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Criteria for unconscious cognition: Three types of dissociation

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To demonstrate unconscious cognition, researchers commonly compare a direct measure (*D*) of awareness for a critical stimulus with an indirect measure (*I*) showing that the stimulus was cognitively processed at all. We discuss and empirically demonstrate three types of dissociation with distinct appearances in *D*–*I* plots, in which direct and indirect effects are plotted against each other in a shared effect size metric. *Simple dissociations* between *D* and *I* occur when *I* has some nonzero value and *D* is at chance level; the traditional requirement of zero awareness is necessary for this criterion only. *Sensitivity dissociations* only require that *I* be larger than *D*; *double dissociations* occur when some experimental manipulation has opposite effects on *I* and *D*. We show that double dissociations require much weaker measurement assumptions than do other criteria. Several alternative approaches can be considered special cases of our framework.

[what do you see?/
nothing, absolutely nothing]
—Paul Auster, "Hide and Seek" (in Auster, 1997)

The traditional way of establishing unconscious perception has been to demonstrate that awareness of some critical stimulus is absent, even though the same stimulus affects behavior (Reingold & Merikle, 1988). To show this, two types of measurement are needed. First, the degree to which the critical stimulus reaches conscious awareness must be assessed, for example by asking observers whether or not they are aware of it or by testing their ability to identify or detect it. This is called a *direct measure* (*D*) of processing, because the task explicitly requires some type of direct report on the perception of the critical stimulus. Second, one must assess the degree to which the critical stimulus, even if not consciously perceived, affects some other behavior. This is called an *indirect measure* (*I*) because responses are usually made to some stimulus other than the critical one, with the latter exerting an influence on processing the former. This research paradigm of comparing direct and indirect measures has been called the *dissociation procedure* (Reingold & Merikle, 1988, 1993).

The traditional criterion for perception without awareness is to establish that the direct measure is at chance level and that the indirect measure has some nonzero value. This so-called *zero-awareness criterion* may seem like a straightforward research strategy, but historically it has encountered severe difficulties. From the beginning, the field was plagued with methodological criticism concerning how to make sure that a stimulus was *completely* outside of awareness. This criticism is still at the heart of the most recent debates (e.g., in Erdelyi, 2004) and has overshadowed the most thought-provoking findings in unconscious cognition (e.g., Kunst-Wilson & Zajonc, 1980; Marcel, 1983). It has placed the burden of proof so one-sidedly on the shoulders of the unconscious cognition hypothesis that the zero-awareness criterion seems to have been more effective in hindering scientific progress than in helping it.

The zero-awareness criterion critically depends on showing that the value of the direct measure is at chance level, which introduces the statistical problem of corroborating a null hypothesis. In principle, this is not a substantive theoretical problem—it could be dealt with pragmatically by setting appropriately strict criteria for maximum effect sizes in the direct measure and for minimum statistical power to detect such effects at a fixed level of significance (Murphy & Myors, 1998). But binding standards of this sort have never been established in applied statistics or in the field of unconscious cognition, and many seminal studies have invited attack by employing somewhat lenient criteria or low statistical power for "proving the null" (e.g., Dehaene et al., 1998; Marcel, 1983). It may thus come as little surprise that in 1960, Eriksen concluded in an extensive review of the literature that no unequivocal evidence for unconscious perception existed at all; but it is irritating that a quarter of a century later, a new review by

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Holender (1986) reached the same conclusion with essentially the same arguments. In a recent article, that author has defended the theory that at least semantic processing is exclusively conscious (Holender & Duscherer, 2004).

These skeptics felt that the hypothesis of unconscious cognition should be tested as rigorously as possible. In their view, proponents of unconscious processing have to refute what we will call the *null model* of unconscious cognition. Under the null model, there is only one type of information processing, conscious processing, which accounts for purportedly unconscious effects and could be revealed as the sole source of these effects by some sufficiently sensitive measure. In this view, the problem of demonstrating unconscious perception simplifies to disproving the null model.

Our purpose here is to explore several methods for refuting the null model, by distinguishing three types of dissociation between direct and indirect measures. We start by specifying the conditions under which behavioral measures can be compared at all. Then we state a set of minimal measurement-theoretical assumptions that must be met for any reasonable kind of comparison between direct and indirect measures, showing that strong additional requirements must be met for the traditional zero-awareness criterion, but also that alternative criteria exist that rest on much milder assumptions. Because these other criteria do not require the critical stimulus to remain outside awareness, they turn out to be more powerful for detecting unconscious influences. Through a comparison of the pros and cons of the different approaches, we will argue that zero awareness of critical stimuli is neither necessary nor desirable for establishing unconscious perception. We will then demonstrate each type of dissociation with empirical data from a response priming paradigm (Vorberg, Mattler, Heinecke, Schmidt, & Schwarzbach, 2003, 2004) and show how some previously proposed criteria for unconscious cognition can be interpreted as special cases of our framework. Although we will deal primarily with unconscious visual perception, our results generalize naturally to other areas of unconscious cognition and, in fact, to any field of research that involves dissociations between two or more variables.

Avoiding *D–I* Mismatch

The most important requirement for any direct measure is that it be a valid measure of those conscious inputs that might explain nonzero performance in the indirect task—in other words, *D* must be a valid measure not of conscious processing per se, but of those sources of conscious processing that are potentially relevant for *I*. For that reason, Reingold and Merikle (1988) have argued that the tasks used to measure *D* and *I* should be directly comparable. Otherwise, there may be *D–I* mismatch whenever *D* is measuring something different from the conscious information actually driving *I*.

In our view, *D–I* mismatch is avoided if (1) stimuli are identical in both tasks, (2) the same stimulus features are judged in both tasks, and (3) the assignment of stimulus features to motor responses is the same in both tasks. Ideally, then, direct and indirect measures should be based on tasks that are identical in all respects except which stimulus serves as target. In a priming experiment as typically employed in unconscious perception research, this would mean that participants always indicate the same feature discrimination on the same stimuli and with the same set of responses; once with respect to the primed target (indirect task), and once with respect to the prime itself (direct task).

Unfortunately, *D–I* mismatch has been the rule rather than the exception. For example, in Dehaene et al.'s (1998) study, the indirect task was to indicate as quickly as possible whether a target number was numerically smaller or larger than 5, in the presence of masked number primes that were also smaller or larger than 5. The indirect measure was the difference in response times when the response evoked by the prime was congruent or incongruent with that evoked by the target (for example, when the prime was smaller but the target was larger than 5, as compared with when both were larger than 5). To match this indirect task, the optimal direct task would have asked for the same feature discrimination—namely, deciding whether the prime was larger or smaller than 5—because this was the information in the prime driving the indirect effect. Instead, the authors employed *two* direct tasks, detection of the primes against an empty background and discrimination of the primes from random letter strings, neither of which captured the critical distinction of whether the prime was smaller or larger than 5.

As another example, Draine and Greenwald (1998, Experiment 1) used semantic priming effects on the classification of target words (e.g., pleasant vs. unpleasant) as the indirect measure. The direct measure, however, involved discriminating the prime words from letter strings consisting of *X*s and *G*s. These tasks differed not only in the stimuli employed (the *XG* strings were never presented in the indirect task), but also in the stimulus dimension judged. Because the *XG* task was supposedly easier than the semantic classification task, it was argued that failing to discriminate words from *XG* strings would give even more convincing evidence for unconscious perception than would failure to classify the words semantically. However, the direct task may have invited a feature search for *X*s rather than a pleasant–unpleasant discrimination of the primes, so different aspects of conscious processing might have driven the two tasks. *D–I* mismatch could have been avoided if both tasks had involved exactly the same prime stimuli (i.e., no *XG* strings), as well as the same type of semantic classification (i.e., categorizing primes as pleasant or unpleasant). The use of a response window technique in the indirect but not the direct task further complicates interpretation of this study.

A more intricate example of *D–I* mismatch comes from the seminal study by Neumann and Klotz (1994; see also Jaskowski, van der Lubbe, Schlotterbeck, & Verleger, 2002). In the indirect task, participants performed a speeded discrimination of whether a square was presented to the left or right of a diamond, using a one-to-one mapping of stimulus configurations to responses. This

target pair was preceded either by a pair of congruent or incongruent primes (a square and a diamond in the same configuration as the targets or in the reverse configuration, respectively) or by a neutral prime pair (e.g., two diamonds). In the direct task, however, participants had to classify the prime pairs as neutral or nonneutral, such that the neutral prime pair was mapped onto one response and both remaining prime pairs onto the other response. Thus, even though the direct and indirect tasks employed identical stimuli, the direct task used a more complex, and presumably more difficult, stimulus–response mapping (Macmillan, 1986), which may have underestimated any true direct effect.

Properties of Direct and Indirect Measures

In general, a stimulus may give rise to some type and amount of perceptual information, which we assume can be represented by some quantitative variables. The null model that we want to refute assumes that one such variable is sufficient for representing all relevant perceptual information, in contrast to models assuming that more than one variable is necessary. To distinguish the null model from these alternatives, it suffices to pit it against the predictions of models with exactly two variables. It is unimportant formally what types of information are represented by the variables. In our context, and without loss of generality, we may label them c and u, denoting conscious and unconscious information, respectively.

To keep the model general, we allow direct as well as indirect measures to be affected by either type of input, and define them as functions of two variables, $D = D(c, u)$ and $I = I(c, u)$ (see section 1 of the Appendix for formal definitions). The logic of our approach is to refute the null model's claim that $u = 0$ by showing that certain observable data patterns of D and I are incompatible with this assumption.

A sensitive measure of cognitive processing should be able to reflect changes in the type of information it is intended to measure. A minimal requirement is that all other things being equal, an appropriately coded measure should not decrease when one of its arguments increases; rather, it should either increase or (at worst) remain constant. We will refer to this property as *weak monotonicity*. In contrast, a measure is said to obey *strong monotonicity* if increases in an argument must lead to increases in the measure.

Our general model of how direct and indirect measures depend on conscious and unconscious information is summarized in Figure 1A. This model rests on minimal assumptions: First, it is assumed that D and I are constructed to avoid D–I mismatch. Second, both D and I are assumed to be weakly monotonic in argument c and in argument u. (Actually, as shown in the Appendix, this assumption can be weakened for some of the proofs.) The null model is identical to the general model, except for assuming that $u = 0$ throughout, or in other words that unconscious information does not exist (Figure 1B). Refuting the null model is tantamount to showing that $u \neq 0$.

Empirical results can be visualized by plotting indirect effects against direct effects in *D–I space* (Figure 2). We will see shortly that it is convenient to convert both measures into a shared effect size metric. The different types of dissociation considered here all have distinct appearances in D–I space. For simplicity, we restrict discussion to the first quadrant of D–I space, within which both measures are positive.

Some types of dissociation will require properties of direct and indirect measures beyond those contained in the general model (see section 1 of the Appendix for formal definitions). Measures are called *exhaustive* with respect to some type of information if they are able to detect any change in that information—that is, if they are strongly monotonic with regard to it. In contrast, measures are called *exclusive* with respect to one type of information if they are unaffected by changes in the other type. For example, a measure is exhaustive for c if it detects any change in the amount of conscious information, whereas a measure exclusive for u will be unaffected by changes in c. Finally, two measures have the same *relative sensitivity* with respect to some type of information if a change in that information leads to changes of the same magnitude in both measures; obviously, this comparison requires that the two measures share the same metric. A measure is *at least as sensitive* as another measure if it changes at least as much as the other in response to a change in relevant information.

A Closer Look at the Dissociation Procedure: Simple Dissociations

We can now examine in more detail the formal difficulties encountered by the traditional zero-awareness criterion. Data patterns that conform to this criterion give rise to a *simple dissociation*, or values of D at chance level in the presence of nonzero I. If scales are such that zero values correspond to absence of sensitivity to the stimuli (e.g., chance performance in an identification task), data points in a D–I plot that conform to a simple-dissociation pattern line up along the $D = 0$ vertical (Figure 3A).

The simple-dissociation paradigm has been extensively criticized by Reingold and Merikle (1988, 1990, 1993; Reingold, 2004; see also Shanks & St. John, 1994, on implicit learning). These authors have argued that even if the dissociation procedure succeeds at producing an indirect effect without a direct effect, this is inconclusive evidence for unconscious perception unless some additional assumptions hold. In particular, D must be an exhaustive measure of conscious information (Figure 3B; see section 2 of the Appendix for a proof); if D reflects only some aspects of awareness but not others, absence of a direct effect does not imply absence of awareness, because some conscious information might have gone undetected that might account for the above-chance performance in the indirect task. We will refer to this as the *exhaustiveness assumption* of simple dissociations.

Actually, the exhaustiveness assumption as originally stated by Reingold and Merikle (1988) is more restrictive

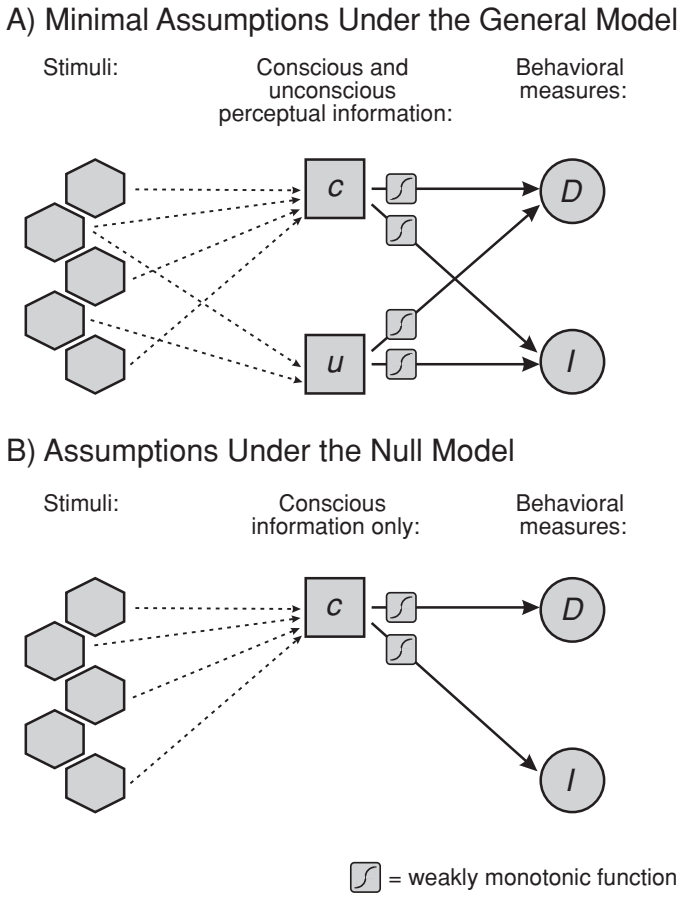

Figure 1. (A) Under the general model, direct and indirect measures (*D* and *I*) of processing are weakly monotonic functions of both conscious and unconscious information (*c* and *u*), which in turn depend only on physical stimulus characteristics. (B) The null model makes the same assumptions, except that unconscious information is disallowed.

than necessary: Because simple dissociations require *D* to be zero, *D* must be strongly monotonic with respect to *c* at the origin of *D*–*I* space only. However, strong monotonicity at the origin still requires a deterministic, noise-free direct measure. Even a noisy measure might approximate this ideal when estimated with high precision, but no empirical measure can be assumed to meet the exhaustiveness assumption in a strict sense.

There is yet another way for a simple dissociation to give conclusive evidence for unconscious processing—namely, when the indirect measure *I* is exclusive for unconscious information (Figure 3C; see section 2 of the Appendix for a proof). Because *I* is then affected by *u* only, unconscious processing is implied whenever *I* is above zero, whatever the value of *D*. We call this the *exclusiveness assumption* of simple dissociations. Of course, researchers believing that they possessed such a measure would have no reason for using the dissociation procedure in the first place, because they could measure unconscious processes directly. In contrast to measures exhaustive for *c*, however, measures exclusive for *u* may actually exist and may be revealed by converging evidence.[1]

It should be noted that Reingold and Merikle (1988, 1990) also state an exclusiveness assumption for the direct measure, demanding that it be sensitive to conscious information only. This assumption is redundant for purposes of interpreting an empirically established simple dissociation, for if *D* is exhaustive for *c* and equal to zero, it is immaterial whether or not it is also sensitive to *u*.

Sensitivity Dissociations

Reingold and Merikle (1988, 1993) have argued for a criterion of unconscious perception that does not require absence of awareness for the critical stimulus. Beyond the minimal assumptions stated in the general model of Figure 1A, this criterion requires that *D* be at least as sensitive to conscious information as *I*; we will refer to this as the *sensitivity assumption* (Figure 4B). As we show in

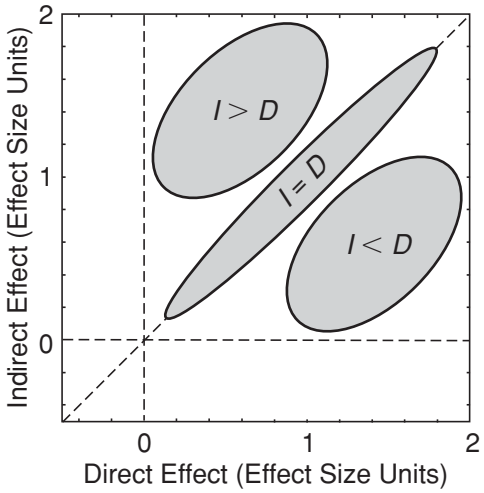

Figure 2. A *D–I* space is obtained by plotting indirect effects against direct ones after converting them to the same effect size metric. We discuss only the first quadrant of the plot, where both *D* and *I* are positive.

section 3 of the Appendix, the finding that I numerically exceeds D then implies that I is influenced by unconscious information to at least some degree.

Of course, two measures can be numerically compared only if they are expressed in the same metric. One way of establishing this comparability is to express them in effect size units (see the next section). In D–I space, with axes equally scaled, any data point lying above the $D = I$ diagonal is evidence for a sensitivity dissociation (Figure 4A). In section 3 of the Appendix, we sketch a proof that does not require additivity of conscious and unconscious sources of information, in contrast to the original proof by Reingold and Merikle (1988).

How do sensitivity dissociations imply the existence of unconscious information processing? Remember that the null model maintains that indirect effects are driven by conscious information only, so that any observed indirect effect is due to some residual awareness that could be revealed by a sufficiently sensitive direct measure. In order to explain the finding that $I > D$, the null model would have to claim that both measures reflect conscious information only, but that I is more sensitive to it (which is one traditional objection to simple dissociations). This is exactly what is ruled out by the sensitivity assumption, so the surplus effect in I can then only stem from an additional source of information. Note that the simple dissociation is a special case of the sensitivity dissociation, and that data patterns failing to show that $D = 0$ often show at least that $D < I$.

Double Dissociations

A double dissociation can be established if both D and I are measured under at least two experimental conditions (for example, a variation in prime duration or intensity, or some difference in visual masking or attention). Essentially, a double dissociation is an empirical finding that directly contradicts the null model. To establish a double dissociation, one has to show that some experimental manipulation leads to a *decrease* in the direct measure and at the same time to an *increase* in the indirect measure, or vice versa (Figure 5A). As we show in section 4 of the Appendix, the only requirement is that both D and I be weakly monotonic in c (Figure 5B). Under this condition, the null model predicts that changing the amount of c can

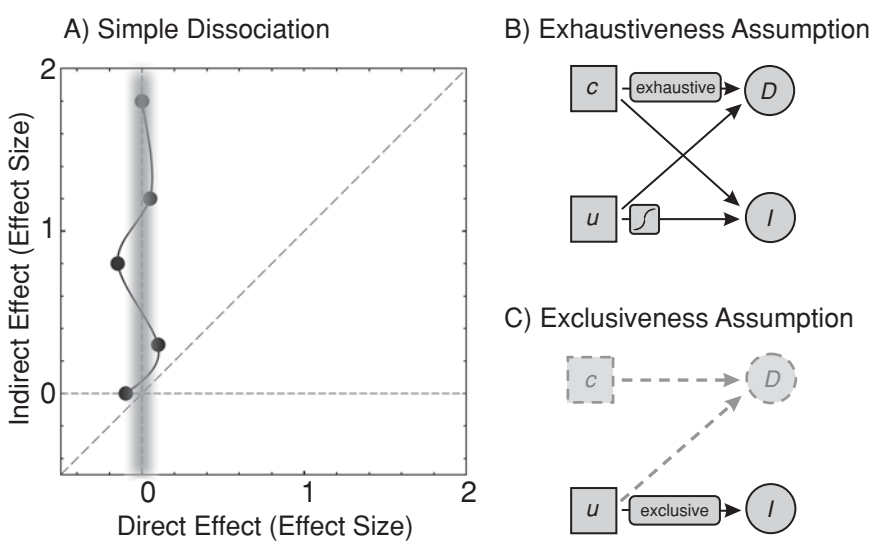

Figure 3. (A) The data pattern required for establishing a simple dissociation. Data points must lie on the $D = 0$ line. In addition, I must be weakly monotonic in u, as indicated by the curve symbol in panel B. (B) Evidence for a simple dissociation is unequivocal only if the exhaustiveness assumption is met, so that D is an exhaustive measure of all aspects of conscious information. (C) Alternatively, I can be an exclusive measure of unconscious information.

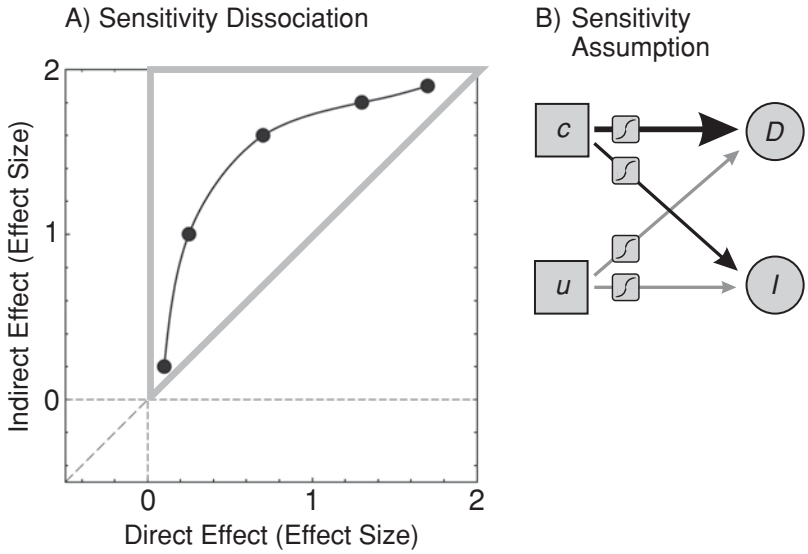

Figure 4. The data pattern required for establishing a sensitivity dissociation. Data points must lie above the $D = I$ diagonal. (B) Evidence for a sensitivity dissociation is unequivocal only if *D* is more sensitive to conscious information than is *I*. Weak monotonicity is required for all functions.

either change *D* and *I* in the same direction or leave them unchanged, but it cannot lead to changes in opposite directions. This is possible only if *D* and *I* are driven by at least two different sources of information that respond differently to experimental manipulation. If *c* and *u* reflect the only sources of information in the model, the double dissociation implies nonzero *u* in at least one of the conditions.

If data points are weakly ordered with respect to one axis of *D*–*I* space, the null model predicts their weak ordering with respect to the other axis ($D_i \leq D_j \Leftrightarrow I_i \leq I_j$ for experimental conditions *i* and *j*; Figure 6A). A double dissociation is suggested whenever two data points show opposite orderings on the two axes, so that they can be connected by a straight line with negative slope [e.g., $(D_i < D_j) \wedge (I_i > I_j)$; Figure 6B]. Such comparisons are not restricted to levels of the same experimental factor: A negative slope between *any* two data points produced by *any combination* of experimental manipulations means

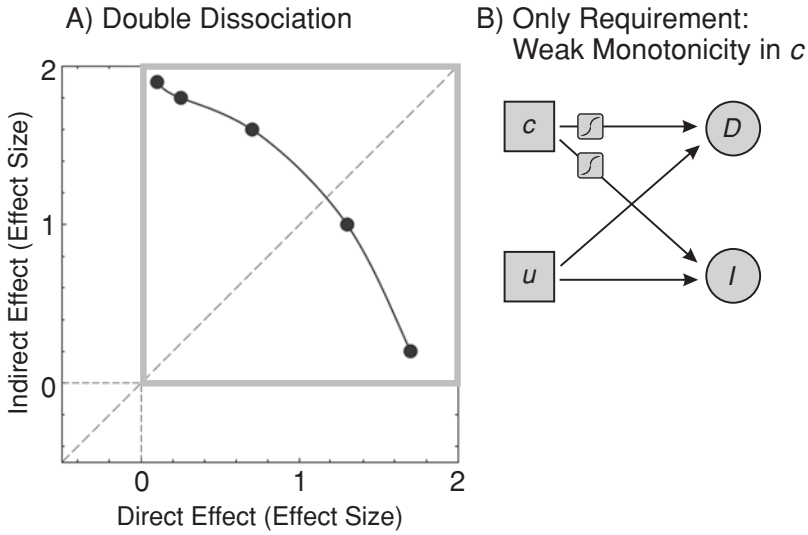

Figure 5. (A) If an experimental manipulation leads to effects of opposite ordering in direct and indirect measures, a double dissociation is demonstrated. (B) Evidence for a double dissociation is unequivocal as long as the minimal assumptions from Figure 1 are met. However, the monotonicity assumption can be abandoned for functions of *u*.

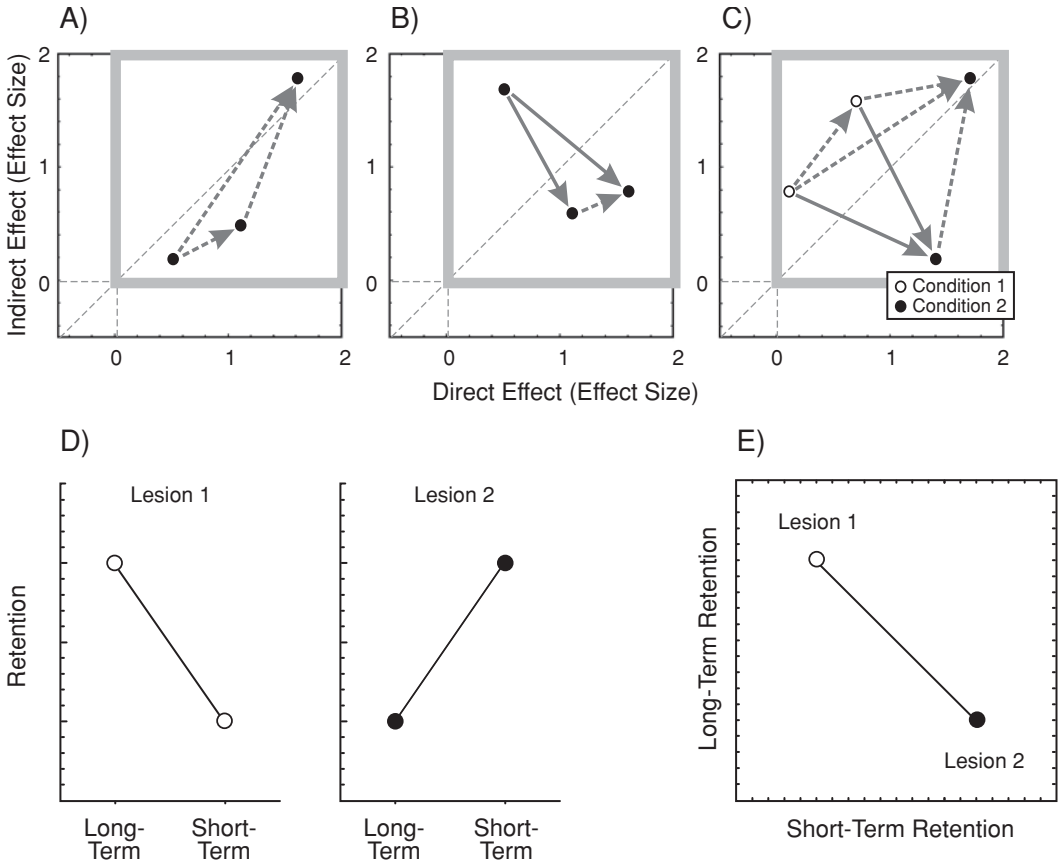

Figure 6. (A) The null model predicts that all pairs of data points in the *D–I* plot can be connected by straight lines with positive slope. (B) Any connection with negative slope constitutes evidence for a double dissociation. (C) This holds also for pairs of data points that differ in more than one independent variable. (D) The double-dissociation pattern traditional in neuropsychology is analogous to our approach. In this example, Lesion 1 (left panel) impairs long-term retention less than short-term retention, and Lesion 2 (right panel) does the opposite. (E) Plotting long-term retention against short-term retention for both types of lesion yields two data points that can be connected by a straight line with negative slope.

that there is some way of affecting *D* and *I* in opposite directions (Figure 6C).

There are two important boundary cases. First, data points may change only vertically—that is, in the direction of *I* but not of *D*. This is no evidence for a double dissociation because we only assume that *D* is weakly monotonic, and the fact that *D* is constant tells us nothing about the direction of change in *c* unless *D* is exhaustive for *c*. It is thus conceivable that *c* did actually change in the same direction as *I* but that the direct measure failed to detect this, so *c* alone might suffice to explain the data pattern, consistent with the null model. For the same reason, data points changing only horizontally, in the direction of *D* but not of *I*, do not suffice to establish a double dissociation under weak monotonicity assumptions.

Double dissociations open up new possibilities for finding dissociable functions of different brain areas (e.g., by considering dissociation patterns among more than two variables in multichannel brain imaging procedures).[2] For instance, brain regions of interest can be examined pairwise for double dissociations, and the two-way double dissociations developed here can be readily generalized to dissociations among three or more variables.

A concept of double dissociations very much akin to ours has long been traditional in neuropsychology and medicine. For example, two brain functions are said to be doubly dissociable if one lesion impairs function *A* but not function *B*, and another lesion does the opposite (Shallice, 1979; Teuber, 1955). Figure 6D shows a hypothetical clinical data pattern in which long- and short-term retention performance is measured in patients with two types of brain lesions. Assume that Lesion 1 (left panel) impairs long-term retention less than short-term retention, and Lesion 2 (right panel) does the opposite. One can see that this pattern is analogous to our concept of a double dissociation by plotting long-term retention against short-term retention for both types of lesion, yielding two data points, one for each lesion, that can be connected by a straight line with negative slope in an opposition space analogous to our *D–I* space. Note that all that matters here is the different ordering of short- and long-term retention performance across lesion groups, not the absolute levels

of performance. The main difference in our double dissociation concept is that the dissociation can only be established between different groups of patients, not within single participants.

Dunn and Kirsner (1988) have strongly criticized the neuropsychological double-dissociation logic, arguing that it requires "process-pure" measures of underlying sources of information. As an alternative, they proposed looking for "reversed associations" among experimental tasks (a synonym for the data pattern presented in Figure 6E), not noticing that this was just a novel way of plotting the traditional data pattern. In fact, the underlying assumptions for double dissociations in neuropsychology are similar to those stated in section 4 of the Appendix, even though some provisions must be made for the fact that the dissociation is between groups of participants. In particular, there is no need for process-pure measures: Because double dissociations do not require each task to be completely spared by the lesion affecting the other task, lesions do not have to exclusively influence one cognitive process but not the other.

Shared Metric for Direct and Indirect Effects

Although simple and double dissociations require no assumptions about the scaling of direct and indirect measures, sensitivity dissociations require them to be scaled equally. However, *I* and *D* often involve different response metrics, like response time and percent correct measures, which has prompted the claim that tasks should be designed in such a way that *D* and *I* are measured on the same type of scale (Reingold & Merikle, 1988). Fortunately, it is not necessary to restrict the tasks in such a way, because it is straightforward to convert differently scaled *D* and *I* measures into a shared metric.

The metric we suggest here is the d' statistic of signal detection theory (SDT; see Macmillan & Creelman, 2005), which is essentially an effect size measure. SDT assumes that in a stimulus discrimination task, stimulus alternatives are mentally represented as noisy distributions along the stimulus dimension judged. For example, if the task is to decide whether a visually masked stimulus is red or green (Schmidt, 2000), red stimuli are assumed to induce a distribution of values near the "red" end of the subjective red–green continuum, and green stimuli induce a distribution near the "green" end. When a stimulus appears, it generates some value along the continuum; if this value is to the "red" side of a decision criterion, the stimulus is judged to be red, and if the value is to the "green" side, the stimulus is judged to be green. A "green" response to a stimulus that is actually green may be arbitrarily called a *hit* (*H*), and a *false alarm* (*F*) stands for a "green" response to a red stimulus. Assuming that the stimulus representations are approximately normal with means μ_{red} and μ_{green} and standard deviations σ_{red} and σ_{green}, the most popular sensitivity statistic is d', which is defined as

$$d' = \left(\mu_{\text{green}} - \mu_{\text{red}}\right) / \sigma_{\text{red}}, \tag{1}$$

which can be estimated by

$$d' = z(H) - z(F) \tag{2}$$

if the equal-variance assumption $\sigma_{\text{red}} = \sigma_{\text{green}}$ is made (Macmillan & Creelman, 2005). Sensitivity is thus assessed by the difference between the normalized hit and false alarm rates.

Numerous alternative measures for sensitivity and response bias exist, based on different mathematical conceptions of the underlying decision spaces and strategies. The experimental design described here is a two-alternative yes–no design (Macmillan & Creelman, 2005). Equation 1 applies unchanged to detection tasks in which one stimulus is discriminated from noise rather than from another stimulus. Note that different mathematical models underlie other task types (e.g., *n*-alternative yes–no, forced choice, or matching-to-sample tasks; see Macmillan & Creelman, 2005).

How can response time data be converted into a d' metric, so that they can be compared with detection, discrimination, or recognition d's? We consider three different techniques, all of which can be used to inquire whether responding is faster in those conditions that should be speeded by the indirect effect.

In the *median-split technique*, response times are first classified as "slow" or "fast" in comparison with the overall response time median, and then cross-tabulated with the appropriate experimental conditions (Schmidt, 2002). By arbitrarily defining fast responses to congruently primed stimuli as hits and fast responses to incongruently primed stimuli as false alarms, d' can be computed from the corresponding frequencies as described above.

In contrast, the *ordinal dominance technique* makes use of the full cumulative distribution functions (*cdf*s) of response times on congruent and incongruent prime trials. If congruent primes shorten response times, the relative frequency of response times shorter than some value *t* for congruent trials will exceed the frequency for incongruent trials, implying that the empirical *cdf* for congruent trials leads the *cdf* for incongruent trials. Plotting $cdf_{\text{congruent}}$ against $cdf_{\text{incongruent}}$ results in an ordinal dominance graph (Bamber, 1975), which is analogous to the receiver operating characteristic curve that results when hit rates are plotted against false alarm rates for different values of response bias. For the equal-variance normal-distribution model, d' is functionally related to the area under the ordinal dominance graph. Tables for converting area measures into d' are given by Macmillan and Creelman (2005).

Finally, the *effect size technique* exploits the fact that d' as defined in Equation 1 is simply the distance between the means of the two stimulus representations, expressed in standard deviation units of one of the two underlying distributions. In contrast to typical applications of signal detection theory, the probability distributions involved here are directly observable.

Let $\bar{x}_{\text{congruent}}$ and $s^2_{\text{congruent}}$ be the observed response time mean and variance for congruent trials, and $\bar{x}_{\text{incongruent}}$ and $s^2_{\text{incongruent}}$ be the analogous statistics for incongruent tri-

als. A reasonable estimator of the response time effect (assuming equal sample sizes) is then

$$d_a = \frac{\bar{x}_{\text{incongruent}} - \bar{x}_{\text{congruent}}}{s_{\text{pooled}}} = \frac{\bar{x}_{\text{incongruent}} - \bar{x}_{\text{congruent}}}{\sqrt{\frac{1}{2}\left(s^2_{\text{incongruent}} + s^2_{\text{congruent}}\right)}}, \quad (3)$$

which estimates a generalized effect size measure that expresses the mean difference in units of the pooled standard deviation. Monte Carlo simulations (Vorberg, 2004) indicate that under a wide variety of conditions (normal vs. shifted gamma distributions, equal vs. unequal variances, or contamination by outliers), this measure is generally the most robust of the three, yielding the smallest mean squared error when based on the observed trimmed response time means and their winsorized standard deviations (see Wilcox, 1997).[3]

Three Types of Dissociation: Empirical Examples

Figure 7 shows a reanalysis of data from our own laboratory (see Vorberg et al., 2003, for details) that illustrate how simple, sensitivity, and double dissociations provide converging evidence for unconscious processing in the visual domain. In each trial, participants saw a large arrow stimulus, which also served to visually mask the prime stimulus, a small arrow that had been presented briefly before at the same position (Figure 7A). This arrangement produces a form of strong backward masking of the prime stimulus called *metacontrast* (Breitmeyer, 1984; Francis, 1997). Because the amount of masking depends on the temporal separation of prime and mask, we varied the stimulus onset asynchrony (SOA) between them. In the indirect task, participants had to indicate as quickly as possible whether the mask pointed to the left or to the right by pressing one of two response keys. The indirect measure was the *response priming effect* (Vorberg et al., 2003; see also Dehaene et al., 1998; Eimer & Schlaghecken, 1998; Klotz & Neumann, 1999; Mattler, 2003; Neumann & Klotz, 1994), defined as the difference in response times when the mask was preceded by a congruent prime (pointing in the same direction as the mask) versus an incongruent prime (pointing in the reverse direction). As a direct measure, we asked participants to identify, without speed pressure, the direction of the masked primes. *D–I* mismatch was avoided by employing identical stimuli and stimulus–response mappings in both tasks and by basing direct and indirect tasks on the same critical stimulus feature, arrow direction. Prime and mask identification tasks were performed in different blocks of trials.

Figure 7B shows a *D–I* plot of the results.[4] The abscissa indicates prime identification performance in terms of d', estimated from the relative frequencies of hits and false

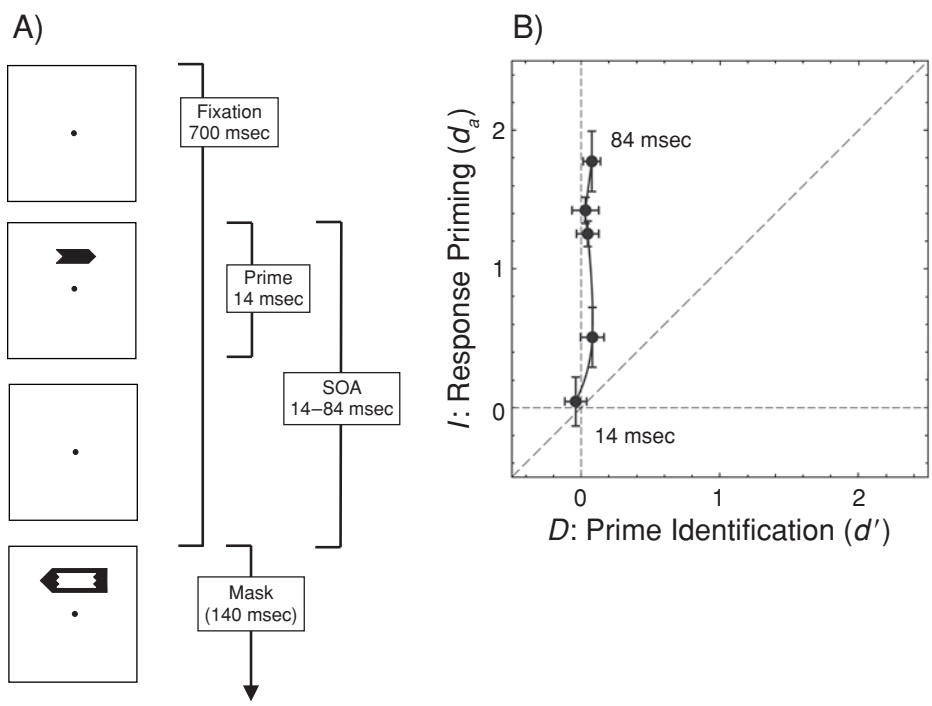

Figure 7. (A) Stimulus timing in Vorberg et al.'s (2003) response priming task. (B) A *D–I* plot of the data from Vorberg et al.'s (2003) Experiment 1. Data points are connected to show their ordering by increasing prime–mask SOA (14–84 msec). Each data point on the *D* variable is based on 6 participants performing about 3,000 trials each. None of the participants performed above chance in any condition. Error bars indicate ±1 standard error around the mean in all directions, with estimates based on the ipsative procedure recommended by Loftus and Masson (1994).

alarms (Equation 2); the ordinate indicates the response priming effect in terms of d_a, estimated directly from the response time distributions by the effect size technique (Equation 3). Priming effects strongly increased with SOA, whereas prime identification performance was essentially at chance, with none of the 6 participants exhibiting better-than-chance accuracy in as many as 3,000 trials. Data points line up along the $D = 0$ line and conform to the simple-dissociation pattern. There is also evidence for a sensitivity dissociation, because all data points but one lie clearly above the $D = I$ diagonal.

Figure 8 shows what happened when the visibility of the primes was altered by varying the durations of primes and masks. This manipulation left priming effects unchanged, so all curves rise with prime–mask SOA in the vertical direction. However, masking (i.e., the degree to which metacontrast affected performance in the direct task) strongly depended on the exposure durations of primes and masks. Masking of 14-msec primes by 42-msec masks was quite efficient but not perfect, so that data points do not line up along the $D = 0$ line. Instead, D tends to increase with SOA, yielding a positively sloped curve in D–I space. However, since most data points lie above the $D = I$ diagonal, there is evidence for unconscious processing by the sensitivity criterion. When mask duration was reduced to 14 msec, primes became more visible, which is reflected in the shift of the curve to the right. Under this condition, most data points fell on or below the diagonal, so that unconscious processing could not be inferred from this subset of the data.

A strikingly different pattern was obtained when prime duration was increased to 42 msec (Figure 8B). With longer mask duration (42 msec), a phenomenon named *type II masking* was obtained, in which visibility first decreased with SOA, then increased again (Breitmeyer, 1984). In contrast to this U-shaped time course of masking, priming effects increased with SOA, so that a part of this curve displays a negative slope in D–I space. This constitutes clear evidence for a double dissociation. Note that most of the data points lie above the $D = I$ diagonal, and thus give evidence of a sensitivity dissociation as well. Reducing mask duration to 14 msec again made the curve shift to the right, eliminating the evidence for a sensitivity dissociation, but still leaving some evidence for a double dissociation.

General Discussion

All three criteria for demonstrating unconscious processing—sensitivity, simple, and double dissociations—can be combined within a common framework that assumes that direct and indirect processing measures may each be affected by conscious and unconscious information. All criteria rest on the comparability of direct and indirect tasks, which must employ identical stimuli and stimulus–response mappings, as well as judgments of the same stimulus features, so that the direct task explicitly assesses the stimulus information driving the effect in the indirect task. Empirical examples for each of these data patterns can be obtained in a response priming paradigm, where all three criteria provide converging evidence for unconscious visuomotor processing of masked prime stimuli (Vorberg et al., 2003).

If D and I are expressed in the same effect size metric, D–I plots of indirect versus direct measures can be checked for all three types of dissociation simultaneously. Data points above the $D = I$ diagonal provide evidence for sensitivity dissociations. As a special case, data points falling on the $D = 0$ line give evidence for simple disso-

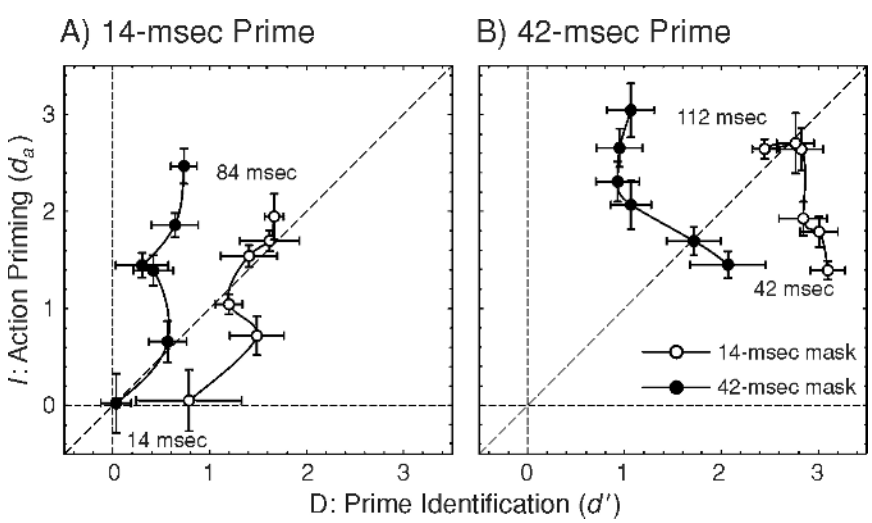

Figure 8. (A) *D–I* plot of the data from Vorberg et al.'s (2003) Experiment 2, for 14-msec and 42-msec masks. Prime duration was 14 msec. (B) The same plot for a prime duration of 42 msec. Data points are connected to show their ordering by increasing prime–mask SOA (14–84 msec or 42–112 msec). Error bars indicate ±1 standard error around the mean in all directions, with estimates based on the ipsative procedure recommended by Loftus and Masson (1994).

ciations. Finally, data points that show opposite orderings of the two measures (revealed by pairs of data points that can be connected by a straight line with negative slope) provide evidence for double dissociations.

Clearly, the different types of dissociation are restricted to different areas within *D–I* space. Evidence for simple dissociations can be obtained only on a single line of this space, which means that experimental conditions must be established in which participants are perfectly unaware of the critical stimuli. In contrast, sensitivity dissociations can arise in the entire upper half-space, which implies that participants may be aware of at least some of the critical stimuli, as long as indirect effects exceed direct ones. Finally, double dissociations can result anywhere in *D–I* space—the critical stimuli may be partly visible even to the extent that direct effects exceed the indirect ones.

For evaluating the relative merits and problems of the three criteria, it is crucial to examine how restrictive their underlying assumptions are. The exhaustiveness or exclusiveness assumptions that underlie the simple-dissociation criterion are highly problematic, because researchers cannot know beforehand whether a given measure meets these requirements, or whether such a measure exists at all. In particular, exhaustiveness requires that the direct measure be strongly monotonic with respect to the amount of conscious information, even under conditions in which such information is virtually absent. To the degree that a direct measure fails to capture small magnitudes of conscious information, it fails to meet exhaustiveness. Annoyingly, such failure must inevitably result from random noise, either in the measurement process or due to the probabilistic nature of difficult discrimination tasks. The only remedy here is massive statistical power in measuring *D*. Given that studies measuring *D* with appropriate precision will also be likely to detect minuscule departures from zero, convincingly demonstrating a simple dissociation is largely a matter of good fortune.

In contrast, sensitivity dissociations require weak monotonicity only, which is a much milder assumption. However, it also requires that direct and indirect measures be expressed in the same metric, which creates new problems. A conversion into effect size units, as proposed here, is a mathematical transformation of the data but does not guarantee equalization of the underlying process metrics. For example, two measures of *D* (or *I*) with identical expected values but different variances would have different coordinates in *D–I* space, so spurious sensitivity dissociations could be produced by employing highly reliable indirect measures together with unreliable direct ones (Reingold & Merikle, 1988). Viewed in this way, the sensitivity assumption seems problematic if employed without thorough knowledge of the inherent properties, including the reliabilities, of the measures involved.

Double dissociations go beyond both sensitivity and simple dissociations, since they require neither strong monotonicity, exclusiveness, exhaustiveness, shared metric, nor relative sensitivity assumptions. If *D–I* mismatch is avoided, the only requirements left are those of the general model introduced in Figure 1, and even these can be weakened (e.g., weak monotonicity of measures with respect to unconscious information is not strictly necessary; see section 4 of the Appendix). As a consequence, double-dissociation patterns allow conscious and unconscious information to interact arbitrarily—for instance, when increased availability of conscious information becomes detrimental to the utilization of unconscious information (Snodgrass, Bernat, & Shevrin, 2004). This is a further advantage over the sensitivity dissociation criterion, which requires monotonicity in both arguments.

There are alternative approaches for demonstrating unconscious perception, some of which can be regarded as special cases of our framework. For instance, the regression method proposed by Draine and Greenwald (1998; Greenwald, Klinger, & Schuh, 1995), though making use of a *D–I* plot, is an instance of a simple dissociation. The authors suggested that, instead of trying to establish experimental conditions that bring the direct measure to zero, one should allow for a wide range of *D* values and test whether the (possibly nonlinear) regression function of *I* against *D* has a nonzero intercept term. Obviously, this is simply another way of stating that $I > 0$ at $D = 0$. Unfortunately, beyond the problematic assumptions needed to establish a simple dissociation, strong additional assumptions have to be met for the regression methodology to be valid. The procedure has met with severe criticism (e.g., by Dosher, 1998; Merikle & Reingold, 1998; Miller, 2000; see also Klauer, Draine, & Greenwald, 1998), the bottom line being that in the absence of a strong and lawful relationship between *D* and *I* (i.e., R^2 of the regression model close to 1), the intercept term will primarily reflect error variance, and the approach will be practically useless.

Merikle and Cheesman (1987) suggested abandoning the simple dissociation in favor of a *qualitative dissociation* approach, which requires showing that a critical stimulus has qualitatively different effects on an indirect measure, which is supposed to switch sign when perceived unconsciously rather than consciously. To demonstrate such a qualitative dissociation, Merikle and Joordens (1997a, 1997b) employed a Stroop task in which incongruent color–prime combinations were presented more frequently than congruent ones, so that participants learned to respond faster to incongruently than to congruently primed targets, provided that the primes were visible (a reverse Stroop effect). In contrast, the regular Stroop effect (with faster responses to congruently primed targets) was observed when primes were made invisible by masking.

As is shown in section 5 of the Appendix, the qualitative dissociation defined by Merikle and Joordens (1997a, 1997b) can be seen as a special case of our double dissociation, and even employs some unnecessary side conditions. In applying our analysis to the Stroop example, we can conceive response times to targets with congruent and incongruent primes, respectively, as two different indirect measures, one of which has to decrease with experimental conditions (i.e., the level of awareness) while the other

increases. If this happens, however, one of them *must* form a double dissociation with the direct measure.

Several things should be noted here. First, in a qualitative dissociation, only one of the measures, I or J, forms a double dissociation with D, and the remaining measure is completely uninformative. The qualitative dissociation is thus not stronger than the double dissociation. Second, the demand that $D = 0$ in one condition is redundant; it suffices that the conditions differ in awareness. Third, it is only required that either I or J has an ordering opposite from D's, not that one of them actually switches sign. At the same time, note that it is not sufficient for I and J to have orderings opposite *to each other*, which could occur if I and J start out at different values when D is small and then both increase with D, so that the initially smaller measure overtakes the initially larger measure. In this case, the reversed ordering could be explained by higher relative sensitivity in the initially smaller measure.

Some rival approaches should be mentioned that require a more detailed analysis than can be given here. Cheesman and Merikle (1984) have argued that, rather than employing *objective measures* of D (which are based on a verifiable match or mismatch between stimulus and response), one should use *subjective measures*, which assess the confidence with which the observer consciously perceives the stimulus. The proposition has not been followed universally, because it is not clear whether data patterns that look like dissociations truly reflect qualitative differences in cognitive processes, rather than uncontrolled changes in participants' response criteria. What is needed is a thorough analysis of the assumptions on which this approach rests and of the conditions under which valid conclusions can be drawn from particular dissociation patterns.

The same can be said about a novel approach by Snodgrass et al. (2004). This proposal is based on two ideas, each of which rests on strong psychological assumptions. The first idea is that an ordered set of perceptual tasks can be constructed such that above-chance performance in a lower-level task (e.g., detection) is a precondition for nonzero performance in all higher-level tasks (e.g., identification). The second idea is that conscious and unconscious sources of information can interact in such a way that increased availability of conscious information interferes with the utilizability of unconscious information. The strategy proposed by Snodgrass et al. is to establish an ordered series of perceptual thresholds for the direct measure, such as a subjective identification threshold (Cheesman & Merikle, 1984), an objective identification threshold, and an objective detection threshold, and to assess an indirect measure at each threshold. The authors argue that if indirect effects *increase* as the threshold becomes stricter, this is indicative of unconscious influences on the indirect measure asserting themselves against receding conscious information.

Their whole approach might be viewed as a double dissociation established at task level rather than parametrically. However, the notion that perceptual tasks can be ordered hierarchically is an assumption we are reluctant to accept, since all these tasks depend on different decision spaces and response criteria (Macmillan, 1986; see also Luce, Bush, & Galanter, 1963), and thus are difficult to compare. Furthermore, their approach could detect only those unconscious processes that work in opposition to conscious ones and would fail to reveal, for instance, early unconscious processing steps later developing into conscious representations. Note that the latter problem also limits application of the double-dissociation approach.

Jacoby's (1991, 1998) *process dissociation* approach tries to pit conscious and unconscious sources of memory against each other by requiring the participant to either reproduce as many items as possible from a previously presented list (*inclusion task*) or try to avoid items from that list when generating new items (*exclusion task*). It is assumed that the inclusion task measures conscious recollection of the item list, whereas the exclusion task identifies those items that were unconsciously activated but failed to be consciously rejected. The primary merit of process dissociation approaches lies in the quantitative modeling of conscious and unconscious memory retrieval, presupposing that such processes exist. Recently, however, Hirshman (2004) has shown that inferences about the ordering of unconscious processes across different conditions can be drawn from the ordering of memory performance in inclusion and exclusion tasks under assumptions similar to ours, demonstrating that the inclusion/exclusion logic can be employed to refute a null model of only conscious retrieval.[5]

Concluding Comments: Beyond the Zero-Awareness Criterion

For more than four decades, methodological debate on unconscious cognition has revolved around the question of how to make sure that critical stimuli completely remain outside awareness. We believe that it is time to leave this stationary orbit. It is now clear that the traditional zero-awareness criterion has relied on the strong assumptions required for simple dissociations, thus upholding overly restrictive methodological standards. Ironically, more convincing criteria can be founded on much weaker assumptions.

One peculiarity of double dissociations is that critical stimuli are not allowed to be invisible throughout experimental conditions, or else the restrictive exhaustiveness assumption will intrude. This leads to the somewhat counterintuitive conclusion that the best way to demonstrate unconscious cognition is to use stimuli that are *not* unconscious. The major drawback of double dissociations is that they may be hard to find: They can only occur if the processes underlying direct and indirect effects work in opposition, which may be the exception rather than the rule. However, examples of successfully established double dissociations do exist. For instance, Mattler (2003) reported a series of experiments in unconscious perception that found response priming not only for overt motor responses, but also for cross-modal attention shifts and task switch sets, with clear double dissociations from visual awareness in each experiment. As we have seen, the findings of Merikle and Joordens (1997a, 1997b) are fur-

ther examples of double dissociations, as are numerous observations in neuropsychology.

However, the problem of demonstrating unconscious cognition cannot be solved by formal arguments alone, since even double dissociations are conclusive only in the context of a preconceived model. The dissociation patterns described here refute single-process models that claim that both *D* and *I* reflect the same source of information. Under the more general assumptions we have made here, dissociations merely imply that there exist at least two separate sources. Formally, nothing requires either of them to be unconscious—any two dissociated sources of information are conceivable, each of which may be conscious or unconscious (or maybe of yet another type).

This argument has two important implications. First, deciding whether the underlying dichotomy makes sense at all requires conceptual considerations as well as empirical work and is outside the scope of this article. Second, our reasoning is not confined to the conscious–unconscious distinction to which it was applied here, but to any such dichotomy of unobservable processes.

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NOTES

1. Recent evidence suggests that visuomotor activation in response priming paradigms can be explained exclusively by successive waves of feedforward motor activation triggered by primes and targets (Schmidt, Niehaus, & Nagel, 2006). At the same time, recent theories (Di Lollo, Enns, & Rensink, 2000; Lamme, 2002; Lamme & Roelfsema, 2000) stress the importance of intracortical feedback and recurrent processing as necessary conditions for visual awareness. Therefore, evidence may corroborate the idea that motor control in response priming and similar tasks is mandatorily unconscious because it precedes intracortical feedback mechanisms.

2. We thank Hakwan Lau for this suggestion.

3. An outlier criterion of 10% from either end of each response time distribution was used for trimming and winsorizing the distributions. *Winsorization* is a procedure that replaces the values beyond the outlier criteria with the most extreme values retained.

4. Results from the indirect task are pooled across parts *a*, *b*, and *e* of Vorberg et al. (2003), Experiment 1. Results from the direct task are pooled across parts *c* and *d*. See that previous article for details.

5. Most of Hirshman's (2004) proofs critically depend on the assumption that inclusion and exclusion measures are strongly monotonic for conscious as well as for unconscious memory information. However, his proof of an implicit-memory analog of our double dissociation is similar to the one reported here and earlier in Vorberg et al. (2003, Supplementary Material section), and it can be shown to remain valid under weak monotonicity.

APPENDIX

1. Definitions and Assumptions

Let A and B denote two types of sensory information, with a and b indexing their strength; for simplicity, we assume $a, b \geq 0$. Consider measures M and M', which seek to assess the sensory information available to an observer. The measures are intended to focus on one type of information only, but they may be contaminated by the other type as well. Therefore, we model them as functions of two arguments.

Monotonicity. A measure M is weakly monotonic in a if for all b, $M(a, b) \leq M(a', b)$ whenever $a \leq a'$. Weak monotonicity in b is defined analogously. A measure is weakly monotonic in both arguments if both properties hold. Strong monotonicity is defined correspondingly, except that the inequalities must be strict.

Note that monotonicity in only one argument allows arbitrary interactive effects of a and b on a measure. In contrast, monotonicity in both arguments permits ordinal interactions only—for example, $M(a, b) \geq \max[M(a, 0), M(0, b)] \geq M(0, 0)$.

Exhaustiveness. M is exhaustive with respect to type A information if $M(a, b) > M(0, b)$ for $a > 0$ and all b—that is, if M is strictly monotonic in a. Exhaustiveness with respect to type B information is defined analogously. Exhaustive measures produce nonzero effects whenever the relevant argument is nonzero, no matter how small the effect.

Exclusiveness. M is exclusive with respect to type B information if it is sensitive to this type of information only: $M(a, b) = M(0, b)$ for all a and b. Exclusiveness with respect to type A information is defined analogously.

Relative sensitivity. A measure M is at least as sensitive to type A information as another measure M' if $M(a, b) - M(0, b) \geq M'(a, b) - M'(0, b)$ for all a and b.

In the following discussion, let C and U denote the types of sensory information potentially accessible to conscious or unconscious processing, respectively, and c and u denote their strengths. D and I are the direct and indirect indices intended to measure them. D and I are conceptualized as sharing the same arguments, $D = D(c, u)$ and $I = I(c, u)$. Unless stated otherwise, we assume either measure to be weakly monotonic with respect to either argument. We define effects on a measure by the difference from the corresponding baseline—for instance, $D^* = D(c, u) - D(0, 0)$ and $I^* = I(c, u) - I(0, 0)$.

2. Simple Dissociation

Proposition. An observed dissociation $I^* > 0$ and $D^* = 0$ implies $u > 0$ if (1) the indirect measure I is exclusive with respect to unconscious information or (2) the direct measure D is exhaustive with respect to conscious information.

Proof. *Case 1*. If I indicates unconscious processing exclusively, $I(c, u) = I(0, u)$ for arbitrary conscious information c. Thus,

$$\begin{aligned} I^* > 0 &\Leftrightarrow I(c, u) - I(0, 0) > 0 \Leftrightarrow I(0, u) - I(0, 0) > 0 \\ &\Rightarrow u > 0. \end{aligned}$$

Case 2. For an exhaustive direct measure, $D^* = D(c, u) - D(0, u) = 0$ implies $c = 0$. Then,

$$\begin{aligned} I^* > 0 &\Leftrightarrow I(c, u) = I(0, u) > 0 \\ &\Rightarrow u > 0. \end{aligned}$$

Note that either derivation requires weak monotonicity of the indirect measure in the second argument, u.

3. Sensitivity Dissociation

Proposition. An observed ordering $I^* > D^*$ implies $u > 0$ if the direct measure D is at least as sensitive to conscious information as the indirect measure I.

Proof. We work from the definitions of I^* and D^* by adding and subtracting the terms $I(0, u)$ and $D(0, u)$:

$$\begin{aligned} I^* > D^* &\Leftrightarrow I(c, u) - I(0, 0) > D(c, u) - D(0, 0) \\ &\Leftrightarrow I(c, u) - I(0, u) + I(0, u) - I(0, 0) > D(c, u) - D(0, u) + D(0, u) - D(0, 0) \\ &\Leftrightarrow I(0, u) - I(0, 0) > [D(c, u) - D(0, u)] - [I(c, u) - I(0, u)] + [D(0, u) - D(0, 0)]. \end{aligned}$$

The difference between the first two bracketed terms on the right-hand side is nonnegative if the sensitivity assumption holds and by weak monotonicity of both measures with respect to c, whereas the difference in the remaining bracket is nonnegative by monotonicity of D with respect to u. Thus,

$$I^* > D^* \Rightarrow I(0, u) - I(0, 0) > 0 \Rightarrow u > 0.$$

Note that the derivation requires weak monotonicity in both arguments for either measure.

4. Double Dissociation

Proposition. Let D_k^* and I_k^* denote the direct and the indirect effects observed under experimental conditions k, $k \in \{1, 2\}$. The joint observation of $D_1^* < D_2^*$ and $I_1^* > I_2^*$ implies $\max(u_1, u_2) > 0$.

APPENDIX (Continued)

Proof. We prove that $u_1 \neq u_2$ by showing that the assumption $u_1 = u_2 = u$ leads to contradiction:

$$D_1^* < D_2^* \Rightarrow D(c_1, u) < D(c_2, u) \Rightarrow c_1 < c_2;$$
$$I_1^* > I_2^* \Rightarrow I(c_1, u) > I(c_2, u) \Rightarrow c_1 > c_2.$$

These inequalities directly refute the null model because they show that direct and indirect effects cannot both be driven by variation in the c argument only. As $u_1, u_2 \geq 0$ by assumption, $u_1 \neq u_2$ implies $\max(u_1, u_2) > 0$, which means that there is evidence for nonzero unconscious information under at least one of the experimental conditions.

Note that the proof requires strict inequalities because, for instance, $D(c_1, u) \leq D(c_2, u)$ does not imply $c_1 \leq c_2$ unless D is exhaustive for c. Mere invariance in one of the measures is thus insufficient to produce a double dissociation. Remarkably, the proof requires weak monotonicity of D and I in the c argument only, in contrast to the requirements for sensitivity dissociations; the measures may depend on u in an arbitrary way. Therefore, we can allow C and U to interact in an arbitrary fashion, as in reciprocal inhibition.

Double dissociations also refute the argument that D and I actually measure the same single source of information, but that D is less sensitive to it than I is. As a definition, we say that A is at least as sensitive as B if for any two experimental conditions i and j, $A_i > A_j$ implies $B_i \geq B_j$, and $A_i = A_j$ implies $B_i = B_j$. The intuition behind this is simple: If A registers an effect when conditions change from i to j, the less sensitive measure may also register an effect or remain unaffected. If A doesn't register an effect, then the less sensitive measure B must also fail to do so.

Proposition. The joint observation $D_1^* < D_2^*$ and $I_1^* > I_2^*$ is incompatible with the assumption that D and I depend on a single source of underlying information and differ in sensitivity only.

Proof. *Case 1*. Assume I is the more sensitive measure. Then $I_1^* > I_2^*$ implies $D_1^* \geq D_2^*$, which contradicts the observation $D_1^* < D_2^*$.

Case 2. Assume that D is the more sensitive measure. Then $I_1^* > I_2^*$ implies $D_1^* > D_2^*$, which also contradicts the data.

5. Merikle and Joordens's (1997a, 1997b) Qualitative Dissociation

Assume that there are two different indirect measures, $I = I(c, u)$ and $J = J(c, u)$. Let I_k^* and J_k^* denote the corresponding indirect effects and D_k^* the direct effects observed under experimental conditions k. A qualitative dissociation is said to exist if either I or J shows an ordering opposite from that of D; that is, there exist conditions, m and n, such that $D_m^* > D_n^*$ and either ($I_m^* < I_n^*$ and $J_m^* > J_n^*$) or ($I_m^* > I_n^*$ and $J_m^* < J_n^*$).

Theorem. Qualitative dissociation implies double dissociation.

Proof. By assumption, either ($D_m^* > D_n^*$ and $I_m^* < I_n^*$) or ($D_m^* > D_n^*$ and $J_m^* < J_n^*$). Thus, a double dissociation pattern exists either for I and D or for J and D, implying $\max(u_1, u_2) > 0$ for the corresponding direct measure.

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