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A Response to Graf and Komatsu's Critique of the Process Dissociation Procedure: When is Caution Necessary?

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In a recent paper, Graf and Komatsu (1994) argued that the process dissociation procedure (Jacoby, 1991) is limited in its ability to separate and measure conscious and unconscious forms of memory and so should be "handled with caution". Given that the study of unconscious influences has always posed a difficult problem for memory researchers, we agree with the general emphasis on caution. In this paper, we too advocate caution, especially as it applies to the use of indirect tests, assessing Graf and Komatsu's critique, and using the process dissociation procedure. We address the substantive issues raised by Graf and Komatsu and also point out the errors, both factual and logical, in their paper. Any method proposing to provide separate measures of conscious and unconscious influences requires judicious use and a careful examination of its underlying assumptions. The assumptions underlying the process dissociation framework are supported by a large number of experiments spanning a diverse range of

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conditions. In contrast, the assumptions underlying implicit/explicit test comparisons, when articulated, are found to be flawed and no solutions seem forthcoming. The process dissociation procedure offers researchers the most promising approach for disentangling conscious and unconscious influences.

INTRODUCTION

The process dissociation procedure (PDP) was introduced by Jacoby (1991) as a technique for separating consciously controlled uses of memory from more automatic or unconscious uses as they operate within a single task. In a recent paper, Graf and Komatsu (1994) criticised the PDP on what they referred to as *a priori*, practical and theoretical grounds. Graf and Komatsu's main arguments were as follows. First, they declared that the processes measured by the PDP ("conscious versus automatic processes") are distinct from those thought to underlie explicit and implicit memory ("intentional versus non-intentional remembering"). Second, they claimed that the instructions used with the procedure are too complex to be understood by certain populations. Third, they noted that certain assumptions underlying the PDP may not always hold. Finally, they introduced "a more general model for quantifying consciously controlled versus automatic processes" and "demonstrated" that the model advocated by Jacoby and colleagues is a special case of their own. In this paper, we question the basis and relevance of these arguments.

In broad outline, our response is as follows. First, we agree with Graf and Komatsu that the relationship between awareness, intentionality and memory is complex. The question is, how do we best go about studying that relationship? Graf and Komatsu believe that the distinction between implicit and explicit memory, and the task dissociation paradigm on which it is based, provides a sound framework for addressing the issue. In contrast, we believe that direct (explicit) and indirect (implicit) test instructions provide only a weak manipulation of awareness and intentionality. At best, this manipulation can suggest the presence of dissociable processes. At worst, by failing to completely control awareness, intentionality and response biases, it leads to mistaken conclusions about the processes underlying performance. It is for this reason that the PDP was developed. Second, Graf and Komatsu are correct in claiming that some implementations of the PDP are more complex than indirect test procedures; we believe that the level of complexity is appropriate to the theoretical issues being addressed. However, they misrepresent the approach by looking only at recognition memory and by implying that complex instructions are integral to the procedure; as documented below, they are incorrect. Third, Graf and Komatsu are justified in reiterating our previous claims that the assumptions underlying the PDP may not always hold (see Jacoby, 1991; Jacoby & Begg, submitted; Jacoby, Toth, & Yonelinas, 1993b). However, the conclusions they draw from such potential violations are extreme given the extensive evidence that the assumptions can often be met (see discussion and references below). Designing situations in which the assumptions are violated (e.g. Komatsu, Graf, & Uttl, 1995) may be noteworthy, but only in so far as they inform researchers what *not* to do in order to use the procedure. Finally, Graf and Komatsu's "more general model" is based on a number of assumptions from different models of memory that, while plausible in isolation, make few testable predictions in combination. Even if their model were tenable, it would still be only speculative because it provides no means for estimating the numerous parameters that are postulated.

From our perspective, it is the first issue—the relative merits of direct/indirect test comparisons verus the PDP—that defines this debate. Our goal in this paper is to make the substantive issues in the debate clear so that researchers can decide which strategy is more appropriate for assessing and understanding memory. The issue is not task versus process dissociations; at least from our perspective, it is clear that process dissociations are a more significant and ambitious goal. Rather, the two main issues are whether, despite their flaws, direct and indirect tests can tell us something that the PDP cannot, and whether the PDP has insurmountable practical limitations and/or untenable assumptions as claimed by Graf and Komatsu. We elaborate on these points below.

WHEN IS CAUTION NECESSARY? WHEN USING INDIRECT TESTS

The most commonly used indirect tests (e.g. stem completion, perceptual identification) are facilitation paradigms; in principle, both conscious and unconscious uses of memory can increase performance on these tests (i.e. lead to the production of studied items). Nothing stops a subject form being aware of, or intentionally remembering, a previously studied word and responding with it on an indirect test. Indirect tests have no way of measuring or preventing this from occurring. Thus, indirect tests, while providing a better measure of unconscious influences than direct tests, have two serious problems. First, they give the experimenter little or no control over the strategies subjects use to perform the task; that is, they constitute a weak manipulation of awareness and inentionality. Second, they are predicated on an assumption of process-purity that is widely acknowledged to be incorrect (Jacoby, 1991; Reingold & Merikle, 1988; Reingold & Toth, in press; Richardson-Klavehn & Bjork, 1988; Schacter, 1987; Toth, Lindsay, & Jacoby, 1992).

Most advocates of the task dissociation (implicit/explicit) approach do not deny these criticisms (e.g. Roediger & McDermott, 1993; Schacter, Bowers, & Booker, 1989). But let us assume that indirect tests could be treated as process-pure measures of unintentional retrieval. Would this make the PDP unnecessary? We think not for reasons that help to emphasise the difference between the two approaches. The PDP is based on the contention that the joint contributions of conscious and unconscious influences to task performance is the rule rather than the exception. It argues that manifestations of process purity are the true exceptions because they may only obtain under extreme conditions such as brief stimulus exposure, divided attention, or following unusually selective brain damage. In contrast, the task dissociation approach assumes that process-purity is the rule, that intentional and unintentional uses of memory most naturally occur in isolation. According to the approach, one can study implicit memory without being concerned about explicit memory or about the relationship between the two.

The difference between the task and process dissociation approaches is exemplified in a thought experiment originally described by Jacoby and Kelley (1992). Consider the folk psychology that recommends getting a person drunk so as to reveal their true opinion about some topic. Analogous to an implicit test, drunkenness is being advanced as allowing a pure measure of opinion or belief. One problem with this strategy is that it does not specify how intoxicated a person has to be before their responding is no longer contaminated by consciously controlled influences. Let us say that we solve that problem by finding that the person must drink *x* amount of alcohol before their responding does not reflect consciously controlled influences. Having solved the problem of process-purity, however, another problem arises: How can we be sure that *x* amount of alcohol affects only consciously controlled processes and not also what the person believes when sober? That is, what one believes when intoxicated might differ significantly from what one believes when sober. Thus, even assuming that one has a process-pure test of true belief (or unconscious influences), you would also need some means of measuring that process under less extreme conditions; that is, in the presence of consciously controlled influences.

The above example points up one of the main drawbacks of indirect tests —namely, their inability to measure unconscious influences when consciously controlled influences are also in play. Without a procedure for measuring both, one can never be sure that only the process of interest is contributing to performance; or that this process is not changed by the procedures used to eliminate other potential influences. The PDP was specifically designed to measure controlled and automatic influences when both are operating under a single set of conditions. By doing so, it allows one to determine whether an experimental manipulation (e.g. intoxication, divided attention, speeded responding) selectively influences one process or whether the other process is affected as well.

The PDP is illustrated in an experiment by Jacoby et al. (1993b), who used the procedure to investigate the effects of divided attention on recollection and automatic influences of memory in cued recall. Words were presented for study under conditions of full or divided attention, and then later tested with word stems (e.g. motel, mot__). For an inclusion test, subjects were instructed to use the stem as a cue for recall of an old word or, if they could not do so, to complete the stem with the first word that came to mind. An inclusion test corresponds to a standard test of cued recall with instructions to guess when recollection fails. The subjects could complete a stem with an old word either because they recollected the old word, with a probablity R, or because, even though recollection failed (1-R), the old word came automatically to mind (A) as a completion: R+A(1-R). For an exclusion test, the subjects were instructed to use the stem as a cue for recall of an old word but *not* to use a recalled word as a completion for the stem. That is, the subjects were told to exclude old words and complete stems only with new words. In this condition, a stem would be completed with an old word only if recollection failed and the word came automatically to mind: A(1-R). Thus, the difference between the inclusion (trying to use old words) and exclusion (trying not to use old words) tests provide a measure of the probability of recollection. Given that estimate, and the assumption that recollection and automatic influences make independent contributions to performance, one can compute the probability of an old word automatically coming to mind as a completion [e.g. A=Exclusion/(1-R)]. The results showed that divided, as compared to full, attention during study significantly decreased the probability of later recollection (0.00 *vs* 0.25) but had no effect on unconscious influences (0.46 *vs* 0.47).

An important aspect of the experiment above is that conscious control was completely eliminated after divided attention (R=0), thereby allowing test performance in that condition to function as a process-pure measure of automatic influences. Moreover, the automatic influences measured in that process-pure case almost perfectly predicted the automatic influences in the full attention condition where consciously controlled uses of memory were also contributing to (or "contaminating") performance. Without the PDP, one could not know that, in this situation, divided attention allowed a process-pure test of automatic influences or that automaticity was the same across different levels of cognitive control. Neither of these conclusions could have been reached by contrasting direct and indirect tests. Perhaps alcohol has effects similar to those of divided attention; after a certain number of drinks, conscious control is reduced to zero while automatic influences (or beliefs) remain unchanged. Alternatively, intoxication may affect both controlled and automatic influences. Direct/indirect test comparisons provide no way of deciding between these alternatives. The PDP, in contrast, provides a principled strategy for investigating this, and similar, issues.

WHEN IS CAUTION NECESSARY? WHEN READING GRAF AND KOMATSU'S CRITIQUE

Graf and Komatsu argue that the PDP (a) does not measure the processes assessed by implicit and explicit tests, (b) is a complex procedure and (c) is based on highly questionable assumptions. Let us look at the evidence.

A priori Considerations

Graf and Komatsu worry that the PDP may not adequately measure implicit memory because subjects are directed to recollect a specific past event whereas implicit memory is measured in the absence of intention to remember. In fact, there is a good quantitative match between estimates of automaticity gained by the PDP and indirect test performance when that performance is "uncontaminated" by intentional uses of memory. For example, Toth, Reingold and Jacoby (1994) found that estimates of unconscious influences were nearly identical to the probability of completing stems on an indirect test following shallow encoding—a condition for which conscious recollection is likely to be low. Similarly, Jacoby, Yonelinas and Jennings (1994b) used a divided attention manipulation to reduce conscious uses of memory and found that estimates of unconscious influences matched the level of performance on an implicit fragment-completion test. These results suggest that uncontaminated implicit tests and estimates of unconscious influences derived from the PDP are measuring the same construct (see Jacoby, Toth, Yonelinas, & Debner, 1994a, for further discussion).

Of course, implicit memory performance will not always match estimates of automatic influences gained from the PDP. One reason for this discrepancy is the conscious contamination of implicit tests (Jacoby et al., 1993b; Toth et al., 1994). Another reason, noted above, is that conditions designed to eliminate conscious control can also affect unconscious influences (e.g. Debner & Jacoby, 1994). Finally, there is evidence that automatic processes operating in the context of intentional use of memory may sometimes be qualitatively different from those operating in isolation (Jacoby, Ste-Marie, & Toth, 1993a; Toth & Reingold, in press; Wegner, 1994). All of these "discrepancies" are interesting and have important implications for how we conceptualise intentional and unintentional processes, as well as the relationship between the two. None, however, could be addressed using direct versus indirect test comparisons, because such comparisons provide no means of investigating the two processes when both are influencing performance.

Graf and Komatsu also question the validity of the PDP "because it does not distinguish between remembering that is initiated and guided by a conscious intention versus remembering that is accompanied by [conscious recollection]" (p. 116). The latter is referred to as involuntary explicit memory (Schacter, 1987; Schacter et al., 1989) and has become one of the main concerns of those hesitant to accept the assumptions underlying the PDP (e.g. Richardson-Klavehn, Gardiner, & Java, 1994; Roediger & McDermott, 1993). The potential problem this poses for the PDP involves performance in the exclusion condition where subjects are instructed to avoid responding with previously presented words. Whereas we have taken correct exclusion to indicate intentional recollection of words' prior presentation, Graf and Komatsu are worried that exclusion could also be accomplished by automatic retrieval that is *followed by* conscious awareness. This *may* be an important concern but it is important to note that since its introduction, the concept of involuntary explicit memory has received little theoretical or empirical treatment. Thus, whether it is a problem for the PDP or any other approach to the measurement of memory is an open question.

In terms of the PDP, the hypothetical impact of involuntary explicit memory would be to decrease errors on the exclusion task. This would produce an overestimation of conscious recollection and, as a result, an underestimation of automatic influences (see Jacoby et al., 1994b). In agreement with other researchers (e.g. Atkinson & Juola, 1973; Hasher & Zacks, 1979; Mandler, 1980; Shiffrin & Schneider, 1977), we assume that conscious recollection—and therefore involuntary explicit memory—is generally slower and requires more capacity than more automatic uses of memory. Thus, if involuntary explicit memory distorts results from the PDP—inflating estimated control and artificially lowering estimates of automaticity—we should find such distortion to change over conditions that affect attentional capacity. Yet in experiments using the PDP to study recognition memory, both Yonelinas and Jacoby (in press) and Toth (unpublished data) have shown that a manipulation of retrieval time that had a large impact on estimates of conscious recollection had no influence on estimates of automatic influences. Similarly, divided, as compared to full, attention to study words can completely eliminate conscious recollection in a stem-completion task, yet leave automatic influences unchanged (Jacoby et al., 1993b). Finally, using a modified exclusion procedure specifically designed to reveal involuntary explicit memory, Richardson-Klavehn, Gardiner & Java (in press) failed to find evidence for it. These results suggest that involuntary explicit memory, if not a red herring, is at least not as widespread as claimed by Graf and Komatsu, and may often reflect the phenomenology of remembering rather than the underlying dynamics of memory retrieval (see Reingold & Toth, in press; Toth et al., 1994).

In summary, Graf and Komatsu's *a priori* arguments would appear to have little theoretical basis and no empirical support. Moreover, if there is an important difference between memory initiated by awareness and memory accompanied by awareness, we are hard pressed to see how it could be investigated using the process-pure strategy advocated by Graf and Komatsu.

Practical Considerations

Graf and Komatsu state that "the process dissociaton procedure is considerably more complex than other commonly used methods, and this complexity limits its domain of application" (p. 117). As noted above, we believe the complexity of the process dissociation framework is appropriate to the issues being addressed. However, we disagree with the claim that exclusion instructions are overly complex. While certainly more detailed than those used with indirect tests, exclusion instructions ask the subject to engage in a common everyday activity; that is, the use of intention to override automatic, habitual or otherwise unintended actions (Bowers, 1975; 1984; Reason, 1979; 1984). It is the specificity of exclusion instructions that allows one to interpret exclusion errors as representing true unintentional influences. In contrast, responses on an indirect test, by not being opposed by intention, cannot unequivocally be identified as reflecting unconscious or unintentional influences.

As an example of overly complex instructions, Graf and Komatsu quote from Jacoby (1991). However, the quote they chose did not represent verbatim instructions but was merely a condensed description taken from the introduction to the experiment; in practice, subjects are always given clear, detailed instructions which they are asked to repeat and ask questions about. As further "evidence" that PDP instructions are difficult to understand, Graf and Komatsu claim that 23% of the older subjects in a recent study by Jennings and Jacoby (1993) had to be rejected for "failure to follow instructions" (p. 117). This is a misrepresentation of the facts. In an experiment combining the PDP with the false-fame paradigm (Jacoby, Woloshyn, & Kelley, 1989), Jennings and Jacoby (1993) rejected 7 of 31 older subjects because of their "inability to show any discrimination between old and new test names, or to discriminate between famous and nonfamous names" (p. 286).

Graf and Komatsu suggest that difficulty with instructions limits the applicability of the PDP. On the basis of little evidence, they claim that children or those with brain injury cannot understand the exclusion instructions. However, this is an empirical question for which we have obtained concrete data. In the past year, we have been measuring inclusion and exclusion at various study/test intervals ("lags"). This procedure builds in an instructional check by containing a zero-lag condition such that no items intervene between study and test (e.g. subjects see the word "grass" and, following a brief delay, are tested with the stem gra___). We have found that elderly subjects as well as patients with closed-head injury and focal (including temporal lobe) brain damage have little difficulty understanding instructions as indicated by nearly perfect performance in this zero-lag condition.

It should also be noted that one particular set of inclusion/exclusion instructions does not exhaust the variety of instructions or methods with which the PDP can be implemented. For example, Jacoby et al. (1994b) have done a number of experiments using the subjective report "remember/know" technique introduced by Tulving (1985) and used extensively by Gardiner and colleagues (Gardiner & Java, 1993). When this procedure is coupled with the independence assumption, estimates of conscious and unconscious influences are nearly identical to those found using inclusion/exclusion instructions. Another example is provided by Lindsay and Jacoby (1994), who used process dissociation logic to separate automatic and controlled attentional processes in the Stroop task. Instructions were to name the ink colour of the word presented as quickly as possible. The important point here is that, unlike the implicit/explicit test approach, the PDP is not defined in terms of a particular set of instructions; rather, it provides a framework for separating processes under a variety of task conditions.

Theoretical Considerations

Use of the PDP to separate conscious recollection (R) and automatic familiarity (F) requires two test conditions, one in which controlled and automatic influences operate in concert (inclusion), and one in which the two processes operate in opposition (exclusion). It also requires that three assumptions are met. The first assumption is that the two processes make independent contributions to test performance; second, that R is of the same magnitude in the in-concert and in-opposition conditions; third, that the probability of responding old to an item on the basis of F is the same in the two conditions. Elsewhere we have extensively outlined and examined these assumptions (see Jacoby, 1991; Jacoby et al., 1993b; 1994b). Graf and Komatsu claim that the latter two assumptions "are violated in many circumstances" but provide only a single experiment (Komatsu et al., 1995, experiment 1) to back up their claim. Moreover, it should be emphasised that all of Graf and Komatsu's comments are directed at the two-list memory paradigm originally used by Jacoby (1991); none of their arguments have parallels in the other paradigms for which the PDP has been successfully applied (i.e. single-list recognition and cued-recall; Stroop). Below, we address Graf and Komatsu's charges as they apply to two-list memory paradigms. We begin by noting that many of their theoretical arguments stem from the tacit adoption of a model of memory quite different from the one we have proposed.

Graf and Komatsu claim that the assumptions underlying the PDP are the same as those made by Kintsch (1970) when describing his generate/recognise (G/R) model. In fact, the assumptions underlying our "direct retrieval" model could not be more distinct from those underlying Kintsch's G/R model. A G/R model assumes that conscious processing is dependent on unconscious processing; that is, their relationship is one of redundancy (see Jones, 1987; Jacoby et al., 1994b). In a stem-completion task, for example, subjects may automatically generate a completion to a stem and then attempt to consciously recognise whether the word was presented earlier. In contrast, our direct retrieval model (Jacoby et al., 1993b) assumes that conscious and unconscous influences are fully independent of one another. Responses to a stem could reflect automatic influences, conscious recollection, or both processes co-occurring. There is no sequential, dependent relation between the two processes.

Invariance in Familiarity

Jacoby (1991; see also Jacoby & Dallas, 1981; Mandler, 1980) hypothesised that, for recognition, R acts as a discrete state; that is, the subject either does or does not recollect episodic details relevant to the memory discrimination. In contrast, F can be viewed as an index of strength similar to that described by signal detection theory (see Yonelinas, 1994). As in signal detection theory, different response criteria across conditions can be indexed by false alarm rates (i.e. "old" responses to new items). Thus, if subjects are using F differentially on the inclusion and exclusion tests (i.e. if they are changing their response criterion), it would be reflected in different false alarm rates. We are perplexed by Graf and Komatsu's assertion that the PDP "does not take into account false alarm rates" (p. 120). False alarm rates are reported and discussed in every empirical paper using the PDP. Toth et al. (1994) include a formal description of how base rate can be included in the PDP equations.

Differential response bias across conditions or populations is a problem for any approach to the measurement of memory. Elsewhere, we have gone to great lengths to articulate the nature of this problem and to solve it (Jacoby, 1991; Jacoby et al., 1993b; Reingold & Toth, in press; Toth et al., 1994; Yonelinas, 1994). As support for the claim that the familiarity assumption is often violated, Komatsu et al. (1995) report a single experiment in which false alarm rates (i.e. performance on new items) were significantly different in the inclusion and exclusion tests. We do not know why this difference occurred, but do note that it is not replicated in their second experiment, nor in a variety of unpublished experiments we have done. More importantly, if Graf and Komatsu are correct in their claim that "the inclusion/exclusion instructions are a clear example of a manipulation that influences subjects' response criterion" (p. 120), how do they explain the numerous experiments (for reviews, see Jacoby & Begg, submitted; Jacoby et al., 1994b) in which no such differences are obtained?

As discussed below, that assumptions underlying the PDP can sometimes be violated is not surprising. What is surprising is that, although crucial for the interpretation of memory performance, the base rate assumptions underlying direct/indirect test comparisons are rarely evaluated or even described. Examination of the literature reveals widely different base rates between implicit and explicit conditions, when they are reported, yet these differences are often ignored and results are reported in terms of "priming" scores. In contrast, application of the PDP requires one to pay careful attention to the issue of response criterion.

Invariance in Recollection

Graf and Komatsu question the assumption that recollection in the recognition-memory paradigm is the same in the inclusion and exclusion tests. The basis for their argument is that, in addition to an old/new decision, subjects must make an additional source discrimination in the exclusion test to determine list membership. However, this criticism assumes that recognition memory is based on sequential memory decisions, one for the item and one for its source. That is, Graf and Komatsu seem to be assuming a source discrimination model of the sort explored by Johnson and colleagues (Johnson, Hashtroudi, & Lindsay, 1993; see also Humphreys & Bain, 1983). The problem with such a redundancy model is that it does not easily accommodate two *independent* bases for recognition because source recollection follows, and thus is *dependent* on, item recollection. Moreover, such a model does not seem compatible with results obtained with the PDP (Jacoby, 1991; Toth, unpublished data; Yonelinas & Jacoby, in press).

Perhaps the more general point Graf and Komatsu are making is that F, as measured by the PDP in the two-list paradigm (Jacoby, 1991), may sometimes reflect partial recollection—that is, remembering an item's prior presentation in the experiment, but not remembering its source (see Yonelinas, 1994; see also Roediger & McDermott, 1994; Verfaellie, 1994). This is an important consideration, but it does not necessarily represent a violation of the recollection assumption. As measured by the PDP, recollection is specific to the particular memory judgement required.

That is, if the task requires a subject to discriminate items on the basis of their source (e.g. list 1 *vs* list 2) or contextual attributes (e.g. whether it was heard or read), estimated R is indexing memory for episodic details that will support such a discrimination. That a subject can sometimes recollect an item's prior presentation but not its source (Komatsu et al., 1995, experiment 2), or that source discrimination can be selectively impaired in amnesia (Shimamura & Squire, 1991) or in the elderly (McIntyre & Craik, 1987), does not, by itself, constitute evidence against the recollection assumption. As with the concept of involuntary explicit memory, whether partial recollection would alter estimates from the PDP, or indeed under what circumstances the phenomenon occurs at all, is an open question that needs to be addressed empirically, not decided by fiat. In contrast to Graf and Komatsu, who seem to want to build in a separate parameter for every possible contingency, we have proposed a relatively simple model of recognition that is highly constrained. If partial recollection turns out to be an important aspect of performance, we can add parameters to our model; but first show us the data that compels the added complexity.

It is important to understand that our claim has never been that the assumptions underlying the PDP will hold in every circumstance, only that we can arrange a situation such that they do, and that doing so has important theoretical advantages. One major advantage is that, by building tasks for which the independence assumption holds, we can investigate variables of long-standing importance in psychology (e.g. divided attention, ageing, brain damage, intoxication). Another advantage is being able to identify conditions in which the assumptions are violated. Jacoby and Begg (submitted) have recently done experiments specifically designed to induce dependent strategies such as those embodied in generate–recognise models of memory. Their results suggest that the PDP can be used to detect these retrieval modes, and thus help to illuminate the conditions under which they occur. Note, however, that violations of the assumptions do not undermine the procedure, but only identify a set of conditions in which it does not apply. Thus, showing that conditions can be arranged such that false alarm rates for high-frequency words differ significantly across inclusion and exclusion tests (Komatsu et al., 1995) may tell us something about those particular experimental conditions but they do not impugn the validity of the PDP or the theory underlying it. That one can violate the assumptions underlying the PDP is no more interesting than showing that one can violate the assumptions underlying analysis of variance.

Graf and Komatsu's unique assumptions about familiarity, recollection, and how they are combined, are brought together in their "more general model for quantifying consciously controlled versus automatic processes". Borrowing ideas from Mandler (1980) and Jacoby (1991), Graf and Komatsu state that "we assume that recognition test decisions are made on the basis of familiarity and recollection, and that these two factors operate independently of each other" (p. 123). Curiously, however, they then go on to elaborate a redundancy (i.e. dependent) model of inclusion and exclusion performance, compounding the independent/dependent confusion by adding additional parameters to our exclusion equation. In this sense, our model does indeed become a "special case" of Graf and Komatsu's given that, with the addition of unconstrained parameters, their model encompasses a wide range of theoretical possibilities. Models need constraints. We have proposed a relatively simple, parsimonious model that makes straightforward predictions, yet accounts for a great deal of empirical data. Graf and Komatsu's model, in contrast, makes few, if any, falsifiable predictions, because it contains twice as many parameters. Falsification is probably irrelevant, however, because Graf and Komatsu provide no means for estimating their parameters.

WHEN IS CAUTION NECESSARY? WHEN USING THE PROCESS DISSOCIATION PROCEDURE

The PDP is a new technique that we believe provides a very promising framework for answering long-standing and important questions concerning the relationship between awareness, intentionality and memory. It is also a more complex technique than its predecessors and should be used judiciously and with caution. Above and elsewhere we have outlined the assumptions on which the procedure is based and the strategies a researcher might use to guarantee that these assumptions are met. One of the most important conditions for application of the PDP is that response criteria are equated in the inclusion and exclusion conditions (the "familiarity assumption"). This assumption is most easily verified by performance on baseline ("new") items. Another prerequisite is that performance in the inclusion and exclusion conditions is not at floor or ceiling because extreme values can artificially inflate or deflate estimates of automaticity (see Jacoby et al., 1993b). Test instructions also play an important role in helping meet the assumptions. In a number of papers we have read or reviewed, researchers often instruct subjects in the exclusion condition to complete stems with the first word that comes to mind, but to avoid old completions (e.g. Richardson-Klavehn et al., 1994). Jacoby and Begg (submitted) present evidence that such instructions engender a generate–recognise approach to the task, thus resulting in a violation of the independence assumption. To avoid such a violation, we strongly encourage subjects in both the inclusion and exclusion conditions to use stems as cues for direct retrieval of study words (see Jacoby et al., 1993b; Toth et al., 1994).

As noted by Jacoby (1991), the recollection assumption is the least amenable to empirical check because, unlike the familiarity assumption which can be assessed through false alarm rates, there is generally no independent measure of recollection. The biggest worry is that subjects may use a dependent generate–recognise strategy as a basis for exclusion in cued-recall experiments. The strongest evidence against such a strategy is the invariance in estimates of automaticity across manipulations which, on *a priori* grounds, should affect consciously controlled processes but leave automatic influences unchanged (see Jacoby & Begg, submitted, for additional signatures of a generate–recognise strategy). Jacoby et al. (1994b) examined controlled and automatic estimates from 20 different conditions investigating variables associated with the concept of automaticity (i.e. divided attention, speeded responding and ageing). Across these conditions, the average difference in estimates of control was 0.24; for automaticity, the corresponding value was 0.002. Thus, manipulations traditionally associated with automaticity provide converging evidence for the recollection and independence assumptions. Implicit (and explicit) tests may also be useful as converging evidence. As noted above, we have found that estimates of automaticity derived from the PDP may match the probability of responding on uncontaminated implicit tests (Jacoby et al., 1994b; Toth et al., 1994).

CONCLUSIONS

We believe the PDP represents a significant advancement in our ability to separate and measure controlled and automatic processes underlying task performance, as well as specifying the relationship between the two. However, the procedure only provides a general framework for addressing these questions; further progress will require creative adaptation of the procedure to different domains and populations. Because of its novelty and promise, we applaud critical analysis and empirical exploration of the PDP. However, Graf and Komatsu's critique is based almost entirely on untested theoretical notions (e.g. involuntary explicit memory, partial recollection) and on a single experiment (Komatsu et al., 1995) which failed to satisfy a boundary condition that we had earlier specified as critical for appropriate use of the PDP. In sum, we see their critique as alarmist; it also contains indisputable errors of fact.

Perhaps the most pernicious aspect of Graf and Komatsu's critique is their belief that implicit and explicit *tests* are the best representatives of intentional and unintentional *processes*. This assumption is clearly seen in their claim that "the PDP is not suitable for learning about implicit versus explicit memory *test performance*" (p. 116, emphasis added). We hold in high regard the contributions that have been made by comparing performance on direct and indirect tests (e.g. Jacoby & Dallas, 1981; Tulving, Schacter, & Stark, 1982). As should be clear, however, we grow weary of the incautious theorising surrounding these tests because it is clear that they do not selectively index conscious (intentional) and unconscious (unintentional) processes. Contrary to Graf and Komatsu, we view "impicit memory" not as a newly discovered phenomenon, but as a contemporary label for a class of phenomena that have been under investigation since the beginnings of psychology as a science. The only thing that defines implicit memory as a distinct area of research is the use of non-directive ("indirect") test instructions, and even this is far from new (see Schacter, 1987; Toth & Reingold, in press). From our perspective, the "field" of implicit memory is best viewed as a stepping stone on the road to more powerful approaches to the measurement of conscious/intentional and unconscious/unintentional influences.

Our true commitment is not to any one procedure, but to separating automatic and controlled influences within the confines of a single task. We have used a variety of procedures in addition to the inclusion/exclusion procedure critiqued by Graf and Komatsu. A prerequisite for any procedure is to start with an assumption about the relation between the processes contributing to performance. Graf and Komatsu, as well as others (e.g. Richardson-Klavehn et al., in press), seem to yearn for process-pure, assumption-free measures of conscious and unconscious influences. We do not believe in assumption-free measures. As any student of the philosophy of science knows, the best one can do is to ask: How would the world look if I adopted this particular assumption? Can I design a situation for which the assumption is likely to hold? Can I find converging evidence to support a claim that the assumption did hold? We have asked these questions, and are heartened by the answers we have received.

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