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Generalization and Induction: More Misconceptions and Clarifications

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ABSTRACT

In 'Generalization and Induction: Misconceptions, Clarifications, and a Classification of Induction', we comment on Lee and Baskerville's (2003) paper 'Generalizing Generalizability in Information Systems Research', which attempts to clarify the concept of generalization and classify it into four types. Our commentary discusses the misconceptions in their paper and proposes an alternative classification of induction. Their response 'Conceptualizing Generalizability: New Contributions and a Reply' perpetuates their misconceptions and create new ones. The purpose of this rejoinder is to highlight the major problems both in their original paper and in their reply and to provide further clarifications. Lee and Baskerville's so-called 'new language' of describing research activities based on their concept of generalization is confusing. Their classification abuses the term 'generalize' and is self-contradictory. Hence, contrary to their claim, their classification and ours are not compatible. Also contrary to their claim that we advocate paradigmatic domination, our commentary is just about the correct use of terms such as 'generalize', 'induction' and 'deduction'.

Keywords: Research methodology, generalization, induction, deduction, Hume's problem of induction, paradigm

INTRODUCTION

With a laudable intention, Lee and Baskerville (henceforth, L&B) (2003: 221) try to 'clarify the concept of generalizability by critically examining its nature, illustrating its use and misuse, and presenting a framework for classifying its different forms'. Unfortunately, their attempt gives rise to a number of misconceptions. In a recently published commentary (Tsang and Williams [henceforth T&W], 2012), we clarify these misconceptions and propose an alternative classification of induction. To our surprise, L&B's (2012) reply not only perpetuates the misconceptions but also creates new ones. As shown by Table 1 of T&W (2012), IS researchers have cited and even built on L&B's (2003) misconceptions. It is likely that their reply will cause further confusion, especially concerning whether the two classifications are compatible and can be used interchangeably. The purpose of this rejoinder is to highlight the major problems of L&B (2003, 2012) and provide further clarifications.

Before we proceed, we summarize here two major problems of L&B's classification, which is supposedly the core of their contributions. To illustrate the first problem, consider the following account of a research project of a fictitious IS researcher, Dr. Smith, with each use of the word 'generalize' classified in parentheses based on L&B's four types of generalizability [i.e., Empirical Statement to Empirical Statement (EE), Empirical Statement to Theory (ET), Theory to Empirical Statement (TE) and Theory to Theory (TT)]:

> After receiving a handsome research grant, Dr. Smith started his research on user perception of the usefulness of enterprise resource planning (ERP) systems. Having conducted a thorough literature review, he *generalized (TT)* from concepts discussed in the literature to a theory of perceived usefulness of ERP systems. Then he successfully negotiated the access to a large company, which had recently implemented an ERP system, and conducted a questionnaire survey of a random sample of 100 managers drawn from the population of managers in the company. With full support from top

management, the response rate was 100%. In addition to other variables, he obtained a score of perceived usefulness from each respondent. He *generalized (EE)* from these 100 scores to a mean score of the sample, from which he further ***generalized (EE)*** to the population mean score. Since all of his propositions, which had been *generalized (TT)* from related concepts in the literature, were supported by his data, Dr. Smith concluded that his theory could be *generalized (TE)* to this company. Unfortunately, he later realized that he had forgotten to validate his questionnaire before carrying out the survey. In other words, 'the data collected from a research subject would lack *generalizability (EE)* to any valid measurement for that individual' (L&B, 2003: 234, emphasis and classification added). As a remedy, Dr. Smith conducted an ethnographic study in the company, interviewing and observing managers who used the ERP system. He tried to assess whether the answers given by his respondents and his observations of their behaviors could be '*generalized (EE)* into a valid, empirical statement' (L&B, 2003: 235, emphasis and classification added). His decision was that if the respondents' replies were believable, the data he collected 'would be *generalizable (EE)* to a valid descriptive statement' (L&B, 2003: 235, emphasis and classification added). Otherwise, the data would lack such *generalizability (EE)*.

In the above account, has the word 'generalize' been abused in the sense that with the exception of its underlined occurrence, it is used to represent meanings that are unrelated to its proper meaning? The answer is obviously yes, as T&W (2012) have painstakingly pointed out.

Another serious, and no less disturbing, problem of L&B's classification of generalizability is that it is self-contradictory. As L&B (2012: 752) admit in their reply, 'theoretical statements are, by definition, general'. On the other hand, empirical statements may refer to particular phenomena, such as 'Amazon is the largest online book seller in the world'. Accordingly, the word 'generalizability' in L&B's classification refers to inferring from something that may be particular to something that is necessarily general (ET generalizability) and at the same time (and contradictorily) refers to inferring from something that is necessarily general to something that

may be particular (TE generalizability). As T&W (2012) have highlighted, their classification also contradicts their definition of generalization. Without any intention of being disrespectful, we have to alert IS researchers that a self-contradictory classification using terms that have confusing meanings provides no useful guide for their work.

For concision, we will focus on L&B's (2012) reply that is directly related to our previous commentary. We appreciate L&B's (2012: 750-754) discussion of a new issue concerning the application of theory to a new setting. It is always recommended that IS researchers be cautious when applying a theory to a setting that is different from the one in which the theory was created. That said, in addition to the problem of the phrase 'Generalizing a Theory to a New Setting' (L&B, 2012: 750), their Table 3 clearly shows misuses of the term 'theoretical statement'. Only the statement in the first row is a theoretical statement. The other three are empirical statements. Once a theoretical statement is, as L&B put it, 'instantiated' in an empirical setting, it becomes an empirical statement. A good analogy is the following two statements:

Metals expand when heated. (theoretical statement)

This iron bar expanded when it was heated in the laboratory yesterday. (empirical statement)

This is another example of their frequent misuses of terms. More examples are discussed below.

A NEW LANGUAGE OR JUST CONFUSING LANGUAGE?

L&B (2012: 749) claim that they 'originally generalized generalizability by offering new language'. Unfortunately, as we point out above and elaborate below, their new language seriously distorts the proper meaning of 'generalize' and worse still, is self-contradictory.

Definition and misuses of 'generalization'

Our commentary points out that L&B's (2003: 221) definition of generalization as 'to form general notions by abstraction from particular instances', which they extract from the *Oxford English Dictionary* (*OED*), is not the sense used by researchers in natural or social sciences. In part D of the online supplement of their reply, L&B (2012: A6) claim that 'Contrary to T&W's assertion, L&B did not adopt, but reported the *Oxford English Dictionary*'s (2003) definition of generalization'. But what L&B (2003: 232) said was:

> Generalizing, according to the definition we cited at the beginning of this essay, refers to generalizing *from* particular instances *to* general notions. … By joining the two building blocks, we recognize that generalizing can occur in four ways: From empirical statements to other empirical statements, from empirical statements to theoretical statements, from theoretical statements to empirical statements, and from theoretical statements to other theoretical statements. Given the definition of generalize ('to form general notions by abstraction from particular instances'), the four different ways of generalizing indicate that the outputs of generalizing (the 'general notions') can be either theoretical statements or empirical statements, and the inputs to generalizing (the 'particular instances') can also be either theoretical statements or empirical statements.

The 'definition' in the first sentence of the above passage refers to the *OED* definition cited on the first page of L&B (2003). This clearly shows not only that they both reported and adopted the *OED* definition but also that the definition forms the foundation of their classification. Interestingly, L&B (2012) cite the following passage as evidence to support their claim because the passage shows that 'L&B even defined one of their four types of generalizability as being the opposite of the *OED* definition' (p. A6):

> Type TE generalizability, which involves generalizing a theory confirmed in one setting to descriptions of other settings – refers to reasoning from theoretical statements to empirical statements, which is actually deduction, not induction. Indeed, such reasoning is the opposite of the *OED*

definition of generalize, which is 'to form general notions by abstraction from particular instances'. L&B (2003: 241)

Instead of being a piece of supporting evidence, the passage is another example of the many self-contradicting arguments put forward by L&B. Even if we accept L&B's (2012) claim (i.e., they did not adopt the *OED* definition), one problem remains: they have defined the four types of generalization but not the stand-alone term 'generalization'.

Note that regardless of the various ways that researchers and laymen have used the word 'generalize', its meaning has a strong element of unidirectionality – from something particular to something more general and *not* vice versa. If this element is not preserved, serious semantic confusion will arise because the word's meaning is not just distorted but destroyed. Unfortunately, this is exactly the case for L&B's classification, as exemplified by their TE generalizability (i.e., inferring from something that is necessarily general to something that may be particular) mentioned above. To elaborate this point further, let us consider an analogy. Suppose a finance researcher has created a new language for describing stock market movements. Within this language, it is legitimate to use the word 'rise' to describe the following events:

The Dow Jones Industrial Average Index rose from 13,300 to 13,400 yesterday.

The Dow Jones Industrial Average Index rose from 13,400 to 13,150 today.

The Dow Jones Industrial Average Index rose 150 points during these two days.

Is this a good language?

In sum, we agree with L&B's (2003: 241) observation (except the phrase in parentheses) that 'researchers have been using (or, in effect, generalizing) the term "generalizability" to refer to many different concepts, some of which go beyond the *Oxford English Dictionary*'s definition and have caused considerable confusion'. Unfortunately, despite their good intention, their classification has aggravated such confusion.

Conflating induction with deduction

Our commentary clearly explains the difference between deduction and induction using the basic terminology of logic (T&W, 2012: 731), and points out that contrary to L&B's (2003: 233-234) claim, 'a sample estimate is *deduced* and not induced from sample points' (T&W, 2012: 735). L&B's reply perpetuates this error by placing this phrase 'Generalizing from sample points to a sample statistic' in their Table 4, without responding to our comment. Their new language uses the terms 'induction' and 'deduction' confusingly and self-contradictorily. For example, on the one hand, they state that generalization is one form of induction: 'To critique statistical generalizability, we will first examine inductive reasoning, of which statistical generalizing is a form' (L&B, 2003: 224). On the other hand, 'Type TE generalizability … is actually deduction, not induction' (L&B, 2003: 241). After all, is generalization deduction or induction?

L&B (2012: 759) even make this claim: 'We mainly anchored our perspective to the philosophy of science'. We would recommend that L&B read Chapter 4 titled 'Deriving Theories from the Facts: Induction' of Chalmers' (1999) *What Is This Thing Called Science?*, a bestselling introduction to the philosophy of science, and see how philosophers of science use the terms 'induction' and 'deduction'. Among the hundreds of articles and books of the philosophy of science that we have read, we have not encountered a single author who uses the terms 'induction', 'deduction' and 'generalization' in a way that is similar to L&B's new language. (Perhaps that's why it is a new language.) Appendix A lists more examples of their misuse of other basic logical terms, and Appendix B clarifies the new misconceptions about Hume's problem of induction in L&B's (2012) reply.

To summarize, L&B's new language is confusing and self-contradictory. Since their classification has destroyed the meaning of 'generalize', it makes more sense for them to create a new word to replace 'generalize' if they still want to keep the classification.

ARE L&B'S AND T&W'S CLASSIFICATIONS COMPATIBLE?

L&B (2012: 755) argue that 'L&B's four types of generalizing allow for T&W's five types of generalization, just as the latter allows for the former'. Given the fact that their classification abuses the word 'generalize' and is self-contradictory whereas ours does not have these problems, it is logically impossible that two classifications 'are mutually compatible' (L&B, 2012: 758).

As we admit in our commentary, theoretical generalization in our classification 'is similar to Lee and Baskerville's Type ET generalizability' (T&W, 2012: 740). However, the similarity ends here. We find repugnant their claim that our within-population generalization 'is a special case of L&B's EE generalizing' (L&B, 2012: 758) because the latter also includes generalizing from sample points to a sample statistic and thus conflates induction with deduction (T&W, 2012). We prefer to keep a distance from a classification type that makes no sense.

For the sake of discussion, let us ignore for the moment that TE generalizing contradicts the meaning of generalization. It is perplexing that L&B (2012) relate our cross-population generalization, contextual generalization and temporal generalization to their TE generalizing for the simple reason that, as shown by Figure 1 of T&W (2012), the former do not involve theory whereas the latter does. For example, cross-population generalization is defined as 'generalizing from a sample in one population to members of another population, with both populations existing in a similar context and a similar period of time' (T&W, 2012: 741). When illustrating this kind of generalization with Gefen and Straub's (1997) study, we state that 'cross-population

generalization might consist in generalizing the results of the American airline (in which Gefen and Straub collected data) to another American airline that existed at the time of data collection' (T&W, 2012: 741). Adopting L&B's terminology, it is about generalizing from one empirical statement to another empirical statement, and should belong to their EE generalizing. In fact, all the four types of generalization – within-population, cross-population, contextual and temporal – are essentially the same insofar as they do not involve theory.[1] Hence, it is logically inconsistent that L&B relate within-population generalization to EE generalizing and the other three to TE generalizing.

Finally, L&B (2012: 758) use the following reasoning to argue that our classification allows for TT generalizing:

> ... Gefen and Straub, whose work T&W use as an illustration, performed TT generalizing in their derivation of new theoretical statements (their extension of TAM) from other theoretical statements (the prior literature on TAM and other research literature, such as literature on gender and culture). To the extent that T&W use Gefen and Straub's theory, T&W's generalization framework allows for TT generalizing.

So the logic goes like this: using their classification, L&B claim that Gefen and Straub performed TT generalizing and that since we use Gefen and Straub's study as an example to illustrate our classification, our classification allows for TT generalizing. With all due respect, we have to say that an argument of this quality will not contribute to a meaningful intellectual dialogue.

[1] A question may arise: what is the purpose of making these kinds of generalization without theorizing? One key purpose is to determine how far a research finding represents an empirical regularity. Although the ultimate goal of scientific research is to develop theory that can explain the phenomenon in question (i.e., theoretical generalization), there have been discoveries of empirical regularities that alone contributed to the well-being of mankind. The discovery of penicillin by Alexander Fleming in 1928 is a good example. The discovery helped save many lives during the Second World War. Yet the molecular structure of penicillin was determined by Dorothy Hodgkin only in 1945, and the mechanism that explained its healing effect was gradually understood even later (Lax 2004). In other words, penicillin was in use even before there was a theoretical explanation of its therapeutic effects.

PARADIGMATIC DOMINATION?

L&B (2012: 759) summarize the differences between their classification and ours as: 'Compared to T&W's ideas on generalizability, the ideas presented earlier by L&B and now by us in this essay are more general, as it were, and are compatible with qualitative research, interpretive research, and design research, not just statistical and positivist research'. Moreover, they view the two classifications as two different paradigms and lament that 'no paradigm need dominate another' (L&B, 2012: 760) and that 'nothing constructive comes from yielding to the temptation of disparaging an idea from a different paradigm as a misconception' (L&B, 2012: 760).

There are several serious problems with their view. First, we are perplexed as to why they label our classification as positivist, a term that we do not use to describe our classification. They fail to provide a rationale. Fortunately we manage to pick up some clue from this comment: 'all of T&W's types of generalization are compatible with L&B's four types of generalizing, which are not restricted to positivist, statistical, and quantitative forms of research' (L&B, 2012: 758). The implication is that our classification is 'restricted to positivist, statistical, and quantitative forms of research'. This is a grave mischaracterization of our classification, which allows for both qualitative and quantitative research. In fact, we refute a commonly held view that results of qualitative research (such as case studies) are necessarily less generalizable than those of quantitative research (such as surveys):

> The findings of a case study are surely less generalizable to the population on which it is based, than the findings of a large-scale random sample survey. But even in this case, the generalizability of survey results depends a great deal on whether a probability sampling method is employed to construct a representative sample.... For the other four types of generalization, there is simply no

reason why survey results should be inherently more generalizable than case study results. (T&W, 2012: 743-744)

Second, L&B's view of positivism is simplistic, if not naive, because a statistical, quantitative research orientation does not necessarily imply that it is positivist. As Phillips (1987: 96) well says, 'A positivist, *qua* positivist, is not committed to any particular research design. There is nothing in the doctrines of positivism that necessitates a love of statistics or a distaste for case studies'. The distinctive features of positivism, such as the verifiability principle and the stress on formal logic, do not mandate the use of quantitative research methods.

Finally, L&B (2012) use the term 'paradigm' casually, without being aware that even Kuhn (1962) himself failed to clearly define the term (Masterman, 1970). We do not consider our classification a paradigm, an ambiguous term that we would avoid. Instead, our classification is just a classification of induction, nothing more, nothing less. L&B (2012: 760) accuse us of 'paradigmatic domination', which they denigrate as unconstructive, disrespectful, immature and uncivil. But even if we were engaged in paradigmatic domination, L&B should have welcomed it because we would then be in the 'good company' of Kuhn (whom L&B [2012: 755] also claim to be in 'good company' of), since urging the overthrowing of old paradigms by new ones certainly counts as paradigmatic domination. Indeed Kuhn thinks that paradigm wars are needed for progress in science.

But in fact our commentary has nothing to do with paradigmatic domination but is only about the correct use of a term. It is constructive to observe that saying something like 'generalizing from sample points to a sample statistic' (Table 4 of L&B [2012]) is a serious misuse of the word 'generalizing' because a sample statistic is *deduced* from sample points. Likewise it is constructive to point out that calling an inference that goes from something general to something particular a 'generalization' is an outrageous abuse of the word because it destroys the word's

meaning. In brief, our commentary is constructive because the result is clarity and the avoidance of conceptual confusion. There is nothing disrespectful, immature or uncivil in insisting on the correct uses of words. By L&B's logic, objecting to Humpty Dumpty's use of 'glory' to mean 'a nice knock-down argument' (Carroll, 1871) legitimizes his reply that the objector is guilty of ' paradigmatic domination' and is being unconstructive, disrespectful, immature and uncivil.

CONCLUSION

The above discussion further explains the nature of some of L&B's (2003) problems and clarifies the new misconceptions in L&B's (2012) reply. We would like to conclude this rejoinder by quoting verbatim below a comment made by a reviewer of T&W (2012) because it nicely summarizes our view and feeling towards L&B's whole endeavor:

> … To me, the Lee & Baskerville paper is about de-emphasizing statistical generalizability in order to make way for other forms of legitimate empirical findings. Beyond arguing for the representativeness of a sample used in a study – or the related limitations – Lee & Baskerville argue that it is equally legitimate to use empirical data to generate theory, validate an instrument, richly describe a situation, etc., without necessarily feeling that they have to make claims regarding the representativeness of a sample.
>
> In doing so, they make two unfortunate decisions. First, they appear to spend a significant amount of space using Hume's problem of induction to argue against the common conception of sample-based generalization, and they put forth an awkward alternative argument involving increased reliability of measures (as you rightly point out, apparently not realizing that the same critique applies to that perspective, as well). Secondly, they offer a framework with four quadrants – arguably grouping things together that do not necessarily belong together, and referring to things as forms of generalizability that, as you indicate, are typically not characterized so (particularism, abstraction, etc.). My guess is that they do this to directly address the need they perceive to address

generalizabilty in all case-based research. With this matrix they are giving authors and reviewers a vocabulary to deal with generalizability of case-based findings, without necessarily focusing on only one form of statistical generalizability.

While I understand, and to some extent sympathize with their effort, I wonder why they felt the need to cloak all forms of research value in terms of generalization, rather than focusing directly on the ways that different forms of research can be valuable. … it seems to me that the interests of case-based researchers are already well-served by methodological research in general, as well as a number of methodological articles in *MISQ* (some of which were written by Lee – such as his classic 1989 article)….

Last but not least, our concern is that if the IS discipline adopts L&B's new language of generalization – such as generalizing from sample points to a sample statistic and generalizing from concepts to theory – IS researchers will face a hard time of communicating with researchers outside the IS discipline, not to mention gaining the latter's respect.

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APPENDIX A. MISUSING LOGICAL TERMS

In addition to misusing the terms 'generalize', 'induction' and 'deduction', L&B have misused other basic logical terms. For example, L&B (2003) often use 'valid' as a synonym of 'true', and 'validity' as a synonym of 'truth value' or 'truth'. Two examples are: 'the conclusion in any syllogism can be valid only if its major premise is valid' (p. 225) and 'to establish the validity of the uniformity of nature premise' (p. 225). Any textbook on logic will explain that validity is a property of the relationship between the premises of an inference and its conclusion. Truth, on the other hand, is a property of a statement (such as premises and conclusion) and is commonly taken to consist in a correspondence between that statement and the real world. Accordingly, many philosophers hold that a statement 'p' is true just in case it is a fact that *p* (Moore, 1959; Russell, 1906). So it makes no sense to say that an inference is true (or false) or that its premises or conclusion are valid (or invalid) (see Copi and Cohen, 1998).

Consider another example. In part E of the online supplement of their reply, L&B (2012: p. A6) discuss the following statistical syllogism:

P1 75% of the women working at Golden Triangle Corporation are secretaries.

P2 Carol is a woman working at Golden Triangle Corporation.

C Carol is a secretary.

L&B now claim that 'to the extent that "Carol is a secretary" does not necessarily follow from the two premises (P1 and P2), one may argue that the logic leading to this "conclusion" (C) is not well described as the logic of the syllogism' (p. A6). This does not follow, because although the argument is inductive and so aims to be strong rather than valid, the fact remains that, as we

point out in footnote 7 of T&W (2012), it 'has two premises and so is *ipso facto* a syllogism, albeit not a categorical syllogism'. A syllogism is just a two-premise argument. Some syllogisms are deductive, such as categorical syllogisms, while others are inductive, as is statistical syllogism.

L&B (2012: p. A7) then add that 'the logic of the syllogism does apply in statistics' and give the following example: 'The major premise (P1) is "if H_0 is true, then p-value > α", the minor premise (P2) is "p-value < α", and the conclusion (C) is "therefore, reject H_0 as true"'. The problem is that these premises cannot be called 'major' or 'minor'. These are terms used to describe the premises of a *categorical* syllogism. A categorical syllogism is a two-premise argument in which the premises and the conclusion are one of the standard categorical forms 'All S are P', 'No S are P', 'Some S are P' and 'Some S are not P' and which contains three terms, each of which appears exactly twice. The major premise is the premise that contains the predicate term of the conclusion. The minor premise is the premise that contains the subject term of the conclusion. For example, the following is a categorical syllogism:

Major premise: All IT managers are people that have college degrees.
Minor premise: All the people in this room are IT managers.
Conclusion: All the people in this room are people that have college degrees.

The major premise is so-called because it contains the term 'people that have college degrees' which is the predicate term of the conclusion. The minor premise is so-called because it contains the term 'the people in this room' which is the subject term of the conclusion. Such errors indicate that L&B have little knowledge of the basic terminology of logic.

APPENDIX B. MISUNDERSTANDING HUME'S PROBLEM OF INDUCTION

Despite the fact that that our commentary has explained in detail the nature of Hume's problem of induction and L&B's (2003) misunderstanding of the problem, their reply still reveals serious misunderstandings. Even worse, their response to our balance-of-evidence argument that there is a solution to Hume's problem of induction commits the straw man fallacy. Once again, we clarify their misconceptions below.

Have L&B escaped Hume's problem of induction?

L&B (2003) claim that although Hume's problem of induction cannot be solved, nonetheless 'a larger sample size does increase generalizability, but it is the generalizability of a sample to other samples, not to the population' (L&B, 2003: 227). In our commentary, we explain why their claim does not hold up. In response, L&B put forward a mathematical explanation, extracted from a standard statistics textbook, to support their claim in part B of the online supplement, and conclude: 'the claim that a larger sample size increases the number or percentage of other samples also leading to the same result (i.e., the same result of including the population parameter between the bounds of the confidence interval or the same result of making the correct decision not to reject the null hypothesis) does not involve induction' (L&B, 2012: A4).

Their reply is both perplexing and flawed. First, the mathematical explanation or proof is completely irrelevant. Second, their claim consists of the following two elements:

a) Inference from the characteristics of a sample of a certain size to other samples of the same size.
b) The larger the sample size, the more accurate the inference will be.

As we argue in our commentary, the first element is an inference from what we *have observed* to what we have *not observed*, and thus is induction. Even if we adopt the definition of induction in their original article – 'a reasoning process that begins with statements of particulars and ends in a general statement' (L&B, 2003: 224), the first element is still inductive because it is about reasoning from one sample (a particular instance) to other samples (something more general).

L&B's position with respect to Hume's problem of induction

L&B's attitude toward Hume's problem of induction is as self-contradictory as their use of the term 'generalize'. L&B (2003) used the term 'Hume's truism' 22 times (not including the one in their citation of Campbell and Stanley [1963] on p. 224) in their discussion. Since they regarded Hume's argument as truism and generalization, as they claimed (L&B, 2003: 224), is one form of induction, why did they propose a classification of generalization, which would go against the truism, in the first place? They argued that 'as a consequence of Hume's truism, a theory may *never* be generalized to a setting where it has not yet been empirically tested and confirmed' (L&B, 2003: 241), but included Type TE generalizability in their classification.

In their reply, L&B (2012: 754) tried to retreat to a softer position: 'The position adopted by L&B was *not* that "induction is not justified or warranted *in any sense*," but instead, that "*induction or generalization is never fully justified logically*," as stated by Campbell and Stanley (1963, p. 17) and as quoted by L&B'. First, there is no evidence that this was L&B's (2003) position. To the contrary, there is clear evidence that L&B (2003) quoted the phrase to describe Campbell and Stanley's position: 'The enormous significance of Hume's truism leads Campbell and Stanley (1963) to take the positions that "*induction or generalization is never fully justified logically*"' (p. 225). Second, since L&B (2003) used the term 'Hume's truism' over 20 times, it

makes more sense to consider that as their position. As such, we are justified in arguing that L&B (2003) were committed to the position that 'induction is not justified or warranted *in any sense*' (T&W, 2012: 732). This is because had L&B (2003) deemed that induction were justified in some sense, they would not have used the term 'Hume's truism' so many times in their paper and would not have described the problem of induction as 'irremediable' (p. 224). We do not object L&B's (2012) adoption of a position that is different from L&B's (2003). What we object is L&B's (2012) claim that their new position was also L&B's (2003).

Overturning their previous view that the problem of induction is irremediable, L&B (2012) now find themselves in 'good company' of Kuhn (1962) in relying on '"social process" to supplement, and thereby contribute to the justification of, reasoning' (p. 755). They appear to challenge the assumption in Hume's argument that any justification must be inductive or deductive, and argue that the practice of induction is a social practice that justifies it. They claim that this stance is consistent with Hume's. Even leaving aside the troubled question of whether Kuhn is good company, this attempted solution fails. That induction is part of a social practice won't justify it because forecasting weather by divination of bones is part of social practice in some tribes. So is purging the body of vital spirits or telling horoscopes. These are bad science. Superstitions are part of social processes but remain unjustified ways of reasoning. As for Hume's position, footnote 3 of T&W (2012) describes a view that is different from L&B's:

> What Hume certainly thought is that our use of induction is a psychologically engrained habit – as he puts it, a 'custom' – that we cannot shed. We cannot refrain from causal inference any more than we can refrain from breathing. But a skeptic could reply that this means that human nature dooms us to irrationality, because we cannot help that our thought processes run in a direction that is unjustified.

Straw man fallacy

In part C of the online supplement of their reply, L&B attack our balance-of-evidence argument that there is a solution to Hume's problem of induction with three objections. Their main objection is that we 'sweep past Hume's problem of induction with two arguments' (p. A5), neither of which succeeds. The first of our arguments is supposed to be that 'all past observations of attempted solutions to Hume's truism have been observations of attempts that failed; therefore the next observation will be an observation of failure; however, there is no uniformity of nature; therefore because all past observations were observations of failures, the next observation will be an observation of success (or the one after that, or after that, etc.)' (p. A5). L&B point out that this fails because it uses higher-order induction. Our second argument is supposed to appear when we 'recite' an argument based on Strawson (1952) that 'draws forward the notion that evidence can mount probabilistically to provide justification for acting on beliefs about the future' (p. A5). L&B then attack this argument in two ways.

This is the straw man fallacy. The fallacy consists of distorting the argument of one's opponent and then attacking the distorted version to give the impression that the opponent's argument has been demolished. In our commentary, we gave one argument, not two. We first showed that there can be no evidence that there is no solution because it is impossible to list in advance all putative solutions. Next we argued that at a minimum, there is one solution – and perhaps more – that has some degree of plausibility. It follows that there is more reason to think that there is some successful solution out there than there is reason to think otherwise, even if we don't know what solution it is. This is nothing like the silly argument that L&B (2012) attribute to us.

As part of the second stage of our single argument we defended the claim that at a minimum, there is one solution – and perhaps more – that has some degree of plausibility. We did this, not by 'reciting' Strawson's (1952) solution, but by giving an original defense of it and by showing that Reichenbach's (1938, 1949) solution protects our original defense against objections, Moreover the argument that L&B (2012) attribute to Strawson before attacking it has no resemblance to the argument Strawson actually gives. This was that to ask whether we should form beliefs on the basis of inductive evidence is tantamount to asking the trivial question, 'Are we justified in being justified?' Trivially, the answer is yes. Once this is understood, Hume's problem is no problem at all. Thus L&B (2012) have not only committed the straw man fallacy against us in misrepresenting the actual argument we gave but have also committed the fallacy against Strawson.