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The Idiographic Approach: Where Do We Come From and Where Do We Go?

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In this interesting and thought-provoking article, Molenaar uses the concept of ergodicity to demonstrate that focusing solely on interindividual variation might lead to erroneous conclusions about the intraindividual variation of psychological phenomena. Regarding the article, I want to make two general comments: The first comment is related to the absence of a literature overview in the article and the second comment is about the reconciliation of an intra- and interindividual approach.

With his manifesto, Molenaar joins a procession of psychologists who have expressed the same concern throughout the history of psychology. For this reason, it is somewhat unfortunate that Molenaar fails to recognize his predecessors who argued for a person-centered or idiographic approach to psychology. Especially in the domain of personality and clinical psychology, the suggestion of studying intraindividual variation dates back to at least Allport (1937) and since then it has been rearing its head regularly (see e.g., Appelt & Strauss, 1985; De Boeck, 1988; Luborsky & Mintz, 1972; Pelham, 1993; Stephenson, 1953). Although the debate in experimental psychology has been less pronounced (or less ideological if you want), single-participant studies have been carried out in operant condition experiments by Skinner (1938), but they are also very common in studies on simple decision processes (e.g., Ratcliff, Van Zandt, & McKoon, 1999). Moreover, the special case of self-experimentation (in which the participant and object of the scientific

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enterprise are the same person) has been employed by Ebbinghaus (1885) and its merits have recently been brought back to attention by Roberts (2001, in press).

It may seem nitpicking to mention that Molenaar did not include a literature overview, but I think it is important to add an historical perspective to the discussion. Realizing that the idiographic approach regularly rings the doorbell of scientific psychology, one can validly ask the question why does no one open the door to welcome the guest? Molenaar is right when he writes that in the overwhelming majority of studies in psychology, attention is restricted almost exclusively to variation between individuals. So we are confronted with the situation that theoretical and empirical studies often appear in the literature to promote the idea of an idiographic approach, but mainstream research is still focused on interindividual differences (or even plain averages).

I see several reasons why an idiographic breakthrough has failed to materialize. First, there is an economical and organizational reason. It is often easy to find several individuals who are prepared to participate in a one-time psychological experiment. It becomes harder if one wants to contact them for follow-up meetings. The task of collecting extensive time-series data from a single person is in many cases considerably more difficult (except for simple tasks, as is often used in experimental psychology). Secondly, the operationalizations used in psychology often hamper repeated assessment. For instance, responding to a mood questionnaire can be done on a daily basis without much difficulty, but repeated measurements using an ability test may lead to unpleasant complications (e.g., learning and memory effects occur and they have to be taken into account but are actually considered as a nuisance). It is my guess that this was also one of the reasons behind the paradigm shift by Lord and Novick (1968). A third reason for the relative absence of the idiographic approach at the scientific forum is because of methodological pitfalls in making causal inferences. Starting from the Rubin–Rosenbaum–Holland (Holland, 1986; Rosenbaum & Rubin, 1983; Rubin, 1974) approach to causality, one has to make the stable-unit-treatment-value assumption (SUTVA; Rubin, 1986). The most relevant threat to this assumption in the idiographic approach is that there may be interference between the units involved in the experiment or quasi-experiment. In a genuine experiment with several participants randomly assigned to the control and treatment condition(s), interference between the units (the participants) offers no problems in most cases. However, this is different for a single person's time series where the effect of a treatment given at time t_1 may still have an influence at time t_2, which is assigned to the control condition. A fourth, and maybe the most fundamental reason, is that the larger part of theorizing in psychology at this moment is inherently static. Change and the modeling of it is all around in physics, whereas (stochastic) differential equations (and systems thereof) are lacking almost completely in psychological modeling. This state of affairs illustrates the fact that there is a shortage of adequate theories of behavioral change.

Turning to my second comment, one may get the impression from reading Molenaar's manifesto that he dismisses the interindividual modeling approach. In his conclusion he states that we should consider looking at intraindividual variability as complementary to looking at interindividual variability. What I get from Molenaar's article is that we should develop models that are able to give a joint description of both intra- and interindividual differences. The fact that some person's behavioral space consists of two dimensions, whereas for others there are four dimensions, automatically bears the question how this difference can be explained. Which between-person factors are related to the different dimensionalities of the person's behavioral spaces? A possible model to consider here is to have a mixed distribution at the between-person level for which the components have different dimensionality (possibly with the restriction that the dimensions of a lower order component are a subset of the dimensions of a higher order component).

It may even be the case that paying attention only to intraindividual variation deceives us. For instance, suppose that personality consists of two factors: Extraversion (vs. Introversion) and Agreeableness (vs. Antagonism). Let us consider the movement of two persons (A and B) in this two-dimensional space. The position of person A on the Extraversion axis does not change throughout time, but the position for Agreeableness does so in a linear way (moreover, some random error is added to the successive positions). For person B, the opposite holds. When factor-analyzing the time-series data from such persons, one would get a one-dimensional solution in both cases. But the two separate dimensions actually mean something very different and they can be, and preferably should be, linked in the original two-dimensional interindividual behavioral space.

It seems to me that part of the problem comes from the somewhat confusing definition of an ensemble. One could view each individual person as a separate stochastic system producing (for a certain one-dimensional dependent variable) a sample path. The collection of all possible sample paths for this individual system can then be seen as the person-specific ensemble. The particular stochastic system corresponding to a person can be ergodic or not. The task of scientific psychology is then to describe the individual time series of a sample of stochastic systems (the participants, i.e., the intraindividual perspective) and explain how they differ (i.e., the interindividual perspective). However, Molenaar considers the collection of all possible sample paths from the population of persons as an ensemble. Even when each individual stochastic system is ergodic in this case (e.g., suppose that the time series is autoregressive of Order 1), taking them all together does not necessarily result in something that is ergodic. As a simple example, suppose that the mean levels around which the first-order autoregressive processes develop in time differ over persons. The average of an individual time series is in this case not linked with the cross-sectional average over the ensemble, unless the construction is extended with a distribution of the mean

levels in the population of participants, thereby adding an interindividual component to the model. In sum, I believe that in an idiographic approach, describing and explaining how and why individuals are different from each another is still an important topic on the research agenda.

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