

對話世界頂尖學者**The Good and Bad of How Theory is Used in Communication Research**

Discussants: Dr. Byron Reeves and Dr. Mu-Jung Cho

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Abstract

Knowledge is accumulated in cycles of induction and deduction; that is, cycles of generating and verifying theories. Of paramount importance in evaluating a field of research and its progress is to determine the success of theories in guiding and guarding observation, explanation, and prediction. In this dialogue, Dr. Byron Reeves shares his views on current uses of theory in communication research and the influence of theory in advancing, but sometimes also hampering, scientific progress. He discusses his observations and his experiences, particularly in the area of media psychology, in conducting research about new technology, often from a bottom-up, data-driven perspective. The conversation concludes

with remarks about future directions in theory development and how that direction may affect careers in communication research.

Introduction to Dr. Byron Reeves

Dr. Byron Reeves is the Paul C. Edwards Professor of Communication at Stanford University. Dr. Reeves has a long history of experimental research on the psychological processing of media and resulting responses and effects. He has applied his research in the areas of speech dialogue systems, interactive games, advanced displays, social robots, and autonomous cars. Byron has worked at Microsoft Research and with several technology startups, and he has been involved with media policy at the Federal Trade Commission (FTC), Federal Communications Commission (FCC), US Congress, and the White House. He is an elected Fellow of the International Communication Association, and recipient of the ICA Fellows book award for *The Media Equation* (with Prof. Clifford Nass), and the Novim Foundation Epiphany Science and Society Award.

MJC: Mu-Jung Cho

BR: Byron Reeves

MJC : Before diving into the various aspects of how theory is used in studying media, would you share with me your views about the role and importance of theory in research, in the broadest sense? Why should we care about theory and how it is used?

BR : Developing a good theory is the goal, at least eventually, of most *all* research. Theories, in our case about media, are useful for description, explanation, and prediction. Theories tell us what is important to examine, the relationships of different concepts within a system of concepts, and they guide how we can use research results to change behavior in ways we value. Beyond this “textbook” definition, and in the latter part of my career, I think theory may be even more important in another way. Theories define *what it is* that we think is important to have a theory about. Our successes as theoreticians are critically determined by an ability to pick out one or a few things, among an almost infinite array of possible things to study, that are important and worthy of our time. That choice is not an empirical one but rather a *judgment* we develop as we become connoisseurs of what is interesting in social science.

Michael Polanyi begins his renowned book “Personal knowledge” with an interesting comment: if every question were of equal importance, then we would all be studying interstellar dust because that’s what there’s the most of. But instead, we make choices. So, I would say that one of the most important pieces of work in creating a theory is the choice of what to have a theory about. Do we study something out of well-informed curiosity? Should we develop a theory that might maximize opportunities to create policies or interventions that help people? And if so, which problems are the most important? By definition, these choices are personal judgments that reflect human values, as well as artistic or aesthetic appraisals. The choices are not impersonal or objective—and that’s an important and good thing.

MJC : So, theory is important because it serves as guard rails for organizing research, once we know what to study. But even before using theory in research, we should

care about it also because it helps define the human judgments that will guide research. That is one good part of how theory is used in research. How should we realize that good part of theory in our work? And are there any bad things that can happen using theory?

BR : One important part of making theory is that we rely, at least over time, on an inductive-deductive cycle of idea generation and idea verification. In that cycle, we observe the world and ground new ideas about how the world might work using observations that are, at least a little bit, unconstrained by prior ideas and biases. Then we can organize the observations into more systematic explanations and rely on empirical tests of hypotheses that a new theory suggests. An important point about induction is that it is a required step in theorizing. The beginning of ideas, however systematically expressed they eventually become, always start with mere observations and descriptions—as much as possible guided by literature and experience but also open to guesses and hunches. Unfortunately, too often we think of theorizing only as deduction. And that’s where problems can arise in research. An overemphasis on deduction may confine us to familiar hypotheses, making only incremental improvements on ideas already established. We forget to step back and ask ourselves: Where did that theory come from? Is the world that suggested the need for a particular theory the same one that we live in today? Without a theoretical beginning that is grounded in rich descriptions of the *current* world we would like to explain, theorizing will suffer. Scholars, however deductive their eventual work, need to walk around, observe what people are doing with media, or sometimes just measure a bunch of different things in the lab—even if they don’t really know which ones might be important. If we find something we think is interesting, we are of course, obliged to formalize ideas about what we’re observing and derive hypotheses to test. But to achieve the ultimate goal of creating a formal theory, we need to be grounded in observations of the things that we want to theorize about—grounded in what people are actually doing in the real world.

MJC : Coming back to the discussion about communication research and our interest in studying media theories, what is your view about how the field of communication research is using and developing theory?

BR : In communication research, deduction seems clearly the preferred contribution. Induction is not valued as much as it should be and not as much as it has been in the past. Let's say we are interested in studying how people use smartphones, and we would like to create a new theory about use of those devices. The inductive part of our research could begin with mere observation and description—we might describe what people are doing with their smartphones, even if we didn't yet know what about their use is important. We might then sift through the list of things we see people doing on smartphones, apply our judgments to decide which parts of the observations to create a theory about, and then zoom into the features of people's smartphone behavior that seem important. As reasonable as that strategy sounds, I believe it's actually rare these days. We're more likely to go directly on an existing theory to deduce research questions, maybe with some sort of small new twist that accommodates at least some of what might be new about the device. Induction gets short shrift, even though inductive thinking could importantly, and perhaps even radically, change what a new theory should try and explain.

MJC : What are the likely causes and the potential consequences of theorizing without sufficient inductive reasoning or relying too heavily on deductive reasoning in research? What caused the problems you described about theorizing in communication research and where might those problems lead us to?

BR : Say that you are an assistant professor in a university. You want to get tenure in communication and need to publish. The best advice I could give right now to achieve your professional goals is to find an extant theory, deduce and test hypotheses, and then use the results to verify a well-known theory, perhaps with some small repair that you might claim as your incremental contribution. That is what I think would be valued by most of the field at the moment. I often hear from journal editors and

reviewers, and from peers during tenure evaluations, the comment that someone is not theoretical because they are merely trying to describe what is going in media. Many now believe that the contributions that count the most for tenure need to be verification rather than generation of ideas. Descriptions, even rich ones, are risky, and the term “descriptive” is often used as a pejorative in scholarly evaluations.

I was recently looking at a catalog of different theories that have been used a lot in the *Journal of Communication* over the last decade (Walter et al., 2018). A reasonable publication strategy would be to deduce hypotheses from these theories and maybe try to add one more brick to whichever theory you choose. The negative consequence of using theory in this way is that it keeps us away from a close connection to what people are actually doing with media—especially whatever might be new this year—so that as scholars we become less certain that we are making theories about the things we care about the most.

MJC : Speaking from your perspective as a media psychologist, can you give specific examples of the “bad parts” of how theory might be used in communication research?

BR : In media psychology, we have all volunteered to be constrained by the media devices, services, channels, programs, software, and content that are being commercialized in the real world. We study psychological responses to what companies produce commercially. If, for example, we wanted to develop a new theory about how virtual reality works psychologically, the nature of our new theory should be considerably determined by new VR inventions, and their successes and failures in the marketplace. So, keeping up with technology development is critical. Otherwise, we could create interesting theories, but they might be irrelevant to what is happening in the world today.

Does anyone doubt that the media world is different today than it was even five years ago? New theories need new grounding, and especially in the media portion of research equations. Consider, for example, a new theory about the uses

and gratifications of media. Researchers emphasize the psychology behind uses and gratification: people have different motivations, and the different motivations will influence how they process information. That background has been applied well in past research, but problems arise when we want to argue that people use *brand new forms of media* in similar ways because of those motivations. We need to ask the question: What does it mean any more to use or be gratified by media? If we study uses and gratifications in relation to older media definitions—for example, situation comedy, crime, and action TV programs—we might develop a great theory, but it could be a theory about something that is increasingly irrelevant to modern media. Or we might just get the theory wrong, because by neglecting actual media use, we have not allowed ourselves to be constrained by what we volunteered to consider as media psychologists—namely, to study psychologically what is influential commercially.

We have a tougher problem than psychologists have. If we are studying emotion regulation in psychology, for example, we would have a broad choice of stimuli for our research. We could create emotional experiences in the lab in a number of different ways, many of them quite separate from emotions as experienced via media. But if we are studying emotion regulation via *media*, then we need to be interested in the ways that emotions are experienced in that particular domain. And importantly, we need to track how media are changing every year with respect to their ability to evoke emotions. Emotions and their regulation in the lightning-fast worlds of TikTok, Instagram, and YouTube are likely quite different than emotions experienced in TV programs and feature films. Because media are changing so much, the traditional theoretical work in communication may become less relevant.

MJC : If I were to summarize what you shared about the bad part of how theory is used in communication research, then I think an important lesson here is that we need to keep reminding ourselves about our goals in doing research. There are many factors that drive research agendas—intellectual, practical, or professional. When we prioritize factors that are less about our commitment to

understanding real-world media, we may be trapped in a cycle of adding small new bricks to an older theoretical façade. In extreme cases, an old theory could be changed so much that we barely recognize its original premise—as some have argued is true for research on media effects (Scheufele, 2000). Intended or not, eventually we get farther and farther away from what we are trying to explain.

Do you think those “bad parts” of using theory are influencing the status of communication as a scientific field of research?

BR : My first thought is that it probably does not jeopardize our standing. I think our field is emphasizing deduction because that is what everybody else in social science emphasizes. I also think, however, that this may be changing a little bit right now because of the scale and sophistication of data we can gather about technology use. For example, data science contributions to media psychology, and especially intensive longitudinal collections of digital trace data, offer sophisticated granular descriptions of what people are doing with media (e.g., Reeves et al., 2021). And those descriptions are becoming scientific contributions in and of themselves, and they are relatively independent of traditional theories. The hope, of course, is that the descriptions will eventually generate new theories.

I would also add, noting that some colleagues would strenuously disagree, that theories may actually be *less* useful in a world full of easily accessed data. One of the greatest values of theory is to guide observation and measurement. Theories can focus our attention, usefully pointing us toward that which *might* be most relevant and away from that which *might* be ignored. That could save a ton of time and effort. But if we can get all kinds of data and create all sorts of variables much more easily and quickly, and then get machines to analyze it all quickly, we have the ability to look everywhere almost all at once. This enables us to approach research from the bottom up more easily as we try and create new ideas about what is going on with media. Some people have even said that an “end of theory” is upon us (Anderson, 2008; Spinney, 2022).

MJC : The fact that media are changing so much and media technology is advancing so quickly also means that our field is well-positioned to benefit from a more bottom-up approach. And there is increasingly more funding and resources to support data-driven, inductive research. That could potentially change how people use theory in communication research. Also, with all the new technology available, we can now observe many things that were not previously observable. That would also change the theory-building process in our field. What are your views on that?

BR : One of the social science stories I have always been fascinated with is the development of speech recognition. Twenty-five years ago, when we were first trying to get computers to recognize speech, there were deduction-oriented theorists who said that we must first formulate a theory about how language works. They argued that we need a theory about how all the different pieces of language work together so that we can then deduce from that theory the principles that could be used to get technology to recognize and understand human speech. But there was also a group of computer scientists studying language who thought a theory might not be needed. Their proposal was that since we have easy access to increasingly fast and cheap computing, we could quickly try many possible solutions for translating words to speech or speech from one language to another—without really knowing theoretically why some solutions worked and others didn't. The computational linguists threw all their new computing power at this speech recognition problem—and I think they were the winners, at least with respect to their ability to build useful products, if not in their ability to satisfy everyone that they totally understood why their algorithms were successful.

Fifty years ago, I had a professor who would say that the most important value of a theory is to save you time in the library. A good theory will tell you all the places in the library that you do not have to visit, maybe saving years of work. Now, with the digitization of the library, and machine learning applied to larger

and more granular data sets, the margin of that saving has decreased. Are we only saving days and weeks of work with the help of a theory? Technology today has the potential to provide empirical foundations for formal theory that we have never had in communication research.

I think this may be the right time to more strongly argue that our theories need to be rooted in observations about the real world. Another example that supports this claim is our current work studying smartphone use. We have found that people often do not stay with the same content for more than about ten seconds. We could ask ourselves, why not generate a theory about those unbelievably fast changes in what people see and do with their screens? Absent the granular description about segment lengths, that question may not rise to the top of any research agenda. We started out with a lot of data, and we were able to let the data tell us what a new theory should be about.

MJC : Can you share a little bit more about your experience in using new technology to study media over the years?

BR : I think it is important to emphasize that we don't just need more data, but also better tools that cover more broadly and in greater detail what people are doing with media. Before there was a microscope, no one had a theory about cell division, simply because no one could see a cell. We need to be able to see things before it even occurs to us that we need to have a theory about how those things work. We need tools to see what was previously invisible. It could be a microscope for seeing smaller pieces or a telescope for seeing bigger pieces. In either case, however, it is less about building better versions of current techniques, like better surveys, and more about new methods for observations that new technology makes possible. In the smartphone research, for example, we can both zoom in and zoom out across several time scales, from seconds to years. This has enabled us to see what was previously invisible, like people switching within seconds between different content, or monthly changes in smartphone use that might be related to mental health. For the quick

switching, we don't yet have a great theory about fragmentation as a general concept or task switching in the context of these short fragments of media, but I think we now know where to look—enabled by new technology.

MJC : I think my experience is also that novel tools and observations fascinate researchers and are often what encourages them to think about new theories. But it often takes time for theorizing to catch up. This reminds me of a project from Maneesh Agrawala's lab that extracted structured data from a decade's worth of U.S. cable TV news. I think that project is revolutionary for many topics in mass media and news research, but it takes time for researchers to see it and think about what they are seeing. There are increasingly more cases like that now. What do you think are some communication research topics that are most in need of new observations and measurement? What are some good examples of applying new technology for theory building in communication research?

BR : I would say almost all topics could use better tools! Media are just so complex. Media includes almost everything: relationships, entertainment, money, sex, games, politics, health, and even algorithms that manage our cars and home irrigation systems. Media now encompass most of life. I think a big opportunity right now in media research is to keep trying to redefine media with respect to the disciplines that interest us. If we are interested in attention, memory, and emotion, then we should keep trying to define media, and especially the new pieces of media that are related to those concepts. Most communication theories will not explicitly be about Facebook, Instagram, or Twitter—those are names of companies and commercial products. Theories will also not primarily be about TV programs, movie streaming, or older commercial categories. It is the pieces of experience within those buckets that will most often be theoretically important. Having a theory about the differences between TikTok, Facebook, and Twitter, for example, may be less useful because they are commercial products and do not map well onto to psychological concepts. They may

be important if you are studying media organizations, but in terms of psychology, it might be much more important to theorize about other features of those products; for example, when they use words vs pictures, the emotional expressions on faces, or the the interface affordances that are used to quickly switch between radically different content. The goal would be to define commercial products in ways that are theoretically relevant.

MJC : Communication research is both driven by theory and application. Guided by our theories, we may sometimes be able to see what will happen in the future of media, but it seems like most of the time researchers are behind the trends. It is often that until some new social media gains tremendous popularity that we begin our studies. By employing a more balanced approach between induction and deduction, do you think we will be better at both explanation and prediction? And with new technology, should we focus more on explanation or prediction?

BR : For both goals, explanation and prediction, I think a very useful way to work on theory is to play with media. Get a TikTok account, buy a VR headset, subscribe to streaming services—spend time as a media consumer. By doing that, we may lessen the time to get an informed grounding of what is happening in the real world. (While we're having fun, we're also working on communication theory!). If we invited three excellent communication theorists to get TikTok accounts, make them meet once a week for five weeks, and in each of those meetings, have them spend time playing with TikTok and discussing their play—with no agenda—I think later theory discussions would be improved. What we often do now in those five meetings is review psychological literature on things like task switching, uses and gratifications, priming, and framing. That's necessary, but the review of psychological literature may be the easier part of theorizing. The harder part is linking that literature to interesting definitions and experiences of actual media.

Another great example of the value of this process comes from new interests in

media and mental health. Almost every prominent mental health scientific journal has an article (or ten!) that says smartphones can predict, diagnose, and treat a whole range of mental illnesses. Almost all these articles focus on the symptoms of mental health problem, for example, with respect to bipolar disorder, depression, or suicidal ideation. This is followed by often relatively brief discussions of how much people use their smartphones. The assumption is that more use, usually measures in the dosage of time spent, will predict the presence or absence of illness. This assumes that the dependent variable—the mental health variable—is the hard part. I think it may be just the opposite. Few approaches this problem by asking what exactly people are doing with smartphones, and then relate the media metrics to symptoms of illness. By starting with the media metrics, I believe the chances of successfully predicting health outcomes will improve, and perhaps dramatically.

MJC : Do you have anything that you think is important to add?

BR : What might the discussion of theory mean if you are trying to start an academic career? If the field emphasizes deduction, for example, then it might be important to do that type of theorizing early in one's career, and especially in the pre-tenure years. I also remember, however, the wording for the important tenure question that faculty at my university are required to write about when making a recommendation for tenure. That question is about the impact that a candidate's work has had on the field—what do we think differently about the candidate's topic as a result of their work. So, while there are risks in being a theory revolutionary, there is also a chance to have significant influence.

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傳播研究中理論運用的優點與限制

The Good and Bad of How Theory is Used in Communication Research

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摘要

知識是在歸納和演繹的循環中積累的；也就是理論的產生和驗證的循環。在評估一個研究領域及其進展時，至關重要的是判定理論在指導觀察、解釋和預測方面的成功。在本次對話中，Byron Reeves 博士分享了他對當今傳播研究如何運用理論的看法，以及理論在推動科學進步（但有時也會阻礙）方面的影響。他討論了他自身在新科技研究上——特別是媒體心理學領域——的觀察與經驗，這些研究經常是採取由下而上的、數據驅動的觀點。此次對談以關於理論發展的未來方向以及該方向如何影響傳播研究職涯的評論作為總結。

Byron Reeves 博士介紹

Byron Reeves 博士是史丹佛大學的 Paul C. Edwards 傳播學講座教授，Reeves 博士在媒體的心理學處理機制，以及由此產生的反應和效果的實驗研究上有長期的資歷。他將他的研究應用於語音對話系統、互動遊戲、高階顯示器、社交機器人和自動駕駛汽車等領域。Byron 曾在微軟研究院和一些科技新創公司工作，並參與聯邦貿易委員會（Federal Trade Commission, FTC）、聯邦通信委員會（Federal Communications Commission, FCC）、美國國會和白宮的媒體政策制定。他是國際傳播學會（International Communication Association, ICA）的會士，並以《The Media Equation》（與 Clifford Nass 教授合著）獲得國際傳播學會會士圖書獎。他也是 Novim Foundation Epiphany Science and Society Award 的獲獎者。

MJC: Mu-Jung Cho

BR: Byron Reeves

MJC : 在深入探討理論如何運用於研究媒體的各面向之前，您能否從最廣義的角度，分享您對理論在研究中的角色和重要性的看法？我們為什麼要關心理論以及如何運用它？

BR : 發展一個好的理論是大多數研究的目標，至少最終是這樣。在我們關於媒體的案例中，理論對於描述、解釋和預測是有用的，它告訴我們什麼是重要的，以及概念系統中不同概念之間的關係，並指導我們如何利用研究結果，以我們所重視的方式去改變行為。除了這個「教科書式」的定義，在我職業生涯的後期，我認為理論在另一個方面可能更加重要：它定義了我們認為重要的是什麼，以至於我們需要一個理論。我們作為理論家的成功取決於我們能否從近乎無限多可供研究的事物中，挑選出一件或幾件重要的、值得我們花時間研究的事物。這種選擇不是實證性的，而是我們在成為社會科學中有趣事物的鑑賞家的過程中所發展出的一種判斷力。

Michael Polanyi 在其知名著作《個人的知識》（Personal knowledge）的開頭中有一段有趣的評論：如果每個問題都同等重要，那麼我們都會研究星際塵埃，因為它數量最多（Polanyi, 2012）。但相反的，我們做出了選擇。因此，我想說，創建理論最重要的工作之一，是選擇要有一個什麼樣的理論。我們是否出於有知識基礎的好奇心而研究某些東西？我們是否應該發展一種理論，最大化地創造那些有助於人們的政策或干預措施的機會？如果是，哪些問題最重要？根據定義，這些選擇是反映人類價值的個人判斷，也是藝術或審美評價。這些選擇並非與個人無關或客觀的——這是有重要意義的事。

MJC : 所以，理論很重要是因為一旦我們知道要研究什麼，它就可以作為架構研究的護欄。但在將理論用於研究前，我們也應該在乎它，因為它有助於定義那些將指引研究的人類判斷，這是理論如何在研究中運用的一個優點。而我們該怎麼在研究中實現這個優點？以及，使用理論會發生什麼缺點嗎？

BR : 建立理論的一個重要部分是，隨著時間的推移，我們仰賴著觀點生成和驗證的這個歸納—演繹循環。在此循環中，我們觀察世界，並利用（至少一定程度上）不受先前想法和偏見約束的觀察結果，建立關於世界「可能」如何運作的新想法。然後，我們將觀察結果組織成更系統性的解釋，並依靠新理論提出的

假設實證檢驗。歸納法的一個重要觀點是，它是理論化的必要步驟。無論思想最終如何系統地被表達，思想仍總是從單純的觀察和描述開始，並盡可能多地以文獻和經驗為指導，但也接受猜測和直覺。不幸的是，我們常常認為理論只是一種演繹、推論，而這正是研究中可能出現的問題，過分強調演繹可能會使我們局限於熟悉的假設，只對已經確立的觀點進行漸進式的改善。我們忘了退一步問自己：這個理論是從哪來的？對某個特定理論有所需求的世界是否與我們今天生活的世界相同？如果一個理論的起點，沒有以我們想要解釋的當前世界的豐富描述為基礎，理論化將受到負面影響。學者們，無論他們的產出最終如何地高度演繹化，他們都需要四處看看，觀察人們在用媒體做什麼，或者偶爾只是在實驗室裡測量一堆不同的東西（即使他們不知道哪些可能很重要）。如果我們發現一些自認有趣的東西，我們當然有義務形式化對觀察到的事物的想法，並推導出假設進行驗證。但要實現創建形式理論的最終目標，我們需要立足於對我們想要理論化的事物的觀察，並以人們在現實世界中實際做的事情為基礎。

MJC：回到關於傳播研究的討論，和我們對研究媒體理論的興趣，您對傳播研究領域如何使用和發展理論有何看法？

BR：在傳播研究中，演繹顯然是被偏好的貢獻。歸納法沒有得到應有的重視，也沒有像過去那樣受到重視。假設我們對研究人們如何使用智慧型手機感興趣，並且想創建一個關於使用智慧型手機的新理論，研究的歸納部分可以從單純的觀察和描述開始，我們可能會描述人們在使用智慧型手機做什麼（即使還不知道這些使用有什麼重要性）。然後，我們可以將觀察到的使用行為進行篩選，運用判斷來決定觀察結果中的哪些部分可以創建一個理論，最後聚焦放大人們使用行為中似乎重要的特徵。這個策略聽起來很合理，但我相信現今實際上很少見。我們更有可能直接在現有的理論上推導出研究問題，也許包含一些新的小變化（能適應手機的一些新特性）。歸納法受到了冷落，儘管歸納法的思維可以極大地，甚至是根本地改變一個新理論應該嘗試和解釋的內容。

MJC：在沒有足夠的歸納推理或過度依賴研究中的演繹推理的情況下，進行理論化的可能原因和潛在後果是什麼？又是什麼導致了您所描述的，關於傳播研究理論

化的問題？這些問題可能將我們引向何方？

BR： 假設你是一所大學的助理教授，你想獲得傳播學方面的終身教職，需要發表文章。為了實現你的職業目標，我現在能給出的最好建議是找到一個現有的理論，推導和驗證假設，然後用結果來證實一個著名的理論，也許會進行一些能被你聲稱為漸進式貢獻的小修正。這就是我認為目前我們領域的大多數人都會重視的。我經常從期刊編輯和審稿人，以及一起進行終身教職評審的同行那裡聽到這樣的評論：有些人的研究不是理論上的，因為他們只是試圖描述媒體中發生的事情。現在許多人認為，評定終身教職時最重要的貢獻應該是驗證，而不是想法的產生。描述，即使是豐富的描述，也是有風險的，「描述性」一詞在學術評價中經常被用作貶義詞。

我最近注意到一份過去十年在《傳播學刊》 (*Journal of Communication*) 中經常被使用的各式各樣理論的彙整 (Walter et al., 2018)。一個合理的出版策略是從這些理論中推導出假設，並嘗試在你所選擇的任何理論中添加一磚一瓦。以這種方式運用理論的負面後果是，它使我們難以密切地連結到人們使用媒體的實際行為（尤其是今年可能出現的任何新行為），因此，作為學者，我們越來越不確定自己是否在為我們最關心的事物建立理論。

MJC： 從您作為一個媒體心理學家的角度來看，您能舉出一些具體的例子，說明理論在傳播研究中的使用可能產生的「缺點」嗎？

BR： 在媒體心理學中，我們都自願受到現實世界中正在商業化的媒體設備、服務、管道、程序、軟體和內容的約束。我們研究人們對於商業產品的心理反應。例如，如果想發展一種關於虛擬現實如何在心理上運作的新理論，那麼我們新理論的性質應該很大程度上取決於新的 VR 發明，及其在市場上的成敗。所以，緊跟技術發展的步伐是至關重要的，不然，我們可能創造出有趣但卻或許與當今世界正在發生的事情毫不相關的理論。

有人懷疑過今天的媒體世界與五年前不同嗎？新的理論需要新的基礎，尤其是在權衡研究的媒體部分時。例如，考慮一個關於媒體的使用和滿足的新理論，研究人員強調使用和滿足背後的心理學：人們有不同的動機，不同的動機會影響他們處理訊息的方式。這一背景在過去的研究中得到了很好的應用，

但當我們想要論證，人們由於這些動機，而以類似的方式使用「全新形式的媒體」時，問題就出現了。我們需要問：使用媒體或被媒體所滿足還意味著什麼？如果我們研究與舊媒體定義相關的使用和滿足，例如情景喜劇、犯罪和動作電視節目，我們可能會發展出一個很好的理論，但它可能是一個與現代媒體越來越無關的理論。或者，我們根本構建了一個錯誤的理論：因為忽視了實際的媒體使用，我們沒有讓自己被約束於身為媒體心理學家所應顧慮的東西，也就是針對什麼是具有商業影響力的事物，進行心理學的研究。

我們的問題比心理學家更棘手。例如，如果我們研究心理學中的情緒調節，我們的研究可以有廣泛的刺激選擇，我們可以通過多種不同的方式在實驗室中創造情緒體驗，其中許多與通過媒體體驗的情緒截然不同。但是，如果我們通過媒體研究情緒調節，那麼我們需要對情緒在特定領域的體驗方式感興趣。重要的是，我們需要隨時間追蹤媒體在引發情緒方面的能力之變化，在TikTok、Instagram和YouTube等快速發展的世界中的情緒及其調節可能與電視節目和電影中的情緒經驗有很大不同。由於媒體變化如此之大，傳播學的傳統理論工作可能變得不那麼相關。

MJC：如果我要總結您分享的有關理論如何用於傳播研究的缺點，那麼我認為這裡的一個重要教訓是我們需要不斷提醒自己我們在研究中的目標。有許多因素推動了研究進展——知識性的、實務性的或職業性的，當我們優先考慮那些與我們對理解現實世界媒體的承諾無關的因素時，我們可能會陷入在舊的理論外牆上添加小的新發現的循環。在極端的情況下，舊理論可能會發生很大變化，以至於我們幾乎認不出它的最初前提，正如一些人所認為的那樣，關於媒體效果的研究也是如此（Scheufele，2000）。不管是否有意，我們最終離試圖解釋的東西越來越遠。您認為那些使用理論的「缺點」正在影響傳播學作為一個科學研究領域的地位嗎？

BR：我的第一個想法是，這可能不會危及我們的地位，我認為我們的領域強調演繹，因為這是社會科學中其他領域也強調的。然而，我也認為，由於我們可以收集到大規模和成熟的有關科技使用的數據，這一點現在可能會發生一些變化。例如，數據科學對媒體心理學的貢獻，尤其是數位足跡數據的密集時序收

集，提供了對人們用媒體所做事物的精細描述（例如，Reeves et al., 2021）。這些描述本身正成為科學貢獻，並且相對獨立於傳統理論，當然，希望這些描述最終會產生新的理論。

我還想補充一點（也注意到有些同僚可能會強烈反對），在一個充滿容易獲得的數據的世界裡，理論實際上可能越來越不有用。理論的最大價值之一是指導觀察和測量，它可以聚焦我們的關注，有效地將我們引向可能最相關的領域，而不是或許該被忽視的領域，這可以節省大量的時間和精力。但是，如果我們能夠更輕易、更快速地獲得各種數據並創建各種變量，然後讓機器快速分析所有這些數據和變量，我們就有能力幾乎一次地查看所有地方。這使我們在嘗試和創造關於媒體正在發生什麼的新想法時，能夠更容易地自下而上進行研究。有些人甚至曾說，「理論的終結」即將來臨（Anderson, 2008; Spinney, 2022）。

MJC：媒體變化如此之大，媒體科技發展如此之快這一事實，也意味著我們的領域處於有利地位，可以從更加自下而上的方法中受益，並且有越來越多的資金和資源來支持數據驅動的歸納研究，這可能會改變人們在傳播研究中使用理論的方式。此外，隨著眾多新科技的出現，現在我們可以觀察到許多以前無法觀察到的事物，這也將改變我們領域的理論建構過程，您對此有何看法？

BR：我一直著迷的社會科學故事之一是語音辨識的發展。25年前，當我們第一次試圖讓電腦進行語音辨識時，一些以演繹為導向的理論家說，我們必須先制定一個關於語言如何運作的理論。他們認為，我們需要一個關於所有語言不同部分如何協同工作的理論，令我們得以從該理論中推導出可以讓科技識別和理解人類語音的規則。但也有一群研究語言的電腦科學家認為我們可能不需要理論。他們的建議是，由於我們可以方便地使用越來越快速和廉價的計算，我們可以快速嘗試許多可能的解決方案，將單詞翻譯成語音，或將語音從一種語言翻譯成另一種語言，而不必從理論上真正了解為什麼某些解決方案有效，而其他解決方案無效。計算語言學家把他們所有的新計算能力都投入到了這個語音識別問題上——我認為他們是成功的，僅就他們有能力製造有用的產品而言，而非因為他們有能力讓人相信他們真正地理解為何算法能成功。

五十年前，我有一位教授會說，理論最重要的價值是省下你在圖書館的時間。一個好的理論會告訴你圖書館中所有你不必去的地方，也許可以省下幾年的工作。現在，隨著圖書館的數位化，以及機器學習被應用於更大、更緻密的資料集，省下的邊際時間已經減少。我們如今在理論的幫助下也許只節省了數天至數週的工作時間吧？現今的科技有可能為某些我們在傳播研究中從未有過的形式理論提供實證基礎。

我認為現在可能是更有力地證明我們的理論需要紮根於對現實世界的觀察的時候了。支持這一主張的另一個例子是我們目前研究智慧型手機使用的研究。我們發現，通常人們在同一內容上停留的時間通常不會超過 10 秒。我們可以問自己，為何不創建一個理論，去關注人們在觀看與使用螢幕上難以置信的快速轉換現象？如果沒有關於每個使用段落時間長短的詳細描述，這個問題可能不會成為任何研究進程的首要問題。我們從大量數據開始，並讓這些數據告訴我們新理論應該要關於什麼。

MJC：能否多分享一下您這些年來使用新科技研究媒體的經驗？

BR：我認為，重要的是要強調，我們不僅需要更多的數據，還需要更好的工具，能更廣泛、更詳細地涵蓋人們使用媒體的情況。在有顯微鏡之前，沒有人有關於細胞分裂的理論，僅僅是因為沒有人能夠看到細胞。我們需要能夠看到事物，然後才會想到我們需要有關於這些事物是如何運作的理論。因此我們需要工具來看到以前看不見的東西。它可以是顯微鏡，用來觀察較小的片段，也可以是望遠鏡，用來觀察較大的。然而，無論是哪種情況，它都更少地是關於構建當前技術的更好形式（例如更好的調查問卷），而更多地是關於應用新科技使觀察成為可能的新方法。舉例來說，在我們的智慧型手機研究中，我們可以在幾秒到幾年的時間尺度上放大和縮小，這讓我們看到了以前看不見的東西，比如人們在幾秒鐘內切換不同的內容，或者可能與心理健康有關的智慧型手機使用的每月變化。對於快速切換，我們還沒有一個很好的理論，能將碎片化看作一個通用的概念，或者將任務切換放在媒體碎片的背景中理解，但我認為新科技使我們知道該看向何方。

MJC：我認為我的經驗也是如此，新奇的工具和觀察吸引了研究人員，並且常常是

鼓勵他們思考新理論的原因，但理論化往往需要時間來跟上。這讓我想起了 Maneesh Agrawala 的實驗室的一個項目，該項目從過去十年間的美國有線電視新聞中提取結構化數據。我認為該項目對大眾媒體和新聞研究中的許多主題來說都是革命性的，但研究人員需要時間來看見它和思考他們所看到的。現在這種情況越來越多，您認為最需要新的觀察和測量的一些傳播研究主題是什麼？在傳播研究中應用新技術進行理論建設有哪些好例子？

BR： 我想說幾乎所有的主題都可以使用更好的工具！媒體是如此複雜，幾乎包括一切：人際關係、娛樂、金錢、性、遊戲、政治、健康，甚至管理汽車和家用灌溉系統的演算法。媒體現在涵蓋了生活的大部分。我認為目前媒體研究的一個重大機遇是根據我們感興趣的學科不斷地嘗試重新定義媒體。如果我們對注意力、記憶和情緒感興趣，那麼我們應該繼續努力定義媒體，尤其是與這些概念相關的新的媒體片段。大多數傳播理論都不會明確地涉及 Facebook、Instagram 或 Twitter（這些都是公司和商業產品的名稱），理論也不會主要是關於電視節目、電影串流或其他較舊的商業類別。在理論上很重要的往往是在那些名目和類別中的媒體經驗片段。例如，對 TikTok、Facebook 和 Twitter 之間的差異進行理論分析可能不太有用，因為它們是商業產品，不能很好地反映心理學概念。如果你是研究媒體組織，它們可能很重要，但從心理學角度來說，對這些產品的其他特徵進行理論化可能更加重要；舉例來說，像是這些媒體何時會使用文字或圖片、臉上的情緒表達，或用於在完全不同的內容之間快速切換的界面符擔性。我們的目標應是以理論上相關的方式去定義商業產品。

MJC： 傳播研究是由理論和應用一同驅動的。在我們的理論指導下，我們有時可能能夠看到媒體的未來會發生什麼，但似乎大多時候研究者都是落後於發展趨勢的，通常是直到一些新的社交媒體獲得巨大的普及度後，我們才開始研究。通過採用在歸納和演繹之間取得平衡的方法，您認為我們在解釋和預測方面會更好嗎？隨著新科技的發展，我們應該更加注重解釋或預測嗎？

BR： 對於解釋和預測這兩個目標，我認為研究理論的一個非常有用的方法是多玩玩媒體。取得 TikTok 帳戶、購買 VR 眼鏡、訂閱串流媒體服務、花時間成為媒體消費者，藉由這樣做，我們可以更快地掌握媒體在真實世界中所發生的事情，

並將之作為知識基礎（在我們玩得開心的同時，也在研究傳播理論！）。如果我們邀請了三位優秀的傳播理論家來取得 TikTok 帳戶，讓他們每週開會一次，為期五週。在每次開會中，讓他們花時間玩 TikTok，在沒有議程的情況下討論他們的玩樂經驗，我認為之後的理論討論會有所進步。我們現在在這五次會議上經常做的是回顧有關任務轉換、使用和滿足、促發和框架等方面的心理學文獻。這是必要的，但心理學文獻的回顧可能是理論化中較容易的部分，更難的是將這些文獻與實際媒體的有趣定義和經驗聯繫起來。

這一過程的價值有另一個很好的例子是，對媒體和心理健康的新興趣。幾乎每一本著名的心理健康科學期刊都有一篇（或十篇！）文章聲稱智慧型手機可以預測、診斷和治療各種精神疾病。幾乎所有這些文章都主要關注精神健康問題的症狀，例如，關於雙相情緒障礙（bipolar disorder）、憂鬱症或自殺意念（suicidal ideation）。在這之後經常是相對簡短的，對人們使用智慧型手機的程度的討論。這些文章的假設是，更多的手機使用（通常以所花費的時間來衡量）將預測疾病的存在與否。這一假設預期依變項——也就是心理健康變項——是較難處理的部分。而我認為這可能恰恰相反。很少有人處理這一研究問題的方式，是對人們使用智慧型手機的具體情況提出問題，並將媒體指標與疾病症狀聯繫起來。我相信，從媒體指標著手，成功預測健康結果的機會會提高，甚至可能會顯著提高。

MJC：還有什麼您認為需要補充的重要事項嗎？

BR：如果你想開始學術生涯，理論的討論可能意味著什麼？例如，如果你的領域強調演繹，那麼在一個人的職業生涯早期，尤其是在任終身職之前的幾年，進行那種類型的理論研究可能很重要。然而我也記得，在我所任教大學，教員在做終身教職推薦時所必須回答的、最重要的終身教職評鑑問題是如何措詞的。這個問題是關於候選人的工作成果對他的領域產生的影響：由於他的工作成果，我們對該候選人的研究主題產生了什麼不同的看法？。因此，雖然作為理論革命者有風險，但也有機會產生重大影響。

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