Hecht's Response

Universal internalization or pluralistic micro-theories?

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Abstract: In my response I revisit the question whether internalization should be conceived as representation or as instantiation. Shepard's ingenuity lies partly in allowing both interpretations. The down side of this facile generality of internalization is its immunity to falsification. I describe evidence from 3-D apparent motion studies that speak against geodesic paths in cases of underspecified percepts. I further reflect on the applicability of internalization to normal, well-specified perception, on the superiority of Gestalt principles, as well as on the evolutionary and developmental implications of the concept. The commentaries to the target article reveal an astonishing lack of agreement. This not only indicates that a satisfactory unifying theory explaining perception in the face of poorly specified stimuli does not exist. It also suggests that for the time being we have to be pluralistic and should treat internalization as a source of inspiration rather than as an irrefutable theory.

I would like to thank the large number of commentators who responded to my paper for their thought-provoking ideas. I take the surprising scope of the commentaries, which range from enthusiastic support of my challenge of Roger **SHEPARD**'s concept of internalization to its complete dismissal, as an indication that I undertook the right step. The concept has been pushed along on its non-linear path. There seems to be some disagreement as to how to define internalization. The empirical evidence is not the subject of disagreement but rather its implications for SHEPARD's theory. I shall first argue that internalization has to be understood as representation and as instantiation. I will rephrase my falsificationist argument where I think it may not have been sufficiently clear, in particular as far as my method is concerned. My method was to challenge the concept by exhausting possible interpretations of internalization, as well as by playing with the alternate concept of externalization. Rather than addressing individual comments, I have identified a number of topics, each of which comprises several comments.

HR1. Internalization revisited

The responses reveal that different commentators have very different notions of internalization. Compare, for instance, internalization as a metaphor, as Michael **KUBOVY** and William **EPSTEIN** treat it, with the idea of internalization as neural space, as suggested by Shimon **Edelman**, and with the outright rejection of the concept by Andrew **Wilson** and Geoffrey **Bingham**. When I introduced a taxonomy of what could be internalized, basing it on the criterion of resolution, I had assumed some consensus on what constitutes internalization. The variability of the comments indicates that such a consensus has not been reached. This may be so because the concept is more or less a non-statement (see, e.g., Balzer et al. 1987). It does not contain testable substance and it is sufficiently vague to get away with it. Consequently, arguments about the essence of internalization are even more misguided than attempts to find experimental evidence for or against the concept. It matters little how we define internalization as long as we do not define it so narrowly that it immediately becomes implausible. Let us revisit the concept at its definition stage. I believe that two related concepts lie at the heart of our joint attempt to grasp internalization: instantiation and representation.

KUBOVY & EPSTEIN analyze the notion of internalization and conclude: "For the cognitive constructivist, the perceptual system *follows* rules; for the computationalists, the system *instantiates* them" (sect. 1.1, para. 7, emphasis theirs). They continue to juxtapose these two varieties of internalization with the Gibsonian position that, supposedly, is incompatible with internalization. This confines internalization to a lesser concept, more limited in scope than is suggested by **SHEPARD**. However, if the sacrifice of limited scope is not rewarded by an increase in explanatory power, this may be the worst of all cases, and we may indeed find that we can live without the thus confined concept. Instead, I believe that Shepard deliberately used internalization as both representation and instantiation without excluding ecological concerns. By doing so, he has traded diminishing explanatory power for universality. The universality lies in the accommodation of both representation and instantiation as ways of internalization.

HR2. Representation or instantiation?

It is all too obvious that the perceiver does not explicitly represent the knowledge about the physical world when she experiences a percept of smooth apparent motion. Perception happens; it is an automatic process. Accordingly, representationalists do not claim that environmental structure is explicitly represented inside the mind in the way that memories of past events are. Rather, world regularities appear in the mind (or the brain) in some indirect manner. Gerard **O'Brien** and Jon **Opie** draw a similar conclusion when they offer an additional form of representation outside the realm of perception. The fact that the solar system represents (or instantiates) Newton's laws is an example for this variety of representation. Thus, we need a tripartite distinction. The first, instantiation in the world, is not very exciting for our discussion. Presumably all laws of physics are instantiated in all matter to which they apply at all. The second is implicit representation or instantiation as the outcome of a perceptual process, and the third is explicit representation in a cognizant system.

If the visual system uses acquired knowledge about world regularities to disambiguate questionable stimuli, then how could representation differ from "mere" instantiation? I see, as the first of two possibilities, that several representations are available and a choice between them is somehow made, while the system can only be one instantiation. The second possible distinction is that representations can be learned and reprogrammed, whereas instantiations are innate or hard-wired. For instance, if different regularities are obeyed at different times, the makeup of the system (instantiation) can no longer be held solely responsible.

The term instantiation has been used in many different ways in our discussion, but I do not see how it differs from representation other than along the just mentioned dimension of reprogrammability. The computational use of instantiation suggested by **KUBOVY & EPSTEIN** seems to make an unrelated distinction, namely whether or not we like to treat the visual system as an agent, which we often do, not only by postulating a homunculus who looks at the

retinal image and then draws conclusions unbeknownst to the perceiver. For, nobody seems to entertain the view that the visual system explicitly follows rules. Helmholtz's (1894) inferences are unconscious and Rock's (1983) "logic of perception" is implicit. Explicit representation seems to be trivially false. And because it is trivially false we should have no trouble agreeing to the view that internalization must refer to implicit knowledge or knowledge that merely arises by virtue of the hardware. Gibson and his followers are in agreement with this broader sense of instantiation. For example, the notion of perception as a smart device (Runeson 1977) states that the makeup of the visual system lets it behave as if it followed complex rules although it is behaving rather simply.

Consequently, implicit representation and instantiation turn out to be the same. **SHEPARD** wisely uses both in the context of internalization and he equally wisely does not take sides on the issue of reprogrammability. For instance, he states that "genes . . . have internalized . . . pervasive and enduring facts about the world" (target article, Introduction, p. 581). This would suggest that he conceives of internalization as innate instantiation. But he likewise talks about learning of regularities and representation. This openness has the additional advantage of avoiding the unresolved debate of direct perception vs. representation. If we follow SHEPARD's broad definition of internalization we no longer have to decide whether perception and cognition are qualitatively different, as David **Schwartz** suggests in his commentary, or if they are similar. Giving internalization a superior status might have freed it from many old debates. This great advantage is unfortunately linked to the disadvantage of losing ever more definition and explanatory power. The compromise of leaving internalization vague amounts to reducing its content to the statement that the visual system cannot but do what it is doing. How fruitful can this be?

It is interesting to speculate if the constructivist notion of a self-organizing percept, such as **Frank et al.** imply, has to be treated as a third way of internalization. In the context of kinematic geometry they conclude that "there is no need for any internalization of the screw displacement mode itself." They view apparent motion paths as the emergent outcomes of a neural process of self-organization. This merely seems to introduce an intermediate concept. Now the question is no longer "what has been internalized?" but "what constrains the self-organization in such ways that we perceive curves instead of differently shaped paths?" It might be in place here to remind us of the initial problem, which was the question of how the visual system deals with situations that are poorly defined, where the stimulus is degraded, intermittent as in apparent motion, or completely absent as in imagery.

HR3. My falsificationalist view

The statement that a regularity R has been internalized is usually taken to mean that some people exhibit behavior in agreement with R some of the time. In the case of the circadian rhythm the evidence is very strong. Most groups that have been tested in complete isolation adopted a cycle of activity and rest that came very close to 24 hours when they were left to their own devices and sheltered from natural daylight, clocks, communication, and so on (Czeisler et al. 1999). Here a moderate falsificationist (compare Popper 1935) would acknowledge that the hypotheses of an internal clock has withstood attempts to prove it wrong. However, other conceivable examples for internalized regularities do not fare as well. Typically the evidence is very mixed. In apparent motion geodesic paths are not seen in 3D (see below), horizontality is only sometimes applied to the perceived stimulus, and so forth. Should we take only the cases that work as evidence for internalization while we conveniently ignore the abundant counter-examples? If we do so and drop the requirement that the behavior has to be consistent we have indeed found evidence for internalization. However, this reduces the original hypothesis to an existence statement. Internalization exists. We can rest the case as soon as we have found one instance of the hypothesized behavior. I find this unsatisfactory. By choosing examples that range from very broad Bayesian probabilities to very literal regularities, such as the regularity of gravitational acceleration, every single candidate for an internalized regularity lives up to the weak existence statement. Some evidence can be found for almost every single regularity that comes to mind. I believe that we should demand more from a theory. Counter-evidence should be able to speak against the theory. For this to be possible, we need to demand that the internalization hypothesis be phrased in such a way that it keeps some generality.

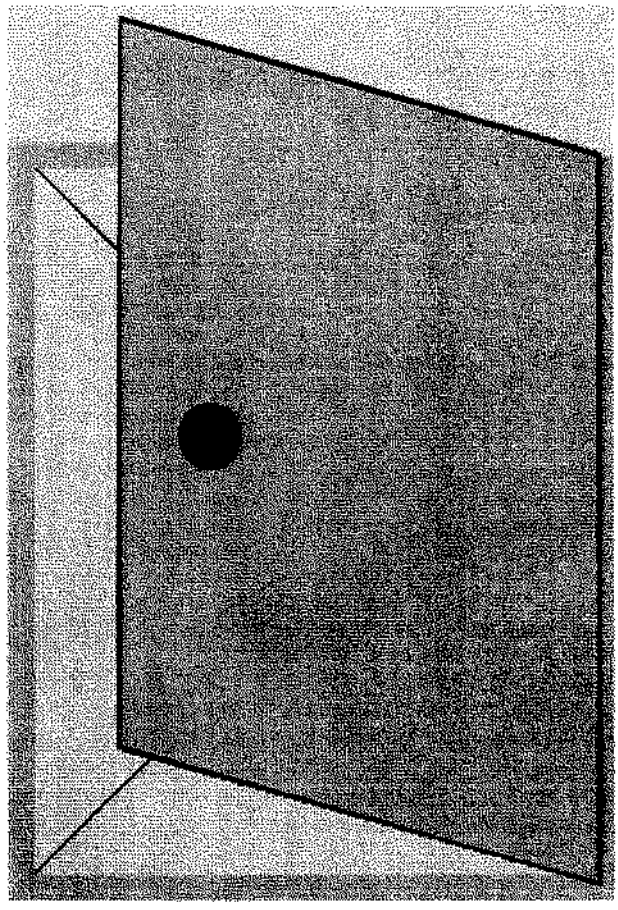

Figure HR1. Do internalized assumptions about object regularity fail to disambiguate the percept?

As long as internalization comprises both learning and instantiation, ontogenetic and phylogenetic knowledge, the concept is immune against all criticism. It may not

even be a metaphor. Does internalization of regularities at least rule out any class of percepts? Can we deduce from it that inherently ambiguous stimuli always get resolved one way or the other? Even this is questionable. While a Necker cube is bi-stable, that is, either one or the other interpretation is seen but never both at the same time, other such figures remain perceptually ambiguous. An interesting example is the ambiguous Figure above (see also http://www.illusionworks.com/). It looks funny. Something seems wrong, but the percept does not flip. Upon closer inspection we notice that the left edge of the dark panel is and is not at the same depth. Some internalized assumption about evenness suggests that the panel is rectangular in 3D while occlusion relations suggest that it is skewed. Here the visual system does not make up its mind. Does this mean nothing has been internalized? No. Does it mean that several things have been internalized? Maybe. The fact is that the unresolved stimulus can also not speak against internalization because the concept is immunized.

HR4. Kinematic geometry

Since **SHEPARD**'s example of perceived apparent motion paths has received a lot of attention, I would like to add a few comments. While we are all in agreement that typically some degree of curvature is perceived when two differently oriented objects are presented in alternation, the distinction between 2D paths and 3D paths seems critical for Shepard's argument. Chasles' theorem clearly predicts circular arcs in 3D. Shepard takes the theorem and does what a good falsificationist should do. He adds specificity to the notion of internalization and operationalizes it for apparent motion in depth as well as in the plane. Hence the testable hypothesis is that unconstrained apparent motion should be perceived along paths that correspond to geodesics. That is, if the stimulus is 2D then a circular arc should be seen. If on the other hand the stimulus is 3D, geodesics that follow helical motions in 3D should be perceived. Hecht & Proffitt (1991) have put exactly this prediction to an empirical test by using a window technique.

Neither **SHEPARD** nor Dejan **TODOROVIČ** seem to have appreciated a fundamental disagreement of our data with the theory. Our Experiment 3 presented displays to observers that showed perspective renditions of dominoes in apparent motion. The stimuli were designed such that all orientation differences could be resolved corresponding to a single rotation in 3D. However, the 3D solution sometimes was considerably steeper and sometimes considerably shallower than the 2D solution that treated the objects as 2D blobs on the screen. Observers first had to adjust a tiltable plane such that it coincided with the perceived plane of the domino's motion. This was done with good accuracy indicating that the domino was indeed perceived to move on the plane in depth that was specified by its orientation. Figure HR2 shows the setup. It also depicts the 2D and the 3D solutions for this particular case, which were of course not visible to the subject. No matter how extreme the orientations, the window probe was practically never set beyond the 2D circle (labeled 100). That is, even though the motion was perceived in depth it fell far short of the circular 3D arc predicted by Chasles's theorem. This can only be interpreted as strong evidence against the internalization of 3D geodesics. The mere fact that the objects follow a curved path is insufficient evidence for the particular operationalization of kinematic geometry.

This piece of evidence against kinematic geometry seems symptomatic for the state of the concept of internalization. In its general format it is a non-statement. It has little or no testable content. The succinct operationalization offered by **SHEPARD** himself is testable and the evidence speaks against it. Now the question is, does it make sense to salvage this particular operationalization by claiming that geodesics are internalized but very sloppily so? This would only be reasonable if we had other examples where the percept does follow the predicted 3D path. Until we find such evidence we should regard it as falsified.

Marco **Bertamini** and **TODOROVIČ** rightfully point out the inadequacy of kinematic geometry as an explanation for how apparent motion paths are perceived, and even as a description of apparent motion paths. Interestingly, both – Bertamini more so than Todorovič – agree that the internalization of laws of physics is fundamentally different from the internalization of rules of geometry. It is not clear to me why they want to make this distinction and thereby go beyond **SHEPARD**'s claim that geometry is more deeply internalized than physics. I take Shepard to merely put forth a generalization argument stating that what is internalized has to be at an abstract level more akin to geometry. Bertamini and Todorovič ascribe two different internalization mechanisms to Shepard. Such a dichotomy, however, appears to be a category error: a geometric principle, once obeyed, is indistinguishable from the internalization of a physical law that predicts the same outcome. Roger **SHEPARD**'s phrase that "geometry is more deeply internalized than physics" (target article, sect. 1.5) is unfortunate because an internalization necessarily has to be some abstraction. Just as a law of physics is an abstraction as soon as it is isolated and used for prediction. Take an object falling from a resting position. Its motion is usually described sufficiently by the rule that it falls straight down. However, this is an abstraction because an indefinite number of other factors influence the object's motion. Thus, any internalization

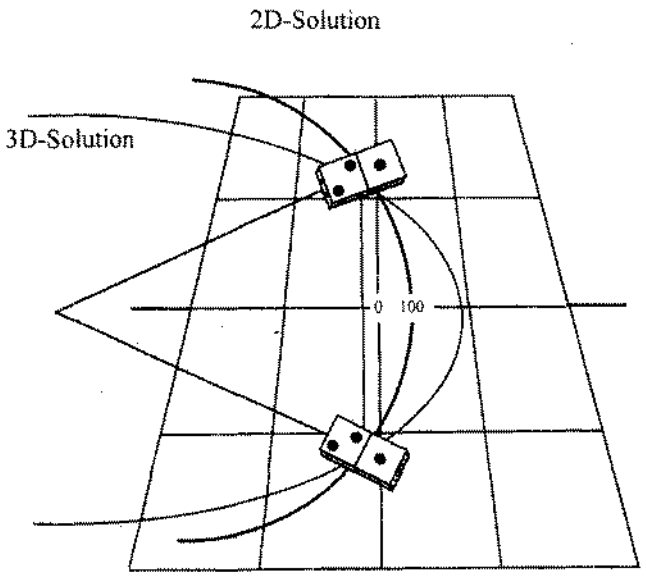

Figure HR2. Oriented objects in apparent motion that should be perceived according to the 3D solution but are not.

necessarily has to extract an idealized or abstracted regularity. Or put differently, the full dynamics of a physical reality could not possibly be internalized. The distinction between physics and geometry can consequently only have a much subtler meaning along the lines that only very abstract rules become internalized.

Thus, stating that physical principles provide a better explanation than kinematic geometry is a result of taking the discrepancy between physics and geometry not for what it is. Internalization is necessarily approximative. Take, for instance, the fact that air resistance often distorts motion trajectories. Suppose that observers prefer squished curves that come closer to what happens with air resistance as opposed to paths in a vacuum. One could claim that effects of air have been internalized, presupposing a complex understanding of that force, or one could claim that squished curves are internalized. The outcome is not qualitatively different. The only thing that changes between these two "internalizations" is the *specificity* of the knowledge about an aspect of the world.

Incidentally, **Bertamini**'s torque explanation of the stimulus presented in his Fig. 1b seems a little far-fetched. The preference of the bottom path could just as well be attributed to differences in air resistance. We would then not need assumptions about a uniform mass distribution of the rectangular objects as are required for the moment of inertia (torque) explanation. This demonstrates that **SHEPARD**'s endeavor was justified. If the visual system has limited resources it should internalize a few general commonalities rather than a large number of specific rules.

David **Foster**'s remark that apparent motion (AM) may be an "internalization not of the ways in which objects move freely in space but of the ways in which observers manipulate or interact with them," basically takes my suggestion of externalization and applies it to the realm of apparent motion. What else could the internalization of ways to manipulate objects be? Since these ways are certainly not located in a world external to the observer, it seems awkward at best to call it internalization. Thus, we seem to differ merely at the level of semantics. I hold that we do not need a representation of the constraints on our effectors. Rather, these constraints seem to affect our representation of the world. But semantic issues aside, I have serious doubts whether this idea is as testable as Foster wishes it to be. Our preferred ways of manipulating objects may not be independent of the way we see them move.

This commonality of perception and action has been discussed in the domain of motor learning. The theoretical framework of common coding (Prinz 1992; 1997) posits that the final stages of perception and the initial stages of action control share a domain of coding where planned actions are represented in the same format as are perceived events. One of the implications of this approach is that, under appropriate conditions, perceived environmental events can induce certain actions by way of similarity or feature overlap. If perception and action share the same codes, it must also be expected that changes in these codes that are due to motor learning are reflected in corresponding changes in perceptual skills. Hence, the principles governing our imagery and unsupported perceptions may be just as much influenced by our motor constraints as by external events. The makeup of our effectors constrains what we are able to perceive and learn just as much as external regularities do. Our body dynamics also set the conditions for what principles may be applied in situations of ambiguity. Moreover, shared representations would necessarily have to be in approximate agreement with external and internal events.

HR5. Evolution

The comments by Ken **Cheng** and Adolf **Heschl** shed an interesting light on my attempt to find regularities at very abstract as well as at very concrete levels. The example of birds extracting the position of the north star shows that very specific knowledge can be exploited. This contradicts the above mentioned weak claim that only abstract principles are internalized. Thus, at least from an evolutionary point of view specific regularities, such as water remaining invariably horizontal, are not too fine-grained to be in principle internalized. Heschl's argument that every existent perceptual-cognitive mechanism must have originated by an evolutionary process of adaptive internalization (i.e., eliminating negative and preferring positive outcomes) supports this. It also underscores my suspicion that internalization is a non-statement. Given that the theory of evolution is accepted, positing internalization does not add anything.

John **Pickering** shows that calling the process of internalization "evolutionary" does not mean stable instantiation. Lamarckian change even at short time scales suggests that some constraints that we find today may have been acquired very recently. This negates any possible differences between representation and instantiation, and it shows that ad hoc internalizations are compatible with evolutionary principles. His suggestion that humans shape their own environment and thereby create new regularities appears even more radical than my suggestion of externalization. It certainly deserves to be taken seriously. I would not go as far as Antonio **Raffone et al.** who think that internalization is not a general bio-cognitive principle because of ever-present context and niche dependencies, but it is remarkable that the commentaries that address **SHEPARD**'s claim of an evolutionary universality of internalization find it unconvincing.

HR6. Gestalt theory

Walter **Gerbino** rightfully points out the proximity of internalization and the Gestalt principle of Prägnanz. He seems to think that both can be tested empirically. This might be possible for Gestalt principles because a whole number of them have been suggested. It is of course possible to test which principle wins and determines the percept when two or more are brought into conflict. A Gestalt principle by itself cannot be tested. Take the principle of proximity, which states that parts that are closest to one another get grouped into one perceptual object. While this principle seems to work for the three dots on the left in Figure HR3, it does not work for the same three dots hidden amongst the other dots on the right. The principle by itself has the status of an existence statement: there exist cases where near objects group together. Like internalization, it cannot be disproved. However, while internalization is a stand-alone principle, proximity is part of a "theory" that consists of a whole number of principles. They can thus be tested with regard to their relative strengths. In this context statements can be made and tested, such as the statement that "given everything else is

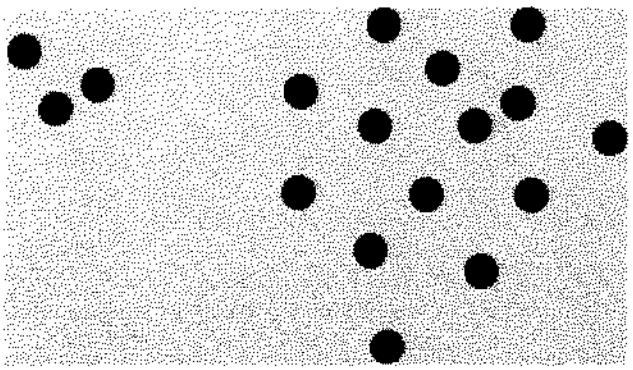

Figure HR3. Grouping by proximity does not always work

the same, proximity is on average a stronger predictor for grouping than is identity of shape." Consequently, as problematic as Gestalt theory is for a falsificationist, testable hypotheses can be derived from it. By going to just one principle, internalization, this opportunity has been forsaken.

HR7. What regularities are internalizable?

Helen **Intraub** suggests a new candidate for an internalized regularity, namely spatial continuity. She tests this candidate in a rather ecological fashion. This could be an important piece in the mosaic of evidence testing internalization. Spatial continuity, as operationalized by boundary extension tasks, can be found in pictures and in real world scenes. Observers typically remember a larger field of view than they actually saw. The suggestion that this memory distortion reflects an internalized regularity is interesting because it is not obvious whether spatial continuity qualifies for internalization. The reason for this doubt is rooted in the usual requirement for internalization that the regularity in question acts in the world independent of the observer. Something can only be said to be internalized if it is not already a part of the beholder. This certainly holds for gravity. However, our visual boundary, as Ernst Mach (1886/1922) so aptly showed, is a condition of perception, just as is our sensitivity range to light of certain wave lengths. The statement that we have internalized light to become visible only when its wavelengths are between 300 and 800 nm, is nonsensical.

David **Jacobs**, Sverker **Runeson**, **and** Isabell **Andersson** make a similar argument when they urge us to differentiate between the constraints under which vision operates, and internalization. Hence, we can distinguish three categories of regularities that constrain what we perceive: (1) external regularities that cannot be internalized, for instance, the physical characteristics of light. Statements to the effect that we have internalized the fact that light enters the eye, or that light travels in straight lines, are nonsensical; (2) external regularities that can be internalized (Shepardian regularities); and (3) perceptual regularities that fall out of our sensory makeup (e.g., the recency effect in memory, priming, the fact that light of 400 nm typically appears as blue). It will not be helpful to call categories 1 and 3 "internalized." That is, a Shepardian regularity (category 2) has to be internalizable and it has to contribute to solving the underspecification problem. As interesting as **Intraub**'s evidence for boundary extension is in its own right, I am afraid it falls into category 3 and does not qualify as a Shepardian regularity.

The issue whether perception necessarily merely relies on environmental regularities, as **Jacobs et al.** like to think or whether some regularities have been internalized relates back to my initial distinction between instantiation and reprogrammable representation. Jacobs et al. obviously dislike the latter and find ecological psychology to be inconsistent with the notion of representation.

HR8. The developmental perspective

Horst **Krist** and Bruce **Hood** separately recommend that we consider regularities that are being acquired during infancy and childhood. In those cases where such regularities emerge in the process of maturation, this seems reasonable. However, both Krist and Hood call for broadening the concept of internalization to include all sorts of representations acquired during childhood. Although such an empiricist interpretation of internalization is certainly possible, it makes the concept indistinguishable from learning and even less accessible to empirical testing. In my opinion, we should do everything but "loosen the criterion," as **Hood** suggests, because the concept is already hard to come by. The looser the criterion the less useful the theory. The nice developmental example of a straight-down belief that he shows supports internalization. But what do we do with a concept that has both supporting and refuting evidence? What we need is a tighter criterion. Or we admit that we are dealing with a non-statement that may be useful to organize our thinking but that ultimately evades empirical testing. In the latter case we should stop designing experiments that have the goal to test the concept of internalization.

Krist's suggestion to define internalized regularities in a task-dependent manner seems directly in contradiction to his suggestion of broadening the definition. Task-dependence takes some abstraction away from the concept, and narrows it down to a specific realm of application. This appears very similar to my attempts at operationalizing the concept, albeit with the additional need to find independent predictors that specify to which tasks it should and should not apply. If we cannot find those predictors and instead have to enumerate the tasks for which internalization holds, the concept loses its appeal dramatically.

Krist is concerned that my differential treatment of perception and prediction has led me to interpret everything as evidence against internalization. This is a peculiar argument and I think it is founded in a difference of opinion on what constitutes the terrain for internalized constraints. I regard the application of internalization to well-specified visual events as nonsensical. We do not need internalization to explain the percept of a straight path when in fact a computer animation of straight path was presented to the observer. In general, correct performance in a rich sensory environment is not a test case for internalization. Internalization lends itself to explain judgments that are without sufficient stimulus support. In the paper-and-pencil version of the C-shaped tube problem, where no such support is provided, the error of predicting curved paths is compatible with kinematic geometry. However, there is also an indirect way to assess internalization by the use of relative comparisons of sufficiently specified events: the comparison of "relative naturalness" judgments of different, but

equally well-specified, events. If such events are judged as natural or anomalous, this judgment has to be based on some representation of how things should behave. Internalized constraints are candidates here. Thus, the incorrect predictions made on the C-shaped tube task speak for internalization of curved paths, while the correct judgments of visually animated versions of the task indicate that knowledge of the correct straight paths has been "internalized." Evidence for and against internalization of kinematic geometry can be found using the same naïve physics task. I believe that the contradiction between predicted outcomes and perceptual judgments is worrisome and that it is incompatible with the notion of universal internalized regularities. Would it help to make internalization task-dependent and claim that curved paths have only been internalized when the nature of the task is cognitive rather than perceptual? I do not think so, unless we want to generally limit internalization to cognitive tasks.

Krist is a radical internalizationist when he equates the external regularity with a perceptual regularity. Indeed, because we have eyes, the retinal image size relates to the size and distance of an external object. This relationship, however, is not a Shepardian regularity. It certainly is not external. That's why I only mentioned it in passing. It is likewise meaningless to talk about our ability to make time-to-contact judgments of approaching objects (*tau*, e.g., when catching a ball) as if it were internalized. Regularities can be internalized, faculties cannot. This relates to the issue of the interaction between the perceiver and her environment addressed below.

Does a developmental approach to internalization make sense? We could operationalize and say that the earlier an infant witnesses knowledge about a given regularity, the more deeply it must be internalized. The nice evidence by Spelke et al. (1992) supports such an endeavor. Based on their findings we would conclude that continuity and object solidity are more deeply internalized than gravity and inertia. But by saying this, have we really said anything other than "knowledge about continuity and solidity develops earlier than knowledge about inertia and gravity"? We always end up with the same problem: unless strictly operationalized, internalization remains a non-statement. We do not add anything to what we already know by using the concept. Also, the more fundamental the regularity, the less appropriate the notion of internalization. The fact that infants stop in front of a visual cliff can prompt us to state that depth has been internalized. But again, depth is not external to the observer. In other words, Gibson's (1979) affordances, such as potential injury, solidity, and so on, appear different from what constitutes an internalization. First, there is no ambiguity to the situation. The visual system has no need to resort to a generalized default. Second, neither depth nor solidity are universal. There are shallow and gaseous objects. Some creatures can walk through rain, others cannot. Failing to recognize a solid object as such does not necessarily mean that the concept of solidity has not been internalized. The infant may have "chosen" the wrong internalized regularity, as I have demonstrated for the case of horizontality.

Let us come back to **Krist**'s suggestion about task-dependent constraints. Adults and infants expect things to fall down in some cases (see also **Hood**'s commentary). We therefore have internalized the regularity of falling or gravity in those tasks where it applies. In a similar vein, we can find some evidence for the internalization of every single regularity that comes to mind. However, such a conceptualization is not very useful unless we could claim that gravity has been internalized across the board. I have provided counterexamples. The existence of examples and counter examples for every regularity that has been proposed should set off an alarm. It certainly sets off my falsificationist alarm. Demanding a task-dependent use of internalization and focusing on supporting examples does not do a service to **SHEPARD**'s theory, rather, it immunizes it against criticism. It is perfectly fine to immunize a concept, as long as we admit to it. Once immunized, we no longer need to waste our time on empirical tests. We now have to judge whether the concept is useful in guiding our thinking or whether it is time to think of more fruitful concepts.

HR9. Does internalization operate in normal perception?

SHEPARD claims that internalized constraints, as observable in imagery or in cases of stimulus paucity, represent universals that are also at work in normal vision. In fact, this is why we went through all the trouble of investigating apparent motion trajectories. Now, if we generously interpret the entire data on the topic as inconclusive, what does this entail for the underlying argument that the internalized constraints also work in normal vision? I contend that even conclusive evidence for internalization under stimulus paucity – if there were such evidence – would not allow strong conclusions about normal vision because of its fundamental difference. In normal vision we recall (or directly perceive) some surplus meaning that is important to us when the object is unambiguously perceived and the stimulus is sufficient for its identification. For instance, the knowledge that cars are made of steel becomes effective when the car is in front of our eyes. On the other hand, the fact that objects move in circular paths becomes effective when the path is *not* in front of our eyes. It could well be the case that the system is only resorting to internalized knowledge when at a loss, similar to inferences we draw about what must have happened when we see a crumpled object. Observers are remarkably good at inferring the causal history of stationary objects based on their shape (Leyton 1989). **SHEPARD**'s hypothesis that unconstrained perception or imagery reveals the workings of normal perception is certainly intriguing but it is not supported by empirical evidence.

HR10. A pragmatist view of internalization

Peter **Todd** and Gerd **Gigerenzer** implicitly question the theoretical content of internalization by calling it a metaphor. The choice of the term "metaphor" is questionable but the underlying pragmatism deserves consideration. They go on to compare internalization to other "metaphors," in particular to Simon's concept of bounded rationality, which posits a loose relationship between environment and perceiver, namely that of "scissors," where one blade stands for the task environment and the other for the computational abilities of the decision-maker (Simon 1990). **Todd & Gigerenzer** opt for the pragmatic choice based on pure convenience, just like picking up a convenient tool from a toolbox. Since every metaphor conveys a little truth we

should use what works best (mirror for perception, scissors for cognition). This pragmatic approach has the advantage that it is indifferent to the theoretical content of any given position and thus need not worry about finding one theory that explains the data satisfactorily. However, the position appears almost fatalistic as to the goal of science. It seems to imply that an approximation to the true state of affairs is futile. While a pragmatist position has many virtues when it comes to personal belief, it may not be very conducive to empirical science. This is as different from my admittedly unfashionable falsificationist position as can be. It can only be surpassed in pessimism by Feyerabend's (1975) anarchistic theory of science, which claims that any theory goes, regardless of evidence or practicability.

HR11. Externalization

By suggesting externalization I only intended to sketch an alternate theory to show that it might be just as plausible as the concept of internalization even if it runs into the same problems. With this move I wanted to criticize the directionality of SHEPARD's hypothesis. Like the now outdated intromission ideas about vision, the impression of external laws into our visual system appears lopsided. We need to consider the other side, our need for motor action, and ask how it impinges on our percepts. Rather than thinking in terms of intake, it might be beneficial to merely look for commonalities between the environment and our perception. Our body is just as much an environment as are the objects outside. In the evolution of a perceptual or cognitive system, both world and self are players. We may even need the additional mental player suggested by Rainer **Mausfeld**. For now, however, I gladly share the physicalistic trap. Adding aspects of the perceiver to the physicalist account may be sufficient to explain regularities that are not internalizable, such as being edible.

In reply to Margaret **Wilson**, my argument that a hypotheses must apply to a potentially indefinite set of phenomena holds for internalization more so than for Gestalt laws. The latter form an ensemble and therefore do not come with the same claim of universality. While it is just an annoyance that each individual Gestalt law is as hard to falsify as it is persuasive, internalization lacks the benefit of being part of a larger whole. Thus, watered-down versions of internalization, such as statements that some regularities have been internalized and influence perception only some of the time, are not able to benefit from similar synergy. We simply end up with a non-statement. Such a non-statement is persuasive exactly because it could not possibly be wrong. Its truth is inversely proportional to its explanatory value. Exactly the same holds – of course – for the principle of externalization. I indeed conjured up externalization as a thought-provoking challenge, not as a full fledged counter theory, as Mary **Kaiser** aptly points out. It is only true in the watered-down version that some physical body constraints are reflected in our perception some of the time. In this context, the concept of an emulator that is mapping body movements onto perceptual objects, as proposed by **Wilson**, is very intriguing. It might indeed offer a venue to elaborate on the externalization idea and to turn it into something more substantial than a challenge to internalization.

HR12. Dissolving the concept of internalization

Douglas **Vickers** and Martin **Kurthen** both suggest dissolving internalism. Their radical suggestions deserve to be explored further and could eventually lead to an alternative concept that is much superior to SHEPARD's notion of internalization. **Vickers'** suggestion of a "concrete instantiation of . . . most general principles" is certainly compatible with the demand for falsifiability. General principles are easy to falsify and therefore highly desirable. As far as I understand his generative transformation, it does not need to make claims about internalization but is content with identifying general rules of perception, which might even include the body constraints I had in mind. It might indeed be better to formulate visual abilities in terms of rules for prediction and thus circumnavigate the issue of internalization vs. externalization. Thereby, the concept of internalization is dissolved or overcome.

Martin **Kurthen** on the other hand, wants to overcome internalization by inverting the explanatory process. Rather than using internalization (as representation) to explain cognition, he suggests that cognition has to explain representation. Although this radical approach sounds very intriguing, it might not work in cases where the explanans is a general principle and the explanandum a very specific behavior. For instance, when internalization of curves is used to explain a very specific apparent motion percept, a general principle is used to explain a particular fact. It is hard to imagine how the particular fact could be used, in turn, to explain the general principle. If we say the fact embodies the principle, the direction of explanation from the general to the specific is maintained.

A different attempt to dissolve the concept of internalization might be even more radical although it has been in co-existence with cognitive theories of perception for a long time. It is the direct realism put forth by **Wilson & Bingham**. They deny that SHEPARD's model is about human perception because dynamics can be directly perceived and representation is not needed altogether.

HR13. Conclusion

In summary, the beauty and the frustration of the concept of internalization lie in its lack of content. To postulate internalized constraints as a structural form of mental representations amounts to a non-statement, albeit an elegant and fascinating one. Without further specification, the idea is appealing because it is true by definition. And much of the argument is about definitions. The lack of precision has the great advantage that the internalizationist does not necessarily have to make up her/his mind and side with or against representationalism, ecological realism, constructivism, and so on. This advantage also frustrates the critic. The critic does not get anywhere if she argues against internalization from a particular theoretical stance. Evidence against particular representations does not put a dent in the concept of internalization, and evidence against the general concept is impossible. The commentaries have shown that the concept of internalization continues to fascinate many, while others think it might have outlived its usefulness. This lack of agreement indicates that a satisfactory unifying theory does not exist. Therefore, we have to be pluralistic and content ourselves with narrower theories that are – desirably – falsifiable.