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Do we necessarily need longitudinal data to infer causal relations?

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Abstract. It is quite uncontroversial that causes precede their effects in time. This usually justifies the preference for longitudinal studies over cross-sectional ones, because the former allow modelling the dynamic process generating the outcome, while the latter cannot. Supporters of the longitudinal view make two interrelated claims: (i) causal inference requires following the same individuals over time, and (ii) we cannot make causal inferences from cross-sectional data. In this paper we challenge this view and offer counterarguments to both claims. We also argue that the possibility to establish causal relations does not so much depend upon whether we use longitudinal or cross-sectional data, but rather on whether the modelling strategy is structural or not.

Keywords: Longitudinal, Cross-sectional, Causality, Time, Structural Modelling.

Contents

1 Introduction: Time and causation

Temporal priority of the cause over the effect has been accepted as a necessary condition for causation (at least) since the seminal work of David Hume (Hume, 1739). For Hume and for most present-day causalists, causes always precede their effect in time. This condition of temporal priority is commonly assumed in demography (Ni Brolchain and Dyson, 2007), in epidemiology (Rothman and Greenland, 1998), or in political sciences (Gerring, 2005), to give some examples. Temporal priority isn't, of course, a sufficient condition for causation. Furthermore, if one sees a putative cause occurring after the effect, this only means that in the present instances the former cannot be a determinant of the latter; this does not preclude the fact that it could be a cause in other contexts in which the putative cause precedes the effect.

Though many philosophers of science, such as Suppes (1970) or Salmon (1984), follow Hume in assuming the temporal priority of the cause, there are however some notable exceptions. Retro-causation or backward causation—i.e., when the cause produces an effect that occurs in the past—is hardly tenable in the social or in the health sciences in particular. It has nevertheless been considered in physics as a possible way of reconciling quantum mechanics with relativity theory (Dowe, 1997, Berkovitz, 2008), although no consensus has been reached so far (for impossibility arguments about backward causation see, for instance, Mellor (1991) and Ben-Yami (2007)). Stronger claims have been made for simultaneous causation, i.e., when the cause and the effect occur at the same time, such as in the case of billiard balls hitting each other, a locomotive driving a caboose, a lead ball depressing a cushion, or a seesaw going up and down, to give some examples from the philosophical literature (for an overview of the discussions about the direction of causation in the philosophical literature, see Shaffer (2008)). Counter-arguments based on relativity theory, on the notion of causal processes, and on the difficulty of replacing the condition of temporal priority by other concepts of causal priority have been developed—for a brief review of the arguments and counterarguments, see e.g. Wunsch (1988, pp. 33-39). In this paper, taking into account the reasons levelled against backward causation and simultaneous causation, we follow the generally accepted view that causes precede their effects in time.

2 Longitudinal studies, cross-sectional studies, and time

As regards the role of time, in the social sciences there are, broadly speaking, two main types of data collection and consequently two types of studies: on the one hand one may follow the same individuals in time (i.e., longitudinal studies), on the other hand one may collect data at a particular point of time (i.e., cross-sectional studies). Longitudinal data can either be

prospective or retrospective. In a prospective study, e.g. a panel study or a disease register, individuals are followed over time, and data on putative causes and their effects are recorded as they occur. In a retrospective study, individuals are questioned on their past experiences, during a single-round survey at a given moment of time. Comparison of the respective advantages and disadvantages of the cross-sectional and longitudinal types of studies for causal inference are discussed later in the paper. Let us make clear from the start that the purpose of the paper is foundational: we are interested in discussing the possibility of drawing causal inferences in each of the two aforementioned approaches when temporal information is explicitly or implicitly available.

Before getting started, a preliminary remark is in order. There is another distinction to be made, i.e. between descriptive studies stopping at the level of *associations* between variables, and studies that instead go further in looking for *cause-effect* relations and *causal* explanations. In this paper we focus on the latter. This is part of a larger project where we defend the following approach to causal modelling as discussed in previous work: Mouchart, Russo and Wunsch (2009), Russo (2009a), Mouchart and Russo (2010), Russo, Wunsch and Mouchart (submitted). Causal relations are fundamentally latent, i.e. not directly observable; consequently, it is problematic, if not impossible at all, to claim that the 'true' causal relations, or that the 'true' causal model, is discovered. On the contrary, we take causality to be interpreted in epistemic terms, namely as the scientist's best interpretation of the results within a given modelling framework (See Williamson 2005, 2006a, 2006b for the epistemic interpretation of causality, Russo and Williamson 2007 and Russo 2009b for an application of the epistemic approach in the health sciences). In causal modelling in social science, such epistemic stance implies two issues. Firstly, the reliability of causal inference essentially depends upon the quality of the strategy of model building, considering the available data. This means that the model should aim at being structural, blending background knowledge and invariance or stability properties with the specification of a causal mechanism, translated into a recursive decomposition of an initial multivariate distribution. Secondly, as a consequence, an algorithmic approach to the automatic detection of causality is considered as condemned to failure because correlation, or statistical association, alone, is not sufficient for uncovering the latent feature of causal relations.

Let us spell out the bulk of the structural modelling approach further. Background knowledge incorporates information that is and ought to be used at all stages of the model building and model testing process. In particular, information about the time ordering of variables guides the construction of the recursive decomposition. As causal relations are fundamentally latent and therefore cannot be directly observed, the bulk of the structural modelling approach lies in the decomposition of a multivariate distribution, specified on the basis of research questions, background knowledge, and data. Thus, the multivariate distribution is decomposed into a recursive sequence of putative cause-effect relationships interpreted as the

mechanism or data generating process. For example, no causal inference can be derived from an observed association between women's education and infant mortality, unless a plausible explanatory mechanism is modelled and tested. The claim of being structural requires a model displaying adequate invariance properties. As a limiting argument, a string of different models each explaining a different observation cannot pretend to reveal the structure underlying the mechanism. In other words, the strategy of model building should endeavour separating an incidental from a structural aspect of the mechanism. As a consequence, the strategy should embody a systematic check for invariance, or stability, both at the level of the sequence of the recursive decomposition and at the level of the characteristics, or parameters, of the involved distributions.

Let us now go back to the issue of time in causal inference. If temporal priority of the cause over the effect is a necessary (but not a sufficient) condition for inferring causation, at the empirical level, putative causes and effects should indeed be ordered in time. In other words, longitudinal data, where the same individuals are followed over time either prospectively or retrospectively, are required for testing causal hypotheses (at the individual level). This is the view commonly held in the population sciences for example, as it enables modelling the dynamic process generating the outcome. However, many data sets are not longitudinal but cross-sectional: individuals are interviewed at a specific point in time and are *not* followed over time, either prospectively or retrospectively. In other cases, even if the data are time-ordered, the succession of events might to some extent be unobservable because events are not recorded on a continuous basis but by discrete time periods. For example, when observing the price and quantity as a result of a commercial contract, it is often the case that the sequence of the bargaining process is not observable. More specifically, it is not known whether the price has been first agreed upon, and then the quantity has been agreed given this price, or vice-versa. In addition, prospective longitudinal data are costly to gather and subjected to loss to follow-up. Many analyses are therefore conducted using cross-sectional or period data. The question immediately arises: Is causal inference impossible in such cases? The common view is that cross-sectional studies assess both putative causes and effects simultaneously and therefore temporal causal relations cannot be identified (e.g. Katz, 2001). In this paper, we want to challenge this view.

We therefore tackle the following question: Do we necessarily need to follow (the same) individuals over time to make causal inferences? The 'shared view' in the social sciences apparently gives a positive answer and makes the following two interrelated claims: (i) we must follow individuals over time and (ii) we cannot make causal inferences from cross-sectional studies. In this paper, we challenge both claims. Sections 3 and 4 present altogether eight counterarguments: counterarguments 1-5 concern claim (i) and counterarguments 6-8 concern claim (ii). Some of the counterarguments apply both to prospective and retrospective studies, while others are only valid for one or the other. Our answer to the question of whether we necessarily have to follow individuals over time relies on a structural modelling framework, where causal inference is based on background knowledge, recursive

decomposition, and invariance. The paper ends with a last section devoted to discussion and conclusions.

3 Problems jeopardising causal inference in longitudinal studies

In this section we discuss the argument that causal inference is possible only by following up individuals over time, in order to take into account that effects occur and therefore are observed after their causes. For example, studying the impact of drinking on self-assessed health would require longitudinal data and the effect would not be determinable from a cross-sectional analysis. Our answer is twofold. On the one hand, the longitudinal approach also faces important problems that jeopardise causal inference. We illustrate this in Counterarguments 1-5. In particular, we show in counterarguments 1-4 that the longitudinal approach suffers from problems that to do not arise to the same extent in the cross-sectional approach.

Counterargument 1: changing behaviours and context

A major difficulty in longitudinal studies–either prospective or retrospective—is taking into account the large number of causes that operate over time, due to changes in behaviours on the one hand, and to modifications of the environment or context on the other hand (Stratford, Mulligan, Downie, and Voss, 1998). The longer the time frame, the more individual and contextual changes one may expect, including changes in the personnel running a prospective study. Let us take the example of following two groups of individuals, one with excess cholesterol levels and the other without, in order to see if high cholesterol levels are a risk factor of cardiovascular diseases (CVD). In the control group with normal cholesterol levels at the onset of the study, some individuals may present higher cholesterol levels after a certain lapse of time due to a change in their behaviour. Moreover, in the group with excess cholesterol levels, some individuals might start smoking (a factor possibly confounding the impact of cholesterol levels on CVD) or, on the other hand, taking drugs in order to counteract the impact of high cholesterol levels. For both groups, in addition, diets and food products may change during the period of the study. It is thus necessary to control for all other factors over time having an impact on CVD, and their possible interactions, in order to detect if high cholesterol levels are a cause of CVD. Actually, many of these other causes remain latent either because they are unknown at the time of the study or because they are not included in the longitudinal data set. In particular, unanticipated future intervening events are by definition extremely difficult to foresee. To give another example, legal measures taken during the observation period, and more generally changes in social norms, may considerably extend or restrict access to e.g. health care, abortion, or immigration. The cause-effect relations in, respectively, morbidity, fertility, and migration, will not be the same before and after these measures were taken. In other words, strictly speaking, longitudinal data alone do not ensure causal inferences, unless

strong assumptions are made. The strongest is that all relevant causes are taken into account during the whole period of observation, prospective or retrospective, and not only at the start. As no follow-up is involved in cross-sectional studies, data requirements can be much less demanding.

Counterargument 2: following units of observation

In some cases, following observational units for making causal inference may lead to problems that are hardly solvable if the units cease to exist, divide, and form new units. To give an example, families or households may split up over time and constitute new families and households (see e.g. J. Duchêne, 1995). Follow-up of the family or household units per se (and not the individuals themselves) is impossible in this case. To give another well-known example, the borders of spatial units–such as municipalities—often change over time; once again, a same stable unit cannot be followed longitudinally. A cross-sectional study does not suffer from these limitations.

Counterargument 3: selection and bias in prospective studies

Prospective longitudinal studies are affected among others[1] by sample selection and by loss to follow up, whether the study is experimental or observational (Imai, King and Stuart, 2008). Consider once again the example of drinking and self-assessed health. Randomisation of the individuals in the sample according to the fact that they drink or do not drink is impossible to achieve here, for ethical and practical reasons; this is the case in most social research, where observational studies predominate. Heavy drinkers might however refuse more than others to take part in the study, especially if the latter is conducted over an extended period of time. Moreover, even if they accept to participate, the follow-up of heavier drinkers might be different from that of the lighter and non drinkers: they may drop out of the study more frequently than the others. If this is the case, the longer the study, the more heavier drinkers will be under-represented in the sample, biasing the actual relationship between drinking and health.

More substantially, one should suspect that the process of self-assessment is associated not only with drinking behaviour but also with participation itself. Consequently, both sample selection and follow up biases call for the necessity of modelling the participation jointly with other variables, rather than considering participation as exogenous, once selection biases become substantial (on the assignment mechanism issue, see in particular D. B. Rubin 2008). This recommendation departs from matching or from weighting procedures, as both are grounded on the contextually questionable assumption of neutral or exogenous selection. Loss to follow-up does not affect, on the other hand, cross-sectional studies and individuals are probably more prone to participate in this one-shot study than in a longitudinal one (Elwood, 1992). In addition, since no tracking of individuals is required,

[1] For example, the interaction between the person interviewed and the interviewer will not be considered here, as this problem falls beyond the scope of the present discussion.

anonymity is easier to ensure than in a longitudinal approach, a fact that might improve participation in the study (de Vaus, 2001). Sample selection biases will therefore usually be significantly smaller in a cross-sectional study than in a prospective one.

Counterargument 4: selection and bias in retrospective studies

Retrospective studies are by definition not hampered by loss to follow-up. Moreover, as they are usually based on a single-round sample, they are less affected by initial assignment bias than the more cumbersome prospective study, though the retrospective questionnaire can sometimes be long and tedious to fill in, thereby possibly discouraging participation.

Only those alive and present in the population of reference can of course be interviewed at the time of the survey. As the saying goes, "dead men tell no tales"! The retrospective approach can hardly tackle therefore e.g. the determinants of death or of emigration[2], a severe limitation indeed. Those who have died or emigrated before the survey might also be different from those still present. For example, survivors are most probably in better health than were those who have died. Vice-versa, emigrants might be in better health on average than those who remain in the country. The prospective approach does not suffer from this handicap, as one can follow those who die or emigrate up till their departure from the reference population.

Contrary to prospective studies, where events are recorded as they occur in time, retrospective studies suffer from memory failures or recall lapses, in particular voluntary or unconscious omissions and errors in localising events in time, that can be more or less severe depending on the topic of the study and the length of the reference period. In addition, except if a short common reference period is chosen, the duration of observation varies from one individual to the other according to his/her age at the time of the survey (Tabutin, 2006). Recall lapses are less severe in cross-sectional studies, where no time frame is envisaged.

Counterargument 5: lack of continuous temporal recording

In a longitudinal study of individuals, in most cases no information is available on expectations and intentions. Those are important because they may be a major part of the mechanism. For example, divorce may occur after the couple opts for independent residences, suggesting that this event causes divorce. However, it is the *intention* to eventually divorce that leads firstly to change residence and subsequently to divorce (Dieleman and Schouw, 1989). Thus even if we see in a longitudinal data set that change of residence precedes divorce in time, we shouldn't conclude that the former is

[2] Some information on the causes of death or emigration might however be collected from the circle of family and friends.

the cause of the latter. Instead, in this example it is the intention and the decision to divorce that causes both the change of residence and divorce. This is a case where a temporally ordered relation between two variables (change of residence and divorce) is mistakenly interpreted as causal.

In other cases, only rudimentary temporal information is available in the prospective or retrospective data set, such as the level and the date respondents attained their highest education degree, instead of the complete education history. In this situation, it is difficult to thoroughly analyse for example the effect of education on entry into motherhood, in cases of irregular educational pathways (Zabel, 2009).

In other situations, even though longitudinal data might be collected, events are not recorded on a continuous basis, discrete time periods being used instead. Depending on the length of the window of observation, it is not always possible to state if the putative cause occurred before or after the effect. For example, if migration and occupational change are recorded on a yearly basis in the longitudinal data set, one does not know on an annual basis whether migration has occurred before or after occupational change, if both have happened during the year. To illustrate, let us consider the example of commercial contracts sketched in section 1. Consider the typical situation in the tour-operator market. Suppose that the tour-operator, acting as a price setter, print in January a catalogue for the coming season. If the price is not altered within the year, the quantity observed on an annual basis may be assumed to have been contracted by a price-taker demand side, and the price construed as an exogenous factor of the demand. But if the price is modified, say around Easter time, annual data will not allow disentangling the demand from the supply, even under a price-taker demand, as long as different quantities have been contracted under different prices and the price changes have been operated under the pressure of the demand. Cross-sectional studies do not of course fare better here, except if detailed retrospective information is collected.

4 Causal inference in cross-sectional studies

In this section we show that causal inference is possible, to some extent, in cross-sectional studies, which, in addition, have some advantages compared to longitudinal ones. We argue, in particular, that structural modelling on cross-sectional data does make use of time, although most often implicitly. We do not deny that temporal knowledge (i.e., causal priority and causal ordering) is needed, but we argue that structural modelling on cross-sectional data can incorporate such knowledge in different formats.

As recalled in section 2, the bulk of a structural model lies in the recursive decomposition of a multivariate distribution built upon background knowledge and preliminary analyses of data. In the case of longitudinal data, observations in time tell us what the time ordering of the variables is, and

consequently tentatively suggest the recursive decomposition (see however counterargument 5). In the case of a cross-sectional data set, the time ordering of the variables is not explicitly given, except if detailed retrospective information is gathered at the time of the survey. A purely cross-sectional study with no retrospective data can only give prevalence but not incidence measures. The latter can however be estimated, but under strong assumptions. For example, in the case of a closed population and a disease which is rather stable over time, one may estimate the disease incidence by the ratio 'prevalence of persons sick'/'average disease duration' (Jenicek and Cléroux 1982, p. 50). For a more complex example of a model applied to HIV incidence, taking into account a times-series of prevalence data, see Sakarovitch et al. (2009).

Nevertheless, in a recursive decomposition, time may be implicit— that is embedded in background knowledge—and some cause-effect relations might be identified (and possibly estimated) cross-sectionally (Beaglehole, Bonita and Kjellström 1994). As D.R. Cox (1992) states, subject-matter knowledge may be used to establish the presumed causal ordering of variables. Sometimes, ordinary logic may reveal the order of events in time, such as in the example on drug consumption among students given by Babbie (1986, p. 82).

Counterargument 6: time in the assumptions on the basis of background knowledge.

Some mechanisms are embedded in time, for instance physiological mechanisms. A pure cross-sectional study can give information on the relationship between hearing and age: as one ages, hearing loss increases, the converse being rather rare! To give another example, smoking at time t causes cancer at time t' (t' > t) but not the other way round. In this example, medical knowledge supports causal and temporal ordering. A cross-sectional case-control study can, moreover, confirm that there are more persons who have smoked among cancer patients—the cases—than among those who do not have cancer—the controls (Jenicek and Cléroux, 1985). When a disease–or more generally an effect—is rare, there may be no one with the disease even in a large sample. In this situation it might be better to study a cross-sectional sample of individuals who already have the disease (i.e. cases). For example, it was found in 1983 that of 1000 patients with AIDS, 727 were homosexual or bisexual men and 236 were intravenous drug abusers; the conclusion that individuals in these two groups had a higher AIDS risk was inescapable (Mann, 2003). This conclusion was later confirmed by longitudinal studies.

On the negative side, if there are cohort effects, a sole cross-sectional study will however not be able to detect them. For example, contrary to hearing loss[3], the age-pattern of smoking reflects both age and birth cohort

[3] Though high listening levels through headphones may deteriorate hearing in the younger generations.

effects, as smoking is less frequent in recent male cohorts. In addition, a single cross-sectional study with no retrospective information cannot give us the time-pattern of causes and effects (Blossfeld, 2009).

The impact of drinking on self-assessed health is more difficult to assess from a cross-sectional survey, though here again–as in the case of smoking–one can collect some information on drinking behaviour in the past and on the reasons the respondent may give for his/her drinking behaviour. On the other hand, poor self-assessed health may lead the person to drink, contrary to the previous example where having cancer does not lead the patient to smoke. The causal relation is reversed in this case and it becomes difficult to conclude on the basis of cross-sectional data to one or the other of these relations, except if the information one has is clearly in favour of one direction of causation instead of the other. For example, if most respondents answer that they drink because of their poor self-assessed health, the putative causal relation would go from health to drinking and not vice-versa.

Some variables do not change or hardly change during one's lifetime; one can define them as permanent or quasi-permanent properties of the units of observation. In such cases, longitudinal studies hardly shed further light on these properties. In a cross-sectional study of the self-assessed health of adults (see C. Gaumé, 2009), one can check if self-assessed health differs between males and females, among ethnic groups, or among educational classes. One can also see if physical health or social support have an impact on self-assessed health, as one can assume that the causal relation usually goes from physical health and social support to self-assessed health and not vice-versa. The same can possibly be said for locus of control, another probable factor of self-assessed health. All these determinants most probably exist prior to the evaluation of one's health at the time of the survey, and there is no plausible reason to believe that reverse causation generally occurs in these cases. To give another example, one may examine with a one-shot survey the impact of school-leaving age on occupational attainment, as the latter occurs after the former. Actually, if both causes and effects are stationary over time, a cross-sectional survey may be adequate (Elwood, 1992). It is well-known in demography, for example, that in a stationary population (i.e. a closed population with constant fertility and mortality schedules by age over time) the period life table is identical to the cohort one.

Counterargument 7: sample size

Prospective longitudinal studies are difficult and very expensive to carry out, especially if the observation period is long. Sample sizes are therefore usually rather small. Cross-sectional samples do not suffer from these limitations and can be much larger. We can thus expect sample selection error (the difference between individuals in the samples and those in the general population) to be smaller in the cross-sectional study than in the prospective one. Moreover, smaller sample size produces less statistical power. Therefore, confidence intervals and p values will usually be larger in the smaller data set. This is a main reason why meta-analyses are preformed

in experimental research: when possible, data from similar studies are pooled together with the objective of increasing statistical power (Borenstein et al 2009). The issue of course is finding multiple *and* highly comparable data sets. It is true, however, that in a longitudinal survey intra-cohort variance is often smaller than inter-cohort variance. Consequently, confidence intervals might be smaller in a cohort study than in a period study.

Counterargument 8: invariance

A model is 'structural' if the recursive decomposition remains coherent with background knowledge (taking into account all available evidence) and invariant across different partitions of the data set and/or across similar populations[4]. Replication of the study is therefore important for testing the invariance of the results in order to reach 'explanatory generalizations', in Woodward and Hitchcock's terms (2003), for the reference population (e.g. the present population of Belgium, patients suffering from Alzheimer's disease, etc, depending on the purpose of the study). This requirement was already pointed out seventy years ago by Yates and Cochran (Yates and Cochran, 1938, cited by Cox, 1992). Due to the fact that they are much less expensive and easier to conduct, cross-sectional studies can be replicated more often than prospective longitudinal ones and invariance can therefore be more easily checked. For example, in the cross-sectional study by Gaumé (op. cit.), results from the same structural model were very similar for the three countries and the two time-periods considered. This stability over time and space reinforces one's confidence in the adequacy of the underlying structural model.

5 Discussion and Conclusion

It is widely agreed both in philosophy and in the social sciences that causes precede effects in time. Leaving aside the metaphysical issue of whether temporal priority is an essential fact of Nature or whether it is simply due to our epistemic access to causal relations, we tackle in this paper an issue concerning the methodology of causal inference.

A well-known stance is that causal inference is possible only if we can follow the same individuals over time because this way we can observe, model and test the dynamic process that generates the outcome. A consequence of this view is that cross-sectional studies cannot establish causal relations. In this paper we challenged this view and offered counterarguments to the claims that (i) we must follow individuals over time and (ii) we cannot make causal inferences from cross-sectional studies. A first range of counterarguments aimed to show that longitudinal studies also face serious threats for making causal inference, threats which are often much less severe in cross-sectional studies. A second range of counterarguments aimed to

[4] This does not preclude the fact that a causal explanation can be given for an event or situation that is unique.

show that cross-sectional studies can establish causal relations under certain conditions, notably when temporal information is encapsulated in background knowledge and used in the assumptions of the model. Some concluding remarks are now in order.

First, whether we can establish causal relations or not does not so much depend on whether we access longitudinal or cross-sectional data, but rather on *whether our modelling strategy is structural*. Structural modelling, as we explained in section 2, is a general methodological framework for causal analysis: the possibility to establish causal relations lies (i) in the use of background knowledge, (ii) in the specification of the recursive decomposition of a multivariate distribution interpreted as a causal mechanism, and (iii) in the positive results of invariance tests.

Second, the argument according to which longitudinal studies are the only way to make causal inferences because they take temporal order of the cause-effect relation into account is ill-founded. This is for two reasons. (i) Following individuals over time in longitudinal studies is no absolute guarantee of causal inference since specific problems arise (counterarguments 1-5), some of which occur to a much lesser extent in cross-sectional studies (counterarguments 1-4). (ii) Cross-sectional studies are indeed able to take temporal order into account, when temporal information is implicit (counterargument 6). Moreover, cross-sectional studies allow, as they are much less costly, larger sample sizes and more invariance tests than prospective studies (counterarguments 7-8).

Third, we are not claiming that we can *always* infer causal relations from cross-sectional data. Actually, in many cases we can't because there is no implicit information on the temporal ordering of causes and effects. Yet, in many other cases we can, to some extent, if a sound structural modelling encapsulating time information is performed. Finally, we do agree that if we have longitudinal data, so much the better, especially for studying changes over time. But if we only have cross-sectional data, we have to make our best to go beyond the descriptive level by adopting, as much as possible, a structural modelling framework. Of course, some will disagree about such an explicit causal stance, but arguably, *causal* relations are required for understanding and forecasting social phenomena and, consequently, setting up sound social policies.

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