How to publish & review a computational model

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How to publish and review a computational model

Computational modeling has revolutionized cognitive and behavioral science, enabling researchers to gain unique insights into the complex processes that are responsible for generating observed behavior (Farrell & Lewandowsky, 2018; Wilson & Collins, 2019). A number of commentators have noted that computational modeling has the potential to do the same within the organizational sciences (e.g., Salas, Kozlowski, & Chen, 2017). However, because the practice of computational modeling is still within its infancy in our field, authors and reviewers often lack clear guidance about the different ways in which computational modeling papers can be written, and the criteria by which they should be assessed. The objective of this chapter is to address this gap.

The chapter is organized as follows. We first provide an overview of the practice of computational modeling. In this section, we identify the steps involved in carrying out a computational modeling project, and describe the different ways in which the results of these projects are published in the top journals within our field. We then discuss issues that authors and reviewers need to consider when publishing and reviewing these types of papers. A key focus of our discussion is on the similarities and differences between a traditional paper and a computational modeling paper, so that authors and reviewers can think about how they adjust their strategy and criteria to take advantage of the benefits that computational modeling provides for our field. A summary of our recommendations is provided in Table 1.

The practice of computational modeling

The lifecycle of a computational model

Figure 1 illustrates the life-cycle of a computational model. Computational modeling projects typically start with either theory or data. A theory describes the phenomenon of interest, and identifies the mechanisms that explain how that phenomenon emerges, or how it evolves over time. The researcher may develop a new theory from scratch, or build off an existing theory. In some cases, the researcher may start with data rather than theory. For example, the researcher may identify a set of empirical phenomena that have not yet been explained by existing theory, or a set of related phenomena that are explained by separate theories. Alternatively, the researcher may run a study in order to identify the empirical phenomena that need to be explained. Once they have identified the empirical phenomena, they then develop a theory to explain those phenomena.

Once the researcher has a theory, or set of theories, they specify that theory in the form of a computational model. A computational model consists of a set of equations that describe the process by which a system (e.g., a person, group or organization) produces observable output (e.g., behavior or performance). These equations are implemented as computer code. In many cases, the equations are dynamic, describing how the variables within the model change over time. The model will also often include internal variables that are not directly observable, but which play an explanatory role in the theory. For example, Kanfer and Ackerman's (1989) theory of ability and motivation explains observed changes in performance using concepts such as skill and effort. These are internal cognitive states that cannot be measured independently of the outcome (behavior or performance), but which are necessary in order to explain the changes that are observed. Many theories within the organizational sciences include variables of this type.

Once the researcher has a working model, they can run simulations to evaluate the behavior of the model. This is done by running the model under a variety of conditions, using a range of parameter settings. This allows the researcher to generate testable predictions by identifying the way that the model responds to variables of interest, and examining the patterns that emerge under different parameter settings. If the researcher has competing models, they can identify the conditions under which the models make qualitatively different predictions.

The model or models are tested by comparing them to data. If there is an existing set of empirical phenomena within the literature, the researcher may examine whether the model or models can reproduce those phenomena. In this case, the researcher would focus on qualitative fit, assessing whether the model can reproduce the pattern of results that has previously been observed. Alternatively, the researcher may collect new data, and examine the correspondence between the model and data. If the researcher has a set of models that make qualitatively different predictions, then the researcher can test those predictions to see which model is supported. In some cases, the researcher may go one step further and fit the model(s) to the data. Model fitting allows the researcher to conduct quantitative model comparisons, and estimate the parameters of the model from the data (Wilson & Collins, 2019). Quantitative model comparison is used to assess which model, out of a set of alternatives, provides the best account for the data. Quantitative model comparison can be useful when models make similar qualitative predictions, or where there are large numbers of competing models that need to be assessed. Parameter estimation can be used to draw inferences about the underlying psychological processes, or the role of individual differences. For example, parameter estimation can be used to test hypotheses regarding the effects of individual and environmental variables on the parameters of the model. This approach provides a test of construct validity of a model (Arnold, Bröder, & Bayen, 2015).

Once the researcher has tested the model(s), they close the loop by revisiting the theory and model(s). If the model fits the data, then this provides support for the theory. The level of support that is provided depends on the breadth of empirical phenomena that the model can account for, and the extent to which models derived from competing theories are able to account for the same phenomena. A model receives "credit" to the extent that it can account for findings that cannot be explained by other models, or explains the findings in a more parsimonious manner (Farrell & Lewandowsky, 2018). In many cases, the model will need to be revised to account for unexpected findings. This can be done by adding new mechanisms to the model, or revising existing mechanisms within the model, and showing that the changes are necessary to account for the findings. This is part of the natural development of a model. Over time, the explanatory power of a theory and/or model will grow as it is extended to account for a broader range of empirical phenomena, and survives tests against competing theories and models.

While confirmation plays an important role in the ongoing development of a theory or model, falsification is equally important. In many cases, there will be clear evidence that a particular model cannot account for the data, and should be discarded. If the researcher tries a series of models that are derived from the same theory, and none of those models can account for the data, then this casts doubt on the underlying theory. Ultimately, the fate of any theory is to be discarded, as it is superseded by a new generation of theories that are better able to account for empirical phenomena. One of the reasons why computational modeling is so useful is because it allows models and theories to be more easily vetted, which is essential if the field is to make progress (Vancouver, Wang, & Li, 2018).

Computational modeling in the organizational sciences

In order to identify the different ways in which computational models are used within the organizational sciences, and the ways that they are published, we conducted a Psycinfo search for papers published in relevant journals with 'computational model' as a search term[1]. Relevant journals included the *Journal of Applied Psychology* (JAP), *Organizational Behavior and Human Decision Processes* (OBHDP), *Academy of Management Journal* (AMJ), *Academy of Management Review* (AMR), *Personnel Psychology* (PP), *Journal of Management* (JoM), *Organization Science* (OS) and *Administrative Science Quarterly* (ASQ). Twenty-five papers were

[1] The search was conducted for papers including 'computational model' in any field. Papers that used statistical models, as opposed to computational models, were excluded.

identified (see Table 1). Two of these papers were published prior to 2000, five were published between 2000 and 2009, and 18 have been published since 2010. The most common outlets were *JAP* (11 papers) and *OS* (6 papers).

As can be seen in Table 1, computational models have been applied to a broad range of phenomena. Examples include organizational learning and adaptation (8 papers), self-regulation (7 papers), and team processes (4 processes). The one thing that all of the phenomena have in common is that they are dynamic. In each case, the authors have used computational modeling as a means of understanding dynamic processes, such as the process by which organizations adapt to changes in their environment (Levinthal & Marino, 2015), people pursue competing goals (e.g., Vancouver, Weinhardt, & Schmidt, 2010), or collective knowledge emerges in teams (Grand, Braun, Kuljanin, Kozlowski, & Chao, 2016). There is considerable variability in the elements of the computational modeling cycle that individual papers focus on. Some papers focus on theory development. These papers develop a computational model of a theory or phenomenon, and run a simulation study to examine the behavior of the model (e.g., Gibbons, 2004). These papers use simulation to explore the logical consequences of a theory. Sometimes, researchers will incorporate a limited form of theory testing within a theory development paper. They do this by evaluating whether the model can account for established empirical effects (e.g., Chen, Croson, Elfenbein, & Posen, 2018). Other papers focus on both theory development and theory testing. One approach is to use a simulation study to generate predictions, and then run an empirical study to test those predictions (e.g., Grand et al., 2016). Another approach is to start with an empirical study to identify the phenomena that need to be explained, develop one or more models to account for those phenomena, and fit those models to the data in order to test competing explanations (e.g., Ballard, Vancouver, & Neal, 2018), or examine the effects of individual and environmental variables on the parameters of the model (e.g., Gee, Neal, & Vancouver, 2018). The key point to note for anyone wanting to publish or review a computational model is that there is no one 'best' way to write a computational modeling paper.

When writing the paper, one of the key decisions that needs to be made concerns the scope. In many cases, it will make sense to focus on specific elements of the computational modeling cycle, rather than trying to include all elements of the cycle within the one paper. The risk with trying to include all elements of the cycle within the one paper is that it may overwhelm the reader, particularly if the model is new or complex. Indeed, if the model is complex, then it may make sense to focus on a specific element of the model within a single paper. One of the common mistakes that people make is to present models that are too complex for reviewers to understand (Repenning, 2003). The scope needs to be broad enough for the paper to make a substantive contribution, but no broader. The question as to what constitutes an adequate scope depends on the type of topic that is chosen, and the state of existing knowledge within the field. These issues are discussed next.

Choosing the topic

Colquitt and George (2011) argue that one of the major factors that determines the publishability of a manuscript is the choice of topic. This is a choice that is made years before the manuscript is written. As Colquitt and George (2011) note, "the seeds for many rejections are planted at the inception of a project, in the form of topics that-no matter how well executed-will not sufficiently appeal to … reviewers and readers" (p432). This is as true for a computational modeling project as it is for a traditional project. There are three major criteria that need to be considered when choosing a topic: significance, innovation, and benefit.

Significance

A topic is significant if it addresses an important problem. Colquitt and George (2011) argue that a good starting point is to consider whether it addresses a "grand challenge". Addressing a grand challenge involves tackling a large, complex and scientifically interesting problem. A problem is likely to be scientifically interesting if the answer to that problem is not obvious at the outset of the project. Reasons why the answer may not be obvious are that the underlying mechanisms are unclear, there are competing views regarding those mechanisms, or there are findings that cannot be explained by existing theory. Computational modeling is ideally suited to the pursuit of grand challenges, because it is used to develop a shared understanding of the processes that are responsible for complex dynamic phenomena. The complexity of these phenomena means that the answers are not obvious at the outset. Most of the phenomena that we are interested in within the organizational sciences fall into this category.

An example of a grand challenge that has not yet been tackled using computational modeling is work design. One of the most fundamental questions within our field concerns the way that activities, tasks, and responsibilities should be allocated to work roles (Parker, Morgeson, & Johns, 2017). The typical way in which work design is examined in the literature is to examine the relationship between work characteristics (e.g., variety, autonomy, significance, etc) and outcomes (e.g., performance, well-being, turnover, etc), and to identify the mediators that may be involved (e.g., motivation). The problem with this approach is that it provides limited insight into the way that roles should be designed, because it is not a process model. A computational model could be developed that explains how the design of work roles influences system effectiveness. The model might include a set of tasks or functions that can be assigned to work roles in different ways. The model may then describe the dependencies amongst the roles in the form of a network structure (e.g., whether work flows in a linear sequence from one role incumbent to the next, or whether the flow is parallel or reciprocal). The researcher could then simulate the ways in which tasks or information flows through the network under a range of different operational scenarios. This could be used to derive a set of general principles regarding the situations under which different types of designs will be effective, which would be of broad interest within the field.

Innovation

A topic is innovative and novel "if a study addressing it would change the conversation that that is already taking place in a given literature" (Colquitt & George, 2011, pp. 432-433). Broadly speaking, there are two ways to do this (Barney, 2018). One is by building on and extending existing theory in new and creative ways. This is what Kuhn (1962) referred to as "normal science". The other is by replacing existing theory with a new theory that overturns existing thinking on a topic. This is what Kuhn (1962) referred to as "revolutionary science". The cycle shown in Figure 1 incorporates both approaches. According to this view, the way to move the conversation forward within a field is by the progressive development, testing, and refinement of computational models (normal science), and their subsequent replacement by a new generation of computational models that provide a better explanation for empirical phenomena than existing models (revolutionary science).

Vancouver et al. (2018) provided a good example of the ways in which a theory development paper can move the conversation forwards. Vancouver et al. (2018) developed a computational model of a prominent theory of work motivation (Locke, 1997). The process of building and simulating the model enabled Vancouver et al. (2018) to identify aspects of the theory that were specified imprecisely, explore different options for how the process might work, and eliminate options that produce behavior that is inconsistent with existing data. They were also able to identify components of the model that were redundant, and areas where additional mechanisms needed to be added. These problems are not confined to Locke's (1997) theory of work motivation. Many theories in the organizational sciences lack the precision that is needed to explain how phenomena evolve over time, or emerge from interactions amongst a set of component processes.

Computational modeling can be used as a tool for theory building, allowing the researcher to generate new insights that go beyond what can be achieved using intuition alone.

The self-regulation papers in Table 1 provide examples of the different ways in which theory testing papers can move the conversation forwards. The first paper in the series (Vancouver et al., 2010) reports the development of a computational model of multiple goal pursuit (the MGPM). The model drew on control theory accounts of motivation and expected utility accounts of decision making. In the first paper, they demonstrated that the MGPM was able to account for a range of existing empirical phenomena, such as the tendency of people to shift resources towards easier goals as deadlines approach, and the effects of incentives on prioritization (Schmidt & DeShon, 2007). Subsequent papers extended the model to account for a broader range of empirical phenomena. They did this by integrating mechanisms from other theories. Vancouver et al. (2014) extended the MGPM by adding a learning mechanism to explain how people learn about uncertainty in the environment, and the efficacy of their actions. Ballard et al. (2016) integrated the MGPM with Decision Field Theory (Busemeyer & Townsend, 1993) to account for the effects of uncertainty regarding the consequences of actions, while Ballard et al. (2018) integrated the MGPM with theories of intertemporal choice to account for the effects of deadlines on prioritization. Each paper has moved the conversation forwards by increasing the explanatory scope of the model, so that it can account for a broader range of empirical phenomena.

Benefit

Colquitt and George (2011) argue that a topic is more likely to be publishable at a top tier journal if it has practical benefit. A topic has practical benefit if a study addressing it has the potential to inform practice or policy, or lead to the development of new products or services. There are many examples of potential applications of computational modeling. For example, a computational model of stress and well-being could have practical benefit by helping inform how and when to intervene in order to best enable recovery. A computational model of team processes could be used to evaluate the costs and benefits of different interventions designed to improve team effectiveness. The advantage of a computational model, over and above a verbal theory, is that it can be simulated to generate quantitative predictions regarding potentially complex phenomena. For example, Grand (2017) used a computational model to understand how the negative effects of stereotype threat unfold and accumulate over time. They demonstrated how a relatively small effect (in statistical terms) at the individual level has large emergent effects at the organizational level.

One caveat that should be noted is that, whilst we agree that it is useful to consider whether a topic has the potential for practical benefit, this does not mean that the project has to produce a set of clear recommendations for practice. The computational modeling papers shown in Table 1 are inspired by practical problems, but do not set out to solve those problems – the focus is on enhancing our understanding of complex phenomena that are practically important. This is what is meant by the term "use-inspired basic research" (Stokes, 1997). Use-inspired basic research is valuable, because it has the potential to generate a wide range of different applications or solutions in the future, many of which will not have been forseen at the time the research is conceived. Thus, the question that the reviewer needs to ask is not whether the project will have immediate practical benefit (e.g., in the form of recommendations for managers), but rather whether it has the potential to generate a range of benefits in the future.

Setting the hook

When it comes to writing the paper, the most important part is the opening section, before the reader gets to the first major heading (i.e., the first 2-3 paragraphs). The key objective of the opening section is to capture the interest of the reader, or in other words, to ‘set the hook’. Grant and Pollock (2011) argue that the opening section needs to address three questions:

> “(1) Who cares? What is the topic or research question, and why is it interesting and important in theory and practice?
>
> (2) What do we know, what don’t we know, and so what? What key theoretical perspectives and empirical findings have already informed the topic or question? What major, unaddressed puzzle, controversy, or paradox does this study address, and why does it need to be addressed?
>
> (3) What will we learn? How does your study fundamentally change, challenge, or advance scholars’ understanding?” (p873)

Who cares?

The first challenge in writing the opening is to present the topic in a way that enables the reader to appreciate the significance of the topic, and sparks their interest in reading the paper. Essentially, this is a matter of describing the grand challenge that the paper addresses. The question, then, is how to do this. Grant and Pollock (2011) identified two archetypal hooks for opening a traditional paper. The first involves using a provocative quote to introduce the topic, while the second involves highlighting current trends. Current trends may include changes in the workplace or society, or issues that are the focus of attention in the academic literature. Whilst these approaches can help highlight the phenomenon, we think that the opening needs to do more than this. Computational modelling papers are typically more complex than traditional theoretical or empirical papers, because they seek to provide a detailed and precise explanation of the underlying process that is responsible for observable phenomena. In our experience, the most difficult part of writing a computational modelling paper is to convince the reviewers of the need to understand that process in greater detail than has been achieved previously. This needs to be done in a way that is short, sharp and compelling.

In our opinion, the key to a good opening for a computational modeling paper is to describe the phenomenon in a way that makes the complexity of the underlying mechanisms apparent, so that it sparks the reader's interest. For example, Ballard, Yeo, Loft, Vancouver, and Neal (2016) opened their paper by describing the phenomenon of multiple goal pursuit, describing the types of decisions that people make on a daily basis whilst striving for goals. A key feature of the problem that they pointed to was that people need to make choices amongst alternative courses of action, when each action has a range of potential consequences that are difficult to foresee ahead of time. Grand et al. (2016) took a similar approach, pointing to the complexity of the process by which team knowledge develops over time. In both cases, they painted a picture of a complex, dynamic, phenomenon, involving a range of different underlying mechanisms, which is of broad significance for anyone who wants to understand human behavior in an organizational setting.

What do we know, and what are the limits of current understanding?

The second challenge in writing the opening is to provide a concise summary of the state of existing knowledge regarding the phenomenon, and identify the limits of current understanding. Grant and Pollock (2011) noted that authors often find it difficult to explain why knowledge about the topic needs to be developed further. They noted that authors often fall into the trap of either: a) making the contribution seem incremental; or b) being overly critical, arguing that there is nothing to be learnt from existing research, because it is fundamentally flawed. We suspect that this is due, at least in part, to the style of theorizing that dominates our field, in which the focus is on statistical relationships amongst observed variables. It can be difficult to explain why we need a set of new variables, or look at a new set of relationships, when there is already an established set of variables and relationships in the field. Adding new variables to an existing nomological network can appear incremental, whilst introducing a completely new set of variables makes it look like one is ignoring existing research.

By contrast, it is often relatively easy to explain why knowledge about the topic needs to be developed further when writing a computational modeling paper, provided the author has effectively motivated the search for understanding. If the reviewer has already been convinced that we need to understand the mechanisms underlying a particular phenomenon, then it is simply a matter of describing the state of existing knowledge regarding those mechanisms, and making it clear what the limitations of current understanding are. For example, in their second paragraph, Ballard, Yeo, Loft, et al. (2016) noted that Vancouver et al. (2010) had taken the first steps towards the development of a formal theory of multiple goal pursuit, by developing the MGPM. Ballard, Yeo, Loft, et al. (2016) described the empirical phenomena that the existing model was able to account for, and identified a number of limitations of that model, pointing to the phenomena that it cannot account for, which were highlighted in the opening paragraph. They followed up with another paper (Ballard et al., 2018) that identified a new set of phenomena that the revised model could not account for. In each of these cases, the objective was not to convince the reader that previous research is fundamentally wrong, but rather to make the point that existing models do not yet provide a complete understanding of the phenomena being considered.

On the other hand, if there is already a verbal explanation of the process, and a large body of empirical research, then it can be more difficult to convince reviewers that existing understanding is limited in some fundamental way. Simply pointing to gaps in empirical research is not enough, because as reviewers frequently note, not all gaps are worth filling. A good way to deal with this problem is to point to the difference between a verbal theory and computational model, and explain why we need a computational model of the phenomenon. For example, this may involve making the point that verbal models are ambiguous, difficult to falsify, or incomplete (e.g., they may gloss over important details). Grand et al. (2016) did this by making the point that the "black-box" of team cognition has not yet been unpacked. This is because existing research on team cognition has focused on examining statistical relationships amongst observed constructs, rather than examining the underlying process by which team knowledge emerges over time. They noted that:

> "the approaches to theory building (e.g., "box-and-arrow" models of construct-to-construct relationships) and theory testing (e.g., cross-sectional, self-report-based research) most commonly used in the organizational sciences do not permit the precision and transparency needed to specify how and why team processes shape important team outcomes" (p1354).

This is clearly an important gap that is worth filling.

What will we learn?

The third challenge in writing the opening is to explain how the paper will move the field forward, by improving our understanding of the phenomenon. Grant and Pollock (2011) noted that the typical ways that studies move the field forward are by challenging widely held assumptions (consensus shifting) or by resolving conflict (consensus creation). The same is true with a computational modeling paper. Regardless of whether one is developing a new model (e.g., Grand, et al., 2016), or extending an existing model (e.g., Ballard, et al., 2016), it is necessary to explain how the modeling will change current thinking. This is typically done by describing how the paper addresses the limitations noted earlier. For example, Grand et al. (2016) finished their opening section by explaining how their paper unpacks the "black-box" of team cognition

Presenting the Theory & Model

The presentation of the theory and model obviously plays a crucial role in the way that reviewers respond to a manuscript. The theory section needs to engage with previous research on the topic, and provide a coherent argument to justify the proposed model. The way that this is done depends on the type of model that is developed. Traditionally, the most common type of model within the organizational sciences has been the "box-and-arrow" model of construct-to-construct relationships. When developing a model of this type, it is necessary to explain why the constructs are linked in the proposed manner (Sparrowe & Mayer, 2011). The argument will often draw on a range of different theoretical perspectives and empirical findings. In some cases, authors may make reference to internal states and/or processes to explain why observed variables are linked in the proposed manner, but will often do so in very general terms, because reviewers may see these arguments as being speculative (Sparrowe & Mayer, 2011).

In a computational modelling paper, by contrast, the purpose of the theory section is to explain the underlying mechanisms responsible for producing observable phenomena. As a result, the focus is processes, rather than constructs. This means that the theory needs to be described in a more precise manner than in a traditional paper, and it needs to make the assumptions regarding the underlying processes explicit. For example, Gee et al. (2018) developed a computational model that explained the process by which people adjust the difficulty of self-set goals. In the past, researchers within the field have typically used expectancy-value theory as a heuristic to identify the variables that predict goal choice, but have rarely made the assumptions underlying the theory explicit. Gee et al. (2018) spelt out the assumptions upon which the theory is based, and made the point that these assumptions are questionable. They then developed an alternate account, which was based on the concept of anchoring and adjustment (Tversky & Kahneman, 1981). It is rare to see this level of detail in a traditional paper.

There are a number of different ways to present a computational model within a paper. If the researcher is using an existing model, the researcher will typically describe the model verbally, and explain how it is being applied to the phenomenon, without presenting the equations. For example, Tarakci, Greer, and Groenen (2016) used an agent-based simulation method called particle swarm optimization (PSO) to examine the way that power disparity affects group performance. They explained how the PSO algorithm works, how they used it to account for interactions amongst group members, and what the parameters mean in terms of the underlying theory. Similarly, Palada, Neal, Tay, and Heathcote (in press) used an existing model of decision making called the Linear Ballistic Accumulator (LBA) to examine the way that people adapt to changes in task demands. They explained how the LBA works and what the parameters of the model mean in terms of the underlying theory. They then described the different ways in which the experimental manipulations could affect the parameters of the model. They did not need to describe the model mathematically, because it was an existing model.

On the other hand, if the author is developing a new model, or adapting or extending an existing model, then the model will need to be presented formally, in the form of a set of equations or computer code. This may be done in the introduction, at a later point in the paper, in the appendix, or in supplementary online material. The advantage of formally presenting the model in the body of the paper itself is that is that it makes it easy for the reader to see how the theory has been translated into the model. It makes sense to do it this way if the development of the model is an important part of the contribution of the paper, and it can be explained in a way that is understandable to the readership of the journal. The disadvantage of formally presenting the model in the introduction is that it can be a barrier for readers not used to seeing theoretical arguments expressed mathematically. If the model is complex, then it may make sense to present some or all of the math or code in the appendix or in supplementary online material.

Regardless of where the model is presented, the way that it is done is the same – the researcher provides a detailed verbal description of the process, and expresses that process as a series of equations. When doing this, it is important not to assume that the equations are self-explanatory. Some readers will find it difficult to read and understand equations. This means that the researcher needs to explain the equation in words. This not only involves explaining what the variables and parameters are, but also how the parameters affect the behavior of the system, and what it means psychologically. As an anonymous reviewer of one of our papers put it: "although the computational model is transparent and unambiguous with regard to the mathematical definitions and relationships among the variables, it can still be ambiguous what psychological construct those mathematical statements are intended to represent." It took us several revisions to get to a point where the psychological meaning was explained sufficiently clearly for the reviewer to accept. We were grateful to the reviewer for persisting, because the paper was stronger as a result.

A final point to consider when presenting the theory and model is whether or not to include a simulation study in the paper. In a simulation study, the researcher runs the model under different conditions (e.g., by sampling the parameters from specified distributions). This is used to generate a series of predictions, which may then be tested in an empirical study (Ballard, Yeo, Loft, et al., 2016; Tarakci et al., 2016). The simulation can be included in the introduction when presenting the model, or presented as a stand-alone study, before the empirical studies. Information that needs to be provided includes the platform or environment that the model is implemented in and the values of the inputs to the model. Inputs to the model include the exogenous variables, and fixed parameters. If it is a Monte-Carlo simulation, then the authors need to describe the distributions that the parameters are sampled from, and the number of times that the model is run. The output of the simulation is typically presented in the form of a series of figures showing the predicted pattern of results.

Whilst it is often useful to include a simulation study, there are circumstances where a simulation study may not be worth including in the paper. For example, Palada et al. (in press) used the LBA as a measurement model to quantify the effects of information load and time pressure on different aspects of the decision process (e.g., the rate of information processing, decision threshold, bias, etc). A simulation study would have been of limited use, because there were hundreds of different ways in which the experimental variables could have influenced the parameters of the model, each of which would have different effects on the predicted pattern of results. In this case, it made sense to fit the model to the data, and to interpret the pattern of parameter estimates from the model, rather than trying to generate a-priori predictions.

Testing the Model

If the manuscript incorporates an empirical study, then the design of the study, and the presentation of the method and results will also play a major role in shaping how reviewers respond to a manuscript. If the design is flawed, or the methods and results are incomplete, unclear, or raise questions regarding the credibility of the research, then this can tip the balance, resulting in a decision to reject rather than revise (Zhang & Shaw, 2012). This advice holds, regardless of whether one is trying to publish a traditional empirical paper, or a computational modeling paper.

Designing the study

A good place to start when thinking about the design of an empirical study for a computational modeling project is to consider the criteria by which design choices are evaluated. The design of an empirical study typically involves a tradeoff amongst three criteria: fidelity, control and generalizability (Brinberg & McGrath, 1985). Fidelity is the extent to which the data are representative of the phenomenon the model is designed to explain. Control is the extent to which the design of the study ensures that the conclusions that can be drawn from the data are defensible. A design allows for a high degree of control if it allows the researcher to manipulate or select the variables that have an effect on the phenomenon under study, and to test the assumptions that are embedded within the model. Generalizability is the extent to which the conclusions from the study can be applied to other situations or settings.

The main design choices that need to be considered include the type of design to be used (observational or experimental), as well as the setting, sample, task and measures. The main advantage of an observational design is that it allows the researcher to track a phenomenon as it evolves over time. If the phenomenon is observed in its natural context (i.e., in a field study), then this will enhance the fidelity of the study. An experimental study, on the other hand, will provide greater control. Regardless of whether the study uses an observational or experimental design, one of the key issues that needs to be considered is the number and timing of the observations, and the content of the measures. If the objective is to differentiate between competing models, the researcher will need to ensure that the models' predictions can be differentiated given the number of observations being made, the time between those observations, and the variables that are being measured. This can be assessed by constructing a simulation study that mirrors the observational or experimental protocol and comparing the predictions from each model.

As with a traditional empirical study, the key issue to consider when selecting the sample and task is to ensure that they are appropriate for question being examined. If the purpose of the paper is to test a theory or model, then the researcher needs to use a sample to which the theory applies, and a task that is representative of the phenomenon. As noted by Highhouse and Gillespie (2010), a theory can be tested in *any* sample to which it applies. This means that in many situations, a student sample will be perfectly appropriate. For example, Grand et al. (2016) tested their model of teamwork using a student sample performing a task that required participants to work collaboratively in order to make effective decisions. The theory applied to the sample, because it is a general theory of teamwork, and is not specific to a particular work setting. The task was designed so that it was representative of the phenomenon being studied, and provided the control needed to test the predictions of the model. This provides a nice example of a laboratory study that balances the competing demands for fidelity, control, and generalizability.

Though it is important to choose the right variables to measure, it also important to be aware that there will be many variables that cannot be adequately measured. In many cases, the existing measures used within the field may not directly map onto the processes described by the model. For example, during the review process, a reviewer asked why Gee et al. (2018) did not measure self-efficacy, given that previous research had identified this variables as a predictor of goal choice. The problem is that the construct of self-efficacy does not directly map onto the processes described by the model. Furthermore, many of the processes that researchers are interested in are not measurable. For example, the MGPM includes a range of internal cognitive processes that people may not have introspective access to. One of the major reasons for using computational modeling is to be able to make inferences about processes that are not directly observable, or where self-report is an unreliable indicator.

Reporting the Results

There are a number of different ways that the results of a computational modeling project can be written up. The approach that is taken largely depends on whether the model is fitted to the data or not. If the purpose of the analysis is simply to test the predictions derived from a simulation study, then the results section can be written up in the same way as a conventional empirical paper. For example, Tarakci et al. (2016) ran a simulation study to derive a set of predictions, and then tested those predictions using a conventional statistical analysis. If this is the case, then the model itself may not appear in the results section.

On the other hand, if the researcher wants to conduct quantitative model comparisons, or to interpret the parameter estimates, then the model(s) will need to be fitted to the data. If this is the case, then the authors need to describe how the fitting is done. Information that needs to be provided includes the platform or environment that the model is run in, the values of the exogenous variables and fixed parameters, the free parameters that are being estimated, the fitting algorithm and the fit indices that are being used. Detailed guidance for how to conduct model fitting is provided by Ballard, Palada and Neal (Chapter X, this volume). One important point that should be noted is that model comparisons will only identify the best model from the set of models that were considered. There is an infinite number of alternative models that could be considered, which makes it important to have a clear scientific rationale for the set of alternatives (Wilson & Collins, 2019).

A second point that should be noted is that it is essential to assess the extent to which the model captures the observed trends in the data when fitting a model to data. This is referred to as "qualitative fit". A good way to visualize qualitative fit is to superimpose a plot of the output of the model against the empirical data. In doing this, it is important to plot the trends in a way that makes it easy to see whether the model provides a good account of the data. For example, Ballard et al. (2018) did this by plotting participants' choices, which were represented as points with standard error bars, as a function of the experimental manipulations. The model's predictions were superimposed over the empirical data, with a line representing the central tendency, and a ribbon representing the 95% credible interval around the central tendency.

Discussing the Findings

The structure of a discussion section in a modeling paper is typically the same as that for a traditional empirical paper. The authors will start by providing a recap of the motivation for the study and summarizing the key findings, and then discuss the theoretical and practical implications, together with the limitations of the study. Geletkanycz and Tepper (2012) argue that in their experience as editors, the major stumbling block for authors is the discussion of theoretical implications. They argue that authors often miss the opportunity to explore the significance of their work, and shape the ongoing conversation within the literature. They argue that this can be done by treating the discussion as both an ending and a beginning. As an ending, the discussion brings closure to the study by answering the original question, and explaining the findings have improved our understanding of the phenomenon. The discussion provides a new beginning by delving into the deeper meaning of the findings, providing a bridge between the findings and the broader literature, and exploring alternative explanations or unexpected findings.

The strategy of treating the discussion as an ending and beginning works equally well for a computational modeling paper. Authors will typically summarize the empirical findings that the model is able to account for, discuss how the model accounts for those findings, and point to the differences between the current model and the other models within the field. As noted earlier, most computational modeling studies will generate unexpected findings, which may generate new insights or raise new questions that can be explored in future work. In some cases, the discussion may include additional modeling work that explores competing explanations, or addresses puzzling aspects of the results. Our advice is to only do this additional modelling, if it can be kept relatively simple. The risk of trying to do too much is that the paper becomes unwieldy. No paper can be expected to provide a complete or final answer to a complex problem, so it is reasonable to leave substantive questions for future work.

Conclusion

The use of computational modeling has been growing in I/O psychology and OB over the last decade. With this growth, however, comes new challenges for authors and reviewers. We hope that this chapter has provided a useful foundation for those who are new to computational modeling, and wishing to publish or review research that makes use of this methodology. It is difficult, of course, to put forth a template for reporting computational modeling research that is universally applicable. The approach that is taken, and the details that are required, will vary from paper to paper. However, we believe that the general principles outlined above provide a good starting point for thinking about how this type of research can be presented in a compelling way.

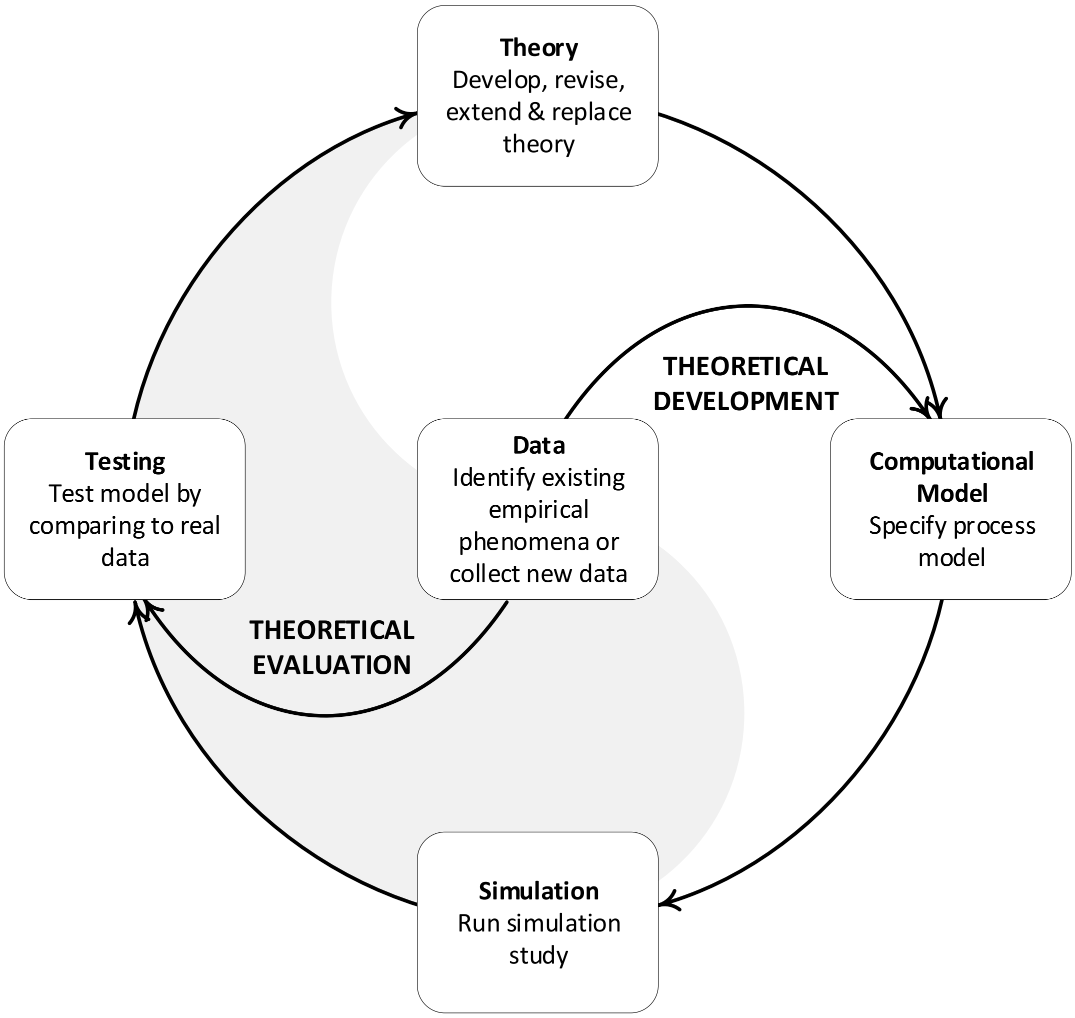

Figure 1: The computational modeling cycle.

Table 1: Summary of recommendations.

Issue	Recommendations
Choosing the topic	
Significance	Tackle a large, complex, and scientifically interesting problem
Innovation	Move the conversation forwards by the progressive development, revision, extension and replacement of models
Benefit	Assess whether the model has the potential to generate future benefit
Setting the hook	
Who cares?	Convince the reader of the need to understand the underlying mechanisms
What are the limits of current understanding?	Identify phenomena that cannot yet be explained, or to problems with the explanation provided by existing models
What will we learn?	Explain how the paper will improve current understanding
Presenting the theory & model	
Presenting the theory	Explain the processes thought to be responsible for observable phenomena
Presenting the model	Explain how the model works (i.e., explain the variables & parameters, and what it means psychologically) Consider running a simulation study to derive testable predictions
Testing the model	
Designing the study	Assess whether the model can be adequately tested using the proposed design (e.g., by running a simulation study)
Reporting the results	Assess whether the predictions of the model are supported If the model is fitted to the data, evaluate the extent to which the model captures the observed trends in the data
Discussing the findings	
Summarizing the findings & exploring the significance of the work	Explain how the study has answered the original question, and how it has opened up new questions for future exploration

Table 1: Sample of computational modeling papers published in selected IO and OB journals.

Topic	Author(s)	Journal	Objective	Approach
Decision-making				
	Gibson, Fichman, and Plaut (1997)	OBHDP	Develop and test a model that explains how decision makers learn from outcome feedback in dynamic tasks	Simulation study to generate predictions Experimental study to test predictions
	Gibson (2000)	OBHDP	Develop and test a model that explains how decision makers learn from delayed feedback in dynamic tasks	Simulation study to generate predictions Experimental study to test predictions
Job performance				
	Vancouver, Li, Weinhardt, Steel, and Purl (2016)	PP	Develop a model of job performance incorporating learning and feedback, and use the model to examine possible sources of skews in distributions of job performance	Simulation study to examine system behavior Comparison of simulated data to existing empirical effects
Leadership				
	Zhou, Wang, and Vancouver (2019)	JAP	Develop and test a model that explains leader goal striving in action teams under different environmental conditions	Simulation study to generate predictions Experimental study to test predictions Model fitting & model comparisons
Organizational learning & innovation				
	Bruderer and Singh (1996)	AMJ	Develop a model of organizational evolution and use the model to examine whether learning can accelerate the discovery of new organizational forms	Simulation study to examine system behavior Comparison of simulated data to existing empirical effects
	Ethiraj and Levinthal (2004)	ASQ	Develop a model of organizational adaptation, and use the model to examine the effectiveness of design efforts in different environments	Simulation study to examine system behavior
	Gibbons (2004)	AMJ	Develop a model to simulate innovation diffusion through interregional network structures in a population of organizations	Simulation study to examine system behavior
	Chang (2007)	OS	Develop a model to simulate how ideas are diffused through social networks, and examine how network structures emerge under different environmental conditions	Simulation study to examine system behavior
	Kane and Alavi (2007)	OS	Extend an existing model of organizational learning (March, 1991) to account for the effects of information technology on exploration and exploitation	Qualitative case studies to extend model Simulation study to examine system behavior Comparison of simulated data to existing model
	Levine and Prietula (2013)	OS	Develop a model to simulate the performance of open collaboration ventures, as a function of the cooperativeness of agents, diversity of needs, and rivalry of goods	Simulation study to examine system behavior
	Levinthal and Marino (2015)	OS	Develop a model of organizational adaptation, and use the model to examine the interplay between learning and selection	Simulation study to examine system behavior
	Chen et al. (2018)	OS	Develop a model of entrepreneurial learning under uncertainty that explains why too many entrepreneurs enter markets, and why too many persist for too long	Simulation study to examine system behavior Comparison of simulated data to existing empirical effects
Psychological Assessment				
	Grand (2019)	JAP	Develop a model to explain the process by which people respond to situation judgment test items	Simulation study to examine system behavior Experimental study to identify empirical effects Comparison of simulated data to existing and new empirical effects

Self regulation			
Vancouver et al. (2010)	JAP	Develop a model to account for how people allocate their time when pursuing multiple goals (the Multiple Goal Pursuit Model: MGPM)	Simulation of existing empirical effects Model fitting
Vancouver, Weinhardt, and Vigo (2014)	OBHDP	Extend the MGPM to account for changes in beliefs by incorporating learning	Simulation study to examine system behavior Simulation of existing empirical effects
Ballard, Yeo, Loft, et al. (2016)	JAP	Extend the MGPM to account for: uncertainty regarding the consequences of actions; avoidance goals; and individual differences in prioritization strategy	Simulation study to generate predictions Experimental study to test predictions Model fitting & model comparisons
Ballard, Yeo, Neal, and Farrell (2016)	JAP	Use computational methods to assess the optimality of prioritization decisions during multiple goal pursuit	Experimental study to identify empirical effects Normative model used to identify optimal response, analysis compared observed to optimal responses
Vancouver and Purl (2017)	JAP	Develop a model to account for the positive, null, and negative effects of self-efficacy on performance	Simulation of existing empirical effects
Ballard et al. (2018)	JAP	Extend the MGPM to explain how people allocate their time when pursuing goals with different deadlines	Experimental study to identify empirical effects Model fitting & model comparisons
Gee et al. (2018)	OBHDP	Develop a model to explain how people adjust the difficulty of self-set approach and avoidance goals	Experimental study to identify empirical effects Model fitting & model comparisons Hypothesis tests on model parameters
Stereotype threat			
Grand (2017)	JAP	Develop a model to estimate the impact of stereotype threat on organizational performance	Experimental study to estimate effect size at the individual level Simulation study to evaluate emergent effects at the organizational level
Team processes			
Mäs, Flache, Takács, and Jehn (2013)	OS	Develop a model of opinion and network dynamics in teams to examine whether demographic faultlines breed opinion polarization, and whether crisscrossing actors enable teams to overcome polarization	Simulation study to examine system behavior
Kennedy and McComb (2014)	JAP	Use computational methods to explore the timing of process shifts within teams and identify interventions that enhance performance by affecting the timing of process shifts	Empirical study to collect data Genetic algorithm used to generate optimal simulated teams from observed data Virtual experiments conducted using simulated teams
Grand et al. (2016)	JAP	Develop a model to account for the process by which collective knowledge emerges from the individual to the team level	Simulation study to generate predictions Experimental study to test predictions
Tarakci et al. (2016)	JAP	Use a model to understand when & how power disparity helps or hurts group performance	Simulation study to generate predictions Experimental study to test predictions Field study to assess generalizability

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