Using Programmatic Research to Build a Grounded Theory

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We are constantly told in the social sciences that we should build theories, that there is nothing as practical as a good theory, and that data without theory have little or no utility. I would agree with this for the simple reason that no one could retain or integrate thousands of disconnected facts divorced from some wider set of principles or theoretical framework. The unanswered question for most aspiring theorists, however, is, How do you build a theory?

A common procedure is to invent a theory a priori based on one or more of the following: a small, preliminary data set from which grandiloquent deductions are made, guesswork, hunches, arbitrary hypotheses, intuition, qualitative observations, and deductions from other, marginally related, theories. Inventors of such theories may then spend the rest of their careers (a) defending their premature speculations to the death or (b) alternating between defending their speculations and scrambling to make post hoc revisions of the theory when additional evidence contradicts it (often accompanied by post hoc reinterpretations of the contradictory studies).

I do not think this is a very good way to build a theory; I never have thought so. It's too deductive and results in an excessive focus on "defending territory" rather than looking at the facts. I followed a different approach in constructing goal-setting theory. I did it with full realization of the limitations of the conventional approach, but without knowing what to call what I was doing. I later realized that I was using the "grounded theory" approach described by Glaser and Strauss (1967; see also Locke, 1991). The grounded theory approach is inductive: Theory emerges from the data rather than being deduced or guessed at in advance.

The grounded theory approach implies that it takes a long time to build a theory, because a great many data have to be collected first. Central to the process of theory building, I believe, are seven key tasks:

1. Definition of the core concept(s)
2. Validation of the main theoretical ideas and/or measurements
3. Replication by other investigators
4. Documentation of the generality of the results (e.g., across settings, tasks, and subjects)
5. Identification of boundary conditions for the theory (boundary conditions are, in fact, the same as moderator variables)
6. Identification of causal mechanisms
7. Conceptual integration with other theories, especially in cases where those theories might seem to conflict with or compete with one's own

One might ask, Why are these the key tasks? My answer is that to build a theory, you have to know what it is you are talking about (task 1), provide evidence for it (tasks 2-6), and integrate it with other relevant theories (e.g., to resolve contradictions; task 7). The bulk of the work in theory building involves gathering evidence.

I know of no readings in the theory-building literature (most of which have been written by people who have never built a theory or studied, in depth, the procedures of those who have) that identify the activities that constitute the essence of theory building, indicate how these activities are to be carried out, or specify the order in which the various tasks are to be done. I cannot answer the second and third points even today. Goal-setting theory, I would have to say, was built piecemeal, without a grand plan. Every study, I believe, had a specific purpose, and these purposes built upon previous work, but there was no grand design involved, no overall blueprint. This was simply because no one really knew how to go about building a theory.

On the other hand, I was implicitly aware of the need to address in some fashion the various theory-building tasks. These tasks were accomplished by Latham and me and by many others over a 25-year period.

Key Tasks

The Core Concept

Let me state right away that I no longer use the term construct, and I discourage doctoral students from using it. The reason is that the term construct is a Kantian idea. Kant has been called the "great destroyer" in the history of philosophy. This is true in two respects: epistemology and ethics (Peikoff, 1982). I will focus here on epistemology. Kant was the first philosopher to declare that the real (noumenal) world is unknowable and that we can be aware only of the phenomenal world, the world of appearances. The world of appearances, he argued, is a creation of the human mind, a set of constructs if you will, that do not give us any knowledge of things in themselves. Thus Kant totally severed reason from reality (his motive for destroying reason was to make room for Christian faith). Social scientists, unfortunately, have uncritically accepted the idea that their terms are arbitrary inventions of the human mind, or hypothetical constructs, to be "validated" by correlating measures of them with measures of other hypothetical constructs! It is not clear how this could ever work if all terms and/or measurements are arbitrary.

The proper term to use in place of construct is concept, which may be defined as "a mental integration of two or more units which are isolated according to specific characteristic(s) and united by a specific definition" (Rand, 1990, p. 10). A valid or objective concept is not an arbitrary invention but an integration material provided by the senses or derived from such material. In psychology, the units of relevance are conscious experiences and thus are validated by introspection.

I first got the idea of using goals as a motivational concept from one of the faculty at Cornell, T. A. Ryan, who was writing a book on intentions, and from the work of C. A. Mace, a British psychologist whose work on goal setting Art Ryan and Pat Smith had cited in their classic 1954 textbook, Principles of Industrial Psychology.

The concept of goal, however, is validated by observing that we have ends that we desire to attain and that these guide our choices and actions. (Later we observe that not all goals are consciously directed—that is, not all are purposes; some goal-directed activity occurs automatically and is inferred from its life-preserving consequences. See Binswanger, 1990.) No further validation of the concept of goal (or of any other concept) is necessary beyond identifying the observed facts that gave rise to the concept and integrating those facts into a coherent definition.
How to measure the content of the goals of particular individuals in particular contexts is a different and separate matter. The simplest way is just to ask people what their goals are; for many purposes such questions yield valid and useful answers. A related question is how to ask the question. For example, Mento, Locke, and Klein (1992) found that the most useful way to measure an individual's personal goal on a task is to ask what minimum level of performance he or she is trying for.

Another question is how to obtain variation in goals. A simple way is to persuade people, for the purposes of an experiment, to pursue different kinds and levels of goals (i.e., goal assignment). Observe that there is no attempt here to "triangulate the construct" of goal through multiple measures, although there is nothing wrong with trying different measures to see what works (Mento et al., 1992). But the concept must be defined first. Then ways are found to measure it that are consistent with the definition and appropriate to the proposed context. (For example, asking a person to report his or her personal goals might not yield accurate information if the individual has poor self-insight or if telling the truth will result in punishment.)

Core Evidence

To support a theory, you need at least some initial evidence. The key problem here is, What constitutes evidence? For example, Freud claimed to validate his ideas based on clinical experience, but much of what he claimed was evidence could have been interpreted in other ways, as many critics of Freud have pointed out. Experimental evidence is especially helpful, because well-designed experiments can rule out alternative explanations. Some phenomena in the sciences, of course, are beyond controlled experimentation (e.g., astronomy, history, most of economics), but, fortunately, goal setting can be studied in the laboratory.

For my dissertation I did several laboratory studies of goal setting and they all worked, so I decided to keep at it. I took my first job, in 1964, at the American Institutes for Research (AIR) in Washington, D.C., then under the direction of Ed Fleishman. Ed suggested that I might get funding from the Office of Naval Research (ONR), which I did. I hired Judy Bryan as my research assistant.

We conducted a series of additional goal-setting studies, and they too were successful. We compared "do best" goals with specific, difficult goals and hard goals with medium and easy goals. We used a variety of different tasks (brainstorming, addition, and others) and experimental designs, including one where we took naturally low-performing subjects and improved their performance through the setting of specific goals. After a couple of years, ONR cut us off, because, as I was told confidentially, our studies had come out so well that ONR did not have to justify its past support with further support, as in the case of other contracts! There is nothing like being punished for success.

However, I obtained another grant to study goal setting in relation to feedback. The idea for this work also came from C. A. Mace. He had suggested that goals and intentions might mediate the effects of other incentives on performance. We conducted a series of studies that showed (a) when goal setting was controlled, there was no effect of knowledge of score-type feedback on performance; (b) feedback often led to spontaneous goal setting and thence to performance effects; but (c) when goal setting was statistically controlled, the feedback effect was vitiated. In sum, the feedback effect on motivation was mediated by goal setting (for a summary of this work, see Locke & Latham, 1990, chap. 8).

Just before I left AIR, I completed a paper (later to become a "Citation Classic") titled "Toward a Theory of Task Motivation and Incentives" (Locke, 1968). It summarized the results of about a dozen of our goal-setting studies plus other relevant studies in the literature. I remember specifically not wanting to call it a theory and being questioned by a number of people as to why. At this point I had not read any literature on theory building, but my caution was based in part on the philosophy of Objectivism (Rand, 1990), which argues that knowledge comes from reason applied to experience—which implies that theories should be tied to reality, that is, based on actual knowledge rather than speculation.

Replication

In medicine and biology replication across laboratories is considered to be very important,
because research procedures in these fields are highly complex, and sometimes idiosyncratic elements of procedure can affect outcomes. The same principle holds for social science research, even though research procedures here may be less technically complex.

Replication of goal-setting results began, I assume, when people read about our results in the journals. When I was a doctoral student, everyone was trying to test Herzberg’s theory, and expectancy theory was just starting to create a stir in industrial and organizational psychology (Vroom, 1964). I believe that the most obvious reason people keep doing research in a given area is that previous research has yielded positive results. When a long series of studies come out with negative or questionable results, people begin to lose interest. Goal-setting studies typically came out well, thus encouraging further work.

However, there is another factor involved in the spread of research on a given topic that, for want of a better term, I will call theoretical growth. If a theory remains focused around one narrow topic or subtopic, people get bored because they find nowhere new to go with it. I constantly tried to help keep goal-setting research going in new directions (see below). The result is that the more new directions you go in, the more additional directions other people think of (e.g., goals and self-efficacy, group goals, goals and leadership, goals and intrinsic motivation, goals and personality). In the end, there are literally hundreds of possible studies to do that nobody else has thought of doing.

A final factor that I think helped the growth of goal-setting research was my attitude toward young researchers. I always tried to be encouraging to such people (except in those few cases where they claimed to have invented goal theory themselves or were too arrogant to take good advice). This has involved such actions as commenting on past research, making suggestions on how to analyze and present results, and offering commentary on the design of future studies (including dissertations). Even when negative results were obtained, I tried to figure out the possible reasons and suggested ways that the hypotheses could be verified or the errors eliminated in future studies. Typically these researchers were grateful for the interest and open to suggestions, which led to additional goal-setting studies and additional discoveries.

Generalization

Generalization involves induction. Philosophers of science have not yet discovered all of the factors that would have to be considered to make valid generalizations from one set of results to other settings and situations. Obviously, the notion of similarity is relevant, but similarity in what? In every detail or just essentials (Locke, 1986)? And what is essential in an experimental situation? In psychology we are concerned with differences in subjects, settings, and tasks, among other things. If a phenomenon comes out with many types of subjects, in many different settings, and with many types of tasks, we usually have more confidence in the generalizability of the results than if it works only in more limited circumstances. Goal-setting studies have been conducted with more than 40,000 subjects in seven countries, in both laboratory and field settings, and with at least 88 different tasks (Locke & Latham, 1990, chap. 2).

I think that I worried most during the early years about settings. I especially wondered about the difference between laboratory and field settings. Enter Gary Latham. At the same time I was doing my early lab studies, he was doing research in lumber companies and was obtaining results similar to mine, although at first he did not know about my work. I was very gratified when, in field study after field study, he obtained significant goal-setting effects. He played a significant role in making my work believable. He also did the bulk of the early research on assigned versus participatively set goals. I had no particular personal interest in this topic, but both of us, imbued with the zeitgeist of the 1960s, assumed that participatively set goals would lead to higher commitment than would assigned goals. We were both surprised when this was not found. But this added to the generalizability of goal-setting results, because we later realized that there are many ways to get people committed, one of which is visionary leadership (Locke & Associates, 1991). I was less worried about generalizability across tasks because, in logic, there did not seem to be any reason goal setting would fail to work on any task or outcome. However, it did occur to me that simply having a goal or full commitment to a goal was no guarantee that one would reach it; one’s desires do not automatically lead to successful action. There
were two issues involved here, although it took me years to grasp them fully. The first was knowledge and ability, and the second was volition (see Binswanger, 1991; Locke & Latham, 1990, chap. 1).

Generalizability across subjects did not concern me very much either, because I considered conscious goal-directedness to be a fundamental attribute of all human action (Locke, 1969), regardless of culture. At a deeper level, goal-directedness is an attribute of the activities of all living organisms, including those that are not conscious (Binswanger, 1990). The goal-setting work in other countries got done simply because researchers in those countries became interested in the topic (e.g., Gary Latham in Canada, Bob Wood in Australia, Miriam Erez in Israel, Uwe Kleinbeck in Germany, C. A. Mace and, just recently, Ivan Robertson, in England).

Boundary Conditions

Boundary conditions for a theory are the conditions that specify when it will work and when it will not. Conceptually, they fulfill the same theoretical function and are really the same idea as moderator variables. Erel (1977) identified one important boundary condition I had overlooked, feedback. I had been so focused on goals as a mediator of feedback, I had neglected to pursue the idea of feedback as a moderator of goals. I had identified goal commitment as important in 1968, but little was done to measure it for many years—probably because commitment was so easy to get in laboratory settings due to the authority of the experimenter and the fact that it was a volunteer subject population. However, a number of more recent studies by Hollenbeck and others have shown that commitment is often significantly associated with performance for subjects with difficult goals, because these subjects are more tempted to set personal goals that are lower than their assigned goals (Locke & Latham, 1990, chap. 6).

A third moderator, which I had suspected for some time, was task difficulty or complexity (Wood, Mento, & Locke, 1987). I reasoned that on complex tasks there are many alternative methods of pursuing the goal—that is, many possible task strategies—many of which will not work. Thus establishing the proper link between goals and goal-relevant outcomes, which means identifying correct strategies, is problematic. The chances of error will be greater on complex tasks than on simple tasks, where the correct path to the goal is more obvious. Thus, as expected, Wood, Mento, and I found a smaller effect size for goals on complex tasks than on simple tasks. There are other moderators, such as ability and situational constraints, but these have been less extensively studied than those mentioned above.

One unique aspect of our 1990 book, which no reviewer ever pointed out, is the analysis of every negative goal-setting study that we knew about. The reason we included this analysis was the law of contradiction: A theory cannot be both true and not true at the same time and in the same respect. Thus a negative result must mean either (a) that the theory, as stated, is wrong and needs modification (e.g., the formulation of an additional boundary condition) or (b) that the theory was not tested properly. Thus we attempted to identify the probable cause(s) of every negative result. Some of these analyses had to be a bit speculative, owing to the post hoc nature of the interpretations. One of the more intriguing propositions we offered (which was never taken up by anyone) was that certain personal traits, such as persistence, might operate motivationally as "goal equivalents" in that they may motivate a person to perform highly in the absence of explicit goals (Locke & Latham, 1990, chap. 2).

Causal Mechanisms

It has always puzzled me how little attention is paid in the theory literature to the need to explain the mechanism(s) by which independent variables affect dependent variables. The issue is especially important in the social sciences, because these mechanisms or mediators always involve some internal ones (namely, beliefs, values, expectations, knowledge). Behaviorism, despite being irrational on the face of it (Locke, 1980), was able to create the illusion of explaining human action only because nobody bothered, until Dulany asked it in 1968, to ask the question, By what means do so-called reinforcements reinforce? I consider mediation studies to be among the most important types of studies...
done in the psychological sciences, although not many people seem to agree with me. The three core causal mechanisms for goals were, in a certain respect, self-evident, because they could be identified through introspection—effort, persistence, and the direction of action and attention. Empirical studies of these mechanisms turned out to be surprisingly difficult due to the need to make fine, internal, micromeasurements of subjective and partly automatic experiences or functions (e.g., effort), but, over the years, relevant experimental studies were forthcoming (see Gellatly & Meyer, 1992; Locke & Latham, 1990, chap. 4).

More complicated to understand is the effect of the fourth type of goal mediator, task strategies. Even on simple tasks, more than one strategy can be used to pursue the goal. As noted earlier, on complex tasks there are many more possibilities, and thus goal effects are less reliable (Wood et al., 1987). Furthermore, although many strategies may fail entirely, others may work, but with different degrees of speed and efficiency. The realm of task strategies implicitly connects the goal-setting realm to three other research realms: problem solving, decision making, and group dynamics. Actually, more than three could be listed (e.g., I could add cognitive psychology, leadership, training, and more). I am sure the reader can now see that if you think of all the implications of a given line of work, eventually everything connects to everything else.

Thus it is a genuine challenge to understand what types of goals lead to the best choice of strategies on what tasks and the processes that lead to the different strategy choices. These problems alone could occupy many researchers for a lifetime. The existence of groups adds another layer of complexity, because group members communicate and work with one another, which means that the ideas, abilities, and motivations of each person can affect those of other people. Such complexity, of course, also adds to the fun and challenge of goal research.

Conceptual Integrations

Just as individual findings cannot contradict a valid theory without the theory being modified or the findings invalidated, a valid theory cannot contradict another theory without at least one of the theories requiring modification or clarification. The most glaring contradiction from the beginning seemed to be between goal theory and expectancy theory, in that one claimed to find a positive association between performance and goal difficulty (which was inversely related to probability of success) and the other claimed to find a positive association between performance and probability or expectancy of success. In this case, it turned out that both theories were correct and that the contradiction was only apparent. When the level or difficulty of the goal was controlled, probability of success was found to be positively associated with performance; the same was found for self-efficacy (Locke, Motowidlo, & Bobko, 1986).

Another apparent contradiction was between "human relations" theory in the form of the findings of Erez and her colleagues regarding the positive benefits of participation in goal setting and those of Latham and his colleagues, which showed a null effect of participation. In this case the apparent contradiction turned out to be caused by different experimental procedures used by the two camps. The solution to this riddle turned out to be fascinating and involved a unique scientific collaboration between the opposing researchers moderated by myself (Latham, Erez, & Locke, 1988).

On the other side of this coin, goal theory and social cognitive theory fit together from the beginning like hand and glove. Bandura's theory is very broad in scope and pertains to an impressive array of situations, from sex to cultural innovation to task performance (see, e.g., Bandura, 1986; Bandura & Cervone, 1986). In contrast, goal-setting theory is narrow in scope (pertaining to performance in work and, to an extent, sports tasks), but the theories intersect in the realm of task performance. Two concepts of social-cognitive theory are especially relevant to goal setting: role modeling and self-efficacy. Role modeling can affect both goal choice and goal commitment (Locke & Latham, 1990). Self-efficacy has a main effect on performance and affects choice of personal goals, goal commitment, and the quality of chosen task strategies (Locke & Latham, 1990, chaps. 3-4). Further, self-efficacy helps to mediate the effects of feedback on subsequent performance (Locke & Latham, 1990, chap. 8).

The reader will note that I have said nothing about integrating goal theory with control theory. I have made it clear elsewhere that I do not regard control theory as a valid theory (Locke...
& Latham, 1990). It is not a grounded theory; it has no database at all. As a model, it is based on a misleading machine metaphor (Binswanger, 1991). Piled on top of this metaphor is a random conglomeration of propositions, all borrowed from other theories (Locke, 1991). In my opinion, control theory is everything a theory should not be.

Goal-setting theory was also integrated with satisfaction theory (Locke, 1976) by showing that goals serve as value standards for appraising the adequacy of one's task performance. Outcomes below one's minimal goal level are judged to be unsatisfactory. Outcomes that exceed the minimum goal level are appraised as increasingly satisfactory (Mento et al., 1992). By adding in causal attributions made for performance, goal theory also can be connected to attribution theory. The satisfaction research led to an interesting paradox, which reflects back on expectancy theory. Hard goals lead to higher performance than easy goals because more is required for one to be minimally satisfied with one's performance when goals are high. Thus anticipated satisfaction, which is the recommended measure of the concept of valence in expectancy theory, is negatively associated with performance, but only when goals differ across individuals. As in the case of expectancies, when goal level is controlled, anticipated satisfaction is positively associated with goal choice and performance (Klein, 1991). Instrumentality is also positively associated with performance.

There are other studies now ongoing that will connect goal-setting theories and other theories. For example, there are studies showing that goals and self-efficacy mediate or form a link in a causal chain leading to performance from money incentives, job responsibility, personality, and charismatic leadership.

**Conclusion**

I hope it is now evident why genuine theory building (as opposed to making up guesses, putting them in boxes, and connecting them with arrows) is a gradual, slow, and painstaking process. There are many tasks to be performed, and each one can take years—none is ever really finished because new knowledge is continually being discovered and thus new integrations must continually be made. I personally doubt that true theories can be built in any other way. For example, could a valid theory of cancer be built in any way other than incrementally—by conducting hundreds of thousands of studies and piecing them together into a coherent whole?

I believe that one requirement of successful theory building (even if it is a theory within a theory, e.g., a theory of goal commitment) is that one stay in the same research area for many years. The reason is that grounded theories are built on data, and it takes a long time to accumulate data. It is unfortunate that few researchers seem to have the interest or tenacity to stick to one area. Fewer still may have the stomach to tolerate the journal review process, which I believe is becoming increasingly onerous (and lower in quality). I hope that I have at least suggested the specific processes that are required for building a viable theory and perhaps have even inspired others to follow the same steps. It has been a fascinating process for me and I have enjoyed doing it, even though I was not exactly sure where it was all going until about 1988, when Gary Latham and I decided: Now there seem to be enough data actually to build theory; let's do it.

One might ask how one can ensure that a theory will be built at all without a grand plan. The answer is that one cannot. But there are steps one can take that will facilitate the process. Most important, I believe that a theory needs a champion. The champion's main tasks are to articulate the basic idea or concept, to conduct preliminary and ongoing research, to inspire others (by example and personal contact), to do relevant research in order to replicate and extend the findings, and, finally, when there are sufficient data, to formulate the actual theory. If enough people with a variety of perspectives get interested, the key tasks of theory building eventually get done. Theoretical "holes" get identified and filled in. Integrations with other theories get made.

In the case of goal-setting theory there were cochampions: myself and Gary Latham. This worked out very nicely, because I was more at home in the laboratory and Gary was very skilled at field research, and each of us inspired others like us. Would things have worked out differently if either Gary or I had not been around? Of course. Does this mean that there is an element of chance in theory building? Not in the sense of uncaused; only in the sense of not predetermined. Research is partly a matter of personal interest and creative ability, but it is also a mat-
ter of volitionally directed thinking and the choice to act on that thinking (Binswanger, 1991). Because people have many options in what they study, one cannot know in advance the direction that a given field will take.

In closing, I would like to stress that I believe the mental frame of the theory champion is critical to the success of theory building. If a champion takes the role of theory defender, the tendency will be to attack the authors of disconfirming studies and theoretical critics rather than to learn from them. As a result, the theory may stultify. In contrast, if a champion takes the role of theory builder, new (relevant) discoveries and insights will be welcomed, and the theory will grow and develop. (Theory defending, of course, is appropriate when a theory has been clearly misrepresented.)

Notes

1. Constructs are sometimes called social constructs, and it is often asserted that people engage in the social construction of reality. I can think of no more epistemologically debilitating idea than that reality is socially constructed. It implies that instead of focusing on reality (evidence and facts) to discover knowledge and build theories, one should focus on the consciousness of other people. But if your own mind cannot perceive reality, how can the minds of other people do it? And if reality is really constructed, rather than perceived, by people’s minds, then why not just make up anything you want, call a group together, get a consensus, and declare it real?
2. This means it was cited 400 or more times.
3. I would like to thank Peter Frost for guiding me to the idea of a theory champion.

References


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