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10. Establishing causality using observational panel data: models and applications

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1 INTRODUCTION

While much empirical research in travel behaviour is based on cross-sectional data, it is well known that models based on cross-sectional data cannot satisfy all criteria for establishing causal effects. Four criteria must be met to qualify a relationship as causal: (1) association (X and Y should co-vary); (2) nonspurioussness (the relation between X and Y should not be explained by a third variable Z); (3) time precedence (X should precede Y in time); and (4) the presence of a theoretical mechanism (there should be an explanation as to why X influences Y). Using cross-sectional data, the criteria of association and nonspurioussness can be satisfied (the latter insofar as potential confounding variables are indeed measured and included in the model). But, the criterion of time precedence cannot be met. To still be able to make inferences on causal relationships, researchers usually *assume* certain causal orders based on behavioural theories. But, empirical verification of these cause-and-effect orders is not possible based on cross-sectional data.

Experiments, particularly true experiments that include randomisation of subjects over control and experimental conditions, offer the gold standard to establish causality. In an experiment, the researcher manipulates the independent variable before measuring the dependent variable, thereby satisfying the time precedence criterion. In addition, in true experiments, effects of potential confounding are controlled by randomising subjects over control and experimental groups. Hence, in contrast to (cross-sectional) observational data – where it may be uncertain whether indeed all relevant control variables are considered (i.e. included in the model) – there is the certainty that established effects are nonspurious. Unfortunately, experiments are often infeasible for many relationships in travel behaviour research, let alone *true* experiments. For example, it is impossible to change a person's attitude to assess the effect on behaviour. In some instances, natural experiments may offer a viable workaround. In natural experiments, respondents are exposed to conditions caused by nature or by other factors beyond the control of the researcher. But, the main drawback of natural experiments is that the researcher asserts little influence over the experimental settings and parameters (Dunning, 2008). Hence, a researcher has to hope for suitable circumstances to address a certain research question that presents itself in the real world.

Since experiments are often not feasible and cross-sectional data do not allow for establishing causality, researchers increasingly appreciate panel data as an approach to establishing causality. Panel data contain multiple data points from the same individuals observed at multiple points in time. Panel data offer several advantages over cross-sectional data to establish causality. In particular, unlike cross-sectional data, panel data meet the criteria of time precedence and nonspurioussness. In the statistical models literature, there are two traditions for handling panel data. The first tradition is rooted in econometrics, where random and/or fixed-effect models are used to capture the panel structure in panel data (Allison, 2009). The second tradition is rooted in psychology/sociology, where cross-lagged panel models (CLPMs) are the primary approach to capture the panel structure in panel data (Finkel, 1995). Presently, models from these research traditions are being merged, resulting in the fixed-effect and random-intercept crossed-lagged panel models (Allison et al., 2017; Hamaker et al., 2015). The latter model has already been applied in several travel behaviour studies.

Against this background, the contributions of the present chapter are fourfold. First, we highlight the relevance of establishing causal effects from both a scientific and a policy perspective, and discuss the limits of experimental research. Second, we provide an overview of the advantages of (observational) panel data over cross-sectional data for satisfying the criteria to establish causation. This is done by reviewing – on a conceptual level – the two dominant statistical models that have been used to handle panel data. In doing so, it also becomes apparent that there is a distinction between 'within-person' and 'between-person' effects. This distinction is crucial for adequate interpretation of panel data models and, thus, for the policy recommendation based on them. Third, we review empirical applications of recently developed panel data models in the travel behaviour research literature. We specifically highlight the new behavioural insights that can be gained from these models. Finally, we identify the theoretical implications of this line of research.

Before moving on, several comments on the exact scope of this chapter are in order. First, the present treatment of panel data models is exclusively focused on using panel data to establish the direction of causal effects between relevant variables. Other benefits of panel data are outside the scope of this chapter, although we highlight some of these in the concluding section. Second, we focus on relationships between variables with interval/continuous measurement levels, but do not consider limited dependent variables (LDVs) – such as multinomial variables, ordinal variables, duration-type variables, and discrete-continuous variables. These variables also play an important role in travel behaviour research and panel models also exist to model these variables, but these are considered beyond the scope here. Third, the review of empirical studies discussed above is by no means a comprehensive review of panel data studies in travel behaviour research; as mentioned, the selected studies are example applications of recently developed panel data models. For earlier work on panel data models in transportation, we refer to the overviews presented by Golob et al. (1997), Golob (2003) and Kitamura (1990).

2 THE RELEVANCE OF ESTABLISHING CAUSAL EFFECTS IN TRAVEL BEHAVIOUR RESEARCH

In essence, establishing causal effects is relevant for two reasons, the first of which is practical. From a practical (e.g. policy-oriented) perspective, it only makes sense to achieve a specific desired outcome (Y) by influencing another variable (X) if this variable indeed leads to this outcome. Consider a classic example of a spurious relationship – for example, the positive association between ice cream sales and swimming pool drownings. If one interprets this relation as causal, one might be inclined to limit ice cream sales to prevent drownings (or recommend this to policymakers). Intuitively, of course, this makes little sense, mainly because it is difficult to readily think of a causal mechanism that would indeed explain how ice cream sales lead to drownings (the fourth criterion of causation). Statistically, we can rule out the causal interpretation with certainty. By including the relevant 'third variable' – in this case the outside temperature – in a multivariate statistical model, the results would indicate that the association is indeed spurious. In short, to properly inform policymakers as to which factors to focus on to achieve specific outcomes (and which not), it is crucial that relationships can be interpreted as causal.

The second reason establishing causal effects is relevant is scientific: we can only really advance our understanding of travel behaviour if there is the certainty that the directional relationships between concepts that are assumed to exist by the theories we apply indeed exist. Consider the Theory of Planned Behaviour (TPB) (Ajzen, 1991) – a prominent theory in social psychology, and one that has also often been applied to explain (variations in) travel behaviour (see, e.g. Bamberg et al., 2003; Heath and Gifford, 2002). Similar to most social-psychological theories, this makes strong assumptions about the directions of causation, namely from the psychological constructs to (intended) behaviour. Unfortunately, tests of this theory – using structural equation models (SEMs) estimated on cross-sectional data – provide no rock-solid proof of the validity of this direction of causation. Based on the chi-square test (and relative fit indices), the fit of the model may be assessed (i.e. how well the model reproduces the observed correlational pattern). But, it is important to recognise that a (good) model fit cannot be used to establish support for the assumed directions of causation. In fact, other model structures – which may not have been explored – could work equally well, or even better. Moreover, given that it is usually computationally impossible to test all possible model structures (and we have a finite number of data points), it is even likely that the true model structure is different from the one that is deemed best. In other words, no amount of replication research (which may all report a good fit) can provide definitive proof that the theory is indeed valid. In the end, due to the cross-sectional nature of the data, the causal directions in studies using TPB (and other social-psychological theories) are assumed by the researcher and cannot be established empirically.

Establishing causal relations is thus crucial from both a practical and a scientific perspective. Of course, these perspectives are also interrelated, as nicely captured by Kurt Lewin's famous statement: 'nothing is as practical as a good theory'. Indeed, if a theory is wrong, policy recommendations that follow from the results of models based on it will not achieve the desired effects. Consider again the application of TPB in travel behaviour research (or other applications of social-psychological theories). In empirical applications, some factors are found to correlate more strongly with the behaviour in question than others. Typically, researchers then recommend that information campaigns should be set up to target those psychological factors that correlate most strongly with behaviour. But what if the processes that give rise to the correlations are due to entirely different causal processes than assumed by the model? In the best case, no effects may be achieved; in the worst case, adverse effects arise (if the sign of the estimate implied by the model is opposite to the real effect). While many researchers are aware that correlation is not causation, often they are still drawn to making policy recommendations that assume causal interpretations (Chorus and Kroesen, 2014). It should be noted that, while cross-sectional data are limited in establishing causal effects, they still represent a relevant approach to gaining (new) knowledge on travel behaviour and in informing theories and hypotheses (that may possibly also guide longitudinal work).

As mentioned in the introduction to this chapter, true experiments offer the 'gold standard' for establishing causal effects but may be difficult to apply to relationships in travel behaviour research. For example, for the relationship between psychological constructs and behaviours, it may be difficult (if not impossible) to manipulate the psychological variables and/or the behaviour (to test for reverse effects). In the literature, we found only one study that attempted to test the TPB experimentally by providing persuasive messages targeted at people's salient beliefs (Sniehotta, 2009). In this study, the messages were only found to have small effects on the psychological constructs that make up the TPB (i.e. the attitude towards the behaviour, subjective norms and perceived behavioural control). In addition, Sniehotta found that, while a small effect on behaviour was observed, this effect was not mediated by the psychological constructs, suggesting that the effect operated via other cognitions. To test effects in the opposite direction (from behaviour to attitudes), one would need to directly influence behaviour, which also difficult but not impossible. For this, people would need to voluntarily change their travel behaviour, for example by using a different mode than before. Of course, some may be willing to do this; but those that are not likely to also differ on relevant characteristics that also influence the outcome, leading to selection effects (and, thus, systematic bias).

Using true experiments to test other kinds of relationships in travel behaviour research may be challenging or even impossible for other reasons. It may be practically impossible for some relationships, while for others it may be unethical. For example, a true experiment to assess the effect of the built environment on travel behaviour would require that people – based on random assignment – move (or not) to specific residential locations. This would be hard to pull off in practice. An example of an ethically challenging true experiment is one to assess the effect of travel behaviour on health. For that, one would need to manipulate behaviour – which, as noted above, is difficult but not impossible. To test the effect of health on behaviour, one would need to manipulate the health of subjects – for example, by letting people deliberately gain weight. This would be considered unethical by many ethics boards at universities. Moreover, selection effects would inevitably occur, lowering the quality of the results of such true experiments, even if they were executed despite practical or ethical concerns.

These examples show that the use and scope of true experiments are limited in travel behaviour research. Therefore, in recent times researchers increasingly appreciate panel data as a viable approach to be able to establish causation.

3 PANEL DATA MODELS AND THEIR STRENGTHS AND WEAKNESSES

In this section, we will review the two dominant traditions in panel data models, along with their respective strengths and weaknesses in terms of being able to satisfy the criteria for causation. But, before examining these models, we first highlight the distinction between within-person and between-person effects, a distinction that naturally arises when dealing with panel data.

3.1 The Difference between Within-Person and Between-Person Effects

With cross-sectional data, there is only one observation per respondent. To establish a relationship between two variables, we can (only) estimate the between-person correlation. Usually, this relation is then assumed to also hold at the within-person level, which is the level where the psychological processes – which presumably give rise to the relationship in the first place – actually take place. Similar to the criterion of time precedence, with cross-sectional data, this assumption cannot be empirically tested. Panel data allow the researcher to also relax this assumption. Having multiple measurements per individual enables the researcher to also estimate a within-person relationship (for each individual).

The relevance of being able to separate the between-person from the within-person effect is illustrated in a compelling example by Hamaker (2012), who considers the relationship between typing speed (the number of words typed per minute) and the percentage of typos made. Cross-sectionally (i.e. at the between-person level), a negative relationship will likely be found: more experienced typists type faster and make fewer mistakes. Yet, this result does not hold at the within-person level: that is, if a particular person is forced to type faster than he/she normally does, that person would be expected to make more mistakes, not fewer. Hence, the relationship at the within-person level may differ from and, in this example, even be opposite to the relationship at the between-person level.

Similar examples have been provided by other authors, for example, considering the relationship between general intelligence (IQ) and alcohol use (Kievit et al., 2013). To give an example in the context of travel behaviour, one can think of the relation between weekly commuting distance and working from home (as shown in Figure 10.1). Commuters living close to work will be less likely to work from home (since it is easy to travel to work). They may travel to work more often but, due to the proximity, still travel fewer kilometres overall than workers living further away. This latter group may be more inclined to work from home but still travel more due to the long commuting distance (one or two long trips can surpass the total distance of four or five short trips). Hence, at the between-person (cross-sectional) level, a positive relationship may be observed. Obviously, this positive relationship does not hold at the individual level; any person who decides to work more from home will commute less. Hence the within-person relationship – that is, the level at which the causal effect actually operates – is opposite in sign to the between-person (cross-sectional) relationship.

Linking the above discussion to travel behaviour and its relationship with psychological factors, it becomes clear that, insofar as psychological factors indeed cause behaviour, their influences reflect intra-individual psychological processes, and therefore operate at the within-person level by definition. Hence, a researcher should strive for models that are able to test causal effects at the within-person level. Of course, the fact that relationships may potentially differ at the within- and between-person level does not automatically mean that this will always be the case in all travel behaviour contexts and for all relevant psychological factors. But, since most of the models estimated in the field are based on cross-sectional data, it is simply unknown whether the interpretation of effects at the within-person level is justified or not. In sum, in addition to the criteria of causation, it is vital that models developed by behavioural researchers are able to disentangle within-person and between-person effects.

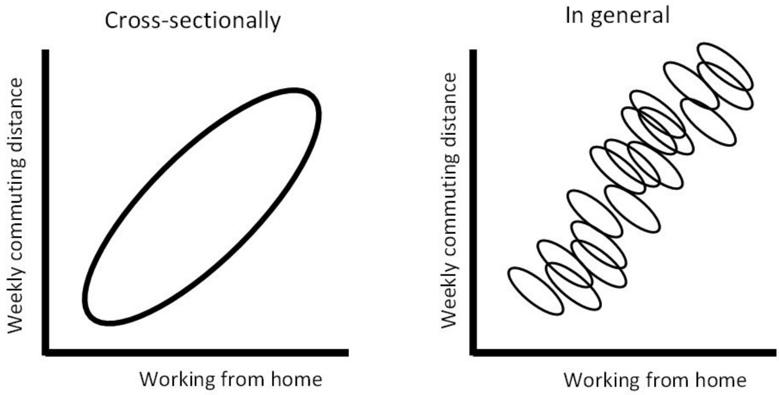


Figure 10.1 *Between-person (cross-sectional) relationship and within-person and between-person relationships between working from home and the amount of commute travel*

3.2 Panel Data Models Rooted in Econometrics

As mentioned in the introduction, there are two traditions to handling panel data: one rooted in econometrics, where random or fixed-effect models represent the common approach; and one rooted in psychology/sociology, where cross-lagged panel models represent the typical approach. Our aim here is not to provide a full technical explanation of these models;[1] instead, we will review them in terms of whether they can satisfy the criteria for causation and whether they can discriminate within- and between-person effects. We start with panel data models rooted in econometrics.

3.2.1 The fixed-effect model

Equation 10.1 presents a general formulation of a fixed-effect model for a continuous dependent variable y_{it} for a sample of individuals ($i = 1,\ldots,n$) and a number of time points ($t = 1,\ldots,T$). In the model, y_{it} is assumed to be (linearly) dependent on a set of time-varying predictor variables ($\boldsymbol{x}_{it}$) and a set of time-constant predictor variables ($\mathbf{z}_i$),[2] and μ_t is a time-varying mean to capture structural change in the dependent variable. The model includes two error terms, α_i and ε_{it}. While ε_{it} represents a pure random variation for each individual at each time point, α_i is fixed for each individual across all time points; α_i captures the influence of all unobserved variables that are constant over time for each individual *i*.

The fixed-effect model has two core strengths. Firstly, all time-invariant variables are controlled for (without the need to measure and include them in the model), thus reducing the risk of establishing spurious relationships.[3]

$$y_{it} = \mu_t + \beta\mathbf{x}_{it} + \gamma\mathbf{z}_i + \alpha_i + \varepsilon_{it} \tag{10.1}$$

To see this, consider the case in which we have two points in time. Applying equation 10.1 yields:

$$y_{i1} = \mu_1 + \beta \mathbf{x}_{i1} + \gamma \mathbf{z}_i + \alpha_i + \varepsilon_{i1} \quad (10.2)$$

$$y_{i2} = \mu_2 + \beta \mathbf{x}_{i2} + \gamma \mathbf{z}_i + \alpha_i + \varepsilon_{i2} \quad (10.3)$$

The 'first difference' equation can be computed (equation 10.4) by subtracting equation 10.3 from equation 10.2:

$$y_{i2} - y_{i1} = \mu_2 - \mu_1 + \beta \mathbf{x}_{i2} - \beta \mathbf{x}_{i1} + \varepsilon_{i2} - \varepsilon_{i1} \quad (10.4)$$

Because $\gamma \mathbf{z}_i$ and α_i are constant over time, these terms drop out of the equation. Hence, although the ability to model the influence of time-constant variables is lost, this comes with the advantage that all time-constant variables are controlled for. Basically, each individual acts as his or her own control.

A second strength of the fixed-effect model is its ability to capture the within-person effects of the (time-varying) predictors on the dependent variable. Because each individual is allowed to have his/her own fixed term (α_i) – which can be regarded as an individual-specific intercept – the remaining variation in the dependent variable relates exclusively to within-person variation. Were we to apply a fixed-effect regression model to the data presented in the right-hand panel of Figure 10.1, we would correctly retrieve the within-person (positive) effect for the relationship between typing speed and the number of typos.

3.2.2 The random-effect model

The random-effect regression model can be formulated in the same way as the fixed-effect regression model (equation 10.1), but makes two additional assumptions with respect to α_i. Firstly, it is assumed that α_i takes a parametric distribution (usually a normal distribution with a mean of zero and constant variance). Secondly, it is assumed that α_i is uncorrelated with the model's explanatory variables ($\boldsymbol{x}_{it}$ and $\boldsymbol{z}_i$). As argued by Allison (2009), the choice between fixed-effect and random-effect models is one between bias and efficiency. When α_i is truly uncorrelated with the observed predictors, the random-effect model will lead to more efficient recovery of the model's parameters (meaning: smaller standard errors of the estimates). Yet, if α_i is correlated with the model's explanatory variables, the model is biased – and so are the parameter estimates recovered by the researcher. Commonly, the Hausman test (which tests the equivalence of the parameter estimates) is used to determine which model is most appropriate (Hausman, 1978).

In summary, the main strengths of fixed- and random-effect models are that they allow the researcher to assess within-person effects and control for possible (time-constant) confounding variables. It should be emphasised, though, that while the fixed-effect model (by definition) controls for unobserved time-constant (between-person) variables, it does not account for the possible confounding effects of time-varying (within-person) variables. Hence, insofar as such variables influence both independent and dependent variables, they should be measured and included in the model. In addition, in the random-effect model, both time-constant and time-varying confounding variables should be included in the model.

In terms of being able to address the criterion of time precedence, nothing prevents the researcher from including lagged versions of x variables in the model, making it possible to assess whether past values of certain (time-varying) predictor variables are predictive of future values of y. Yet, it is assumed that $\boldsymbol{x}_{it}$ is strictly exogenous, meaning that $\boldsymbol{x}_{it}$ is statistically independent of ε_{it}. This assumption is violated if the effect of x on y actually operates in the opposite direction or when both variables reciprocally influence each other. Hence, the temporal order between the variables under consideration still needs to be established beforehand (e.g. based on theory), and cannot be empirically verified. This means that, to a limited extent, fixed- and random-effect models are only able to satisfy the criterion of time precedence.

3.3 Panel Data Models Rooted in Sociology/Psychology

The cross-lagged panel model (CLPM) is a popular model for handling panel data in sociological and psychological fields of science. Below we provide a brief conceptual description of this model.[4]

Figure 10.2 presents a CLPM for three measurement occasions, which can be specified as a structural equation model. In essence, the CLPM model can be used to test whether variation in a specific variable (e.g. x_1) can explain variation in another variable at a later point in time (y_2) (via coefficient β) while controlling for prior values of that variable (y_1) (through the stability coefficient (δ) and the correlation between x_1 and y_1); and, similarly, vice versa. Typically, this is referred to as Granger causality. The correlations between the error terms of the endogenous variables (u and v) capture the influences of unobserved (time-varying) variables and/or possible synchronous effects between x and y (i.e. effects with a shorter time lag than the period between the measurement occasions). The significance and strength of the 'cross-lagged' parameters β and γ are informative as to which of the two variables, x or y, is the strongest temporal predictor (or whether both variables influence each other).

While the CLPM relaxes the assumption that x is exogenous to y, it does not control for (between-person) unobserved heterogeneity. In addition, as argued by Hamaker et al. (2015), it (thereby) also confounds within-person and between-person covariation. As such, the cross-lagged parameters are not (merely) reflective of the within-person effects, but an (undesirable) mix of within- and between-person effects.

Table 10.1 summarises the strengths and weaknesses of the panel data models used in both modelling traditions. This overview clearly shows that both types of model form natural complements: the strengths of the fixed-effect/random-effect (FE/RE) models address the weaknesses of the CLPM, and vice versa.

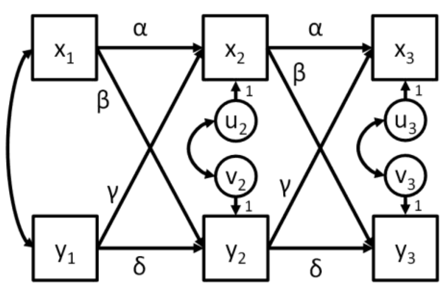


Figure 10.2 A three-wave, cross-lagged panel model

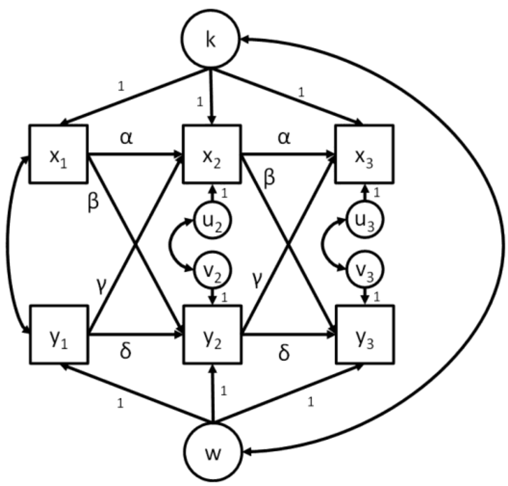
Table 10.1 Strengths and weaknesses of panel data models

	Control time-constant variables	Disentangle within-person and between-person effects	Estimate reciprocal effects between variables of interest
Fixed-effect model	X	X	
Random-effect model	X	X	
Cross-lagged panel model			X

3.4 The Fixed-Effect and Random-Intercept CLPM

Given the complementary nature of the strengths and weaknesses of the panel data models shown in Table 10.1, it is no surprise that researchers have been trying to develop models that blend the two streams. For instance, Allison et al. (2017) present a fixed-effect cross-lagged panel model (FE-CLPM), while Hamaker et al. (2015) present a random-intercept cross-lagged panel model (RI-CLPM) (both models can be specified as a SEMs). In essence, both these models extend the CLPM above by including an additional term (either fixed or random) to capture stable inter-individual differences.

Figure 10.3 presents the structure of Hamaker et al.'s RI-CLPM. This extends the CLPM with two additional latent variables, ω and k, which are assumed to have a time-constant influence on the observed scores at each point in time. By 'factoring out' these stable individual differences, the model controls for time-constant variables; and the cross-lagged parameters (subsequently) only capture within-person carry-over effects from one occasion to the next. The correlation between ω and k reveals to what extent both variables are associated as well as the between-person level.



Source: Hamaker et al. (2015).

Figure 10.3 Structure of the random-intercept cross-lagged panel model (RI-CLPM)

To account for structural/period effects, the observed values for *x* and *y* are typically mean-centred around the respective mean of each wave. The models of Allison et al. (2017) and Hamaker et al. (2015) present the state of the art and the best available models to establish causality as well as disentangle within-person and between-person effects. That said, as goes for all models, a model is only as good as its underlying assumption. Specifically, estimates of the state-of-the-art models may still be biased if relevant time-varying variables that influence both *x* and *y* are not taken into account. Hence, insofar as these indeed play a relevant role, such variables should still be measured (on each occasion) and included in the model. In addition, estimates may also be biased if the time between measurement occasions does not match the time it actually takes for the causal processes to evolve. Unfortunately, for many relationships, these optimal lags are simply unknown. In other words, state-of-the-art panel data models are not a panacea. Therefore, researchers need to remain cautious when interpreting results from such models and making policy recommendations. Nevertheless, these models are a major step in the toolbox of researchers aiming to establish causal effects.

4 EMPIRICAL APPLICATIONS OF PANEL DATA MODELS IN TRAVEL BEHAVIOUR RESEARCH

Table 10.2 provides an overview of previous and ongoing efforts to collect panel data on travel behaviour. Given the difficulty of setting up a panel and dealing with practical

Table 10.2 Panel datasets on travel behaviour

Name	Start	End	No. of waves	Sample	Data	Study
Dutch National Mobility Panel (LVO)	1984	1989	10	1600 households (with refreshments)	Trip diary, personal and household surveys	Van Wissen and Meurs (1989)
American Puget Sound Transportation Panel (PSTP)	1989	1993	4	1700 households (with refreshments)	Trip diary, personal and household surveys	Murakami and Ulberg (1997)
German Mobility Panel	1994	present	28	1800–2000 households (3-year rotating panel)	Trip diary, personal and household surveys	Ecke et al., 2019
Chilean Santiago Panel	2006	2008	4	250 individuals	Trip diary, personal surveys	Yáñez et al. (2010)
Netherlands Mobility Panel (MPN)	2013	present	10	2000 households (with refreshments)	Trip diary, personal and household surveys	Hoogendoorn-Lanser et al., 2015
MOBIS	2020	present	3	3680 individuals	GPS tracking, personal survey	Molloy et al. (2022)

problems such as attrition, it can be seen that these are indeed limited in number. In this section, we review empirical studies that have used these data and the methods discussed above to assess the relationships between various relevant concepts in travel behaviour research. We will look into the relationships between psychological variables (attitudes) and travel behaviour, and between travel behaviour and health/well-being.

4.1 Attitudes and Travel Behaviour

While studies from the late 1970s were already attempting to investigate bidirectional relationships between psychological variables (attitudes) and travel behaviours using cross-sectional data (Dobson et al., 1978; Tardiff, 1977; Reibstein, 1980), only a few have empirically explored these reciprocal effects using panel data (Thøgersen, 2006; Kalter et al., 2021; Kroesen et al., 2017). These will be discussed briefly below.

Thøgersen (2006) developed and estimated a three-wave, cross-lagged panel model using data from 1300 Danish residents. In the model, the constructs of the TPB were assumed to have synchronous effects on public transport use, while public transport use was assumed to have lagged influences on the TPB constructs. The model revealed significant effects in both directions. According to Thøgersen, the reverse effects (from behaviour to attitudes) could be explained by learning (i.e. people update their perceptions based on experiences) and/or self-perception theory – the notion that people infer their attitudes from their behaviour (Bem, 1972).

More recently, Kroesen et al. (2017) developed two-wave CLPMs considering car, public transport and bicycle use, and the respective attitudes towards these behaviours. Using data from a (representative) panel of Dutch respondents, these authors also found significant effects in both directions. Surprisingly, effects from behaviours on later attitudes were found to be larger than the other way around. Kroesen et al. used cognitive dissonance theory to explain these findings. This theory assumes that inconsistencies between attitudes and behaviour lead to a state of psychological discomfort, which people try to reduce by either adjusting attitudes or adjusting behaviour. It seems plausible that, when people cannot adapt their behaviour, they will change their attitudes.

Finally, similar to Thøgersen (2006), Kalter et al. (2021) developed a three-wave CLPM, but one with random intercepts. Like Kroesen et al. (2017), separate models were estimated for different modes. Data to estimate the model were drawn from the Mobility Panel Netherlands (2014–2016). Again, bidirectional effects were revealed between mode preferences and behaviours; and, similar to Kroesen et al., the frequencies of mode use were found to have stronger effects on (later) mode preference than vice versa. Since Kalter et al. specified an RI-CLPM, these cross-lagged effects can be interpreted as within-person carry-over effects from one occasion to the next (i.e. the level at which the psychological processes actually take place).

Synthesising the results above, reciprocal effects likely exist between travel attitudes and behaviours, although more panel studies are needed to confirm this. The empirical results also point to the need for a new dynamic theory of travel behaviour, which accounts for the reciprocal effects. We will return to this point in Section 5.

4.2 Travel Behaviour and Health/Well-Being

Similar to the attitude–behaviour relationship, it is theoretically plausible that bidirectional effects exist between travel behaviour – and in particular the use of active modes (walking and cycling) – and physical and mental health. For example, considering the body-mass index (BMI) as a potential health outcome, the physical energy balance explains why active travel may reduce obesity (or help maintain weight). Yet, since physical activity is more strenuous for obese individuals than for individuals of normal weight, a reverse effect may also exist. Similarly, active travel may improve mental health (e.g. due to dopamine release), but people who feel mentally well may also be inclined to walk and cycle more.

Relevant reviews have shown that active travel likely leads to increased overall physical activity and fitness. Still, the evidence for other physical health benefits (including lower risk of coronary heart disease, cancer, and obesity) is limited, mainly due to weak study designs (Oja et al., 2011; Wanner et al., 2012; Saunders et al., 2013). Generally, there is a call for more intervention studies; but, as argued above, these may be affected by selection effects.

Observational panel data may therefore offer a solution. In this regard, some studies have used longitudinal designs to assess whether active travel (over time) leads to better physical health – for example, looking at BMI (Martin et al., 2015; Mytton et al., 2016a; Flint et al., 2016) – as well as improved mental well-being (Mytton et al., 2016b; Martin et al., 2014). Generally, these studies report significant effects on the considered physical and mental health outcomes. Yet, bidirectional effects are typically not explored, and therefore provide no information as to whether the effects really operate from active travel to health and/or in the other direction. The first author was involved in two studies that did consider bidirectional effects, which will be discussed briefly below.

Kroesen and De Vos (2020) estimated an RI-CLPM using data from ten waves (years) of the Longitudinal Internet Studies for the Social Sciences (LISS) panel to assess the bidirectional effects of walking, on the one hand, and BMI and mental health on the other. The study showed that walking did not lead to lower BMI over time; instead, people with higher BMI were found to walk less at a later point in time. For mental health (assessed via the short-form mental health inventory), positive effects were found in both directions. De Haas et al. (2021) also estimated RI-CLPMs using data from three waves (years) of the Mobility Panel Netherlands (Hoogendoorn-Lanser et al., 2015), focusing on the reciprocal relationships between walking and cycling (including e-bike use) and BMI and self-reported health. Overall, small effects were found; but, similar to Kroesen and De Vos (2020), results indicated that the effects of BMI on walking/cycling were greater than the other way around. In addition, cycling was found to have a small positive effect on self-reported health, but not vice versa.

Methodologically, the studies by Kroesen and De Vos (2020) and De Haas et al. (2021) can be improved. For example, both studies relied on self-reported measures of behaviour and health, which may be affected by systematic measurement errors (e.g. due to social desirability bias) and/or random measurement errors (due to incorrect recall). Yet, the fact that effects were established in the opposite direction from the one typically assumed (namely from BMI to active travel) supports the recommendation that future research efforts focused on establishing the relationships between travel behaviour and health should explicitly consider effects in both directions.

5 THEORETICAL IMPLICATIONS

So far, we have shown: (1) that panel data offer several advantages over cross-sectional data to establish causation; (2) that these advantages are effectively exploited by recent methodological innovations in panel data models; and (3) that, at present, these models are finding their way into empirical studies on travel behaviour research. Up till now, however, we have not dealt with the theoretical implications of this line of research. Existing theories, such as the TPB, are essentially static in nature and do not explicitly consider the temporal dimension and/or non-recursive effects. It seems logical therefore that, if we are moving towards dynamic empirical models, we should also start considering dynamic theories of travel behaviour.

Recent theorising in travel behaviour research is increasingly focusing on these aspects. De Vos et al. (2022), for example, recently introduced the travel mode choice cycle (TMCC). This framework integrates constructs from multiple social-psychological theories in a dynamic model of travel mode choice. A key feature of the model is that reciprocal effects are assumed to exist between travel choices and travel attitudes, in which travel satisfaction and desires/intentions act as relevant as mediating constructs.

In similar fashion, Van Wee et al. (2019) introduced a theoretical model of attitude change. This model assumes that attitude change may come about due to three (interrelated) processes – namely cognitive, affective and behavioural processes. In turn, these processes may be activated by external triggers, which are categorised into personal, social and environmental triggers. Again, a striking feature of the model is that reciprocal relationships are assumed to exist between various processes – for example, between cognitive, affective and behavioural processes. The authors provide numerous anecdotal examples of such effects.

While these theoretical models provide interesting insights (e.g. provoking thoughts on the causal mechanisms involved), one might question their relevance considering the advances in panel data models. To apply an RI-CLPM it is not necessary to a priori assume that certain relationships exist or not; this is 'learned' from the data. Hence, the (panel data) models essentially allow us to develop bottom-up theories (model structures) from the data. This trend can also be identified in psychology, where it is embodied by the sub-field of network psychometrics (Borsboom and Cramer, 2013; Schmittmann et al., 2013). In psychological network models, it is assumed that the items of a psychological factor function as autonomous entities that causally influence each other within (dynamic) systems that can be formalised as networks consisting of nodes (i.e., the items) and edges (the causal relations between the items). The general aim of network psychometrics is to discover the structure of the psychological networks and the role of individual items within that network (which can be psychological items but also behaviours). Depending on the available data, different types of model are available to estimate undirected and directed psychological networks at the between-person and within-person levels (Bringmann et al., 2013). Recently, Kroesen and Chorus (2020) estimated (both between-person and within-person) psychological network models using data from a mobility survey. To conclude, whereas previously, qualitative data arguably formed the primary source to develop new theories, the current panel data models (and network models) allow researchers to develop theories (model structures) from quantitative data. Indeed, this is a very interesting development.

6 CONCLUSION

In this chapter, we have highlighted the merits of panel data for testing and establishing causality. Panel data help overcome several limitations of cross-sectional data: they allow the researcher to: (1) control time-constant variables; (2) estimate within-person relationships; and (3) estimate reciprocal effects between variables of interest. Unlike true experiments – which also enable researchers to establish causation – panel data do not require manipulation of the 'independent' variable, which in practice may be unethical or difficult to achieve. With the use of panel data, new insights can be gained into the within-person effects between relevant variables. For example, they may show that behaviour is (also) predictive of later attitudes; and that, for some health outcomes (such as BMI), it is more likely that they are causing travel behaviour (the use of active travel) rather than vice versa. In addition, panel data models can be used for theory development; the true model structures can actually be learned from the data.

That said, panel data are not a cure-all in addressing all criteria of causation. Even the most advanced panel data models do not control for time-varying confounding variables; these still need to be measured and included in the models (here, theory also plays a role, namely in identifying relevant third variables). In addition, panel data models still require (theoretical) assumptions about the temporal lags of the (causal) effects between the variables of interest, which are often unknown. Finally, similar to cross-sectional data, panel data cannot shed light on the causal mechanisms between the variables of interest. For this, theoretical thought experiments and qualitative research are required.

Finally, we have treated two general traditions in dealing with panel data – in particular two traditions that focus on addressing the criteria of causation. However, this discussion should not be regarded as a comprehensive review of panel data models. There are many other models that have been developed for panel data, such as (latent) growth curve modes, latent transition models and variations of the cross-lagged panel model (e.g. latent difference score models). These models may offer other benefits, such as being able to capture development processes or/and discrete changes in latent categorical variables. For example, Kroesen (2020) discusses how the latent transition model can be used to test a range of theoretical notions, including the concept of habit (state dependence/inertia), cognitive dissonance/consistency and social influence in the context of travel behaviour research. In addition to these concepts, it would be worth considering and exploring other theoretical mechanisms that may be examined using panel data, such as novelty-seeking behaviour or learning over time (e.g. in the context of cycling).

To conclude, while acknowledging that panel data require much more time and effort to gather than conventional cross-sectional data, we believe these efforts pay off substantially. Panel data help advance the field of travel behaviour research in its quest to understand travel behaviour and how it comes about. This is crucial for the field in order to provide policy recommendations that help steer behaviour in directions that are desirable from a societal point of view.

NOTES

1. We refer readers interested in a more technical discussion to Allison (2009).

2. Note that variables in bold denote vectors.
3. Note that time-variant variables are not controlled for and that time-constant variables are only controlled for insofar they also have constant effects.
4. For a more extensive introduction to the CLPM see Finkel (1995).

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