of this discussion is not to defini-

tively enunciate the question(s) of interest, but rather to explore a variety of relevant questions and to more clearly delineate the issues involved in choosing which one(s) to address. Several pertinent questions are discussed in the subsections below.

4.1. Is there a statistically significant effect of BE on TB after self-selection has been accounted for?

This is the first and simplest way to pose the question of interest. We have indicated in the discussion of each methodological approach to this issue how that question could be answered, and indeed, if the answer is “no”, then the remaining questions of this section become moot. In point of fact, however, based on the empirical evidence to date, the answer would have to be a straightforward and resounding “yes”. Virtually every quantitative study reviewed for this work (see Cao et al., 2006 for details), after controlling for self-selection through one of the various ways discussed above, found a statistically significant influence of one or more built environment measures on the travel behavior variable of interest.

4.2. What is the size of the true impact of BE on TB?

On the other hand, although many academic studies tend to focus purely on the question of statistical significance, the magnitude and practical relevance of an effect is arguably at least as important (Ziliak and McCloskey, 2004). In other words, is the true influence of the built environment even worth bothering over, after we go to all the trouble to assess it properly? For example, to ascertain whether changes to the BE are a cost-effective way to change TB, given the opportunity costs of spending resources another way, it is necessary to determine the magnitude of the effect, not just whether one occurred or not. The ways to answer this question differ depending on whether the built environment is measured as continuous (BE) or discrete (RC). We treat each case in turn.

4.2.1. True marginal effects on TB for continuous-valued measures of BE

The second column of Table 2 summarizes how the true marginal effects of continuous-valued measures of BE on the expected value of TB can be ascertained for the methodologies discussed here. For the statistical control, instrumental variables, and longitudinal models, the magnitude of the true marginal impact (purging BE of the influence of AT) can easily be obtained from the appropriate coefficient of BE, \widehat{BE} , or ΔBE , respectively, in the equation for TB. Note that in these cases, expressing TB as a linear function of BE (or its variations) implies the assumption of a constant marginal effect of BE on TB. For the remaining methodologies, however, ascertaining the marginal impact is not so straightforward.

For the selection model approach, a number of definitions of the marginal impact are of potential interest (similar to the discussion in Section 4.2.2, for assessing the effects of the discrete-valued RC variable). The application literature is not always clear about which definition a given study uses, nor which is most appropriate to the problem at hand. It is first of all important to distinguish between (see equation system (7)) (i) effects on the expected values of the *potential* TB_S^* and TB_U^* , if those quantities could be observed for the entire population instead of only for those for whom $RC = 0$ and $RC = 1$, respectively; versus (ii) effects on the expected values of the *actual* TB_S and TB_U , where TB_S is observed if $RC = 0$ and TB_U is observed if $RC = 1$. With respect to effects on the actual TB_S and TB_U , it is further vital to distinguish between *conditional* effects – on $E[TB_S|RC = 0]$ and $E[TB_U|RC = 1]$, and *unconditional* effects – on $E[TB_S]$, $E[TB_U]$ and $E[TB]$, where $E[\bullet]$ is the expectation operator. It is also necessary, in the most general case, to account for the appearance of BE in both the participation and the outcome equations. Respectively, the effects of interest have the following interpretations in this context (see Huang et al., 1991 and Vance and Geoghegan, 2004 for applications in different contexts):

- $\partial E[TB_S^*]/\partial BE$ is the effect of increasing BE by one unit on the expected potential travel behavior of a randomly selected person living anywhere, if that random person were to be governed by the TB_S equation. This is simply given by the coefficient of BE in the equation for TB_S (Maddala, 1983; Huang et al., 1991).

Table 2

Detecting the true effect of the (continuous-valued) built environment on travel behavior under the assumption that attitudes affect both BE and TB

Method	True effect on TB of increasing BE measure by one unit ^a	Proportion of total effect of BE on TB that is due to the BE alone rather than due to the effect of AT on BE
<i>Statistical control</i>	Coefficient of BE in Eq. (5) for TB	Incremental contribution to R^2 of BE (given AT and all other variables included), divided by incremental contribution to R^2 of BE and AT entered together (given all other variables included)
<i>Instrumental variables</i>	Coefficient of \widehat{BE} in Eq. (6) for TB	Not possible to calculate
<i>Selection models</i>	Multiple possible effects, conditional and unconditional. Must separate out the effect of BE on RC* (or participation probabilities) from the effect of BE on TB_U , TB_S , and/or TB; the latter component is the “true” effect	Proportion of total marginal effect of BE that is due to its direct effect on TB_U , TB_S , and/or TB (as opposed to its effect on RC* or participation probabilities)
<i>Nested logit (NL)</i>	Elasticity of marginal probability of a given TB outcome, minus elasticity of the conditional probability of that outcome given fixed RC ^a	Quantity to left, divided by elasticity of marginal probability of a given TB outcome
<i>Simultaneous joint discrete choice</i>	Same as for NL ^a	Same as for NL
<i>Structural equations model</i>	<i>Recursive models:</i> Total effect of BE on TB. <i>Nonrecursive models:</i> Difficult or impossible to isolate from the impact of changes in AT	<i>Recursive models:</i> Same as for the statistical control method. <i>Nonrecursive models:</i> No guidance from the literature
<i>Longitudinal model</i>	Coefficient of ΔBE in Eq. (11) for ΔTB	Incremental contribution to R^2 of ΔBE (given ΔAT and all other variables included), divided by incremental contribution to R^2 of ΔBE and ΔAT entered together (given all other variables included)

^a For the discrete choice models, the percentage effect, on the probability of a discrete TB outcome, of increasing BE by a percentage.

- $\partial E[TB_U^*]/\partial BE$ is the effect of increasing BE by one unit on the expected potential travel behavior of a randomly selected person living anywhere, if that random person were to be governed by the TB_U equation. Similarly, this is given by the coefficient of BE in the equation for TB_U . The remaining effects, however, are more complex and will differ by individual.
- $\partial E[TB_S|RC = 0]/\partial BE$ is the effect of increasing BE by one unit for those living in suburban neighborhoods, on the expected actual travel behavior of those living in suburban neighborhoods. This conditional effect is not the same as would be obtained from the regression of TB against BE (and other variables) for the sub-population of individuals living in suburban neighborhoods, i.e. not just the coefficient of BE in a stand-alone equation for TB_S . The proper formula for the conditional effect (found in Huang et al., 1991) corrects for the bias inherent in self-selection into a given type of neighborhood, by incorporating the effect of BE on RC in the participation equation.
- Similarly, $\partial E[TB_U|RC = 1]/\partial BE$ is the effect of increasing BE by one unit for those living in urban neighborhoods, on the expected actual travel behavior of those living in urban neighborhoods.
- $\partial E[TB_S]/\partial BE$ is the effect on expected actual TB of increasing BE by one unit for a randomly selected person living anywhere, if that random person were to be governed by the TB_S equation. As Huang et al. (1991) show using the product rule of differentiation, this effect can be decomposed into (1) the change in TB_S (because of changing BE) weighted by the probability that the selected person lives in a suburb, plus (2) the change in the probability that the selected person lives in a suburb (i.e. the effect on RC of the change in BE) weighted by the expected value of TB_S given that the person lives in a suburb. In other words, a change in the built environment will alter travel behavior in two ways: by directly affecting it, and by affecting the probability of living in an urban versus suburban neighborhood, which itself affects travel behavior (by controlling which TB equation is in effect).
- $\partial E[TB_U]/\partial BE$ is the effect on expected actual TB of increasing BE by one unit for a randomly selected person living anywhere, if that random person were to be governed by the TB_U equation; it can be decomposed in a similar way.

- $\partial E[TB]/\partial BE$ is the effect on the expected TB (regardless of whether it is observed for *S* or *U*) of increasing BE by one unit for a randomly selected person living anywhere. $E[TB]$ is the weighted average of the conditional expected values, where the weights are the probabilities of living in the respective types of neighborhoods:

$$E[TB] = E[TB_S|RC = 0]Pr[RC = 0] + E[TB_U|RC = 1]Pr[RC = 1],$$

and $\partial E[TB]/\partial BE$ is just the sum of the unconditional marginal effects $\partial E[TB_S]/\partial BE$ and $\partial E[TB_U]/\partial BE$. It decomposes into the weighted average of the coefficients of BE in the two equations for TB_S and TB_U (where the weights are the respective selection probabilities), plus a term representing the correction for self-selection into neighborhood type (incorporating the influence of BE on RC).

It is interesting to realize that the complexity of marginal effects for the selection model is not purely due to potentially correlated error terms of the participation and outcome equations (although that is one factor). Rather, it is mainly due to the presence of the same *observed* variables in both types of equations. Thus, if AT is unobserved, as is often a motivation to use selection models, the error terms in the participation and outcome equations are likely to be correlated – but *even if AT is observed*, if it is present in both types of equations (as would be expected) the complexities described above will arise.

Which of these marginal effects is most appropriate for the problem at hand? In some cases it might be the final one; in other cases it might be one or both of the conditional marginal effects. In some cases the choice is clear, in others it may require some debate till consensus is reached or until it is agreed that multiple measures have value. The key point here is that it is imperative to identify (and justify) which effect is being analyzed in a given context. It is all too easy to misstate the effects of changing BE by applying a conditional effect unconditionally, or conversely. Specifically, for example, it would not be appropriate to project the conditional marginal effect of a change in BE on TB_U to the population as a whole.

As indicated above, to obtain the *total* marginal effect of BE on TB, it is generally necessary to account both for its effect on RC and its direct effect on TB, given RC. By contrast, the “*true*” effect of BE referred to in Table 1 is the portion of the total effect that is *not* due to residential self-selection. Again, however, that effect is in general not simply the coefficient of BE in the equation for TB, and its computation differs across the different measures listed above. For the final total unconditional marginal effect $\partial E[TB]/\partial BE$, for example, the true effect of BE would be the weighted average of the coefficients of BE in the equations for TB_S and TB_U , where the weights are the respective selection probabilities.

Turning to joint discrete choice models, the idea of the decomposition of the effect of BE on TB into two components – that of the direct effect on TB and the indirect effect through its effect on RC – applies here as well. For such models, the elasticity approach applied to nested logit by Salon (2006) and described in Section 3.5.1 above would be also appropriate for the Bhat and Guo methodology.

For structural equations models, an important distinction is whether the model is *recursive* (i.e. having neither feedback loops, nor correlated error terms between directly linked endogenous variables) or *nonrecursive* (the in-between types of block and block-recursive are also possible, but for simplicity we focus on the two extremes). To illustrate a recursive model, consider the simple structure of Fig. 2, where BE is represented by the residential location construct. In this situation, it is useful to start with the *total association* (zero-order

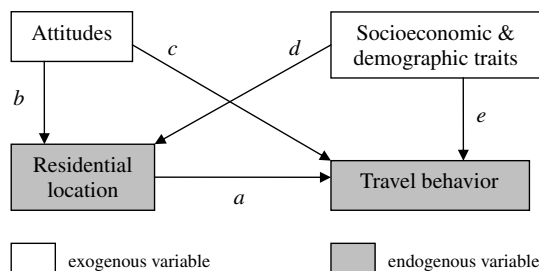


Fig. 2. A simple recursive structural equations model of residential location and travel behavior.

correlation) between BE and TB. This total association can be decomposed into (1) “spurious” components due to the common dependence of BE and TB on the antecedent variables AT and sociodemographic traits, (2) terms related to the unanalyzed correlations between the predetermined variables AT and sociodemographics themselves, and the (3) direct and (4) indirect effects of BE on TB (for an example, see the block-recursive model analyzed by Wolffe, 1980 and let his X_1 represent our TB, his X_3 be BE, and X_4 be AT; also see Alwin and Hauser, 1975). In Fig. 2 there are no indirect effects of BE on TB (i.e. those occurring through the impact of BE on an intervening variable that then affects TB), so the total effect, which is the sum of the direct and all indirect effects, is simply the direct effect. This is captured by the coefficient a of BE in the equation for TB, and represents the true effect of BE on TB (however, see Cao et al., 2007 for a discussion of the case in which c is insignificant).

For nonrecursive models, e.g. if in Fig. 2, TB were modeled as affecting AT (consistent with Fig. 1b) as well as the converse, computation of the total effect of BE on TB becomes more complex, and also includes the effect of AT on BE (see, e.g., Mueller, 1996). In such cases, the familiar regression-model interpretation of a coefficient as representing the marginal effect on the target variable of changing another variable by one unit, holding all other variables constant, is essentially meaningless (Hayduk, 1987). Changing BE would change TB, which would change AT – so AT could not be held constant. For the most robust SEMs, then (i.e. those allowing multiple directions of causality), it is quite difficult to isolate the true effect of BE on TB (that is, the separate BE effect remaining after the influence of AT is accounted for). The stability index of a nonrecursive system (Bentler and Freeman, 1983) gives a mathematical indication of whether the infinite loops of impacts converge or diverge, but gives no advice on how to isolate the true effect of BE from the portion of BE’s total effect that is due to AT, nor does it confirm whether the system is truly in equilibrium or not (as required for the model to be valid).

4.2.2. True effects of discrete-valued measures of residential choice on TB

For the case of a discrete-valued RC measure, the program/policy evaluation context provides a useful framework within which to view the current topic. In fact, in view of the linkage between the built environment and physical activity, some scholars addressing the residential self-selection question come from a medical/health perspective (see, e.g., Vol. 18, No. 1 of the *American Journal of Health Promotion*, a special issue on health-promoting community design; and TRB-IOM, 2005), in which it is routine to evaluate the impacts of discretely measured new medical treatments or public health-promotion programs or policies. The recent theoretical and applied econometric literature on policy evaluation (e.g., Winship and Morgan, 1999; Heckman and Vytlačil, 2005) offers a deepening understanding of this framework. The basic scenario in this literature is that there is a discrete *treatment* (e.g. a new policy), which is chosen by or applied to some of the population to whom it is available (the *treated*) but not others (the *untreated*); there can be multiple categories of (non)treatment, but for simplicity we will restrict the discussion to two).

The generic question is, what is the true effect of the treatment on an *outcome* variable of interest, particularly in the presence of nonrandom selection into the treated versus untreated groups (specifically, when selection into groups is correlated with variables affecting the outcome)? In cross-sectional analysis, each individual is observed in either a treated or untreated state, but not both. To evaluate the treatment effect, what we typically *want* to do is compare the outcomes of a randomly selected person who is moved from untreated to treated (but see below for further discussion of this point). What we *can* do, instead, is compare the observed outcomes of the treated to those of the untreated. That comparison will be a biased estimator of the true effect if (1) treated individuals initially differ from the untreated on variables relevant to predicting the outcome (which means the two groups would have different outcomes even in the absence of the treatment); and/or (2) the treated differ from the untreated in their potential reaction to the treatment (different functions, or different parameters of the functions, relating explanatory variables to outcomes).

In our context, we have generically referred to the outcome of interest as travel behavior (TB) and the treatment as residential choice (RC). We have alluded to both forms of bias: one in which the treated (say, residents of traditional neighborhoods) differ from the untreated (residents of suburban neighborhoods) on the levels of their initial attitudes (AT) toward BE and TB (among other variables), and the other in which the impact of a *given* level of, e.g., BE or AT on TB could differ by residential neighborhood type. We are interested in the effect of “switching from 0 (suburban) to 1 (urban)”, but what exactly does that mean? A fundamental con-

tribution of the recent literature is the articulation of numerous potentially relevant effects of interest, and their unification under the common framework of marginal treatment effects (see, e.g., Heckman and Vytlačil, 2005). We mention several possibilities here, described in terms of the current topic. For concreteness, we take the treatment to be the choice to live in an “urban” (meaning traditional or neo-traditional, transit- and/or pedestrian-oriented, etc.) neighborhood, and the outcome of interest to be vehicle-kilometers of automobile travel (auto VKT) for residents of such neighborhoods:

- *(conditional) marginal treatment effect* ($MTE(x, \varepsilon_R)$): What would be the average effect on auto VKT of moving from a suburban neighborhood to an urban one, given observed variables affecting the outcome (the X , Y , and BE of equation system (7), here referred to collectively as x), and unobserved variables (say, including AT) affecting participation (ε_R)? In essence, this question asks, what would be the effect of moving a specific randomly selected person from suburban to urban, averaged over *all people in the population having identical observed characteristics affecting TB and identical unobserved characteristics affecting RC*?
- *the average treatment effect* ($ATE(x)$): What would be the average effect on auto VKT of moving a randomly selected person having observed characteristics x from suburban to urban? This effect would be obtained by averaging the MTE over the distribution of ε_R in the entire population.
- *the average effect of treatment on the treated* ($TT(x)$): What would be the average effect on auto VKT of having moved a randomly selected *urban resident* with observed characteristics x from a suburban neighborhood to an urban one? This effect is obtained by averaging the MTE over the distribution of ε_R in the population of urban residents.
- *the average effect of treatment on the untreated* ($TUT(x)$): What would be the average effect on auto VKT of moving a randomly selected *suburban resident* with observed characteristics x to an urban neighborhood? Naturally enough, this effect is obtained by averaging the MTE over the distribution of ε_R in the population of suburban residents.
- *local average treatment effect* ($LATE(x, z_0, z_1)$): As mentioned in Section 3.4, it is generally assumed that a sample selection model contains at least one explanatory variable Z that influences participation but has no direct effect on the outcome; such variables are instrumental variables for participation. A typical example of such a variable is one indicating the presence or degree of an *incentive* to participate. In a study of the impact of a college education on earnings, for example, a variable indicating the availability or amount of financial aid might be considered a useful instrumental variable in the participation model, without affecting the outcome given participation. In our context, such a variable might be the presence of financial incentives for households to move to urban neighborhoods. Then it is of interest to ask what would be the average effect on auto VKT for a person with characteristics x , if Z changes from z_0 to z_1 . That is, loosely speaking, *what is the effect for those who needed an incentive to move from a suburban neighborhood to an urban one*? This is the LATE (Imbens and Angrist, 1994; Imbens, 2001); it focuses on the effectiveness of the incentive by eliminating those who would have moved anyway (but see Winship and Morgan, 1999 p. 685 for some problems of the LATE approach).

In general, we expect $TT \geq ATE \geq TUT$. If the effect on auto VKT is independent of ε_R given x , then MTE, ATE, TT, and LATE are equal; estimating the true effect is challenging precisely because the effect of treatment is assumed to differ with different values of AT (which is a component of ε_R). On the other hand, if AT is observed (part of the x), then independence of the treatment effect from unobserved influences on participation may be a plausible assumption.

The answer to “which effect do we want?” is similar to that for the marginal effects of a continuous-valued BE in the previous subsection: “It depends”. Any of the measures above could be of interest in a given context. As with BE in Section 4.2.1, the key message here is that they do differ, in general, and accordingly should not be confused.

4.3. What is the effect of BE on TB in a relative sense?

Identifying the size of the true effect of the built environment on travel behavior (the subject of Section 4.2) is important, but in isolation it is often not very informative. It is desirable to have a way to put that effect in

context, to scale it or judge it in a relative sense. This suggests two other questions of interest, differing in whether it is the “totality” of BE or of TB which is taken as the benchmark. In the first question, the total variation of TB constitutes the relevant denominator; in the second question it is the total impact of BE (on TB).

4.3.1. *What proportion of the total (or even explained) variation in TB is truly due to BE?*

Answering this question is important to properly evaluating the ability of changes in the BE to stimulate meaningful changes in TB: is a given-magnitude marginal change large in relative terms, or a tiny drop in the bucket relative to an individual’s overall travel? It is telling that while virtually all of the studies reviewed for this work emphasize the statistical significance of the BE after self-selection has been controlled for, and several comment that the BE is only one (type) of a number of variables influencing travel behavior, only one study (Salon, 2006) directly quantified the *true* contribution of the BE to the explained variation in travel behavior. This is despite the fact that it is relatively easy to assess the proportionate magnitude of the impact of the BE, controlling for self-selection (at least approximately) for several of the methodologies reviewed here, by assessing the incremental contribution of BE variables to an R^2 or model log-likelihood measure, after AT is included. Alternatively, one could examine the change in TB predicted by a change in the BE (which has the advantage of expressing the BE influence in terms of “real” measures such as trips or distance traveled), but in so doing, it is essential to control for confounding factors.

We suspect this type of analysis is missing from published studies in part because the answer is expected (or found) to be “very little”, compared to the contributions of sociodemographic and unmeasured variables (as implied by the elasticities reported and computed in the review article of Ewing and Cervero (2001) and by the earlier work of Hanson (1982) and Weisbrod et al. (1980)). However, such an outcome should be neither terribly surprising nor embarrassing for a complex behavior such as travel, which has numerous influences both systematic and idiosyncratic. For the contribution of the BE to TB to be small would not render pointless any attempt to reshape the BE – as discussed elsewhere (e.g. Handy et al., 2006), there are many reasons for improving the BE beyond influencing travel behavior (such as increasing the diversity of available housing options), and even small contributions can be useful at the margin. But as long as changing travel behavior is one of the reasons evinced for changing the BE, it is relevant to know how effectively that particular goal is likely to be met (not to mention, *which* elements of the BE are most effective at influencing TB).

4.3.2. *Of the total influence of the built environment on travel behavior, what proportion is due to residential self-selection, and what proportion due to the separate influence of the built environment itself?*

This question reflects the desire simply to decompose the total influence of BE on TB (whether determined to be large or small) into the component that is due to AT, versus the component due to the true influence of BE. The final column of Table 2 summarizes how to answer this question using each of the approaches studied here. Several points are noteworthy.

First, using the IV approach, it is not possible to answer this question: while the BE explanatory variables are purged of their correlation with attitudes (thus allowing the separate influence of the BE itself to be determined), the attitudes themselves remain in the error term, and the extent of their influence cannot be distinguished from that of other unobserved variables.

Second, we have not been able to find a discussion of this question in the literature on selection models. However, since as discussed in Section 4.2.1 above it is possible to decompose the effect of a change in BE on expected TB into components due to (1) the direct change in TB due to the change in BE and (2) the change in the probability of a particular RC outcome due to the change in BE, it seems natural to answer the title question with (loosely speaking) $(2)/[(1) + (2)]$ and $(1)/[(1) + (2)]$, respectively.

Third, with respect to using the SEM approach to answer this question, the situation again differs depending on whether the model in question is recursive or nonrecursive. For recursive models, computing equation-by-equation R^2 s is appropriate and relatively straightforward (Bentler and Raykov, 2000), with interpretation (proportion of total variance in the “left-hand side” variable explained by the model) identical to that of the R^2 for a single-equation regression model (see, e.g., Mueller, 1988 for an application). Thus, it is reasonable to suggest that, for recursive models, the title question of this subsection can be answered in the same way as for the statistical controls method. With respect to Fig. 2, specifically, we can compute the ratio of the incremental

change in the R^2 of the equation for TB when BE is added to a system containing all other variables (including AT) and relationships, to the incremental change when AT and BE are added together. For nonrecursive models, not surprisingly, the situation is more complex. An important recent paper (Hayduk, 2006) offers a meaningful definition of R^2 for an endogenous variable in either a recursive or nonrecursive SEM,⁶ but decomposing such an R^2 into components due to specific explanatory variables has not, to our knowledge, been addressed by the literature.

5. Conclusions and recommendations

Over the past few years, disentangling the influences of the built environment and residential self-selection and determining their relative importance has become one of the most important emerging issues in understanding the relationship between the built environment and travel behavior (Krzizek, 2003b; TRB-IOM, 2005). This paper has identified and discussed seven approaches used in previous research to empirically address the issue of residential self-selection.

The direct questioning method is designed to qualitatively evaluate the process of residential choice and travel choice, and hence is statistically unable to establish the evidence required for confident causal inference. All the *statistical* methods reviewed here can rely on the travel price changes suggested by Boarnet and Crane (2001) as a plausible causal mechanism, and all can be considered strong in terms of their ability to identify significant associations between the BE and TB. Thus, they differ only in how well they meet the nonspuriousness and time precedence criteria. In our view, approaches that explicitly include attitudes can perform well on the nonspuriousness criterion (by leaving little room for significant results to be due to spurious correlation with unmeasured variables), while those that permit multiple directions of causality and/or involve measurement at multiple points in time can excel on the time precedence criterion. In many cases of interest, the conceptual ideal is the longitudinal structural equations modeling approach, which combines most of the strengths of the other methods: measurement of attitudes, allowance of multiple directions of causality, and measurement at multiple points in time. If, when used to evaluate a “treatment” such as a residential move or BE intervention, control groups as well as experimental groups are involved, this approach comes very close to being “airtight” (though questions about generalizability could still remain, and the limitations discussed in Section 3 should be kept in mind). Although this method has not yet been fully operationalized in the present context (Cao et al., 2007 comes the closest, to our knowledge, but does not include a control group and is only quasi-longitudinal in that “prior” measures are obtained only retrospectively, and do not include attitudes – though current attitudes are measured), a project is underway in Australia (Giles-Corti, 2006) which aims to do exactly that.

Unfortunately, given the various limitations discussed throughout this paper, we are unable at this point to confidently specify the nature and extent of the causality between the built environment and travel behavior. We identified a number of different ways the question of interest could be posed. In general, ironically, it seems as though the more sophisticated the approach to treating self-selection (and therefore, presumably, the more trustworthy the resulting effects that are identified), the more difficult it becomes to answer questions about the absolute and relative magnitudes of the true impacts of the built environment on travel behavior. In fact, those impacts differ by mode and trip purpose, as some studies have shown. They are also likely to differ for different segments of the population, an issue not addressed by any of the empirical applications reviewed for this study (though most of the methodological approaches can accommodate it conceptually).

Nevertheless, we can improve our understanding by designing studies to satisfy as many requisites of causality inference as possible. Future studies adopting research designs that more closely resemble a true experimental design will lead to more definitive inferences regarding causality. Two types of studies are important (both of them ideally to include comparison groups of unaffected individuals similar in other relevant ways): (1) true panel studies of residents who move from one type of neighborhood to another, with measurements of attitudes as well as sociodemographic traits and travel behavior before and after, and further exploration of

⁶ In our context, the measure, called the “blocked-error R^2 ”, is defined as the ratio of the reduced variance in TB accounted for when the ω_1 of equation system (10) is blocked from affecting TB but the rest of the model is in place, to the full variance in TB accounted for when the entire model (including the effect of ω_1 on TB) is operating fully.

the reasons behind the move; and (2) natural experiments that examine the impact on travel behavior in response to a change in the built environment, such as the implementation of a traffic calming program. Only by causal findings based on such evidence can we determine whether land use policies designed to increase opportunities for driving less and walking more will actually lead to the desired behavioral outcomes.

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