PERSPECTIVES

Conducting Research as a Busy Clinician-Teacher or Trainee

Starting Blocks, Hurdles, and Finish Lines

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By far the best way to become proficient in research is to get on with it . . . It is psychologically most important to get results, even if they are not original. Getting results . . . brings with it a great accession of self-confidence.[1]

Peter B. Medawar, in his classic work *Advice to a Young Scientist*, speaks to the unshakable inertia that so commonly shackles junior investigators. As a Nobel laureate in the biological sciences, he understood the flypaper effect: that the project which entices a beginning researcher can at the same time entrap him or her in a poisonous glue. Indeed, Medawar reflected on his own early career: "When the time came for me to begin research I had not a clue how to start." In reality, potholes can appear not only at the commencement of a project but anywhere along the road to completion. Supplementing the advice provided by previous authors,[2-7] I offer some practical suggestions for beginning investigators and busy clinician-teachers. Although full-time or senior investigators may find some useful tips here, this article is intended primarily for those embarking on a project early in their careers as well as those whose jobs allow them only limited time for research. Funding is not addressed. Grantsmanship is an essential skill for career investigators but is beyond the scope of this article and, moreover, may be less critical for those conducting their first or an occasional project. Obstacles other than money commonly impede the initiation and completion of research, particularly for the busy clinician-teacher or trainee. Strategies for surmounting these obstacles are the focus of this essay.

FEELINGS

Do you really want to do research? Sometimes, a project is compulsory rather than elective, as when residency review committees require research in certain graduate medical education programs.[8] Fellows also may have a mandatory project, and medical school faculty can feel browbeaten into research by the traditional publish-or-perish mantra. This raises questions like (a) Should the rationale for requiring research of all physicians-in-training be revisited? (b) Are there better methods of motivating and enabling those required to complete a project? (c) Can we accelerate the movement toward promoting clinician-educators for their teaching, a doctrine that in many institutions is still given lip service? In any case, "Do you really want to do research?" remains a critical question for the stalled investigator, as a lukewarm response may be the main reason for stasis.

How enthusiastic are you about a particular project? New investigators often sign onto projects recommended by someone else, but in so doing should make sure their own interest exceeds at least a certain threshold. The following is a useful scale to gauge how you feel about a potential assignment:

- Passionate
- Curious
- Open
- Skeptical
- Averse

Persons are seldom passionate about an idea not of their own making, but be wary of taking on a project about which you do not feel at least "open" or, better yet, "curious." Apathy will make completion especially difficult.

How much of your other activities do you want to sacrifice? Many academic physicians value patient care, teaching, and administration as well as research, and they realize that involvement in all of these activities entails tradeoffs between depth and breadth. Research has the broadest reach, well beyond institutional walls, but connectedness to the scientific community at large seldom approaches the intimacy of a doctor-patient or teacher-student relationship. Also, research is notable for its delayed rewards compared with the immediacy of the doctor-patient or teacher-student relationship. An apt metaphor is dropping a pebble into a pond: the most intense and immediate impact is the point at which the pebble hits the water (patient care), followed by concentric waves (teaching) that diminish in amplitude (research) the farther they spread out from the initial splash.

None of these activities is superior to the others; substituting more of one for less of another can leave lin-

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gering feelings of sacrifice and nostalgia. Different individuals will find different mixtures of clinical, teaching, administrative, and investigative pursuits most comfortable, and the balance may also change throughout a particular individual's career. Nonetheless, research unavoidably subtracts from other tasks. The periods of relative unavailability for patients, pupils, and coworkers are an inevitable compromise.

IDEAS

Picking a Field (Locus)

The field is the broad area from which the investigator samples specific questions. Most commonly, this is a *content* area, such as a disease (cancer, AIDS, or depression) or a discipline (preventive medicine, ethics, geriatrics, or the doctor-patient relationship). Occasionally, it may be an area of *methodology*, a special research skill or technique (decision analysis, meta-analysis, prediction rules, analysis of large databases, or education research) that one can ply as either principal investigator or collaborator in any number of content areas.

Picking a Question (Focus)

Is your question *important*? In conversations with colleagues listen for, "So what?" Clinical research should have an impact on someone's clinical practice or decision making or, if it is a more methodologic project, on other researchers' work. Gertrude Stein is attributed with saying, "A difference, to be a difference, must make a difference." Ginsberg and Ostow wrote: "The challenge . . . is to formulate good questions and to avoid being overimpressed with proposals that demonstrate high orders of methodologic sophistication. Progress can make use of such sophistication, but it needs more."[9] Pellegrino expressed chagrin that "investigators seem to have settled for what is measurable instead of measuring what they would like to know."[10] Immanuel Kant observed: "It is wisdom that has the merit of selecting from among the innumerable problems which present themselves, those whose solution is important to mankind."[11] The rigor of a project should not outshine its relevance.

Potential *sources* of research questions include journal reading, scientific meetings, daily tasks, and mentors. In reading articles relevant to patient care or teaching or in perusing abstracts and posters at scientific meetings, one often happens on questions or study designs that could be modeled, adapted, or improved upon for projects of one's own. There is the sudden realization in coming across a published article or abstract, "Hey, *I could do* that!" For early projects, more realistic sources of inspiration might be the secondary rather than lead journals in one's field.

Regarding daily tasks, a useful maxim is: Make your work your play. What problems do you see in the clinic or on the wards that intrigue you and for which the literature provides inadequate answers? As a teacher, are there projects in medical education or even simple innovations you could describe in writing? Can administrative chores such as quality assurance or utilization review yield data that would be useful to analyze and publish? A potential laboratory surrounds us each day.

A mentor is probably the ideal solution for the junior investigator in search of a project, not only as a source of ideas but also as an adviser to guide one safely through all phases of the research. However, as mentioned above under "Feelings," make sure your enthusiasm about the line of research is in synchrony with your mentor's.

Finally, what is the project's *feasibility* in your system? Is the method needed to answer your question user-friendly, given your time and resources? Pursue something that is reasonably do-able. If necessary, aim small. Residents in particular, as well as fellows or faculty whose time for an initial project is limited, are better off carrying *a tiny project to completion* than an ambitious proposal half-way. First projects are like beginning piano lessons, a time to master scales rather than favorite tunes. Admittedly, grantsmanship compels full-time researchers to develop a theme early in their careers. Nonetheless, the investigator who attempts this differentiation prematurely takes a risk similar to that of the medical student who chooses a specialty before being certain of his or her true preferences.

TIME

Following are nine suggestions for maximizing limited research time:

1. *Protected time* that is not supported by extramural grants is a dwindling commodity in academic medicine and is frequently in short supply for busy clinician-educators. Nonetheless, even *a little bit of protected* time—one or two half-days per week for a junior faculty member, or one or two research blocks for a resident—can decompress an overbooked schedule. Patient care is a very dense activity, and a week saturated with clinics and ward rounds can deflate one's enthusiasm for research. Although more porous than direct patient care, heavy teaching loads can also be exhausting. Even a little protected time provides the breathing space that makes research emotionally as well as physically possible. It also provides some time for making phone calls, meeting with other investigators, reviewing charts, seeing study subjects, and carrying out other research activities that are best done during business hours.

2. Even with protected time, much research is conducted during *off-duty time*. Reading, writing, and analyzing frequently occur at night and on weekends, just as preparation for teaching is often done at home. Although the academic physician may be less encumbered than the

community practitioner by early morning phone calls or by midnight trips to the emergency room, research activities often extend beyond office hours.

3. *Partitioning of tasks* must be inviolable. The faculty member awarded one or two half-days of research time must be vigilant against the five P's: patients, phone calls, paperwork, pupils, and peers. That extra patient scheduled as a walk-in on your free afternoon, an open-door policy for eager learners, the ever-present memorandums with suspense dates marked yesterday, the ringing telephone, the colleague anxious to bend your ear or socialize—all dissipate protected time. Likewise, the resident who schedules a month of research may fall short if he or she is tagged for extra call duty, takes one or two weeks of vacation, or attempts another educational objective like studying for the board examinations or learning to read echocardiograms. Protected time must be sheltered with extraordinary self-discipline.

4. Research approached through *incremental tasks* seems less formidable. Projects can be divided into eight stages: (1) find a question; (2) open it to criticism; (3) review the literature; (4) prepare the protocol; (5) collect the data; (6) analyze the data; (7) submit an abstract to a scientific meeting; (8) write the paper for publication. Taken one at a time, each stage is less intimidating than the project in its entirety. Beginning investigators can be overwhelmed by the realization that they have to complete a project. In reality, research is a series of bite-size tasks, each more digestible than the project swallowed whole.

Literature review can be an unsettling experience. Before searching the literature, a new idea may seem special, if not unique. Not uncommonly a letdown occurs when studies are discovered that even partially resemble the proposal being contemplated. Be careful, however, of prematurely abandoning what seems like a good question simply because you are not the first to ask it. Often you can build on previous studies, learn from their methods and findings, improve on their weaknesses, and further what is already known. Indeed, the absence of any previous studies on a question makes one wonder whether it is of broad clinical interest or whether a study designed to rigorously answer this question is feasible. Sir Francis Darwin wrote that "in science the credit goes to the man who convinces the world, not to the man to whom the idea first occurs."[11]

5. *Diversification* of your research portfolio means you can have different roles on multiple ongoing projects, being principal investigator on one or several studies, and collaborating as a coinvestigator on others. The productive investigator eventually commits to different levels of projects simultaneously, including one or more that mature quickly (reviews, descriptive studies, cross-sectional studies, analysis of data already existing in medical records or databases) as well as long-term investments (cohort studies and clinical trials).

6. *Prioritization* of your research goals is essential. Lee Goldman writes: "It always takes longer than you think . . . . Time spent on low-priority studies limits time available for high-priority studies."[5] Although this advice may seem contrary to the principle of diversification, overcommitment can produce a Starling curve effect: higher filling pressures increase output up to a certain point, after which efficiency plateaus or even declines. This effect has several implications.

First, you must develop exclusion criteria for the types of projects you do *not* want to become involved in. Although the criteria would be highly individualized, examples of low-priority research for some investigators might include (a) drug company studies in which the major reason for participation is extra funds for the individual or division; (b) projects conducted by other investigators, especially if they are outside your division, in which you are asked to help out as a worker bee, but for which your personal enthusiasm is low; (c) requests-for-proposals (RFPs) that are driven by the potential of funding rather than a vision. John Eisenberg cautions that "this RFP mentality creates a research program of opportunism rather than creativity."[4]

Second, even for projects that exceed the threshold you have established for personal participation, successful completion of one stage of a project often requires temporary blinders regarding other projects. Dedicating a full month to the completion of an overdue manuscript is often preferable to fragmenting the 4 weeks among numerous unfinished projects. Creative energy can be difficult to mobilize in the repetitive picking up and laying aside of a task. Momentum for writing a protocol, analyzing a data set, or finishing a paper is hard enough to sustain without the added distractions of flipflopping.

7. *Doublebooking* nonresearch activities can be efficient. Whenever possible, squeeze that extra patient into an already scheduled clinic half-day; finish light administrative tasks during clinic precepting in the interludes between learners; return telephone calls in blocks; meet with others immediately before or after clinical, teaching, or administrative activities rather than in the middle of a research afternoon.

8. Clinical investigators can fruitfully make their work their play, but it is not always the optimal playground. For those whose clinic office and private space are one and the same, periodically *vanishing* to a library, home, or other secluded haven may be necessary to attain the uninterrupted peace required for sustained concentration and creativity.

9. Large, ongoing responsibilities in a study typically require dedicated research monies to support the efforts of others. On the other hand, discrete tasks can be delegated by *identifying allies* who surround you, willing yet unrecognized. For example, mailings, telephone calls, handing out self-administered questionnaires, and limited screening of records can sometimes be carried out by secretaries, receptionists, nursing staff, or other personnel, even though their primary duties are in support of clinical or educational activities. Certainly, the time in-

volved should be small in relation to their main duties, and participation should be negotiated rather than coerced. However, when research is viewed as an integral part of the body of medicine, synergistic with patient care and teaching rather than parasitic, coworkers may actually value participation as a special dimension to their job.

BLOCKS

Accept creative blocks. Like an artist, a scientist has waxing and waning periods of inspiration. Because productivity fluctuates, compromise may be necessary. While rejecting mediocre ideas, be willing to proceed with adequate ones.

Begin. Some investigators are 95% satisfied with their ideas or proposals yet delay protocols or projects for months, or even indefinitely, seeking that last 5% of perfection. Julia Cameron, a consultant for artists, observes:

> *Perfectionism has nothing to do with getting it right. . . . Perfectionism is a refusal to let yourself move ahead. It is a loop—an obsessive, debilitating closed system that causes you to get stuck in the details of what you are writing or painting or making and to lose sight of the whole. . . . A book is never finished. But at a certain point you stop writing it and go on to the next thing. A film is never cut perfectly, but at a certain point you let go and call it done. That is a normal part of creativity—letting go. We always do the best that we can by the light we have to see by.*[12]

Medawar counsels, "Beware of the . . . novice's inclination to spend weeks or months 'mastering the literature.' Too much book learning may crab and confine the imagination, and endless poring over the research of others is sometimes psychologically a research substitute."[1]

Admit that clinical research is intrinsically "messier" than basic research. This is not an apology for sloppy or weak studies, but rather a frank admission that it is seldom possible to design the perfect clinical study. None of the biological sciences is as "pure" as physics, and within the biological sciences, in vivo clinical studies will always be at a scientific disadvantage to the in vitro nature of the bench. Moreover, the integrative rather than reductionist focus of clinical research, examining humans rather than cells or molecules, makes it a softer science at the same time that it remains an essential field of inquiry.

Set the deadline. Dividing the project into one of the eight stages previously described and attacking them one at a time is one strategy for overcoming blocks. Setting deadlines is another. How many citizens complete their tax forms in January? Except for some of those expecting a generous refund, the majority of taxpayers wait until April 15th gets much closer. Anyone who has reviewed abstracts for a scientific meeting understands that while calls for submissions go out months in advance, abstracts trickle in until a few days before the deadline whereupon an avalanche of express mail arrives. Why do requests from administrators often have suspense dates, insisting we reply NLT (no later than) a certain date? These examples simply illustrate a tendency for many people to delay completion of tasks unless a deadline is established. Research is no exception. Indeed, the very fact that the frustrations of research are immediate while its gratifications can be quite delayed make it essential to establish a series of time objectives for the various stages of the project.

Agree on a reasonable time period for completing the paper. For protocols, universities generally have regularly scheduled institutional review board meetings, while funding agencies divide the calendar year into submission cycles. Scientific meetings typically have deadlines for receipt of abstracts, and writing an abstract requires not only completion of data collection and analysis but also the crafting of one's findings into the IMRAD format (introduction, methods, results, and discussion) necessary for eventual publication. Since finishing the manuscript is one step for which deadlines are often the most vague, it is imperative that the principal investigator and any coauthors or mentors decide on a time frame for the project. This is necessary not only to achieve ultimate credibility for one's work—which depends on publication in a peer-reviewed journal—but also to avoid the "embittered collaborator syndrome," wherein others who have donated varying amounts of their own valuable research time to a project feel unrewarded when the person responsible for leading the writing efforts falls behind. In such instances, it is far better for a willing coauthor to step forward and complete the paper, appropriately rearranging the order of authorship.

Writing the paper can indeed be one of the more unassailable blocks, and useful resources are available for preparing manuscripts[13–15] and other scientific presentations.[16–19] Although the manuscript length is variable, anything more than 15 double-spaced pages of text (excluding tables and references) should be carefully scrutinized for potential pruning, while 20 pages approaches frank corpulence. A 15-page text might consist of an abstract, a 1-page introduction, 3 to 4 pages of discussion, and the middle 8 to 10 pages divided between methods and results.

Choosing the appropriate journal may require a reality check. Authors have a natural tendency to overestimate their own work and to aim high. Submitting to the most prestigious journals, which typically have an acceptance rate less than 20%, simply delays the publication of many papers. This is not to say that authors should undervalue their work or refrain from submitting excellent research to major journals. All the same, even the most successful investigator typically has a handful of articles accepted in premier journals with many more publications in secondary and tertiary journals.

Handling rejection letters from journals is something to which authors must get accustomed; a related experience is getting "pink slips" on grant proposals. In both instances, rejection is really a five-stage process:

- Anger (They missed the point; didn't read it; were biased.)

- Dejection (Nobody likes my work. I failed in this project.)
- Reflection (Hmmm . . . maybe they have a point here.)
- Resurrection (Well, let me try to fix what I can.)
- Resubmission (It's back in the mail. I feel a little lighter today.)

Although sometimes it is necessary to percolate cognitively and emotionally, usually it is best to work through all five stages in 60 days or less.

TOOLS

Study design can be learned in formal clinical epidemiology courses, in journal clubs that emphasize skills in critical appraisal of the literature and evidence-based medicine,[20] in divisional research meetings and seminars, by attending abstract presentations and poster sessions at scientific meetings, by the actual process of writing research proposals and responding to the critiques of institutional review boards and funding agencies, and by serving apprenticeships on ongoing projects. General internal medicine fellowships as well as clinical research fellowships available in other areas (e.g., health services research; clinical epidemiology; medical education; clinical ethics) have grown rapidly in the past 15 to 20 years and often include core course work in clinical epidemiology, biostatistics, computer sciences, grant writing, and other research skills.[21, 22] Even for full-time clinician-investigators, the optimal balance between a curricular approach to research training (e.g., obtaining a master's degree such as one in public health or a science) versus an apprenticeship (doing as much research as possible with an effective mentor) remains to be determined.

Introductory courses in medical schools along with the advent of statistical packages have made *statistics* less exclusive and more vernacular. Moreover, for a number of projects a knowledge of descriptive statistics (percentages, means, and standard deviations), *t* tests, and contingency tables is adequate.[23] Admittedly, over the past 20 years increasingly sophisticated analyses have appeared in biomedical research, including multivariate analysis, decision analysis, meta-analysis, receiver-operating characteristic curves, life tables, and other advanced statistical techniques. Still, it is uncertain how much the investigator needs to learn in advance and how much is best acquired when a particular project demands it. Medawar reflects on this:

> *The process of "equipping oneself" has no predeterminable limits and is bad psychological policy, anyway; we always need to know and understand a great deal more than we do already and to master many more skills than we possess. The great incentive to learning a new skill or supporting discipline is an urgent need to use it. For this reason, very many scientists do not learn new skills or master new disciplines until the pressure is upon them to do so; thereupon they can be mastered pretty quickly.*[1]

An introduction to advanced statistical techniques is fine, but true comfort with a particular technique, such as logistic regression or Kaplan-Meier curves, will best be gained when working with a "living" data set.

Collaboration can be another mechanism for fostering research. Potential benefits of working with other investigators include (a) wider expertise, especially when certain statistical, methodologic, or subspecialty skills are needed on a project; (b) increased productivity, in which a person can be principal investigator on one project and coinvestigator on others; (c) collegiality within a division, in which research is not perceived as a favor for a few but as a team sport, a common mission complementing patient care and teaching; (d) increased patient numbers and generalizability of findings if collaboration involves other study sites.

Collaboration is not without risks. Dependency can sour quickly when tasks you were relying on (patient accrual, collection of data, special assays or procedures, statistical analyses) are interminably delayed. Goldman warns that "the probability of success is inversely correlated with the number of collaborators; be prepared to take personal responsibility to be sure the project is completed."[5] Still, do not prematurely abandon collaborative efforts. Like a volunteer coordinator, the principal investigator successfully delegates by (a) defining the task explicitly, (b) establishing deadlines for desired completion, (c) giving periodic reminders as well as second and third chances, (d) building time cushions into the project, (e) providing leeway for exactly how a task is done, short of compromising methodologic rigor—understanding that delegation invariably involves relinquishing some control, (f) reclaiming others' jobs only as a last resort when it becomes clear they have neither the time nor the interest to fulfill their obligation.

Medawar puts collaboration into perspective when he says that it "is a joy when it works, but many scientists can and many do get on very well as loners." This may not be true of major proposals, which increasingly require a multidisciplinary team. As early as 1934, a famous pathologist noticed: "Research has deserted the individual and entered the group. The individual worker finds the problem too large, not too difficult. He must learn to work with others."[24] Nonetheless, one or several investigators may still be adequate for that modest early project or humble clinical study.

Clinical research has many secondary gains—academic promotion, recognition outside one's own institution, answers to troubling questions, variety in the professional life of the clinician and educator, and the possibility of making a difference in the lives of patients other than one's own. However, the gratifications of research are delayed, its dead ends appear plentiful, available time and funding are often inadequate, and one's findings can seem infinitesimal compared with the endless universe of questions. As Theobald Smith concludes, "The joy of research must be found in doing since every other harvest is uncertain."[22] The *doing*, however, can be facilitated.

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