Consilience Within the Biopsychosocial System

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Ψ Psychology Press
Taylor & Francis Group

REPLY

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It was a sobering experience reading these commentaries, as their authors are all very smart people who raise difficult issues for and criticisms of the Multilevel Personality in Context (MPIC) model. I am grateful for the opportunity to respond to their critiques, and to develop my thinking about the model.

Before responding to specific commentators I first venture some further general thoughts about the MPIC model, attempting to clarify what it is and what it is meant to do. In essence, the model attempts to allow for representation of what is actually happening at any time t for any person X, nested within multiple relationships with other persons, dyads, and groups. Furthermore, by considering many person Xs in conjunction with each other, using multilevel modeling and social network analysis, in principle not just the behaviors and outcomes of persons could be understood (within the limits of the particular level exemplar variables chosen for the study), but also the behaviors and outcomes of dyads, families, social groups, organizational settings, and political/cultural matrices.

The model is offered in the spirit of E.O. Wilson's (1998) call for greater consilience between the natural sciences and the social sciences, and Wilson's notion that reality is a single seamless web that must ultimately all fit into a single comprehensive model. The MPIC is based on the idea that this seamless reality can be represented as a nested hierarchy of processes, ranging from atomic to cultural. Each level of process is running off simultaneously, although as Deci and Ryan (this issue) point out, the levels vary relative to each other and over time in their regnancy (or dominance) with regard to the determination of behavior (i.e., biological levels may have greater effect on food-seeking behavior, and cultural levels on norm-following behavior). The MPIC model is not conceived of as a theory to be tested, but rather as a heuristic framework in which to consider "the biggest picture." The assumption is that researchers should be able to obtain a reasonably complete description of what is happening at any time t by attending to the various different levels that

the model stipulates. The 23 culture subjective well-being (SWB) data presented in the target article were not meant to test the MPIC, nor were they meant to suggest that SWB is the primary outcome addressable via the model; rather, the data were presented to show the MPIC's potential generativity for integrative study design.

Ironically, before submitting the target article, I was afraid that the general structure of the overall model might be too obvious or widely accepted to be worth pointing out. However, the responses of the commentators have shown me that the model might be controversial, or even flat-out wrong. In considering their critiques, I defend two versions of the model: the target article's Figure 1 model, which simply locates personality within the context of everything else, and the target article's Figure 2 model, which locates four sublevels of personality within the personality level of Figure 1. Admittedly, the notion that needs, traits, goals, and selves are nested in some hierarchical sense within personality, in the same way that neurons are nested within brains or people are nested within groups, is dubious. However, in responding to the McAdams and Manczak (this issue) commentary I consider some ways in which such a nesting might make sense.

One limitation of the target article that appeared to arouse confusion was that it gave insufficient attention to the ordering principle (or principles) underneath the hierarchical arrangement of the MPIC. The target article focused mainly on time scale, noting that processes tend to run off faster at lower levels of the hierarchy and slower at higher levels (atomic and cellular processes happen fast; large group and cultural processes happen slow). In their commentary, Mayer and Lang (this issue) correctly pointed out that there are exceptions (a latent virus finally emerges after decades, or a government topples overnight). Such exceptions are not surprising, because the time-scale difference between levels is an on-average phenomenon, not a universal or necessary fact.
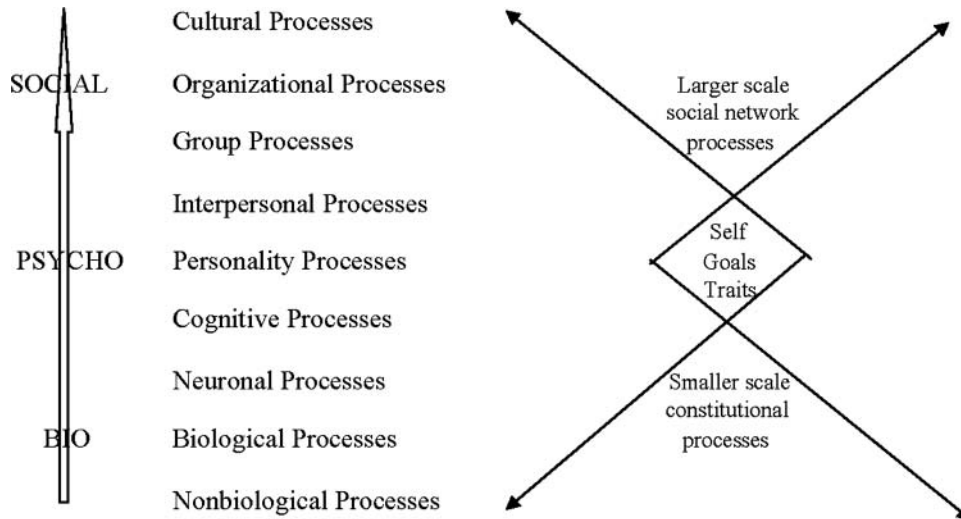
Figure 1. Conceptualizing the self as the interface between the person and the social world on the biopsychosocial continuum.

We focused on the time scale dimension in the target article mainly because it helps explain the concept of "emergence" and helps answer the question of how higher order aggregates may have top-down causal influence on their lower order constituents, namely, by slowly changing the context in which the lower order constituents operate. For example, although corporate leadership requires a functioning company that it can manage, and is constrained by history and by current company structure in its decision making, corporate leadership nevertheless makes decisions which impact the subsequent operating conditions and functioning of all of the lower level parts of the company. As another example, top-down influence is one way that a conscious self might have effects on behavior (Baumeister, Masicampo, & Vohs, in press); although it emerges last in a temporal sequence, the "ghost in the machine" can give greater weight to particular action tendencies already coemerging with consciousness, tipping the balance within a global workspace toward "tendencies I like" (Baars, 1997). Such decisions can influence the destiny of the system as a whole, exerting executive control that reaches back down to organize and reorganize constituent lower level processes.

Despite our previous focus on time scale and emergence as ordering principles, the MPIC should be orderable by a variety of other principles, given its supposed depiction of the single seamless web; I now consider some other such principles. Perhaps the primary difference between levels is complexity. Each level is thought to build on the provisions of all of the levels below, adding a new layer of organization on top. Cognitive processes could not exist without neural machinery, but once such machinery is present and functioning, cognitive processes emerge that make

use of that machinery to derive pragmatic solutions to adaptive problems. Personality processes require cognitive processes, but once such exist, personality processes emerge to make use of cognitive processes to pursue the person's goals and needs. Social processes could not exist without personality processes, but once personalities are present, social processes emerge to influence the contained personalities. Other ways of naming this complexity dimension are molecular to molar, less information density to more information density, and less spatially extended to more spatially extended. The MPIC hierarchy can also be discussed in terms of a physicality/mentality dimension ranging from physical to biological to cognitive to personal to transpersonal to cultural and even species level (and perhaps even to a spiritual level for religious fans, or to a sentient species level for science fiction fans).

Figure 1 in this reply provides a new graphic depiction of the MPIC model, which may further clarify the ordering principles underlying the model. In this graphic, the "self" aspect of personality sits at the waist of an hourglass shape. Reaching down from the self are all of the nested processes which constitute the person, best approached by within-subject and repeated measures analysis of that person. Reaching up from the self are all of the relationships and networks within which the person is nested, best approached by between-subject (and between-dyad, between-groups, and between-nation) analyses of the person-in-context, as compared to different contexts.

Locating the self at the center of the graphic may simply reflect my bias as a personality psychologist, but I believe it is justified because the self is who we *actually are*, as people living our way through the world moment by moment. It is also *who we talk to*,

53

when we serve as therapists or interventionists, or even as friends giving advice; we are talking to the conscious person who is listening, who we hope can learn something from us. The person/other person interface represents a radical boundary between two different regimes of functioning, although of course they are both contained within the single seamless web: the regime of the within-body, within-brain, within-subject processes going on inside the person, and the regime of the between-subject, trans-body, group-coordination processes going on outside the person.

To manage the very difficult person/other person interface, humans have apparently evolved a very useful function—the conscious self (Sedikides & Skowronski, 1997). Conscious selves are the faces or persona that we create, appropriate, and inhabit (Allport, 1937), as we live out of our bodies and into the future, frequently in the company of conscious others. The presence of others means that we must continually construct selves that represent us and that must be presented dialogically to others (Goffman, 1959), such that moving interfaces are created between individuals and other individuals, linking them despite their divergent goals and motives. In addition to interfacing with the social world above, human selves must also orient and regulate the action system below, such that the person as a whole is enabled to act in ways that meet the needs of the system and fulfill the system's potential. How well the "selfing process" (McAdams, 1996) performs these two functions may be the most important determinant of a person's adaptive and positive life-outcomes, further justifying placement of self at the center of Figure 1. In a very real sense, the self (or self function) has the "keys to the car." I discuss the self's vital role further throughout this reply.

The commentaries converged in pointing out some other ambiguities of the general MPIC model as presented. First, what about time? Alternative models were proposed by some commentators that attempted, in one way or other, to depict unfolding processes over time. The current MPIC model does *not* include time; again, it is meant to represent the total state of affairs at some particular time t for some person X nested within some network of relationships and groups. Still, nothing prevents using the MPIC to design longitudinal or time-varying studies; researchers can apply any measures, theories, or analytical techniques they like to try to understand and predict change from time t to time $t +$ 1 and beyond. Also, nothing prevents use of the MPIC heuristic in designing rigorous experimental studies that nail down causality over time. For example, in their commentary Martin, Sanders, Shirk, and Burgin (this issue) critique the target article's cross-sectional prediction of SWB from autonomy need-satisfaction, saying that causality could not be determined with the data. This was true, but this merely represents a limitation of correlational and cross-sectional data, not a

limitation of the MPIC model per se. To illustrate that self-determination theory (SDT) psychological needs research does not have to be merely correlational, Sheldon and Filak (2008) conducted a $2 \times 2 \times 2$ experimental study of autonomy, competence, and/or relatedness need-satisfaction within a game-learning context, showing causal effects on changes in participant mood and engagement depending on randomly assigned need condition (three parallel main effects were detected, consistent with SDT).

Mayer and Lang (this issue) specifically include time in their dimensional view of personality, which contains the three dimensions of molecular-molar (vertical axis), inner-outer (horizontal axis), and time (receding depth, in Mayer and Lang's Figure 1). However, it is not clear exactly how time is to be measured and used within their model when designing longitudinal or experimental research, or how the time dimension interacts with the other two dimensions of the model. It would be possible to add a three-dimensional depth illusion to the MPIC hierarchy and call it time, but this would seem to provide limited value. Vallerand and Lalande (this issue) also discuss time in the context of their alternative vertical model, which I consider in a moment.

Several commentators also discuss *situations* as missing from the MPIC, or nonsocial situations. I agree that the MPIC as currently formulated is not well suited to deal with nonsocial situations, as above the person it focuses on people nested within groups of other people, using a multilevel modeling perspective (thus, the model focuses on social situations at time t). However, there is no reason not to identify various categories of nonsocial situations in which people are also nested. For example, over time, people are contained in many different kinds of nonsocial situations (watching TV by themselves, hiking in the woods by themselves, working on a paper by themselves). Armed with an explanatory typology of situations, a between-subjects experimenter could randomly assign participants to various solitary situations (TV, nature, paper) or compare solitary situations to various social situations (with friend, spouse, stranger), to examine the effects of the different kinds of nestings on people. In fact, many experiments do precisely this kind of thing. Also, within-subjects research could collect repeated measures of people as they encounter different situations over time.

Where should situations be placed, in an integrative hierarchical model? Although the MPIC depicts people as nested within (i.e., below) situations, it is possible to reverse the hierarchical order and put the person at the top of a hierarchy, and to put the different contexts and situations the person encounters at lower levels of the hierarchy. Vallerand and Lalande's (this issue) Hierarchical Intrinsic Motivation Extrinsic Motivation (HIMEM) model does just this, positing a stable/global level of the person at the top, with a less

global "context" level below (at school, at work, during recreation, etc.) with a least global/most unstable "situation" level at the bottom (Saturday at 3 p.m. on the tennis court). This makes sense from a psychometric perspective and helps us to conceptualize and design repeated-measures studies of the person in different situations. However, I urge that we not lose sight of the fact that at any actual time t, the person is in only *one* of many possible situations. In other words, the person is always nested within particular situations ("Darn, I just lost the first set!"), situations which in turn are nested within contexts (in this case, say, "playing tennis with Dan"). Of course one could proceed to identify larger contextual nestings, such that "playing tennis with Dan" is nested under "playing tennis," which is nested under "physical activity," and so on. It is *not* the case that situations are nested inside of people; people can be thought of this way only if repeated observations at different time ts (or recalled time ts) are made. At time t, there is only one situation at hand.

Like Mayer and Lang (this issue), Vallerand and Lalande (this issue) also included a horizontal (time) dimension in their vertical HIMEM model, a horizontal dimension that is specifically occupied by the basic SDT (Deci & Ryan, 1985, 2000; Sheldon & Krieger, 2007) path model, in which the social context affects need-satisfaction, which in turn affects motivation and a variety of outcomes. Two comments: First, I suggest that the vertical hierarchical ordering (person above context above situation) identified by Vallerand and Lalande (this issue) could apply to *any* horizontal process model, not just SDT's process model, and thus that the HIMEM might be generalized beyond SDT to consider person/context/situation effects upon any process of interest. The MPIC does this by allowing any temporal process to be conceptualized and tested, not just SDT processes, via modeling of change across different time ts. Returning to the question of where to locate situations in a hierarchical model: The question is, Which hierarchical ordering is more accurate with respect to Wilson's "seamless web of current reality"—a hierarchy in which the person contains contexts which contain situations (the HIMEM model) or a hierarchy in which persons are contained within situations which are contained within contexts (the MPIC model)? I suggest that the MPIC depiction is most accurate, at least as a heuristic for conceptualizing what is happening right now.

As a second comment, the HIMEM's horizontal time dimension essentially depicts only top-down causality (represented in the target article's Figure 1 as an arrow pointing from context down to person); specifically, the HIMEM says that it is the social context that determines whether a person's needs are satisfied, which then determines whether positive cognitive/affective/behavioral outcomes occur. I suggest that this is inconsistent with SDT's focus on the person as

an active agent, who might, through effective action, have bottom-up effects on his or her own social environment and hence his or her own need-satisfaction. This is represented in the target article's Figure 1 by an arrow pointing up from person to social context. Of course, both directions of causality can occur, and ideally, a model including time would be able to represent both top-down and bottom-up processes and different combinations of such processes.

Indeed, one way to incorporate time within the MPIC model might be to include a horizontal time dimension in the target article's Figure 1. This horizontal dimension might represent where, along the hierarchy of causality, lies the regnant or dominant cause of behavior (Deci & Ryan, this issue) at a given time t. A line plotting the level of the regnant cause at each moment would likely zigzag up and down in the MPIC hierarchy over time. Imagine "Bill" and "Bob" at a sporting event. Neural activity at time t might give rise to a conscious experience within Bill at time $t+1$ (the thought "I hope we win!"), which might give rise to a social behavior at $t+2$ ("What do you think our chances are, Bob?"), evoking a response from Bob ("at least 50/50!"), producing new input to Bill's perceptual systems, back down at a low level of the Bill hierarchy. When the crowd all stands up for the national anthem, the social group level of analysis becomes the regnant cause of behavior for both Bill and Bob; thus, people can become behaviorally tuned when they all yield to the same social influence. Of course, the notion that a single regnant cause of behavior might be identified at a given moment, and that this cause might vary up and down the MPIC hierarchy over time, requires a lot of further development.

I now turn to consider each set of comments separately. Space precludes addressing all of the critiques made. However, I believe the following addresses the most important critiques, and that many of the non-addressed critiques are handled, indirectly, by my responses to other commentators.

Deci and Ryan

Deci and Ryan (this issue) offer no major critiques of the model, instead using the opportunity to elaborate on various related issues. They do suggest that the levels in the target article's Figure 1 may be misspecified; sociology perhaps should be included with social psychology at the social interaction level, rather than being included with cultural psychology at the culture level. This might be correct, or perhaps sociology deserves its own level between social psychology and anthropology, because it describes large-scale social processes occurring within, not between, cultures. I am not wedded to the particular arrangement of sciences in the target article's Figure 1, or even to the

particular arrangement of levels; it is the general hierarchical approach that I am promoting, not the specific terms within the model. Deci and Ryan also question why psychology got three levels and chemistry got only one level; the answer is that the model is designed to explain human behavior, and thus a more detailed focus is appropriate at levels relevant to this goal. A scientist interested in chemical behavior might well insert more levels of organization into lower levels of the scheme, as necessary.

In the target article's empirical example we tested the SDT proposition that autonomy, competence, and relatedness are basic psychological needs common to all human beings, which can explain how variations in higher levels of the person and his or her context affect SWB. Conceptually, we located psychological needs at the "bottom" level of personality, as one potential exemplar of the species-typical human nature that individual differences emerge from and rest upon. However, it is important to realize that other universals besides psychological needs could also be identified and studied within the context of the MPIC (Malinowski, 1944). For example, Buss's (1989) well-known study of 37 cultures attempted to identify universal patterns of mate preferences that differ between male and female minds. The MPIC model might accommodate and help to elaborate such findings, facilitating consideration of how various personality, social, or cultural processes might act down to moderate or shape these gender-specific main effects. Or the MPIC could be used to fully contextualize a supposed universal process such as "cheater detection" (Cosmides, 1989). Perhaps peoples' ability to detect (or punish) cheaters varies as a complex multilevel interaction between personality type, relationship type, group type, and cultural type.

As part of their commentary, Deci and Ryan eschewed reductionism while focusing on psychological events as an important point of entry for many different purposes. I agree with this observation, and note that it is quite consistent with Figure 1 in this reply, and the notion that the "self-in-the-world-at-time-t" sits at the waist of the hourglass. Even though we may eventually discover the mechanisms underlying the functioning of this conscious self, it will still make the most sense for the interventionist to address the self (and not its mechanisms) directly in many cases. As Deci and Ryan (this issue) put it,

> It is nonetheless comparatively rare that the mechanisms of brain are a useful point of intervention, or provide full explanations of motivated, goal-directed behaviors. Rather social psychological interventions are frequently the more fruitful routes for affecting individuals' behavior, development, and well-being when compared with manipulations of variables ei-

ther lower or higher within the levels of analysis in the Sheldon hierarchy. (p. 19)

Deci and Ryan also acknowledge that "psychological analyses . . . would not be the regnant causes for someone interested in the mechanics of reflexes, or the basic mechanisms of the visual system. Here, other levels of analysis are both more pertinent and useful" (p. 19). Once again I agree. Richard Dawkins (1986) used the analogy of a motorcar, saying that although the car's functioning must of course ultimately be explainable in terms of atomic and molecular-scale processes, it may be more efficient to study the higher level processes of plugs sparking and pistons pumping, certainly if one wants to fix the car when it breaks down. Again, however, the chemist might be more interested in the molecular processes influencing the ignition of the fuel, or the durability of the paint job.

Vallerand and Lalande

I have already addressed some of Vallerand and Lalande's (this issue) thought-provoking comments, concerning differences between the HIMEM and the MPIC on the hierarchical ordering of processes (the MPIC says that at time t, people are nested within situations, whereas the HIMEM says that situations are nested within people) and differences concerning time (the HIMEM contains time, but only as a specific SDT path model emphasizing top-down causality from social context to person; the MPIC does not reference time but is in principle amenable to any longitudinal analysis of multiple time ts using any theory, not just SDT). Here, I first say more about the HIMEM's vertical ordering of situations at the bottom, with contexts above, and people at the top. Again, this may be a useful heuristic device for conceptualizing within-subject studies and analyses, but it makes sense only because people actually find themselves, at different times, nested within different situations and contexts. The view that "the person contains the situation" seems to reflect a notion of a stable self that is somehow raised above time and the vagaries of mere experience, a questionable assumption. Another reservation with the HIMEM model is that its main contribution beyond accepting the basic SDT model is to emphasize differences between global, contextual, and specific measurement contexts, and it is not clear how theoretically significant this is. The HIMEM makes predictions such as, "A predictor variable measured at level X should have stronger effects upon outcomes at level X than the same predictor variable measured at a different level has upon level X outcomes." But isn't this simply what we would expect given similar referents and shared method variance at any particular level?

It seems what is needed, to better justify the Vallerand and Lalande (this issue) vertical arrangement, is some description of functional connections (top-down or bottom-up) between characteristic Q measured as a global trait of the person, characteristic Q measured as something about a person in a particular context, and characteristic Q measured as something about a person in a particular situation within a context. Vallerand and Lalande do suggest, at one place within their commentary, that the person level might exert some functional control over contexts and situations; "global and contextual-level constructs serve, in part, as schemas to store relevant motivational information, guide perception, and lead to action even in the absence of awareness" (p. 49).

I agree that the broader person level regulates functioning but believe that this occurs not so much through trait (near the bottom of personality) or cognitive schema (below personality) processes but rather through goal processes (near the top of personality). Carver and Scheier's (1982, 1998) control theory describes such processes well, via its hierarchical analysis of the functioning of goal-based action systems. According to their model the "system" (self) level sets very broad goals for the person, which in turn affect "principles" (general strivings), which in turn affect "programs" (specific and temporally localized strivings), which in turn affect more concrete behavioral sequences. The MPIC is consistent with Carver and Scheier's model in that it locates self processes at the top level of personality (just as the self is located at the top level of Carver and Scheier's action system), with goals (strivings) located just below. Goals are presumably being regulated by self-level processes, with goals in turn regulating more trait and cognitive processes nested below.

In contrast, the HIMEM's functional depiction of behavior, if top-down regulation is to be inferred, has the global/trait level regulating contexts which in turn regulate situations. It seems that the global personality is more likely to be reaching down to regulate its own internal processes than it is to be reaching up (crossing the person/other person boundary) to directly regulate contexts and situations. Although I noted earlier that the self can exert bottom-up causal influence on its own social contexts, I suggest this must occur via the functioning of the self's action system, producing behavior that, over time, influences the context in which the self is nested. For example, a person encounters a business opportunity. She recognizes the opportunity and acts down to create relevant goals, subgoals, and action sequences. If these goals are effective over time, they enable her to successfully act "up" to manipulate the context, thereby effecting change in the social network of agreements, impressions, and relationships in which she is enmeshed. In short, I suggest that if the HMEM was based on a goal model of the person rather

than a trait model of the person, it might work better. Instead, we are left with the perhaps trivial observation that global traits by definition express themselves in particular contexts and situations. I consider the issue of top-down regulation within personality further when I respond to McAdams and Manczak's (this issue) commentary.

Martin, Sanders, Shirk, and Burgin

Martin et al. (this issue) supply an interesting philosophy of science critique of the MPIC, arguing that the model is insufficient on a number of scores. One of these is the MPIC's underlying assumption that an outcome is explained when it is placed in a nomological net that references all relevant levels, and the MPIC's further implicit assumption that an optimal explanation is one that accounts for the most variance. Martin et al. assert that these two assumptions create problems, but I am not sure that they do; their commentary's specific critiques seemed to be mostly criticisms of correlational and cross-sectional methods, such as were employed in the example data presented in the target article. As noted earlier, there is no reason why longitudinal and experimental methods cannot be employed in the context of the MPIC model.

Martin et al. (this issue) also suggest that the MPIC provides "no clue with regard to the relevance of possible initial conditions. What do we include? What do we exclude?" (p. 32). This is always a difficult problem, the solution to which depends on the knowledge of the researcher, his or her intuitions about what is important, and his or her resources for gathering data of different kinds to test those intuitions. Admittedly, the MPIC says nothing about the inclusion issue. Instead, it provides a heuristic device within which to think about bigger-picture issues, perhaps prompting more attention to larger scale and/or smaller scale issues within a study than was originally intended. The MPIC's potential value is that it is supposed to be a catalogue of different kinds of hierarchical processes to be considered as one tries to build a theory further out from its home level. Bearing the MPIC in mind perhaps makes it more difficult to always stay in one's home clump of trees, inadvertently ignoring the forest.

Martin et al. (this issue) also suggest that the MPIC provides "no way of ruling out arguments in which the effects predict the causes" (p. 32). Again, this is a limitation of correlational research, not of the MPIC's hierarchical ordering. Experimental research does, of course, reveal causes, and such research can be conducted in the context of an MPIC analysis. For example, as discussed earlier in this reply, Sheldon and Filak (2008) showed that the three SDT needs could be experimentally manipulated, manifesting the predicted effects.

Martin et al. (this issue) also criticized the absence of "stop-rules" in the MPIC. "What constitutes an optimal explanation? At what point do we stop including levels and variables?" (p. 33). Again, this seems mainly a practical question, dependent on the researcher's resources and purposes. In practice, we do as much as we can, then we stop; a complete explanation is only an ideal, never actually reached. What *is* the most optimal research design? Perhaps it is the most *efficient* design, the one that yields the most information for the least resources; ideally it includes most of the major predictors of variance and does not waste resources measuring predictors or levels that account for less variance. In other words, there may be some kind of trade-off between additional effort/resources to be invested and the additional variance to be thereby explained, with a law of diminishing returns holding sway.

Martin et al. (this issue) also object to the postulation of universal processes or generalizations, such as the SDT (Deci & Ryan, this issue) notion that there are universal evolved psychological needs. Noting that needs were the "explanatory force" in the target article's empirical demonstration (in that need-satisfaction mediated higher level personality effects upon SWB), Martin et al. suggest that this only holds "if you accept the needs as universal." Actually, the MPIC does not require this. Instead, the model merely states that there *are* universals that exist beneath individual differences, including perhaps SDT's proposed psychological needs. In the target article we attempted a new empirical test of this SDT proposition. So far the SDT proposition is holding up well, including in experimental investigations (Sheldon & Filak, 2008), but if SDT is eventually shown to be untrue, it does not preclude looking at and testing other types of presumed universals.

Martin et al. (this issue) also asked, "What are the advantages to the simultaneous modeling recommended by the MPIC?" In response, I can only say: As more levels are considered, more complete understanding results. For example, Sheldon and McGregor (2000) found that people's value orientations (intrinsic or extrinsic) had no main effect influence upon their ability to score points within an iterated social dilemma. This was because of a cross-level crossover interaction, such that individual outcomes completely depended on what type of group participants were nested within (extrinsic participants did very well if grouped with intrinsic participants; they did poorly if grouped with other extrinsic participants).

I was initially thrown by Martin et al.'s observation that the target article's empirical example violated the stated "irreducibility" postulate, because the example showed that personality and context effects upon SWB were mediated by need-satisfaction effects. If every level is important, how come they all drop out when need-satisfaction is entered into the equation? Upon reflection, I realized that the mediational analysis was attempting to identify a mechanism by which variations in personality and context produce variations in individual outcomes. If we want to know why citrus fruits cause health, the mechanism is the universal need for vitamin C that citrus fruits satisfy (of course, the specific positive processes enabled by vitamin C could then be considered). This does not explain away the positive effects of citrus fruits (i.e., it does not "reduce them to unimportance"), rather, it explains why they *are* important. The SDT needs theory is meant to explain positive effects of varying personality styles and contexts in the same way.

Mayer and Lang

Mayer and Lang's (this issue) first critique was that "there is, it turns out, a major problem with using biopsychosocial continuum by itself. The situations in which personality operate do not lie on the biopsychosocial continuum" (p. 36). Mayer and Lang further state, "Situations do not lie along the biopsychosocial continuum . . . Situations lie *outside* the brain and outside the person" (p. 37). I was somewhat confused by these observations, because Engel's (1977) biopsychosocial continuum clearly references the person's social relations, relations presumably occurring with other people located outside the person and his or her brain. Similarly, the MPIC is designed to depict a particular person at a particular moment in time, that is, in a particular situation, representing those situations in terms of multilevel nestings of the person within broader and broader social contexts that clearly lie outside of the person. Of interest, although Engel pointed the way toward a biopsychosocial model, he never formally described such a model (http://en.wikipedia.org/wiki/Biopsychosocial_model). Perhaps the MPIC is that model. In any case, I suggest it is more parsimonious for a consilient model to have a single ordering dimension, which includes social and nonsocial situations in which a person can be nested at time t, rather than to include a second, inside/outside dimension that in some ways duplicates what is already contained within the MPIC's single dimension. Again, the "hourglass" graphic of Figure 1 here clarifies this by locating the self at the interface between two regimes: inside the person and outside the person.

To illustrate their contention that a second "inner-outer" dimension needs to be included in a complete model, Mayer and Lang (this issue) reference the target article's multilevel consideration of why a particular person might donate to a charitable foundation. Mayer and Lang postulate an example person, "Michael,"

whom readers are asked to imagine taking a nature walk with an Audubon group. In their words,

> Suppose the tour concluded with Michael and his companions resting at a lakeside picnic area beside pine trees listening to birdsongs. Suppose further that the Audubon guide then mentioned the importance of preserving the environment, the Society's efforts to do so, and the ways that people could contribute to the Society. (p. 37)

These factors presumably influenced Michael's decision to donate.

In response, I would point out that Michael's situation is readily described by the MPIC. There are a group of companions interacting with Michael (one of many possible groups of companions, presumably, and thus type of group composition could be examined as a predictor variable), a guide directing the group (one of many possible guides), and an organization directing the guide (the Audubon society, one of many possible conservation organizations). Because these "outer" levels of multilevel social organization and variation are already included and modeled in MPIC's single dimension, we do not need to add a separate dimension to contain them. And even if Michael had instead taken a solitary hike, one that so inspired him that he spontaneously sought out a conservation group to donate to (deciding, finally, upon Audubon), researchers could still consider that solitary walk as a particular type of situation and compare its effects to other situations Michael might have been nested in (i.e., what if Michael had instead watched TV by himself that day—would he have donated?) to examine that type of situation's effect on Michael's behavior.

As a further argument for their inner/outer distinction, Mayer and Lang (this issue) present a figure (see their Figure 1) that contains *two* vertical ordering axes: a biopsychosocial brain/personality/social groups axis (as in the MPIC model) and, to the right of this axis, a settings/situations/social groups axis. I have difficulty understanding the second vertical axis; the distinction between a setting and a situation is not clear, nor is it clear why situations are located vertically above settings, making situations somehow more complex than settings on a molecular-molar dimension. Most important, it is unclear why the same term, "social group," is located at the top of both vertical axes. If the first vertical axis is inside the person, then why are social groups (an outside factor) at the top of that axis? Also, why do the inside and outside vertical axes overlap at their top levels? In short, I believe the Mayer and Lang inner-outer horizontal dimension may only confuse and confound issues that are more parsimoniously addressed by the MPIC's single vertical axis.

Mayer and Lang (this issue) do make a very interesting critique of the four levels of personality idea,

based on Henrique's (2003) notion that the psychological system has no obvious discontinuities within it (as between vertically adjacent levels of a causal hierarchy), because systems located within personality interpenetrate one another. I believe this is correct, and is one of the main reasons the four levels of personality insertion into the target article's Figure 1 model might be viewed as dubious. This leads to my response to the McAdams and Manczak (this issue) commentary.

McAdams and Manczak

Obviously, my thinking has been greatly influenced by McAdams's seminal depiction of personality as containing "three tiers," and I am sorry that, in my enthusiasm, I have placed Dan in the position of having to recant his model and its implications! As pointed out by Mayer and Lang (this issue), it is difficult to make a convincing argument that the different features of personality are hierarchically arranged. It is also difficult to argue for an emergence principle within personality, that is, the notion that universals "emerge" from the functioning of cognitive processes, that traits "emerge" from the functioning of universals, that goals "emerge" from the functioning of traits, and that selves "emerge" from the functioning of goals. Finally, it is difficult to support the arrangement based on time scale: As McAdams and Manczak (this issue) note, "it is not clear how traits are faster and smaller than goals (Level 3). Indeed, if speed translates into rate of change, it would seem that traits change more slowly than do goals" (p. 40). As stated earlier in this reply, I do not review the time scale difference as crucial to the vertical ordering. Still, the other criticisms are weighty. In the next paragraphs I consider some ways in which the MPIC's particular vertical arrangement may make sense and be useful.

First, the arrangement may make sense from an action theory perspective (Carver & Scheier, 1982, 1998), as discussed earlier in the context of my reply to the Vallerand and Lalande (this issue) commentary. Again, Carver and Scheier (1998) located the self at the top of the action system, as a process that supplies broadest-level goals for the system as a whole. For example, the possible self (Markus & Ruvolo, 1989) of "me as a doctor" might regulate and organize a long-term sequence of goal-based activity that ultimately results in doctorhood for the person who possesses that possible self. The MPIC also depicts goals as nested below selves, fitting Carver and Scheier's (1982) notion that broad possible selves can create and regulate the more specific strivings a person has (i.e., "me as a doctor" dictates and regulates goals to "get straight As as a pre-med," "seek out medical volunteer opportunities," etc.). The MPIC then depicts traits as nested below goals, and it seems reasonable to view traits as strengths or

automatic behavioral propensities that the personality contains, which might be organized and regulated by the goals he or she is seeking (i.e., the goal to "get straight As" makes use of the person's conscientiousness trait to meet specific deadlines or obligations, or perhaps even strengthens that trait if it is in need of boosting). Thus, the MPIC's particular vertical hierarchical arrangement of personality is consistent with the Carver and Scheier model and may help depict how the self can bootstrap itself into its own future by making top-down use of its own available internal resources.

Second, I suggest that the MPIC's particular vertical ordering may also make sense from a bottom-up perspective, when the ordering is viewed as varying along a biopsychosocial dimension (or inner to outer) dimension. McAdams and Manczak (this issue) ask, "Why are selves (Level 4) closer to social relations (one step up from personality) than goals and traits are?" (p. 40). The reason is that universals are grounded in evolutionary biology, and traits are grounded in biological variations in temperament. These are in a sense "far" from the person/other person interface existing at time t. In contrast, the self (at the top level of personality in the MPIC hierarchy) is located right at the interface, at the place where people negotiate with one another as they bear in mind their own goals (at the level below) that they bring into the negotiation. In short, traits are more biological and selves are more social, and thus selves should be located at the person/person interface, not traits. Of course, some traits (i.e., agreeableness, extraversion) are themselves social, and here the analogy may break down. I suggest that social traits can be viewed as automatic preferences, habits, or orientations that influence the nature of the person/other person interface but that are not the controlling features within that interface; rather, the current self being inhabited by the person within that social context is more the controlling factor.

A third ordering principle that may make sense of the MPIC's four levels of personality concerns the complexity of the cognition involved at a particular level, and the relation of that level to consciousness. Traits are habitual, automatic, and genetically and temperamentally based. Goals are prospective and representational, imbued with motivational energy. Selves and identities combine self-representations into complex narratives and goals. In consciousness terms, traits are styles that function mostly nonconsciously (we don't have to "try" to enact our traits); motives are more cognitive (involving representations of what the person wants) and exist partially within and partially outside of consciousness (as in the explicit/implicit motive distinction), and selves are people's representations of the whole system within consciousness, including the person's representation of who the person wants to be in the future. Of interest, McAdams and Manczak (this issue), in their emphasis on the self as a

narrative "story" about the person's own life, seem to emphasize the past: The story is always written after the fact, to explain what has already occurred. Viewing the self as having the function of supplying high-level goals that prospectively influence the person's future may allow for more dynamic understanding of what the person is doing now, at time t.

Fourth, the particular hierarchical arrangement of the MPIC may make sense from a developmental perspective, as McAdams and Manczak (this issue) themselves acknowledge. McAdams and Olson (2010) argued that within the child, traits are there first; then during late childhood and early adolescent typical goals and motives emerge; then during late adolescence and early adulthood identities and life-stories emerge. This notion of long-term developmental emergence is of course not the same as the earlier definition of emergence based on increasing complexity at higher levels of analysis in the here-and-now; it is more of an unfolding over time. McAdams and Manczak (this issue) thus suggest that a "layers" of personality metaphor is better than a "levels" of personality metaphor (i.e., an onion develops by adding additional layers, not new levels). I believe that layers may be an acceptable term, although it would not capture the potential top-down regulation that may occur within a hierarchically organized action system in which an emerging self is trying to gain greater control and mastery of the machinery with which it finds itself entrusted.

I now consider a few additional comments by McAdams and Manczak (this issue). First, in questioning the logic of the MPIC's particular vertical ordering, they stated,

> Perhaps Sheldon et al. are suggesting that social relations themselves have more influence on the development of goals than they do on the development of traits. Even if this is true, is it also true, as the logic would dictate, that social relations have more influence on the construction of selves than on the formulation of goals? Any response short of a resounding "yes" suggests the need for greater articulation of the guiding spatial model. (p. 40)

My answer to this is a tentative, if not a resounding, yes. I do believe that social relations affect selves most directly, because selves are what are involved in social relations (between "I and thou"; Buber, 1937). Of interest, McAdams and Pal's (2006) "fifth principle" of a "new Big Five" postulated that cultures affect personality primarily via selves; in this, they agreed with the proposed vertical ordering of the MPIC and its presumption that the self is the interface between the social milieu and the rest of the person. Indeed, their fifth principle received specific research support in the target article's empirical example.

Of course, social relations might sometimes affect goals in ways that bypass the self, as when the premed's goal of becoming a doctor is not a self-initiated or self-endorsed goal but rather a goal imported (or introjected) directly from her parents' wishes for her. SDT gives considerable attention to this dynamic, in which social contexts may not support the self-internalization of the motives those contexts promote, or may promote motives that are inimical to the person's well-being (Deci & Ryan, this issue). Still, ideally, people's motives can and should be processed through the self, to ensure that those goals are consistent with the self and so that those goals may in turn be regulated by the self (Carver & Scheier, 1998). As a result those goals will feel self-determined and self-concordant to the person (Sheldon & Elliott, 1999).

In addition, McAdams and Manczak (this issue) state,

> Goals (higher level) do not subsume traits (lower level) in the ways that cells subsume (literally contain) molecules. And although it may be true that traits (lower level) constrain and provide resources for goals (higher level) in some ways, the reverse also seems possible. My life goal to be a concert pianist, for example, may reign in my impulsivity. (p. 40)

I agree with this statement, and would like to point out that this is exactly the sort of top-down regulation of the action system and lower level traits by the self that I argued for above by way of Carver and Scheier's control theory model. More generally, doubtless both bottom-up and top-down processes occur within personality. Suppose the preexisting childhood trait of "musical aptitude" (or interest) gives rise to the developmental emergence of musical goals, finally giving rise to the "me as concert pianist" possible self. This is a bottom-up sequence, fitting the layers metaphor. But once the pianist self emerges, it can act back down to regulate its own traits—controlling the impulsivity trait and further harnessing the musical aptitude trait. This better fits the levels of control metaphor.

McAdams and Manczak (this issue) also say,

> The self encompasses traits, goals, stories, and lots of other stuff, too, as William James . . . contended—things like my home, my favorite objects, my pets, and on and on. Therefore, designating a separate level of "self" in any personality hierarchy makes no sense to us. (p. 42)

It is true that the self-concept can reference many things, including traits, favorite objects, pets, and so on. This in part illustrates the "interpenetration" of aspects of personality noted by Henriques (2003). I hope that in this reply I have managed to clarify the notion of self employed in the MPIC model. I view the self not as

a self-concept or story, but rather as a complex mental process, probably evolved (Sedikides & Skowronski, 1997), with two major functions (Sheldon, 2004): to interface the personality with other personalities outside of the person and to help specify and regulate the action system inside of the person. The self is a fictional character that we create, appropriate, live inside of, and project into the world at time t, with strong influence on both who we are perceived to be and what we strive to do. Of course, in the empirical example presented in the target article, "self" was measured in terms of independent self-concept. Admittedly, this does not well fit the definition previously offered, but unfortunately this was the best self-level measure that we had. It may be that a measure of motivational self-determination might be a better measure of authentic and/or effective "selfing" (McAdams, 1996), in that such measures reference a sense of self-ownership both in one's relationship with others and with respect to one's own action initiatives (Deci & Ryan, this issue).

One final reply to McAdams and Manczak's (this issue) commentary. At the end of their commentary they state,

> The three members of the team are not exactly equals. Traits have been around, in one form or another, from the very beginning. Their developmental seniority, rooted as it may be in genetically driven and epigenetically constructed temperament tendencies, may give them more power to constrain life stories, compared to the power that life stories may have to constrain traits. Still, dispositional traits, personal goals and motives, and narrative identity develop on their own paths across the life course, making it impossible to reduce one layer to another. In adulthood, we move through life as social actors, motivated agents, and autobiographical authors, revealing and expressing the full panoply of psychological individuality—all three layers of personality—at any given time or place. (p. 43)

Here, the commentators suggest that the bottom-up effect of traits on selves (construed as "life stories") is stronger than the top-down effect of selves on traits. This seems to be an empirical question. As in the concert pianist example, it certainly seems the arrow can go both ways (see the target article's Figure 2), and we know that traits can change over the lifespan in part due to the person's own goal-directed efforts (Roberts, Caspi, & Moffit, 2003). Yet, I certainly agree that the three levels cannot be reduced to one another, as McAdams and Manczak (this issue) suggest: Researchers need knowledge of all three to even begin approaching comprehensive knowledge of a person. Finally, I would again point out that just because the three levels develop at different times and different ways, and are not reducible to one another, this does not mean that they do not have functional relations.

I suggest that the MPIC model provides a potentially powerful way of considering how different parts of personality are organized and regulated as people live their lives.

Heintzelman and King

I now turn to the comments of my Missouri colleagues, Heintzelman and King. These authors raised questions about the model's ultimate utility. How many researchers are really going to try to carry out the highly ambitious study designs recommended in the final section of the target article? Clearly, Heintzelman and King (this issue) doubt that the model would have much impact on research practice, which is probably true. Heintzelman and King also raise cautions about the potentially misleading nature of the MPIC model, urging that the "local baby not be thrown out with the global bathwater." In their words,

> Breadth should not be gained at the expense of other, equally valuable (if less expansive) research goals. In essence, we argue for a balance between the need to understand local processes and the MPIC model's global goal of creating a comprehensive science. Yes, it is important to see the forest of science beyond the specific trees of particular variables or associations. But there is still much to be learned about the trees before we leave them to consider the forest. Nevertheless, those of us who are occupied with the trees may benefit from the MPIC model. (p. 23)

Later in their commentary, Heintzelman and King state, "The critical question becomes, Is research aimed toward understanding something more broadly, in fact, inherently superior to work devoted to building the foundation of knowledge within each level of analysis?" (pp. 24–25).

I can certainly agree with this critique. In the target article we did not mean to disparage or devalue "local" research, or to say that all research should be conducted with an MPIC perspective in mind. Our goals were more modest than that: to suggest one possible way of putting everything together within a single model, if and when such is desired. In fact, at any particular time t there are only local phenomena occurring, and departing from the local too soon to explore the grand context would be a mistake. Arguably, we can learn about the grand context *only* through careful attention to the local—only there can the evidence for broader-scale influences be detected. Again, the MPIC is proposed mainly as a heuristic for considering the forest, should researchers want to do that. The question I am asking is, What would consilience look like, at least in principle—even if it is too soon to approach it in practice?

Heintzelman and King (this issue) do provide a few suggestions for revising the model. First,

> We suggest eliminating the direct causal arrows, seen in Figure 1, between the lower order levels of analysis and behavior. Behavior is not independently or directly caused by atomic, molecular, or cellular factors in the absence of a higher order determinant serving as a co-contributor. The absence of these levels of analysis as independent influences on behavior is reflected in the observation that scientists in the areas associated with the lower levels of this model are not pursuing the goal of understanding behavior, at least not behavior as it is typically defined. Chemists and molecular biologists might be quite surprised to hear that they study human behavior. (pp. 23–24)

I can somewhat agree with this observation, but not completely. For example, it seems that psychoactive drugs (molecular level of analysis) can affect a person's underlying mood or state of mind directly, unmediated by higher level processes. The research chemists at Pfizer certainly subscribe to this view. Although it is more tempting to remove the arrow from atomic processes to behavior, even this may be premature: There are quantum theories of consciousness and memory that may ultimately turn out to have merit.

Heintzelman and King (this issue) also note that the MPIC model leaves out the physical environment: "The person is embedded not only in a social system but also in a physical one. That physical system includes not only people but objects, places, climates, and events that all play a role in behavior" (p. 24). I agree with this and, as discussed earlier, believe that one might include nonsocial situations as well as social situations in the MPIC model, and thus that one might also include environments within the model (such that the forest environment of Michael's solitary walk helped imbue him with a desire to donate to a conservation fund). Following the expanding logic of the hierarchy, one might develop a typology of different forest environments in which a person might find themselves at time t (i.e., "savannah," "old-growth," "conifer"), with forest environments in turn representing only one type of the broader category of "natural environment" (along with "beach," "mountain," and "desert"). I am not sure how useful this would be, but it is possible in principle.

Heintzelman and King also question the MPIC's seeming reliance on self-report, saying, "There are variables that might play a causal role, even in well-being, that are not available to conscious reflection. How might the MPIC model be applied to such outcomes and variables?" (p. 24). The response here is similar to our response to Martin et al. (this issue): The MPIC is not wedded to any particular types of measures or methodologies, although the empirical example provided by the target article certainly was. Of

course, when studying self-level processes, self-report often provides a good measurement approach. However, when studying other levels (such as motives), implicit measures of nonconscious processes will often be desirable. Indeed, implicit measures could represent the motive level in MPIC-inspired studies just as easily as explicit measures could. Also, nonconscious processes (i.e., reaction time differences) could be used as the outcomes to be predicted via an MPIC analysis. More generally, problems with self-reports remain as problems for the field as a whole, and not just for the MPIC.

Finally, Heintzelman and King (this issue) pose two goals for the MPIC model. The first:

> Identifying *the proximity* of levels of analysis. If these borders were well specified and empirically supported, researchers would face a different and perhaps less daunting challenge. Specifically, identifying truly adjacent levels of analysis would allow researchers to include, not the entire model but, perhaps, variables from one level up or down to begin branching out. (p. 25).

I agree that this is an important issue, and hope that the level boundaries suggested in the target article are correct, or at least useful as a starting point. The second goal involves

> identifying *the direction* of influence from one level to another. Sheldon, Cheng, and Hilpert perhaps hedge their bets in this regard, preferring bidirectional arrows between each level. It might be the case some of these arrows are, in fact, one-way streets. (p. 25)

This is an interesting proposition, but it is hard to actually think of any one-way streets. Reality interpenetrates itself, just as personality does, and no part is immune to influence from other parts. Getting up to turn off a light switch has ramifications for countless neural, bodily, molecular, and atomic processes within one's own body, and a cultural revolution can have ramifications upon countless relationships and personalities and bodies within that culture. But conversely, even the highest levels of organization are ultimately built upon and constrained by every level below, such that a "butterfly" effect at a molecular level can potentially influence global phenomena far away. However, it is likely true that for many phenomena, the regnant, dominant, or most important causal factors reside at one or just a few levels at a given time t, and that some levels rarely have direct influence upon some phenomena. In the quest for an efficient and cost-effective design (maximizing information yield per unit of resource expenditure), discussed earlier, one might thereby choose to ignore these levels.

Kitayama and Na

Kitamaya and Na (this issue) are appreciative of the scope of the MPIC model and agree that hierarchical perspectives such as the MPIC's may turn out to be very fruitful. However, they also express three main reservations. The first:

> We feel hesitant in endorsing the proposal to regard autonomy, competence, and relatedness as psychological needs that provide the foundation of personality. Unlike Sheldon and colleagues, we believe that autonomy, competence, and relatedness can best be conceptualized as three of (potentially many) eudaimonic dimensions of well-being. (pp. 26–27)

In response, I would again point out that acceptance of the MPIC does not require acceptance of SDT. What the MPIC proposes is that there are human universals beneath of individual differences, a topic that is typically the focus of evolutionary psychological research. SDT's psychological needs proposals were used in the target article data example to illustrate how the activation of a universal process by varying personal, social, and cultural factors may explain a particular outcome, in this case, SWB. But again, other proposed universal processes (attraction, behavior in social dilemmas, etc.) could just as easily be targeted within the MPIC.

Turning to the interesting question of whether there are psychological needs, and if there are, whether autonomy, competence, and relatedness are them: Kitayama and Na (this issue) suggest that these three experiences are actually facets of eudaemonic well-being, endorsing the broad conception of well-being proposed in the Ryff and Keyes (1995) model. Although it is certainly possible to conceptualize the needs this way, that is, as outcomes and as facets of well-being only, I believe it is more useful to take a motivational perspective, conceptualizing the needs as universal motives that evolved because they helped humans solve important adaptive problems. All humans have to "crack" the very basic problems of behaving effectively and competently, of creating nurturing social relationships, and of making appropriate choices and achieving greater self-regulation in life (Deci & Ryan, this issue). It is not difficult to believe that those who wanted more of these experiences than their conspecifics, and who thereby got more such experiences, might have had a selective advantage over those without these motives (Baumeister & Leary, 1995). Indeed, the three needs are among the most important topics of psychotherapy; clients come to therapy because they want to know themselves better, want to have better relationships, and want to function better in their personal and vocational lives. Keeping the needs on the "predictor" side instead of the "outcome" side of the equation affords targeting them via interventions, and

affords validating them as the mediators that can explain the positive effects of interventions.

Thus, my view is that SWB should be restricted to basic mood and context-free global life satisfaction. Various "psychosocial qualities" (or contents) can then be examined and tested as predictors or causes of content-free SWB. This cannot happen if psychosocial contents are instead lumped in with SWB by definition (Kashdan, Biswas-Diener, & King, 2008). For example, Sheldon, Elliott, Kim, and Kasser (2001) tested "10 candidate needs" as predictors of the mood felt within "most satisfying events," finding support for SDT's three postulated needs as well as for self-esteem (but not for luxury, security, meaning, pleasure, health, or popularity). Kitayama and Na (this issue) suggested that honor, religious purity, and hierarchical social order might also be tested as needs, and I agree that such testing should occur. However, I am skeptical that these would turn out to be as universally important as are autonomy, competence, and relatedness.

Now I turn to Kitayama and Na's (this issue) second concern, which

> relates to the notion of levels in personality organization. We worry that Sheldon et al. might not have pushed far enough the theoretical potential of the notion of levels in personality organization. We believe that the levels concept is powerful because of its ability to highlight the dynamic process by which a higher, emerging level of organization can transform functions and meanings of existing elements in the lower levels of organization. Sheldon et al. fail to give a sufficient emphasis on this theoretical potential of the levels concept. (p. 27)

I agree that the potential of emergent processes to organize and regulate lower levels was not given enough attention in the target article. I hope that this reply's analysis of potential executive control by the self, applying Carver and Scheier's (1982, 1998) control theory model, helps to underscore this important potential. Kitayama and Na suggest many subtle and interesting ways of developing the notion of interactions within and between different levels of hierarchical causation. Chiefly, they argue that higher levels do not merely influence the levels below but can also actually transform the functions and meanings of lower elements. Thus, Kitayama and Na seem to endorse a somewhat radical top-down view in which "an organization at a higher level can realign, redefine, and reconfigure existing processes and structures at lower levels. At the new level, the existing processes and structures are given new functions, new configurations, and new meanings" (p. 28). I certainly agree that such radical reconfiguration can happen, and point to the highest, self level of personality as the place where such processes might have greatest impact upon the rest of personality (and

the person's temporal destiny). Returning to the doctor example, the emergence into consciousness of the possible self of "me as a dancer, not a doctor" might change a person's whole life course. Further consideration of Kitayama and Na's (this issue) comment reveals that they are thinking primarily of culture, as containing the potential to shape everything else below it. Once again, I agree that this can happen, and I applaud these authors for their sophisticated consideration of these cultural processes.

However, it is possible that Kitayama and Na go too far in their emphasis on cultural determination, as perhaps illustrated in their third reservation, which

> is an extension of our second point in respect to the nature of culture and cultural influence. As a higher order organization of behavior, culture can transform functions and meanings of existing psychological processes and structures. By incorporating this point, we argue, the Sheldon et al. framework will fulfill its potential. (p. 27)

Here, the commentators seem to be arguing for the ultimate prepotence of the cultural level of analysis, which is of course their own primary topic of study. It has been common in the turf wars between different sciences, and different levels of science, for advocates of each level to espouse their particular level as most important. This is typically a strategy of reductionists, who argue that everything boils down to brain or neuronal processes. However holists can overemphasize their level as well, and Kitayama and Na seem to be taking a "sociological holism" perspective (Durkheim, 1938; Kincaid, 1997), according to which the top-down influence of culture supercedes other levels. Again, the MPIC asserts that each of the levels has irreducible influence, and thus that arrows from each level to behavior must remain. The MPIC attempts to represent the moderator processes discussed by Kitayama and Na via a cross-level arrow reaching from culture to moderate social effects at the level below. This may turn out to be a simplistic and inadequate model, but I hope that more sophisticated models will still be contextualizable in some expanded depiction of the MPIC model.

This concludes the response. I thank the commentators for taking the MPIC seriously and for suggesting multiple fruitful avenues for developing and applying the model further.

Note

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