Analyzing estimates of automatic and conscious retrieval in between- and within-subjects designs

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The process dissociation procedure (PDP) was developed as a means of quantifying the contributions of conscious and automatic retrieval to memory performance. Although the required inclusion and exclusion conditions are manipulated within subjects in most experiments, several researchers have expressed concerns that the instructions may be particularly difficult to follow under these conditions. Thus, manipulating instructions between subjects may have some advantages, although some aspects of the statistical analysis of conscious and automatic estimates are not routine in such a design. We describe a procedure for this analysis and present some initial data to illustrate its application to PDP. We also comment on other issues related to the calculation of estimates of automatic and conscious retrieval in between- and within-subjects designs.

The process dissociation procedure (PDP) was developed by Jacoby and his colleagues (Jacoby, 1991; Jacoby, Toth, & Yonelinas, 1993; Toth, Reingold, & Jacoby, 1994) as a technique for isolating the contributions of conscious and automatic influences in memory performance. A primary impetus for this development was the concern that standard implicit memory tasks may be "contaminated" with conscious retrieval, even though they have often been described as measuring automatic retrieval (see, e.g., Masson, 1984; Moscovitch, Goshen-Gottstein, & Vriezen, 1994). In addition, performance on standard explicit tests is often described as reflecting conscious retrieval, although automatic retrieval may contribute to performance on such tasks (Jacoby, 1991). The PDP in fact makes use of the latter for purposes of evaluating the contributions of conscious and automatic retrieval.

The direct retrieval model is the principal theoretical tenet in the PDP (see, e.g., Reingold & Toth, 1995). It specifies that subjects can engage conscious and automatic retrieval processes upon presentation of a test cue, and, importantly, these retrieval processes can run off independently under some conditions. Estimates of conscious and automatic retrieval are derived from performance on two tests that subjects are asked to complete. On the inclusion test, subjects are presented a cue and asked to use that cue to produce an item that was studied previously. Failing that, they are to respond with the first word to come to mind. On the exclusion test, subjects are instructed to use the cue to produce an item that was not studied pre-

viously. Because the conscious and automatic retrieval components are assumed to be independent, performance on the inclusion and exclusion tests are defined as

$$I = C + A(1 - C) \quad (1)$$

and

$$E = A(1 - C), \quad (2)$$

where I and E refer to performance on the inclusion and exclusion tasks, respectively, and C and A refer to the conscious and automatic components, respectively. The C and A components can then be derived as

$$C = I - E \quad (3)$$

and

$$A = \frac{E}{(1 - C)}. \quad (4)$$

In most research using these tasks, the inclusion and exclusion tests have been manipulated within subjects during a single test trial (e.g., Jacoby et al., 1993; Toth et al., 1994). This type of design makes statistical analysis of the effects of an independent variable on C and A quite straightforward, because each subject provides an estimate of both C and A . However, inasmuch as several researchers have suggested that mixing the two tests in a single test trial may create confusion for subjects (e.g., Buchner, Erdfelder, & Vaterrodt-Plünnecke, 1995; Curran & Hintzman, 1995; Graf & Komatsu, 1994; but see Toth, Reingold, & Jacoby, 1995), manipulating the two tests between subjects may be a reasonable way of addressing this potential methodological problem. Occasionally, inclusion and exclusion tests have been conducted on independent groups of subjects (Buchner et al., 1995; Dodson & Johnson, 1996; Jacoby, 1991, Experiment 3; Russo & Andrade, 1995). Yonelinas and Jacoby (1996) have advised against this type of design, because it may result in

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differential baseline rates in the inclusion and exclusion conditions, indicating different output criteria. Although such findings can occur (Dodson & Johnson, 1996), they are not inevitable, as we will show later. A second reason that Yonelinas and Jacoby offer for not using a between-subjects design is that there is no procedure available for analyzing estimates of C and A , because group means for C and A are derived directly, rather than through individual subject estimates. In his own research, Jacoby (1991, Experiment 3) presented only summary values. Dodson and Johnson (1996) elected to create macrosubjects by randomly pairing subjects in the inclusion and exclusion conditions. The latter procedure has an advantage in that it allows C and A estimates to be compared with those derived from the more standard within-subjects manipulation of inclusion and exclusion tests. However this must be done cautiously, inasmuch as any subject or subject/item correlations that exist in the data (cf. Hintzman & Curran, 1997; Jacoby & ShROUT, 1997) may be reduced or eliminated with this procedure. The preferred solution is to use an accepted procedure for analyzing the group estimates of C and A derived from such designs. The key problem is to identify a variance estimate for the distributions of C and A . We provide such a procedure below. We elaborate on some of the theoretical issues later.

The procedure for deriving the variance of the distributions of C and A that we describe here is based on Cochran (1977) and Stuart and Ord (1987). To avoid confusion, we continue to use E to refer to scores on the exclusion test, and we will use EV to refer to the expected value.

Let I and E be measured either on the same groups or on independent groups. Let I have mean μ_I and variance σ_I^2 , and let E have mean μ_E and variance σ_E^2 . Define $\mu_C = \mu_I - \mu_E$ and

$$\mu_A = \frac{\mu_E}{1 - \mu_C}.$$

Our estimates of μ_C and μ_A are denoted $\bar{C}$ and $\bar{A}$, respectively, given by

$$\bar{C} = \bar{I} - \bar{E}, \quad (5)$$

where $\bar{I}$ and $\bar{E}$ are the usual sample averages, and

$$\bar{A} = \frac{\bar{E}}{1 - \bar{C}}. \quad (6)$$

If $\bar{I}$ and $\bar{E}$ are measured on n individuals simultaneously, the variance of $\bar{C}$ is defined as

$$\sigma_{\bar{C}}^2 = \frac{\sigma_I^2 + \sigma_E^2 - 2 \text{cov}(I, E)}{n} = \sigma_{\bar{I}}^2 + \sigma_{\bar{E}}^2 - 2 \text{cov}(\bar{I}, \bar{E}), \quad (7)$$

whereas, if I and E are measured on two independent groups of size n_I and n_E , respectively,

$$\sigma_{\bar{C}}^2 = \frac{\sigma_I^2}{n_I} + \frac{\sigma_E^2}{n_E} = \sigma_{\bar{I}}^2 + \sigma_{\bar{E}}^2, \quad (8)$$

which can then be estimated using sample values, specifically

$$S_{\bar{C}}^2 = S_{\bar{I}}^2 + S_{\bar{E}}^2. \quad (9)$$

The derivation of the asymptotic variance of $\bar{A}$ is based on an adaptation of a proof shown by Cochran (1977, p. 31). By using Taylor series expansion and ignoring terms of the order n^{-1} (Stuart & Ord, 1987, p. 324), it can be shown that

$$\bar{A} - \mu_A = \frac{\bar{E} - \frac{(1 + \bar{E} - \bar{I})\mu_E}{1 + \mu_E - \mu_I}}{1 + \bar{E} - \bar{I}} \approx \frac{\bar{E} - \frac{(1 + \bar{E} - \bar{I})\mu_E}{1 + \mu_E - \mu_I}}{1 + \mu_E - \mu_I}, \quad (10)$$

and, thus,

$$EV(\bar{A} - \mu_A) \approx \left(\mu_E - \frac{(1 + \mu_E - \mu_I)\mu_E}{1 + \mu_E - \mu_I} \right) / (1 + \mu_E - \mu_I) = 0. \quad (11)$$

Hence $\bar{A}$ is unbiased (asymptotically) for μ_A .

In calculating variances of variables, we often require the expected value of the square of a variable [e.g., $EV(X^2)$]. When we know the mean and variance of X , then $EV(X^2) = \sigma_X^2 + \mu_X^2$. This is just a rearrangement of the formula for calculating variances:

$$\sigma_X^2 = EV[(X - \mu_X)^2] = EV(X^2) - 2\mu_X EV(X) + \mu_X^2 = EV(X^2) - \mu_X^2. \quad (12)$$

Similarly, the formula for calculating the covariance between two variables can be rearranged to give $EV(XY) = \text{cov}(X, Y) + \mu_X \mu_Y$. Thus,

$$\sigma_{\bar{A}}^2 \approx \frac{1}{\mu_{1-C}^2} EV[\bar{E} - \mu_A(1 - \bar{C})]^2 \quad (13)$$

$$= \frac{1}{\mu_{1-C}^2} EV[\bar{E}^2 - 2\mu_A \bar{E}(1 - \bar{C}) + \mu_A^2(1 - \bar{C})^2] \quad (14)$$

$$= \frac{1}{\mu_{1-C}^2} \left\{ \sigma_{\bar{E}}^2 + \mu_E^2 - 2\mu_A [\text{cov}(\bar{E}, 1 - \bar{C}) + \mu_E(1 - \mu_C)] + \mu_A^2(\sigma_{1-\bar{C}}^2 + \mu_{1-C}^2) \right\} \quad (15)$$

$$= \frac{1}{\mu_{1-C}^2} \left[\sigma_{\bar{E}}^2 - 2\mu_A \text{cov}(\bar{E}, 1 - \bar{C}) + \mu_A^2 \sigma_{1-\bar{C}}^2 \right] \quad (16)$$

$$= \frac{\mu_E^2}{\mu_{1-C}^2} \left[\frac{\sigma_{\bar{E}}^2}{\mu_E^2} - 2 \frac{\text{cov}(\bar{E}, 1 - \bar{C})}{\mu_E \mu_{1-C}} + \frac{\sigma_{1-\bar{C}}^2}{\mu_{1-C}^2} \right]. \quad (17)$$

This is identical to the formula for the variance of a ratio (Stuart & Ord, 1987, p. 325). Note that we have used the fact that the expected value of a sample average is the population mean to simplify the above expressions.

If I and E are measured simultaneously on a common group of size n , then, at least asymptotically,

$$\sigma_A^2 = \frac{1}{n} \frac{\mu_E^2}{\mu_{1-C}^2} \left(\frac{\sigma_E^2}{\mu_E^2} - 2 \frac{\sigma_E^2}{\mu_E \mu_{1-C}} + 2 \frac{\text{cov}(E, I)}{\mu_E \mu_{1-C}} + \frac{\sigma_C^2}{\mu_{1-C}^2} \right), \quad (18)$$

whereas, if I and E are measured on independent groups, then again, asymptotically,

$$\sigma_A^2 = \frac{1}{\mu_{1-C}^4} \left[\frac{\sigma_E^2}{n_E} (1 - \mu_I)^2 + \frac{\sigma_I^2}{n_I} (\mu_E^2) \right], \quad (19)$$

which can then be estimated using sample values, specifically,

$$S_A^2 = \frac{1}{\bar{X}_{1-C}^4} \left[\frac{S_E^2}{n_E} (1 - \bar{X}_I)^2 + \frac{S_I^2}{n_I} (\bar{X}_E^2) \right]. \quad (20)$$

If

$$A = \frac{E}{(1 - C)}$$

and $1 - C$ are independent, it is easy to show that the expected value of A is exactly μ_A , as defined above. Further, we have the exact result

$$\sigma_A^2 = \frac{\sigma_E^2 - \mu_A^2 \sigma_{1-C}^2}{\sigma_{1-C}^2 + \mu_{1-C}^2}. \quad (21)$$

AN EXAMPLE

We have completed several experiments in which the standard inclusion and exclusion tests were manipulated between subjects. For illustration purposes, we provide the analysis of the data from one of these experiments here. A more detailed report of the research is in preparation.

A total of 32 subjects studied a list of 24 critical words (plus 6 buffers) under semantic and nonsemantic orienting task instructions. Following an unrelated 5–7 min filler task, half the subjects were given a 48-item word stem completion test under inclusion instructions, and half were given the test under exclusion instructions. For this test, half the items could be completed with studied items, and half with baseline items. Which items served as studied and which as baseline items was counterbalanced. All the stems had multiple familiar solutions.

Following the format often used in reports of PDP experiments, we begin with an analysis of variance of inclusion and exclusion scores as a function of orienting task (semantic, nonsemantic). Orienting task was manipulated within subjects, and test type was manipulated between

Table 1
Empirical Values for the Calculation of the Means and Standard Errors of the C and A Distributions

Empirical values*	Orienting Task	
	Semantic	Nonsemantic
Mean of I	.496	.321
Standard error of I	.046	.041
Mean of E	.070	.185
Standard error of E	.021	.036
Estimate of C	.426	.136
Standard error of C	.051	.055
Estimate of A	.122	.214
Standard error of A	.035	.034

* I , inclusion; E , exclusion; C , conscious; A , automatic.

subjects. The data appear in Table 1. The analysis revealed a reliable effect of test type [$F(1,30) = 42.69$, $MS_e = 4.43$], a marginal effect of orienting task [$F(1,30) = 3.44$, $MS_e = 1.82$, $p < .07$], and a reliable interaction [$F(1,30) = 30.96$]. As is evident in Table 1, subjects were better able to utilize the episodic history of semantically encoded items, as indicated by their higher inclusion rates and lower exclusion rates. A separate analysis of inclusion scores revealed a reliable effect of orienting task [$F(1,15) = 12.76$, $MS_e = 0.02$], as did exclusion scores [$F(1,15) = 16.73$, $MS_e = 0.01$]. There was no reliable difference in baseline scores for the inclusion (.14) and exclusion (.13) conditions [$t(30) = 0.43$]. Overall, these effects parallel those seen in other PDP studies (e.g., Toth et al., 1994, Experiment 1), although it is notable that the difference between inclusion and exclusion scores for nonsemantic items was much larger in our study than in the earlier study (.14 vs. .04).

Estimates of conscious and automatic influences were derived from inclusion and exclusion test scores with the standard procedures (Equations 5 and 6), except that they were based on overall group averages rather than on individual subject estimates. The estimate of the standard error for the distribution of C is derived from Equation 9. The estimate of the standard error for the distribution of A is derived from Equation 20. The critical values used in the calculation appear in Table 1.

Although it would be particularly desirable to analyze conscious and automatic estimates within the same statistical design, as is done when inclusion and exclusion tests are given to the same groups of subjects, this is problematic, given that I and E are measured on independent groups of subjects but the calculated values C and A incorporate both sets of values

$$\left(\bar{C} = \bar{I} - \bar{E} \text{ and } \bar{A} = \frac{\bar{E}}{1 - \bar{I} + \bar{E}} \right).$$

As a result, it is not obvious whether C and A should be treated as a between- or a within-subjects factor. Accordingly, we conducted separate analyses on the conscious and automatic estimates. The semantic-orienting task led to a reliably higher estimate of conscious influences than

did the nonsemantic-orienting task [$t(30) = 3.90$]; however, the numerical advantage of the nonsemantic task on the automatic influences only approached significance [$t(30) = 1.88, p = .07$]. Again, this pattern largely replicates that reported previously, although the tendency toward significance in the automatic estimates is not typical (but see Curran & Hintzman, 1995; Toth et al., 1994, Experiment 2). It should be noted that the problem of exclusion scores of zero that must be addressed in a within-subjects design is not an issue in the present design, because group data are used, rather than estimates from each subject individually. Thus, no subjects were deleted from the analysis presented here. This issue is taken up below.

DISCUSSION

We have described a procedure that allows for statistical analysis of the effects of independent variables on conscious and automatic influences in a PDP study when the type of test (inclusion, exclusion) is manipulated between rather than within subjects. Given concerns that have been expressed regarding the latter, a between-subjects design may provide a satisfactory methodological solution, and the procedure that we have described allows for analysis of such data.

We have not addressed the theoretical implications of manipulating the inclusion and exclusion tests between subjects. Although the case has been made that a within-subjects manipulation of inclusion and exclusion tests is desirable to ensure that subjects engage in direct retrieval (Yonelinas & Jacoby, 1996), no theoretical or empirical evidence has been offered to support this hypothesis. One justification for using a within-subjects design is that it will tend to avoid differences in baseline attributable to differences in response bias (Yonelinas & Jacoby, 1996). We found no evidence for such differences in the present study, or in any other study in which we have manipulated type of test between subjects (but see Dodson & Johnson, 1996).

What seems most relevant in the decision as to the type of design to use is whether it induces a direct retrieval or a generate/recognize strategy. The former simultaneously invokes conscious and automatic retrieval, whereas the latter induces an automatic generation phase, followed by an attempt to use familiarity to make a recognition decision. This logic is tied to the *awareness assumption* that is built into the PDP model—namely, that conscious awareness of the prior episodic history of an item can only occur following a successful conscious retrieval attempt. The term *recollection* has been used to refer to conscious retrieval followed by the awareness of the membership of the item in the study list (Jacoby, 1998; Yonelinas & Jacoby, 1995). By contrast, the awareness assumption also specifies that subjects can only use familiarity to make a recognition decision if the retrieval is completed automatically. Others have challenged the as-

sumption that conscious awareness of the prior episodic history can arise only following successful conscious retrieval (e.g., Graf & Komatsu, 1994; Richardson-Klavehn & Gardiner, 1998; Richardson-Klavehn, Gardiner, & Java, 1996).

The issue of whether *only* within-subjects designs can be used to deliver a direct retrieval strategy is, as noted, without empirical foundation as yet. There is, however, reason to be concerned about manipulating inclusion and exclusion tasks randomly within subjects. Bodner, Masson, and Caldwell (1999) have made the empirical point that subjects may become confused, so that they sometimes make an inclusion decision on an exclusion trial (see also Buchner et al., 1995; Graf & Komatsu, 1994; Richardson-Klavehn et al., 1996). They demonstrated that the probability of subjects providing a target solution to a stem on an exclusion trial when it immediately followed two or more inclusion trials was reliably higher than when the exclusion trial followed two or more exclusion trials. If such evidence reflects confusion in following instructions, the likelihood of obtaining the problematic exclusion score of zero is reduced. Thus, a higher rate of exclusion scores of zero when the test type is either manipulated between subjects or blocked within subjects is not necessarily a concern in itself and, indeed, may be evidence that subjects are not having the difficulty in following instructions that they do when test type is manipulated within subjects. Importantly, as long as the instructions induce conscious retrieval from the onset of the test cue, the conditions for direct retrieval may be satisfied by a between-subjects manipulation of test type, and the problems associated with a within-subjects manipulation may be reduced. Other evidence, reported by Bodner et al. takes these concerns even further. These authors challenged the conclusion that subjects given direct retrieval instructions in fact reliably invoke a direct retrieval strategy, as opposed to a generate/recognize strategy. They concluded that subjects probably used a combination of the two strategies throughout the test trial, even though the “signatures” of a generate/recognize strategy were not forthcoming.

The final matter to be addressed deals with exclusion scores of zero. Although such data are a problem in terms of analysis, it should be kept in mind that they may be evidence of subjects with good memory for studied items, to the extent that they can exclude studied words when expected to do so (Curran & Hintzman, 1995). Perfect inclusion scores yield the same problem; however, such findings are rare. When test type is manipulated within subjects, of course, the data from both the inclusion and the exclusion conditions are eliminated when these subjects are dropped from the analysis. Thus, when test type is manipulated between subjects, deleting the data from those subjects who yield very good memory in the inclusion condition would be reasonable. When this was done in the study reported here, the estimates of C and A

for the semantic condition were .212 and .351, respectively. For the nonsemantic condition, they were -.025 and .253. The near-zero score in the former cell indicates that the subjects who remained in the analysis were simply not discriminating studied and nonstudied items on the inclusion and exclusion tests. Not surprisingly, this procedure results in much lower estimates of C than those in the initial analysis and somewhat higher estimates of A , at least in the semantic-encoding condition. These findings are entirely consistent with a model in which C and A are correlated processes (Curran & Hintzman, 1995, 1997).

However, there is another means of addressing the problem of exclusion scores of zero. This solution has been used in the signal detection literature for some time. When calculating discriminability (d') and criterion (β) measures in such paradigms, it has been common practice to "correct" perfect scores in a variety of ways that, although they do not perfectly reflect the high degree of accuracy that subjects sometimes achieve, will at least allow a reasonable approximation for d' and β . One such practice has been to set the perfect score .01 above zero or below unity, as appropriate, or to give the subject a score of a half item above zero on false alarms or a half item less than unity on hits. Either strategy could be considered an acceptable alternative to simply discarding data from subjects who are able to follow exclusion (or inclusion) instructions perfectly.

The impact of assigning a score of a half item to exclusion scores of zero should be relatively small in the study described above, because group data are used in a between-subjects design, thus requiring the elimination of none of the exclusion subjects. (No correction was considered in the inclusion condition, because no subject provided a perfect score.) The resulting estimates for C and A in the semantic condition are .406 and .151, respectively. The corresponding values in the nonsemantic condition are .128 and .221. Overall, there is a small decrease in C estimates (an effect that is larger for the semantic condition) and a corresponding slight increase in A estimates. Importantly, the effects of this type of correction would normally be exactly the opposite, and larger, when test type is manipulated within subjects, because presumably those subjects with the best memory performance would now be included in the analysis, whereas they would otherwise be deleted. Thus, estimates of C must necessarily increase (those with high inclusion scores and low exclusion scores are now retained), and, if C and A are inversely correlated, A would be expected to decrease.

In summary, whether a within-subjects manipulation of the inclusion and exclusion tests is a requirement for satisfying the conditions of direct retrieval has yet to be demonstrated, either theoretically or methodologically. Certainly, it seems possible to establish conscious retrieval at the time of test cue presentation when test type is not manipulated within subjects, and it is this feature that is

required by the theoretical assumptions of PDP. Furthermore, whether a within-subjects manipulation of test type is even desirable is questionable, given the concern over and evidence of confusion that subjects experience under these conditions. Resolution of these issues awaits further theoretical development and empirical work.

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