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Revisiting the Constraint Attunement Hypothesis: Reply to Ericsson, Patel, and Kintsch (2000) and Simon and Gobet (2000)

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This article is part of an exchange concerning the contributions of the constraint attunement hypothesis (CAH) to the understanding of expertise effects in memory recall. K. A. Ericsson, V. Patel, and W. Kintsch (2000) and H. A. Simon and F. Gobet (2000) claim that the CAH is not novel and that existing theories of this phenomenon do not have the limitations that were attributed to them. In this reply, the CAH is argued to be the only theory of expertise effects in memory recall to adopt the abstraction hierarchy as a theory of the environment, a feature that has important theoretical implications. Also, other theories focus on psychological mechanisms but have not satisfied the burden of scientific proof required of process theories. Progress can be made by integrating the complementary advantages of existing theories into a unified theory that acknowledges the equally important roles of the organism and the environment.

> Viewed as a geometric figure, the ant's path is irregular, complex, hard to describe. But its complexity is really a complexity in the surface of the beach, not a complexity in the ant. (Simon, 1981, p. 64)

I am very pleased to participate in this exchange with Ericsson, Patel, and Kintsch (2000) and Simon and Gobet (2000) regarding the constraint attunement hypothesis (CAH) proposed by Vicente and Wang (1998) as an ecological theory of expertise effects in memory recall. Given the limited space available for this reply, I focus on the two most important criticisms that have been levied against the CAH: (a) "[The] theory is not novel" (Simon & Gobet, 2000, p. 593), and (b) "other current published theories . . . do not have the defects that Vicente and Wang attributed to them" (Simon & Gobet, 2000, p. 593; see also Ericsson et al., 2000, p. 578). In doing so, I argue that the CAH and previously published theories make complementary contributions, not only to expertise effects in memory recall but also to the expertise literature at large. First, however, I clarify some basic distinctions that provide the theoretical foundations for the subsequent discussion.

Theoretical Foundations

For one to appreciate the unique contributions of the CAH, three distinctions need to be clarified: (a) the difference between a theory of the environment and a theory of psychological mechanisms, (b) the difference between a product theory and a process theory, and (c) the difference between pointing out the limitations of a theory and rejecting it.

Theories of the Environment Versus Theories of Psychological Mechanisms

The gist of Vicente and Wang's (1998) CAH can be summarized succinctly:

> There can be expertise effects when there are goal-relevant constraints . . . that experts can exploit to structure the stimuli. The more constraint available, the greater the expertise advantage can be. . . . To realize these potential advantages, experts must be attuned . . . to the goal-relevant constraints in question. (p. 36)

For this hypothesis to have any potency, there must be an explicit theory of the environment that can be used to identify goal-relevant constraints across domains of expertise in a comparable fashion. In terms of the parable quoted above, one must model the "beach" in a principled way. The abstraction hierarchy (Rasmussen, 1985) was adopted to satisfy this need. I believe that the abstraction hierarchy is the most important theoretical contribution of the CAH to the expertise literature.

Because theories of the environment are not commonplace in psychology, it is important to clearly define this and related notions. For the purposes of this article, I propose the following definitions. The *environment* is that which is external to a person. For example, for a weather forecaster, everything external to that person is part of the environment. A *model of the environment* is a selective description of the environment for a particular domain of expertise based on a set of equivalence classes. Because it is an abstraction, the model of the environment has a finite set of dimensions. Thus, it should be distinguished from the environment itself, which is not an abstraction, and thus has an infinite set of dimensions. For example, Stewart, Roebber, and Bosart (1997) developed a model of the environment for weather forecasting. A *theory of the environment* is a generalizable structure, consisting of an invariant set of principles or properties that can be used to

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create comparable models of the environment. Its role is similar to that of a cognitive architecture. Just as a single cognitive architecture can be used to develop different but comparable cognitive models for diverse psychological phenomena, a theory of the environment can be used to develop different but comparable models of the environment for diverse domains of expertise. For example, Brunswik (1955) provided a generalizable structure consisting of environmental cues, each with its own ecological validity. This structure was used by Stewart et al. to develop their model of a weather environment and by many other researchers to develop environmental models for other domains of expertise (see Hammond, 1996, for a review). The details of such models usually differ tremendously from one domain to the next because the relevant cues and their ecological validities can change dramatically. Nevertheless, all such models are comparable because they were developed from a common theory of the environment. Such consistency allows researchers to develop cumulative findings by comparing and integrating results across environments.

In the case of the CAH, the abstraction hierarchy serves as a theory of the environment. Its generalizable principles or properties have been described in detail by Rasmussen (1985), Vicente (1999), and Vicente and Wang (1998). When the abstraction hierarchy is instantiated for a particular domain of expertise, the result is an abstraction hierarchy model that serves as a model of that environment. Such models have been developed for several domains of expertise from the following sectors: aviation, computer software, conventional power plants, engineering design, library information retrieval, medicine, nuclear power plants, petrochemical plants, and workplace design (see Vicente, 1999, for a review). These models of the environment are comparable because they all were developed from a common theory of the environment.

It is important to distinguish a model of the environment from an expert's mental representation of that same environment. At the risk of overly simplifying matters, this distinction is similar to the venerable philosophical distinction between ontology and epistemology, respectively. Ontological models make claims about the environment, whereas epistemological models make claims about an individual's knowledge of that environment. Of course, ontological claims are always limited by the science of the day and thereby are subject to revision. However, this does not change the fact that the object of description is different in the two cases (i.e., the environment vs. an individual's knowledge, respectively).

A simple example can illustrate this distinction. If one were interested in developing a mental representation of an expert weather forecaster, one might use a number of knowledge elicitation techniques to determine, among other things, how much weight that expert gives to the variable known as *minimum vorticity advection* when forecasting the probability of precipitation for a 48-hr period. In contrast, if one were interested in developing an environmental model of weather forecasting, one might instead measure the minimum vorticity advection over many days as well as the actual precipitation 48 hr later over the same period. On the basis of these data, one could develop a model of the environment that would specify the diagnosticity of minimum vorticity advection as a cue for making 48-hr forecasts (e.g., Stewart et al., 1997). Although not all examples are this clear-cut, this contrast illustrates the fact that an expert's mental representation of an environment and a model of the environment describe two different things. As I argue later, both are valuable because they provide complementary insights that are critical to understanding the adaptive nature of expertise.

Product Theories Versus Process Theories

The aims of the CAH are unique because it is a product theory rather than a process theory. Long-term working memory (LTWM) theory (Ericsson & Kintsch, 1995) and template theory (Gobet & Simon, 1996) were deliberately intended to identify psychological mechanisms and representations that were hypothesized to be responsible for expert recall performance. In contrast, the CAH is a black box "mechanism-free casting of a psychological theory" (Anderson, 1991, p. 4). As such, it makes no attempt to identify mechanisms or representations in the head; the CAH addresses three basic questions: "1. How should one represent the constraints that the environment (i.e., the problem domain) places on expertise? 2. Under what conditions will there be an expertise advantage? 3. What factors determine how large that advantage will be?" (Vicente & Wang, 1998, p. 35). Thus, product and process theories have different aims and are not subject to the same burden of proof. This point is explored in more detail later.

Criticism ≠ Rejection

For one to appreciate Vicente and Wang's (1998) intentions, it is also important to distinguish between critiquing other theories and rejecting them. More specifically, Vicente and Wang made three related claims. First, other theories make important contributions not captured by the CAH: "Internal mechanisms and structures specified by a process theory add valuable knowledge that is complementary to that offered by a product theory" (Vicente & Wang, 1998, p. 35). Second, other theories have limitations indicating that there is room for improvement: "An adequate process theory of expertise effects in memory recall has remained elusive" (Vicente & Wang, 1998, p. 35). Otherwise, there would be no need for further research on this topic. Third, the CAH addresses some of these limitations: "The constraint attunement hypothesis makes a novel and significant contribution to the literature" (Vicente & Wang, 1998, p. 50).

Some reviewers have expressed the opinion that this set of claims is contradictory. That opinion holds if theoretical development is viewed as a mutually exclusive, bipolar activity. My commentators appear to hold this view because they are interested in establishing whether "the process-based framework of LTWM theory is . . . superior to . . . [Vicente and Wang's] product theory" (Ericsson et al., 2000, p. 578) or which are "stronger theories" (Simon & Gobet, 2000, p. 599). However, if theoretical development is instead viewed as "modifying and extending successful theories to improve their scope and accuracy" (Simon & Gobet, 2000, p. 599), then Vicente and Wang's (1998) three claims are entirely consistent with each other. The fact that other theories make unique contributions does not mean that the CAH cannot also do the same. Rather than presenting a conflict, this viewpoint instead opens up the possibility of theoretical integration that can foster cumulative, unified psychological science.

Is the Constraint Attunement Hypothesis Novel?

Given this theoretical foundation, the first major criticism levied against the CAH can be addressed. Vicente and Wang (1998)

stated that the CAH "is a novel theoretical explanation for expertise effects in memory recall" (p. 36), but Simon and Gobet (2000) claim that "[the] theory is not novel" (p. 593). In this section, I define more precisely the unique contribution of the CAH and discuss why that contribution is important theoretically.

An Ecological Theory

The CAH is novel because it is the only ecological theory of expertise effects in memory recall, and thus as shown in Figure 1A, it requires a theory of the environment as well as a theory of the organism. Of course, ecological theories are hardly new to psychology. Several prominent psychologists have argued that the environment itself is an object worthy of direct investigation (e.g., Anderson, 1991; Brunswik, 1955; E. J. Gibson, 1969; J. J. Gibson, 1979; Gigerenzer, Todd, & the ABC Research Group, 1999; Neisser, 1987; Tolman & Brunswik, 1935). In other publications (e.g., Ericsson & Lehmann, 1996; Simon, 1956), my commentators have agreed that it is important to take account of the environment in understanding human behavior. However, just because they do not deny the importance of the environment does not mean that they have constructed explicit theories of the environment to better understand expertise effects in memory recall. They have not. Both LTWM theory and template theory focus on describing psychological mechanisms. Figure 1B shows this organism-centered approach. The environment half of the figure is labeled with a "?" because such theories do not provide an explicit theory of the environment.

Thus, the primary novelty in the CAH was to apply the ecological approach to the memory recall literature by bringing the role of the environment to the fore.[1] As shown in Figure 1C, this goal was achieved in two steps. First, it was hypothesized that experts are attuned to goal-relevant constraints in the environment, a view

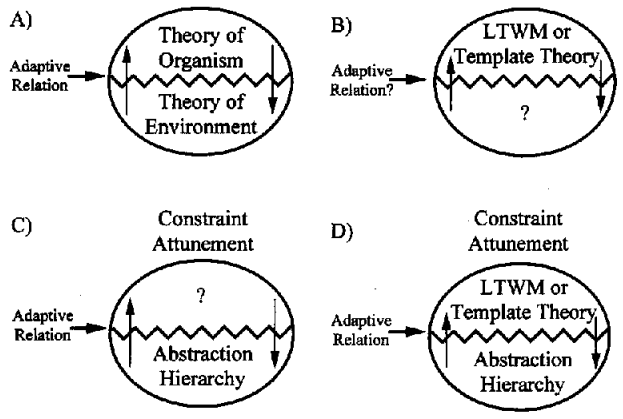

Figure 1. Schematic diagrams of (A) the ecological approach to psychological theory, (B) organism-centered theories of expertise effects in memory recall (e.g., long-term working memory theory or template theory), (C) an ecological theory of expertise effects in memory recall (i.e., the constraint attunement hypothesis), and (D) a hypothetical integrative theory that combines the complementary contributions of existing organism-centered and ecological theories. Upward arrows indicate inputs from the environment to the organism, whereas downward arrows indicate outputs from the organism to the environment. LTWM = long-term working memory.

that has received a great deal of empirical support in other areas of psychology (e.g., E. J. Gibson, 1969). This hypothesis provides a rough approximation for the input–output function that describes the organism's behavior, which is why the label *Constraint Attunement* appears above the organism half of Figure 1C. However, the psychological mechanisms responsible for constraint attunement were deliberately left unspecified, which is why there is a "?" inside the organism half of Figure 1C. Second, the abstraction hierarchy was adopted as a theory of the environment (see Figure 1C), thereby providing an explicit basis for modeling diverse domains of expertise in a comparable way. The rationale behind such an approach was appreciated by Simon (1956): "We might hope to discover, by a careful examination of some of the fundamental structural characteristics of the environment, some further clues as to the nature of the approximating mechanisms used in decision making" (p. 130). The CAH is the only theory of expertise effects in memory recall to use the abstraction hierarchy as a theory of the environment.

Simon and Gobet (2000) dispute Vicente and Wang's (1998) claim of novelty: "A process theory that incorporated the characteristics of the task domain as integral components long preceded CAH" (p. 599). Note, however, that Simon and Gobet do not refer to the abstraction hierarchy, which is a contribution that is unique to the CAH. Thus, their criticism that the CAH "is not novel" (Simon & Gobet, 2000, p. 593) is incorrect.

Perhaps this disagreement is caused by underlying differences about what it means to have a theory of the environment. The definition provided earlier is consistent with the examples set by Brunswik's (1955) concept of *ecological validity* and J. J. Gibson's (1979) concept of *affordance*. There are two features that appear to distinguish this view of a theory of the environment from the views of my commentators. First, the environment itself, rather than an individual's mental representation of the environment or a participant's activity in the environment, is the object of theorizing. The abstraction hierarchy fulfills this criterion:

> Each level in the [abstraction] hierarchy represents a normative model of the goal-relevant constraints in the world. Note that the abstraction hierarchy represents the problem domain and is therefore not a task analysis. This distinction is key: A task describes or prescribes human problem-solving activity, whereas a problem domain is the object of that activity. (Vicente & Wang, 1998, p. 36)

LTWM theory and template theory do not fulfill this criterion because *chunks, retrieval structures, problem spaces, situation models,* and *templates* are all mental constructs. Second, a theory of the environment must also provide an explicit basis for modeling different domains in a comparable manner. Like Brunswik's ecological validities and J. J. Gibson's affordances, the abstraction hierarchy satisfies this criterion as well:

> The abstraction hierarchy is not a specific [model] . . . but rather a [theory] . . . that a modeler can use to develop [models] . . . for various problem domains. The number of levels and their content will vary as a function of the types of constraints in each domain. Regardless of

[1] In situating LTWM theory, Ericsson et al. (2000) state, "We considered our research to be consistent with pioneering efforts in ecological psychology" (p. 578). However, Ericsson and Kintsch (1995) did not discuss or cite any of the ecological literature.

> the domain, however, an abstraction hierarchy [model] . . . will have the properties described above. (Vicente & Wang, 1998, p. 37)

LTWM theory and template theory also do not fulfill this criterion because they provide no constructs that can be used to model different environments in a comparable manner.

To be fair, Simon and Gobet (2000) correctly point out that process theories do not ignore the influence of the environment. As shown by the upward arrow in Figure 1B, this influence cannot be avoided; otherwise, the process model will have no inputs. Simon and Gobet also correctly point out that "proponents of chunking processes have made detailed analyses of the constraints offered by the statistical properties of the chess environment" (p. 598). However, the definition of inputs from the environment and the conducting of detailed analyses for one domain of expertise do not add up to a theory of the environment, as defined earlier. Simon and Gobet implicitly admit as much when they state that "template theory . . . acquires chunks by scanning a database of [chess] games" (p. 594). This process of scanning a database to identify chunks—a psychological construct—is essentially a surrogate for an explicit theory of the environment and a direct investigation of the structure of the environment. Given that the abstraction hierarchy provides an explicit and systematic basis for modeling different environments, it is surprising that Simon and Gobet repeatedly criticize the CAH because goal-relevant "constraints must be constructed and rationalized ad hoc for each task" (p. 598; see also p. 597).

This point is important because, as Simon and Gobet (2000) point out, "relevance is a condition for attention" (p. 597). The question then becomes, how does one establish what is relevant? The abstraction hierarchy provides an explicit basis for answering this question across domains of expertise in a comparable fashion. This explicit, theory-driven approach seems preferable to the implicit, data-driven approach of scanning a database of chess games.

In summary, Simon and Gobet (2000) are incorrect in stating that the CAH is not a novel theory of expertise effects in memory recall. The CAH is the only theory of expertise effects in memory recall to provide an explicit theory of the environment in the form of the abstraction hierarchy. In the remainder of this section, I show that this novel aspect of the CAH provides valuable theoretical insights that are not as readily obtained from other perspectives.

Implications: Defending the Claim of Adaptation

One important benefit of having an explicit theory of the environment is that it helps defend the claim that human expertise is best viewed as an adaptive process. Both of my commentators have described expertise as adaptation, briefly in their comments and in greater length elsewhere. For example, in a prominently featured review article, Ericsson and Lehmann (1996) stated that "analyses of expert performance in many domains . . . reveal maximal adaptations of experts to domain-specific constraints" (p. 273). Simon (1956, 1990) expressed a similar view. I also view expertise in terms of adaptation, so this topic provides a good focal point for showing how the novel aspects of the CAH can help advance the theoretical aims of my commentators.

Evolutionary biologists have defined *adaptation* as "the good fit of organisms to their environment" (Gould & Lewontin, 1979, p. 592). Thus, as shown in Figure 1A, "adaptive change . . . is as much governed by a system's environment as by its internal constitution" (Simon, 1990, p. 2). It follows that "we need a theory of processes . . . as well as a theory of the requirements of the task" (Simon, 1990, p. 11). Neither LTWM theory nor template theory includes an explicit theory of the environment that can be instantiated across diverse domains of expertise. Consequently, one of the two pieces of the adaptive relation is missing (see Figure 1B). Adaptation, yes, but to what? Because this question has not been answered explicitly and systematically, it is not clear how my commentators can defend scientifically the claim that human expertise is an adaptive process.

It is interesting to note that a loose theoretical claim of adaptation can be identified in evolutionary biology. As Gould and Lewontin (1979) pointed out in their frequently cited article, "Often, evolutionists use consistency with natural selection as the sole criterion and consider their work done when they concoct a plausible story. But plausible stories can always be told" (p. 588). This loose approach has grave consequences because it "makes the adaptive program an exercise in plausible story telling rather than a science of testable hypotheses" (Lewontin, 1979, p. 11).

The CAH provides a missing piece of the puzzle—a theory of the environment that provides a referent for empirical tests of the inherently relational claim of adaptation (see Figure 1D). As such, it makes a significant contribution to the expertise literature that could be, but has not yet been, incorporated into existing process theories, such as LTWM theory and template theory.

Implications: Accounting for Differences Across Domains

An explicit theory of the environment can also help to account for what might otherwise appear to be conflicting empirical results across domains of expertise. Vicente and Wang (1998, p. 46) highlighted this point by citing one study from process control and five studies from medicine for which the results seemed anomalous.[2] Ericsson et al. (2000) cite six more studies and emphasize that "there are many instances in which experienced individuals' memory performance for representative stimuli is not superior to that of novices" (p. 582). Collectively, these studies represent a challenge for any theory because their findings indicate a failure to replicate the classic Expertise × Stimulus interaction that has been observed in chess and many other domains of expertise.

How can one account for the existence of these conflicting results? Simon and Gobet (2000) do not address this question, so I do not know how their template theory may attempt to account for these different findings. Similarly, Ericsson et al. (2000) do not mention the seemingly anomalous findings from process control, so I do not know how Ericsson and Kintsch's (1995) LTWM theory might try to account for those results. For the case of medicine, Ericsson et al. argue that experts rely on "higher level information about the patients' medical condition to facilitate reasoning and evaluation of diagnostic alternatives" (p. 588) and that these concepts do not allow medical experts to reproduce a literal lower level recall of the presented information. However,

[2] Therefore, contrary to what Ericsson et al. (2000) claim, Vicente and Wang (1998) explicitly cited considerably more than one study for which the results could not be explained by LTWM theory.

experts in other domains (e.g., chess) also rely on higher level concepts (e.g., Queen's Gambit) for reasoning and evaluation and are still able to exhibit an advantage over less skilled participants on literal lower level recall. Ericsson et al. are aware of these differences across domains of expertise: "[Higher level concepts] may or may not be effective for literal memorization and reproduction of presented information" (p. 584). However, neither they nor Simon and Gobet provide a basis for knowing, a priori, under what conditions there will be an Expertise × Stimulus interaction.

In contrast, the CAH can directly address these varying results. The CAH attributes these differences to variations in the strength of coupling between lower level (physical) and higher level (functional) levels of an abstraction hierarchy model. As examples, consider process control and chess. In process control, there is a very loose coupling between physical and functional levels because there is an infinite set of physical variable values that can account for a given functional state. Thus, it is not possible to reconstruct lower level details solely from knowledge of high-order relations. In chess, there is a tighter coupling between physical and functional levels because the precise physical location of a constellation of pieces has an important role in determining the meaning of a position. Thus, memory of the higher level functional state of a chessboard (e.g., Queen's Gambit) provides a great deal of help when a chess player is reconstructing the specific lower level position of individual pieces. This tighter coupling allows experts to reconstruct many lower level details merely from knowledge of higher order relations.

In general, functional higher level mental representations can be developed only if there is higher order structure in the environment in the first place. If one has an explicit theory of the environment, then one can provide a systematic account of differences in results across domains. Without such a theory, these differences cannot be explained as convincingly because "it is what is outside the human head not what is inside that is controlling the memory performance" (Anderson, 1991, p. 12). Therefore, not only is the CAH novel, but its unique contributions have important theoretical implications.

Do Other Theories Have Limitations?

Both Ericsson et al. (2000) and Simon and Gobet (2000) claim that their respective theories do not have the limitations that Vicente and Wang (1998) attributed to them. They each describe a (different) set of psychological mechanisms that they claim account for all of the data reviewed by Vicente and Wang. Whether their respective claims are accurate is an empirical question and thus cannot be decided by fiat. Therefore, rather than assessing in detail the plausibility of the arguments put forth by my commentators, I examine the empirical basis for their claims. This examination shows that LTWM theory and template theory cannot claim to have identified the psychological mechanisms and representations that are responsible for expertise effects in memory recall across the domains of expertise that have been studied to date.

Recall that the CAH is a product theory, so it makes no attempt to identify the psychological mechanisms that are responsible for memory recall. An implication of this choice is noted by Ericsson et al. (2000): "By explicitly stating that their hypothesis is not a process theory, they [Vicente and Wang] insulated themselves from criticism regarding any issues concerning mechanisms that mediate this type of superior expert memory performance" (p. 579). Because they attempt to identify psychological mechanisms, process theories (e.g., LTWM theory and template theory) are not immune from such criticisms. That is why Vicente and Wang (1998) "still criticized the mechanisms specified by other competing theories" (Ericsson et al., 2000, p. 579). Scientific accountability is commensurate with scientific aims, and for this reason, process theories must live up to a different burden of evidentiary proof than product theories.

Vicente and Wang (1998) argued that both LTWM theory and template theory have not satisfied the standard of evidence that is consistent with their stated (process) aims. Specifically, these process theories have not explained all of the expertise effects in memory recall that have been documented in the many domains studied to date. Ericsson et al. (2000) and Simon and Gobet (2000) disagree and provide detailed plausible arguments in their favor. However, if process theories are to live up to their burden of proof, then there must be direct evidence concerning the psychological representations and mechanisms that comprise the respective theories. Otherwise, the claims made by process theories are speculative, and evaluations of their validity must await further data collection.

Richman, Staszewski, and Simon (1995) provided an excellent role model for this type of research. They used the concept of retrieval structures to build a computer simulation to explain data obtained from a longitudinal study of an expert mnemonist. This study is impressive because (a) the evidentiary support for the role of mental representations is clearly documented because the retrieval structures used by the expert are tightly grounded in empirical data and are explicitly shown (see Figures 3 and 7 in Richman et al., 1995) and (b) there is a very good quantitative fit between the performance data produced by the computer simulation and that obtained from the single human participant.

Simon and Gobet (2000) and Ericsson et al. (2000) have chosen to propose process theories, and they repeatedly emphasize the advantages of that choice in their articles. However, the question remains: Is there strong evidence of the type provided by Richman et al. (1995) to support the claims made by template theory and LTWM theory?

Template Theory: Where Are the Simulations?

Gobet and Simon's (1996) template theory is more ambitious than LTWM theory because, in addition to being a process theory, it has been formalized and embedded in computer programs that make detailed quantitative predictions for the domain of chess, an impressive achievement. Nevertheless, Simon and Gobet (2000) have not yet tried to meet the burden of proof required of process theories for the many other domains in which expertise effects have been observed.

The need for a different burden of proof for computational process theories is convincingly illustrated by Simon and Gobet's (2000) own words. They try to explain Chase and Simon's (1973) observation of an expertise advantage on memory for random but legal chess moves. In 1973, Chase and Simon were surprised by

this result and could not offer any theoretical explanation for it.[3] Now, Simon and Gobet state that the result is actually consistent with their theories. Moreover, they attribute Chase and Simon's surprise to "the incautious prediction of the experimenters, who did not use MAPP [i.e., a computer program] to check their guess (illustrating the frequent superiority in reasoning from precise models rather than words)" (Simon & Gobet, 2000, p. 596).[4] So to generalize, when one has a formal process theory embedded in a computer simulation, a prediction made without running the simulation is incautious and should be treated only as a guess. Accordingly, using Simon and Gobet's own criteria, the "predictions" made on pages 596–598 of their article should be treated as guesses generated by an incautious procedure. These guesses cannot be taken as evidence that template theory can account for the same evidence explained by the CAH. To reach that conclusion, Simon and Gobet would need to create computer programs for process control, medicine, computer software, and the many other domains that have been examined with the memory recall paradigm. Even in chess, they would need to simulate a program under the same conditions used in all of the experiments for which they aim to explain the results. Simon and Gobet provide no such evidence. As far as I know, it does not exist.

The paucity of tests of template theory's generality has been acknowledged by Gobet (1998): "The generality of this [i.e., template] theory outside the realm of chess has yet to be established" (p. 147). "Whether it will be as successful in other domains of expertise, or whether another theory would fare better, has to be established by rigorously testing it against empirical data along several dimensions, as has been done in this paper for chess" (p. 148). Elsewhere, Gobet (1997) stated, "One of the main teachings of recent research is that different task environments tax cognitive functions differently. . . . Hence, it is unlikely that findings from chess research will apply without qualification to other domains" (p. 311). These statements indicate that template theory has not been tested outside of chess and imply that it is unlikely to generalize to the other domains reviewed by Vicente and Wang (1998). There is a stark contrast between Gobet's (1997, 1998) frank, but realistic, statements and the speculative claims made by Simon and Gobet (2000).

Long-Term Working Memory Theory: Where Are the Retrieval Structures?

Does Ericsson and Kintsch's (1995) LTWM theory fare any better? Retrieval structures are a key mental construct in LTWM theory (see Figures 1, 2, and 4 in Ericsson & Kintsch, 1995). Their explanatory power should be demonstrated by empirical evidence showing that such mental constructs are at play in each domain of expertise that has been studied. Ericsson et al. (2000) offer no such data. For instance, Ericsson et al. refer to "the integrated structures generated during the comprehension of a chess position" (p. 585), but unlike Richman et al. (1995), they do not show what those structures look like or how their existence is supported by empirical data.

To take a more detailed example, consider the research of Coughlin and Patel (1987). Ericsson et al. (2000) provide a problematic description of how LTWM theory might account for the findings in that study.[5] Aside from those problems, Ericsson et al. do not show the retrieval structures that experts purportedly used in Coughlin and Patel's study. What do the retrieval structures for medicine look like? Where are the data to identify such structures? If such questions have not been answered, then Ericsson et al.'s claim that LTWM theory can explain Coughlin and Patel's findings is premature. The same can be said for most of the other domains of expertise in which expertise effects in recall have been observed. Direct and detailed empirical evidence of the type required to meet the burden of proof for a process theory (e.g., Richman et al., 1995) has not been presented. I am not alone in arguing that LTWM theory has limited empirical support for this phenomenon:

> The general mechanism [that Ericsson and Kintsch (1995)] propose does not apply to the wide range of domains they claim it does. It is not the case that generic retrieval structures develop within domains such as medical expertise or chess, or in other domains where there is no deliberate attempt to improve one's memory. The concept of

[3] Simon and Gobet (2000) say that the results from Chase and Simon's (1973) experiment were inconsistent with "the experimenters' reported expectations" (p. 596). The use of the qualifier "reported" is puzzling. Did Chase and Simon have other expectations that they did not report?

[4] Given this quotation, it is natural for one to wonder if Simon and Gobet (2000) have since used MAPP to determine if their new reported expectation is the one generated by their theory. No new simulation results are presented in their commentary, suggesting that their new reported expectation is another incautious prediction.

[5] Ericsson et al. (2000) dispute Vicente and Wang's (1998) interpretation of Coughlin and Patel's (1987) study, so it is important for me to respond to the points that they raise. First, Vicente and Wang's observations about the different results obtained in the two medical cases merely echo the statements originally made by Coughlin and Patel (1987): "In the unstructured form of the endocarditis case, the differences between physicians and medical students on recalling and inferring critical or noncritical information diminished. . . . The text structure did not, however, affect the recall of critical cues by the experts on the temporal arteritis case" (pp. 823–824). In their commentary, Ericsson et al. now present a different interpretation. Second, Ericsson et al. state that "Ericsson and Kintsch (1995) considered only the statistically reliable findings of Coughlin and Patel's (1987) study" (p. 586), do the same in their commentary, and admonish Vicente and Wang for not following suit. This practice (a) reflects a lack of appreciation for the logic of statistical inference because "significance tests cannot separate real findings from chance findings in research studies" (Schmidt & Hunter, 1997, p. 39) and (b) has been an impediment to the development of cumulative psychological findings (Rossi, 1997). Third, Ericsson et al. criticize Vicente and Wang for citing the results of only one of three dependent variables reported by Coughlin and Patel. This variable, exact recall, was chosen because it is the standard measure of performance in this literature. Choosing a second variable, such as number of inferences, would not provide a meaningful basis for comparing the results of Coughlin and Patel with those of other studies. For the same reason, Vicente and Wang also did not discuss the results from a third dependent variable, the sum of exact recall and number of inferences. In any case, this third measure is not meaningful because it lumps together apples and oranges. Fourth, Ericsson et al. state that LTWM theory would not predict that changing the order of symptoms in the arteritis case would have no impact on recall (p. 586, yet Ericsson and Kintsch (1995) originally stated that "experts should be relatively insensitive to the order in which information is presented" (p. 236) and that "the strongest evidence for retrieval structures concerns the ability of experts to independently store pieces of information when they are presented out of their normal context in scrambled order" (p. 238).

> generic retrieval structure seems to offer a theoretically plausible explanation mostly in domains where memory for order is important, where there is a conscious effort to both construct and use a memory structure under strategic control, and where the input is encoded serially. Chess, which offers a bi-dimensional structure where reliance on the order of encoding is important, and which is a domain where memory of positions is not a primordial goal, does not fit this description, nor do many (most?) other domains of expertise. (Gobet, 1998, p. 147)

In sum, despite Simon and Gobet's (2000) and Ericsson et al.'s (2000) arguments, the existing evidence shows that LTWM theory and template theory cannot claim to have identified the psychological mechanisms and representations that are responsible for expertise effects in memory recall across domains. The simulations and data required to support such a claim have not yet been conducted or collected.[6] Therefore, there is no empirical basis for questioning the deficiencies that Vicente and Wang (1998) attributed to LTWM theory and template theory.

Summary: Toward Cumulative, Unified Theory

By specifying psychological mechanisms and representations that may be responsible for expertise effects in memory recall, LTWM theory and template theory each make unique contributions that are not captured by the CAH. These facts were explicitly acknowledged by Vicente and Wang (1998) and so are not in dispute. LTWM theory also makes important contributions to text comprehension and other areas of expertise. Template theory makes important contributions to chess psychology that extend beyond expertise effects in recall. As important as they are, these additional insights are not pertinent to the CAH because they are outside of its theoretical scope: "The constraint attunement hypothesis focuses on explaining recall performance after a very brief presentation of the stimulus material" (Vicente & Wang, 1998, p. 47). Thus, the only remaining controversial question is whether the CAH has a useful and unique contribution to make to the body of literature that it was intended to address.

In this reply, I have provided several independent reasons for answering this question in the affirmative. In the literature on expertise effects in recall, the CAH is novel because it provides a theory of the environment (i.e., the abstraction hierarchy) that can be consistently applied across domains of expertise to develop comparable models of the environment. The abstraction hierarchy provides a basis for scientifically testing Simon's (1990) and Ericsson and Lehmann's (1996) claims of expertise as adaptation to goal-relevant constraints. It also shows that conflicting empirical results across domains of expertise (e.g., process control vs. chess) can be reconciled by representing explicitly the influence of the environment on behavior. Because other theories do not incorporate an explicit theory of the environment, they cannot lay claim to these benefits. Furthermore, a careful comparison of the claims made by LTWM theory and template theory with the limited empirical evidence available shows that, to date, these theories have not fulfilled the burden of proof demanded of process theories. Therefore, although making unique contributions of their own, LTWM theory and template theory cannot explain the data accounted for by the CAH.

Given that the CAH, LTWM theory, and template theory have complementary advantages and disadvantages, I believe, as did Vicente and Wang (1998), that progress in this area of research can be best made by integration of existing theories: "The extension of the constraint attunement hypothesis to a process theory of expertise effects in memory recall should be pursued" (Vicente & Wang, 1998, p. 49). Ericsson et al. (2000) and Simon and Gobet (2000) have not chosen to take up that suggestion. Notwithstanding, I continue to believe that substantially more mature theories will not be developed without integration of existing contributions into a single theoretical perspective (see Figure 1D). The first step to achieving that goal is to acknowledge that all existing theories, one's own included, are inadequate. The CAH is no exception because much work could be done to refine its theoretical content. Only this frank acknowledgment of inadequacy will motivate a search for cumulative, unified scientific knowledge.

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[6] Simon and Gobet (2000) claim that Gobet and Waters's experiment contradicts the CAH, but from the brief description given, it is not possible to evaluate the validity of this claim. For example, it is important to know whether the pieces were sampled with or without replacement and whether goal-relevant constraints associated with appearance were retained or violated (see the description of the board level in the preliminary abstraction hierarchy representation for chess in Table A2 of Vicente & Wang, 1998).

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