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The science pendulum: From programmatic to incremental—and back?

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Abstract

The climate in which scientific research is conducted changes over time, and in recent years there has been a shift from a positive view of programmatic science to a more negative evaluation that its contribution to scientific progress is only incremental. In this special issue focusing on the tools of a programmatic approach—replication, reliability and reproducibility—I reflect on changes in scientific practice over my research career, considering some factors contributing to changes in emphasis and highlighting potential pitfalls, particularly in terms of the impact on scientific progress and future scientists. In concluding, I suggest that, as members of the scientific community, we can influence current scientific practices in our day-to-day roles as authors, reviewers, investigators, editors, employers, and educators.

Descriptors: Programmatic, Incremental, Science

Despite a common view of the scientific enterprise as constant, impartial, and implacably devoted to truth, its practices are affected by the policies and preferences afoot in the global scientific community. A variety of factors, including economic, sociopolitical, demographic, and others, impact decisions concerning the way research progresses, which can change over time, sometimes swiftly. In recent years, for example, programmatic research, which was viewed quite positively when I started my scientific career, is now often labeled “incremental” science and accompanied by implicit (and sometimes overt) disdain. This special issue of *Psychophysiology* focuses on important tools of programmatic science—replication, reliability, and reproducibility. In this article, I consider some of the factors that have contributed to changes in scientific practice and, most importantly, highlight potential pitfalls that have accompanied the fall of programmatic research, particularly in terms of the implications for scientific progress and future scientists. In concluding, I suggest that we have the ability to impact current scientific practice in our daily roles as members of the scientific community.

In My Beginning

When I first began to participate in grant and journal reviews, the preferred scientific paradigm was clearly programmatic. Senior scientists, reviewers, and editors strongly communicated this preference in both their oral and verbal comments, and it became routine to emphasize and evaluate the programmatic aspects of grant proposals and published research. What this means is that the scientific question (a) relies heavily on past theory and data to justify its rationale, (b) clearly identifies the specific question and how the results will add to existing knowledge, and (c) includes replication and extension as its main methodology. That is, each new experiment is clearly linked to previous research by summarizing the pertinent existing database and considering current theories. The relevant question is then clearly identified, together with the specific gap in the knowledge base that its results will address. Each study generally includes a replication of some of the previous findings and extends the inquiry, typically by testing alternative hypotheses with the goal of ruling out one or more hypotheses (Popper, 1959), rather than simply confirming a single hypothesis. Good programmatic research, with its clear ties to the methodology of detective and forensic investigations, prompts an excited reaction of “I cannot WAIT to see these data” in the case of grant reviews, or a rapid turn to the results section in the case of submitted manuscripts.

Over time, I found that programmatic science was initially downgraded by referring to it as methodological research, which viewed its tendency toward replication, its parametric manipulations, and its consistent focus on a particular paradigm in a negative light. Indeed, one of the pitfalls (perhaps only) of a programmatic approach is if the paradigm itself becomes the focus of study, rather than the phenomenon it was first designed to investigate. Even this pitfall, however, can be critically important for those who subsequently adopt the paradigm, as the moderating factors have been well worked out. Most recently, programmatic study is labeled *incremental* science, with, for example, a rejection letter arriving

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"The NIH Announces Enhanced Review Criteria for Evaluation of Research Applications"

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Does the application challenge and seek to shift current research or clinical practice paradigms by utilizing NOVEL theoretical concepts, approaches or methodologies, instrumentation or interventions? Are the concepts, approaches or methodologies, instrumentation or interventions NOVEL to one field of research or NOVEL in a broad sense?

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Figure 1. New criteria for evaluating research applications (emphases added).

(for a submission generally garnering positive reviews) that states: "In reaching the editorial decision, factors other than the referees' criticisms and recommendations are taken into account. These include novelty and the extent to which the paper is considered to be incremental." To a charge of conducting programmatic research—I'm pleased to plead guilty. Relatedly, in a description of funding for the U.S. National Science Foundation's Computer Systems Research, the applicant is informed that "CSR-funded projects will enable significant progress on challenging high-impact problems, as opposed to incremental progress on familiar problems." Thus, despite the fact that most, if not all, important scientific advances are incremental—adding new knowledge to a developing data base—such efforts are currently not funded/published/supported.

A Shift to Novelty

The variables and pressures contributing to shifts in the preference for how science is done are complex, but because of its reliance on grants for funding in many contexts, changes in how research projects are evaluated can critically affect the resulting science. Of interest for researchers in the United States is a change in the National Institutes of Health (NIH) research criteria listed in Figure 1, which reoriented the evaluative priority to novelty, primarily asking (emphases added): does the research "utilize *novel* concepts, approaches . . .. are concepts, approaches, methodologies . . . *novel* to . . . field or *novel* in a broad sense?" And, of course, science needs new concepts, methodologies, approaches—it cannot move forward without these. But will an explicit focus on the novel, innovative breakthrough produce knowledge independently of supporting the slower, more deliberate experimentation that provides the stable platform for an innovative leap? More importantly, can novel breakthroughs occur, or old concepts fall, in the context of each study, paper, or project—in a single afternoon or even year? Probably not. Rather, science requires time to identify the critical issues, develop an appropriate paradigm, acquire and analyze the data, replicate the results, assess the reliability of the data, and test alternative hypotheses.

One narrative held to contribute to the shift from an emphasis on programmatic research to novelty paints grant reviewers as hyperconservative, unwilling or afraid to fund science that is novel or ground-breaking (presumed to be the domain of the young), in favor of funding established research by experienced "graybeards" (Azoulay, 2015; Bourne, 2013; Garrison & Palazzo, 2006; Kaiser, 2014). Data sometimes used to support this hypothesis (e.g., Kaiser, 2014; Physician-Scientist Workforce, 2014; Rockey, 2015) are illustrated in the inset of Figure 2 (top panel), which shows a decline in funded NIH R01 grants over the past 20 years to investigators less than 36 years old, accompanied by an increase to those 66 years and older. The charge is that the continued support of older PIs stifles new, innovative research.

Of course, what isn't usually appreciated, but which is shown more explicitly in the body of Figure 2 (top panel), is that the bulk of funded R01-type grants is consistently given to those aged 36–66, which is consistent with recent data that notes that, aside from theoretical physics in the early 1900s, the best experimental science is typically conducted by more experienced investigators (e.g., Cole, 1979; Jones & Weinberg, 2011). The very slight increase in funding to older researchers in recent years also reflects the overall youth of the biomedical research initiative in the United States, in which there were no older investigators until recently. With the addition of multiple funding mechanisms designed specifically for younger researchers (Office of Extramural Research,

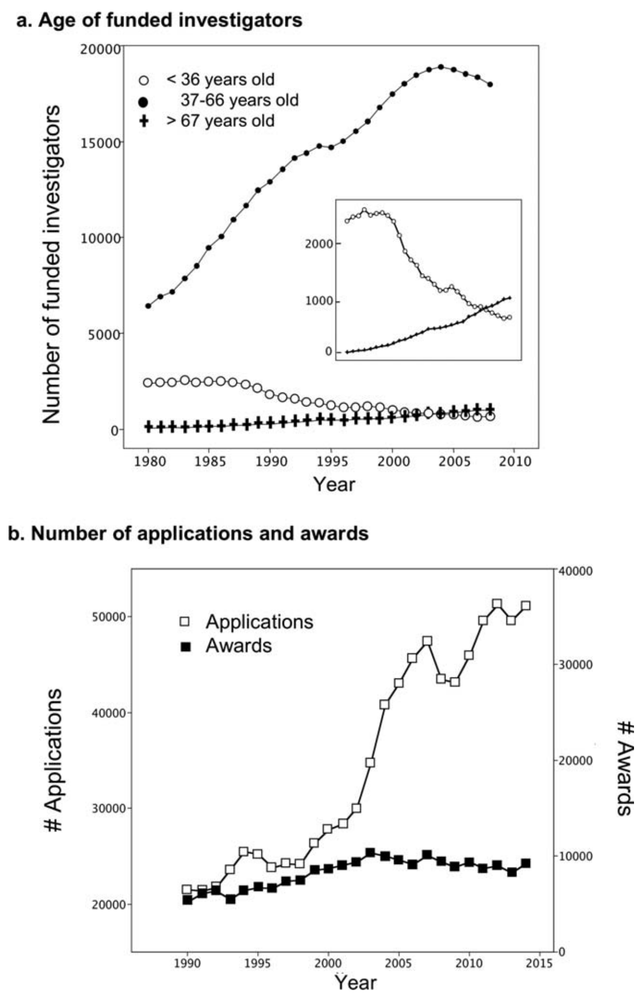

Figure 2. a: Funded applications by age of principal investigator across time; inset shows those > 66 and < 36 with the exaggerated scale typically used to illustrate these data. b: Number of applications submitted from 1980 to 2014 (left axis) has exploded, whereas number of applications funded 1980 to 2014 has remained relatively flat (right axis).

2014; Zerhouni, 2007; Pathway to Independence, PA-06-133; New Innovator Award Program. RFA-RM-07-009, etc.) and policies to explicitly fund early career R01 projects at rates equal to more experienced investigators, youth will be served.

Perhaps a more important factor prompting a change in research criteria is illustrated in Figure 2b, bottom panel, which illustrates that, whereas the absolute number of awarded grants has increased only slightly over the years, the number of applications has exploded, from about 9,000 in 1970 to over 50,000 in 2014[1] (Drugmonkey, 2012; NIH, 2015a; Rockey, 2013). With so many applications, and so few funded, determining which proposals have priority becomes an increasingly onerous, and potentially arbitrary, process. Relatedly, the overreliance on quantitative indices in current hiring and promotion practices not only increases the number of grant submissions, but has simultaneously resulted in an explosion of published papers.[2] One solution, in both the funding and publishing venues, is a shift in priority to the end point of most research endeavors—the novel finding or significant outcome that represents the desired culmination/completion of a research endeavor. Whether a focus on novelty will be successful in producing better science, of course, awaits future data. In the meantime, however, several serious and perhaps unintended pitfalls encourage practices that undermine programmatic science and could prove detrimental in the education of future scientists as well as misdirecting, and probably slowing, future progress.

Pitfall #1

Novelty: Ignore Past Theory and Data

When novelty is the focus of the scientific enterprise, it actively discourages an in-depth investigation of past theory and data as it relates to a new study. Including comprehensive information regarding prior experimentation in the field will quickly be accompanied by criticism that the current effort is, therefore, not novel. Thus, rather than precisely summarizing relevant theory and data, many abstracts and introductions now introduce the current research question by stating that "Little is known about . . .", or "Only a few studies have investigated . . .," as if the low number of prior investigations represents an important justifying rationale. Not only are these statements typically unfounded, but they also require little intellectual effort—the more difficult aspect of justifying a new scientific investigation is clearly summarizing relevant data and theory and precisely articulating how the current research question relates to what has gone before, as well as the specific gap in knowledge its results will address. Moreover, if there really is nothing (or little) known about a specific topic, there is a very good possibility that it is not worth investigating.

Shortly before his death in 1936, Ivan Pavlov described his view of the important features of science in a thought-provoking essay that is illustrated in Figure 3, and which has been hanging in our lab since Peter Lang posted it in the 1980s. In it, Pavlov first emphasizes the inherent, valuable slowness of scientific work: "Gradualness, gradualness, gradualness," he says. And, I love the fact that he adds, "I can never speak without emotion about this most important condition." In discussing this key feature of scientific work, he instructs his students to become comfortable with "severe gradualness," rather than believing that progress occurs in a flash, with a sudden novel insight or breakthrough. He urges patience and a clear grasp of the existing facts (data)—dedicated attention to what has gone before. Without these facts—reliable, replicable, reproducible data—theories are nothing but "vain efforts." If, as a student, you learn that science proceeds slowly, incrementally, it will be much more comfortable to do than to think that each new effort must culminate in a novel, innovative, or significant breakthrough.

The utility and necessity of relying on past data and theory is clearly captured by Newton's famous phrase in a letter to a friend (1676; p. 416, Newton & Royal Society of London, 1959) that "If I have seen further it is by standing on the shoulders of giants." Modern thinking echoes this sentiment, as evidenced, for example, by Google Scholar's admonition to "Stand on the shoulders of giants." But, unfortunately, 21st century researchers are often only able to pay lip service to this useful tenet, given the struggle to avoid considering relevant past data and theory in the quest for novelty. In terms of psychophysiology, for example, it is rare to see mention of fundamental studies conducted by Lacey, Obrist, Graham, and others, which could provide a foundation for many investigations claiming novelty but which instead ignore the past and may, in turn, be ignored in the future.

Pitfall #2

Innovation: Do Not Replicate

To be innovative, one definitely does not want to replicate. Replication, in which the exact same study is conducted for the second or third time, is anathema to innovation. Rather, both grants and journals often implicitly (and sometimes explicitly) demand a statement from the researcher claiming that "This is the first time, to our knowledge, that . . ." The descriptions are getting quite tortured in trying to highlight the novel details of a particular effort. Moreover, in many disciplines, including psychophysiology, finding an effect on one occasion does not guarantee its authenticity. It is finding the same effect in a subsequent study—replication—that is the more difficult, more thrilling, and most persuasive evidence that a new fact has been found. When novelty is front and center, however, interest in replication is quickly abandoned, rendering the replicability and reproducibility of many effects unknown. Distracting, "first time to my knowledge" findings proliferate, providing little direction to new studies and precluding a systematic advance of scientific knowledge.

When considering similar issues regarding progress in science, Lang (2010) emphasizes that "One scientist must be able to see what another one sees, procedures must be replicated and achieve the same result." Ideally, each experimental finding is a building block, providing the foundation for a cumulative science that is shared among researchers. When replication is instead viewed as an unwelcome intruder, the results are blocks that ". . . lay scattered about like the rocks on a New Hampshire farm" (William James, cited by Lang, 2010). When there is no agreement on Pavlov's "facts," there is little chance of a sustained communal effort to

1. The increase in applications is primarily due to an increase in investigators, rather than in the number of applications submitted per investigator (Rockey, 2011).

2. Taking advantage of the scientists' need to publish, journal publishers have never reimbursed or paid researchers for their contributions. Now, a new practice has evolved in which the researcher actually pays the publisher—a sort of scientific vanity press. This model is marketed as a charitable move, in which, rather than keeping new scientific discoveries locked up in elite libraries with paid journal subscriptions, the scientist pays to release it freely to the world. While this does sound admirable, one analogy is to a farmer who buys and tills the field, purchases the seed, plants and harvests the crops, picks, and ships, and then pays those who need to eat (i.e., everybody) money to consume it.

Bequest of Pavlov

"*What do I wish to the youth of my country who devote themselves to science?*

Firstly, gradualness. About this most important condition of fruitful scientific work I never can speak without emotion. Gradualness, gradualness, and gradualness. From the very beginning of your work, school yourselves to severe gradualness in the accumulation of knowledge.

Learn the ABC of science before you try to ascend to its summit. Never begin the subsequent without mastering the preceding. Never try to cover up the gaps in your knowledge with bold guesses and hypotheses. No matter how much this soap bubble will rejoice your eyes by its play, it inevitably will burst and you will have nothing but confusion.

School yourselves to restraint and patience. Learn, compare, collect the facts! Perfect as is the wing of a bird, it never could raise the bird up without resting on air. Facts are the air of the scientist. Without them, you will not fly. Without them, your theories are vain efforts.

But learning, experimenting, observing, try not to stay on the surface of facts. Do not become the archivists of facts. Try to penetrate to the secret of their occurrence, persisently search for the laws which govern them.

Secondly, modesty. Never think that you already know all. However highly you are appraised, always have the courage to say to yourself - I am ignorant. Do not allow haughtiness to take you in possession. Due to that you will be obstinate where it is necessary to agree, you will refuse useful advice and friendly help, you will lose the standard of objectivity. In the team of which I am leader, everything depends on the atmosphere. All of us are harnesses to a common cause and each pulls his weight. With us it is often impossible to discern what is "mine" and what is "yours," but our common cause only gains thereby.

Thirdly, passion. Remember that science demands from a person a whole life. If you had two lives that would not be enough for you. Be passionate in your work and your searchings.

Written just before Pavlov's death, at the age of eighty-seven years, on February 27, 1936.
Translated from the Russian by Professor P. Kupalov, Chief assistant in the Pavlov Institute at Leningrad.
From Blakeslee, Albert F. Science, Vol 83, no 2155, April 1936, pg. 369.

Figure 3. Bequest of Pavlov.

move the science forward. For newer scientists, its negative impact can be seen in publication lists that are populated by studies utilizing an array of paradigms, populations, and procedures, as the young investigator shifts rapidly from one project to the next in an effort to never do the same thing twice.

Pitfall # 3

Significance: Immodest Claims

Claiming that each new study or project has significance is probably the most emotionally disturbing pitfall, as it continually presses the scientist to formulate, articulate, and broadcast immodest claims. As the second most important scientific principal, Pavlov noted *modesty*—the humility required not only when an individual assesses their own knowledge base, but also when evaluating the potential scientific contribution of a research effort. Many, if not most, of the paths on the way to victory will seem dull to the uninitiated and trivial to the impatient. Nonetheless, these steps cannot be skipped—the paradigm must be tweaked, the analyses changed, the hypotheses reformulated. The data will not replicate; dead ends will occur. Each effort, although providing necessary information, will not unconditionally deliver new facts or truth to the world. To require scientists to continually claim that each step of their scientific journey is highly significant does science a disfavor, forcing the scientist to claim more than he would like to, earlier than he is able to.

And . . . Back?

There are signs that the pendulum is swinging back towards valuing the tools of programmatic research. Somewhat amusingly, replication and reproducibility are now themselves heralded as significant new "novel" initiatives.[3] For instance, in addition to the recent highly publicized effort of the Open Research Collaboration (2015) to reproduce results of a large cohort of psychological studies, current comments include "Journals, funders, and scientists are paying a lot more attention to replication" (Srivastava, quoted in Yong, 2015), and "Many people in their early career seem willing to challenge the status quo and find a way to do this better" (Nosek, quoted in Novotney, 2014), as if replication and reproducibility were suddenly new scientific tools, rather than the long-standing foundation tools of programmatic scientists.

Moreover, recent initiatives to coordinate and administer large-scale replication studies somehow miss the mark, as replication is instead best conceived as part of the everyday science toolkit. Thus, it is most useful when each investigator makes an effort to determine whether a new finding replicates previous studies prior to submitting the data for publication, as well as to include replication conditions in new studies. In this way, the quantity of studies with unreproducible findings will naturally diminish. And, when investigators in the same field of study pursue programmatic efforts to replicate this natural use of replication will move the field along more smoothly than large-scale replication efforts that are coordinated and conducted by individuals with little or no real interest in the specific paradigm or effect. Many have noted that large-scale replication efforts hold the possibility of evolving into a type of witch hunt, explicitly or implicitly blaming the failure on the initial investigator, which will not prove helpful in advancing scientific progress.

Given the sheer increase in the number of those pursuing a research career and their associated output, however, some fundamental changes in the structure of scientific society may be necessary. Among the possibilities is abandoning the single scientist/single lab modal model favored in many fields, including psychology, in favor of supporting integrated, geographically proximal, normally evolving research teams (e.g., Bennett, Gadlin, & Levine-Finley, 2010). That is, rather than forcing existing independent scientists to form unnatural or long-distance liaisons, the goal is to start developing teams from the ground up by explicitly seeking new faculty with overlapping, closely related interests with others in the department, rather than the more typical search for a scientist whose research interests are completely unrelated. Research teams are not only economically more feasible and scientifically more productive than single individuals pursuing similar research in far-flung locations, but consolidating research into teams could reduce the quantity of grant applications and manuscript submissions. A team model not only holds the promise of facilitating the speed of scientific progress but also prompts natural modesty, since, as Pavlov noted, ". . . it is often impossible to discern what is 'mine' and what is 'yours,' but our common cause only gains thereby."

In addition, our students—the scientists of the future—need to be brought up to speed, quickly, to learn that there are no shortcuts in science and no significant research that is so novel that it does not build on what has gone before—rather, science is fundamentally cumulative, and it advances incrementally. Fears that young scientists in the current climate will be deterred (Kaiser, 2014) are ill-founded. Pavlov's third principle of scientific progress—passion—suggests why the best young scientists do not slip quietly away. When a person has what Pavlov called the "instinct for research," one's passion for the scientific enterprise sustains a peculiar persistence. It is scientists of all ages, fueled by this passion, working carefully, consistently, and incrementally in laboratories all over the world who have led us to where we are today and who are not easily dissuaded by shifts in scientific fashion or funding.

It is as members of the scientific community—reviewers, editors, educators, employers, authors, and investigators—that each of us can contribute to setting the current priorities in scientific practice. Among the more pressing issues in returning to programmatic science are requesting a clear summary of relevant past data and theory in justifying the rationale for new studies rather than relying on statements that "little is known"; acknowledging and appreciating replication and reproducibility when these appear in grant and manuscript submissions; refraining from asking scientists to claim novel, first-time-ever status for current methods or results; and patience in terms of understanding the eventual significance of a specific finding (which is often not realized until many years later). As an emerging "slow science" movement afoot in Europe says, "We cannot continuously tell you what our science means; what it will be good for; because we simply don't know yet. Science takes time" (Slow Science Academy, 2010). And, as Pavlov notes in his final statement, even two lives might not be time enough to make the truly novel, truly significant contributions that result from incremental, programmatic research.

3. Portions of this essay were originally presented in a symposium at the 2010 meeting of the Society for Psychophysiological Research (SPR), with the writing undertaken and completed in the summer of 2015. More recently, NIH released updated criteria for applications and grant reviews, developed specifically to enhance "reproducibility", "rigor" and "transparency" in scientific research (NIH, October, 2015b). Although the (implicit) shift from a focus on novelty to reproducibility is an important step in the right direction, the mandate that designs and methods now explicitly include information and evaluation of "how they will achieve robust and unbiased results" will necessarily include experimental replication, as it is the best method for achieving these goals. How quickly reviewers, editors and other members of the scientific community are able to shift from an emphasis on novelty to reproducibility remains to be seen.

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