

Interviewing Scientists

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1 Why Interview? Exploring Scientists' Knowledge Beyond Papers

When I interview a scientist, I am most interested in the scientist as a producer of a body of work, and as a member of a greater scientific community producing the work that constitutes a field of research.

A scientist's works are published individually or jointly, often through various journals or publishers, and via various formats: journal articles, books, proceedings, working papers, monographs, preprints.

Nevertheless, what gives rise to each publication is a scientific labor that one doesn't see. Between the publication of papers, scientists are working on them. In fact, they are doing more. They are doing science. They are speaking with colleagues. They are enduring false starts and revisions. They are, perhaps, making academic visits or attending conferences. They're at the bench in the lab, at the chalkboard, or in a code notebook.

Much transpires between publications. Eventually, scientists allot time to write. The science that they do is given representation in a publication. However, the more one speaks with scientists about their research, the clearer it becomes that papers are no more than representations. One becomes more convinced – at least, I became more so – that a scientific field is much more than its literature. It's represented by the literature.

Whereas publishers are keepers of the written record, the scientist is the keeper of the greater working or folk knowledge that guides scientific labor and strategy. Scientific communities are distributed, living archives of the actual practice of science. Their knowledge is what constitutes the field; that's what the papers represent. An interview, if sufficiently thoroughgoing, can occasion an interface with the greater body of knowledge held by a community.

Even if one reads everything a scientist has published in full, when speaking with them, one might quickly come to find that there is much that they know – novel to their own research – that wasn't conveyed in the publications. Such knowledge might not have even been known to their colleagues; scientists only share so much.

Thus, I take great interest in interviewing members of scientific communities, for they are the ones who carry the greater knowledge of contemporary science. In the most virtuous scenario, however, one isn't merely drinking knowledge from this reservoir for personal edification; the knowledge that one receives is itself unevenly distributed within the community. One can speak with a scientist and then – so long as the conditions are right and everyone agrees – share that knowledge with the community. Although internal knowledge is often unevenly

distributed, there are sometimes opportunities for redistribution, and an interview-driven medium might sometimes be able to play a role, if invited to do so.

In principle, a deep interview can uncover and (re-)distribute valuable knowledge beyond the published

record. The prospect of deepening an organizational capacity to give representation to such knowledge is, for me, an interesting one. In the following piece, I will discuss this prospect, and the techniques SciSci Research has deployed to date in attempts to explore it.

2 Consider the Interviewee: Why Do Scientists Do What They Do?

One can pose many questions regarding the behind-the-scenes processes of labor and strategy that guide science.

Before we begin with greater methodological commentary, an initial exercise might be helpful. Let's begin with a relatively simple question: after completing a study, how does the scientist go about choosing their next research topic?

Now, put yourself in the position of the scientist, the interviewee. They might begin to recall the factors that motivated their choice. Were they content with their last paper, seeking afterward to build upon it? Did they foresee the need for amendment, or feel the need to take the research in a new direction? Alternatively, was their next piece of research a parallel development? I can think of real examples of each of these scenarios.

By considering such scenarios, we are beginning to think about possible mechanisms of scientific labor and strategy. In very crude terms, every new research endeavor is a kind of gamble. Although we don't always think about matters this way, few would deny that the scientist is, materially, allocating capital – be it a grant, a donation, or their salaried time – towards knowledge production. Sometimes, the scientist hedges over parallel pursuits. Sometimes, one is “all-in”, reinvesting the fruits of previous re-

search. Sometimes, one is recovering from a disappointing outcome.

However – and I think this is what makes science so honorable a pursuit – in many cases, the scientist is not assessing risk according to economic or market parameters. What I mean is: although the scientist is allocating capital, the degree of success or failure that they enjoy is not, in many cases, related to profit or return; success is actually defined by scientific communities.

Thus, although the input to scientific labor is capital, the output is knowledge; and assignment of scientific value to produced knowledge is very much a community affair, which can be hard for outsiders to understand.

A scientist's labor and strategy is in many cases in correspondence with the greater folk knowledge, aspirations, and culture of their community, which help to inform the valuation of scientific work. Community dynamics as such are often not made explicit in publications (and, indeed, understandably so).

Those with scientist friends may find that they speak of such dynamics often. One might have presumed that scientists would spend more of their time, during casual conversation, talking about theories, concepts, data, results. That's their work; and that's what is shown in their papers. Indeed, the first thing that happens to their

CONSIDER THE INTERVIEWEE:
WHY DO SCIENTISTS DO WHAT THEY DO?

work, often, is publication. That's what we see. The greater fate of their work, however, is communal. Thus, one can understand why scientists might speak at length about community dynamics; they often have a vested interest in what becomes of their research.

Let's return to the question of a scientist selecting their next research topic. One can consider various factors, but how they are made functional in decision-making is another matter. In many cases, the scientist's choice will depend on some evaluation of the "success" of their previous efforts, or those of others. So, we can ask: how does the scientist evaluate success? What renders an outcome satisfying or disappointing?

Perhaps the greater community has agreed, at least majority-wise, on certain strategic goals. If the paper makes a contribution theretoward, the scientist might take satisfaction in the paper. On the other hand, goals are regularly updated. Perhaps achievement of a research goal, shockingly, reveals the goal itself to be based on

a partial understanding of the topic. Perhaps the acquisition of knowledge reveals darkness deeper than the light it sheds. Perhaps the scientist's own work is, even if respected, understood by few, and has inspired a more personal set of goals that are different from, though not necessarily incompatible with, those of the community.

Again, I can think of real examples of each of the above scenarios.

Various outcomes follow. Sometimes, a paper will achieve a certain standing, only for its reputation to change later. It may have been a cause for celebration, only later to be regarded as contingently wedded to an outmoded paradigm. It may have amounted to no more than a whisper upon its release, only to achieve resonance later. Furthermore, valuation of a piece of science can be heterogeneous within a community. Thus, understanding a scientist's work may require grasping the longer-term valuation of research; theirs and others'.

3 Serving Scientist Communities: The Insider-Outsider Relationship

Sometimes, shorthand terminology can be convenient. Nonetheless, where terminological matters are concerned, what is shorthanded for some is heavy-handed for others. Thus, I beseech the reader to make an allowance for one, possibly heavy-handed, terminological play. I'd like to subsume the community knowledge discussed above – to include, for instance, the evolving strategic sensibilities of communities; the faculties of communal reception determining the valuation of research in communities; and the auxiliary, behind-the-scenes knowledge regarding scientific labor, strategy, and valuation – under the cheap umbrella term of “meta-scientific knowledge”. This can be knowledge used by communities when making research decisions, appraising each other's findings with respect to goals, or organizing work. I am interested in such meta-scientific knowledge because it rarely manifests itself fully in papers but is often the first thing a scientist talks about when one discusses work with them.

At the risk of irony, it will be mentioned here that the use of the term "meta-scientific" is employed in a manner that is mostly indifferent to certain ongoing discourses on "meta-science". SciSci is aware of numerous think-tanks and groups raising funding and producing works as authorities on "meta-science", which is often a kind of policy-making discourse on questions

of progress in science. SciSci seeks no such bestowal of authority. Rather, "meta-science" is, for us, simply an expedient residual category by which one can distinguish the types of knowledge discussed previously from that conveyed by the scientific record itself. Nonetheless, one might ask why SciSci uses the same term, whilst also expressing our disinterest in calling ourselves "meta-scientists" or authorities on meta-science or progress.

The SciSci view is that scientists themselves are the meta-scientific authorities; that's why we assign such significance to interviews. The best role for an outsider is not to vie for authority, but to help redistribute the knowledge that meta-scientific authorities hold within communities. It may not be necessary to pay outside meta-scientist policy scholars to write white papers on trends or progress in science. One might instead simply find a way to give fuller expression and representation to the meta-scientific knowledge that all scientists and communities already hold but infrequently communicate. Scientists are the meta-scientific authorities, and they are the ones for whom rarefied metascientific information is directly actionable. Outsiders obviously can't direct communities based on their own external progress theses. Nonetheless, outsiders can perhaps play a role in helping communities communicate with themselves.

4 Publishing First, Interviewing Later: Stages of the Research Life Cycle

Indeed, one doesn't routinely find meta-scientific knowledge attached to a publication. After all, a piece of research, more or less, begins its official life cycle as a paper (or preprint) in a community. One doesn't know, reading a paper, how it will be received, because it is received only after it is published and circulated. On the other hand, one could find out, later, through interviews. Thus, a greater practice of technical interviewing can help to lend expression or representation to the greater life cycle of a piece of work.

Papers, because they are conversation-starters, are austere and modest in style. They're not blogs. They don't belabor in exhaustive detail what a scientist, colleagues, or the greater community is thinking. The paper, often quite economically, tenders a new contribution to the literature, in accordance with the formalities, conventions, and structure set by the journal.

It's difficult to say, precisely, why scientific papers are written as they are. There is, perhaps, a kind of mimetic aspect at play; somehow, scientists learn to write in a style found commonly in other papers. In many cases, the very format of journals or the writing culture of communities does not allow or inspire heavy meta-scientific detail. For instance, there is typically no section in an article template for narrat-

ing anecdotes of conversations one had with colleagues about one's research. (Perhaps, sparingly, one can make a very brief comment in the acknowledgment section.) If one abandoned certain ideas, code, or methods midway, one does not necessarily find designated space for articulating what one discarded.

However, the conventions of journals may be less influential than certain social and communal aspects of writing papers. One can reason that the austerity and modesty of the academic style also owes to the fact that papers present new work to the community. Meta-scientific commentary necessarily involves longer-term valuation of research; however, papers are largely presented "pre-valuation".

It could be sanctimonious for a scientist to embalm each of their works with thick meta-scientific exposition; they might risk coming across as over-historicizing their own work in advance. After all, if their paper is ultimately deemed unremarkable by colleagues, their preemptive efforts to situate it against the greater corpus of literature could come across as an unearned indulgence.

One consequence of such, entirely reasonable and laudable, modesty is that it decouples the valuation of a work from its written form. Papers begin their life cycle with publication; valuation unfolds with time.

One can recall examples of revered papers originally published with amusingly unassuming titles; one can detect a kind of principle of etiquette that the paper is by default but a modest contribution, unless treated by the community otherwise. It is with this assessment in mind, in fact, that I do not believe that papers can be uniformly made more meta-scientific in style. One doesn't know in advance (despite behind-the-scenes conversations with colleagues) how it will ultimately gel with respect to the field.

The community mediation through which a work is embedded within a greater scientific discourse is often done without documentation. Hints at its fate might be distributed over a large number of small, formal comments in papers. The occasional review article may be written, but often with broad brush strokes, the content being meta-scientifically "light". There might be adequate space only for summary remarks concerning certain swaths of the literature. Beyond that, it's not as though communities maintain repositories detailing what they've made of papers, or the current reputation of published works.

With time, scientists nevertheless do acquire reputations. So, one could consider, or instance, a technical interview with an esteemed scientist who is 50 years old. At this point, the scientist can reflect on the outcomes of their papers. In many cases, adequate time for valuation, of some kind, has transpired.

Although valuation is, of course, ongoing, one can nevertheless ask, for instance, the question considered previously: why did they choose to conduct a particular piece of research at a particular time? By now, the scientist may be in a position to describe both how they viewed their research at the time and how they view it now.

If one had asked 20 year prior, the answer might have been more difficult to deliver. Perhaps the scientist could not yet form a comprehensive assessment of their work because they knew that much more related work needed to be done. Perhaps they wouldn't want to assess a paper with respect to a greater strategy, since much of their strategy remained to be executed. Perhaps the greater community was still in the process of evaluating the work, or engaged in a greater effort whose character would determine the valuation.

With time having passed, the research has proceeded further along its life cycle, whose initiation roughly coincides with publication.

Granted, a scientist could speak about their labor much sooner. They could talk about why they chose to conduct a particular piece of research. Nevertheless, a fuller description of their labor, and the choices made in their careers, will often relate to some notion of value. In certain cases, the scientist's understanding of value is highly personal. Nevertheless, it is difficult to discuss scientific value in-

dependently of community, and the relationship between works, careers, and communities takes time to form.

Another way of saying it is: meta-scientifically speaking, the publication of a paper is itself a kind of community experiment. A publication may be about the result of a *scientific experiment*, but, from a meta-scientific position, one can view the release of a new paper into a community as an *experiment in valuation*; one doesn't know what its value will be.

Many decisions and strategies involved in the science represented by papers are excluded from published content, I suspect, because they are related to meta-scientific notions of

value. Whatever value it accrues is the product of its post-publication life cycle, wherein the decisions made by the scientist, such as the choice of topic or result, are evaluated by the greater community, or at least a portion. This also applies to individuals. For instance, even for esteemed scientists who are progressively given the license to chart their own paths, formation of their own independent valuations is a process nonetheless.

Thus, it seems unlikely that more meta-scientific knowledge will find itself in publications. Such knowledge seems to be more suitably encountered through *ex-post* recollection, precisely due to the life cycle of research. Interviews are engagements for facilitating such recollections.

5 Interviewing Society's Scientists: Promoting Knowledge Liquidity

Much of what SciSci does involves interviewing working scientists.

Prior to founding SciSci, I was myself involved in conducting and managing research. I am asked, on occasion, why I pivoted; typically, if one can do research, one doesn't interview other researchers as a core facet of one's work. It might even seem like a "step down", but this is only because science interviews are often rather non-technical and cursory. I am of the view that technical interviews are still underrated.

Most professional scientists I know, when asked about the press, complain. Taking interviews can be, in unfortunate cases, an unpleasant perfunctory ritual. Interviewers aren't always prepared. They don't always ask deep questions. In certain regrettable cases, they might produce no more than clickbait from their engagement with a scientific community, rather than, for instance, cover the details of the latest research.

From a societal view, the lack of a general, or even minor, capacity to interview scientists at a technical level is, in my view, something of a systemic shortcoming. I say this not only because scientists complain, but because I think that a greater technical interview capacity could play a beneficial social role.

Science is – at least in principle, and in addition to sectors such as the hu-

manities – one of society's most cherished assets. It's also a highly scarce asset – particularly in the case of knowledge held by experts and research leaders – insofar as it is, at present, relatively illiquid. It doesn't circulate widely. Very few access it. It is mostly "deposited" at universities, companies, and similar institutes.

Such is to some extent a consequence of both specialization and human capital stratification: scientists have niche knowledge, and there is a wide gap between what they know and what the general public knows. However, even within expert communities – even at the top – I've found knowledge to be far from liquid.

Experts don't necessarily know what their close colleagues know. They don't always understand each other's papers. Meta-scientific knowledge and context can help. However, many of the kinds of meta-scientific information that I have discussed above are scarce; some know them, others don't. Why? Experts overcome issues of misunderstanding through back-channel conversations: academic visits, teatime chats between conference talks, email. Thus, behind-the-scenes information is typically interpersonally shared.

However, it seemed to me that interviewing, if conducted with a technical but informal tone similar to back-channel conversations, could

INTERVIEWING SOCIETY'S SCIENTISTS: PROMOTING KNOWLEDGE LIQUIDITY

achieve, perhaps, similar communication outcomes, but in a manner shareable through a medium that can reach more of the community. Of course, certain information is too delicate to be shared beyond interpersonal channels. Nonetheless, what I have found so far with SciSci is that there is a wealth of meta-scientific

knowledge not contained in papers that scientists are eager to share with the community through interviews.

With communities producing and processing publications according to their own meta-scientific labors, improving liquidity of meta-scientific knowledge is, for SciSci, an investment in scientific community capabilities.

6 Forming Questions: What Isn't Written Down Anywhere?

The role of the interviewer – according to the SciSci point of view – is to facilitate exchanges with scientists that give expression to otherwise-undocumented information that may be valuable to scientific communities. Thus, one key purpose of interview preparation is to survey the information already on offer in the available literature.

One might try to ensure that one's interview doesn't redundantly reproduce the available literature. Instead, one might seek to craft questions that probe absences in the published literature. Thus, one prepares by reading the existing literature and looking for hints of latent scientific knowledge and reasoning that can be inferred from publications. They might seem to "orbit" a publication without being made explicit. One might say that they comprise certain kinds of "latent meta-scientific structure".

At first, one might find the task of ascertaining latent structure or absent details difficult. In fact, I should probably say more about what I even mean by "absent" or "latent". When crafting an interview question, what I have in mind is a question about a topic in the literature that seems as though it could be readily discussed, but doesn't appear to be answered anywhere in the literature.

One simple example is the question of the motivation for a paper, which

is very much related to the question considered previously regarding why scientists choose research topics. Some papers include in the abstract or introduction a motivation: the scientist writes explicitly why the research was conducted or why the paper was written. When the motivation isn't written, one might nonetheless conclude that the question of motivation should be answerable, despite being hitherto undocumented. Doing research and writing a paper is laborious; there must have been a motivation. I think of motivation, when not written, as a latent meta-scientific structure, one that can be associated with a paper but not found in the paper itself. When it isn't in the paper, the interviewer can work with the scientist to give it written representation, should they wish to share it with the community.

As a general methodological matter, I begin by first recalling the kinds of meta-scientific information that I find contextually valuable when reading papers. I think about a hypothetical "meta-scientifically maximal paper", one which explains itself in great detail. (Although not expecting such papers to be written in actuality, I employ the notion of them as a heuristic.)

What information might a meta-scientifically maximal paper contain? If the paper is written in pursuit of some overarching goal, the goal is invoked. If the outlook of the paper contends

with another, such contention is somehow articulated. If it nudges the outlook of the field along a newer pathway, it says so. If the paper constitutes a response to unsuccessful attempts made in the past, this responsive aspect is made salient. One can think of other contextual details that such a hypothetical paper might contain; and indeed, some papers include some of these details.

With such a meta-scientifically maximal paper in mind, posing questions is straightforward: a good question topic is a piece of meta-scientific context that would be contained in the meta-scientifically maximal paper but isn't in the actual paper.

Such a heuristic could, perhaps, help one craft good question topics. With a question list in hand, however, one might then need to ask how to comparatively assess the quality of one's questions relative to each other. If one must, for instance, prune down one's question list, how might one comparatively rank one's questions?

One may find that certain meta-scientific details more fully encapsulate the terms in which a scientific work is given its valuation. For instance, perhaps a paper advances a new method or technique, whose most intriguing aspect is the way it fulfills an

inadequacy of a classical or founding aspiration of a field. Thus, what may seem, at first glance, to be a new direction or an innovative novelty is really an endeavor to satisfy a long-standing mandate. In such a case, if one embellishes upon novelty in one's questions, one risks "missing the point" as to why the work is interesting to the greater community, a group of colleagues, or the individual scientist.

On the other hand, perhaps the paper is celebrated for achieving an outcome that the community had sought for some time, but does so by somehow subsuming the framework of the sought outcome in a broader framework. Perhaps – as a twist – this broader framework is controversial. It might even be the case that using this broader framework indeed delivers the sought outcome but is nonetheless somehow incomplete. Thus, if one claimed that the the community is celebrating the work for "having extended" the field along a greater expanse, one might misrepresent the delicate nature of disentangling community views on the deliverables of the new framework from views on the mettle of the framework itself.

For the interviewer, posing a question that illustrates a meta-scientifically remarkable quality of scientific work is the name of the game.

7 Literature Review: In Search of Implicit Structure

It goes without saying that interviewing requires a deep review of a scientist's work. One might thus expect this section to include a strong emphasis on full absorption of the literature and acquisition of an understanding of the interviewee's research. Such is a prerequisite for a thoroughgoing interview, but is not really the emphasis of this section.

In fact, I might even opine that the greatest mistake an interviewer can make, whilst dedicating time to extensive preparation, is to set mastery over the material as the goal. One might expect that such a goal is misplaced because it invites hubris, or because the pursuit of short-term acquisition of mastery is a fool's errand. Both causes for dismissal are valid; yet, there is another point to keep in mind.

Mastery-seeking is also misguided because the role of the interviewer is, in fact, not to cultivate expertise with respect to known information. Rather, the goal of the interviewer is to perform an entirely different scientific role: namely, to find implicit or latent structure. Identifying potentially valuable information that isn't documented is rather different from mastering the existing content.

Thus, one might not prepare for an interview as a student would for an exam. Ironically enough, one is reading the literature more like a professional researcher, in a certain sense;

searching for what isn't yet there, rather than preparing to rehearse what is.

As an analogy, think of a retro "platform" video game, like *Super Mario Bros*. These games require the character to jump across floating objects (i.e., the platforms). The papers in a corpus of literature can be thought of as being like the series of platforms. The content of the papers are the coins (or other valuables) situated on the platforms. The undocumented content is like the empty space between the platforms. The goal of preparation is not to spot all the coins on the platforms; the goal is to ascertain the gaps between the platforms themselves. In the games, the gaps are visible and obvious. In the scientific literature, they aren't.

A priori, some may presume a scientific field to be a single contiguous landscape. In practice, there are many open meta-scientific questions that pronounce themselves, implicitly, as gaps between papers, thus rendering the landscape more akin to the layout of those cherished 8-bit games. Of course, one cannot see a gap without seeing the platforms. In turn, one cannot detect a gap in the literature without knowing the literature well. Thus, preparation does indeed require close reading.

However, the goal pursued whilst reading is not to develop a knowl-

edge of the work about which one can brag. The goal is not to read closely so that one can gloat about having read many papers. Rather, the goal is to begin to perceive the very textual limits of the papers themselves; to begin to appreciate what existing (meta-)scientific knowledge has and hasn't been written down.

One is after the empty space of scientific literature. One knows that empty space resides just beyond the boundary of a textual object, and that determining the boundary of a textual object, which is often rugged and fine,

requires a close reading. So, one reads closely in order to determine the very limitations of what one can learn from the available literature.

The first time one conceives of a piece of background information that one really wishes one knew, but cannot ascertain, one has the makings of an interview question. One is, in a sense, in search of the questions that, if answered, might deliver new and valuable information to the interested community. One seeks hints of opportunities for deeper scientific sharing.

8 Interview Preparation: Forming Models Beyond the Literature

With all this being said, we can now discuss, more concretely, the kinds of preparations SciSci prefers to make in advance of an interview.

Before interviewing a scientist, I prefer to read as many of their publications as I can. I read their journal articles. I read the chapters they contribute to edited volumes. If they are the editor of the volume, I read their introduction or preface. I read their PhD thesis, even if it was written decades prior. If they've received awards, I read their acceptance speech transcripts. If they have given high-profile talks or famous lectures, I refer to the proceedings.

I read the acknowledgment sections in their publications; they indicate who the scientist's colleagues are, or have been. I like to know if they cite other works widely. I like to see the journals they choose for various topics. I like to understand the evolution of their research. If they have changed topics in recent years, I ask why. Was previous research a decisive success, thus warranting a new chapter? Did funding opportunities dry up? Is the scientist applying their previous research to current research, or has their investigation forked?

I tend not to watch educational videos (e.g. on YouTube), leaf through popular science communication pieces, browse online fora (e.g. Stack Exchange), or read introductory

textbooks. This might be surprising, as such material are often presented as "on-ramps" to scientific fields. However, insofar as publications are representations of research, these materials are "representations of [...] representations of research". If I intend to interview a scientist, but instead read a textbook written by another scientist mentioning the paper of the interviewee, or watch a video made by someone who read that textbook, or read a journalist article by a writer who watched the video made by the creator who read the textbook, I position myself far afield from the original text, and consume content that has already passed through numerous interpretative filters.

It seems a little bit irresponsible to rely on the interpretative filters of others. The SciSci approach emphasizes working directly with scientists so that they can express themselves in technical terms. Thus, building questions from second-hand (or *n*th-hand) material distances one from the interviewee's works, and also may introduce some politics (which may have indeed have shaped the lens of non-primary-source material). My own principle is: if I can't yet make sense of a scientist's own works (rather than second- or *n*th-hand material), then I'm not yet ready for the interview.

From there, I like to piece together a kind of portrait of the scientist. When

doing so, I ask many questions. Does the scientist work at the theoretical frontier of the field? Does the scientist invent new methods? Is the scientist best known for new discoveries? Does the scientist help colleagues when they reach an impasse? Does the scientist help to bridge the research of colleagues? Is the scientist a trend follower? Has the scientist taken risks? Has the scientist received many rewards? Did the scientist take advantage of early success, or was receipt of recognition a gradual process?

If the scientist has been interviewed before, I read the articles written from such interviews. What kind of questions are typically asked? (One might seek not to repeat them.)

With such questions in mind, I endeavor to model the career arc of a scientist. By career arc, I mean a working model of the factors that led from one paper to the next. Under the publish-or-perish paradigm, one is often presented a picture of perpetual novelty in science; experts are continually writing up new content. Quite often, however – at least in my observation – a scientist thinks in a certain way or is driven by certain sought outcomes or outstanding questions, and has been for some time. Thus, there is a yarn – sometimes threadbare, sometimes braided; sometimes split, sometimes knotted – of professional continuity stringing together their publications: an arc. The scientist could belong to a program with a strong philosophy. The scientist could have a process in place for choosing new topics.

Of course, one's arc model might be disastrously inaccurate. Such disaster can be fortunate, nevertheless. If one holds in mind a model, without revealing it, only to find that the scientist thought about research in an unexpected way, one might even view the interview as a success, for it uncovered something non-obvious. One's models are to be broken.

If the model is more or less accurate, then, if the interviewer and interviewee appear to be synchronized, they can discuss matters in greater detail. Eventually, however, the model should be proven wrong. So long as one's models are well built, proving them wrong can be an indicator that the interview has given expression to content that might not have been easily inferable from the written record.

So, being right and being wrong carry their own respective advantages. If the interviewee agrees with one's "close reading" and analysis of a body of literature, then it becomes easier to explore new pastures beyond the literature together. If the scientist continues to surprise the interviewer, then one can take satisfaction in the hitherto-unknown details that come to light.

On the other hand, if one has the wrong model in mind and attempts to impress it upon the interviewee – perhaps in hopes that one can build rapport that way – the situation may deflate. There are few scenes farther from professional grace than those starring an overbearing, misinformed

interviewer. On the other hand, if one's model is more or less correct, but always left implicit in one's questions, one can still indeed build rapport. If one believes that one has some kind of insight into the prospect of ascer-

taining a certain latent meta-scientific structure, it is best to somehow implant one's view softly and approachably in an engageable question, rather than hammer out a thetical position. Thus, I prefer to keep my models to myself.

9 Posing Answerable Questions: Lead With Context, Promote Analysis

I prefer to type up interview questions, and share them with participants, beforehand. The questions should be relatively short, without unduly long preambles. Nonetheless, it is helpful to provide a technical framing, so that the scientist can appreciate one's level of understanding and one's interest in a topic. The scientist cannot really accommodate an interview question under ambiguity.

Consider the question:

Will **Theory X** succeed?

This question provides the scientist with little context. Rather, it might be better to ask something along the lines of the following:

Theory X has garnered confidence in recent years as a candidate explanation for **Phenomenon A**, one that **Theory Y**, although standard for decades, was unable to provide. However, it is known that the presumptions of **Theory X** are rather inconsistent with **Phenomenon B**, whose more recent discovery has been replicated numerous times over the past 5 years. However, it appears from the early literature as though **Phenomenon B**-type behavior was somewhat foreseen during the

development of **Theory Y**. What is your view on the relative explanatory advantages of **Theory X** and **Theory Y**?

Perhaps we should examine the "anatomy" of this question. First, it communicates that one is aware of theories *X* and *Y*, and presents them as the foci. Second, it furnishes some basic meta-scientific context: *Theory X* explains Phenomenon *A*, unlike *Theory Y*. Next, it brings to bear further meta-scientific context indicated from the greater literature, emphasizing certain kinds of meta-scientific information. One kind is experimental: *Theory X* is inconsistent with Phenomenon *B*, a finding that has been replicated. Another is historical and theoretic: *Theory Y* was developed with Phenomenon-*B*-type behavior in mind. Finally – and I'll say more about this in a subsequent section – the question is itself meta-scientific, rather than political. That is to say: it asks for views on the relative explanatory advantages of the theories, not, for instance, if the theorists favoring *Theory Y* are right and the scientists developing *Theory X* are wrong.

This question might appear somewhat long, but it's self-contained. In practice, it might take 30 seconds or so to ask. If one has an hour for an interview, asking 5 or so questions such as this leaves almost the full hour avail-

POSING ANSWERABLE QUESTIONS:
LEAD WITH CONTEXT, PROMOTE ANALYSIS

able to the interviewee; one eats little of the available time. On the other hand, ill-posed questions may require clarification or foment misunderstanding, which can consume more time.

If one had a chalkboard available, one could visualize the relationships

between the theories and phenomena at play. I recommend interviewing at a chalkboard or whiteboard, or at least sharing some notebook paper with the interviewee. Doing so provides a more familiar atmosphere for discussing scientific detail.

10 Recording New Knowledge: Discovery Through Interview

It is often brought to my attention that scientists are, at times, interviewed by journalists or communications professionals who exercise little preparation. One lamentable consequence is that the questions are quite often predictable. The scientists will be asked questions that someone unfamiliar with their field will typically ask. So, if a scientist works on a new method for a particular problem in a sub-field of a field, they will be asked a question – one that is almost definitional – about “what the field is”. SciSci pursues a different prospect.

It’s well to recall that professional researchers are under considerable pressure to publish papers. They publish through certain – preferably high-impact – journals, with a relatively strict template, format, tone, and length. As a consequence, it has been my observation that scientists typically know much more than they write, even in the case of scientists who publish consistently.

Thus, non-technical interviews risk forfeiting the opportunity to hear what scientists know, beyond the published record. Moreover, elementary interview questions pertain to details that are very much already in the literature; often in the first paragraph of a literature review or introduction section; or even on Wikipedia.

A detailed, prepared interview, on the other hand, gives the scientist an op-

portunity to share what they know but haven’t written down. It offers a different format – a conversational format. A new format can help to represent content that wasn’t documented via other formats.

In my experience, an interview prepared with such an objective can give the scientist an opportunity to speak less formally, and more expressively, on their research. They can offer assessments. They can speak to future prospects. They can discuss failures. They can situate research within a greater historical context. An interview can give the scientist an opportunity for professional expression.

One might think that elementary questions offer fewer constraints, allowing for greater expression. However, when elementary questions are asked, the scientist must drop much of what they usually think about and instead focus on saying something simple. When asking such a question, one is asking the scientist to pivot from research mode to something akin to undergraduate lecture mode.

On the other hand, if a technical tone is set, the scientist can talk about anything they think about. I feel with the utmost conviction that it is important to help scientists feel comfortable speaking casually and informally in technical detail; such are the requisite conditions for expression.

I have been surprised by the kinds of

insights scientists share when an atmosphere permitting such expression is set. Scientists have recounted background details I had never heard of. They discuss deep problems that few notice. They introduce perspectives cultivated over a career. They may even discuss challenges they currently face, or past failures.

Sometimes, the scientist has kept these details in mind but hasn't said them on record before. On certain occasions, scientists have surprised themselves, and have even referred to some of the details unearthed as "discoveries". In fact, during certain interviews, genuine, new scientific ideas

have emerged.

Generation of new ideas might not be the goal of an interview. Nonetheless, such is possible, as SciSci experience to date suggests. Nonetheless, the full epistemological heights reachable through technical interviews, as one saunters beyond the confines of the written record to the greater recollections and knowledge of professional scientists, has yet to be witnessed in full. It will be a source of great interest to see, should other organizations or communities take an interest in this prospect, the heights that can be reached.

11 Direction and Wandering: The Art of Interview Improvisation

Although I prefer to draft a question list for the interviewee in advance, I nonetheless take the opportunity to proceed off-script when the appropriate occasions arise.

In most cases, the interviewee presents the interviewer with such an occasion. After all, the real goal of the interview is to allow the scientist to express and represent the sophisticated details of their work in a new fashion. That the interviewee does so by responding to planned questions is, under the most auspicious circumstances, a mere initial formality for priming a discussion, one that comes to acquire, after a few such questions, its own directionality.

Directionality can be acquired, first, by giving the interviewee ample time to respond. The interviewee might answer the question, as stated, only to proceed to elaborate on related details. Such elaboration is no longer a direct response to the question. An unimaginative interviewer might fear that the conversation has gone off-track, but that's precisely where one must go in order to explore the fuller range of a scientist's knowledge.

An interviewer might feel uncomfortable in such a situation if they don't know how to respond to the extemporaneous remarks of the interviewee. Here, an appreciation for preparation can be found.

Previously, I had invoked the analogy

of a platform game. Now – at the risk of employing too many analogies – I'd like to suggest that the interview be thought of with respect to a map. The content published in papers can be thought of as being like the features of a town. If a given paper is deeply enmeshed in a complex of others, it belongs to a bustling city. If it interacts with fewer papers, it belongs to something like a hamlet. As the interviewee speaks more extemporaneously, one might indeed be wandering into hinterlands beyond the urban blocks or village roads; a fortunate circumstance. Here, preparation gives one a sense of "cartographic awareness" as one wanders.

For instance – to extend the analogy further – if one jaunts, off-road, until happening upon a freshwater spring, preparation should help one ascertain the towns its water serves. Indeed, such is analogous to critical meta-scientific information. The analogue of the freshwater spring might be a private workshop, an influential mentor, or a seemingly unrelated area of research with which some interpersonal connection was formed. Sometimes, there is an under-recognized source of vitality for research. The graduate students living in the big cities on the map might not even know where their bottled water comes from.

One regains one's contextual awareness, as conversations proceed in un-

expected directions, by asking improvised questions. So long as one has made adequate preparations, one should be able to ask follow-up questions. Indeed, these questions can be simpler. In some sense, when given extemporaneous remarks, one need only re-contextualize them and relate them back to the literature (i.e., pinpoint them on the map).

For instance, if the interview begins speaking at length about a meeting between colleagues that they realize is significant, one can begin to ask questions that allow the scientist to speak on its downstream consequences. If a recollection is made of a past discussion on a topic with a colleague, and one recalls that the interviewee had not yet published on that topic yet, one can ask if the discussion led the interviewee to begin research on the topic. If one recalls that they later wrote a joint paper on a related matter, one can ask about the link between the two developments.

Thus, the interviewing process is really just about 1) extensive preparation and crafting a list of question prompts; 2) allowing the scientist to

enjoy the opportunity to freely express themselves in technical terms; and 3) asking improvised questions that refer their remarks back to the literature.

Sometimes, follow-up questions are quite natural. As soon as one asks the question, the interviewee says, “right – I was just about to get to that”. In other situations, when reaching a fork, one sees a pathway that is different from that seen by the interviewee. One can surprise the interviewee with an unanticipated direction. If rapport has been established, the interviewee may entertain a jaunt in an unexpected direction.

In fact, both scenarios offer room for surprise. Scientists can be accustomed to communicating, doing, and thinking about their research in a familiar way. Frequently interviewed scientists often have a kind of script. On the other hand, by allowing the scientist to go off-script, or by offering questions that invite the scientist to think about their research in a manner that they have not done before, one can, in the best case, foster an atmosphere of novel intellectual expression.

12 From Interview to Text: Transcribing Conversation

Eventually, the content of the interview will enter the published record. Rendering an interview as text entails a different professional relationship with the scientist. Even after the most free-flowing or unabashed interview, the scientist, once seeing their comments written down, will want to see it written in a manner that plays well with the rest of the literature. Thus, whereas interviewing is about fostering a culture of liberty, transcription is about giving freely given remarks the right tone and structure.

Scientists speak differently than they write. Extemporaneous remarks from a scientist are typically a bit more offhanded and a bit less polished than their written equivalent. Conversations are carried out in real time; thus, even if an interviewee wishes to restart a sentence, they have to abort it (perhaps mid-thought) and start anew. Words are omitted; grammar, eschewed. In my experience, such blemishes are, in fact, favorable indicators of an interesting conversation. If the interview is of novel value, the interviewee will likely spend much of their time thinking, rather than delivering rehearsed remarks.

Nonetheless, in an effort to repay the interviewee for their cognitions, I try to render the best transcriptional presentation of their comments that I can. I often use ellipses and bracketed in-

sertions to suture together remarks into prototype-sentences. These editorial remarks may vanish later, if the interviewee finds them unsightly. Or, they might be kept for purposes of editorial transparency.

When I first started doing interviews for SciSci, my initial goal was to represent the comments of the interviewee with the greatest fidelity or verisimilitude possible. Thus, if the interviewee often flaunted grammar for effect (rather than by accident), I'd keep it. If the interviewee spoke in fragments as they thought through a multifaceted issue, I wouldn't smooth out the splintered form of their remarks. What I found, however, is that – despite the literary value one might perceive in such choices – interviewees will, generally, want to come across as composed, professional, and eloquent.

With time, I have come to understand that such polishing is more than a matter of presentability. It is about giving the results of informal discussion a palatable form in order for them to come together as a metabolizable textual object. Thus, the aesthetic labor of transcription is really about carrying the value produced under an atmosphere of informality and vesting it, through professional form, with the authority of voice needed for it to belong to the published record.

13 The Final Product: Information, Not Narrative

The methodological commentary included here has mostly been positive: I describe the techniques SciSci Research uses. Of course, such techniques have also been shaped by critiques I hold of other practices, and negative positions on practices that I wish SciSci Research not to reproduce. Thus, I feel as though a methodological discussion is not complete without some allowance for sharing such negative positions. I'd like to describe the kind of practices and outcomes that I wish for SciSci Research to avoid.

One tendency among certain science media outlets that I find particularly troublesome is that of shoehorning the results of interviews with scientists into a preconceived or contrived story. It is particularly nettlesome when the narratives are reductive, averring perpetually to a new breakthrough or a crisis, manufacturing clickable events rather than distributing information.

Scientists I know, across fields, are dismayed by crisis narratives; after all, the status of a field can change at any time, and failed attempts are, in some sense, just as valuable as successful ones in understanding how nature works. The breakthrough narrative is also, in my view, not particularly valuable. Scrolling through social media, one feels continually barraged with attestations to some hot breakthrough in science, whereas, in

actuality, the articles merely concern new papers, which are published all the time. Whether or not a paper constitutes a breakthrough is for the community to decide. The process can take time, and subsequent research. One might recall that major prizes are often awarded for research conducted a few years prior. Thus, it seems meta-scientifically disingenuous to present new papers as breakthroughs, especially when such is done so often.

A related narrative trope is one that endeavors to (cost-)justify a scientific result by appealing to some marvelous future prospect. In biomedicine, it might be longevity or immortality. In mathematics, as narratives of proof formalization and AI become increasingly interwoven, the narrative is increasingly swept up in the greater exuberance surrounding the rather numinous concept of artificial general intelligence (AGI).

What troubles me about these grand narratives is they allude to, in some sense, climaxes of research. Immortality would be, to some degree, the end or culmination of biomedicine. If AGIs were the ones doing mathematics, such could, maybe, spell the end of human mathematics as we've come to understand it. It's not my place to opine on whether or not such outcomes are possible, nor to opine on timelines. However, it is a bit discon-

certing when narratives try to stuff a particular paper from a field into a future narrative that effectively amounts to what would be the end of that field.

Here, perhaps the reader will entertain a few comments on modernity and science. Indeed, the 20th century was one of many weighty events. On the political stage, many great wars were waged, and many new nations were formed. The events were weighty, in part because the modern international political system was being set. Likewise, 20th century science featured many weighty events, which helped to shape and form our modern scientific institutions.

Now, with our modern institutional system having been built, the atmosphere is different. There is indeed consequential and inventive work done all the time, but it doesn't always fulfill the role it once did. In the 20th century, such results helped make institutions. Of course, this still happens; entirely new fields are created, with new institutions to carry them. However, if a field is well established, a new result doesn't make the field; it is absorbed into the field, debated, applied, rejected, promoted.

This might seem, to some, a bit more boring, but it's how modern rational institutions function.

On the other hand, one can see in "climax narratives" an appetite for major events, namely those which

bring a field to its conclusion. What such climactic events and early 20th century events have in common is that they bookend an institutional mandate; one event initiates it, and another event concludes it. In between the two, argumentation, deliberation, and iteration on a work proceed. Such processes may seem less fascinating, but they're the stuff of institutional science. The workings of a thing can be less thrilling to some than it's beginning or end, because they pertain to what it does rather than the epochal character of its very existence. Interviewing, on the other hand, is all about what science does.

SciSci conducts its coverage with the thesis that the institutional milieu, which may be historically bookended by formative and climactic events, is where the real modern substance of science is found. Here, simple stories are less relevant than information. Stories might be amusing for outsiders, but institutional machinery runs rationally on information. Such is increasingly the case as the institutional modernization of science stabilizes. We have more scientists than ever before, more papers than ever before, and more complex scientific communities. It seems rather inadequate for interviewers to do no more than smith popular tales from all this. I might even venture to say that science, so often counterintuitive and surprising, is the world's great story-destroying force.

14 A Principle of Etiquette: Focus on Science, Not Drama

One attempt that is not infrequently made by other outlets to cover the institutional character of science, which SciSci also seeks to avoid, is that of partaking in gossip or political dramatization concerning research. One might wonder how one can possibly engage with meta-science, and community dynamics, without delving into gossip or drama. My own view is that meta-science is quite distinct.

Increasingly, we see the growth of popular discourses around fields of research. If research leaders disagree, or a paper is retracted, or theoretical "camps" appear to become pronounced, commentators may weigh in. Popular commentaries on various "crises" in science proceed thusly. On the other hand, we see currents of popular optimism. Funds may be investing into research areas. Startups may be pursuing them. Others may be hyping them on social media.

As online commentators, journalists, companies, investors, influencers, and others become involved in crafting a discourse, the spectacle they fashion takes on a life of its own, and is quite acutely decoupled from the textual corpus of a field and the community labors of valuation of the corpus. Of course, popular conversations on science will be detail-light; what is pernicious nonetheless is the decoupling, and the mistake of conflating spectacle with scientific discourse.

That is to say, gossip and dramatic discourse is, relative to the scientific corpus, an exogenous discourse. It's not the discourse at play in taking a paper as a community and doing something with it. Scientific discourse will refer back to the medium of the published record, whereas online discourse is channeled through its own medium of memes. Often, memes cast a scientific drama in terms of threats to a field, warring parties, heroes, villains, underdogs, tyrants, and so on. However, as you can imagine, when one attends to the details of the field, one finds that the fuss largely concerns a difficult question, a communication shortcoming, or some kind of organizational challenge, none of which are uncommon in science. Memes struggle to capture the dynamics of modern rational institutions.

SciSci's interest in the meta-science of communities, and aversion to drama, shapes the questions that are posed during interviews. I don't ask scientists to respond to online comments. I don't mention interviews others have given with journalists. I don't discuss podcast remarks. My questions pertain to the literature. I'll ask about a particular paper; a particular section in the paper; a particular scientific detail. After all, the SciSci objective is to ascertain latent scientific information not found in the textual corpus and to help surface it. Thus, I ask questions that circle back to the text.

As an anecdotal matter, scientists seem quite relieved to be asked about the actual textual corpus of the field, and not the orthogonal discourses that increasingly surround it. In many cases, I find scientists are now a bit nervous when interviews begin. As soon as one begins to refer to publications and ask questions about their contents (so long as one does so without misunderstanding them), the mood of the conversation lightens.

Moreover, if one is interested in interviewing scientists for the purpose of producing novel scientific information, it's simply a professional disadvantage to appeal to online discourse or drama. Here, we confront a core aspect of scientific culture itself. In science, one is wrong often. Mistakes are made often. One writes a paper, only to be disappointed with it later. One publishes an attempt at an advancement, only for a more successful one to be made later. When discussing a body of work from a meta-scientific lens, one will see developments of this kind in abundance.

A meta-scientific lens is necessarily comparative and attentive to the relationship between strategy, scientific work, and progress across an ensemble of works and a network of research pathways. It goes without saying that adoption of a political tone, which will do no more than elicit a defensive posture on the part of the scientist, is inimical to meta-scientific exploration. Here, we can perhaps appreciate what is arguably the most pernicious consequence of the tendency

of media to sell scientific drama: it inspires a defensive atmosphere that is opposite to that needed for meta-scientific discussion.

For instance, if one asks why a scientist was inspired to write a given paper, it could be the case that the scientist was motivated by certain inadequacies witnessed in other works. The new paper might have performed better, or worse. If one fosters a dispassionate atmosphere dedicated to an understanding of the field, one might be able to discuss such matters; not in terms of competition, winners, or losers, but in terms of research questions and the relative advantages of different theories or methods.

In the real practice of science, with the exception of extreme cases, the notion of winning and losing is rather inappropriate. In the most virtuous case, everyone is working to reduce ignorance, and is happy to be wrong, as negative feedback is essential to theory building: one wants to know what nature can and cannot do.

Moreover, scientists with healthy careers are more than happy to talk all about their mistakes. They're even happy, in my experience, to speak about their dissatisfaction with recent or present work. Such dissatisfaction is really the motor of scientific investigation. Nothing is ever good enough. Theories never explain enough. Research programs are duct-taped together. Colleagues don't understand each other. Frameworks are often of ghastly inelegance. Knowledge gaps

remain taunting. Open questions remain haunting. A healthy scientific culture makes a kind of “Halloween” out of such haunting; it welcomes it and plunges into it indefatigably, celebrating it all along.

On the other hand, the political drama projected by media onto scientific affairs makes insecure any kind of engagement with such culture. What journalists often seem to misunderstand is that, independent of their narratives, the textual culture of science continues; it’s not decided, in the virtuous case, by the parallel discourse that commentators create. Thus, by asking questions that refer to exogenous discourses, one merely risks sealing off whatever greater window one might have been able to make with the scientist into the textual corpus and discourse constituting their scientific community.

Put differently, a successful interview provides an opportunity to lift one’s perspective above mundane challenges (which include, increasingly, issues of social media gossip) and take a broader view on scientific work. Once the discussion is lifted to such

a position, so long as the setting is dispassionate and analytic, one can, in my experience, discuss many scientific matters that have never been written down or (sometimes) even discussed before. When doing so, by talking about science, one lends greater expression to the very freedom of science. This freedom is exercised in scientific labor as an understated principle. By speaking about scientific labor and scientific communities, by probing the meta-scientific, this freedom finds new expression.

However, such is only possible if one is unanchored from the popular discourses and ephemeral content that are rather often packaged and carried by many science media. Towards this end, the SciSci style goes in a different direction; an interview, even one lasting only an hour, can create an environment in which one is at liberty to meditate on developments across deeper scientific time. Untethered from the pressures of the present, immersed in meta-scientific deep time, perhaps elements of the fuller character of science can come to the surface.

15 An Invitation to Share What You Learned

Recently, an entrepreneurial colleague asked SciSci about the prospect of preparing a kind of "bullet-point executive summary" of *Interviewing Scientists*. Such might, in principle, be feasible, but only if done with great care.

Although SciSci has abstained from referring to specific interactions in this report, we nonetheless base our commentary on our engagements with scientific communities. Such engagements are delicate, and we have striven, when recounting our methods of engagement and describing the (abstract) dynamics of science communities, to handle such matters with the delicacy they deserve. This report has been written with some length precisely as an endeavor to lend our commentary, we hope, the context and padding necessary in order to make it tenable and comfortable. (Length, although sometimes an indulgence, can serve advantageous purposes, such as allowing hard topics to breathe.) Uprooting such commentary and presenting it on a slab just seems a bit too crude.

We would also risk misleading the reader; there's no generic, two-sentence recommendation that a reader can absorb in order to be prepared for interviewing scientists. Every case is different. We also try not to produce short-form methodological content, as the crux of our methods involves reading carefully and allotting

time for analysis. Thus, those without time to read about our methods filter themselves out.

Moreover, in terms of reader relations, SciSci wishes to avoid telling readers what they should regard the take-away messages to be. We try to share our thoughts as thoroughly as possible, so that readers can avail themselves of whichever details are most pertinent to their needs.

On the other hand, we would be delighted to list the points that readers regard to be the most valuable. This would allow us to create a summary resource on the report without the risks highlighted above. Rather than fast-tracking the report, it would summarize what readers have derived from it, which may encourage others.

So long as it isn't of undue inconvenience to the reader, we invite all who are interested to anonymously share their thoughts on the report **here**. We will post a (likely somewhat edited and curated) compilation of responses as a "community summary".

SciSci recognizes the value of producing summaries, but would prefer to do so by sharing what our readers find valuable (if they find anything valuable at all) in our reports. Executive summaries are typically written to squeeze out the value of a report in concentrated form. Our position, however, is that SciSci writes these methods reports as resources; their value is for readers to assess.

SciSci - Science for Scientists

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Interviewing Scientists

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