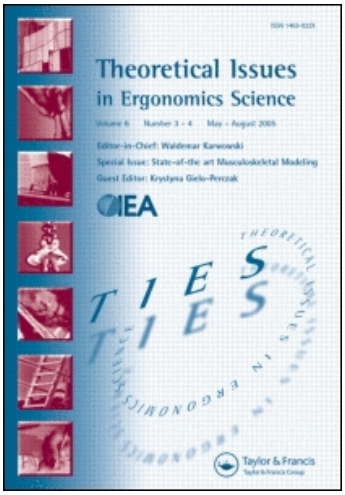

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The role of auxiliary assumptions in the falsification of ergonomics theories

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Based on the accusations that some important ergonomics theories are not falsifiable, the present article reflects on the issue in a different way than has been previously presented by ergonomics researchers. This discussion highlights the importance of auxiliary assumptions in deriving testable predictions and emphasises the complications that result from understanding falsification. These arguments are made more concrete by considering them in the context of an important ergonomics theory that has been the target of falsification-related accusations – Multiple Resource Theory. Finally, the implications of this specific discussion for larger philosophical issues relevant to the falsification of ergonomics theories are explicated.

Keywords: Popper; falsification; resource; theory; ergonomics

1. Introduction

It sometimes happens that theories in ergonomics receive a great deal of attention, are neither proven to be true nor proven to be false to everyone's satisfaction and are finally deemed to not be capable of falsification. The present goal is to examine claims about the potential testability of ergonomics theories in a critical way that emphasises the importance of auxiliary assumptions in theory falsification. Such a critical examination makes clear that although there is no such thing as absolute falsification, it is possible to have falsification within the context of one's auxiliary assumptions. Within these limits, it is argued that all theories in the ergonomics domain are potentially falsifiable. As an example, this reasoning is applied to what is clearly one of the most famous theories in the domain of ergonomics—Multiple Resource Theory (MRT) (Kantowitz and Knight 1976, Navon and Gopher 1979, Wickens 1980, 2002).

2. The importance of auxiliary assumptions in falsification

Although the concept of falsification has been around at least since Galileo's experiments falsified Aristotle's theory of motion, it was popularised in the twentieth century by the writings of Sir Karl Popper (e.g. 1959, 1972, 1983). At the risk of oversimplification, Popper's central point depended on contrasting the valid logic of Modus Tollens with the invalid logic of Affirming the Consequent. First consider the Fallacy of Affirming the Consequent in the context of a theory (T) and an observation (O). Imagine that someone

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uses the theory to predict an observation, the observation works out and so the researcher concludes that the theory is true. Clearly, there is a problem here because the observation might have worked out for a reason other than the truth of the theory.

The issue can be stated more succinctly in syllogistic form, where there is the following invalid logic:

{Premise 1}	$(T \rightarrow O)$	[If T is true then O is true.]
{Premise 2}	O	[O is true.]
{Invalid Conclusion}	T	[T is true.]

As Popper noted, if one wishes to make predictions from theories, test them with data and then draw logically valid conclusions, it is necessary to use the logic of Modus Tollens. That is, if the observation does not work out, then one can validly conclude that the theory is false, as is demonstrated with the valid syllogism below.

{Premise 1}	$(T \rightarrow O)$	[If T is true then O is true.]
{Premise 2}	$-O$	[O is not true.]
{Valid Conclusion}	$-T$	[T is not true.]

Given, then, that verification is invalid but that falsification is valid, Popper concluded that scientists should falsify rather than verify. But, of course, one cannot falsify theories if the theories under investigation are not capable of being demonstrated to be wrong. So the number one requirement of a scientific theory is that it be capable of being falsified and subject to the ruthless logic of elimination that is engendered by Modus Tollens. Any theory that is not subject to this logic is not scientific.

In actuality, Popper recognised that matters are not quite this simple but it was one of his students, Imre Lakatos (1970, 1978), who made the clearest statement of the complications that are inherent in the process of falsification. According to Lakatos, scientists make predictions not only from theories, but from a combination of theories and assumptions that are not included in theories. These added assumptions are called auxiliary assumptions. For example, it is often said that Newton's laws of motion can be used to predict the orbits of the planets. Strictly speaking, this is clearly not so, as Newton's laws say absolutely nothing about any of the planets. Rather, one can make auxiliary assumptions (hopefully backed by good measurement) about the present speeds, positions and masses of the planets. When combined with Newton's theory, these auxiliary assumptions allow scientists to make predictions about the future positions of the planets. Thus, Newton's theory truthfully can be said to make predictions about the positions of the planets only when combined with appropriate auxiliary assumptions.

To understand how auxiliary assumptions complicate falsification, consider the following valid syllogism with two noteworthy characteristics. First, the major premise assumes that predictions about observations come from the conjunction of a theory and auxiliary assumptions. Second, the conclusion is that either the theory is wrong or at least one auxiliary assumption is wrong. So, although something is falsified, it is not clear whether it is the theory or at least one of the auxiliary assumptions.

{Premise 1}	$((T \,\&\, A_1 \,\&\, A_2 \,\ldots\, A_n) \rightarrow O)$	[If T and A_1–A_n are true then O is true.]
{Premise 2}	$-O$	[O is not true.]
{Conclusion}	$-T \mathrm{v} - A_1 \mathrm{v} - A_2 \ldots - A_n$	[T or A_1 or A_2 … or A_n is not true.]

Supposing that the observation is not in accord with the prediction, it might seem unscientific to blame an auxiliary assumption rather than the theory. In fact, there are

many examples on both sides. For example, when an experiment by Michelson and Morley (1887) failed to support the existence of luminiferous ether, Einstein assumed that the auxiliary assumptions used in conducting the experiment were right, that the theory that assumed the existence of luminiferous ether was wrong, and successfully replaced the old theory with a new one. On the other hand, when observations were inconsistent with some of Newton's predictions about planetary orbits, Newton argued that the theory was right but that the auxiliary assumptions were wrong. In particular, he argued that there must be other astronomical bodies that had not been detected, which influenced planetary orbits. So here is an example where a scientist (Einstein) was right to blame the theory rather than the auxiliary assumptions for a failed prediction, but there is also an example where a scientist (Newton) was right to blame auxiliary assumptions rather than the theory for a failed prediction. These opposite examples highlight the falsification dilemma that Lakatos so clearly explained: is it the theory that has been falsified or merely one of the auxiliary assumptions that was used to help derive the prediction? Obviously, the fact that this question cannot be answered with 100% confidence in any particular case means that absolute falsification of theories is impossible.

One way to address this dilemma is to admit the impossibility of absolute falsification, but to instead argue for some sort of 'reasonable' falsification. For example, if the same theory, when combined with a variety of different sets of auxiliary assumptions, consistently results in wrong predictions, it becomes increasingly plausible that it is the theory that is at fault rather than the auxiliary assumptions. Or, if increasingly complex and implausible auxiliary assumptions have to be generated to 'save' the theory from wrong predictions, then this is clearly a problem for the theory. It is reiterated, however, that even in these cases, it remains possible, even if not plausible, that the theory is nevertheless true. Consequently, it makes no sense to insist on absolute falsification and it seems clear that scientists will have to settle for a lesser standard – some kind of 'reasonable' falsification.

But auxiliary assumptions are important to falsification in yet another way that has not received much attention, particularly in the ergonomics literature. Specifically, consider again that to falsify a theory, one must have auxiliary assumptions to combine with the theory; it is the conjunction of the theory and auxiliary assumptions that leads to a prediction. But which auxiliary assumptions need to be combined with the theory to obtain a testable prediction? Suppose that one set of auxiliary assumptions is combined with a theory and no testable prediction results. This does not prove the theory to be incapable of falsification because some other set of auxiliary assumptions, if combined with the theory, might result in a testable prediction. In fact, the only way to prove that a theory is not falsifiable is to combine it with all possible sets of auxiliary assumptions to determine if a testable prediction results from one or more of the conjunctions. It is only if none of the infinite number of possible conjunctions results in a testable prediction that the theory truly can be said to not be falsifiable. Obviously, it would be impractical (or impossible) to carry out this exercise in actual scientific research.

The foregoing considerations cause an interesting dilemma for a researcher who wishes to emphasise the falsification of theories. If the chosen standard is absolute falsification, then the necessity of combining theories with auxiliary assumptions to make predictions renders all theories as being not falsifiable. Also, if the chosen standard is reasonable falsification, then any theory might be capable of being falsified if only one knew which auxiliary assumptions to combine with it to generate a testable prediction. In summary, under the criterion of absolute falsification, all theories fail to be falsifiable whereas, under the standard of reasonable falsification, every theory is, in principle, falsifiable.

Thus, whether the chosen standard is absolute or reasonable, theories cannot be distinguished from each other on the basis of their potential for falsification.

3. Multiple Resource Theory

Thus far, the discussion has been very abstract. To make it more concrete, the issue of auxiliary assumptions will now be considered in the context of MRT (Kantowitz and Knight 1976, Navon and Gopher 1979, Wickens 1980, 2002). It is important to note that MRT is referred to merely as an example of a theory that has been subject to criticism of falsification issues. It is not the purpose of this paper to focus solely on, or defend, MRT, but rather to use this example as a way of making the point about ergonomics theories in general.

In brief, proponents of MRT assert that people have different pools of resources and that each pool can be devoted to different tasks. The model highlights four dimensions that 'account for variance in time-sharing performance' (Wickens 2002, p. 163). These dimensions include the stages of processing (perception, cognition, responding) (Isreal *et al.* 1980), perceptual modalities (visual vs auditory) (Sarno and Wickens 1995), visual channels (focal vs ambient) (Leibowitz and Post 1982, Previc 1998) and processing codes (spatial vs verbal) (Wickens and Liu 1988, Liu and Wickens 1992). Wickens (2002) proposes that when two tasks share the same level of a particular dimension, then time-sharing between tasks is more difficult than when the two tasks require different levels of the same dimension. For example, combining an auditory task with a visual task should result in more efficient time-sharing relative to combining two visual tasks, assuming the secondary task is of equal difficulty.

The notion of different resource pools implies that it is possible to perform tasks more efficiently in parallel as long as the two tasks require different levels of the proposed dimensions. In a typical experiment, then, people might be asked to perform two concurrent tasks, in which the two tasks either differ across dimensional levels or share the same level. Again returning to the visual–auditory example, some participants would perform two visual tasks (VV), while others would perform an auditory task combined with a visual task (AV). If performance in the AV combination is superior to that of the VV combination, then there is evidence of more efficient parallel processing (simultaneously using resources from different pools) in support of the theory. An example of this type of study was conducted by Horrey and Wickens (2004). They had participants perform a primary driving task while simultaneously engaged in a concurrent task that was delivered in an auditory, visual heads-up or visual heads-down modality. The authors reported that the MRT model accounted for 98% of the variance in predicting hazard responses and 92% of the variance in in-vehicle task performance.

Obviously, matters are not that simple or else Navon (1984) and others (e.g. Kantowitz 1987) would not have claimed the theory to be incapable of falsification. On the one hand, if one finds that the AV task combination is performed more efficiently than the VV task combination, then the argument could be made that the alteration of the modality somehow made the task easier. Thus, the difference in performance would not be due to accessing different resource pools, but instead would be due to a reduction in task difficulty. On the other hand, if one finds no benefit from the AV task relative to the VV task, then the argument could be made that the change in modality somehow made the task more difficult.

A second argument could be made that changing the modality somehow altered the task such that a valid comparison is no longer feasible. For example, a common method of

comparing A to V is to present sets of digits to participants and ask them to recall and/or repeat the digits (Wickens *et al*. 2005). These digits would be presented either visually (on screen) or auditorally (via speakers). The visual presentation is instantaneous, while the auditory presentation takes some time for the person to enunciate the digits. Does one allow the participant to respond as soon as the auditory presentation begins or must he/she wait until the auditory presentation has ended? Clearly, there are advantages and disadvantages to each method and this conundrum is not easily remedied.

A third argument is that if a researcher finds evidence contrary to MRT, then this can be countered by claiming that the finding is just 'excess baggage' (Navon 1984) or unimportant to the overall picture. Or one could add yet another dimension or level to explain that finding (Kantowitz 1987). Or one could claim that two tasks that normally access different resource pools used the same pool at some point in the experiment, which was the cause of the unexpected interference. Pashler's (1994, 1998) research on the Psychological Refractory Period has provided strong evidence that there is a bottleneck in resources at the response selection stage. On the surface, this might appear to disconfirm MRT, which seems to support the notion of parallel processing throughout. However, Wickens (2008) counters that Pashler's bottleneck theory really:

> ... represents a version of multiple resource theory (although not cast in those terms), based primarily on the stage dimension. Here response selection depends on a very limited supply of resources, forcing essentially on sequential processing with high time demand tasks, whereas perceptual-cognitive resources are limited but more available.

Not surprisingly, then, as some have argued, MRT appears, on the surface, to be incapable of falsification (Navon 1984). Kantowitz (1987) went so far as to say of MRT: 'I do not trust a model that cannot be falsified' (p. 91). But reconsider the problem in a way that explicitly keeps separate the assumptions that are part of the theory and the assumptions that are auxiliary to the theory. The theory itself assumes that there are different pools of resources and that tasks can draw on one or more of these pools. To the extent that two tasks draw on the same pool of resources simultaneously, there will be interference and performance times will increase. But assumptions about which tasks draw on which pools of resources are auxiliary to the theory. In addition, assumptions about which tasks are more difficult or less difficult to perform are also auxiliary to the theory. Clearly, then, the ambiguity in the literature is due, at least in part, to the problem that it is unclear which auxiliary assumptions one should believe.

In practice, this is a difficult problem that has not been solved, or else the potential for falsification of MRT would not remain a matter of debate (Navon 1984, Kantowitz 1987). In principle, however, the present authors believe that the problem can be resolved, depending on how creative researchers are about finding or addressing auxiliary assumptions. Consider, for example, the issue of task difficulty. The authors see nothing, in principle, to prevent researchers from comparing and contrasting the difficulty of different tasks in independent experiments. Suppose that, to justify the rejection of the results of an experiment that fails to support MRT, the auxiliary assumption that task A is equal in difficulty to task B is challenged, and an alternative auxiliary assumption is made that task B is actually more difficult. Researchers could perform independent research testing the two tasks against each other to find out which is more difficult, whether measured in terms of time taken to perform the task, subjective estimates of task difficulty or others. Suppose that task B is not shown to be more difficult than task A across a variety of measures of task difficulty. Clearly, then, the alternative auxiliary assumption would not be supported and it could not reasonably be used to justify the failed prediction.

In this case, the theory would have been falsified under the standard of reasonable falsification. Of course, it is always possible to argue that the independent experiments are flawed in some way and so absolute falsification cannot be obtained, but this is true of any research and so one need not be discommoded by the possibility.

As another example, consider the case where tasks from different dimensional levels do not support time-sharing. To explain the failure of time-sharing, an aficionado of the theory might argue that the two tasks only seem to be from different levels, but actually they have elements from the same level and therefore draw upon the same resource pool. Several pathways might be traversed to deal with this issue. For example, some discussion of what is meant by a 'level' might be in order. If the aficionado insists that levels be defined in terms of experimental findings, then it is clear that he or she is introducing a circularity into the system and can rightly be criticised for it. Clearly, it is unsatisfactory to say that two tasks are from different levels if the findings are in accord with MRT and that they are from the same level if the findings are not in accord with MRT. To complete the circle, of course, the hypothetical aficionado would then claim that the findings obtained from tasks from different levels support MRT, whereas the other findings are irrelevant because the tasks used draw from the same resource pool!

But a well-intentioned researcher does not have to resort to such a vicious circle. With some care, it may be possible to propose new auxiliary assumptions that set up independent criteria (e.g. Wickens refers to 'neurophysiological plausibility' as an independent criterion in multiple resources), including possible converging operations, for deciding when two tasks are from the same level or different ones. In this case, experiments can be performed, using these auxiliary assumptions to test whether tasks are or are not from the same level. Assuming the performance of the requisite research to find tasks that pass these independent criteria, the failure to obtain savings with simultaneous task performance over serial task performance would indeed falsify MRT, again under the standard of reasonable falsification.

4. Why are auxiliary assumptions necessary?

Thus far, this paper has sidestepped the issue of why auxiliary assumptions are necessary for testing theories and has merely asserted that they are and, from this assertion, the rest of the argument follows. But one might ask why this has to be so – why is it impossible to derive predictions from theories alone, without the addition of auxiliary assumptions? To answer this question, it is useful to start by considering that theories are general statements, or sets of related statements, about the relations between variables. Some examples from psychology might be as follows: frustration causes aggression, attitudes determine behavioural intentions, resources increase performance, and others. Note that the terms in these theories (e.g. attitudes) refer to unobservable variables. This is not just a characteristic of psychology. As the Nobel Prize winning physicist Leon Lederman (1993) pointed out, Newton's most important statement, that force = mass × acceleration, also depends on unobservable variables. For example, although mass might seem to be observable (one can weigh objects), the fact that the same object can have different weights on different planets clearly demonstrates otherwise. Mass is an unobservable that does not have a clear independent definition, yet Newton's equation is argued by some to be the most important single statement in the history of physics.

The astute reader may see a problem with this. If the terms used in theories (theoretical terms) refer to unobservable variables that do not have definitions, then one can only

understand the meaning of these terms in the context of the other terms in the theory. For example, mass must be understood in terms of force and acceleration. Doesn't this make Newton circular and didn't this article argue against circularity earlier when discussing the potential of MRT to be falsified? The quick answer to this question is that yes, circularity is a necessary feature of theories but that it is possible to distinguish necessary circularity from the vicious kind that was described earlier.

To see why circularity among theoretical terms is necessary, consider the following. Suppose that one wished to define mass independently of force and acceleration. Well, then, this means that mass would have to be defined in terms of some other words. But one could then ask what the meaning of these other words is, which would necessitate bringing yet more words into the theoretical system. In short, the result would be an infinite series of words. To avoid the problem of infinity, circularity is a necessary feature of theories and therefore it is unreasonable to require theorists to have independent definitions of all theoretical terms. An unnecessary, and vicious, type of circularity is when researchers set up auxiliary assumptions such that only studies that conform to predictions are deemed to be fair tests of the theory (as was seen earlier). Put another way, circularity within the theory is necessary, whereas circularity induced by auxiliary assumptions that are not part of the theory is at the same time not necessary and vicious.

Given that theories contain theoretical terms referring to unobservable variables that do not have independent definitions, the impossibility of deriving predictions about specific observable events solely from theories is obvious. Clearly, what is needed is a way to traverse the distance between the unobservable variables in theories and the observable variables that are manipulated and measured in actual experiments. This is what auxiliary assumptions do – they bridge the gap between that which is unobservable and that which is observable. Or to be philosophically correct and take into account that the observable–unobservable distinction is really more of a continuum than a dichotomy (e.g. Foucault (1970)), it could be said that auxiliary assumptions bridge the gap between that which is less observable and that which is more observable.

5. Auxiliary assumptions in ergonomics theories and in basic psychology theories

Although the basic philosophical logic that embraces theories, auxiliary assumptions and empirical tests is similar across scientific domains, the nature of the auxiliary assumptions is likely to be very different in the context of ergonomics theories than in the context of theories in basic areas of psychology, such as social psychology or cognitive psychology. In basic areas of psychology, investigators tend to be interested in the theories in their own right, without much concern about applying them to solve real-world problems. In contrast, investigators in the ergonomics area, though very interested in and respectful of theory, have the added concern of application to real-world problems.

This is not to say that theories in basic areas of psychology cannot be applied, because there are many examples to the contrary in both social psychology (e.g. Fishbein and Ajzen 1975, Trafimow 1996, Trafimow and Miller 1996) and cognitive psychology (e.g. Treisman and Gormican 1988, Trick and Pylyshyn 1994, Wolfe 1994). Nor is it being argued that ergonomics theories are not interesting in their own right. The point here is simply that there is a greater emphasis in ergonomics than in other areas of psychology on successful theory application. A consequence of this emphasis is that the auxiliary assumptions tend to be more complicated. This is because it is insufficient merely to bridge

the gap between theoretical terms and observation; rather, ergonomics researchers are expected to do this in such a way that a real-world problem is addressed.

Thus, as is true in other areas, ergonomics researchers are forced to make auxiliary assumptions to bring abstract theoretical constructs down to the level of operational definitions and, in addition, they often make additional auxiliary assumptions linking the specific operations to solutions to real-world problems. To make compelling these latter linkages, ergonomics researchers may have to make additional auxiliary assumptions pertaining to the cost, prevalence or general cost to society of the problem (or the general savings to society engendered by the solution).

The emphasis in ergonomics on application suggests an additional, though related, difference: utility tends to be a greater consideration in ergonomics than in other areas. The concern about utility adds yet more complexity to auxiliary assumptions because ergonomics researchers are driven to demonstrate that their research is useful and has value. But making the case for what one means by 'useful' and 'value' can be a complex undertaking, thereby necessitating even more complex auxiliary assumptions. It is perhaps to compensate for the complexity of the auxiliary assumptions that theories tend to be simpler in ergonomics than in social psychology or cognitive psychology.

The ergonomics emphasis on utility sometimes comes with a relative de-emphasis on falsification. Put simply, from the point of view of utility, the truth of the theory might not be considered to be of particular importance. Rather, what may matter to an ergonomics researcher is how well the theory can account for the phenomena of interest or how easily it is perceived to suggest solutions to real-world problems, while simultaneously keeping the number of theoretical assumptions to a minimum. But if ergonomics theories are to be evaluated based on utility rather than on truth, then the auxiliary assumptions might reasonably be expected to follow suit, and they sometimes do. Auxiliary assumptions in ergonomics tend to be designed to demonstrate the utility of the theory at hand rather than to generate competing predictions from alternative theories.

In summary, although the bridging function of auxiliary assumptions holds for all areas of psychology, the nature of the bridging, and where the bridges go, may differ substantially. The accent on utility in ergonomics extends the distance to be traversed beyond simple operational definitions of abstract theoretical constructs, thereby necessitating more complicated auxiliary assumptions, and ones that facilitate demonstrations of utility rather than of truth. In addition to militating in the direction of simpler theories, which the authors consider to be a good thing, the complexity of the auxiliary assumptions in ergonomics sometimes engenders in researchers in other areas a feeling that the research paradigms are inelegant. Although elegance is generally to be preferred over inelegance, all else being equal, the foregoing discussion demonstrates that not all else is equal. The concern for utility and addressing real-world problems forces ergonomics research paradigms to possess a certain degree of inelegance, but that may be a cheap price to pay for solutions that increase the quality of life that humans enjoy.

6. Conclusion

There is one way in which a theory can fail to be falsifiable, even when the standard used is one of reasonable rather than absolute falsification. A theorist may change the meaning of the terms in his or her theory to meet whatever experimental findings researchers acquire or render the findings irrelevant; the age old technique of saying 'I did not mean that' whenever one's theory is disconfirmed, thereby requiring researchers to

'hit a moving target'. Although there have been cases of this obviously shady practice in the history of psychology (no references are provided to protect the guilty), few ergonomics researchers have been guilty of it, nor has Wickens been guilty of it in the example that has been discussed. Consequently, the possibility of theorists using the moving target strategy does not provide a serious obstacle to the main arguments herein.

Thus, to summarise the main points, because theories are general and include unobservable (or less observable) terms, auxiliary assumptions are necessary to navigate the gap from them to observable (or more observable) manipulations and measures that are used in ergonomics experiments. In this sense, auxiliary assumptions are wonderful entities because they make possible that which would otherwise not be possible. But there is a price to be paid for this benefit: the use of auxiliary assumptions renders falsification as being much more complicated than if auxiliary assumptions were not necessary. Worse yet, the conjunctive logic necessitated by auxiliary assumptions forces absolute falsification to be impossible and, consequently, researchers are reduced to having to settle for reasonable falsification.

But even here there is a problem: the possibility that a heretofore undiscovered set of auxiliary assumptions may result in a testable prediction causes all theories to be potentially falsifiable under a reasonableness standard. The foregoing discussion of MRT provides an example of a famous theory that has often been accused of being incapable of falsification but that, nevertheless, clearly is reasonably falsifiable if only researchers put more effort into considering possible relevant auxiliary assumptions. Speaking more generally about ergonomics theories, it is hoped that this article will stimulate ergonomics researchers to devote less effort towards making pronouncements about how important theories are not falsifiable and devote more effort towards discovering the auxiliary assumptions that would actually allow them to be tested.

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