"ILLUSIONS OF NORMALITY": A METHODOLOGICAL CRITIQUE OF CATEGORY-SPECIFIC NAMING

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Abstract

Category-specific disorders are perhaps the archetypal example of domain-specificity – being typically defined by the presence of dissociations between living and nonliving *naming* ability in people following neurological damage. The methods adopted to quantify naming across categories are therefore pivotal since they provide *the criterion* for defining whether patients have a category effect and necessarily influence the subsequent direction and the interpretation of testing. This paper highlights a series of methodological concerns relating to how we measure and define category (or any) dissociations. These include the common failure to include control data or the use of control data that is inappropriate e.g. at ceiling, unmatched. A review of past cases shows that the overwhelming majority suffers from these problems and therefore challenges conclusions about the purported empirical demonstrations of dissociations and double dissociations in the category specific literature. This is not a refutation of category deficits, but skepticism about the current existence of any convincing empirical demonstrations of category specific double dissociations. As a potential solution, certain minimal criteria are proposed that might aid with the attempt to document category effects that are more methodologically convincing.

Key words: dissociation, double dissociation, modularity, category-specific, review

Much of cognitive neuropsychology is underpinned by the empirical documentation of dissociations and double dissociations that are used to fractionate cognition into *domain-specific* processes. Category-specific disorders (CSDs) are perhaps one of the archetypal examples of domain-specificity – being typically defined by the presence of dissociations between living and nonliving *naming* ability. The methods adopted to quantify naming across categories are therefore pivotal since they provide *the criterion* for defining whether patients have a CSD and necessarily influence both the direction and the interpretation of further testing.

A search of bibliographic databases and recent reviews reveals that by far the most common strategy for documenting CSDs has been to use a *within-patient* comparison of living and nonliving naming (Table I). Two-thirds (22/33) of case studies have used a within-subject comparison (with χ^2 analysis) to establish category naming effects; with only approximately 1 in 5 studies including any controls for comparison purposes. It will be argued that the pervasive use of a within-subject approach is likely to mislead about both the *presence* and the *direction* of CSDs. Moreover, the *empirical* status of the dissociations and double dissociations in this literature therefore require closer examination, especially with regard to methodological requirements for defining a category-specific naming deficit (be it living or nonliving).

The Assumption of Normality

Why would the majority of category-specific studies fail to include control data? This may partly reflect the assumption that patient performance would be an *exaggeration* of the *normal* profile. Most commonly, this assumption suggests that normal subjects would find living things more difficult to name than nonliving things because the former are, for example less familiar, have lower name frequencies, or have greater visual complexity (Stewart et al., 1992; Funnell and Sheridan, 1992). Nevertheless, as with patient studies, it is necessary to examine the performance of controls on sets of living and nonliving stimuli that are not confounded by these and other potential artefactual variables. In contrast to expectation, recent studies using matched sets of stimuli have reported better and faster naming of living than nonliving things in neurologically intact subjects (Laws, 1999, 2000, 2001a, 2001b; Laws and Gale, 2002; Laws and Neve, 1999). We cannot, therefore, simply assume what is *normal* – this must be explicitly examined anew in each case[1].

Moreover, such 'artefactual explanations' can *only* ever account for half of the phenomenon e.g.

[1] Patients and controls should also be gender-matched. Recent studies have consistently reported better naming of nonliving things by males and better naming of living things by females in Alzheimer patients (Laiacona et al. 1998), aphasics (Laiacona et al., 2003), as well as normal controls (Laws, 1999, 2000, 2004) and this even extends to semantic fluency (Laws et al., 2005).

TABLE I

Details of category specific case studies

Study	Patient name	Stimuli type	Identified variables	Matched for identified variables?	Living	Nonliving	How was effect defined?
Arguin et al., 1996	ELM	S + V	VC, Fam	*Post-hoc* testing	26/66	70/79	No statistical analysis
Barbarotto et al., 1996	LF EA FA FI	S + V	Fr, Fam, NA, IA, VC, Proto	Regression scores adjusted	16/30 1/30 7/30 4/30	26/30 13/30 18/30 17/30	L-NL difference in regression
Bunn et al., 1998	JBR	S + V S + V Colour photos	 Fam, VC, Fr Fam, VC	✗ ✓ ✓	25/97 3/24 14%	93/160 12/24 59%	Within patient χ^2
Cappa et al., 1998	GP	S + V	–	✗	76/94 (86/94) post 7 months	31/64 (46/64) post 7 months	Within patient χ^2
Caramazza and Shelton, 1998	EW	S + V	Fam, Fr, VC	✓	7/17	16/17	z score comparison to controls
De Renzi and Lucchelli, 1994	Felicia	Colour pictures	–	✗	32/80	27/30	Below worst control and discrepancy greater than maximum in controls
Farah et al., 1989	MB LH	S + V	Fr, Fam, VC	Regression scores adjusted	Not reported	Not reported	Within patient χ^2
Farah et al., 1996	MB LH	S + V	VC, Fam, Fr, IA, name specificity	Included in regression analysis	159/475 247/475	633/825 694/825	L-NL difference in regression
Forde et al., 1997	SRB	S + V Photographs Real items	Fr, Fam, VC, SS – –	Included in regression analysis ✗ ✗	60/75 23/32 12/21	69/70 32/32 20/20	Reaction time data used for regression Within patient χ^2
Funnell and Davies, 1996	JBR	S + V	Fam, VC	✓	16/54	29/54	Within patient χ^2
Gainotti and Silveri, 1996	LA	Colour pictures S + V	checked a subset for Fam (but not reported) Fam and Fr.	 ✓	46/93 (40/58 without MI and food) 10/30	46/93 (40/58 without MI and food) 23/30	Within patient χ^2
Hart and Gordon, 1992	KR	S + V	Fr, Fam, VC	✓	7/15	29/30	Within patient χ^2
Hart et al., 1985	MD	Line drawings Colour drawings Photographs Real items	–	✗	12/22 9/13 23/36 23/36	11/11 18/18 222/229 11/11	No statistical analysis
Hillis and Caramazza, 1991	JJ PS	Line drawings Line drawings	Mean word length and syllables, Fr.	✓	42/46 21/58	12/98 77/86	Within patient χ^2
Kolinsky et al., 2002	ER	S + V Matched subset (Snodgrass) BW photos Colour photos	– all usual variables (17 in all) – –	✗ ✓ ✗ ✗	Animals (33%) f + v (30%) 36% 50% animals 39% f + v; body parts (100%) 26% animals 43% f + v	artefacts (71%) 83% 82% artefacts –	Within patient χ^2 Within patient χ^2 Compare to controls Compare to controls
Laiacona et al., 1993	FM RG	S + V	Fr, Fam, NA, IA, VC, Proto	Regression scores adjusted	6/30 4/30	21/30 22/30	L-NL difference in regression
Laiacona et al., 1997	LF EA	S + V	Fr, Fam, NA, IA, VC, Proto, difficulty index	Regression scores adjusted	16/30 1/30	26/30 13/30	L-NL difference in regression
Laiacona and Capitani, 2001	PL	S + V	VC, IA, Proto, Fam, Fr.	Logistic regression	37% (6.7% after year)	13% (0% after year)	Within patient χ^2 (after adjusting for variables)

Continued →

continued TABLE I

Details of category specific case studies

Study	Patient name	Stimuli type	Identified variables	Matched for identified variables?	Living	Nonliving	How was effect defined?
Lambon-Ralph et al., 1998	DB	S + V	–	Regression	49/84	110/135	
		Colour pix (after Gainotti and Silveri)	Fam Fr.	Pairwise matched	16/30	27/30	Within patient χ^2
		S + V	Fam, Fr, phon, IA	✓	19/32	29/32	
Laws et al., 1995	SE	S + V	Fam, VC, Fr.	*Post-hoc* testing	75/100	152/158	No statistical analysis
Mauri et al., 1994	Helga	S + V	Fam, VC and Fr.	✓	44%	78%	Within patient χ^2
Moss et al., 1997	SE	S + V	Fam	Checked matched subset	66/97 (51/82 without plants and body parts)	132/154	Within patient χ^2 & discrepancy greater than control maximum
		Photographs	Fam, VC	✓	58/69	44/69	Within patient χ^2
Moss et al., 1998	RC	S + V	–	✘	9% (excluded body parts)	50%	Within patient χ^2
		Colour photos	Fam, VC	✓	10%	49%	
Pietrini et al., 1988	RM JV	Not specified	–	✘	27/60 12/60	23/30 14/30	
Sacchett and Humphreys, 1992	CW	S + V	NA, IA, Fr.	✘	19/20	7/20	Within patient χ^2
Samson et al., 1998	Jennifer	Greyscale pictures	Fam, VC and Fr.	✓	4/18 animals 9/18 f + v	14/18 implements 14/18 transport	Within patient χ^2
Sartori and Job, 1988	Michelangelo	S + V Colour pictures	–	✘	28/85 38/59	131/175 47/61	Within patient fishers exact test
Sartori et al., 1993	Michelangelo	Line drawings (S + V and others)	Fam, VC, Fr	✓	12/40	28/40	Within patient χ^2
Silveri et al., 1997	CG	Colour pictures (amended S + V)	Word length, Fr, Fam	✓	28/34	22/31	Within patient χ^2
		S + V	Fr, Proto, Fam, NA	✓	36/40	31/40	Within patient χ^2
		Line drawings (S + V and others)	Fam, VC, Fr.	✓	22/40	15/40	Within patient χ^2
Silveri and Gainotti, 1988	LA	Colour pictures Colour pictures	– Fr.	✘ ✓	11/54 6/20	22/28 10/10	Within patient χ^2
Sheridan and Humphreys, 1993	SB	S + V	Fr, Fam, VC	*Post-hoc* testing	2/30	7/20	Within patient fishers exact test
Turnbull and Laws, 2000	SM	S + V	–	✘	50%	54%	Within patient χ^2 compared with undergraduates
		S + V (from Funnell and Sheridan)	Fr, Fam, VC, IA, AA	✓	20/31	16/29	
			Fam, Fr, VC	✓	11/11	10/11	
		Real objects/ toy versions	Fam, Fr, VC	✓	11/11	8/11	
Warrington and Shallice, 1984	JBR SBY	Photographs	Fr	✓	3/48 0/48	32/48 0/48	Within patient χ^2

Glossary: fr = name frequency (typically Kuçera-Francis); Fam = familiarity with name (typically from S + V); VC = visual complexity (from S + V); IA = image agreement (from S + V) is extent to which picture looks like imagined object; AA = age of acquisition (usually from estimates from S + V); NA = name agreement (percentage agreement over naming item from S + V); phon = number of phonemes; proto = prototypicality rating; SS = structural similarity ratings.

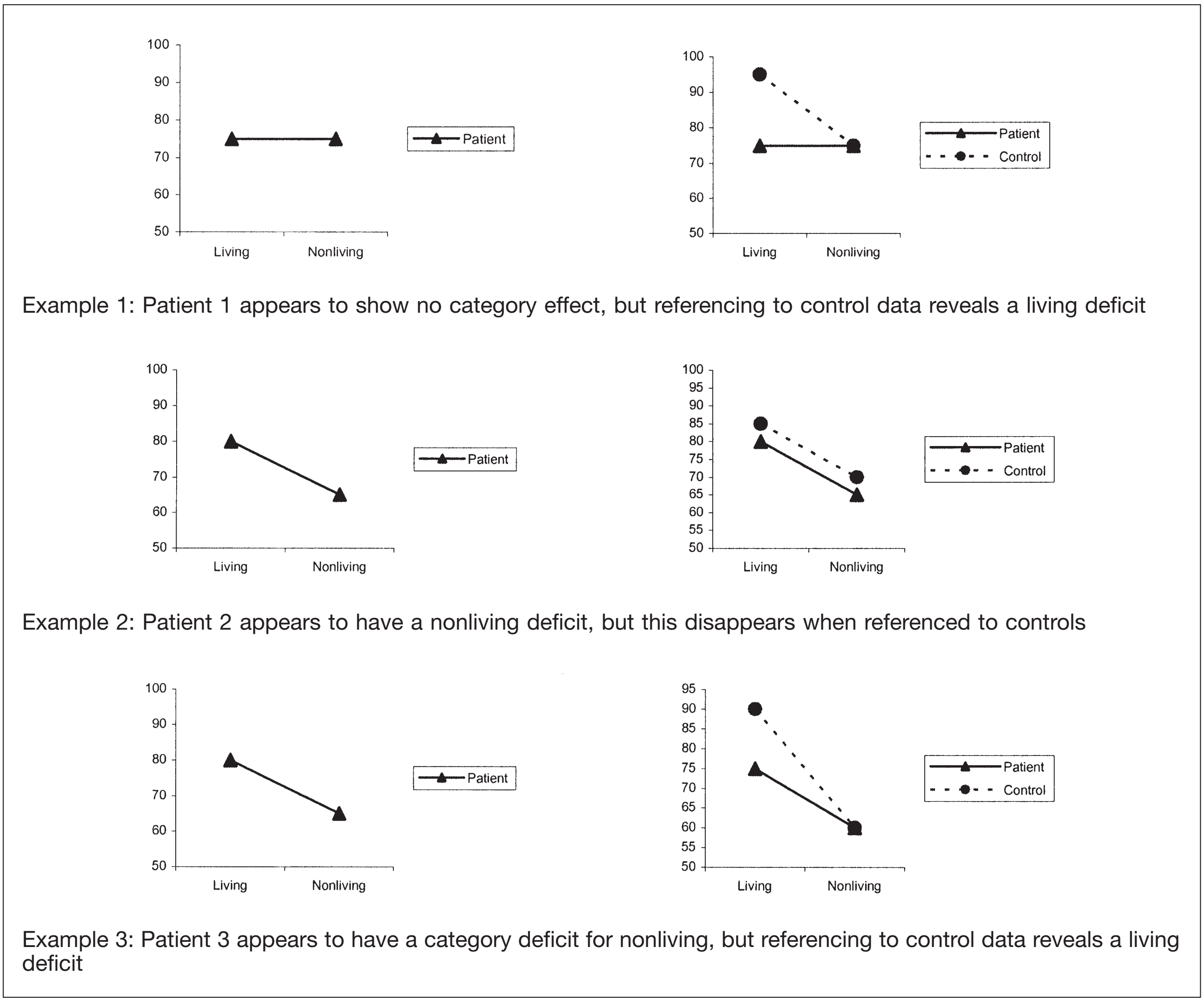

Fig. 1 – *Hypothetical examples showing how a lack of control data could distort the interpretation of category effects.*

the above-named variables cannot explain nonliving disorders. It is also conceivable that different variables act as naming moderators for normal subjects and patients and even perhaps at differing degrees of impairment (for a discussion see Laws et al., 2002)[2].

Problems of Within-patient Analysis

What are the consequences of failing to evaluate naming in normal controls? Consider the hypothetical examples outlined in Figure 1. In each case, the *absolute* difference reveals a quite different dissociation from that disclosed when a control comparison is included. Patient 1 shows no absolute difference, but when referenced to control data, exhibits a dissociation (false negative). Patient 2 appears to have a dissociation, but this disappears when referenced to controls (false positive); and finally, patient 3 seems to have a dissociation in one direction, but when referenced to controls actually has the opposite pattern of dissociation (paradoxical dissociation). Indeed, Laws et al. (2005) compared the impact of using and not using controls in category naming tasks with Alzheimer's patients and found individual cases with patterns of performance that corresponded to most of the hypothetical examples in Figure 1. Additionally, Laws et al. (2005) found that the commonly used within-patient analysis (using χ^2) produced far fewer significant deficits than between-group comparisons, pointing to a potential variant of the file-drawer problem in this literature. Hence, the *absolute* living-nonliving naming difference alone (be it exceptionally large or even non-existent) provides a misleading indicator of both the *presence* and the *direction* of category effects.

Spurious dissociations would, of course, contribute to spurious double dissociations. Indeed,

[2] Although patient studies now routinely address the differences in such variables via various techniques (matching, regression etc), this is an "eternal null hypothesis". Additionally, we might consider how much matching is required and how this might affect the stimulus choice in the two categories? For example, a recent study matched stimuli on 17 such variables (Kolinsky et al., 2002). This must have consequences for the validity and representability of categories (especially for nonliving things because they are usually chosen to match the familiarity, visual complexity and so on for living things).

they raise the possibility of bizarre inverse double dissociations: for example, a patient appearing to have a nonliving deficit (that is actually living) and another patient showing an apparent living deficit (who actually has a nonliving deficit). By contrast, others (like patient 1) would be ignored because of the lack of an absolute living-nonliving difference despite the fact that their performance is ab*normal*[3].

A Method to Empirically Demonstrate Dissociations

The interpretation of double dissociations has generated substantial debate (see forum in Cortex 39; Crawford et al., 2003); however, issues relating to the *empirical* demonstration of dissociations (and thereby double dissociations) have received far less attention. As outlined above, the widely-used within-patient χ^2 analysis is misleading and so, it is important to consider what might be a more appropriate method for documenting category dissociations.

Even where normal data are available, some analyses (e.g., *z*-scores with small normative samples) will overestimate the degree of impairment and inflate the Type I error rate (see Laws et al., 2005). This is because the statistics of the control sample are treated as population parameters rather than sample statistics. Additionally, it is possible for patients to be impaired at naming living or nonliving things, but that the *difference* between their scores fails to reach significance; conversely, a patient may be severely impaired on both tasks, but still show differential impairment. Therefore, for those patients showing impaired naming of living and/or nonliving things, it is also necessary to examine the living-nonliving discrepancy score by comparing this with the mean discrepancy score in a normative sample.

Fortunately, methods have recently been developed to overcome these difficulties and are outlined in detail elsewhere (see Crawford and Garthwaite, 2002; Crawford et al., 2003; Laws et al., 2005). Moreover, Crawford et al. (2003) have recently provided fully specified criteria for Shallice's (1988) classification of "strong" and "classical" dissociations (for examples see Laws et al., 2005; Laws and Sartori, 2005)[4]. A patient is considered to exhibit a *strong* dissociation if they were impaired at naming both living *and* nonliving and show a significant difference between the two scores. A patient would be considered to exhibit a *classical* dissociation if they are impaired at only living or nonliving naming and showed a significant difference between the impaired and intact category.

Control Data in the Category Specific Literature

What about when control data have been presented and analysed? One area of the category-specific literature where controls have more commonly been tested is that involving Alzheimer's patients. A review of category specific studies with Alzheimer patients, however, shows that most have controls who perform at ceiling (see Laws et al., 2005).

Ceiling effects in controls do, of course, distort analyses involving comparison with patients. Explicitly examining this, Laws et al. (2005) compared a series of DAT patients and matched controls on two stimulus sets: the typical Snodgrass and Vanderwart (1980) stimuli (Table I shows that the majority of studies have relied on this corpus) that produced ceiling effects in controls and another set that produced equivalent below-ceiling levels of naming for living and nonliving things. Laws et al. found that the presence of ceiling effects exaggerated the number of living deficits and underestimated the number of nonliving deficits. Moreover, *paradoxical* category effects occurred across different stimulus sets for the same patients i.e. living and nonliving deficits on different sets both for group and even individual patient analyses (Laws et al., 2003, 2005; see also Laws and Sartori, 2005).

What about the case studies that used controls? Of 33 case studies, only seven (21%) analysed patient naming data using control performance (six others had normal controls, but they were not used in any inferential analysis: see Table II). In three studies (with the vast majority of controls), the patients and controls were not matched for example on age, gender or background (e.g. Turnbull and Laws, 2000; Caramazza and Shelton, 1998; Laiacona et al., 1997). One study had control data, but did not present these data (DeRenzi and Lucchelli, 1994). Another did test naming, but controls performed at ceiling and were not used in the comparison (Kolinsky et al., 2002).

[3] Consider patient SE (Laws et al., 1995) was initially overlooked because of his comparable living and nonliving naming; however, further investigation revealed an underlying associative semantics deficit for animals. This raises an important issue about the common use of naming to define category effects. In fact, it is difficult to conceive of any *a priori* finding for determining a category disorder e.g. a category difference for: picture naming, attribute verification, naming to definition, fluency, drawing or any combination of these. It raises a critical tension between empirically and theoretically driven category effects. For example, we could decide arbitrarily that, for a category effect to be convincing, a patient must exhibit the same effect on three tests, A (picture naming), B (drawing) and C (attribute verification); however, another patient impaired on A and B and not C may tell us something about the true nature of category effects e.g. that they might be related to visual input (see Laws and Sartori, in press).

[4] Shallice (1988) originally proposed three main types of dissociation: *trend*, *strong* and *classical* and all are assumed to be documented with respect to control data. The *trend* dissociation is the weakest since it documents only a nonsignificant trend in the direction of dissociation (and the patient is impaired on both tasks); *strong* reflects a significant difference across the two tasks (but the patient is impaired on both); and *classical* where, compared to controls, the patient is impaired on one task and normal on the second task.

TABLE II
Studies that have tested control subjects

Study	Patient	N	Living %	Nonliving %	Notes
Pietrini et al. (1988)	RM/JV	10	96	96	No reported matching of stimuli
Sartori and Job (1988)	Michaelangelo	10	90.5 98	93.7 100	Within-patient analysis only; did not use control data in analysis
DeRenzi and Lucchelli (1994)	Felecia	10	Unreported	Unreported	No matching across domains on critical variables control data from the controls not presented
Mauri et al. (1994)	Helga	9	98	98	Within-patient analysis only; did not use control data in analysis
Laiacona et al. (1997)	EA/LF	60	67 worst control	87 worst control	EA was severely impaired on both categories at first test LF not significant at 2nd testing controls were unmatched to patients
Moss et al. (1997)	SE	12	84	91	Conflicts with Laws et al. (1995) who found no living naming deficit in same patient (see Laws, 1998 for possible explanations) controls in 1 of 2 comparisons
Caramazza and Shelton (1998)	EW	5	98	94	no matching of controls no matching details for stimuli used z-score comparison
Bunn et al. (1998)	JBR	40 8	93 95	96 95	Within-patient analysis only; did not use control data in analysis
Samson et al. (1998)	Jennifer	8	94 (animals) 86 (fruits)	83 (implements) 86 (vehicles)	Within-patient analysis only; did not use control data in analysis
Cappa et al. (1998)	GP	3	93.3	95.3	Within-patient analysis only; did not use control data in analysis
Moss et al. (1998)	RC	40	92.7	95.5	Within-patient analysis only; did not use control data in analysis
Kolinsky et al. (2000)	ER	4	98	99	Unmatched stimuli controls of "similar age and sociocultural background"
		4	98	98.5	matched set of stimuli (on 17 variables) controls of "similar age and sociocultural background"
		6	97.75	98.5	B +W photos Unmatched stimuli controls of "similar age and sociocultural background"
Turnbull and Laws (2000)	SM	39	79	67	much younger controls used a rapid presentation paradigm to overcome ceiling effects showed nonliving effect on matched sets and living one on non-matched sets
Laiacona and Capitani (2001)	PL	60	76.7	90	Within-patient analysis only; did not use control data in analysis

Note. This table shows that most studies have had controls performing at ceiling; while others have included controls and not used them in the analysis of patient performance.

One study (Moss et al., 1997) produced naming data that were contradicted by an earlier study of the same patient (Laws et al., 1995). This leaves one study that had 10 matched controls (Pietrini et al., 1988) but critically, control naming was at ceiling. So, not one single case study, examining category effects in patients, compares patient performance with data from *matched* controls on *matched* stimuli (with performance below ceiling).

Double Dissociations in the Category Specific Naming Literature

What are the implications for documenting dissociations and double dissociations in the category-specific literature? Given that a minority of category specific studies (7/33) have used control data, most reported dissociations would not fulfil the requirements for the weakest dissociation i.e. *trend* dissociation (which Shallice rejects as being insufficient to infer cognitive architecture whether it documents a putative *double dissociation* or not). Of those that do include controls, what might they tell us about category-specific dissociations and double dissociations?

One notable feature that emerges from reviewing category-specific studies concerns the manner in which double dissociations are empirically demonstrated. Rather than being double dissociations, they are in fact *complimentary dissociations.* As Shallice (1988) points out, it is often assumed that a double dissociation occurs "…when patient A performs task I significantly better than task II, but for patient B the situation is reversed", but that it is, however, "theoretically unsound to operationalise a double dissociation merely as two complimentary dissociations in two patients, as its dangerously misleading name suggests" (p. 234). Rather "…*the valid formulation of double dissociation* …is that on task I, patient A performs significantly better than patient B, but on task II, the situation is reversed" (p. 235). Indeed, purported *double dissociations* in the category-specific literature invariably reflect *complimentary dissociations*. In other words, most studies document that: patient A performs task I significantly better than task II, and contrast this with patient B for whom the situation appears to be reversed. This may be an unavoidable difficulty; Crawford et al. (2003) note that they are unaware of any inferential statistical method that would allow us to test whether patient A is significantly more impaired than patient B and vice-versa.

Fortunately, a reliance on complimentary dissociations is not as problematic as it may appear since, as Shallice (1998) concedes, the conditions in which such dissociations stem from resource artefacts are unlikely to occur with real data. In the category specificity literature, however, many of the putative dissociations do not even qualify strictly as complimentary dissociations. Instead they often take the following form: patient A achieves a significantly larger absolute score on test I than on test II*;* and patient B achieves a significantly larger absolute score on test II_a than on test I_a. In this case, tests I and I_a (II and II_a) are not the same, but assumed to be ostensibly similar tests. Nevertheless, tests I and I_a would invariably differ in many ways that could strongly influence performance, including differences in: presentation conditions; the stimuli themselves; the numbers of stimuli; the stimulus characteristics (e.g., familiarity, name frequency, visual complexity etc); resource demands; they may or may not be matched; they may contain different subcategories across different studies (e.g. animals may include or exclude sea creatures, insects, birds; living things generally may or may not include fruits and vegetables etc); and some may not include body parts or musical instruments (and some may consider them to be either living or nonliving)[5]. Hence, almost all attempts to demonstrate double dissociations in this literature are neither expressed on nor comparable on the *same scale*.

A Single Double Dissociation is all that's Required?

Despite the reservations outlined above, a *single* convincing double dissociation might suffice to demonstrate the fractionation of living and nonliving naming. It might also be argued that a double dissociation is only as good as the evidence for the less well-documented of the two dissociations. Since *nonliving* cases are in a minority (of 5:1), it could be argued that category-specific double dissociations are only as convincing as the evidence for the nonliving cases.

A search of the literature reveals 6 cases that present naming data from nonliving cases (see Table III)[6]. Of the six studies, three have controls but in two, the patient data were not referenced to the control data (which are at ceiling in any case: Cappa et al., 1998; Laiacona and Capitani, 2001). In the remaining study with controls (Turnbull and Laws, 2000) the data were unusual in being derived from the same stimuli but using a different paradigm (rapid presentation) and so control performance was below ceiling, but it comes from undergraduates

[5] It is notable that different studies deal with musical instruments and body parts in an *ad hoc* fashion. For example, see Table 3 where both are presented as nonliving things by Sacchett and Humphreys (1992); MI as nonliving and BP as living by Silveri et al. (1997); or include BP and not MI (Hillis and Caramazza, 1991); while others do not include either (Laiacona or Capitani, 2001).

[6] Some studies have documented nonliving disorders in aphasics, who are tested on word-picture matching and category sorting tasks (e.g. Warrington and McCarthy, 1987; Behrmann and Lieberthal, 1989). In these cases, any double dissociation would be documented with very different testing procedures i.e. poorer *matching* of nonliving versus poorer *naming* of living.

TABLE III
Studies documenting nonliving category naming effects

Study	Patient	Comments
Hillis and Caramazza (1991)	JJ	Impairment extends to fruit and vegetables and not therefore restricted to NL things (also relatively intact on vehicles) Did not include MI Living and nonliving stimuli matched for frequency and word length (but analysis compares animals and non-animals and these were not matched for any variables – though this is less problematic with PS and JJ showing opposite profiles) no controls
Sacchett and Humphreys (1992)	CW	Only 20 L and 20 NL stimuli included 2 Body Parts and 5 Musical Instruments in nonliving no fruit and vegetables stimuli unmatched no controls
Silveri et al. (1997)	CG	included Musical Instruments in NL and Body Parts in L no controls
Cappa et al. (1998)	GP	NL problem applies to tools only 3 age- and education-matched controls
Turnbull and Laws (2000)	SM	NL impaired only on matched sets impaired for L on unmatched sets 39 unmatched controls
Laiacona and Capitani (2001)	PL	Severely impaired on living and nonliving and at 'floor' on both when retested after 1 year (6.7% and 0%) 60 unmatched controls, who were not used in the analysis

(patient SM was over 80 years old) and ultimately was used for comparison rather than analysis.

Of the remaining three studies without controls, each has problems aside from not having controls. If we turn to the study by Hillis and Caramazza (1991) in more detail because this paper claimed to present a double dissociation across two patients (JJ and PS)[7]. Unlike other putative double dissociations, this study does test the two patients on the same materials[8]. Hillis and Caramazza (1991) argue that the profiles reflect a disproportionate impairment of animals in PS (or a relative sparing of non-animals) and a relative sparing of animals in JJ (an impairment of non-animals). While this study comes closest to fulfilling the requirements for documenting a double dissociation (albeit between animals and *non-animals*), the failure to reference the patient profiles to control data makes it impossible to provide an unambiguous interpretation of the patient data.

Conclusions and Recommendations

This paper highlights several previously ignored methodological issues concerning how we measure and define dissociations. In particular, it raises doubts about whether published category-specific cases have documented the deficits that are claimed because of their failure to include control data or using control data that is inappropriate, e.g,. at ceiling, unmatched. In this context, a review of past cases shows that many suffer from these problems; and therefore challenges conclusions about the purported empirical demonstrations of dissociations and double dissociations. This is not a denial of the existence of CSDs, but skepticism about the existence of any currently convincing empirical demonstrations of category specific double dissociations.

In addition to previously outlined criteria for examining category effects (e.g. matching of stimuli across category), researchers might also wish to consider whether their data meet – what appear to be – important minimal criteria required for the empirical documentation of a category-specific dissociation[9]. These criteria would include: that the analysis compares the patient with normal controls (who are adequately matched); that the controls perform below ceiling on the task; and finally, that the analysis involves a comparison of the (standardized) living-nonliving difference score against the distribution of difference scores obtained in the controls.

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[7] The one other study claiming to demonstrate a double dissociation is that of Gonnerman et al. (1997), who argue that type of category deficit relates to overall severity of illness in Alzheimer's patients. Nevertheless, they did not present any statistical analysis of their data and the 'nonliving' cases appear to be patients who could be showing a *normal* advantage for living things (and were not, in fact, impaired).

[8] Sartori et al. (1993) and Silveri et al. (1997) used the same materials to demonstrate a living and nonliving case respectively, but again no controls were used.

[9] These would be additional to other conditions relating to the stimuli e.g. matching across category.

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