What is Research? Part II: Acting Like a Researcher[1]

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Why do you believe what you believe? My students often believe things like drinking alcohol on a cold Saturday during football season will warm them up and that going outside with wet hair will cause them to catch a cold. When I ask them how they have come to trust these assertions, they often draw recourse to authority figures like parents or to their own experiences.

One of my favorite philosophers, Charles Sanders Peirce, suggests that our tendency to draw certain types of inferences and thus settle doubt is a habit of mind (Peirce, 1877). In the first paper in this series, I discussed several attitudes necessary for conducting good research; if held, these attitudes help constitute a habit of mind akin to a scientist whose sole concern according to Peirce is to settle opinion in a particular way.

For people who are passionate, curious, and persistent, an irritation of doubt will certainly arise that cannot be settled by other ways of knowing. Tenacity won't do. It seems impossible to "go through life, systematically keeping out of view all that might cause a change in opinion." Likewise, relying on authority may work in some cases, but in general, preserving belief because of tradition or "my mother said so" seems ill advised especially for matters of any importance. And while it may be tempting to allow our inferences to be drawn from things "agreeable to reason" or otherwise in harmony with our own unique experiences, some of the most interesting findings from science suggest that many things go against intuition and that our experiences don't tell the whole story. Peirce suggested science as a method for fixing belief that is "determined by some external permanency – by something upon which our thinking has no effect … The test of whether I am truly following the method is not an immediate appeal to my feelings and purposes, but, on the contrary, itself involves the application of the method."

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So, in this paper I want to briefly define how I see this thing we call the scientific method and spend most of our time with the nuts and bolts of design and data issues you should face as you think about how to fix belief regarding listening and human communication.

Science: A Working Definition

First, I want to be clear about one thing: Science is A way of knowing, with a big emphasis on A. Scientists are driven by the prime component of understanding (Berger, 1991; Berger & Chaffee, 1987). As the natural sciences were created to understand the natural world, the social sciences were created to understand the social world. Natural and social scientists alike are interested in "sense-making" -- solving puzzles, understanding patterns, and investigating social problems. Thus, to the extent a researcher is interested in understanding a concept such as listening, she should be equipped with tools necessary to engage in this understanding.

Second, I view most of what listening researchers do as scientific, even if numbers are not used to substantiate claims. The data that one researcher needs to answer her specific questions may be different from the data that another researcher needs to answer his. Some of those data may come in the form of numbers, while other data may be presented in the form of a narrative. Either way, researchers are trying to make claims about the larger nature of listening in some specified context; they are trying to understand listening in all its complexities and subtleties. Thus, they are scientists. As scientists, they face two primary sets of issues in the process of inquiry, those associated with designing studies and those associated with analyzing data.

Study Design: Consideration I – Population and Process Inferences

In terms of design, there are 3 primary issues that need to be addressed. The first is the population of primary interest – that is, what is the universe of objects about which you are trying to make some kind of inference? When a public opinion pollster calls a randomly generated list of phone numbers, she isn't interested necessarily in the people who happen to be called. Likewise, the media researcher interested in how nightly news portrays terrorism or the war on drugs isn't interested in the specific programs that were selected and analyzed. The focus of these inferences is on the population – the public's opinion in the former case and the nightly news in the latter.

It would be quite burdensome if every time we had a question we had to ask every relevant person or read or watch every relevant news story. Instead, researchers rely on samples – smaller subsets of the population. When a researcher uses a smaller subset of the population and then uses those data to draw inferences about the population, she is making a population inference – a statement about a population by generalizing a description of the data in the sample to the population from which the sample was derived.

Many of the claims we want to make about listening take the form of population inferences – what are barriers to listening; what listening strategies do salespeople use and are they effective; are better leaders also better listeners; do parents who paraphrase their children have closer relationships with those children; and so on. Each of these inferences tries to make a generalization about some population.

Perhaps the best example of population inference in listening research is the time study, those studies trying to estimate how much of a person's day is spent listening compared to engaging in other communicative functions (Bodie, in press). The original study was conducted by Paul Rankin who asked 21 adults to record the types of communication activities in which they engaged every 15 minutes for one day. Subsequent studies have attempted to replicate the Rankin study using slightly different methods. For instance, Barker and his colleagues collected responses from 645 undergraduate students at Auburn University in 1977 who were asked to think about their last 24 hours and estimate time spent in various activities (Barker, Gladney, Edwards, Holley, & Gaines, 1980).

Several aspects of these studies are relevant to the population inference they are trying to make. First, none of them utilized standard sampling procedures developed to generate samples that are representative of the overall population of interest. The Rankin study had merely 21 participants, and most subsequent studies have sampled only from one US institution (for an exception, see Emanuel et al., 2008). As Hayes (2005) stated, if the goal of the research is to generalize from the data to some population of interest, we want to do our best to make sure that the sample is representative of the population. A sample is representative if it is similar to the population in all important aspects relevant to the research. Studies interested in the time spent in various communicative activities of college students are thus well justified in limiting data collection to college students. At the same time, limiting the collection to one institution is not – indeed, I am not sure how many people care about how LSU students spend their communicative time; instead, we are interested in getting a more representative estimate of the population of college students which would require sampling from a number of institutions. LSU is a large, public university with a research intensive classification. Other types of institutions including private, community college, and Jesuit are not representative. If there is reason to believe that the individuals sampled in these two studies are different from individuals who were not sampled, then the estimates provided in these studies do not serve their intended purpose. Of course, if these authors were merely interested in collecting these data and generalizing to their particular student body, then no harm done; such might be the case if you were interested in developing a curriculum for your students that emphasized particular student needs, for instance.

So, one type of inference you can draw with research is a population inference – when you want to know who is likely to win an election, whether a referendum is likely to pass, or how students spend their communication time. These types of inferences are fully reliant on the representativeness of your sample. The only way to retain a fully representative sample is by employing some form of probability sampling like simple random sampling that begins with a list or database of all members of the population from which the researcher selects at random or random digit dialing that is often used with telephone surveys.

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Of course, sometimes research is not conducted to make population inferences. The second type of inference listening scholars seek to make is called a process inference which assesses the degree to which data conform to theoretical predictions (Hayes, 2005). In the first manuscript in this series, I defined a theory as an answer to the question why. Researchers primarily “give meaning” to actions, objects, events, etc. through their placement within a theory, and concepts like listening take on various meanings as they are studied through various theoretical perspectives. Thus, theories help give structure to otherwise structureless concepts and guide practice in complex situations.

So, when we are talking about theory-driven research or research which primarily attempts to understand and explain some practical problem in a general and abstract way, the focus of inference making turns from population to process. With theory-driven research, we are less concerned with estimating the size of an effect such as the comparison between time spent listening and time spent surfing on the Internet and more concerned with determining whether a prediction our theory makes about what should happen in a research study actually does happen.

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I will use as an example my work on the differential effects of supportive messages we started discussing in the last manuscript.

A host of studies interested in population inferences have observed the fact that there are many ways in which a helper can provide assistance. Some was of providing assistance are quite helpful while others can be detrimental to our health and well-being.

The typology of supportive messages that I work with explains why some messages are likely to be perceived as more sensitive than others: Messages are more sensitive because they are more "person-centered." Person centeredness (PC) in the context of support refers to the extent to which messages explicitly acknowledge, elaborate, legitimize, and contextualize the feelings and perspective of a distressed other (Burleson, 1994). In other words, PC is a theoretical explanation for why some messages "work" better than others—they "take into account and adapt to the subjective, emotional, and relational aspects of communicative contexts" (Burleson, 2007, p. 113). Messages in the lower third of the hierarchy (levels 1–3) are referred to as low person centered (LPC) because they deny the other's feelings and perspective by criticizing or challenging their legitimacy, or by telling the other how he or she should act and feel. Moderately person-centered (MPC) comforting messages (levels 4–6) afford an implicit recognition of feelings by attempting to distract attention from the troubling situation, offering expressions of sympathy, or presenting nonfeeling-centered explanations of the situation. Finally, highly person-centered (HPC) comforting messages (levels 7–9) explicitly recognize and legitimize the other's feelings by helping the other to articulate those feelings, elaborate reasons why those feelings might be felt, and explore how those feelings fit within a broader context.

The example we used last time was a set of messages that are relevant to a study in which students were asked to imagine there were enrolled in a class that is required to enter their academic major. They need a B to be enrolled, but make a D. A friend approaches them and says:

> Well, it makes sense that you feel bummed out about the grade. I mean, I know how frustrating it is to work really hard in a class and still not do well. That can drive you crazy – it can sort of blow your self-confidence. You've got every right to feel that way. I mean, getting a bad grade is always hard. I'm sure that you can figure something out;

> you're one of the brightest people I know. That's why this must be getting to you right now.

Now contrast that to the LPC example:

> Well, maybe you just didn't try hard enough. Maybe that's why you got a D. You're probably just gonna have to study harder from now on. You know, you shouldn't be so upset about the class if you didn't work as hard as you could have. Just try to forget about the class. You know, there are more important things in the world than getting into a certain major. Anyway, it's a pretty dumb class; it's really not worth worrying about. So, just try to forget about it. Think about something else.

Over 30 years of supportive communication research finds that messages higher in PC lead to a range of positive outcomes, from making the recipient of such messages feel better about a particularly stressful circumstance to helping the recipient cope successfully with later stressors (High & Dillard, 2012). But, it is also true that sometimes the impact of PC messages is variable – sometimes we feel better after a HPC message, but other times these messages don't make us feel quite as good. WHY?

My colleagues and I have proposed that the impact of supportive behaviors on outcomes is both a function of the content of those behaviors **and** how they are processed by recipients (Bodie & Burleson, 2008; Bodie & Jones, in press; Bodie & MacGeorge, 2014; Burleson, 2009). One of the primary contributions of our dual-process framework (DPF) is to suggest a reason why messages often have variable impacts on people's coping. Specifically, the theory predicts that when individual motivation and ability to process are high, messages and other behaviors that require extensive thinking will have a greater impact than when people are not motivated or unable to process these elements of support. In conditions of low motivation and ability, message content has less of an impact compared to factors such as the closeness of the relationship between the two people and the biological sex of the support provider.

One of the primary contributions of the DPF is to suggest revisions to current theories explaining how the comforting process works. Nearly two decades ago, Burleson and Goldsmith (1998) theorized supportive conversations as a *process of facilitating reappraisals* whereby emotional experiences result from appraisals which in turn, are about how events are evaluated in the context of personal goals and needs. In 2006, Jones and Wirtz published the first empirical test of Burleson and Goldsmith's theory, finding support for the reappraisal framework. In particular, they reported that HPC conversations stimulate the use of positive emotion words by the distressed individual which leads to heightened cognitive reappraisal and finally affect improvement. The modification suggested by the DPF to the theory of cognitively induced reappraisals is that the conceptual model will be moderated by processing motivation.

In particular, when people are able and motivated to elaborately process supportive message content, they will; and thus support will work through high elaboration mechanisms. When people are unable or unmotivated, these high elaboration mechanisms will not explain the relationship between supportive behavior and outcomes – that is, support will work differently (through different processes) as a function of how much cognitive effort people are expending.

To test this, I had 192 college students imagine they were experiencing one of two hypothetical scenarios manipulated to create moderately stressful and mildly stressful versions (Bodie, 2013). After exposure to the stressor, participants were exposed to either a LPC, MPC or HPC comforting message. Positive emotion words and cognitive reappraisal were coded from participant thought listings taken after message exposure and used to operationalize the process through which support has its effects; anticipated affect improvement was measured as the effect of primary interest.

Three models were estimated, one for all participants, one for those in the moderate condition, and one for those in the mild condition. While the Jones and Wirtz data were replicated for the combined data, the mediated model fit much better in the moderate condition in which 32.8% of the VPC-AAI relationship was mediated by positive emotion words and reappraisal compared to only 7.8% in the mild condition. Thus, there is support for the DPF– we have confirmation of a process inference.

A critique I hear often about this type of work is that the study was conducted using students enrolled in a particular type of college course at one institution who were conveniently accessible to use for this purpose. To that I respond, yes! The critic continues:

> So, the results are not generalizable to "people in general" or even college students; we still don't know much.
>
> *Me*: That's unfair. The point of the study was to test a theoretical proposition. It does not matter if the participants were not randomly selected from some larger population of interest because the goal of the study was to test a process. So, we know the process works for at least this sample of data. Future work can now go about testing boundary conditions for the theory. While it is possible that different types of people are affected differently by supportive messages, the mechanisms underlying effects should be rather consistent. Focusing on surface level similarities of a sample with a population is too simplistic in this case.

In other words, a second type of inference that is important for researchers to draw is of the nature of the process underlying the effect of one variable on another variable. This discussion thus also highlights a second inference dichotomy – inferences of the correlational kind versus inferences of the causal kind.

Study Design: Consideration II – Correlational or Causal Inferences

If researchers are concerned with causal relationships (as we often are in the field of communication) a more appropriate design is the *experiment*, offering full control over the independent variables under question.

Experimental research involves the manipulation or pre-structuring of the environment and observing/measuring reactions to that manipulation. The variable that is manipulated or the element of the environment that is pre-structured is called the independent variable.

The core feature of all experiments is to "deliberately vary something so as to discover what happens to something else later – to discover the effects of presumed causes" (Shadish, Cook, & Campbell, 2002, p. 3)

In the study on supportive communication discussed above, two variables were manipulated: the person-centeredness of message content and the putative severity of the stressful event. Each can be considered an independent variable, though in the DPF stress severity is also called a moderator variable because its impact is to moderate the relationship between message content and the dependent variable, affect improvement. The DPF further suggested that cognitive reappraisal was a mediating or process variable while affect improvement is a dependent or outcome variable.

Variable types

- **Independent variable**
- **Dependent (outcome) variable**
- **Moderating variable**
- **Mediating (process) variable**

Manipulation

Effect

Cause

Listed in the box to the right are the main vocabulary used in experimental work. If any of this is confusing you should consult sources, several of which are included at the end of this presentation.

While I am able to make causal claims with the data I collected, oftentimes the only valid claim a researcher can make (or even wants to make) is of a correlational nature. *Post facto* research "begins with a measure of the dependent variable and then retroactively looks at preexisting subject independent variables and their possible influence on the dependent variable" (Sprinthall, 2003, p. 213). Moreover:

- Post facto research relies on classification IVs such as biological sex as opposed to manipulated IVs, those directly manipulated by the researcher.
- For example, we could be interested in how a person's typical communication style (e.g., dominant, dramatic, relaxed) is related to his/her listening style
- To study this we could have participants fill out questionnaires that assess their general communication and listening styles.
- We would then run one or more types of statistical analyses to ascertain if communication and listening styles are associated.
- Although post-facto research is useful, it is often misinterpreted.
- *The only warranted conclusion one can make with this type of research is that there is a relationship between the variables*. **"Correlation does not prove causation."**

- It is theory that helps the researcher determine if a particular cause-effect sequence makes more sense than other possible sequences. But it is experimental work that ultimately has to be conducted to adjudicate these possible sequences.

Oftentimes, but not always, when a researcher utilizes survey methods she is interested in making a population inference. A rather popular use of survey methods for listening research, however, includes scale validation research – work that seeks to test whether new measures of listening facets and constructs measure what they are supposed to measure (Bodie & Worthington, in press). Although beyond the scope of this paper, it is important for you to be able to determine the types of inferences researchers are trying to make when they report results in published form and for you to be able to think about these issues as you go about designing your own research studies.

After Data Are Collected

A second set of issues that we will address involve the data you collect as part of trying to make inferences of some sort. Putting aside the issue of whether your data come from a representative sample and whether they were collected via correlational or experimental means, we will now concern ourselves with how to go about presenting data in order to substantiate claims.
It is important to note first the role of data – to help you make principled arguments.

As Abelson (1995) put it,

> "good statistics involves principled argument that conveys an interesting and credible point. Some subjectivity in statistical presentations is unavoidable … Data analysis should not be pointlessly formal. It should make an interesting claim; it should tell a story that an informed audience will care about, and it should do so by intelligent interpretation of appropriate evidence from empirical measurements or observations" (p. 2).

I would add to this that it is not just the presentation of statistics that should conform to the notion of principled argument –the presentation of data in any form, whether numerical or not, should tell the most coherent and meaningful story possible (Keaton & Bodie, 2013).

Jan Bavelas (1995) made an excellent case for abandoning the dichotomy often forwarded that separates quantitative and qualitative research – research that does not use numbers from research that relies heavily on numbers to substantiate claims. As she states, "this dichotomy would require that all words such as 'many,' 'often,' 'several,' 'usually,' and so on, be expunged from the word processors of qualitative researchers" (p. 59). It also is true, however, that researchers who rely on methods such as in-depth interviewing or case study analysis often have different goals for their research compared to someone who primarily conducts laboratory-based or even field-based experimental work. Individuals employing in-depth interview or case study methods are typically interested in a particular case or perspective – they are trying to understand the meaning of a particular occurrence from the perspective of those involved. The goal may not be to draw large scale generalizations of the population or process kind.

Using Numbers to Argue for Claims

As was suggested earlier when we discussed process versus population inferences, two main goals of scientists are to understand the underlying mechanisms driving some relationship and to understand the size of an effect. To do this, researchers often rely on numbers when making claims. One set of numbers used in research is referred to as descriptive statistics – numbers that are solely a function of the data under question. These include measures of central tendency and measures of variability as well as measures of effect size.

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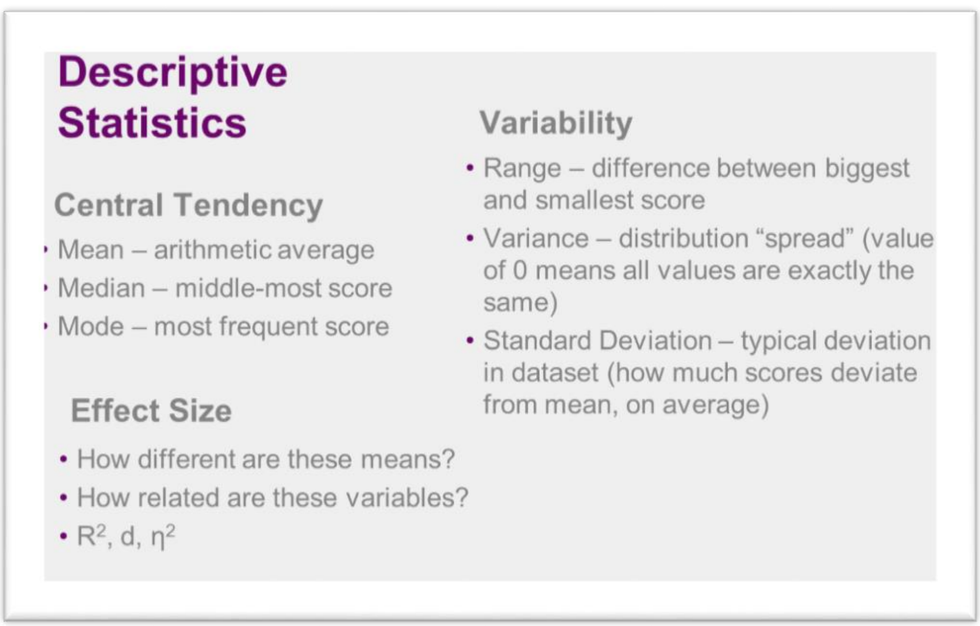
Measures of central tendency include the mean, median and mode. Variability statistics include the range, the variance, and standard deviation. Ultimately, by reporting these statistics, researchers are letting the audience know about the specifics of the data collected – who are they, on average, and how do they vary about that average.



A second set of numbers used in research is referred to as inferential statistics. The most common type of inferential statistics include t-tests, ANOVAs, multiple regression, and other techniques that fall under a class of statistics called the General Linear Model. Each of these techniques relies on what is called Null Hypothesis Significance Testing (NHST). NHST involves making a probability statement that the data obtained from a particular sample is a certain degree likely to have come from a population where the null hypothesis is exactly true. The null hypothesis is usually cast as the hypothesis of no difference or no association. In the case of supportive communication, for instance, a viable null hypothesis is that the person centeredness of message content has no effect on affect improvement (more specifically, that the slope coefficient of the linear trend of message PC is flat). Study after study suggests that this is not a tenable prediction – several datasets using a variety of samples drawn from a variety of populations suggest that person-centered message content has a consistent effect on affect improvement. In these studies, a statistically significant result would be reported, usually in the form of an italicized p followed by a number below .05 (e.g., $p = .02$). This means that there is less than a five percent chance that the sample data obtained comes from a population where there is absolutely no relationship between person-centered speech and affect improvement.

A statistically significant result does not mean that this result will be replicated 95 out of 100 times, and it does not give you any information about the probability of the truth of the null. The null still may be true, and other studies may not find this same result. Your results may still be a function of your particular sample – one can never be 100% absolutely sure about a generalized claim from sample to population. Basically, statistical inference is weighing, in a manner relevant to the substantive issues of an investigation, two kinds of error (Type I and Type II).

	"Reality"	
"Decision"	Null is true	Null is false
Null is true	Correct decision	Type II error (retaining a false null)
Null is false	Type I error (rejecting a true null)	Correct decision

The purpose of data analysis is to allow us to examine the extent to which the data provide corroboration for the theory-based answer to the research question. NHST allows you to rule out some alternative explanations, namely sampling error. So, when a researcher reports a statistically significant result, she is basically saying that sampling error is not a viable explanation for these results, though there is still a possibility that the null is true.

When, however, a result is reported as statistically significant, the tendency is to claim it as practically significant. Take for example these results reported in a study interested in the differential impact of presenting people with statistical evidence versus narrative based evidence on their attitudes.[2]

TABLE 1
Descriptive Statistics, Reported *F*-Values, and Associated Eta squares

Variable (SD)	$M_{exemplar}$ $n = 51$	$M_{statistical}$ $n = 44$	F(df)	η^2
Content Analysis				
Counterargument (1.2)	0.61	1.23	$F(1, 92) = 6.7$*	.06
Total number of responses (2.1)	4.73	5.79	$F(1, 92) = 6.6$*	.07
Agreement (1.9)	3.80	4.00	$F(1, 92) = .26$	.00
Miscellaneous (.4)	0.05	0.12	$F(1, 92) = .63$	.00
Measures				
Trustworthiness (3.7)	17.73	17.67	$F(1, 93) = .01$	.00
Expertise (2.7)	16.13	15.98	$F(1, 94) = .07$	.00
Persuasiveness (2.9)	10.59	11.04	$F(1, 94) = .55$	.00
Clarity (1.7)	7.98	7.42	$F(1, 92) = 2.4$	.02
Current Behavior (4.8)	18.11	18.80	$F(1, 93) = .49$	.00
Attitudes (2.6)	16.22	16.23		

*denotes a significant difference at the .05 level

[2] *Limon, M. S., & Kazoleas, D. C. (2004). A comparison of exemplar and statistical evidence in reducing counterarguments and responses to a message. Communication Research Reports, 21, 297-298. doi: 10.1080/08824090409359991. Used with permission.*

In one condition, participants were exposed to claims that were backed by numbers, while participants in the other condition were exposed to claims that were backed by personal stories. For a few dependent variables, the researchers found statistically significant differences – namely between these two conditions in the number of counterarguments participants produced and in the total number of responses they generated after exposure to the message. In particular, people who were exposed to the personal stories were less likely to think of arguments opposed to the evidence presented when compared to those exposed to the statistical arguments. But even so, the magnitude of this difference was rather small. As you can see in the far right hand column of the table, the eta-squared value for counterargument is .06 meaning that condition explains only 6% of the variance in counterarguing. Basically, the counterarguing scores vary about the mean. The authors are wanting to explain this variability – is there anything that can account for why scores vary. The answer is yes: condition can account for that, but not much of it. Another way to put this is that if all I knew about you was that you were in one condition or the other, I could guess whether your counterarguing would be higher or lower and be right approximately 6% of the time.

This 6% statistic is an effect size which we introduced briefly under descriptive statistics. If you were to conduct this study or similar studies several times, you would come up with a range of effect sizes – some studies would find .06, while others might find .13 or .01 or even zero. If you averaged all of those effect sizes, you would get a better idea of the true effect of exemplar versus statistical evidence, at least better than one study alone.

Such a process can be estimated by the use of confidence intervals, estimates of the approximate population value based on what is generated by a single sample. You can build a confidence interval around any descriptive statistic. Take for example the mean number of counterarguments generated by the exemplar and statistical groups, .61 and 1.23, respectively. I can build a 95% confidence interval around these means using standard equations and come up with values between .38 and .84 for the exemplar group and values between .80 and 1.66 for the statistical group. What this means is that I can be 95% sure that the population value of counterarguments for people like this exposed to exemplar evidence is between .38 and .84; ditto for statistical evidence.

If I am interested in this sort of population inference, these are important data. When you read public opinion poll results, the margin of error is basically a confidence interval for the mean attitude values reported – the mean is our best point estimate of the population, but it is just an estimate; there is still the potential that I am wrong. Similarly, there is still a chance that I am wrong in my inference about whether exemplar evidence confers and advantage in terms of getting people to counter-argue less with my position. As you can see from the confidence interval data, they overlap a bit – the values of .80, .81, .82, .83, and .84 are shared between the two conditions meaning that it is possible that people exposed to exemplar evidence produce a similar number of counterarguments as those exposed to statistical evidence.

What this suggests is that Peirce is right: Science in all its capacities is about settling opinions. The best settlement will come from data that is strong enough to help support inferences of primary interest. Knowing how to present those data and make a principled argument is thus of the utmost importance.

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