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A Taxonomy of Applied Research Categories as an Aid to Research Pertaining to Aviation

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Abstract

Based on the notion that applied research can be theoretically grounded or not theoretically grounded, I suggest that there are roughly four categories within which quality work in this area can fall: basic research, auxiliary assumptions, theory combined with auxiliary assumptions, and research without theory. Although the importance of auxiliary assumptions in traversing the distance between theories and their applications cannot validly be denied, applied researchers, including those in the aviation area, often have ignored them. But auxiliary assumptions play an important role in understanding the four categories that I propose. Finally, I argue that much of the applied research that actually is performed, again including aviation research, falls into none of the four categories and fails to contribute substantially to the field. My hope and expectation is that the present article will stimulate higher quality applied research by clearly explicating the important issues researchers should consider prior to beginning a line of investigation.

Keywords

Basic research; applied research; auxiliary assumptions; theory; application categories

Introduction

Aviation research is a subset of the larger area of *applied research.* Therefore, any insights that apply to applied research ought to apply to aviation research as well. Because I do not know much about aviation research, specifically; whereas I know more about applied research, generally; my goal is to provide insights about applied research with the anticipation that they will apply to aviation research too.

When speaking of applied research, there is a tacit question that I wish to make overt, “What is being applied to what?” The most obvious answer is that a theory is being applied to address some kind of problem or practical issue. And yet, anyone that regularly attends colloquia or reads papers in applied areas such as applied psychology, human-computer interaction, engineering psychology, and others, will quickly discern that although the presented research ostensibly is derived from some allegedly more general theory, the studies often have very little to do with the theory. To see that this is so, it is sufficient to ask oneself the following question: “What if the results had not come out as expected; would this cause me to question the theory to a significantly greater degree than if the results did come out as expected?” If the answer is “no,” as it often is, then that suggests that the studies were not tightly derived from the theory. To be sure, much basic research also suffers from this problem, but because my goal is to discuss applied research, I will not focus too much on basic research issues except where needed to set up my arguments about applied research.

Though it is important to recognize that in much applied research, the theory is taken as a given and there is no intent to test it, it also is important to recognize that if the applications really were closely tied to the theories, then application failures would cast doubt on the corresponding theories. In fact, however, although there are such examples in other sciences, application failures in aviation-relevant areas practically never cast theories into doubt. And proceeding to the issue of solving problems, although there have been some successes, practical solutions have been few relative to the frequency of published articles. In the hope of gaining insight into the reasons for these problems, let us take seriously the idea that applied research can involve applying a theory to address a real problem. If theories are taken seriously, it is possible to categorize different types of applied research based on the connection, or lack of a connection, of that research to a theory.

The Importance of Auxiliary Assumptions

To understand how thinking in terms of a theory implies different categories of applied research, it is important to recognize first that theory-based basic research is not as philosophically simple as it sometimes is considered to be. The complication becomes clear when one considers the falsification issue. Although it has become a truism, even at the level of many introductory textbooks, that theories need to be falsifiable, it is worthwhile to review briefly where this comes from with an eye to accomplishing two purposes. First, the falsification issue itself will turn out to matter even for applied research. Second, it is important to place the role of auxiliary assumptions in the foreground, rather than having them languish in the background, as is typical. Auxiliary assumptions are going to play a crucial role in the proposed scheme for differentiating different types of applied research pertaining to aviation.

Let us commence with Sir Karl Popper (e.g., 1959) who was critical about the notion that successful predictions prove theories to be true. He characterized the "proof" via the invalid syllogism below that I designate as Syllogism 1.

If the theory is true, the prediction should come out. {1.1}
The prediction comes out. {1.2}
Therefore, the theory is true. {1.3}

As Syllogism 1 obviously is invalid, the question arises about how to make it valid, without changing the major premise (Premise 1.1). Popper suggested that this be done via the ancient Greek form sometimes known as *modus tollens*. In essence, the idea is to imagine that the prediction does not come out, in which case the theory is disconfirmed. This valid logic is rendered as Syllogism 2 below.

If the theory is true, the prediction should come out. {2.1}
The prediction does not come out. {2.2}
Therefore, the theory is not true. {2.3}

Because it is logically valid to disprove theories via Syllogism 2, whereas it is logically invalid to prove theories via Syllogism 1, Popper suggested that scientists should attempt to disconfirm theories rather than confirm them. Science progresses, in Popper's scheme, by replacing falsified theories with better ones. But if theories are inherently unfalsifiable, there is no way to get the Popperian machinery running, and so Popper deemed such research as failing to meet his demarcation between science and pseudo-science.

One problem even with Syllogism 2 is that the major premise (Premise 2.1) fails to accurately capture the way scientists derive predictions from theories. Never, in the history of science, has a prediction arisen solely from a theory. Rather, scientists derive predictions from a combination of theory and assumptions that are not part of the theory but are nevertheless necessary to traverse the distance from the theory to the prediction. For example, consider how one would predict where Venus would be at a particular time. Newton's Laws of Motion, even in conjunction with his Universal Gravitation, would be useful but insufficient unless one made assumptions about the current location of Venus, its velocity, the presence or absence of other planets, and others. These assumptions, which are not contained in Newton's theorizing, are often termed *auxiliary assumptions* (Duhem, 1954; Lakatos, 1978; Meehl, 1990; 1997; Trafimow, 2003; 2009). It is impossible to emphasize too strongly that any theory-based prediction arises from a combination of a theory and auxiliary assumptions and never solely from theory.

From a falsification point of view, the necessity to consider auxiliary assumptions poses an important challenge. To see why this is so, let us rewrite the major premise of Syllogism 2 to include auxiliary assumptions. Please note the change in the conclusion that is necessary to render Syllogism 3 valid, given the rewritten major premise.

> If the theory is true and a set of auxiliary assumptions is true, the prediction should come out. {3.1}
> The prediction does not come out. {3.2}
> Therefore, the theory is not true or the set of auxiliary assumptions is not true. {3.3}

A less clumsy way to rewrite Conclusion 3.3 is to say that the theory is wrong or at least one auxiliary assumption is wrong (of course, both could be wrong too). The inescapable implication is that, in the event of an empirical failure, the researcher can blame an auxiliary assumption, and so he or she is not forced to blame the theory. There is much more to say about the falsification of theories, but we have covered everything necessary to set up the next section. How should we classify applied research so as to recognize explicitly the relationship between application and theory?

An Auxiliary Assumption Based Classification Scheme

We already have seen that all theory-based predictions in science arise from the combination of theory and auxiliary assumptions. In at least one sense, applied research is similar to basic research in that the researchers set up hypotheses that are, in essence, predictions about the observation that will come about under the conditions that the researcher sets up. The fact that predictions, even in applied research, nevertheless can depend on theory and auxiliary assumptions suggests two important points. First, any applied research that is theory-based is more likely to be successful with a valid theory than with an invalid one; the theory really does matter (but with an important exception that will become clear later). Second, because predictions arise from the theory and auxiliary assumptions, the predictions are more likely to come out if the auxiliary assumptions are true than if they are not true (Trafimow, 2012). What categories of applied research do the foregoing considerations imply?

Category I: Basic Research is Applied Research!

It would be very strange to have a strong theory, one that passes impressive empirical tests, that never is applied to solve a problem. As Lewin (1951, p. 169) famously pointed out, "nothing is so practical as a good theory." Given sufficient creativity, there is a way to apply almost any theory to the betterment of society, provided that the theory really is a good one. It is very difficult to name a theory that has been successful in the history of science that has failed to lead to advantageous applications; whereas, it is easy to find successful theories that have been advantageously applied. Given, then, that top-level basic research is likely eventually to lead to applications, it seems reasonable to state that top-level basic research *is* a category of applied research. I hasten to add that lower-level basic research is much less likely to lead to successful applications—an argument, perhaps, that only high quality basic research should be considered to be applied research too.

Category II: Find the Auxiliary Assumptions!

Imagine that an engineer wishes to build a rocket ship to go to the moon. Obviously, Newton's Laws of Motion and Universal Gravitation are going to be desirable to have, but they are not sufficient. As an example of the insufficiency, consider that the Earth has an atmosphere so that the engineer has to take friction into account. And yet friction is not included in Newton's theorizing. How can this be done? What assumptions should be made about the amount of friction the rocket ship is likely to experience, the ability of various materials to handle different amounts of friction, and so on? Assumptions that eventually will have to be made about these matters constitute a subset of the totality of needed auxiliary assumptions. But on what basis should the engineer make such auxiliary assumptions?

In the so-called hard sciences, there are engineering researchers who study, for example, the extent to which different kinds of materials are able to withstand varying amounts of stress. The information they collect can provide a strong basis for making high quality auxiliary assumptions. Again, it is not just Newton that makes the rocket ship work, but rather Newton in combination with the auxiliary assumptions based, at least in part, on engineering advances.

In many aviation-relevant areas, there is no category termed "engineering" (engineering psychology might be considered an exception, but the connection of engineering psychology to psychology is not analogous to the connection of engineering to physics), and so the role that applied researchers of this type play is more easily obscured. Nevertheless, there exist researchers who function at the level of auxiliary assumptions. For example, basic researchers worked out the principles of conditioning a long time ago. But if one wishes to use those principles to cure children of bed-wetting, the principles by themselves are insufficient without auxiliary assumptions. For example, some types of punishment or reinforcement work better than others in a bed-wetting context. Furthermore, there is the issue of the optimal delay between the initial bed-wetting and the punishment or reinforcement. The point here is not that the principles of conditioning cannot be used, only that there are additional factors that come into play and it is necessary to make auxiliary assumptions pertaining to those additional factors. That researchers in this area have done a good job with respect to auxiliary assumptions is testified to by the impressive success rate of treatments for bed-wetting (see Redsell & Collier, 2001 for a review). Of course, it also should be admitted that the success rate in clinical psychology more generally is less enviable.

Another interesting psychology example, that strongly highlights the importance of finding good auxiliary assumptions, is the reasoned action approach to explaining behavior (Ajzen & Fishbein, 1980; Fishbein, 1980; Fishbein & Ajzen, 1975; 2010). According to this approach, the immediate determinant of behavior is behavioral intention. In turn, behavioral intention is caused by attitude (how much one likes or dislikes to perform the behavior), subjective norm (one's perceptions of whether important others think he or she should perform the behavior), and perceived behavioral control (one's perception that the behavior is under his or her control). Finally, these variables are determined by various kinds of beliefs. There is a voluminous literature that supports each of these causal connections (see Fishbein & Ajzen, 2010 for an exhaustive review but see Trafimow, 2007 for an exception). Consequently, the interventions that have arisen from the theory, to increase condom use, increase seatbelt use, increase exercise, and so on, ought to work well based on the strong empirical support each of the theoretical connections has received. And yet the interventions mostly do not work, thereby prompting some to call for retiring the theory (e.g., Sniehotta, Presseau, & Araújo-Soares, 2014). But is the theory really where the problem lies?

The present discussion that focuses on the importance of auxiliary assumptions suggests that there is an alternative possibility (Trafimow, 2015). Specifically, the problem might not be within the theory at all, but rather that researchers have not been successful in generating auxiliary assumptions of sufficiently high quality to make the theory work to solve real world problems. Consider for example, the theorized causal chain from attitude to behavioral intention to behavior in the context of eating healthy foods (e.g., broccoli). As stated earlier, attitude denotes the extent to which the person likes or dislikes performing the behavior. Well, then, according to the theory, if people could be induced to like to eat broccoli, they would intend to eat broccoli, and broccoli-eating behavior would increase. Does the lack of success of a broccoli-eating intervention (assuming one were attempted) really provide a strong argument that the theorized causal chain is not true, or is the problem that it is difficult to figure out a way to induce people to like to eat broccoli? If you favor the latter explanation, then you recognize the importance of auxiliary assumptions. It is not sufficient to know about the theorized causal chain; one needs to know what auxiliary assumptions one can make to construct a successful intervention. As of now, despite the voluminous literature containing empirical findings that support the various theoretical connections that the reasoned action approach makes, the lack of actionable auxiliary assumptions renders successful application rare. To make the theory applicable, it would be useful to have a set of researchers whose goal is to find ways to actually influence the antecedent variables in the theory (e.g., attitude). We need something like reasoned action engineers to find the requisite auxiliary assumptions.

Category III: Construct the Conjunction of Theory and Auxiliary Assumptions!

A third type of theory based applied research involves combining the theory with the auxiliary assumptions. Suppose that a theory has been strongly supported by basic research (Category I) and the requisite auxiliary assumptions have been found, proposed and studied in sufficient detail for them to be used (Category II). It might be a good time to actually solve a problem or otherwise make a practical contribution. If an applied problem is to be solved by theoretical means, eventually someone has to combine theory and auxiliary assumptions to solve it.

Returning to the reasoned action example, imagine two kinds of research. First, imagine that much research has been performed in Category I to validate the theoretical approach (as actually is so in this case, see Fishbein & Ajzen, 2010 for a review). Second, imagine that the requisite auxiliary assumptions have been found and been supported empirically (Category II). Nevertheless, the specific problem of influencing the behavior of interest might not yet have been solved. Someone actually has to combine the reasoned action approach with the validated auxiliary assumptions to find an intervention that influences the behavior of interest. More generally, research determining the crucial place where theory and auxiliary assumptions conjoin to solve the problem can be distinguished from research on the theory itself or research on auxiliary assumptions themselves.

It seems worthwhile to introduce a caveat here that auxiliary assumptions can be at different levels and these different levels might blur the distinction between the three categories. Continuing with the reasoned action example, suppose that the problem of how to induce attitude change were solved but at either a general level or a specific level. An interesting characteristic of the reasoned action approach is that attitudes are assumed to depend on beliefs about the likelihood of various consequences of performing the behavior and evaluations of how good or bad these consequences are. Unfortunately, the theory does not say how to actually change beliefs or evaluations. But it is possible to imagine that someone comes up with an effective method to induce changes in beliefs, evaluations, or both. This method might be at a general level that influences any kind of belief or evaluation regardless of whether it pertains to seat belt use, condom use, and so on. Or this method might be at a specific level that pertains only to condom use. Nor are these the only possibilities. Perhaps the new method is influential for influencing males but not females, or females but not males. Or perhaps the new method is relevant to behaviors in the sexual domain more generally but not relevant outside of the sexual domain. An interesting philosophical argument could be had about the conditions under which auxiliary assumptions might reasonably be considered to be theories in their own right.

A recognition that auxiliary assumptions can come at varying levels implies that even if one has a good theory, and even if one has high quality auxiliary assumptions at an intermediate level, it might nevertheless be necessary to have yet more auxiliary assumptions to traverse the distance to the specific problem at hand. For example, even with the reasoned action theory, and even with a method to influence attitudes (or the beliefs or evaluations that underlie attitudes), it nevertheless remains necessary to figure out how best to employ the method in the specific domain at hand, among the particular population where one wishes to intervene, and in the specific context and time at hand. These complications increase the difficulty of applied research, may induce the researcher to mentally toggle between Category III and Category II (and perhaps even Category I), but also provide a fascinating challenge for applied researchers.

Category IV: Do Without Theory!

It is difficult to publish applied research in top journals—even top applied journals—without at least giving lip service to a theory. The insistence on a theoretical perspective makes sense from the point of view of Categories I-III, but suppose that one has valuable applied work that does not stem from any theory. In fact, to dramatize this point, suppose that an application is not based on any science whatsoever but still solves a problem. (Depending on how one defines words such as "theory" it is possible to argue that there always is a theory even if it is not specified. I'm using the word "theory" to refer to formal theory or a theory that has been specified, rather than an implicit theory.)

Consider, for example, the invention of the paper clip by Johan Vaaler (March 15, 1866 – March 14, 1910). There was no science involved whatsoever; no theory (Vaaler did not use advances in chemistry pertaining to the characteristics of metals), no auxiliary assumptions, none of the usual scientific trappings, and yet it worked! Unless one wishes to discount the value of the paper clip, it must be admitted that it is possible to have valuable applied work even in the absence of science. In fact, many inventions are not based on science but are nevertheless good to have.

Given that useful applied work can be done even in the absence of science, we must suspect the existence of some useful applied research in areas that are relevant to aviation not based on any theory. An example might be work on product usability (see Landauer, 1991 for a widely cited argument). Although published articles on the topic often make reference to theories in cognitive psychology, the fact of the matter is that these merely provide a theoretical gloss; the real work has nothing to do with cognitive theory and everything to do with the experiences of those who do the real work (Landauer, 1991; McDonald, 2005). This is not to say that cognitive theories are not useful in general, only that usability provides an example of applied psychology that has little to do with any formal theorizing.

Discussion

We commenced by asking what seems like a crucial question about applied research, "What is being applied to what?" In addition, the obvious thing to apply is a theory. However, it is impossible to apply a theory without making auxiliary assumptions, and this led to four categories of applied research. But certain questions arise. For example, a colleague of mine, who is a pessimist about applied research, asked, "Why is there so much applied research that never results in the solution to a problem?" A serious consideration of the question suggests that there actually is a good answer that makes use of the four categories. In essence, research that falls into none of the four categories is likely to fail to contribute in an important way.

We might commence by considering a common requirement of Categories I-III, which is that there actually is a theory. But if the majority of theories in psychology or other aviation-related areas are not of high quality, then we might expect that the application of a low quality theory is not likely to result in a good solution to an important problem.

Because I am a social psychologist, I am more familiar with social psychology theories than with theories in other areas of aviation-related research. And there are hundreds of such theories in social psychology! Or at least there are hundreds of statements that are called theories and perhaps that is where some of the problem resides. Many of the theories are simply summaries of empirical findings using generalized language rather than thoughtful explanations of empirical findings. This becomes particularly obvious when someone finds an exception to the generalized statement, in which case the context specified by the experiment is converted into another generalized statement. Now the theory contains the interaction of two generalized statements, for example, "X causes Y under C_1 but not under C_2." And as more empirical work is performed, more generalized statements might be added. For example, "X causes Y under C_1 but not under C_2 when condition D_1 is true; whereas, X causes Y even under C_2 when condition D_2 is true." Although these sorts of theories are good descriptive summaries of empirical research, they do not provide underlying explanations. And it can be a danger sign when theories become increasingly complex without a corresponding increase in the domain of relevance.

In contrast, in sciences that have a better record with respect to successful applications, the theories become increasingly basic and provide real explanations rather than generalized empirical statements. Of course, to account for the constantly increasing plethora of findings, there must be complexity someplace. But in sciences that often lead to successful applications, it is in the auxiliary assumptions, rather than in the theory, that this necessary increased complexity resides.

It is important not to state the argument too strongly. There are times when generalized empirical statements can be useful too, and it would take a cynical person to claim that none of the generalized statements in aviation-related literatures have aided in solving a problem. But taking my colleague's question seriously suggests that the poor quality of theories is likely to be part of the reason for a less than enviable record of practical accomplishments.

And then, even when there is a theory that is a real theory rather than just a summary of empirical findings in generalized language, there remains the problem of finding the necessary auxiliary assumptions (Category II) and of combining them with the theory in a fruitful way to solve a problem (Category III). Although many researchers know that auxiliary assumptions exist, auxiliary assumptions tend to languish in the background. But to perform research in Category II and Category III (and even Category I), auxiliary assumptions are crucial. Unfortunately, there is a lack of equivalence between knowing that auxiliary assumptions have to be made and appreciating their importance in full. Engineers who wish to apply physics theories are trained to focus much of their attention on auxiliary assumptions; whereas, researchers in aviation-related areas mostly are not. Perhaps if researchers in aviation-related areas were taught routinely to focus on auxiliary assumptions, as is the case with engineering, matters would change.

Worse yet, it is possible to argue that the use of theories, as it takes place in applied psychology under some circumstances, actually is worse than not having any theories whatsoever. In some cases, as we saw with respect to the invention of the paper clip, scientific theories simply are not necessary. Consider again the irrelevance of science when Johan Vaaler invented the paper clip. Fortunately for him, he saw no need to publish his research in an applied journal. Had he wished to publish in a top applied journal (at least if he lived today), he would have had to invent some connection to theory to satisfy the reviewers. Although a connection to theory makes sense for Categories I-III, it does not make sense with respect to Category IV, and we might be reducing the creativity of researchers by insisting on always connecting to theory. There should be a place for researchers to say something like the following: "Here is solution *Y* to solve problem *X*, and the solution does not derive from any theory" (i.e., it is in Category IV).

All of these considerations lead to what I see as the largest problem in applied areas, including aviation-related ones, and I see this time and again in my role as Editor-in-Chief of *Basic and Applied Social Psychology*. Specifically, authors often tout their submissions as intersecting basic and applied research. The problem is that if I ask whether a new theory has been proposed or strongly tested, the answer is "no." If I ask whether a societal problem has been solved or a practical accomplishment made, again the answer is "no." If I ask whether any auxiliary assumptions have been uncovered that make it substantially more likely that the theory will lead to a solution to a problem or a practical accomplishment in the reasonably near future, again the answer is "no." The upshot is that these papers contribute substantially neither at the level of basic research nor at the level of applied research. Rather than insisting that researchers always have to integrate basic research with applied research, it would be better to train them to consider precisely what the basic research contribution is, if there is one, or to state precisely what the applied contribution is, if there is one. If neither of these can be stated, the research does not constitute an important contribution and ought to be rejected. To use a soccer analogy, there is a "no person's land" between being positioned in the area of the goal versus coming out of the goal to claim the ball. Goalkeepers are trained to be in the area of the goal or out of it to claim the ball, but not to be in the no person's land in between. Much research that is touted as intersecting basic and applied research seems characteristic of a no person's land akin to the soccer example. That is, many of these studies fail to contribute substantially to basic or applied research because there is no new theory or strong new test of a theory (Category I), no new auxiliary assumptions are discovered or strongly tested (Category II), there is no solution to a problem based on combining theory and auxiliary assumptions (Category III), and there is no practical accomplishment (Category IV). It might be helpful if editors of applied journals commenced to insist that the onus is on authors to explain to which of the four categories their research contributes. An inability of authors to render that explanation should constitute a strong force in the direction of rejection.

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Biography

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