

Thoughts on Theory -Reflections from Senior and Junior Researchers-

Jung E. Ha-Brookshire[†] • Seong Eun “William” Kim^{*}

Dept. of Textile and Apparel Management, University of Missouri

^{*}Eskenazi School of Art, Architect + Design, Indiana University

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Abstract

What is a theory, and what is its role in our research? Although we use them every day as researchers or as individuals, many of us consider applying theories challenging and view creating a new theory as even more daunting. In this manuscript, we discuss the definition of the term “theory,” why theories are important, how to develop a new theory, and ways of evaluating a theory’s usefulness. The authors, a seasoned researcher and a novice researcher, share their unique experiences and perspectives on theories, offering insights into their personal journeys of theory development and evaluation, along with valuable takeaways learned throughout the process. We hope that this manuscript offers unique insights into how we apply, build, and critique theories in both academic and practical contexts. By sharing our experiences, we aim to provide guidance for readers, helping them embark on their own journeys to develop new theories and assess the theories of others effectively.

Key words: Theory, Development, Evaluation

I. Introduction

Writing this manuscript has been on my bucket list of things I wanted to do for a long time. Many of us think theories are something difficult to understand, that only certain special groups of people can create new theories, and/or theories are not something we use every day. Therefore, theories are to be studied by serious scholars; for only graduate students or researchers to use; and that, for us to be researchers, we must find theories (new theories are even better) and build research topics that are new and popular. Are all these statements true? As I reflect on my time as a scholar and educator, I realize none of these statements are true. In fact, good theories are clever and insightful. I use good theories all the time. I used them for my

scholarship. I used them for teaching and mentorship. I also used them for my life in general—both professional and personal. I find myself using theories all the time and for almost all the decisions I make. I even developed and am developing new theories to better explain life events and phenomena I observe and experience. Some of the people who know me will tell you, I have many theories!

Then, how can one develop new theories? It could be daunting for any scholar, especially beginning scholars and graduate students, to even think about the possibility of developing new theories. I hope this manuscript will dispel some misconceptions about theories in general and help scholars explore their own theory development and evaluation journeys. Certainly, my way is not the only way to do so. I am sure other theorists will offer different perspectives. With that in mind, I hope the readers will be open-minded to gain

[†]Corresponding author

E-mail: habrookshirej@missouri.edu

insights from a person who was in the industry for over eight years and in academia for over 20 years. In this manuscript, I describe how real-life events helped create new theories.

A junior researcher is part of this manuscript because I believe both my insights and his experiences as a beginning researcher will help readers navigate the theory development and evaluation journey from both the macro/more experienced level and the micro/less experienced level. On that note, this manuscript is written in two parts. One part is what I narrate to share the experiences and lessons I gained over the years. The other part is where a Junior Researcher shares his experiences in his own journey while learning from me. Ultimately, by learning from both perspectives, I hope the readers can follow the journeys that both of us took and find ways of their own. The next section starts with a question, “What is a theory?”

II. What is a Theory?

1. From the Senior Researcher

What is a theory then? People define a theory in many ways. Hunt (2002, p. 7) defines it as “a systematically related set of statements, including some law-like generalizations, that is empirically testable.” That sounds very scholarly. In fact, I am not even sure if anyone really understands what this means. Meanwhile, Merriam-Webster (n.d.) defines a theory as “a hypothesis assumed for the sake of argument or investigation.” This definition seems easier to understand than Hunt’s version. The key here is that a theory is an assumed statement! Hunt would say they are hypothetical statements that are systematically related. Either way, theories are essentially someone’s assumptions. Some assumptions are empirically testable (therefore, researchers use them), and others are not (so all of us, even if we are not researchers, can use them). With this, then, let me ask you a question. Can you assume something and develop statements about your assumptions? If you said yes to this question, then you are capable of developing your own theories!

I explain a theory as a set of assumed statements that attempt to explain what, how, why, and when things occur in our world. Or, let me make it even easier by using a metaphor. To me, a theory can be explained as a tool (such as a fishnet you might use when fishing) to catch specific fish that you want (the expectation of successful outcomes). Let’s say if you want to catch a large fish, then you will need a net that has small enough holes (at least smaller than the size of the fish) to catch your fish but large enough holes to let out other smaller fish (that you do not want to catch). The appropriate size of the hole in the fishnet will increase the success rate of your fishing. Therefore, if you are a fisherman/woman, you want the right size and the right kind of fishnet specifically designed for the fish that you want to catch. Otherwise, all the work you put in to catch the fish will be useless.

To be successful in fishing (and research), you must consider two key concepts when choosing the tools or nets. The first concept is validity. As a researcher, you want a valid theory for your problem-solving; if you are a fisherman/woman, you need an appropriate tool (or the tool designed specifically for your fish) for your fishing goal. Certainly, you do not want to bring a net that is used to catch shrimp when your goal is to catch sardines. Similarly, as a researcher, you want to use the right kind of tool or valid theory when the objective is to understand a certain phenomenon in the world. In other words, are you using the right theoretical framework for the questions you are trying to solve? Are you using valid theories to solve the problems in your hands? Are you trying to catch fish with a net designed for catching your specific kind of fish?

You also want the right quality of fishnet for a consistent and successful outcome when fishing. You want a net that you can trust, that will hold the weight, amount, and volume of fish that you want to catch every time you use it—that is, you want a reliable fishnet that you can trust to use every time, and you can expect consistent outcomes. Similarly, as a researcher, you want a theory that you can rely on. In other words, are you using tools that are reliable? Do you expect to see consistent results when you use the tools used in this

manuscript? Does the theory have reliability?

One thing to consider here is that the fishnet's validity and reliability can be judged subjectively sometimes, depending on the limitations and/or contexts you, as a fisherman/woman, are facing. Sometimes, the tool you have might not be ideal but it is good enough to catch some fish. Other times, the fishnet needs to be precise because you want adult male fish 10-15 inches long only, not young female fish. Therefore, even if you use the right kind of tool, you may need additional scrutiny for inspection to decide what to catch and what to let go. It is the fisherman/woman's decision for the specific purpose of that fishing project. In other words, one net can be said to be good enough in some cases but not good at all in other cases, depending on your specific goals. You, as a researcher, face similar challenges. Not all theories are perfect for the problem you want to solve. Each theory offers different utility (or usefulness, I call it), and it is up to you to decide which problem you want to solve while letting go of other problems the theory in use may or may not be able to catch. So, a theory might work perfectly for one researcher's problem, yet it may not have the same utility for another research problem. That is one of the reasons why quantitative researchers justify their use of a certain theory and the context of its use, while qualitative researchers may use that theory for different purposes, such as the exploration of new topics or ideas. In this light, one cannot or should not judge qualitative theories using the rubrics created to assess the quantitative theories. So, it calls for careful consideration when using or adopting others' theories into your problem-solving endeavors.

2. From the Junior Researcher

During my academic experiences, I thought that theories were previous studies that would guarantee (how little I knew!) my research results and findings. This might be true in that proper use of a theory could help me validate my study findings/results (that is why I cited previous papers that offer similar outcomes). Therefore, I thought I must use widely used theories to

build my research framework so I would have successful research outcomes with statistically significant positive results. As a result, I never really defined what a theory is for myself or for my own research questions. I just assumed that constructing a research model in a certain way by referring to others' research was good enough. I also thought that citing more studies would make my study appear more in-depth. Thus, to be honest, I lacked confidence in why my research model should be the one I designed, and therefore, I just made my models just more complex. I simply thought my complex models were robust enough because those who studied before me said they were, and if they said my model was not then I was ready to explore building other models. Even in doing so, I never had conviction in my proposed theories or models.

However, my approach to studying theory or model development changed completely after learning what a theory is, how to use it, and what it means to use a theory in my studies. Now, I first organize my research questions (in this case, think about what kinds of fish are out there, and decide which fish to catch) and look for theories (fishnets) that can test my assumptions (or catch the fish I wanted to catch). Then, I conduct various thought experiments regarding how each theory (tool) can address my research questions (fish catching) and help construct my research hypotheses (imagine if this tool will work for my fish). In the end, I found the theory (tool) that has the best utility (usefulness) for solving my research questions (fish). During this process, I also know what to let go of. We cannot catch all the fish we want in one fishing trip anyway. This process helped me feel confident with my research models because I know that my models solve the problems that I want to solve (what and how to catch my fish with the tools I selected).

III. Why is Theory Important?

1. From the Senior Researcher

The fishnet explanation clearly illustrates why theories are important for any researcher. Without the

fishnet, the fisherman/woman would be blindly heading out to the ocean, hoping to catch whatever fish is out there. This means a fisherman/woman would have no idea what to catch nor how to catch but still goes out to the ocean hoping for a successful outcome. Randomly and occasionally, this strategy can work. Indeed, there are many fish out there, and one could catch something, or anything even, with no specific plans. However, this is neither a realistic nor efficient way to catch fish. Almost all fishermen/women have specific goals and estimate the expected returns and the costs for any fishing trip. For such goals, they prepare with proper tools, gear, and bate. They anticipate the possible quantity of the catch, the demand for fuel, and personnel, and then estimate the overall bottom line of their fishing trip. Just like these fishermen/women, researchers cannot go out blindly and think that they will be able to solve a certain problem. Researchers need to focus on specific issues and problems to solve, anticipate outcomes and costs (or limitations), and what that finding means for our lives, society, and the literature. In this light, a theory can be said to be the key to problem-solving and, therefore, provide implications for future problems. Without the theory, the findings provide no implications that can be applied to anyone's current or future problems. Without the theory, the research findings could be purely coincidental and random, with little or no utility to solve future problems. In other words, even if a researcher found "something" randomly without a theory, the findings would have no explanatory power for any future or other similar problems because such research would provide neither validity nor reliability of the findings.

This does not mean that all researchers must use the latest tools, however. In fact, many fishermen/women prefer older and proven-to-be-reliable tools. These nets have been working fine and have proven to catch the fish they want. Why should they change? After all, they spent a lot of time finding the most appropriate fishnet to begin with. Once they find the right net, many fishermen/women do not want to change their tools unless new tools are guaranteed to work and work even better. They might repair and fix the net, but

once the utility of the fishnet is proven, many of them will use the same tools over and over. If a theory is useful and helpful, there is no reason why researchers cannot use the older, proven-to-be-reliable theories. The problems that the researchers are trying to solve might be new as our society evolves, but some theories have ever-lasting utility—such as the theory of supply and demand (a theory that says that when the supply is high, the price goes down; and when the demand is high, the price increases, in most cases or for normal goods). In fact, this supply and demand relationship has been proven to work for so many years and decades that we now call it the "Law of Supply and Demand." The term, law, emphasizes that this relationship is extremely robust, and it is anticipated that the relationship will always happen no matter what. Therefore, no one will fault people who use the tools that are proven to work effectively even though this theory is old, not the latest.

Rather, new theories may pose more questions or vulnerabilities as to whether they will work in certain contexts or settings. Therefore, new theories call for a higher level of scrutiny, testing, and investigation. Given this, I caution anyone who wants to use new theories that are yet to be tested enough—that is, researchers who use new theories may or may not get the findings that they hypothesize, and that would create either distorted or inconsistent findings or no findings at all. Meanwhile, the new theories must be tested empirically over and over to show various utilities and limitations so that the theories will continue to evolve and become useful for future problem-solving.

Also, complicated theories are not always useful. After all, most people want tools that are simple and easy to use. What good would it be if the fishnet is so complex and difficult to use that the fisherman/woman gives up using it? In most cases, the simpler the explanation is, the better off it is. The philosophers call this the "Occam's razor" principle (Domingos, 1999). Basically, it is the problem-solving principle that the simpler the explanations, the more effective they are. Therefore, I encourage you not to make your theories complex and difficult to understand or use. Make them

simple, yet powerful and insightful. After all, a theory is there to solve our problems, not create more.

So, carefully review and select the appropriate theories that you want to use for the specific problems that you want to solve. The most important thing here is knowing the problem that you want to solve, not the tools that you want to use. Tools can become outdated and obsolete if they are not deemed useful. However, if you focus on the problems, then you might find existing tools and modify them. You might even want to make new tools for that problem. So, the more time you spend on determining the problems you want to solve, the more chances you can use theories appropriately and even develop new theories.

2. From the Junior Researcher

During my studies, the biggest change in my understanding of theory was the way I see the importance of theory in my studies, specifically the way to assess the novelty in research. Before I learned about theory, I thought theories were important, especially new theories, to make my study novel. At that time, I thought the novelty meant following the latest trends in research topics, exploring consumer experiences and perceptions of the newest technologies, and using complicated models and new theories. As a result, I often felt anxious, wondering if I could find new technologies or new theories. If I was not interested in the latest technologies or if I could not find new theories, then I thought I was not a good researcher, and my research findings had no important implications.

This manuscript states that theory is important because it solves our research problems. In problem-solving, the most important thing is to define my problems and find the right tools to solve them. Whether the theory is novel or outdated does not matter in this selection process. After all, what we really must think about is finding new problems, not new theories and technologies. For example, artificial intelligence (AI) might be a new technology, but consumers' adoption behavior of AI might not be new. In this case, any other technology adoption model would be helpful. However,

AI might generate a new problem for fashion designers, who will now experience the confusion of intellectual property rights given that AI can create designs automatically. In this case, I might add new variables, merge other existing theories, or apply my study context to other theories, all of which means that I am building a new theory for my new problems. Thinking in this way, I no longer wade through numerous previous studies just to find a novel and rarely used theory. Rather, I now focus on what are problems in the phenomena and what is the gap I need to fill. Then, I find proper theories to help me solve the problems. If my problem can be solved and explained with a simpler model, even better.

I now know that theory is also important because it gives explanatory power to research problems and, thereby, research findings can have implications for future studies. If I find new problems and solve them with a proper theory, I can explain why the problems happen, and ultimately, the findings become meaningful, giving implications to the literature. When the results I obtain from using a "new" theory that I thought previously may not solve my problems at the problem, and cannot explain why the problems happen, then the implications would simply be that "the problems not solved" with no meaningful implications. This perspective has shifted my habit of blaming myself for getting meaningless findings and weak implications, thinking it was because I was not using new theories. This is why it is so important to have a clear understanding of why theory is important. I find that I constantly remind myself that a theory is just a tool to fill gaps in the literature and to best address these gaps.

IV. How Do You Develop a Theory?

1. From the Senior Researcher

Then, how can one develop a new theory? How do we create a new fishnet? First, let's think about the individuals who want to catch fish. Some might be experienced, and others are not. Yet, these individuals have a specific goal to catch a certain kind of fish, and they

know the behavior, tendencies, and characteristics of the specific type of fish they want to catch. With such knowledge, they devise a proper net. They think about the size of the fish to decide the right size of the holes in the net. They think about the fish's group behavior—that is, if fish move in schools and move together in large quantities, they might make a large net to catch many fish at one time. If the fish tend to travel alone, the fisherman/woman might devise a hook or fishing pole to catch one fish. If the fish are active at night, then they might want to add a reflective coating to the net. This example shows that one must know a lot about the fish themselves to find ways to catch them. Even with the proper net or tool, they will still need to correct and/or modify the fishing tools for a specific circumstance, depending on weather situations, seasonal temperature changes, and so on.

Similarly, researchers must have a great deal of knowledge about the problem they want to solve. The researcher must be observant about the problem itself and the environments or contexts of the problem. The researcher must also be aware of which theory worked and which ones did not work in the past for that problem. If the theory worked, how and with what limitation? If it did not work, then why not? Was it a sampling issue? Was it a measurement issue? Was it a context issue? This knowledge can then be used to modify existing theories and/or create new theories. The more you use them, the better off you will be able to use, modify, and change them.

There are many ways to develop new theories. In fact, Jaccard and Jacoby (2020) show 26 different ways to think about new ideas. Out of those, some of my favorite ways are: (a) analyzing my own experiences, (b) reframing the problem in terms of the opposite, (c) applying the continual why and what, and (d) conducting a thought experiment. I will describe each method in more detail next. Let's continue to pretend that we are fishermen/women. Based on my own experiences as a fisherwoman, I should know what would work or not when embarking on a new fishing trip, depending on which fish and how many of them I want to catch. I would gather all the tools I believe would be

useful for my purposes and hope that I would indeed be able to catch them. However, past experiences also make me think that certain tools need to be tweaked or repaired, and/or I might think that I would need a whole new tool. My personal experiences in fishing allow me to select the right tools but also critique the existing tools and, therefore, provide an opportunity to make a new tool.

As a researcher who is seeking to encourage sustainable business practices in the global supply chain, I used to use various theories developed by others to explain sustainable supply chain management strategies. Triple bottom line theory is one of them. I also consulted and used various strategic management theories, buyer-supplier relationship theories, resource-based theories of the firms, firm capability theories, and so on. As some of these theories were helpful, I thought that none of the findings I found using these theories provided good solutions to other questions I have: How can we make a firm or an entire supply chain truly sustainable? Why are there so many different degrees of sustainability performance in the corporate world?

One day, while I was presenting and teaching about sustainable global supply chain management of textile and apparel products, a senior executive of one of the largest retailers in the US commented, "Ma'am, sustainability sounds great, but we are here to make a buck to satisfy our shareholders' best interests, not to save the world. We are not sure how sustainability can be one of our company's key objectives." When this was stated, I could not come up with a satisfying reply, only "because it is the right thing to do." This was not a logical or theoretical answer. Rather, I was appealing to his emotion—basically I begged him to accept my position! I finished that session, but I wondered if my answer was correct. So, this personal experience gave me an opportunity to analyze my question from the "reframing the problem in terms of the opposite" perspective. Now, instead of trying to find out what makes the company and the entire supply chain sustainable, I want to know why these companies do not engage in sustainability. From the perspective of the

executive described above, a sustainability goal contradicts their financial goals—that is, these two goals of sustainability and profit may not be oppositional rather than complementary.

I held onto my questions of the “why” of this contradicting relationship between sustainability and profit continuously. One day, I attended a social science conference in which I had opportunities to learn from sociologists, philosophers, lawyers, and more, and that is where I was able to solve my questions of the why! Immediately, I went into “thought experiment” mode. If the law says that a company is a legal person, and the philosophers say that all humans have fundamental duties to fulfill to keep the society and communities going, then wouldn’t companies have fundamental duties to fulfill to keep the society and communities going—i.e., sustainability? Does that mean that sustainability is not a goal, rather it is a fundamental condition within which all companies must operate to be functional and productive members of society? In other words, we individuals do not violate our fundamental duties (such as honesty and not hurting others) to make money. Therefore, shouldn’t companies fulfill their fundamental duties (such as not lying and not killing) while making money? – Here now I am experimenting with my thoughts. I question, I hypothesize, I connect, and I create a new assumption. These thought experiments eventually led me to create a new theory called “Moral Responsibility of Corporate Sustainability,” which was published in the *Journal of Business Ethics* in 2017 (Ha-Brookshire, 2017). In this case of theory development, I used four heuristics of theory development: (1) using my own experiences (talking with an executive of a large retailer), (2) reframing the problem in terms of the opposite (by trying to understand his point of view, not mine), (3) applying the continual why (because I couldn’t answer his question), and (4) conducting a thought experiment (by connecting legal and philosophy literature and applying it to the problems I was trying to solve).

Similar methods can be used when mending/correcting/extending existing tools/theories. Your experiences in using a theory will inform you that some

parts of the tool work well but others do not. Especially when your models or hypotheses were not supported, this provides an opportunity to look at why not, figure out what was not captured and why not, look at the tool from different perspectives, like from those of a fish, and experiment with the new ideas in your head. This process will help you develop new modified models. It might even push you to create a new theory. Please try that. Go back to all the research you have ever done or read all the papers you have ever read. Instead of focusing on what “worked” or was supported, find the relationships or hypotheses that did not work and were not supported. Then, start asking why not? How? What will fix that? This will give you new ideas to create new theories.

2. From the Junior Researcher

As a 30-year-old beginning researcher, I just never imagined that I could even attempt to develop my own theory. Many people who have done research for 20 or 30 years still do not propose their own theories. I thought that attempting to create my own theory at my age seemed impossible and even arrogant. I thought I was not qualified to develop and address new theories, and I was simply not ready yet. I do not think my hesitation is wrong. I understand that whatever I propose might not work at all, or other existing theories might explain the phenomenon better, given my lack of experience. But despite this hesitation, I have practiced and followed the senior researcher’s advice.

To my surprise, I am happy to say that I now feel confident in constructing new theoretical frameworks for my research and even attempting to develop new theories. I am combining existing theories and adding my own variables to expand the scope of the existing theories. Sometimes (actually, most of the time), I combine two to three theories from various disciplines into one research model and then test them through empirical research to obtain answers to the research questions. If the new model is statistically supported, I can say that I developed a combination of working theories, explaining the phenomenon I am inves-

tigating. Throughout this process, I focus on the problems, not the theory itself. I believe all of these activities helped me create a new theory to solve a new problem.

My favorite ways to build new models are through personal experiences, thought experiments and the questions of why. Some of my friends are fashion designers and they often share their latest designs with me. Many of their designs were new and interesting, but a good amount of new designs seemed to look alike with each other or similar to those from other designers. I wondered how such designs can be marketed as new or original designs legally, and whether general consumers can tell apart similar designs from the original. In addition, I wondered why some consumers never allow any similarities in fashion designs, but others even enjoy them. When I had these questions in my mind, I started seeking the fishnet to catch my fish in the legal literature. First, I was required to have a better understanding of U.S. laws related to intellectual property rights, specifically copyrights. So, I started learning about such laws by taking a class at the Law School. I read many legal documents, courtroom decisions, and related literature. Finally, I could find a useful theory from the legal literature regarding how consumers process their legal knowledge regarding illegal copyright appropriation in their consumption.

Even though I found the theory in the legal literature, I thought the legal theory itself could not catch my fish entirely (I felt I caught the tail of the fish). This is because consumers' decision-making mechanisms are complex as they simultaneously evaluate the multiple utilities of the product, and the design similarity is one of these utilities. So, I explored the philosophy literature, finding a useful theory that helps explain consumers' decision-making processes. Throughout the exploration, I continuously performed "thought experiments." What would cause the confusion in consumers' minds in terms of design piracy? What if I change the placement of design logos? What if I change the shape of a design? What if I change the pattern of a design? My thought experiments helped me develop various fashion design variations, and I then

tested them to check the level of consumer confusion. As I analyzed the findings, I questioned why this variation caused confusion but not others and why this confusion can be detrimental to some and beneficial to others. My questions of why then ultimately helped me create theoretical and hypothesis models to solve my questions (Kim & Ha-Brookshire, 2024). I enjoyed this process very much, and I intend to take this approach as I build new research programs in the future.

V. What is a Good and a Bad Theory?

1. From the Senior Researcher

Of course, not all new ideas are good ideas. Not all new theories are good theories, and not all old theories are bad theories. Then, how do we know what a good theory is or what a bad theory is? How can one evaluate a new theory that is being proposed? I understand people have different preferences. Some fishermen/women like brand-new tools, while others like historic and old ones that are widely tested. I think both have their own places in this world. As much as we love using computers and new technologies, we also appreciate antiques and history. To me, when a new theory is proposed or a new thought is being developed, I look at "the possibilities for new discussion/tests/research/discussions" because of this new idea. In other words, when a new fishing tool is presented to me, I consider, "Would this work for this type of fish?" and if I can say, "Yes, let me try it," then it is considered a good theory to me. It offers the possibility to help solve new problems. I call this an expected utility of the theory. If this new theory offers new or different possibilities to solve certain problems or explain certain phenomena, then it is good. For me to want to try, however, the new theory must have a certain set of characteristics as discussed next.

First, the new fishnet should look sturdy and seem to be well-constructed so that I can trust that it will work for my fishing trip. In the research setting, the theory needs to be well-constructed, or logically con-

sistent. The net is constructed with fibers and wires, and our theories are constructed with concepts and statements that elicit the relationships between the concepts. These statements are systematically and logically constructed so that the statements make sense to me. For example, "The sky is getting dark with more clouds, so I theorize that rain will fall soon because clouds possess moisture." This statement shows the possible relationship between the dark sky, clouds, moisture, and rain. These variables are explained systematically and logically by the phrases of "if-then" as well as "because." On the other hand, a statement like "The sky is getting dark with more clouds, so I theorize that I will get mail soon because the mailman runs when it rains" has variables that sound like they are systematic, since the statement includes "if-then" as well as "because," but the logic in the relationship between the rain and mailman running is not sound. The mailman may run whether it rains or not; and the mailman may deliver the mail soon whether it rains or not. Therefore, the second example is not a good theory.

Second, a theory must have clear definitions of the constructs and provide a clear scope of the phenomenon or problem that it addresses. For example, one might want to theorize that "the more time you spend with a person, that person will love you more." The concept of love is so big, complex, and context-driven that it is almost impossible to say that love refers to one concept or idea. In this statement, the concept of love could mean the affection between a child and mother or that between lovers. The way we assess and measure love in this statement might be different depending on the context and the scope of the research. Furthermore, this statement is not clear whether it refers to a simple amount of time or the quality of time they spend. Indeed, we all know that the quantity of time does not consistently correlate with the quality of time.

In the last phrase, "love you more," we face bigger challenges. How does one really know that another person loves you more? Would it require a number of hugs, a number of times saying I love you, or the amount of money or resources one gives to another to measure the "more"? Depending on the context, none

of these indicators might work or all of these would work. In terms of a mother-child relationship, a child's love for the mother might mean respect, desire, or enjoyment. Therefore, to be more precise, the original theoretical statement can be revised to "the more quality time a mother spends with her child, the child will respect his or her mother more." This statement is not perfect. However, at least it provides a more focused scope and context for this theory. At least we know the "respect" dimension of the love this theory is referring to, and we now know that it is talking about a child in this statement. This exercise can continue until the researchers can clarify which person's love they are interested in investigating, which dimension of love, and what type of time they want to measure, etc., in order to properly state what the researchers want to theorize for the concept and relationships of the problem they refer to. Clear definitions and scope of the theory are critical for a researcher to assess its expected utility.

Good theoretical statements provide opportunities for the researchers to operationalize the statements and develop hypothesis tests. In the case above, the researcher would operationalize the concepts of the child, respect, quality time, and mother to make this theoretical statement into a hypothesis statement. Theoretical statements are the statements that explain the possible (or assumed) relationships between the constructs or concepts. Hypothesis statements are those that show what relationships are to be tested using specific variables that measure the theoretical concepts. Suppose that a researcher wants to examine a 3-5-year-old child to measure the number of times the child complies with the mother's requests (i.e., how many times during the day the child listens to the mother's requests and follows) as a way of measuring the child's respect for mother. The researcher then wants to measure the incidents from the moment the child wakes up until he or she goes to bed. The researcher might want to measure this for multiple days and average them. In this case, the researcher uses the number of times the child complied with the mother's requests as a proxy to measure the child's degree of respect for the mother. The researcher also needs to de-

cide how to measure “quality time.” It won’t be just superficial time that both are together in the same room. The researcher might decide that quality time will be measured by times when both child and mother actively interact and dialogue, such as time playing games together or paying attention to each other without disruptions. Finally, the mother might refer to not only the biological mother, but also the adoptive mother or the stepmother. The researcher might say this study is only for biological mothers or stepmothers. The researchers might say this study is for all mothers who live with their child every day. Some biological mothers might not live with the child every day, or vice versa. All these decisions must be made (or hypothesized) to test this theoretical statement. Although the theoretical statement of “the more quality time a mother spends with her child, the child will respect his or her mother more” is not perfect, it provided enough conceptual framework and scope so that the researcher could develop a hypothetical statement to operationalize it and test it. In this case, the hypothesis statement could be “the higher the number of hours mother and child are together, for a mother who lives with the child every day and actively interacts and dialogues with the child, the more likely the child will comply to the mother’s requests.” Certainly, this hypothetical statement is different from the theoretical statement. After all, the hypothetical statement is the statement that is ready to be tested or operationalized for a test. This hypothesis will provide insights into the overall relationship in a mother-child relationship, and others may create different hypotheses and test them. In this light, we can say that this one theoretical statement may offer many different hypotheses and many opportunities to contextualize and operationalize in all sorts of different settings and contents, and the research will continue to test whether this theoretical statement holds true in various contexts and circumstances.

Finally, a new theory should be original. In other words, a new theory should provide new insights, new ways to solve the problem, or new or different concepts. The degree of “new” or “original” may differ from person to person and cause arguments about

whether the proposed idea is new or not. To me, as long as it offers new opportunities to conduct new research, I consider it has utility and is, therefore, worth pursuing. It may or may not solve the problem I want to solve in the way I want to, but it offers an option, and so one day someone might take the opportunity to use it. That is new and original enough for me. It might not look new today, but it could be considered new years later. It might look new to me, but it might not be new to others in different circumstances. Since these are subjective measures, I rely on the possible utilities of the new theory in this regard—again, does this new theory make me want to try and use it for any possible future problem-solving?

2. From the Junior Researcher

Before learning from the senior researcher, I thought a good theory was a novel one—those that were not widely used, not well-known, and seemed to involve complicated research models. However, I no longer believe that a freshly baked theory is necessarily a good one. As a newly minted PhD, I now believe that a good theory is one that can effectively “catch my fish.” No matter how fancy and complicated the model is, my fish might be so large that even a coarse net might be able to catch it. In that case, an investment in a finer fishnet is wasteful. If my fish is very small, despite how fine my fishnet might be, I might not be able to catch it at all. To determine whether a theory is good or not, I now know clearly that I need to understand the characteristics of my fish in the first place—how big my fish is, where it is staying, and whether it requires a coarse or finely woven fishnet. Then, I can choose a good fishnet to throw into the deep sea, where my fish swims.

While learning this “fishing method,” I realized that I had been confusing the concepts of “right or wrong” with those of “good or bad.” As I mentioned earlier, a good theory is one that can effectively catch my fish. If the fishnet I chose failed to catch the fish, it doesn’t mean the net was bad; rather, it means I chose the wrong net. That is, the theories themselves are not at

fault; it is just that I chose the wrong theory for my problem. Before, I thought that using a more complex and novel fishnet meant I conducted the “right research with a good theory.” Conversely, I believed that using an old and simple fishnet meant I conducted “bad research with the wrong theory.” I now see that it does not matter what theories I use if I can solve the problems that I want to solve.

Another confusion I had was related to whether my new research model was “worth it or not.” I often had this question when faced with assessing the quality of quantitative vs. qualitative research. Given my preference for quantitative models, I did not know how to assess qualitative research. Once I learned that “as long as it offers new opportunities to conduct new re-

search,” and if “a theory has clear definitions of the constructs and provides a clear scope of the phenomenon or problem that it addresses,” then whether it is quantitative or qualitative does not matter. Even if it is a qualitative theory, as long as there are clear definitions and scopes for possible variables within the conceptual model, a hypothesis model can be constructed using existing quantitative variables, effectively solving my research questions. I now realize that I held such biases because I did not fully understand “what a theory is” and “what makes a good theory.” Once I realized why I needed theories, I was able to erase all these confusions. <Table 1> shows the perspectives on theories from the senior and the junior researcher’s perspectives.

Table 1. Perspectives on theories from senior and junior researchers

Perspectives	Senior researcher	Junior researcher	
		Before Ph.D.	After Ph.D.
What is a theory?	[Theory as a tool] Theory is a set of assumed statements that attempt to explain what, how, why, and when things occur in our world. It can be used as a tool (such as a fishnet you might use when fishing) to catch specific fish that you want (the expectation of successful outcomes).	[Citing previous studies] Theory is previous research that would guarantee and validate my future research results and findings. [Uncertain about my model] Therefore, I never had conviction in my proposed theories or models.	[Theory as a tool] I now look for the best possible tool to solve my research questions. [Becoming confident in my model] I became more confident that my theories and models would solve my problems in some way.
Why is theory important?	[To gain explanatory power] Without the theory, the findings could be coincidental and random, with little utility to solve future problems because such research would provide neither validity nor reliability of the findings. [Implications of your research findings] Without the theory, the findings provide no implications that can be applied to anyone’s current or future problems.	[To make my study novel and important] I thought <i>novelty (a novel study)</i> meant following the latest trends in research topics (i.e., new technologies) and using complicated models with new theories, not focusing on the problems I want to solve.	[To gain explanatory power and implications] I now seek to find new <i>problems</i> , not new theories. Good theories elevate the explanatory power of our research findings for future problem-solving and have the findings have important implications.
How do you develop a theory?	[Research, take action, and reflect on yourself] Researchers must have a great deal of knowledge about the problem they want to solve. The researcher must also be aware of which theory worked and which ones did not work in the past for that problem. The more you use theories, the better you will be able to use, modify, and change them.	[Never thought before] I just never imagined that I could even attempt to develop my own theory. I thought that attempting to create my own theory at my age seemed impossible.	[It’s not a big deal] I developed a combination of working theories, explaining the phenomenon I was investigating. I am now ready to propose a new theory to solve a new problem.

Table 1. Continued

Perspectives	Senior researcher	Junior researcher	
		Before Ph.D.	After Ph.D.
What is a good and a bad theory?	[A good theory: New possibilities of problem-solving] A good theory offers the possibility to help solve new problems. It is called the <i>expected utility</i> of the theory. If this new theory offers new or different possibilities to solve certain problems or explain certain phenomena, then it is good (or has expected utility).	[Confusion: Wrong theory] I realized that I had been confused between the concepts of “right or wrong” and those of “good or bad.” I believed that using an old and simple fishnet meant I conducted “bad research with the wrong theory.”	[A good theory: An effective problem-solver] I now believe that a good theory is one that can effectively “catch my fish.” No matter how fancy and complicated the model is, my fish might be so large that even a coarse net might be able to catch it.
Stages	Theory ideation	Theory development	Theory evaluations
To-do List	Look for the problems that require new solutions. Use various heuristics, including using our own experiences, reframing the problem, applying the continual why, shifting the unit of analysis, or making the opposite assumptions.	Conduct thought experiments. Ask “what if” questions continuously until you offer new solutions.	Check if the new theory (1) offers expected utilities, (2) has clear definitions and a scope, (3) operationalizable, and (4) original.

VI. Conclusions

As we conclude this manuscript, we want the readers to know that we all are capable of using and developing theories. As humans, we theorize (assume) things all the time. “I bet it will rain soon since the sky is dark from clouds.” This theory allows us to take specific actions, such as avoiding being outside or looking for an umbrella. A good theory provides possible solutions to face a potential problem that might occur. Sometimes, my actions are helpful because the rain indeed comes. However, we also face many times when the rain does not come. My actions of going inside or finding an umbrella are not harmful, although they could be inconvenient. Despite this lack of certainty, we still grab an umbrella when the sky gets dark. We have faith and trust in this theory, and we change our behavior. We use this theory as much as we can.

Some theories have a lot more severe consequences if the theory is wrong. Statisticians use the term Type I error – or a false positive – to describe a situation where the hypothesis was supported when, in fact, it was not true to reality. In the case of the rain example, the lack of rain (despite all the symptoms of rainy days) does not really harm anyone. Yes, we might have

experienced minor inconvenience by finding an umbrella or canceling our outdoor picnic. However, the overall damage of a false positive is not severe. However, a high degree of Type I error might have huge implications if a new drug is found to have statistically significant positive impacts when, in fact, the drug does not have a positive effect on patients. Therefore, the job of researchers is to repeatedly test the theory in various settings, populations, and contexts to be sure that the theory indeed works and that the statistical results are not false positive. For that reason, some research needs tighter Type I errors (like p-value less than .01), and others can tolerate more generous Type I errors (like p-value between .05 to .1). Therefore, purely following anything below p-value .05 may not be helpful for many researchers—especially social scientists. We need to think about the possible false positive and then decide what threshold we can tolerate if a false positive occurs. This example shows that both everyday people and researchers can use theories for various reasons and offer different implications and contributions.

Through this manuscript, our wish is that the readers realize that each of us uses theories every day, not just researchers with PhDs, and that every theory has

its own usefulness, limitations, and consequences. More importantly, when existing theories do not work or are not effective, we all create new solutions, tools, and explanations. That is, we all are capable of creating new theories as researchers. We hope the readers will look at the problems they are focusing on today and start thinking about making a new theory. We would be delighted to hear about their new theories.

1. Practical approaches to the theory development

As we conclude this manuscript, we would like to present practical steps for theory ideation, theory development, and theory evaluation.

First, look for the problems that are new in society, new in our lives, new in literature, and new in solutions for theory ideation. Not the latest trend or hottest topic. New problems that many people suffer and need answers to. Jaccard and Jacoby (2020) show 26 different ways to think about new ideas. So, try each of them to see if you can come up with new problems, even if the phenomenon might be the same. Some of the popular heuristics are: using our own experiences, reframing the problem in terms of the opposite, applying the continual why, shifting the unit of analysis, or making the opposite assumptions.

Second, let's conduct thought experiments for theory development. This is where our creative problem-solving will shine. Try as "what if" questions. These questions will be the best tools to develop new relationships or theories with new and analytical thoughts. We might create graphs, figures, and tables to explain the problems that we are trying to solve.

Third, once we have the proposed theories, evaluate them to see if they: (1) provide the possibilities for new discussion/tests/research/discussions, (2) have clear definitions of the constructs and provide a clear scope of the phenomenon or problem that they address, (3) offer opportunities for future researchers to operationalize the statements and develop hypothesis tests, and (4) are original providing new possible utilities.

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JHB wrote the entire manuscript except the sections of "From the Junior Researcher", and SEWK wrote the entire sections of "From the Junior Researcher." SEWK also created the <Table 1>.

7. Authors' information

Jung E. Ha-Brookshire

Dept. of Textile and Apparel Management, University of Missouri, Professor and Department Chair

Seong Eun "William" Kim

Eskenazi School of Art, Architect + Design, Indiana University, Assistant Professor

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Appendix

Senior Researcher. I am a professor and department chair at the University of Missouri. I have been mentoring and teaching PhD students over 17 years, and many of them are now tenured faculty members in major institutions in the US and China. This manuscript reflects my time as a researcher as well as a mentor and

teacher of graduate students and junior scholars. I hope this manuscript will help those whose goal is to pursue new knowledge and to make meaningful contributions to the literature and body of knowledge.

Junior Researcher. I am an assistant professor at Indiana University-Bloomington. I have just begun my journey to develop my own theories at the time of writing this manuscript. I am a recent graduate of University of Missouri. I hope this manuscript will help other junior researchers like me pursue their own theory development journeys.