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OBSERVATIONS

In Defense of Functional Independence: Violations of Assumptions Underlying the Process-Dissociation Procedure?

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T. Curran and D. L. Hintzman (1995) claim to have shown that the independence assumption underlying the process-dissociation procedure (L. L. Jacoby, 1991) is not justified. They argued that correlations between processes at the level of items can result in an underestimation of automatic processes large enough to produce artifactual dissociations between process estimates. In contrast, the authors show that the effects of extremely high correlations at the level of items are likely to be trivial, and not differential across conditions. Curran and Hintzman's dissociations probably reflect violations of boundary conditions for use of the process-dissociation procedure, rather than violations of independence.

It is important to distinguish between automatic and consciously controlled memory processes. For example, although amnesic patients are often unable to consciously remember previously presented words on direct memory tests, such as recall or recognition tests, they use the words on indirect memory tests, such as stem- or fragment-completion tests, more often than would be expected by chance (Moscovitch, Vriezen, & Gottstein, 1993). Similar dissociations are found in people with normally functioning memory (Roediger & McDermott, 1993). Comparing direct and indirect memory tests has significantly advanced our understanding of automatic and controlled processes. However, performance rarely reflects only one process acting in isolation; that is, controlled processes often influence performance on indirect memory tests (Holender, 1986; Toth, Reingold, & Jacoby, 1994), and automatic processes affect performance on direct memory tests (Jacoby, Toth, & Yonelinas, 1993). The process-dissociation procedure (Jacoby, 1991) was designed to separate automatic and controlled memory processes when both are affecting performance.

As a brief introduction to the process-dissociation procedure, consider Experiment 1B reported by Jacoby et al. (1993). Subjects studied words under full or divided attention and then were tested with word stems (e.g., *mot__* for *motel*). For an *inclusion test,* subjects were instructed to use the stem as a cue to recall 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 is like a standard test of cued recall with instructions to guess when recollection fails. Subjects could complete a stem with an old word either because they recollected the old word, with a probability of R, or because the old word came automatically to mind, with a probability of A. If these two bases for responding are independent, then inclusion performance equals $R + A - RA$. For an *exclusion test,* subjects were instructed to use the stem as a cue to recall an old word but not to use recalled words to complete the stems. That is, subjects were told to exclude old words and to complete stems only with new words. In this condition, subjects would complete a stem with an old word only if the word came automatically to mind without recollection of its prior presentation: $A(1 - R) = A - RA$. The difference between the inclusion and exclusion tests provides an estimate of the probability of recollection. Given that estimate, one can compute the probability of an old word automatically coming to mind: A = Exclusion/$(1 - R)$. When these equations were applied to the data from Jacoby et al., results showed that dividing attention significantly reduced estimates of recollection (.25 vs. .00) but left automatic influences almost invariant (.47 vs. .46). That is, the estimates showed a process dissociation similar to the task dissociations found between direct and indirect memory tests (Koriat & Feuerstein, 1976; Parkin, Reid, & Russo, 1990).

Table 1 summarizes results from experiments that have used the process-dissociation procedure to examine the effects of attention and of presentation duration on R and A.[1]

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[1] Results from Curran and Hintzman's (1995) Experiments 1, 4, and 5 are included in the table, but results from their Experiments 2 and 3 are not. The latter experiments used a *recollect-and-exclude*

Table 1
Changes in Recollection and Automatic Influences as a Function of Attention and Presentation Duration

Source	R_1	R_2	A_1	A_2	ΔR $(R_1 - R_2)$	ΔA $(A_1 - A_2)$
Jacoby, Toth, and Yonelinas (1993)						
Experiment 1A	.20	.00	.27	.27	.20	.00
Experiment 1B	.25	.00	.47	.46	.25	.01
Debner and Jacoby (1994)						
Experiment 2	.83	.41	.76	.75	.42	.01
Experiment 3	.75	.11	.66	.68	.64	−.02
Experiment 4	.62	.06	.50	.51	.56	−.01
Jacoby (1996a)						
Experiment 1						
Related	.32	.15	.45	.46	.17	−.01
Unrelated	.09	.04	.37	.38	.05	−.01
Jacoby (1996b)						
Experiment 2	.44	.22	.59	.58	.22	.01
Curran and Hintzman (1995)						
Experiment 1	.24	.13	.17	.18	.11	−.01
Experiment 4	.32	.16	.40	.39	.16	.01
Experiment 5	.40	.30	.28	.37	.10	−.09

Note. Scores of zero on the exclusion test were removed before computing estimates; see text for discussion. R_1 and A_1 refer to full-attention and long-duration conditions; R_2 and A_2 refer to divided-attention and short-duration conditions.

The table separates the estimates into conditions associated with relatively *good* performance (full attention and long presentation: R_1 and A_1) and those with relatively *poor* performance (divided attention and short presentation: R_2 and A_2). The performance measures were word-fragment completion and stem completion. Note that in each contrast, the *good* condition exceeds the *poor* condition in the estimate of controlled processes ($R_1 > R_2$), yet there is little or no difference between the estimates of automatic processes (A_1 vs. A_2). Indeed, if the one exception to this pattern is removed (Experiment 5 from Curran & Hintzman, 1995), the mean difference between A_1 and A_2 is −.002. Note further that the estimates of R and A span a considerable range, as does the size of $R_1 - R_2$; nevertheless, A_1 and A_2 remain nearly identical. We take these results to show that manipulations of attention and study time can have large effects on controlled processes but little or no effect on automatic processes.

The estimates presented in Table 1 are based on the assumption that the processes estimated by R and A are functionally independent. Despite the consistency of these findings, Curran and Hintzman (1995) have recently claimed that the independence assumption underlying our use of the process-dissociation procedure is unlikely to have been met. They argued that correlations between processes at the level of items violate the independence assumption, producing a bias in the estimation of automatic processes. Further, they argued that the bias is differential across conditions such that automatic processes are underestimated to a greater degree as conscious recollection increases. The result of this increasing underestimation was said to be artifactual (or "paradoxical") dissociations that reflect violations of the independence assumption rather than true dissociations between controlled and automatic processes.

Curran and Hintzman's (1995) arguments predict that an advantage of A_2 (divided attention or short presentation) over A_1 (full attention or long presentation) should emerge and increase in size as the difference between R_1 and R_2 (ΔR) increases. That is, there should be a negative correlation between change in R (ΔR) and change in A (ΔA). Against that prediction, the correlation between ΔR and ΔA for the studies presented in Table 1 is in the wrong direction and near zero (.13).

Curran and Hintzman (1995) reported significant item- and subject-based correlations between estimates of R and A and claimed that these correlations render the process-dissociation procedure "invalid." But if this is the case, how could invariances as complete as those shown in Table 1 have been obtained? One answer to this question is that, contrary to claims by Curran and Hintzman, estimates derived from the process-dissociation procedure are quite robust with respect to item-level correlations. In fact, in the next section, we show that even extremely high item correlations generally have only a small effect on estimated A. Of more importance, we show that the effect that can occur is unlikely to be differential across conditions. But if

procedure in which subjects attempted to give two responses to each stem: one in a column labeled *Remember,* another in a column labeled *New.* We see this as a variant of the remember–know procedure used by Gardiner and colleagues (e.g., Gardiner & Java, 1991). Curran and Hintzman allowed only two judgments (remember and new). Elsewhere (Jacoby, Yonelinas, et al., 1996), we describe our use of three judgments (remember, know, and new) in conjunction with stem-cued recall and discuss the relation of remember–know judgments to the process-dissociation procedure. Regardless of its exact nature, Curran and Hintzman's recollect-and-exclude procedure is distinct from the inclusion–exclusion procedure and thus does not weigh on interpretation of the results considered here.

Table 2
Example to Show the Effect of Aggregation Over Correlated R and A Values

	Short presentation				Long presentation			
Item	Inclusion	Exclusion	*R*	*A*	Inclusion	Exclusion	*R*	*A*
1	.36	.32	.04	.333	.52	.24	.28	.333
2	.60	.44	.16	.524	.70	.33	.37	.524
3	.80	.56	.24	.737	.85	.42	.43	.737
4	.96	.60	.36	.938	.97	.45	.52	.938
True	.68	.48	.20	.633	.76	.36	.40	.633
Estimate	.68	.48	.20	.600	.76	.36	.40	.600

Note. True values are the means of the values for the four items; for the estimate values, *R* and *A* are computed from the mean inclusion and exclusion scores. *R* = Inclusion − Exclusion; *A* = Exclusion/(1 − *R*).

item correlations do not produce large, differential biases in process estimates, then what explains the "artifactual" dissociations obtained by Curran and Hintzman? We believe Curran and Hintzman did not meet boundary conditions for applying the process-dissociation procedure. Those boundary conditions are the avoidance of floor effects and the participant's use of direct retrieval, as opposed to generate–recognize, as a strategy for recalling previously presented items. We elaborate on these points later in the article. First, we show how and why estimates gained from the process-dissociation procedure are relatively unaffected by item-based correlations. We begin with an example of correlation without artifactual dissociation and then further explain the arguments that gave rise to that example.

Correlation Without Artifactual Dissociation

The data summarized in Table 1 are mean values of *R* and *A* that were computed individually for each subject. When estimates are computed in this way, any correlation between measures across subjects is irrelevant for assessing the tenability of the assumption of independence. However, because the calculation of process estimates requires aggregating over items within subjects, correlations between *R* and *A* across items could result in estimates that do not reflect the true relation between processes.

Table 2 illustrates the effect of aggregating across items to estimate *R* and *A*. The example supposes that we know *R* and *A* for each of four items and compares that "true" *R* and *A* with estimates gained by aggregating across items. Consider the short-presentation data in the table. The true values are the actual means of the four items; the "estimates" are the results derived from the aggregated data (i.e., the mean inclusion and exclusion scores). Estimating *R* from the observed means (inclusion minus exclusion) gives .20, which is also the true value, but the estimated value of *A* (.600) is less than the true value (.633). The hidden covariate (*cov*) is the correlation between *R* and *A* across items within this subject. The amount of the underestimation of *A* (.033) is not overwhelming, even though the correlation is very high (.996); the respective standard deviations of *R* and *A* are .117 and .227. As described below, the amount of underestimation is equal to $cov/(1 - R)$, where $cov = r * SD_R * SD_A$.

What is the effect on the underestimation of *A* if, for each item, increasing study time produces a proportionate increase in *R* but leaves *A* unchanged? Since Ebbinghaus (1885/1964), learning curves have usually been found to be exponential, meaning that the increase in learning is a constant proportion of the amount remaining to be learned. To produce a long-presentation condition, we increased *R* for each item in the short-presentation condition by an amount that is a constant proportion (x) of the amount of possible increase in *R*: $x(1 - R)$. In the present example, $x = .25$, and so, R_1 in the short-presentation condition (.04) increases by .24, that is, $.25 * (1 - .04)$, to produce .28 for the long-presentation condition and so on for each item. New probabilities of inclusion and exclusion were computed from the proportionately increased *R*s and unchanged *A*s. The effects of a proportionate increase in *R* are shown by comparing estimates from the short-presentation condition with those from the long-presentation condition in Table 2. Although *R* and *A* are almost perfectly correlated, a proportionate increase in *R* does not change the estimates of *A*—that is, correlation does not produce an artifactual dissociation when a manipulation produces a proportionate increase in *R* for each item.[2]

So, as argued by Curran and Hintzman (1995), positive correlations between processes at the item level can produce an underestimation of estimated automatic processes (*A*), but the underestimation is negligible, even when the correlation between *R* and *A* is extremely high. Of more importance, the underestimation is not differential across conditions, producing a proportional increase in *R*. Of course, it is possible that learning is not proportional, but rather increases linearly; if so, the underestimation in *A* would be differential across conditions (see below). But would the differential underestimation be enough to produce artifactual dissociations of the magnitude reported by Curran and

[2] To understand why proportional increase of *R* does not influence estimated *A*, consider the equation for estimating *A*: Exclusion/$(1 - R) = A(1 - R)/(1 - R)$. Proportional increase in *R* results in a new value of *R* equal to $R + x(1 - R)$, so that the new value of $(1 - R)$ is $(1 - x)(1 - R)$. So, proportional increase of *R* multiplies both the numerator and the denominator by $(1 - x)$ and has no affect on the estimate of *A*.

Hintzman (1995)? To answer this question, one needs a method for calculating the size of the underestimation. We provide such a method below in the context of a closer examination of Curran and Hintzman's claims concerning the effects of correlations on estimates of R and A.

Estimation Bias Caused by Correlated Processes: Curran and Hintzman (1995) as a Case Study

Curran and Hintzman (1995) manipulated presentation duration in their attempts to produce artifactual dissociations. An artifactual dissociation is said to occur if A decreases because of an increase in R, rather than because of an actual decrease in the contribution of automatic processes; such a finding would represent a violation of the independence assumption. Comparisons of direct and indirect memory tests (e.g., Greene, 1986; Jacoby & Dallas, 1981) lead one to expect that in the range across which presentation duration was varied (1 s vs. 10 s), there should have been an effect on controlled processes but not on automatic processes. That is, the predicted process dissociation for varying study duration is the same as that obtained by Jacoby et al. (1993) when attention was manipulated. However, Curran and Hintzman found that increasing study time produced the expected increase in R but "paradoxically" decreased A. They argued that the decrease in A was an artifact of the estimation procedure produced by a correlation between processes at the item level.

Could process correlations bias estimates enough to result in artifactual dissociations? Curran and Hintzman (1995) argued that they could because of a sampling bias inherent in the process-dissociation procedure.[3] We agree that a positive correlation between R and A can result in an underestimation of automatic processes. However, instead of describing the underestimation of A as reflecting a sampling bias due to conditionalization, we describe the underestimation as reflecting covariance. An important advantage of describing the effects of correlation in terms of covariance is that it allows one to calculate the amount of underestimation that could be produced by correlation. Knowing the maximum amount of underestimation is important because it tells one how problematic correlations are for interpreting estimates gained from the process-dissociation procedure. In fact, as we showed above and elaborate below, the magnitude of underestimation is generally not sufficient to produce artifactual dissociations. Another advantage of emphasizing covariance is that it allows one to make contact with related issues in the literature—issues such as Simpson's Paradox (Hintzman, 1980) and the statistical relation between different tests of memory (Flexser & Tulving, 1978). As described in the next section, claims about estimation bias caused by a violation of stochastic independence should be evaluated in the context of arguments about the impossibility of assessing stochastic independence.

The major advantage of emphasizing covariance is that it focuses attention on the intersection between the two processes (i.e., the joint probability of recollection and automatic processes: RA). The effect of positive correlation is to increase the intersection by a quantity that is equal to the covariance, and it is this increase that provides a way of quantifying the effect of correlation on estimates of R and A. Focusing on effects of conditionalization, as Curran and Hintzman (1995) have, might reflect the particular algebraic form that they chose to write the independence equations. Writing the equation for the inclusion condition as $R + A(1 - R)$ suggests thinking in terms of conditional probabilities: Because of a sampling bias produced by positive correlation, the joint probability of A and recollection failure—that is, $A(1 - R)$—is smaller than it would be if A and R were uncorrelated. However, writing the same equation as $R + A - RA$ changes the focus to the intersection (RA) and to the effect correlation has on the size of the intersection: If R and A are positively correlated, the joint probability of automatic processes and recollection success will be larger than estimated by RA. Of course, if correlation influences RA, it must also influence $A(1 - R)$. Indeed, the problems of bias because of effects on conditional probability and bias because of effects on covariance are one and the same.

Assessing Stochastic Independence: Simpson's Paradox

Curran and Hintzman (1995) hold that a lack of stochastic independence, directly evidenced by significant correlation between R and A at the item level, invalidates the process-dissociation procedure. A standard definition of stochastic independence is: $P(A \cap B) = P(A)P(B)$. Rejecting stochastic independence might be thought to be a simple matter of showing that the preceding equality does not hold. However, Hintzman (1980) argued that correlation, as well as other measures relying on contingency tables, does not provide a means of testing stochastic independence because of the possibility of hidden covariates. When two or more contingency tables are collapsed into one, the resulting table may show a relationship between variables that differs from that shown by any of the original tables (Simpson's paradox). Although underlying processes are correlated, results gained by collapsing across contingency tables can show zero correlation. Also, aggregated results can show a nonzero correlation although underlying processes are stochastically

[3] To illustrate the effects of sampling bias, Curran and Hintzman (1995) presented a hypothetical bivariate distribution of values of R and A and drew a vertical line over the R axis to depict a criterion on R; items to the right of the line were said to be recollected, whereas those to the left were said to be not recollected (see their Figure 1). However, to represent use of the process-dissociation procedure, one would have to add probability values to their axes (the axes were not labeled in their figure). Once their axes are labeled with probabilities, it is clear that a criterion on R is inappropriate. For example, consider an item whose value for both R and A is .80. Although plotted in their Figure 1 as always being to the right of the criterion and therefore recollected, on 20% of the occasions the item will be on the left of the criterion and therefore not recollected. Moreover, on those occasions when the item is not recollected, its value of A (.80 in this case) will contribute to the estimate of A. Because the process-dissociation procedure deals with probabilities, no fixed criterion can be drawn to separate items into the deterministic states of recollected and not recollected.

independent. For example, Hintzman (1980, Table 4) demonstrated that goodness of encoding as a hidden covariate can produce a correlation between recognition and recall performance in results summed across contingency tables for good and poor encoding, each of which shows independence. A similar demonstration was provided for the effects of trace strength as a hidden covariate (Hintzman, 1980, Table 2).

It is futile to attempt to escape the interpretive problems produced by Simpson's paradox. This is true because one must collapse across something (items or subjects) to analyze results. Measuring the abilities that are of interest (e.g., recall and recognition) separately for each subject avoids the risk that subject differences act as a hidden covariate but leaves the possibility that aggregating across items allows differences among items to be a hidden covariate. Covariance produced by such item differences cannot be truly assessed. Collapsing across subjects to measure abilities separately for each item allows subject differences to act as a hidden covariate and can produce a false correlation—a correlation that is not descriptive of that between recognition and recall at the level of items for individual subjects. That is, just as illustrated by Hintzman (1980) for effects of item differences as a hidden covariate, subject differences as a hidden covariate can produce the result of a positive correlation at the item level collapsed across subjects, although the two abilities of interest are independent at the Subject × Item level. Unfortunately, correlation at the Subject × Item level cannot be measured because at that level one has only a single observation of a particular subject's performance on a particular item.

As shown in Table 2, it is the aggregation of results across items that creates the possibility that a correlation at the Item × Subject level can bias process estimates. If R and A could be measured separately for each item within each subject, then correlation could not bias our estimates—just as the correlation between height and weight in the overall population cannot bias the measurement of those dimensions when they are obtained separately for a particular individual. Unfortunately, the "true" values in Table 2 are impossible to measure, because one must aggregate across something (subjects or items) to compute any estimates of R and A. Doing so opens the possibility that A estimated using aggregated data will not reflect the true A because of a correlation at the level over which responses are aggregated.

Curran and Hintzman (1995) reported the correlation between R and A at the item level (aggregating results across subjects and conditions) and at the subject level (aggregating across items and conditions). They ignored the possibility of Simpson's paradox by taking those correlations as "direct evidence" against the stochastic independence of R and A. Perhaps the discrepancy between the correlations reported by Curran and Hintzman and the consistency of dissociations shown in Table 1 is best resolved by dismissing their correlations as false because of Simpson's paradox. That is, at the Item × Subject level, R and A might be stochastically independent, and the finding of significant correlations at higher levels might reflect the effects of aggregation.

Because of Simpson's paradox, it is impossible to prove or disprove stochastic independence at the Item × Subject level. For the same reason, the true correlation between R and A at that level cannot be measured. However, the process-dissociation procedure is not meant to prove stochastic independence. Rather, violations of stochastic independence are of interest only to the extent that they bias estimates in a way that is differential across conditions. For our discussion about the magnitude of estimation bias caused by correlated processes, we accept Curran and Hintzman's (1995) assumption that the unmeasurable correlation at the Item × Subject level is directly evidenced by the correlation at the item level, aggregated across subjects. As we show, estimation bias caused by correlated processes would not produce Curran and Hintzman's results even if the unmeasurable correlation was higher than estimated from aggregated results.

Effects of Nonzero Covariance

When introducing the process-dissociation procedure, Jacoby (1991) noted that the equations used were based on the assumption that covariance between automatic and controlled influences was zero. Curran and Hintzman's (1995) work was useful in leading us to examine the effects of nonzero covariance. It is the covariance between R and A that is responsible for the sampling bias they describe. If R and A are correlated across items within subjects, aggregating across items to estimate R and A will result in an intersection between the two processes that is larger than if covariance was zero, and consequently, A will be underestimated. Describing the effects of correlation in terms of covariance allows one to derive an equation that can be used to calculate the magnitude of underestimation. That equation, described in detail in the Appendix, follows directly from the definition of covariance. Briefly, combining the definition of covariance in terms of correlation (i.e., $r * SD_R * SD_A$) with the equation for the error in estimation of A produced by covariance, that is, $cov/(1 - R)$ (see above), yields:

$$\text{True } A - \text{Estimated } A = cov/(1 - R)$$

$$= r * SD_R * SD_A/(1 - R) \quad (1)$$

In the next section, we illustrate the effect of covariance in an example involving correlated item differences in pairs of biased coins. This example is meant to give the reader an intuitive grasp of how, although correlated, R and A can be functionally independent.

Correlated but Functionally Independent: A Coin Example

Suppose you are asked to make a series of bets on whether tossing a pair of coins will yield at least one head. Call this the inclusion bet. Assume there are two pairs of coins (R_1 and A_1, R_2 and A_2), but you do not know from which pair the two tosses will come on any one bet. You do know that on average the probability of a head is .5 for the first coin tossed (R_1 or R_2) and .5 for the second coin tossed (A_1 or A_2).

Consequently, you might reason that the probability of obtaining at least one head is .75 [$R + A(1 - R) = R + A - AR = .75$]. However, you are further told that the coins are biased and that the probability of a head for the two coins is perfectly correlated over the pairs. For one pair, the probability of a head is .8 for both coin R and coin A, whereas for the other pair, the probability of a head is .2 for each coin.

Despite the correlation, the coins in a pair are functionally independent because the probability of a head on coin A in no way depends on whether a head or a tail is obtained on coin R. However, because of the correlation, the probability of getting two heads, the intersection (RA), is larger than it would be if the coins were uncorrelated across pairs (zero covariance). The probability of obtaining a head on both coins in Pair 1 is $.8 * .8 = .64$, whereas that probability for Pair 2 is $.2 * .2 = .04$. Averaging across the two pairs, the probability of the intersection is .34 (.68/2), which would compare with the intersection of .25 if covariance were zero. That is, covariance has increased the probability of the intersection by .09.

What effect does this covariance have on the probability of winning the inclusion bet? The effect is to decrease the probability of winning the inclusion bet. If one aggregates across the two pairs of coins, the positive correlation between coins in a pair will result in an underestimation of the probability of obtaining at least one head, as compared with the case for which R and A are uncorrelated. This is true because coin A will contribute to the probability of obtaining at least one head only when R fails (i.e., its being flipped will matter only when R returns a tail): R_1 will not return a head less often than will R_2 and so will produce the "sampling bias" identified by Curran and Hintzman (1995). In contrast with the probability of .75 that would be obtained if the coins in a pair were uncorrelated, the probability obtained by averaging across pairs is only .66. (It is the correlation that is important for this underestimation, not the fact that the coins are biased. To see this, do the same computations with the following pairs: .8 and .8; .8 and .2; .2 and .2; and .2 and .8.) Note that the difference produced by covariance ($.75 - .66$) is .09.

The same amount of underestimation occurs if the bet is changed to the probability of obtaining a head on coin A and a tail on coin R (the exclusion bet): $A(1 - R) = A - RA$. If coins in a pair are uncorrelated, the probability of a tail on coin R and a head on coin A is .25. However, because of covariance, this probability averaged across pairs drops to .16. The underestimation produced by correlation within pairs for the inclusion and exclusion bets is the same (.09) because computing the probability of a win involves subtracting the intersection, which is larger than it would be if correlation were zero. The increase in the magnitude of the intersection produced by the correlation within pairs is equal to the covariance between R and A. Covariance is defined as $r * SD_R * SD_A$. For our example, correlation is equal to 1.0, and the standard deviation for both R and A is .3; therefore, covariance is equal to .09.

Suppose we provided the probabilities of winning the inclusion and exclusion bets and asked you to estimate the average probability of a head on the R and A coins. R, estimated as the difference between the probability of winning the inclusion (.66) and exclusion (.16) bets, would be .50, just as it would be if the coins in a pair were uncorrelated. That is, correlation between R and A does not influence the estimate of the average R. However, the correlation between R and A will produce an underestimate of A. If R and A were uncorrelated, Exclusion/($1 - R$) would correctly estimate the average A as being .50. Because of the correlation, however, A is underestimated as being .32, that is, Exclusion/($1 - R$) = .16/($1 - .5$) = .32. The magnitude of underestimation is .18, which (from Equation 1) is equal to the covariance of R and A divided by ($1 - R$), that is, .09/($1 - .5$) = .18.

The amount of underestimation is large because of the perfect correlation and high standard deviations that we chose for illustrative purposes. In most instances, as shown below, the difference between the true and estimated A is much smaller. More important for assessing the possibility of artifactual dissociations is whether the underestimation of A differs across conditions that differ in R. To answer that question, one must make an assumption about the manner in which R increases across conditions, as we did above in Table 2.

Were Curran and Hintzman's (1995) Artifactual Dissociations Caused by Correlation?

Using the inclusion–exclusion procedure, Curran and Hintzman (1995) found artifactual dissociations—increases in R accompanied by decreases in A—in their Experiments 1 and 5. They interpreted those dissociations as meaning that because of item-level correlations, A is progressively underestimated as R increases. Curran and Hintzman did not explicitly state an assumption about the form of increase in R, but their Figure 1 depicts R as increasing by a constant across conditions. Adding a constant leaves unchanged the standard deviation of both R and A, as well as the correlation between the two. Consequently, increasing R by a constant (i.e., a linear increase in R) results in a progressive underestimation of A because the only change in the equation describing the amount of underestimation (Equation 1) is in the denominator (i.e., $1 - R$). Of course, the increase in R cannot be linear for high values of R, because R cannot exceed 1.0, but the increase could be linear from low to intermediate values.

Assuming that the increase in R is linear across conditions, can the increased underestimation of A be big enough to account for the artifactual dissociation found by Curran and Hintzman (1995) in their Experiment 1? We used Equation 1, along with the measures of correlation and standard deviation obtained by Curran and Hintzman, to calculate the effects of correlated item differences. Pooling across the 1-s and 10-s conditions (see their Table 4), the standard deviations are .19 for R and .145 for A. The correlation between R and A was .26 (their Table 5). For the 1-s condition, R was .17 (their Table 3). With Equation 1, the correlation between R and A would result in A being underestimated by .0086 in the 1-s condition, that is, $.26 * .19 * .145/(1 - .17)$. For the 10-s condition, the numerator would be the same, but the denominator would reflect the higher probability of recollection (.32) for that condition. The underestimation of A for the 10-s condition would be .0105. The difference in underestimation for the 10-s and 1-s conditions, then, would be .0019. That is, the absolute underestimation of A produced by correlated item differences is tiny, and the differential underestimation for conditions is even smaller—much smaller than the significant .04 difference obtained by Curran and Hintzman.

The artifactual dissociation obtained by Curran and Hintzman (1995) in their Experiment 5 was also much larger than could be produced by differential underestimation of A resulting from covariance. For that experiment, R was .33 for the 1-s condition and .47 for the 10-s condition. The correlation between R and A was .55, and standard deviations for R and A were .31 and .24, respectively. Because of the higher correlation and standard deviations, the absolute underestimation of A would have been much larger in their Experiment 5 than in their Experiment 1. The underestimation of A would be .061 for the 1-s condition and .077 for the 10-s condition. The .016 difference in the underestimation of A produced by correlated item differences is much smaller than the significant .12 difference in A obtained by Curran and Hintzman. Thus, the effect on A obtained by Curran and Hintzman is much larger than what one could expect from covariance alone.

Our calculations rest on the assumption that the unmeasurable correlation at the item level for each subject is the same as the correlation gained by aggregating across subjects. It is unlikely that a larger correlation within subjects is responsible for the discrepancy between the magnitude of observed dissociations and calculated effects of covariance. For Curran and Hintzman's (1995) Experiment 5, even a correlation of 1.0 between R and A would not produce the magnitude of the paradoxical dissociation that they observed. Given the magnitude of the effect on R in that experiment, the standard deviations of R and A, and the assumption of a linear increase in R, a correlation of 1.0 between R and A would result in an increase of .03 in underestimation of A over levels of R, which is much smaller than the observed difference of .12.

Furthermore, Curran and Hintzman's (1995) item-based estimates combine with their subject-based estimates to reject the hypothesis that correlations caused artifactual dissociations. The correlation between R and A for item-based estimates, which is described above, was positive, whereas that for subject-based estimates was negative. Extending Curran and Hintzman's arguments for the effect of correlation at the item level on subject-based means, the negative correlation between R and A at the subject level should have resulted in an overestimation of A on item-based means. Just as a positive correlation produces an underestimation of A, a negative correlation produces an overestimation of A. Because the direction of correlation and, thus, the direction of bias was opposite at the item and subject levels (see their Table 5), one must predict that dissociations caused by correlation were opposite for item- and subject-based estimates. However, for each of Curran and Hintzman's experiments showing an artifactual dissociation, the pattern of effects for estimates was the same for item- and subject-based means. That is, rather than opposite effects, the dissociations were the same regardless of whether the correlation was positive or negative.

Reasons for Artifactual Dissociations

As shown above, correlated item differences would be extremely unlikely to produce differential underestimation of A of a magnitude necessary to produce the artifactual dissociations obtained by Curran and Hintzman (1995). Also, as noted earlier, the results in Table 1 contradict the inverse relation between ΔR and ΔA that would result if estimation bias were caused by a linear increase in R. What did produce Curran and Hintzman's artifactual dissociations? We think it likely they were produced by violating the boundary conditions set by Jacoby et al. (1993) for use of the process-dissociation procedure.

Zeros in Exclusion Performance

Jacoby et al. (1993) illustrated the effect of zeros in exclusion performance on estimates of A. Their Experiment 1A differed from their Experiment 1B, described earlier, only in the baseline completion rate (i.e., the probability of completing word stems with target words that were not studied earlier). For Experiment 1A, the baseline was .14, whereas for Experiment 1B it was .35. The results of Experiment 1A showed a large effect of full versus divided attention on estimates of recollection (.20 vs. .00) and a small effect in the opposite direction on estimates of automatic processes (.21 vs. .27). However, some subjects in the full-attention condition had no errors on the exclusion test. By the equation used to estimate automatic processes, exclusion/$(1 - R)$, a score of zero on the exclusion test results in an estimate of zero for automatic processes. The probability of a zero in exclusion was larger for the full- than for the divided-attention condition, which is not surprising because recollection was higher in that condition.

When estimates from Experiment 1A were recomputed without zero scores, A was identical for the full- and divided-attention conditions (.27). Experiment 1B used stems that had a higher baseline completion rate to avoid zeros and to thereby avoid any possible bias that might result from their removal. Results of Experiment 1B showed that full versus divided attention had a large effect on recollection (.25 vs. .00) but essentially no effect on estimated A (.47 vs. .46). That is, results obtained by using stems with a higher baseline to avoid zeros replicated the results produced by removing zeros.

Curran and Hintzman's (1995) Experiments 1 and 4 replicate those reported by Jacoby et al. (1993). In Curran and Hintzman's Experiment 1, the baseline completion rate was .12, and A computed with the zeros included showed that A decreased significantly from the short-duration to the long-duration condition (.16 vs. .12), but when zeros were removed, estimates were near identical for the two conditions (.18 vs. .17). In their Experiment 4, they used stems with a higher baseline completion rate (.30) to reduce the likelihood of zeros for the exclusion test. Results from their Experiment 4 showed that A was nearly identical for the short-duration and long-duration conditions (.36 vs. .35).

When zeros were removed, the small difference between conditions was reversed (.39 vs. .40).

Despite the parallel between their results and results reported by Jacoby et al. (1993), Curran and Hintzman (1995) argued against removing zeros. They suggested that doing so forces the selection of subjects whose recollection is poor or who did not correctly follow the inclusion–exclusion instructions. However, if the invariance in *A* found with zeros removed was because of a subject-selection artifact, that invariance should not be found when zeros are avoided by increasing baseline performance. Using a manipulation of study time identical to that used by Curran and Hintzman, we have replicated their finding that avoiding floor effects eliminates the artifactual dissociation (Jacoby, 1996b). To avoid zeros in exclusion performance, we used items with an even higher base rate than those used by Curran and Hintzman in their Experiment 4 and increased the length of lists to allow more opportunity for errors in the exclusion condition. For our experiment, the base rate was .40. The only other differences in procedure between our experiment and those of Curran and Hintzman were that our test instructions encouraged direct retrieval (see below) and that we required subjects to pronounce words aloud during study.

Results of our experiment replicated those of Curran and Hintzman's (1995) Experiment 4 by showing no effect of presentation duration on estimated automatic influences; estimates of *A* for the 1-s and 10-s conditions were .58 and .59, respectively. Estimates of *R* were .22 and .44—a difference in recollection that is larger than that found by Curran and Hintzman. When zeros are avoided by increasing baseline, results agree with those produced by removing zeros. We cannot offer a general rule for dealing with zeros beyond saying that they should be avoided as floor effects should be avoided in other domains.

Misunderstanding of Exclusion Instructions?

Curran and Hintzman (1995), along with others (Graf & Komatsu, 1994), have suggested that subjects' misunderstanding of exclusion instructions may be a factor in producing dissociations between estimates of controlled and automatic processes. Although subjects' understanding of instructions is obviously an important issue, equally important is how subjects comply with instructions; that is, whether subjects use a generate–recognize strategy for recall, as opposed to the direct-retrieval strategy that is a prerequisite for use of the independence equations. We first consider the general issue of subjects' understanding of instructions. Next, we discuss the importance of instructions in producing a retrieval strategy necessary for functional independence and findings of invariance.

Curran and Hintzman (1995) dismissed the results of their Experiment 4 on the grounds that subjects in that experiment failed to understand exclusion instructions. Their evidence for failure in understanding exclusion instructions was that a number of subjects produced old items with a probability of .50 or above even in the long-presentation condition for which *R* was high. However, a high probability of mistakenly producing old words does not necessarily reflect a failure of instructions but could be due to a high base rate idiosyncratic to the items used for a particular combination of condition and subject. Understanding of instructions is better measured as the difference between performance on inclusion and exclusion tests. For example, if probabilities were .95 and .55 for the inclusion and exclusion tests, one would conclude that the subject understood instructions despite the high probability for the exclusion test. In more recent experiments (e.g., Jacoby, Jennings, & Hay, 1996), we have used an explicit check on subjects' understanding of exclusion instructions by varying the spacing between presentation of an item and its exclusion test. If people successfully exclude items that are tested immediately after being presented for study, subsequent exclusion errors at wider spacings cannot be attributed to misunderstanding of instructions. Dissociations found using this new procedure are the same as those in our earlier experiments.

Curran and Hintzman (1995) implied that our previous findings of invariance in *A* (some of which are shown in Table 1) reflect a delicate balance between artifactual dissociations and subjects not understanding instructions. Such a delicate balance seems implausible, however, because failures to understand instructions would have to become more likely in conditions with high *R* to offset the larger underestimation of *A* supposedly caused by correlated processes. Against this possibility, conditions with high *R* are almost universally accompanied by more accurate exclusion performance, which requires that instructions were understood.

Effects of Instructions on Recall Strategy: Direct Retrieval Versus Generate–Recognize

Our work using the process-dissociation procedure has been based on the assumption that memory performance can reflect the independent contributions of automatic and controlled processes, a model we have referred to as *direct retrieval.* A direct-retrieval model is consistent with demonstrations of encoding specificity that have been used to argue against generate–recognize accounts of cued recall (see Tulving & Thomson, 1973). In our version of this model, retrieval cues are combined with information about a prior context in an attempt to recollect specific target items. Automatic retrieval of target items may also occur but is assumed to operate independently of intentional retrieval. Support for the model is provided by showing that variables traditionally associated with the concept of control have large effects on estimated conscious recollection but few or no effects on estimated automatic processes. Variables producing this pattern include divided versus full attention during study (Jacoby et al., 1993; see Table 1), fast versus slow responding at test (Toth, 1996; Yonelinas & Jacoby, 1994), and aging (Jacoby, Jennings, et al., 1996; see Jacoby, Yonelinas, & Jennings, 1996, for a review).

Comparisons between direct and indirect memory tests also provide converging evidence for our independence model. If indirect memory tests provided process-pure measures of automatic influences and if automatic and controlled influences are independent, one would expect estimated *A* to converge with performance on indirect memory tests. Under conditions that are least likely to result in conscious contamination of indirect memory tests, there is a close match between estimated *A* and indirect memory test performance (Jacoby, Yonelinas, et al., 1996; Reingold & Goshen-Gottstein, 1996; Toth et al., 1994). If *A* were badly underestimated because of correlated processes, such a close match should be impossible to obtain.

We believe that the results cited above establish that direct retrieval is a tenable model for cued recall. However, direct retrieval is not the only way cued recall can be accomplished. Jacoby and Hollingshead (1990; see also Bahrick, 1979) suggested that subjects may often perform cued-recall tests by automatically generating words in response to retrieval cues and then subjecting those words to a consciously controlled recognition check for their prior presentation. For this model, controlled and automatic processes cannot be completely independent because recognition follows and thus is dependent on successful generation. Thus, the choice between direct retrieval and generate–recognize as models of cued recall is the same as that between an independent and redundancy relation between controlled and automatic uses of memory (see Jacoby, Yonelinas, et al., 1996; Jones, 1987).

Our goal in using the process-dissociation procedure has been to arrange conditions that encourage direct retrieval and, thereby, meet the independence assumption. We have been interested in direct retrieval because it is this strategy that allows one to examine the role of intent in bringing an item to mind. Nevertheless, generate–recognize is also a reasonable strategy for cued recall, and its use may underlie "artifactual dissociations" such as those reported by Curran and Hintzman (1995). Of course, such dissociations are not truly artifactual but, rather, reflect the inappropriate application of independence (direct retrieval) equations to data for which a redundancy relation holds because subjects rely on a generate–recognize strategy (or a mixture of strategies).

What factors influence subjects' choice of strategy, and how can one determine which strategy is being used? Most important, perhaps, are the instructions given at test. Critics of the process-dissociation procedure (e.g., Graf & Komatsu, 1994) have focused on subjects' understanding of the exclusion instructions; however, we believe the inclusion instructions are as important as the exclusion instructions for eliciting a direct-retrieval strategy for recall and for thus finding invariance in *A* across levels of *R* to support the independence assumption. We have instructed subjects to use stems as cues for earlier studied words and to either use or withhold recalled words dependent on whether inclusion or exclusion instructions were given.

Suppose our instructions were changed such that subjects did not use direct retrieval for the inclusion test but simply completed stems with the first words that came to mind, just as they would for an indirect test of memory. Further, for the exclusion test, suppose they rejected generated completions that were recognized as old. The result of this generate–recognize strategy would be that inclusion performance would not be influenced by study duration (e.g., Greene, 1986; Jacoby & Dallas, 1981). Moreover, any improvement in exclusion performance—produced, for example, by increasing study duration—would result in an artifactual dissociation because improved exclusion performance in combination with unchanged inclusion performance necessarily results in an increase in *R* and a decrease in *A*. Such a dissociation would reflect the process dependency between generation and recognition.

We have done a series of experiments to show that instructions that encourage a generate–recognize strategy produce the pattern of results described above, whereas direct-retrieval instructions produce effects on *R* but leave *A* invariant across manipulations of full versus divided attention and short versus long study duration (Jacoby, 1996b). As noted by Curran and Hintzman (1995), their artifactual dissociations may have occurred because subjects used a generate–recognize strategy. Although the instructions for their Experiment 5 were apparently the same as for their Experiment 4, they "worked with" subjects in Experiment 5 during practice in an attempt to ensure that subjects understood instructions. The change in procedure may have encouraged subjects to use a generate–recognize strategy.

Results of Curran and Hintzman's (1995) Experiment 5 show the signature of a generate–recognize strategy. Although "working with" subjects did increase the accuracy of exclusion performance in Experiment 5 as compared with Experiment 4 (particularly for the 10-s study condition), the results also changed for the inclusion test condition. The substantial advantage in inclusion performance for the 10-s over the 1-s condition observed in Experiment 4 (.60 vs. .49) largely disappeared in Experiment 5 (.59 vs. .55). This reduced effect of study duration on inclusion test performance would be expected if the change in procedure resulted in subjects' adopting a generate–recognize strategy and, thereby, largely treating the inclusion test as an indirect memory test.

Another signature of a generate–recognize strategy—differences in baseline performance—was also obtained in Curran and Hintzman's (1995) Experiment 5. Baseline was significantly lower for the exclusion test than for the inclusion test. There are two ways that use of a generate–recognize strategy could produce this difference. First, falsely recognized words might be mistakenly rejected on the exclusion test, whereas false recognition would have no influence on inclusion test performance (Jacoby, 1991). Second, greater reliance on recollection might effectively reduce the amount of time available for generating a completion for the exclusion test, as compared with the inclusion test. In contrast to results of their Experiment 5, Curran and Hintzman did not find a difference between baselines in their Experiment 4, which did not show an artifactual dissociation. We also did not find a significant difference in baseline in our experiment that replicated the results of their Experiment 4. A significant reduction in baseline for the exclusion test, as compared with the inclusion test, can be treated as a strong indicator of subjects' reliance on a generate–recognize strategy (Jacoby, 1996b) and has the effect of producing an overestimate of recollection.

Our claim is not that invariance will always be found. Rather, the finding of invariance relies on the retrieval strategy used by subjects. Our direct-retrieval instructions were designed to discourage subjects from using recognition memory as a basis for rejecting old words for the exclusion test. If subjects use a generate–recognize strategy, results will not show invariance in *A* computed using the independence assumption. Rather, in that case, a redundancy assumption of the sort made by Jacoby and Hollingshead (1990) is necessary to dissociate processes.

A pessimistic view is that it is impossible to control subjects' retrieval strategies in a task as complex as cued recall using word stems (see Curran & Hintzman, 1995, p. 544). However, gaining such control is a prerequisite to separating automatic and consciously controlled processes within a task, which is important for both theoretical and applied purposes (e.g., Jacoby, Jennings, et al., 1996). We see results such as those in Table 1 as a source of optimism that we have gained a substantial degree of control over subjects' strategies. The alternative to such optimism is to believe that estimation bias caused by subjects' partial reliance on a generate–recognize strategy is delicately balanced with other sources of estimation bias (not understanding exclusion instructions and bias caused by process correlations) to consistently create an "illusion" of invariance in *A*. A delicate balance of this sort seems particularly unlikely because estimation bias caused by partial reliance on a generate–recognize strategy should increase in magnitude when recognition is made easier (increased *R*)—adding to, rather than offsetting, supposed estimation bias caused by correlated processes.

Conclusions

The process-dissociation procedure was developed to address a widely recognized problem in memory research: the fact that test performance—and most performance in the "real world"—reflects the operation of more than one kind of memory. Since its introduction, the procedure has generated considerable controversy, much of it centered around the assumptions underlying use of the procedure. However, it is important to recognize that many of these assumptions—such as the functional relation between processes—are not specific to the process-dissociation procedure but will have to be addressed by any approach that assumes two (or more) mnemonic processes or systems. Indeed, although they are among our most vehement critics, researchers using the task-dissociation (implicit–explicit) approach face many of the same problems that we have encountered—problems such as the functional relationship among processes (or systems), process correlations, and the fact that functional relationships may change as a function of task strategy. Few investigations based on the task-dissociation approach have addressed these issues, but it seems clear that they cannot be avoided forever (see the response by Toth, Reingold, & Jacoby, 1995, to Graf & Komatsu, 1994).

Curran and Hintzman (1995) identified the important problem that correlations between processes might result in an underestimation of automatic influences of memory (*A*). One can only say *might:* The magnitude of any measurement problem produced by correlation cannot be truly assessed because one must aggregate across something (items or subjects) to estimate *R* and *A*. We agree that correlations could produce an estimation bias, but we have shown that such biases will likely be trivially small and not differential across conditions by an amount necessary to produce artifactual dissociations. The dissociations observed by Curran and Hintzman were likely produced by violations of boundary conditions for using the process-dissociation procedure rather than by correlated item differences. Floor effects can result in artifactual dissociations as can subjects' reliance on a generate–recognize strategy, which produces a redundancy relation between *R* and *A*. The process dependency that characterizes a redundancy relationship is a causal one (recognition must be preceded by generation) and does not simply reflect an estimation bias caused by correlated processes.

Let us end by noting that use of the inclusion–exclusion procedure to investigate controlled and automatic processes does give rise to complexities. One has to worry about floor and ceiling effects. Instructions are also a cause for worry because the relationship between *R* and *A* is partly determined by a subject's retrieval strategy. This being true is not reason for abandoning the approach but does encourage the refinement of procedures and the development of alternative means of implementing the approach. The particulars of the inclusion–exclusion procedure are less important to us than is the rationale underlying the process-dissociation approach, along with its goal of separating automatic and controlled processes within a task. Manipulating the relation between a habit created through training and an event whose presentation is to be recollected at test serves as an alternative means of creating *in-concert* and *in-opposition conditions* of the sort required by the process-dissociation approach (e.g., Hay & Jacoby, 1996). Use of this alternative method is, in some ways, less problematic and provides results that agree with those gained from inclusion–exclusion tests. Procedures are necessarily reliant on underlying assumptions and must be used with care to satisfy boundary conditions. However, concern for the unobservable correlation between processes at the Subject × Item level should not blind one to the regularity of results observable in Table 1.

The process-dissociation procedure led us to observe those regularities, and confirms our belief that the procedure provides a useful way of separating the influences of automatic and consciously controlled memory processes. It is true that there are boundaries that limit the use of the procedure, but those boundaries are currently being extended, and we hope that process of extension will continue for some time.

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Appendix

Effects of Correlation

We describe the derivation of Equation 1. First, we need to relate correlation to covariance. This is easily done because correlation is covariance uncorrected for the magnitude of the standard deviations of the correlated variables. That is, as can be found in most introductory statistics books, covariance is defined in terms of the correlation and the standard deviations of variables of interest (R and A):

$$cov = r * SD_R * SD_A \tag{A1}$$

Next, we need some means of relating covariance to the equations used to estimate R and A. This can be done by noting the computational formula for covariance $[(\Sigma R_i * A_i)/N] - RA$. Suppose we knew R and A for each item for each subject and wanted to calculate the effect of correlation between R and A at the item level on the intersection RA estimated by aggregating across items. The true intersection of R and A could be calculated as the mean of the products of R and A for each item, $(\Sigma R_i * A_i)/N$. A positive correlation between R and A at the level of items would result in the intersection computed from the means aggregated across items (RA) being larger than the true intersection by an amount that is equal to covariance. By the computational formula for covariance, the difference between the intersection estimated from the means and the true intersection computed using individual items is equal to covariance (cov); therefore, the true intersection is equal to $RA + cov$. Combining this definition of covariance with equations for the inclusion and exclusion conditions yields a more general form of those equations:

$$\text{Inclusion} = R + A - (RA + cov)$$
$$= R + A - [(\Sigma R_i * A_i)/N] \tag{A2}$$

$$\text{Exclusion} = A - (RA + cov) = A - [(\Sigma R_i * A_i)/N] \tag{A3}$$

When covariance is zero, these equations are the same as written earlier because $(\Sigma R_i * A_i)/N$ equals RA. However, when one aggregates across items to gain estimates and the covariance of R and A is positive, as will be the case if there is a positive correlation, the intersection, $(\Sigma R_i * A_i)/N$, will be larger than RA. This will have no influence on the estimate of R because the increase is the same for the exclusion test as for the inclusion test and because recollection is measured as the difference between performance on the two tests.

However, positive covariance will result in the underestimation of A. Estimating A as Exclusion/$(1 - R)$ will now yield $A - (RA + cov)/(1 - R)$ rather than $A - RA/(1 - R)$. These two equations differ by $-cov/(1 - R)$. Thus, the estimate of A will be smaller by $cov/(1 - R)$ than it would be if the correlation of R and A were zero.

Combining the definition of covariance in terms of correlation with the equation for the error in estimation of A produced by covariance yields:

$$\text{True } A - \text{Estimated } A$$
$$= cov/(1 - R) = r * SD_R * SD_A/(1 - R) \tag{A4}$$

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