Are we on the same page? Latent variable modeling suggests different nomothetic and
idiographic factor structures for momentary affect

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Abstract

Objective: This study investigated the factor structure underlying affect and whether it differs within and between persons. **Method:** In an exploratory study, 12 datasets of self-reported momentary emotions collected via experience sampling method (11 datasets) and daily diaries (one dataset) were analyzed using graded response models based on item response theory. Before doing so, two pools of datasets were extracted so that the intraindividual (sampling all measurements of a person) and interindividual (sampling measurement occasions of different individuals) variations are disentangled. Various factor models were then fitted to each pool via an alternative-model confirmatory factor analysis. The candidate models were compared according to their relative goodness of fit. The best-fitting models were defined using a vote-counting method. **Results:** The intrapersonal momentary variations of affect can be best explained by a unidimensional factor structure resembling valence while individual differences are explained by a two-dimensional construct similar to positive and negative activation constructs. **Conclusions:** The structure of change in affect is not the same between and within persons. This implies that the nomothetic and idiographic analyses do not converge, providing evidence for construct nonequivalence between the two levels of analysis.

Keywords: nomothetic and idiographic analyses; factorial structure of affect; individual differences; experience sampling method; graded-response item response theory.

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Introduction

Historical Background to Idiographic and Nomothetic Paradigms and Controversies

Philosopher Wilhelm Windelband, in an address given at the University of Strasbourg in 1894, made a distinction between two forms of evidence-based human knowledge: one that pursues explanation of a specific phenomenon; and the other that seeks “generalities that are common to a class of particulars and deriving theories or laws to account for these generalities.” (Robinson, 2011, p. 32) He coined the words *idiographic* and *nomothetic* for them, respectively (Windelband, 1980; Windelband & Oakes, 1980). Soon after that, Tufts (1895) introduced the terms to American psychologists. The dichotomy of the terms in psychology, however, was taken seriously only after Münsterberg's speech (1899) right before the turn of the century. He proposed to take individual cases into the focus of the new field of psychology. This focus on the case-by-case study of individuals, later known as the *Wundtian* methodological approach, was the common scientific practice in experimental and quantitative psychology as well as qualitative and clinical studies until the 1930s. Later, Allport (1937), a student of Münsterberg, revived and popularized the distinction in his seminal work, *Personality*. Although he was ardent to the idiographic approach, he rejected the dichotomy since he believed that it is “too sharp: it requires a psychology divided against itself” (Allport, 1937, p. 22; as cited in Mos, 1998, p. 39)¹. Quite ironically though, his *Personality* gave rise to what later became known as the *Galtonian* methodological approach: the line of research that advocated the study of psychological phenomena at group level where statistical analyses were conducted over a sample of persons. Galtonians quickly gained reputation and later, by marginalizing Wundtians, dominated quantitative behavioral

sciences. As a result, *person* was absent—or more precisely, *lost*—in the mainstream research in scientific psychology for decades.

This, by and on itself, might not seem catastrophic for a scientific discipline to many scientists. However, one should have in mind that psychology, by definition and as emphasized by Münsterberg and the late Allport, is a science of studying individuals. Galtonians were successful in convincing the field that the nomothetic approach ultimately contributes to the principal goal of psychology since the individuals are instances of the humankind and the same laws should, “in principle,” govern their cognition and behavior. This promise did not come true, though; psychology has not been yet able to reach a comprehensive theoretical framework like, for example, physics. This failure has been long noted by scientists since the golden era of psychoanalysis and behaviorism (e.g., Jastrow, 1935) to the recent decades (e.g., Lykken, 1991; for a more detailed account see Teo, 2005). That, to a noticeable extent, is because structural differences of individuals are “averaged out” in the nomothetic approach (Lykken, 1991). This has been a concern of many, including Cronbach (1957). A detailed historical review of the attempts to materialize the integration of the two disciplines is discussed in (Borsboom, Kievit, Cervone, & Hood, 2009).

The idiographic/nomothetic dichotomy was mainly used in the context of Wundtian vs. Galtonian paradigms of scientific psychology. Cattell (1952) was the first to look at the controversies through a more methodological lens. He suggested that any psychological measurement can vary across persons, occasions, and variables. He used the metaphor of the *data box* to think about the relationship between the sources of variations. He also coined six terms (namely *O-*, *P-*, *Q-*, *R-*, *S-*, and *T-techniques*) for various ways of analyzing the relations between variations of measurements found. Among those techniques, *P-technique* and *R-technique* were the most prevalent in quantitative analyses as they correspond to factor analysis at the within-person (WP) and between-person (BP) levels, respectively. In P-

technique analyses, the measurements of a set of variables for specific individual(s) are pooled over all occasions. Similarly, for R-technique, measurements of a set of variables at certain occasion(s) are polled across all individuals (for a detailed treatment of the matter, see Cattell, 1988). Cattell's terminology allows distinguishing between the dominant theme of research (i.e., being idiographic or nomothetic) and the operationalized methodology of the study.

The focus on the top-down approach (i.e., inferring idiographic properties based on nomothetic analyses) has made the implicit assumption of the equality of constructs (and subsequently, processes) between those levels. The assumptions so intuitive that Bem and Allen (1974) called "nomothetic fallacy," which "resides in the fact that this entire research tradition is predicated upon nomothetic rather than idiographic assumptions about the nature of individual differences." (p. 508) Although this deceptive assumption had been widely taken as granted in psychological sciences, it has raised quite a few concerns in social sciences and econometrics—in different ways though. In the former, the objections were around the fact that interrelation of the findings derived from the nomothetic and idiographic approaches was not clear enough (Blalock, 1967), and in the latter, the validity of cross-sectional estimates in time series analysis was questioned (Kuh, 1959). The debate on the relation of nomothetic and idiographic analyses has been revitalized in recent years. Molenaar (2004), for instance, with solid mathematical proofs based on classic ergodic theorem, argued that "only under very strict conditions—which are hardly obtained in real psychological processes—can a generalization be made from a structure of interindividual variation to the analogous structure of intraindividual variation." (Molenaar, 2004, p. 201) In a later work, Molenaar (2008) elaborated on the drastic consequence of the (rather bold) nonergodicity of psychological processes. Ergodicity strictly requires the processes of the ensembles (of individuals) to follow the same dynamics (ensemble homogeneity) exactly, and

the individuals maintain constant statistical characteristics over time (stationarity of individual processes). Any violation thereof invalidates the generalizability of the results obtained from the standard analyses at within- and between-person, i.e., P- and R- technique. Molenaar argues that individuals usually do not have stationarity in their trajectories within the behavior space, and the ensembles (groups of individuals subject to the R-technique analysis) are heterogeneous. In fact, those violations are the primary reason that P-technique and R-technique could be applied as a means of the analysis of covariations in the first place. As a result, paradoxically, slicing Cattell's (1988) Data Box, which has been the main (if not only) methodological tool to discover the hidden sources of variations in the measurements, happens to be the sole cause of this problem. Molenaar (2008) further discusses the implications and consequences of this (mostly neglected) issue and the way they shadow over fundamental assumptions and methodologies in psychometrics—from classical test theory to factor analysis and the analysis of developmental processes.

Efforts to Bridge the Paradigms

One proposal to solve the matter has been the adoption of a bottom-up approach to the generalization: reaching an overarching nomothetic understanding of the processes based on idiographic constructs and measurements. Although this seems to be a promising approach, it is typically neither wanted nor possible; a central pillar of (modern) psychometrics is measurement invariance (MI), which implies that the instruments have to measure the same constructs across samples and measure them similarly. MI is discussed in more details in the next section. MI casts doubts on this form of generalization as it seems paradoxical; idiographic constructs and measurement models are, by definition, tailored to individuals. As a result, bringing the person-specific measurement models to the higher levels of interindividual differences is impossible without violating measurement invariance—unless certain criteria, most importantly, ergodicity, are met. There have been efforts to resolve this

–apparently– inevitable paradox (e.g., Lamiell, 1981; Nesselroade, 2007; Nesselroade, Gerstorf, Hardy, & Ram, 2007). The more recent attempt of Nesselroade et al. (2007) to solve the matter, however, has received substantial criticisms (viz., Borkenau, 2007; Borsboom & Dolan, 2007; Hamaker, 2007; Loehlin, 2007; Schaie, 2007; West & Ryu, 2007; Willoughby & Sideris, 2007).

Another line of research in the field, reflected in a handful of published articles in the past decades, tried to counter the issue from another angle: by focusing specifically on measurement invariance across the two levels, i.e., by developing measurement models that are not significantly different for the constructs believed to be shared at the idiographic and nomothetic levels, and quantifying the degree to which the current models are invariant at WP and BP levels. Lebo and Nesselroade (1978), using a 75-item instrument, studied mood changes of five pregnant women over 120 days to answer two main questions: can the previously obtained state dimensions of mood found in cross-sectional R-factor analyses (namely energy, well-being, fatigue, concentration, social affection, anger-hostility, depression, anxiety) replicate in the P-factor analysis; and whether the loadings of the P-factor analysis are comparable among individuals. They found that the first four dimensions of R- and P-technique analyses converge to some extent, but there is mixed (and not conclusive) evidence for their second research question. Building on Lebo and Nesselroade (1978), Zevon and Tellegen (1982) conducted similar research on a bigger sample of both men and women (23 individuals, 11 males) and measured their positive and negative affect over the course of 90 days. Using various quantitative measures, they report remarkable consistency among the individual factor loadings for 19 subjects, for whom they also find strong congruency between the intraindividual and cross-sectional factor structures. The more recent instances of this line of research open doors to more systematic ways of quantifying (and thinking about) the WP-BP invariance. Voelkle, Oud, von Oertzen, and Lindenberger

(2012) introduced a maximum likelihood estimation of dynamic factor model (DFA) for multivariate time series of a population that fills the gap between the standard time series analysis (where the focus is on a single individual, neglecting interindividual differences) and structural equation modeling (where the number of measurement occasions is relatively low, and the interindividual variation is the matter of interest). Using their approach, one can model temporal, within-person variations together with between-person variations at the same time. Their model also quantifies the degree of ergodicity, which is an indicator of the degree of factorial (non)equivalence between the WP and BP levels. Building on Voelkle et al. (2012), Voelkle, Brose, Schmiedek, and Lindenberger (2014) introduced the concept of conditional equivalence as a means of assessing the partial equivalence of the WP and BP constructs. Based on that measure, they construct likelihood planes which allows identifying the source of variation accountable for any kind of (partial) factorial nonequivalence. Finally, Brose, Voelkle, Lövdén, Lindenberger, and Schmiedek (2015) investigated positive and negative affects in the 100-day daily diaries of 101 younger adult participants of the COGITO Study (Schmiedek, Lövdén, & Lindenberger, 2010). They report noticeable divergence of individual WP constructs from the shared BP DFM. This divergence, they state, is an indicator of interindividual differences and can be predicted by stress and psychological well-being of the individuals.

The studies comparing WP and BP constructs mentioned so far have two things in common: they posit construct equivalence between the intraindividual and interindividual factors (IAF and IEF, respectively); and they use linear factor models for discrete-valued, categorical measurements. These two issues constitute the point of departure for the current study, which is discussed in the next section.

The Current Study: Research Questions and Novelty

The current study aims for two main contributions: addressing the overlooked question of construct equivalence (and if that is the case, configural invariance) between the WP and BP levels; and using a latent trait modeling method that is suitable for rating scale responses. Before proceeding, we need to make a distinction between two terms—configural invariance (CI) and construct equivalence (CE). In psychometrics, CI is used in the context of measurement invariance (MI). MI is a property of psychometric instruments that assures that the estimates of latent variables for similar response patterns taken from different ensembles are comparable. For example, if a test for (latent) mathematical ability has MI, it functions similarly across different groups as such the estimates of the ability only depend on the responses and not group membership (“Measurement Invariance,” 2018). In the context of educational measurement, the focus of MI is on reducing differential item and test functioning (DIF and DTF, respectively) across different groups (“Differential Item Functioning,” 2010). In the context of MI, it is implicated that the instrument is measuring the same construct (e.g., mathematical ability), and the instrument has to function similarly for all groups. In the broader literature of psychometrics and latent trait modeling, MI is attributed to factor models, and the DIF/DTF of the instrument are not in the spotlight anymore. In social sciences, for instance, MI can stand for three distinct, yet related, concepts: configural, metric, and scalar invariance (Brown, 2015, Chapter 7). CI addresses equal form of the factor structures, i.e., having the same number of latent factors and an identical pattern of loadings between the factors and the indicators (Brown, 2015, pp. 242–243). It is crucial to note that CI does not entail equivalence of the factors; two factor models can have similar factor configuration when fitted to two samples with by the same indicators (i.e., manifest variables) yet measure two distinct constructs—CI is essentially blind to the content of the constructs in question hence cannot speak of any *equivalence* relation between

them on its own. In pure R-technique (or P-technique) analyses, if the samples are taken from a homogeneous (or comparable) sample, then CI implies CE; Ockham's Razor rules out the possibility of an external effect that might prevent such implication. For instance, if R-technique factor analysis on two samples shows identical factor configurations (i.e., CI), then one can conclude that the constructs estimated via the analysis are equivalent in both populations (i.e., CE).

As we have seen earlier in the introduction, many psychologists are easily deceived by the nomothetic fallacy. As a result, it is not out of the realm of possibilities that an instrument can measure two distinct latent constructs in the P-technique and R-technique analyses. On the contrary, it is quite plausible—the variations of measurements in the data box can be jointly caused by *state factors* and *trait factors*, accounting for intraindividual and interindividual variations (IAV and IEV, respectively). These models have been studied in the structural equation modeling (SEM) since at least 1990s when Kenny and Zautra (1995) introduced their trait-state-error model (TSE). The more recent development of TSE models, which led to trait-state-occasion models (TSO; Cole, Martin, & Steiger, 2005) can be found in Prenoveau (2015). In the current study, to investigate the factor structures at WP and BP levels, P- and R-factor analysis were conducted on two pools of samples in which the WP and BP variations were disentangled. As a result, the P- and R-techniques only modeled the within- and between-person variations. The details are discussed in the method section.

To answer the research question, the current study used item response theory (IRT), which is well-suited to model discrete-valued measurements. As noted earlier, the data used in the previous studies of MI across WP and BP levels (viz., Brose et al., 2015; Lebo & Nesselroade, 1978; Voelkle et al., 2014, 2012; Zevon & Tellegen, 1982) were collected using polytomous, Likert-like surveys. Van der Eijk and Rose (2015) have shown that factor models are prone to model misspecification for Likert-like measurements. On the other hand,

Maydeu-Olivares et al. (2011) have shown that IRT models result in better fits compared to linear factor models on polytomous responses when the complete response data (and not merely the covariance structure) is at hand—which has been the case for the studies in question. As a result, this study used graded-response IRT models (GRIRTMs) instead of ordinary linear factor models. These models are, in a sense, the closest members of the discrete-valued latent trait models to the continuous-valued linear factor models (Samejima, 1969, 1997)

Factor structures were estimated via alternative-model confirmatory factor analysis (AMCFA). In this approach, a set of plausible factor structures are proposed, each corresponding to competing hypotheses justified conceptually or empirically. Then, via confirmatory factor analysis (in our case, using GRIRTM), parameters of all models are estimated with the same data, and the models are compared according to their relative² goodness of fit. The “best-fitting” model favors its corresponding hypothesis. AMCFA is a compromise between exploratory and confirmatory factor analysis (EFA and CFA, respectively) and is preferred over purely EFA since it allows a more focused alternative hypothesis testing.

This study investigated five families of factor structures (Model A-E). Model A, D, and E were, respectively, one-, two-, and three-dimensional factor models with no constraints on factor loadings. In these three models, loadings were freely estimated, and the only constraint imposed was on the variances of the factors. These three models can be seen as EFA models of degrees one to three. Model D (with 2D latent variable) was based on Russell’s (1980) *valence-arousal* (V-A) model of affect. The 3D factor structure (Model E) was built upon Mehrabian’s (1996) *pleasure-arousal-dominance* (P-A-D) model. Model B was constructed according to (Watson, Wiese, Vaidya, & Tellegen, 1999). In this model, all positive indicators (i.e., items measuring positive emotions like happiness) load on one factor

named *Positive Activation*. Similarly, the indicators of negative feelings (e.g., sadness) load on *Negative Activation*. In this model, commonly known as the PA-NA model³, all cross-loadings (of positive items on NA and vice versa) are fixed to zero. Finally, Model C was a bifactor hybrid of PA-NA and P-A-D; like Model B, there are no cross-loadings between the items and PA and NA, but all items load on a common factor. The theoretical justification behind including this model among the candidates was as follows. Conceptually, PA-NA spans the same plane in the affect space as V-A, and V-A is theoretically similar to P-A in the P-A-D model. Consequently, if the affect space is truly three-dimensional (as P-A-D model suggests), one can hypothesize that a third factor, on top of PA and NA, can capture the variations along the third dimension.

In latent trait modeling, one can set additional constraints on the covariances of the latent factors. Since we did not hypothesize any covariance structure for the latent variables (to be freely estimated or be set to zero), all models except Model A were represented in the competition with two versions with suffixes BARE (with latent covariances set to zero) and COV (not imposing any constraint on the latent covariance structure). As a result, we had nine competing models in the AMCFA. The factor structures of these models are shown in Figure 1.

Method

Data Collection and Preprocessing

In this study, 12 datasets of self-report momentary emotional states collected through daily diaries and experience sampling method (ESM) taken from (Dejonckheere et al., 2019) were analyzed (viz., Dejonckheere, Bastian, Fried, Murphy, & Kuppens, 2017; Dejonckheere, Houben, Bastian, & Kuppens, n.d.; Houben et al., 2016; Kalokerinos, Tamir, & Kuppens, n.d.; Koval, Pe, Meers, & Kuppens, 2013; Pe, Brose, Gotlib, & Kuppens, 2016; Provenzano, Dejonckheere, Verduyn, & Kuppens, n.d.; Sels, Ceulemans, & Kuppens, 2017, 2018; Van der

Gucht et al., 2019). All datasets had been collected in accordance with the ethical regulations of the host institutions. A detailed description of the sample characteristics and data collection protocols of each dataset is reported in Table 1. Table 2 indicates the properties of the instruments (i.e., the items and the response scales) used in each study.

The raw responses in all 11 ESM datasets were initially a value between 0 and 100. However, the continuity did not seem to carry significant additional information compared to polytomous, Likert-like responses; a person with a sadness score of 9/100 today is perhaps not much more miserable than her yesterday self when she rated her sadness with a 4/100. This suggested that the data could potentially be mapped on a Likert scale with enough levels. Leung (2011) has shown that a Likert scale with more than six levels can sufficiently preserve the distributional properties of the continuous variable. The responses to the only non-ESM dataset, i.e., the daily diaries in (Dejonckheere et al., 2017), were already polytomous with seven levels—more than the minimum number of levels that Leung (2011) had suggested. To have consistency across datasets—and in order to benefit from IRT and GRMs—the continuous responses of the ESM datasets were discretized into seven equally sized intervals.

Disentangling Inter- and Intra-individual Variations

In order to answer the research question, the AMCFA had to be applied to subsets (i.e., slices) of the data box suitable for the P- and R-technique analyses. At the same time, the slices should be taken in a way that minimizes the contamination of P-technique sample with interindividual variations, and vice versa for R-technique sample and the intraindividual variations. For the P-factor modeling, one can only analyze the measurements of one participant at a time. In order to make use of all available data, all participants were subject to P-factor AMCFA. The slices of the data box for P-technique modeling is called the WP (within-person) pool. For the between-person analysis, each individual can be represented by

only one measurement, drawn randomly from her multiple occasions. To make the R-factor analyses robust, this random selection of one occasion from every participant was repeated 200 times, resulting in a pool of cross-sectional datasets (BP pool) where no trace of intra-individual variation was left out, making them ready for pure R-technique AMCFA. Using this method, the idiographic and nomothetic sources of variation are disentangled. This process is depicted in Figure 2. Before moving on, it should be noted that IRT models can only be fitted to those samples where the variances of all items are non-zero. As a result, the samples that had at least one item with a zero variance were excluded from the WP and BP pools.

Model Fitting and Model Comparison

All data processing and analyses were conducted in R programming language version 3.5.2 (R Core Team, 2018). The IRT modeling was done using the open source package *mirt* (Chalmers, 2012) which allows applying various multidimensional IRT models, including graded response models. For a given dataset (e.g., PETEMADDY) and a pool of its samples (e.g., BP_{PETEMADDY}), one sample s (i.e., BP ^{s} _{PETEMADDY}) was selected and then modeled by the nine competing models. Then, the estimated models for that sample were compared with each other, and the model with the lowest value of AICc was selected as the best-fitting model⁴. This process was repeated for every sample within the pool (in this case, BP_{PETEMADDY}), and the number of times each model won the competition was stored. The number of votes was visualized for each dataset within each pool. The final winner in each pool was the model which had won in most datasets. This procedure is depicted in Figure 3.

Investigating the Loadings

In order to be able to interpret the results of the factor modeling, one has to make sense of the factor loadings. Since there were multiple datasets and multiple samples were subject to AMCFA, for each model, the distributions of the loadings of samples were plotted

for all items and datasets, and they were later clustered per items. For better readability, the distributions were then transformed into density heat maps using the open source R package *ComplexHeatmap* (Gu, Eils, & Schlesner, 2016). This procedure is shown in Figure 4. The same has been applied to the communalities of the items. In factor analysis, the communality of an item is the amount of variance of the indicator, which is explained by the factors on which the item loads (Brown, 2015, Chapter 2). Communalities can be used as measures of local goodness of fit. Note that not all items were included in all datasets (e.g., Content only appears in JULIAN EGON and Laura ESM 2014). This is reflected in sparse distributions for certain items (viz., Restless, Calm, Euphoric, and Excited) where except for the few vertical bars (corresponding to the datasets that contained those items), all other densities are zero.

Results

The outcomes of the election procedure (based on AICc) among the candidate models of the AMCFA for WP and BP pools are plotted in Figure 5. Since the BARE models with zero latent covariances are special cases of their COV counterparts (where the latent covariances were estimated freely), the decision on election outcome is based on the sum of votes of both versions of each model. For the WP pool, it is evident that Model A has the most votes across all datasets. For BP pool, however, the results are rather mixed; one dataset (ELISE ESM14) favors Model A, while seven and four datasets endorse Model B and Model C, respectively. In the BP case, it is evident that the best fitting model in most cases is a model with different, more complex factor configuration than the winning model of the WP pool. This finding suggests that the factorial structure of affect is different at the idiographic and nomothetic levels. Some of the possible reasons behind the discrepancies observed in the BP pool are examined in the discussion section.

Factor loadings and communalities of the winning models, i.e., Model A and Model B COV are shown in Figure 6. It can be seen that the *Communal* factor of WP pool resembles

valence, but their loadings are relatively weak and its communalities are noticeably low. The density histograms of the WP pool show that the pattern of loadings and communalities are rather inconsistent across datasets. On the other hand, except for items Restless, Calm, Euphoric, and Excited, the factor loadings in the BP pool are remarkably high and consistent across datasets.

Discussion

Making Sense of the Observed Incongruency

The evidence reviewed so far makes a compelling case for factorial incongruency of the idiographic and nomothetic analyses of momentary affect. The authors of the current study are convinced that the factorial congruency is, in fact, an indicator of construct nonequivalence. This implies that the instruments in question measure two different constructs at the same time; a unidimensional construct of affect states, and a two-dimensional construct responsible for person-specific affective traits that shape their trajectories in the trait space, and imposes boundaries to their affective state variability. One can think of a metaphor for this case: a group of ants that are trapped in specific curves wherein they can only travel in one direction. Their traits, namely the parameters defining their tracks in the 2D plane, limits their movement, and their states are the distance they have traveled on the curve. This metaphor is elaborated in what follows.

Mary, a myrmecologist, is studying the daily life of six ants with genetic markups GM^i living in a terrarium to find the factors underlying their metabolism. During the day, she regularly measures momentary body temperature (T_t^i , an indicator of metabolism) and position ($P_t^i = [x_t^i, y_t^i]$) of each ant i , and ignores Ω_t^i , a set of other variables that could have been included in the study. She knows that the ants are destined to walk along their genetically-determined tracks. Panel A and B of Figure 7 show positions of the ants in the morning and at noon, respectively. In panel B, the difference in body temperature of each ant

is also reported. R-technique analysis reveals that the interindividual differences of change in the body temperature of the ants can be attributed to a latent trait $\theta^i = [a^i, b^i]$, where a^i and b^i are, respectively, semi-major and semi-minor axes of the elliptic track of ant i which is of form $\left(\frac{x^i}{a^i}\right)^2 + \left(\frac{y^i}{b^i}\right)^2 = 1$. P-technique analysis of individual ants suggests that the intraindividual variation of momentary metabolism of ant i is a function of the distance he has traveled during the day, as such $\Gamma_t^i = \mu(l_t^i, \lambda^i)$, where λ^i is a constant summarizing his individuality in the measurement model⁵ μ . One can, as an initial step and for the sake of model parsimony, assume a simple measurement model for Γ_t^i where it is linearly proportional to l_t^i , viz., $\mu(l_t^i, \lambda^i) = l_t^i \times \lambda^i$. In panel C of Figure 7, the tracks of the ants are plotted, and the Θ parameter of Fabian is specified. Panel D of the same figure shows the length each ant has traveled during that period, where the starting and ending positions are marked by diamonds and circles, respectively. It can be seen that Bryan and Fabian have identical genetically-determined tracks (i.e., $\Theta^B = \Theta^F$) although they have walked different distances ($l_{noon}^B \gg l_{noon}^F$) and have different changes in their body temperatures ($\Delta T_{noon}^B > \Delta T_{noon}^F$). On the other hand, even though Charlie and Eric (with $\Theta^C = \Theta^E$) have traveled the same length ($l_{noon}^C = l_{noon}^E$), their body temperatures have changed differently ($\Delta T_{noon}^C < \Delta T_{noon}^E$). Conversely, Alex and Fabian experienced similar changes in body temperature despite having dissimilar tracks ($\Theta^A \neq \Theta^F$) and very different achievements ($l_{noon}^A \gg l_{noon}^F$).

In the comparisons made so far, the cases of Charlie-Eric and Alex-Fabian can only be explained by their interindividual differences in their personal λ^i —as such $\lambda^C \neq \lambda^E$ and $\lambda^A \neq \lambda^F$. One can imagine that λ^i is a function of the genetic markup of the ants and the ignored variables Ω_t^i , i.e., $\lambda^i = \phi(GM^i, \Omega_t^i)$, implying that the MI is violated here. Moreover, given the knowledge about genetic determinism in ants' behavior, one can imagine that the individual track of ant i is also a function of his genetic markup and Ω_t^i , i.e., $\theta^i =$

$\psi(P_t^i, GM^i, \Omega_t^i)$. The P-technique and R-technique, if applied separately (which is the case by their technical definitions), are incapable of discovering the functions μ^6 and ϕ —this is the very moment Mary faces the same psychometric paradox that has been haunting minds of others for more than a century.

An Explanation for the Disparity of Outcomes Across Datasets

The discrepancies observed in the WP pool can be attributed to the psychometric characteristics of the instruments (namely the number of items and the choice of items in each dataset) and the sample characteristics—there are notable group differences in the recruited samples. For instance, among the incongruent datasets, three of them, viz., LONGITUDINAL W1–3, belong to a sample of freshmen university students, and one, viz., KATHLEEN STRESSKLINIEK, has been sampled from patients with mental disorders who were significantly older than the other datasets. However, the overall picture is clear and consistent: there exists a substantial discrepancy between the factor configurations at the two levels, which is a matter of degree—and that is the case for all datasets.

It is also noteworthy that the robustness of the findings has also been confirmed with another sampling algorithm not described in the methods section. In the other attempt, to form a BP pool of 100 samples, the same occasion was selected for all participant in each draw. Since the number of draws (i.e., 100) exceeded the number of occasions (ranging from 30 to 98), not all draws were unique, and some occasions appeared multiple times in the pool. On the other hand, the R-technique analysis was performed on a pool of 100 samples of individuals in each dataset. That implies that in at least five datasets (those with less than 100 unique samples) the samples drawn for the BP pool were not unique. Interestingly, the outcomes of vote counting obtained by the two algorithms were not visually distinguishable.

A Closer Look at the Idiographic Factor Loadings

The datasets have used PANAS-like instruments to measure affect (cf. the items mentioned in Table 2 with Watson, Clark, & Tellegen, 1988). However, as shown, the idiographic construct of momentary affect is a unidimensional factor. Hence, one can skepticize the use of such instruments (that are validated for 2-dimensional constructs) in measuring a unidimensional construct. A closer examination of the factor loadings of the P-factor analysis supports this doubt. As shown in the upper panel of Figure 6, the factor loadings of items vary remarkably across individuals—the distribution of the loadings are quite uniform for all items over almost all datasets, the only exception being LONGITUDINAL W1. This can be observed in the factor loadings of all candidate models (see the supplementary material). This is also the case for the communalities of the items in the WP analysis. This undermines MI of the instrument among individuals.

Moreover, the communalities, even for those with relatively low variance (e.g., in LONGITUDINAL W1), are rather low—i.e., the items do not explain the WP variations of affect well. These two issues indicate low psychometric quality of the instruments for the measurement of the WP variation of affect. This is also the case for other factor models—in fact, this is perhaps the strongest consistency among different factor models and datasets (cf. the supplementary material). An implication of these issues (especially the latter) is that the instrument is probably not measuring much of the change in momentary affect.

This is in line with the findings of Dejonckheere and colleagues (2019) that the dynamic measures derived from momentary affect—most importantly, emotional inertia (i.e., the slope coefficient of the autoregressive model), emotional granularity or differentiation (i.e., interclass correlation of responses in PA and NA), emotional instability (i.e., MSSD of PA and NA), and emotion variability (i.e., standard deviation of PA and NA)—add very limited predictive power on top of the mean levels of PA and NA. Putting the pieces of

evidence together, one can conclude that the PANAS-like instruments barely measures any state-like, WP latent variables on top of a trait-like, BP construct—have all the ESM research mistaken noise with an underlying idiographic factor of momentary change in affect?

Limitations

The main limitation of this study is that it did not address reliable measurement models of WP and BW variations of affect since it was not the aim of the research. As a result, although not promised in the beginning, this study, at its current state, does not put an end to the perpetual conflicts of idiographic and nomothetic approaches. The second limitation of this study was that it did not include the serial dependencies of the WP time series into the models. Quite interestingly, however, Molenaar and Nesselroade (2009) have shown that the classic P-factor models, despite flouting the temporal WP dependencies, can still provide amply accurate and robust estimations. However, an educated guess would suggest that, since adding the temporal dependencies to the models increases the number of parameters (at least) proportional to the number of latent factors, the ranking of the models are not expected to change significantly since the complexity penalty of the more complex models grow at a higher rate than the simpler models.

Concluding Remarks

The chasm between the idiographic and nomothetic understanding of behaviors, emotions, and mental states can only be filled through a holistic approach that looks at the data box from a higher perspective than P- and R-techniques. In the rather simple case of Mary's ants, she needs to estimate the following complex system of interrelated functions. Here is where Cattell leaves Mary alone with her measurements, ants, and four functions (τ , μ , ϕ , and ψ)—as well as an Ockham's Razor to help her keep Ω_t^i as small as possible.

$$\left\{ \begin{array}{l} T_t^i = \tau(\Gamma_t^i, \Theta^i) \\ \Gamma_t^i = \mu(l_t^i, \lambda^i) \\ l_t^i = \text{dist}(P_0^i, P_t^i)^7, \\ \lambda^i = \phi(GM^i, \Omega_t^i) \\ \Theta^i = \psi(P_t^i, GM^i, \Omega_t^i) \end{array} \right. , \quad \text{for } \forall i \in \text{Sample and } \forall t \in \text{Day}$$

Ultimately, the authors of the current study wish Mary luck for the challenge she faces, and further hope that the idiographic and nomothetic variations be modeled simultaneously in the future affective research, in a holistic manner. They humbly propose multilevel or multi-group GRIRTMs for this end. The authors, until then, remain faithful that the holistic approach they cherish settles the clash of the two worlds—at least to some extent.

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Supplementary Material and Code

The supplementary material, including additional plots not included in this manuscript, the monotonicity analysis, and the reproducible R codes, can be accessed on Open Science Framework via <https://osf.io/a8jqt/>.

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¹ It is worth mentioning that Allport later in his life, in his other influential book *Pattern and Growth in Personality*, came to the conclusion that individuality, in essence, cannot be studied by (the intrinsically nomothetic) science: “The individual, whatever else he may be, is an internally consistent and unique organization of bodily and mental processes. But since he is unique, science finds him an embarrassment.” (Allport, 1961, p. 8); consequently, the study of the individual must be left to “history, art, or biography whose methods are not *nomothetic* (seeking universal laws), but

² The word *relative* is crucial here since, as will be discussed later, information criteria were the basis for comparing models for their goodness of fit. However, the information criteria can only be used for comparisons since their absolute values are not meaningful at all.

³ Note that in the context of the more recent ESM research, PA and NA have different connotations; they are known as *positive* and *negative affect*, defined as the unweighted average of scores on the positive and negative items, respectively.

⁴ There were several reasons behind the decision to choose AICc as the measure for goodness of fit. Among the information criteria used in the comparison between models of different complexities, AIC, AICc, BIC, and HQ are the most common. In AIC, the penalty term does not depend on sample size. As a result, it has been shown that AIC tends to *overfit* on small sample sizes by favoring models that are known to be more complex than sufficiently necessary. The WP and BP samples of this study had sample sizes ranging from 20 to 170, which are very small compared to the number of parameters estimated in GRIRTMs with five to nine indicators. Moreover, the model complexities of such models grow relatively fast by adding new latent variables, meaning that the judgment based on AIC is not justifiable in this study.

The penalty terms in BIC and HQ, on the other hand, depend on sample size. This results in less overfitting on small sample sizes. However, both BIC and HQ are suitable for *model identification* and rely on two strong assumptions: that there exists a true model behind the generated data, and that the true model is among the candidates. As mentioned earlier, the GRIRTMs cannot describe cases where the variance of some items are zero—which is the case for a remarkable number of samples among both WP and BP pools. As a result, those assumptions are clearly violated. Hence, neither BIC nor HQ are suitable for the comparison in question. Interestingly, a later, post hoc investigation into the outcomes of model selection based on BIC and HQ revealed that they do not show any consistency in their selected models across datasets, which is by itself an interesting observation.

AICc, the corrected version of AIC for small sample sizes, neither suffers from overfitting in small samples and nor does it depend on the strong assumptions of BIC and HQ. As a result, it was a suitable measure for the relative goodness of fit for the current study. For a more detailed treatment of the matter, and for formal proofs for the abovementioned statements, see (Claeskens & Hjort, 2008, Chapter 4).

⁵ The term measurement model is used rather roughly here. The reason is measurement model is, by definition, an attribute of the instrument, (ideally) independent of the subject. In Mary’s experiment, however, λ^i is, in a sense, an intermediary buffer between the manifest and the latent variables (viz., l_t^i and Γ_t^i). As a result, μ has been essentially contaminated by a component of individuality, contravening the very technical definition of measurement model.

⁶ More precisely, λ^i and consequently $\mu(\cdot, \lambda^i)$.

⁷ The function *dist* is trivial hence does not require estimation.

Table 1

Overview of the sample and protocol characteristics of the datasets used in the current study, adapted from (Dejonckheere et al., 2019)

Datasets		Sample characteristics			Protocol characteristics				
Name	Main study	<i>N</i> participants	% Female	<i>M</i> age	Type	Occasions / day	<i>N</i> days	Max. occasions	<i>M</i> compliance
1. CLINICAL ESM	(Dejonckheere et al., n.d.)	118	66%	35.69	ESM	10	7	70	87.11%
2. ELISE ESM14	(Kalokerinos et al., n.d.)	100	77%	24.12	ESM	7	14	98	88.80%
3. JULIAN EGON	(Provenzano et al., n.d.)	34	59%	23.85	ESM	10	7	70	87.35%
4. KATHLEEN STRESSKLINIEK	(Van der Gucht et al., 2019)	46	76%	41.96	ESM	10	4	40	87.39%
5. Laura ESM 2014	(Sels et al., 2017)	100	50%	27.75	ESM	10	8	80	80.14%
6. Laura ESM 2016	(Sels et al., 2018)	185	50%	26.30	ESM	6-14*	8	64	92.94%
7. LONGITUDINAL W1	(Pe et al., 2016, Wave 1)	200	55%	18.32	ESM	10	7	70	87.27%
8. LONGITUDINAL W2	(Pe et al., 2016, Wave 2)	190	56%	18.64	ESM	10	7	70	87.87%
9. LONGITUDINAL W3	(Pe et al., 2016, Wave 3)	176	55%	19.28	ESM	10	7	70	88.57%
10. MARLIES BPD	(Houben et al., 2016)	50	84%	29.96	ESM	10	8	80	79.86%
11. MTURK DAILY DIARY	(Dejonckheere et al., 2017)	94	51%	34.12	Daily diary	1	30	30	95.04%
12. PETEMADDY	(Koval et al., 2013)	95	62%	19.06	ESM	10	7	70	90.34%

Note. * participants were sampled 14 times during the weekend, yet the time interval between two consecutive occasions remained identical

Table 2

Overview of the instrument characteristics of the datasets used in the current study, adapted from (Dejonckheere et al., 2019)

Datasets		Instrument characteristics		
Name	Main study	Positive items	Negative items	Scale
1. CLINICAL ESM	(Dejonckheere et al., n.d.)	happy, relaxed, euphoric	anxious, depressed, angry, stressed	0 – 100
2. ELISE ESM14	(Kalokerinos et al., n.d.)	happy, relaxed	sad, angry, stressed	0 – 100
3. JULIAN EGON	(Provenzano et al., n.d.)	happy, relaxed, excited, content	sad, anxious, angry, stressed	0 – 100
4. KATHLEEN STRESSKLINIEK	(Van der Gucht et al., 2019)	happy, relaxed	sad, anxious, depressed, angry, stressed	0 – 100
5. Laura ESM 2014	(Sels et al., 2017)	happy, relaxed, cheerful, content	sad, anxious, depressed, angry, stressed	0 – 100
6. Laura ESM 2016	(Sels et al., 2018)	happy, relaxed	sad, anxious, angry	0 – 100
7. LONGITUDINAL W1	(Pe et al., 2016, Wave 1)	happy, relaxed, cheerful	sad, anxious, depressed, angry, stressed	0 – 100
8. LONGITUDINAL W2	(Pe et al., 2016, Wave 2)	happy, relaxed, cheerful	sad, anxious, depressed, angry, stressed	0 – 100
9. LONGITUDINAL W3	(Pe et al., 2016, Wave 3)	happy, relaxed, cheerful	sad, anxious, depressed, angry, stressed	0 – 100
10. MARLIES BPD	(Houben et al., 2016)	relaxed, cheerful	anxious, depressed, angry, stressed	0 – 100
11. MTURK DAILY DIARY	(Dejonckheere et al., 2017)	happy, relaxed	sad, anxious, depressed, angry, stressed	1 – 7
12. PETEMADDY	(Koval et al., 2013)	happy, relaxed	sad, anxious, depressed, angry, stressed	1 – 100

Figures

Figure 1. Factor models used in the AMCFA.

Figure 2. Sampling methods used in this study. The top and bottom panel shows the procedure for making WP and BP pools, respectively. The cubes are adapted from (Boker, 2017).

Figure 3. Voting procedure.

Figure 4. Making the density heat maps of an earlier WP sample for the Communal factor of item Relaxed.

Figure 5. Election results. Top and bottom panels show the results for the WP and BP pools based on AICc, respectively.

Figure 6. Factor loadings and communalities of the winning models clustered by items. **Top:** Outcomes for Model A in the WP pool. **Bottom:** Outcomes for Model B COV in the BP pool.

Figure 7. Mary's experiment with ants.