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What makes for interesting developmental research? Perspectives from the sociocultural and information processing frameworks

Takeshi Okada & Kevin Crowley

Read any introductory methods textbook for cognitive psychology and developmental psychology: These are the "official" lessons that students in our field are expected to know as they begin their scientific apprenticeship as advanced undergraduates or beginning graduate students. And we think these are fine lessons to learn. We do not disagree with the sensible suggestions that experimental designs require control of variables or that looking for evidence to falsify an hypothesis is an effective means to make progress on research questions. However, we do argue that methodological skills alone are not enough for a developing scientific apprentice to become an expert. In particular, we argue that there is an art to good research: The art of finding and answering interesting research questions.

As we have spoken informally with our colleagues who train graduate students, we have uncovered a number of theories about what constitutes an interesting research question. Where does the inspiration for a new project come from? How do expert scientists recognize good research questions when they see them? How do they recognize the right answers when they see them? These theories are, for the most part, informal and shared primarily among faculty—often over beers. Almost all textbooks spend very short amounts of space for describing how to find good research questions and how to make a clear hypothesis. Instead, they spend a fair amount of space for detailed description for formal methodology such as how to design research to test a hypothesis, how to counterbalance, how to code data, and how to conduct statistical analysis. In the developmental psychology textbooks, in addition to this information, they describe in detail the methods unique to developmental psychology such as cross-sectional method, longitudinal method, and micro-genetic method.

However, they are almost silent about how to find good research questions and how to conduct interesting research. This may in part reflect the tradition in scientific psychology community that emphasized the context of justification to test existing hypotheses or theories instead of the context of discovery to focus on how to find good research questions or how to come up with hypotheses (See Okada & Shimokido, 2000; 2001). This may in part reflect the truth that there is no one best way to come up with interesting research questions. Or, this may reflect the possibility that skills—even scientific skills--that touch on vague notions such as creativity or inspiration are difficult to teach directly through textbooks and testing. They are perhaps best learned through apprenticeship or through hearing the thoughts of successful researchers on the matter.

In this paper, we let the psychologists speak on this topic. Top researchers, whose work has been influential in cognitive and developmental psychology, were asked about their own definitions of and methods for good research. We present interview data with four psychologists from each of two major frameworks in the learning sciences: information processing and socio-cultural theory. We chose these two approaches because we believed that they made great contribution to the psychology of learning and its implication to education in the past several decades. Four psychologists from information processing approach are David Klahr, Brian MacWinney, Robert Siegler, and Herbert A. Simon and four psychologists from socio-cultural approach are Ejro Engstrom, Babara Rogoff, Jaan Valsiner, and James Wertsch. We chose them because we think that they are prominent researchers in each approach. It is difficult to decide whom we should interview in such a situation. We chose them by asking several psychologists in each domain whom we should choose. Accessibility for us was also an important aspect for the choice, especially for the psychologists in the information processing approach. Both of the authors are trained in the tradition of information processing approach in a school in Pittsburgh. Due to this practical reason, all of the interviewees in the informational approach are from the schools in Pittsburgh. At a first look, it may sound that our sampling method is not valid to reflect the fields. However, when we took a look at several popular textbooks of developmental psychology (e.g., The development of Children: second edition by Cole and Cole), we found all of the names (except Engestrom who was relatively new to American Psychology community when the books were published) are cited in those volumes. Therefore, we believe that the sample of these interviews somewhat represent the prominent researchers in the domain.

The original interviews were conducted by the first author for the Newsletter of the Japanese Society for Developmental Psychology in 1994 and 1997. The main parts of the interview data were translated into Japanese and published in 1995 for information processing approach and in 1997 for socio-cultural approach. Also, follow-up interviews were conducted to three psychologists in the information approach (Simon in 1998 and Klahr in 1999). A part of those follow-up interviews and the original interviews has been included in a book chapter on interesting research published in Japanese (Okada & Crowley, 2000).

The contents of the interview questions were as follows: (1) What are the features of interesting and/or good research like?; (2) What are the important things to do in order to conduct an interesting and/or good research?; (3) Picking up one of your own research and explain concretely how you came up and developed the research ideas? The following question was also added to some of the interviewees: (4) What do you think the relationship between theory and data?

In the interview, the interviewer implied to the psychologists that the main readers of this Newsletter would be Japanese graduate students and young psychologists in developmental psychology.

When we use interview method to investigate the research strategies that researchers adopt, it is possible that the researchers answered based of the ideological concepts about methodology rather than the way that they actually use. However, we believe that such interview data, although they might somewhat suffer from such problems, would offer useful insights for graduate students and young psychologists who just started their carrier as psychology researchers. We believe that the strength of this paper is in the words themselves of the prominent psychologists. For this reason, we used quotes from the interviews as much as and as effectively as we can so that readers could appreciate the nuance of their words.

The two approaches, information processing approach and socio-cultural approach (and situated cognition), have caused active argumentation in developmental psychology of learning and its educational implication in the past decade (e.g., Anderson, Reder, & Simon, 1996; 1997; ###). The two approaches seemed to have opposite views of human activity, valid ways to conduct research, values on what qualifies as good research and so on. However, recently researchers started to claim the importance of integration of perspectives from both approaches to understand and improve human activites (e.g., Anderson, Greeno, Reder, and Simon, 2000; Rogoff, 1998###; Sfard, 1998). We also found in these interviews that there are many surprisingly common features in their opinions about conducting good and interesting research. In this paper, we focus mainly on the common features of their methodologies in the two approaches arranging the interview quotes in the following three main questions: (1) How do you choose research questions for interesting research? (2) Where does the idea for interesting research come from? (3) What kind of strategy do you use to find answers for your questions?

How do you choose research questions for interesting research?

Find important questions. Think about impact of the research.

Good scientific research raises new research questions, encourages new areas scientific work, and often has implications for important problems in application. For example, Newell, Simon, and Shaw created the first Artificial Intelligence programs simulating human thinking processes in domains such as chess and logical reasoning. Their work opened new fields, cognitive science and artificial intelligence, and had great impact on various aspects of modern society. In this research, they found important questions about human cognition and foresaw the impact of the research when such questions were answered.

Thus, one of the keys to find a good research question is to focus on the impact of the research. The prominent researchers in our interviews were also concerned with the academic and social contributions of the research finding (i.e., How seriously people would want to listen to the findings, how much impact it would have, how much discussion it would evoke, and so on.).

Simon

First of all you have to ask, can I think of some questions which if I were skillful enough and lucky enough to get an answer, people would be interested in, people would care to have the answer to the question? So the first characteristic of good research is, you're asking something that people would really like to know the answers about.

Rogoff

I would emphasize what makes an impact, not what gets published. And I think that many of my colleagues, maybe around the world, but certainly in the United States, think of getting published as being the end goal, but the end goal, there's lots of stuff that never gets published, what a waste. So the aim is to do things that are - I think publication is an important step towards the goal of having an impact, without publishing it's much less likely to have an effect, but stuff on paper that's just ink blots, that nobody does anything with, it might as well have never happened.

Siegler

One sign of good research is that people--on reading it--want to pose a lot of new questions, to do a lot of new research, that it leads to disagreement about what the findings mean, though in all liklihood agreement that yes this is likely to be a real phenomemon.

Klahr

Well, um, the question has to be interesting to somebody. There's a large, a large diversity of opinion about what's interesting. Some people think that the interesting questions are ones that have implications in the real world, other people like questions that answers some fundamental puzzle about, well, we're talking about psychology, so in this case is would be some fundamental puzzle about human behavior. Other people think that the interesting questions are the ones that are out there, defined already by the field. Other people like to attack existing ideas, they find some line of work that's fairly visible, fairly popular, they look at some flaw in it or something in there that doesn't make sense to them, and they try to change people's ideas about it.

Search for intellectual excitement.

In his best selling books, "Winning the games scientists play" and "Survival strategies for new scientists," Sindermann (1982; 1987) claimed that scientists have to play a kind of political game that involves high level of social skills in order to become productive. He described various strategies such as how to behave in an ivory tower, how to work with an advisor, and how to survive as a female scientist. These books provide very useful tips for survival in an academia. However, they might also give us an impression that a belief that science starts from intellectual excitement is too naive. Readers may think that scientists should focus on political aspects of science without paying much attention to their own intellectual curiosity.

However, many famous scientists showed strong intellectual curiosity from their childhood (Brennan, 1996). It has also been pointed out that it takes ten years to become an expert in creative domains such as science and art (Hayes, &&&). People have to spend more than 10,000 hours on task to acquire organized knowledge and to become able to achieve high level of performance. Without intellectual excitement, it is very difficult to continue to work on a task for such a long time. Thus, intellectual excitement may well lie close to the heart of most good research. Researchers in this interview also pointed out the importance of choosing questions that they can enjoy investigating with intellectual excitement.

Simon

I think in psychology, we've had a considerable development of formal methodology, especially statistical methodology and experimental design. All of that is very good, but we must not lose sight of the fact that here is human behavior, what we see everyday working and living with other people - human behavior, that's what we want to understand, the whole goal of this effort is to understand it. And the first characteristic you need to do is to be curious and observant.

Rogoff

I guess what I think bad research is research that is done without any intellectual excitement, and without honest efforts to learn. So if it's just done as a business, and I think there are colleagues of ours who do the things just as a business, and get publications and they go, but it doesn't have any thought behind it or curiosity or excitement. I think those are the criteria for good research, something that's exciting and that you and others can learn things about.

Klahr

Some people when they do research they think early on in the process about where they're going to publish it, what journal it will go into. Other people don't worry about that till they're done. They say, I'm pursuing a problem and when I'm done I'll find a place to put it. I think that's ideally that's a better way to do your work. That way you don't conform exactly to the norms of the field. You may have pushed the boundaries of the field, some people are very strategic oriented in the way they do their work. They think, well I'll do this and I'll put this is journal A and then I'll put another little piece of this in journal B. I'm not always sure that's the best way to do things but it is a way to do things.

<u>Find feasible questions.</u>

Suppose you are travelling in a foreign country and taking pictures while sight seeing. When you get your film processed, you may find pictures that you could not remember what you were trying to capture then. When you take a picture, you have to choose a target and focus on it. If you did not focus appropriately, the picture would be blurred. If you try to capture too many things at once, the picture would not be very appealing. Taking pictures requires you to think ahead about what and how you really want to shoot.

Research activity is somewhat similar. When new students start to participate in research projects, they often talk about very vague and big ideas such as that they want to figure out human cognition as a whole. When they realize that this may be too ambitious for a student project, they may swing to the other extreme and think that they can only conduct experiments by fiddling with variables in established designs they read about in published articles or that their advisor has been doing for years. And then there's the wide space between: Finding research questions that are interesting, useful, and doable.

Klahr

Interesting research is research that addresses an important question and that is feasible to do. There's a lot of interesting questions that can't be answered very well. They're too big, they're too vague, they're too grandiose. There's a lot of research that can be done but it's not very interesting. And then right in the middle there's something that is important, reveals something about the way the mind develops, and that is feasible and tractable.

Another part of the apprenticeship is making sure they have the component skills to do the job correctly. So you came and you [the first author, who was at one time Klahr's graduate student] wanted to study collaboration on the first day. I said no-no, you can't study collaboration, you have to study methodology of problem spaces and representations and task analysis. And second year you wanted to study collaboration, and I said no-no, you can't study collaboration, you have to study problem spaces and fine-grained analysis and learn how to write up research papers. And it's only when all those component skills were in place that you could take on something that's as complex as collaboration, and even there it was only a dyad. But by then you were acculturated, you were indoctrinated to the notion that the bigger picture would have to wait until you could study a small part of it. And I think that's part of the apprenticeship, that's part of the socialization of a scientist.

Rogoff

One of the ways that the methods can be bad is that there's too many motivating questions. And I think that most of the people I know, that's the main problem that they run across in research, is trying to answer too many things at once. You may have seen some of my writings in which I argue for the importance of not just understanding one thing but understanding the larger context. So that may sound inconsistent, but I think there has to be one thing in the foreground, one question, and it can be a rather multi-faceted question, not a one variable question, but something that holds together. And I think a lot of research projects that fail is because there's too many conflicting questions and so to try to answer all of them at once, the procedures are weak for each one of those separately. An exercise that I use to try to check myself on whether I have too many questions is to think about, could I write this up in one research article.

Push theory forward.

Many philosophers of science and working scientists would agree that one of the main goals of science is to build a theory to explain phenomena and push it forward. For example, Lakatos (1978) claimed that a progressive research program would increase in content as it develops, explain the success of its predecessors, and have independent corroboration. He thought that a theory is to be rejected when its research program is degenerating. Therefore, it is important for researchers to try to construct a theory that explains the phenomena and push it forward. Although there are some differences in expressions among the researchers we interviewed, they generally agreed with the importance of constructing a new theory and pushing it forward.

Engestrom

So, I would emphasize then really good research needs to produce not only valid empirical findings using already existing theoretical tools but rather good research also produces theoretical tools. In other words, really good research is always also, you know it always pushes the theory forward, so it's not just research within the existing theory - applying the existing theory to new phenomena, but it's also pushing the theory forward.

Rogoff

There's a lot of papers that aren't data papers, and the authors think they're conceptual papers, but all they are is a list of lots of things. And so they put in everything that they think matters, and they give a paragraph to each one, but it doesn't tell me anything new, it's just a list. So good conceptual work I think has to go beyond making a list of everything that matters, and think about how they relate, and ideally to help the reader see something in a new light, so like turning around a phenomenon, like Vygotsky's conceptual work differs from Piaget's in saying, 'well if we don't start with the individual as the basic unit of analysis, if we turn this around and look at the individual as part of a social system, let's see where we go with that', so it's sort of questioning the assumptions and beginning with a different assumption and seeing where that takes you. I guess the main criterion is that it opens your eyes, or ears or whatever other senses could be used, but something that allows you to see something differently or think about something differently.

Simon

In the depths of my heart I'm probably a theorist more than I am an experimentalist, although I don't think theory goes very far unless it's tied into good experimental work, but as far as I'm concerned, I'm particularly interested in taking experimental work, taking the kinds of facts that have been gathered about human thinking, and seeing how we can put these together in theories and my experience has been with this, but the powerful tool we have today for building such theories is to model the human thinking system as a computer program, to simulate human thinking with computer programs.

Where do the ideas for interesting research come from?

Learn from real life and daily experience.

Observation has always been an important step of science. From the time of Piaget and Vygotsky, research in developmental psychology began with observation of children's daily life. The psychologists in this interview emphasized the importance of learning from daily life experiences. It is interesting to see that many of the researchers have talked about their own tricks of observation.

Siegler

One thing that I think that is important is relying more on issues that are important in children's everyday lives both in and out of school, and less on issues that are getting attention in the literature, cause it's very easy to get captured by the literature. So you read lots of studies on a particular topic and you say: Oh well, this must be very important. And when there are a lot of studies, you can always think of ways they can be extended , alternative interpretations, or confounds that were present. You can do more and more, but it makes less and less of a contribution.

I think a better way to go about is to ask yourself: What is really interesting that I see children doing? What are the real problems that they confront and how do they go about them? So a lot of times I think the best studies are ones that take as their fundamental phenomemon something that everyone has seen, but that no one has analyzed or understood very well. I think this is a way to be creative on one hand, but also linked to reality on the other.

Klahr

Well, I think in developmental psychology, in particular, we're always introspecting even though we like to act as if there's a kind of external objectivity. In psychology, whenever I read a study I think about whether it makes sense, whether I would do the task that way. So if I see something in adult cognition, then I look at the experimental situation, I always try imagine myself being a subject, see if it makes any sense at all to me. In developmental psychology, I think if you observe children, you do as a teacher or parent or researcher. You get an abstract, intuitive sense of what they can do, and then you try to match that to what you see in the literature.

Simon

In many of important cases that I could think of, I would start with some phenomena that struck me as being interesting or unusual, and the question was - 'what's the pattern in this? I think an awful lot of my research started with an observation of some interesting phenomenon.

Rogoff

Ideas also came from my own experiences as a parent. My computer was in my study and my children would have friends over and they'd play computer games, and I'd eavesdrop. You're not supposed to be listening in but you're listening in. So I'd be working at my desk and my daughter, who was in second grade, would be playing some game with a friend, and the friend was new to the game, and so my daughter would need to explain, this is how you play such and such, and so I'd listen to their explanations.

From here follows that experimental control over the situation where we started to do the study is impossible, because each child brings to the given physically neutral situation one's own previous experiences. So understanding that, I decided I should look for settings that are meaningfully organized before my ever trying to organize them without my intervention. So from that follows the interest in looking at everyday life contexts that are so ordinary that we fail to notice them. We don't see them. And meal time contexts are contexts which are so ordinary that we do not pay attention to the fact how they are culturally organized. So I decided to look at children during breakfast and lunch time, accepting the kind of organization that the given family had given to this, and analyze that structure of organization and then analyze the actual conduct by children and parents in that context. And that was the starting point for the meal time study. It was the starting point also for the study looking at how American parents try to get children to say bye-bye to the departing visitor. That is I noticed during these meal time home visits, which started at six months of age and continued to 26, that when I left the house, the mother would take the child with her to the door, carrying the child, then takes the child's hand and wave bye-bye to me. And I became curious, this was a perfectly non-sensical act from point of view of any aspect of child's development except one, that of the socialization of the child toward politeness to visitors. The child of course was simple object of moving through the motion, but for the mother, this was an important act to introduce the child to the departure context. So we went on to study that in the child's own home, looking at how a child would be asked to say bye-bye, and how the child does it. This was in 12 to 18 months range. But again the starting point was an ordinary everyday observation of something that is so ordinary that we usually don't pay attention to it. Now, ten years later, I have been turning to adults, and again the basic tactic is the same, we look for something that is so ordinary that we never pay attention to it, or what we start paying attention to it when - in our understanding, a particular social norm is violated.

<u>Stay close to data.</u>

Johannes Kepler discovered three laws that describe the orbits of planets at the beginning of Seventeenth century (ref.). These discoveries became possible because Kepler was able to access to the data of planets that Tycho Brahe collected through his life long observation. With careful analyses of Brahe's data, Kepler found exceptions that he could not fit them with the model of circular motions. This finding led him to construct the correct model.

Stories such as this suggest to us the importance of staying close to data and to be open to what the data tells us instead of being preoccupied by our hypotheses or beliefs (Okada & Shimokido, 2001; Simon, 2001). The researchers in our interviews also pointed out the importance of connecting data and theory closely.

Simon

I think in psychology we've had a considerable development of formal methodology, especially statistical methodology and experimental design. All of that is very good, but we must not lose sight of the fact that here is human behavior, what we see everyday working and living with other people. Human behavior. The whole goal of this effort is to understand it.

The first characteristic you need is to be curious and observant. An experiment is simply a formal way of observing behavior. If we get all immersed in the formalities and forget that we're really trying to understand behavior, then we don't get very far. For example, there's sometimes a tendency, especially now that you can do all of your statistical analysis with your data in a computer, to simply prepare your data for the computer and then analyze it and then never look at the original data again. To only look at the analysis. I think that leads to poor psychology, I don't think that's leads to deepening our understanding.

I think we need to keep our data, including our raw data, always in front of us. We need to always keep the phenomena in front of us and go back to it. And when we seem to see regularities, when we seem to see generalizations, we go back and ask - why? What is there in the original phenomena that would lead us to expect this? There's no substitute for looking at real data and being deeply familiar with it.

Siegler

I think in my own experience a lot of it comes from watching children, both my own and those of other people, and others of it comes from looking at data that I've collected in a lot of different ways, because often I have the feeling that even though I could get significant results out of some analysis that I did, that it wasn't really getting at the heart of the matter, that it was getting at peripheral parts, and I find that by graphing data in a lot of different ways and by trying to put myself in the child's position to try to think about how I might imagine and think about this task if I didn't really know very much about it, that this could be a very useful way of thinking about how children do. And also the more different aspects of the data that you know about and can keep in mind at the same time, the greater the chance that some idea that you hadn't expected at all will occur to you.

Rogoff

I think the piloting phase of research is probably the most important time, and it's - actually there's piloting in two phases that are important and that are under taught. Most graduate students I don't think get enough guidance in piloting the procedures and in piloting the analysis, and what I mean by piloting the analysis is mostly graphing the data. You know form that doesn't jump right into statistical analysis but explores the data, looks to see if there's patterns of things that should be collapsed or things that should be distinguished, gets a feel for the data. So both phases of piloting, I think give the researcher a feeling of solidity about the phenomenon that they are trying to study when they are piloting the procedures. And the findings themselves from their graphing, and getting [unintelligible (096)] about the data.

Engestrom

I think when you do your analysis, you have to proceed in a data driven fashion from bottom up so to speak, and at the same time from a theory driven fashion from top down. So you need the meeting point where these two directions come together, and that's where you create these intermediate concepts.

Wertsch

Many times in my professional career, I have found myself understanding a theoretical point for the very first time only when I try to use it to look at a piece of data. The data in a sense forces me to understand or reinterpret what the theoretical claim was, and so I'm a very big believer that you can not understand theory in the human sciences without trying to carry out empirical research that is supposedly driven by the theory but the theory gets driven by the empirical interpretation many times as well. I often times - American psychologists anyway - get accused by our Russian Soviet colleagues of being obsessed with data and not being interested in theory, and I think there's something to that. I think many studies are done without really thinking through the theoretical claims or the theoretical implications. Again that has to do with the reward system for disciplines and things than any kind of intellectual agenda, I think.

But on the other hand, theory is basically useless in the absence of data - useless in the sense that I don't think you can understand a theory unless you have dealt with data of some kind. And so when I say data, that doesn't mean you have to have large scale data sets or experimental studies or analysis of variance, it can be qualitative data, but there must be some kind of data that you - I always think of it as some kind of data that you push your theory against and realize where the holes are in your theory, and inform your theoretical discussion a great deal rather than just - I mean I think it's an absolute mistake to think that you have a theory and you just go out and either support or corroborate or don't corroborate the theory, I think you enrich your interpretation of the theory by using data, usually.

Klahr

The relationship between theory and data? It's a good relationship. Every theory needs some data and all data need theory to explain them. You know, it's a cyclical relationship. I believe that phenomena stimulate vague ideas and hypotheses, and then those hypotheses get transformed into some kind of further observation. Sometimes they're experimental, sometimes they're just further observations. And then they get refined, until they get to a point where you can actually try to run experiments for which you have predictions.

I don't believe in what Al Newell called a "falsification bullet." I don't believe that a single a falsification bullet can shoot down a theory; but many falsifications do. I think Popper was correct conceptually in saying that if something is not falsifiable in principle, then it's not a scientific assertion. There are many assertions that might be true and might be false, but if they're not falsifiable, then they're not scientific. So I can believe that human beings have a soul, maybe they do, or I can believe that the picture on my wall is good art, but there's no way they can be falsified. There are many claims in the world, some are scientific and some are not. What makes scientific claims special is that you can conceive that it's in principle possible that there's some evidence in the world that will change, that will make that statement no longer tenable. So popper's right about that.

His other normative statement is that we should then go around trying to falsify our theories. And nobody does that. Nobody starts out trying to falsify their theories. Everyone just tries to provide evidence that confirms their theories in the beginning. But if they see a lot of falsification they realize that they're wrong

Since data is so interpretable in so many different ways, there's no single result that makes or breaks a theory. At least not in behavioral sciences. Now, in areas where the theories are extremely precise, highly constrained, like in phsycics, at least in early 20th century physics, then there are cases where the data really conclusively support or refute a theory. So the Michelson -Morley experiment is a famous example. But today, in astrophysics, there's the relationship between data and theory is quite tenuous. Because some of the data say that the existing theories are completely wrong. And then other people question the data. Because the data are built on many auxialliary hypotheses, instrumention, augmentation, all kinds of other things. Looks like the so-called hard sciences are starting to look a lot more like the behavioral sciences these days.

<u>Learn from history of psychology.</u>

Theories and methodologies in psychology have been changed as time goes by. However, perhaps the basic questions psychologists ask are still the same. For example, Gestalt psychologists focused on problem solving and learning. However, the findings were not well accepted by the researchers in Behaviorism because Behaviorists thought that the methodology that Gestalt psychologists adopted were not scientific. After the Cognitive Revolution, cognitive psychologists paid attention to the very same questions with new methodologies developed with computer technology. Now, a similar process of revisiting classic questions from the cognitive revolution appears to be happening with the advent of cognitive neuroscience (Scneider & Chien, in press).

In that sense, paying attention to the questions that in the history of psychology researchers were interested in and shedding new light on them seem to be a useful way to find good research questions.

Valsiner

The second possibility is to look carefully into history of psychological ideas. In modern day psychology, history is often seen as a museum discipline, old ideas are all put into museum and shown to students and psychologists as examples of what we have gone through, but they didn't work, these ideas didn't work. In contrast I argue that the reasons why these ideas were forgotten have nothing to do with not working but rather with social processes in psychology. Many old ideas are as valid now as they were 100 years ago, and in fact many modern ideas are simply reformulations of old ideas. So from that point of view, instead of, for instance saying that somebody works with within social culture frame of work, one can talk about what is this framework, how is it linked with ideas 100 years back, and how these ideas could be developed further. So history of psychology has a role of informing our present and future psychology rather than something separate from our present work, and also it's not separate from our empirical work efforts

Siegler

I think that in psychology, as in any science, there are a set of inherently important questions, and most of them were stated—proposed—early in the history of psychology. At any historical time some subset of these questions tend to be highlighted and others for one reason or another tend to be ingored, and to me sometimes it's possible to do a study that makes a large and interesting contribution to an area that is already being highlighted to an issue that's already been forefront, but more often the most interesting studies come about from posing an inherently important question, but one that hasn't been looked at in the way that you're addressing it in the past, and that you can pose in a somewhat different way, perhaps bringing new technologies or new concepts to bear on it in a way that advances understanding well beyond where it was before.

What kind of strategy do you use to find answers for your questions?

<u>Response to surprising data.</u>

Many studies in cognitive psychology suggest that people have a confirmation bias– a tendency to be captured by their preconception, pay attention to the phenomena that fit their hypotheses, and ignore unexpected results (e.g., Wason, 1960). However, research of scientific discovery has also shown that discoveries occur when researchers can overcome their bias and pay attention to surprising results (e.g., Dunbar,##, Simon###).

Actually, many discoveries were achieved through, so called, serendipity, in which paying attention to unexpected results, people find something that they were not originally aiming at. For example, in 2000 a Japanese chemist Hideki Shirakawa received Nobel Prize for his discovery of a plastic that conducts electricity. He was conducting experiments with a visiting student using a catalyst for polymerizing acetylene. One day, the student accidentally added one thousand times more than normal amount of the catalyst. The result turned out to be a silver colored film that was completely different from the ones that obtained with normal amounts of the catalyst. This accident triggered him to pursue further study on this material and led him to the discovery of the new plastic. Thus, noticing and effectively using surprising results seems to be an important aspect of good research.

Rogoff

I think another thing is for people to keep their eyes open to the parts of the research that they aren't anticipating. I think it's very easy for people to go into a research project with preconceived ideas. We all do. But if we don't learn from our efforts to try those then we don't learn much from the project.

Simon

Pastuer (sp?) said it very well, he said "accidents happen to the prepared mind", so if you come back from vacation and you have a dirty petrie dish with dying bacteria in it and mould near it, you have to know some bacteriology to know that the bacteria are dying and what the mould is and why the one might have something to do with the other. Otherwise you could look at it all night and not discover penicillan, right? So, you look at phenomena but you look at phenomena which you have studied and studied and studied until you know a whole lot about them. And then you know when the phenomena are unusual or interesting or patterned. You've put in your ten years.

Counter your commonsense. Focus on weak points of a dominant theory.

We are often heavily constrained by commonsense. However, in order to conduct creative research, suspecting the commonsense is often necessary. For example, twenty years ago, in spite of the widespread commonsense in the medical field that stomach ulcers are only caused by stress or poor diet, Marshall and Warren discovered that they are also caused by a bacterium, Helicobacter pylori (Ref.). Their first paper reporting this finding was turned down from an academic society in Australia because the finding was so counter-commonsense. However, once it was accepted in the medical communities, this discovery drastically changed the way in which ulcers are treated.

Overcoming commonsense is not an easy task, because we are not usually aware of what kind of commonsense we have. Therefore, it would be very useful if we carefully pay attention to what we are doing and intentionally counter our own commonsense. Similar to this point, intentionally suspecting the premise the dominant theory (i.e. commonsense of the field) might also enable us to reflect upon our own beliefs that we often take it for granted.

Valsiner

The first thing is to think about what is target of the interesting question that one wants to ask, and how is it situated relative to the phenomena that the question refers to. Let me give you an example of an uninteresting research. Uninteresting research for me is for example in the United States we see efforts to study why children drop out of school. This is a question that is given to psychologists by educational institutions. Educational institutions has an of interest in keeping children in school, but some children drop out of school, and then the [graphical] problem arises, 'why do they drop out'. So psychologists are asked then to study, 'what are the reasons for children dropping out of school'. For me this is an uninteresting question because the question of whether somebody drops out of school or does not drop out of school tells us nothing about human development. It tells us something about what educational institution wants to find out but it doesn't tell us about basic processes of human development. So, from my point of view, I would replace that research question by the opposite one, mainly 'why do children stay in school'. So why on earth would they stay in this environment that they have open ambivalent relations with and they still first of all stay, and second they succeed. That is I would replace the question of school drop out with a question of school 'stay in'.

And because that would be a question that has greater generalizability, that these children not only stay in school and succeed, they also stayed in that - for instance, out of school activities - [and] stay, they persist and they prevail. If they are street children, they prevail in the streets, in economic activities and so on from a very early age. The question is that of child resilience, is a strength of children coping with different environments, one of which could be the school. And the fact of the dropping out of school is not interesting, it is just moving away from one environment to another. Some children drop out of school and drop into some other ways of living. And school and these other ways are equal from my point of view. They are not equal from the point of view of educational institutions.

MacWhinney

One thing that is very important for the theoretical work is to find ideas that have been articulated by other people that you find truly wrong, and this is very helpful. If you see a position - a particularly important major position that is really, really wrong, this will help to clarify your own thinking, and to me Chomsky is a good example of this, because I think he is so very much wrong and it's very helpful. At the same time you need to rely on people who have taken the other point of view. This is very much the notion of the dialectic which is the interplay between two different opposing ideas and I think that that's an accurate view of aspect of theoretical work, so that would be perhaps the most important part of it.

Communicate ideas with others

New ideas are often generated when we encounter people with different perspectives. Research on scientific collaboration has suggested the importance of discussion with others (e.g., Okada & Simon, 1997). In our interviews, researchers also pointed out the importance of explaining their own ideas and discussing them with other people. However, it is also important to notice that just explaining our own ideas to others or ourselves is also equally important. Kuhn (2001) describes this phenomenon as "orangutan theory": "If I have some new ideas and I go into a room with an orangutan to explain them, the orangutan will simply sit there and eat its banana. I will come out of the room, however, knowing more than I did before."

Siegler

So, I guess that this points to another aspect that's important coming up with new ideas which is to talk to other people about new data sets that you're coming up with and to really delve deeply into the data and throw ideas around about how all these different parts of the data could be produced by the same person, and when you do that, then sometimes ideas that you don't think at first are particularly good ones, that their value emerges.

That (study on rule assesment approach) actually was a funny kind of discovery because it was a study that I didn't think was special when I started it, I just thought, well it could be interesting, and I was talking to an undergraduate class on adolescent psychology, and I was talking about cognitive development and I decided to tell them about what we were finding in this study, and before doing the study I hadn't proposed these rules at all, all I had were the kinds of problems which I thought would tell us something or other, and when I was talking to the class I went to the blackboard and I said it's as if the children have these rules in their head's, and I drew the - I said the five year olds - it's like they have this rule with weight and sometimes distance and so on, and I left the class afterward and I was kind of pleased with myself that I had been able to teach the class in a coherent way, but I realized that I had mastered these rules so poorly that I wouldn't be able in all liklihood to repeat them so I went back to the classroom and brought a notebook with me and copied them into my notebook because I was pretty sure that I'd forget them, that I just barely understood them at the time, and then when I had them down I was able understand them better. So again this kind of social impact was pretty great, though the students didn't help in any tangible sense, it wasn't like chain work - giving reactions and saying yes you can apply the idea here and here, but even just talking with another person who wasn't answering or saying anything in that case was helpful.

MacWhinney

I think it requries a lot of hard work, and it may be helpful to have a group of people who are able to communicate about certain issues, so that teacher student professor relation is often very helpful to be critical because here you need strong criticism of ideas to look at confounds, alternative explanations, ideas of potential problems in the literature.

I mean you could have a very determined researcher but if they didn't have good communication with other people, they would fail to understand some of the issues, and particularly in psychology this is a problem, because the issues are often not very clearly stated, and so you might misinterpret what is necessary. Of course one possible approach is the so-called paradigmatic approach you set up your own issues, and you simply continue to explore, explore, explore, but this is often becomes a little boring, millions of studies all based on the same thing because they don't really necessarily test something that is debatable, you know they just explore, explore. So, I think maybe if I were to think longer about this I would say even within the empirical area there are again some genre. Some of these are the programatic genre. This can be done without talking to other people. The other type, the type that is the more that is the more what we might say sexy research, this really requires that you be going to conferences and all this stuff.

Rogoff

Another piece is to think about the whole thing, both the empirical and conceptual work as a process of communication. I think it's probably different in Japan than in the United States, but in the United States I think many scholars think of themselves as solitary genuises, and if they have this great idea, then it's the having the great idea that matters. And so what I'm emphasizing is having the great idea might be fun, but that's not what progress in the field is based on. It's based on communicating the great idea. And I think there's a lot of people who may stop short of the communication part of things, they just have fun having the idea. Or they think that if they put it down on paper the reader should be able to understand it, but a huge part of scholarly work is figuring out how to help the readers understand it.

<u>Connect theory with practice and intervention</u>

Brown (1989) proposed design experiments in which a researcher designs an environment and see what happens in the environment. She thought that this is a way to understand and improve an education system effectively. This way of conducting research is fairy different from a traditional experimental design in which a researcher controls variables by varying one variable at a time to see the effect of a variable conclusively.

Engstrom

The classical dichotomy here is that if you do analysis you try to avoid intervening, try to avoid influencing the object of research. And I think that we have to learn to bring these together and actually use interventions as a key research method, and still we have to be very, very careful so that when interventions are used, we also have to examine the interventionists, that is the researcher must be very, very aware of what he or she is doing, and we must analyze our own actions as researchers, not just the object of our research. If you want combine or integrate analysis and intervention, you need to be very conscious of your own actions as interventionist, and they have to be submitted to a rigorous data collection and analysis also. Otherwise you very easily sort of take the role of where you of sort of think that you have the right to make interventions as you wish, and this is obviously not the case. A good intervention has to be based on very careful analysis of the possibilities and the challenges in the activity that you are analyzing. So, I think that the good research is very dependent on bringing together or overcoming these dichotomies.

<u>Broaden concept of objectivity.</u>

For some people, especially for young students, it may look that science requires solid and stable criteria about what is the right way of conducting research. However, philosophers and sociologists of science have been pointing out that it is not true. For example, sociologists of science, Fujigaki (Ref.) proposed the concept of validity boundary. A validity boundary means a boundary that a journal accepts or rejects submitted papers. She claims that such a boundary changes over time. For example, after a fraud was discovered in a Japanese archeology community in 2000 in which a researcher faked Paleolithic stone tools, the members of the archeology community have been discussing and constructing new criteria to decide what are the authentic Paleolithic stone tools (Yamnouchi and Okada, 2003). This suggests that a validity boundary in the archeology community is now drastically changing.

A concept of objectivity can also be regarded as one of such a kind. When Behaviorists were dominant in psychology, verbal data were regarded as subjective and discarded from analyses. However, after Cognitive Revolution, protocol analysis became a part of standard methodology for formal analysis. More recently, researchers in socio-cultural approach adopted the concept of inter-subjectivity claiming that there is no "objectivity" about human activities. This concept is now accepted in the community of socio-cultural psychology. Engestrom's claim resides in the center of this trend.

Engestrom

I think objectivity, which is closely connected to questions of validity and generalizability - so how generally valid your findings are - those are very related concepts, objectivity, validity and generalizability. I like to think that in this question I feel that first of all, we should emphasize issues of first of all dialogical objectivity, so that objectivity is something that can be constructed together in a dialogue between the researchers and their subjects. It is not something that is given, no statistical procedure or any other such procedure will guarantee objectivity because objectivity is continuously, you know it's like a prism for different participants in an activity. For them the activity necessarily presents itself in a different way, so objectivity is something like building this prism together so that people can better grasp it's complexity jointly, together. So I think that's the first quesiton. This dialogical aspect of the second issue of objectivity or generalizability is that when you do your interventions, you have to follow them long enough to see what happens, so you have to stay with the system long enough, so this requires longitudinal work. If you do an intervention, it looks like it tests some wonderful impact, and then you write a report and maybe you come a few months later and you realize that there's nothing left. So it's very important that you stay with the system long enough to be able to sort of not say whether something is right or wrong, but how the development actually takes place. No intervention ever is completely successful. I don't think that success is a right measure. I think that the important issue is to construct objectivity over time, as something which evolves. In other words, let's say we have hypotheses, a certain kind of intervention will radically improve the lives of the participants in a certain activity. And I think that if you see it actually happening in some form, it's important first of all that you don't [keep it unanalyzed?, counter 209] over time, how that improvement takes shape, usually it always surprises you, there is always new twists and turns which you didn't expect. But another question is then, will it generalize, will it be adapted by others that just knows with whom you worked. And that for me is the generalizability. So for me generalization of results is not just an academic exercise, but it's a very pragmatic issue. Let's say if you do an experiment in which try to improve the learning in a system of activity, and then that becomes somehow first of all sustainable that they actually acquired new tools or new ways of learning. Then the interesting question is will those spread, will they take a life of their own so that other people will adopt them, or will they be very limited and encapsulated and gradually die away. So these processes, or actually following up, I think are crucial for objectivity. So dialogism and objectivity and the longitudinal aspect, let's say objectivity as something that evolves, as an evolutionary notion of objectivity I think is very crucial. And it's very different of course from the standard textbook notion, because in the standard textbook notions you always have various techniques are which supposed to establish objectivity, or guarantee that you don't have too many biases. Certainly those can be useful and important, I'm not saying that we should abandon standard techniques to improve the objectivity of our research, but I'm saying, especially in the development of research which is interested in change, you need much more than just the sort of standard statistical or other procedures, you need a much broader of objectivity.

Use historical analysis.

When examining a patient, a doctor would first ask him or her to tell history of illness that s/he had. Understanding history of the patient's illness is an essential step for treating him or her correctly. If s/he has a record of allergic response to certain drugs, treating him or her without knowing it could kill him or her. Research activity also has a similar aspect. In order to fully understand the mechanism of a system, you have to know the origin of the system. Engestrom point out the importance of the historical analysis of a system for effective intervention.

Engstrom

I would emphasize that if you want to integrate interventions and analysis, you have to do it on a basis of studying the history of the system rather than just pushing your own values. If you start to understand the history of the system, you can enter into a dialogue with the system, with the participants or the subjects. If you only bring your own values, often times you'll be very disappointed because they have little impact, you're interventions may not have any sustainable impact, because you don't understand what is going on in the system you're studying, what is the historical dynamics there, only if you carefully study it's historical development you can find the sort of critical issues or tensions or contradictions in the system which you can then address and which can be key points of dialogue.

Consider unit of analysis.

In 1609, Galileo turned his telescope to the sky and revealed various new findings, such as that moon has craters and mountains (Ref.). In 1665, Robert Hooke discovered “cells” using his microscope (Ref). Zooming in the targets, they saw what human never saw before. On the other hand, the Nasca Lines, big figures on the ground of Andes desert, were “discovered” by an airplane pilot in early 20th century (Ref.). We cannot see some kind of targets without zooming out. Thus, the level of focus is essential when we want to fully understand a target phenomenon. Engestrom’s suggestions about the unit of analysis echo with this point.

Engestrom

From the point of view of a cultural-historical activity theory, the key issue is that you have to bring together certain previously often sort of separate points of view or perspectives and reconnect these. And one obviously is that such research is not only research on individuals, that the unit of analysis in not just the individual, but it’s also not the whole society, and somehow bring these two together and create a sort of intermediate unit of analysis. From my point of view, such a unit of analysis would be a collective activity system, but obviously there are many other possibilities of conceptualizing such a unit, an intermediate unit of analysis. So think the first the question is the choice of appropriate unit of analysis which overcomes the dichotomy between the individual and society, and creates something where these two meet, where they come together in real life. And for me, it’s concrete activity systems where individuals come together and actually construct the society. So, another key issue for me is the issue of bringing together analysis and intervention.

Practical Advice for Apprentices

Have a secret weapon.

A Japanese physicist, Masatoshi Koshiba, was awarded the Nobel Prize of Physics in 2002 for his pioneer contribution to the detection of cosmic neutrinos. For this research, he built a gigantic water tank, called Kamiokande, in a Japanese mine and spent more than twenty years conducting observations. In 1978, he detected neutrinos released from supernova explosions for the first time and developed a new research field as neutrino astronomy. This discovery became possible because only his team developed and watched a gigantic tank of water. In this sense, Kamiokande was literary his secret weapon.

In the history of science, there are many examples of discoveries that were possible because the scientists who made them had some kind of secret weapon that others could not have at that time. As mentioned above, Tycho Brahe's data of planets that only Johannes Kepler was able to access after Tycho Brahe's death was also an example that a secret weapon took an important role for scientific discovery.

Although we have previously argued that passion and interest are perhaps more important elements to choosing good research questions than the tactics of building a career, we should note that researchers, especially young ones, cannot ignore competition. At the choice of the phrase "sercret weapon" might suggest, the life of science is sometimes a battle between competitors. Young scientists must publish papers, get grants, and get tenure at good universities. So they must not only do good science, they must sometimes get it done before anyone else beats them to the punch.

Simon

Well I'd suggest first getting in a habit of working an 80 hour week and enjoying it. Then, in research you have to find something new. That means you have to find it before other people find it. So if you have a good question now, why do you think you can find an answer to it? Other people probably know this is a good question and they haven't found an answer.

So, do you know of some first steps you can take that will get you closer to an answer to the question? Not the last steps—if you knew the last steps it wouldn't be very interesting research. But can you see what you would do the first weeks of studying something like this?

And then the even harder question: Why do you think you'll get there before anybody else? Now, there may good answers to that. Maybe the university you're working at has a research tradition and research activities which put them at the forefront of a certain area. Then you have a better chance than other people at other places to make contributions to that area. Maybe your research laboratory has equipment that's not available at all universities—equipment,for example, for recording people's eye movements. Then research which makes use of that particular way of getting information gives you a chance of being there first. So you always must have a secret weapon, you must have some reason for thinking that you can get there faster than other people have. Some people think they can do that just because they can be smarter than other people, but that's a dangerous assumption. It's much better if you have some tangible reason in terms of particular background you have, the particular experience you have, the particular strengths your laboratory has, particular equipement your laboratory has to be - for choosing that area.

Use opportunity effectively.

Responding to new opportunity could lead us to new research topics or a new field in which we might find a lifework. For example, the second author was somewhat at wits end as he finished graduate school with no job offers and no well formed plans for a post-doc. At the time his wife was applying for medical residencies so he traveled with her on her interviews to cities across the USA. He killed time while she interviewed by visiting each city's museums. During these visits, he noticed that parents gave various explanations to their children in front of exhibits. Although that notion was not directly connected to his ongoing research projects at that time, he foresaw the potential of the new research field and started a research project to study museum learning by applying psychological methodology. Rogoff suggests that being open to such a new opportunity is important but also difficult.

Rogoff

People in journals tend to say - they ask for the rationale for why you do a study, and I think people are kind of shy to say 'because I was there and it was interesting'. But I think that is a really important reason to be doing a study. Here's an opportunity. It's also important to be able to pass up some of those opportunities, because there's many more opportunities that are available to most people than the ones that they should actually follow up. So it takes some restraint not to do all of those, but to be open to the ones that look like really good ones is really important.

<u>Try to make your own coherence.</u>

It seems that prominent artists often have their own themes or styles such as that Monet painted waterlilies for 30 years and that Jiacommetti produced many tall and thin bronze sculptures. Through such sustaining efforts, they were able to produce their masterpieces. Even though Picasso changed his style many times in his lifetime, we can see that there is a reasonable coherence in his transition if we read a biography of him (Ref.).

As such, it seems that it is also important for researchers to have coherent themes in their research activity. With such coherence we could use our limited time and physical resource effectively and think deeply on those themes. Thus, thinking about coherence of your research would help you to find the strength as a scientist.

Rogoff

It needs to fit with a conceptual question that is interesting to the field, and for any researcher it needs to be something to follows their line of work. So some people take up opportunities but the opportunities aren't coherent. So study A may not relate at all to study B which might not relate to study C. That's not a good idea because then you just [spot] around and you don't have the background in the literature. Most research projects are much more complicated than they look on the surface. So if you spot around in too many different areas you can't become deep enough in thinking about the tricky parts. So it has to cohere, but for me it coheres in ways that other people might have not thought it would cohere. I think graduate students think that they have to follow a line of thinking that their advisor might tell them 'this is a good line of thinking'. But it might cohere in a different way than what the older generation thinks are the boundaries between research topics. My topics did not cohere in a way that my advisors would have recognized. I had to work very hard to tell people how they did cohere, and in working very hard to tell people how they did cohere, that's where I began making conceptual advances.

And I think that that's the burden of each generation, to say 'OK, my advisors might not see what sense this makes, I need to tell them how it makes sense', and I think that's part of the creativity of each new generation to say, 'Ok, I know there's something important here, and I know that both of these ideas connect', and of course you can't just leave it at that, you have to say 'here's how they do', and for me that's been an important part of my conceptual work.

Use multi-method and multidiscipline

Historically, psychology has been an interdisciplinary field. At the very beginning of its birth, psychology focused on problems that philosophy had addressed. Trained originally as a physicist, Fechner wrote a book about psychophysics in 1960 after ten years of conducting research and proposed a new field in which human perception are focused on with methodology inspired by experimental methods in physics (Fechner, Ref). This episode suggests that applying methodologies from different fields to answer psychological questions has been our forte.

In both approaches that we focused on in this chapter, the researchers tend to integrate multi-methods from various fields to conduct research project. For example, many studies on scientific discovery in information processing approach have incorporated interdisciplinary methods by using case analysis of a famous scientific discovery in history of science and collecting psychological experimental data to form a computer simulation model of discovery. As shown below, researchers in the socio-cultural approach also emphasized the importance of multi-method and multi-level analyses.

If your ideas about theory or research methodology are really unique and new to the field, there might not be a community to accept the ideas. In such a case, new community needs to be formed. As Schunn, Crowley, & Okada (1998; in press) demonstrated in an historical analysis of

Cognitive Science Society, a new approach requires a new community for exchanging and developing research ideas. Wertsch suggests the importance of such a community as well.

Wertsch

I think it's important to be familiar and comfortable with the methods used by more than one discipline, basically, and so we often times get in useless arguments about this - either this or that method - but a combination of some kind of qualitative analysis might be ethnographic analysis, it might be a kind of discourse analysis, in depth linguistic analysis with some kind of sampling and controlled experimental study. I think that's the key.

The problem is that that creates very big challenges - especially for junior scholars - because it's hard to be comfortable with more than one method, um, for starters, but that's an intellectual reason, but there's also a political professional reason, namely, that - at least in the United States - it often times is rewarded to become very well known in one narrow-narrowly defined discipline or sub-discipline with one method. That's a safer way to conduct your early career. For somebody starting out trying to do socio-cultural research and drawing on different methods, they don't have as much research of using the same method in one field as somebody who just stays in one field. And so that's a big problem with the way disciplines are currently defined, and it's something for example that - it's one of the reasons that we've created organizations like the international society for socio-cultural reasearch. We had a meeting in Madrid in 1992 and another one in Geneva in 1996 and we hope to go to Brazil in 2000. But part of the reason for doing that is that we need to provide a kind of strong organization that recognizes this multiplicity of methods and rewards a kind of work of junior scholars that is otherwise not recognized within a single discipline as well.

Do not rely heavily on prescriptive method of hypothesis-testing

Okada & Shimnokido (2000; 2001) showed how strongly Japanese psychology community has been influenced by hypothesis-testing method. Adopting a hypothesis-testing method is one of the very useful strategies for productive research. However, it sometimes acts as a prescriptive method and may suppress an innovative way of conducting research. MacWhinney more bluntly claims this point.

MacWhinney

I think one of the standard answers that I really don't totally accept is this notion that interesting research involves studies that clearly prove points, that make up a point that is solidly established by hypotheses testing, they exclude this notion of falsification, that they exclude some hypotheses by absolutely no strong way. Those studies would be interesting, but they don't exist in psychology. I think it's a silly idea to think that they exist.

Conclusion

Having now heard the psychologists speak, what are we to make of their advice? When we were graduate students, after reading thought provoking papers or listening an exciting talks at a colloquium, we often chatted over coffee about what are the features of good research or what kind of researchers we want to become. Although such discussions were not directly related to the research projects that we participated in at that time, we feel that it was a quite important experience in order to form our academic identities. As we mentioned at the beginning of this chapter, we believe that methodology textbooks and classes should pay more attention to topics such as how to find a good question. Many people may think that because generating creative ideas or finding interesting questions requires special talent or sense to be born with, it is impossible to describe or teach graduate students how to generate good questions. However, the art of finding interesting questions might not be such a mystical process and we might be able to decompose it to various skills or ways of thinking that top researchers are using in their daily research activities. It seems that there are very few literatures that focused on this aspect. So, we wrote this chapter through asking prominent psychologists to speak their own art of finding good research questions. It would be our great pleasure if this chapter inspires discussions among graduate students at a coffee break and helps their future research activities.