

# How to Make and Sell a Meaningful Empirical Contribution

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The main reason empirical submissions to *Cornell Hospitality Quarterly* (*CQ*) are rejected is that the authors failed to adequately answer the “so what?” question. Almost all empirical submissions to *CQ* provide compelling evidence that some relationship exists between two or more variables, but fewer of them make a compelling case that those relationships are worth knowing more about. To help future authors make that case more successfully, I will use this essay to share my definition of a meaningful empirical contribution and to provide suggestions about how to make and sell such contributions. I will also identify some common errors in communicating a study’s contribution that authors should avoid.

To me, a meaningful empirical contribution is one that changes our collective knowledge about (or confidence in) one or more important causal relationships and processes. A causal relationship/process is important if it enhances our prediction or control of outcomes we care about. Thus, authors of empirical papers targeted at *CQ* need to argue both that (a) the causal relationships and processes they test allow us to better predict or control outcomes we care about, and (b) their tests change our knowledge about, or confidence in, those causal relationships and processes. Some guidelines to follow and pitfalls to avoid in making these arguments are described below.

Authors should begin by identifying the key causal relationships to be examined in the article and explaining why readers should care about those relationships. Explaining the importance of the causal relationships being studied seems obvious, but it is amazing how rarely it is done. Too often authors focus on the importance of their study’s general topic (such as customer loyalty programs or online reviews), context (such as ethnic restaurants or particular national markets), or dependent variables (such as customer satisfaction or employee turnover), but ignore the importance of the specific causal relationships they are testing. Not all independent variables are good predictors of the outcomes they affect—to be a valuable predictor, an independent variable must be strongly related to the outcome and easier to get information about than is the outcome variable it predicts. Nor are all independent variables good sources of control over the outcomes they affect—to be a valuable source of control, an independent variable must be manipulatable. Ideally, it should also have few negative side effects. Thus, authors must explain how the independent variables they study enable greater prediction or control of the out-

come variable as well as why readers should care about predicting or controlling that outcome.

After identifying and explaining the importance of the causal relationships being studied, authors should describe one or more process-based explanations for why those causal relationships are plausible and (if desired) to lay out formal hypotheses. Note that the emphasis at this point should be on underlying theoretical processes and not on previous tests of the relationships. Too often authors mistakenly rely on previous research findings instead of an explication of underlying processes to develop and justify hypotheses. This is a mistake because previous findings of a relationship do not reduce the need to identify an underlying explanatory process, but they do reduce the perceived need for another test of that relationship.

Next, authors should explain how their study will change our knowledge about, or confidence in, the key causal relationships and processes being studied. This is where authors should critically review existing tests of those relationships or processes and summarize what we do and (more importantly) do not already know about them. It is important for authors to be explicit and clear about how many previous studies have tested the focal (or hypothesized) relationships and with what results. Oddly, I often finish reading the literature review sections of *CQ* submissions without knowing this critical information because the authors have failed to provide it. Do not make this serious mistake because it hides the true nature of your study’s contribution.

If previous tests of a focal relationship *do not exist*, it is fairly easy to argue that your study has the potential to change our knowledge about or confidence in that relationship. However, even in this case, authors should try to build value by enhancing a-priori uncertainty about the relationship. One particularly effective way to highlight value-enhancing uncertainty is to find and describe competing processes that are expected to drive an effect in opposite directions and then to test those opposing expectations. It is not always possible, but I encourage *CQ*’s authors to use this technique, called *strong inference*, for maximizing a study’s contribution when they can (see Platt, 1964, for more details about this technique).

If previous tests of a focal relationship *do exist and their findings are significantly heterogeneous*, that alone creates uncertainty about the causal relationship, but this type of uncertainty does not justify another test of that relationship. In that case, conducting another test of the relationship

would just add to the mixed results. Instead, heterogeneous effects in the existing literature argue for hypothesizing and testing the relationship's moderation by some variable that might explain the mixed findings.

If previous tests of a focal relationship *do exist and their findings are consistent* (exhibiting only chance variability), then authors need to raise some uncertainty about the reliability, validity, or generalizability of those earlier findings to justify another test of the relationship. For example, if you are replicating a causal relationship in a new context or setting, then you should enhance the value of your replication by providing readers with reasons to believe the relationship may not generalize to the new setting. Note that in the case of homogeneous effects in the existing literature, a well-done failure to replicate may make more of a contribution than would another replication, because a failure to replicate may reduce our confidence in the relationship or its robustness more than a replication would enhance it. In general, the more uncertainty your study can resolve or create, the greater its potential contribution. However, it is easy to get null results for trivial reasons, so failures to replicate

must pass very high standards to change our confidence in a relationship or its robustness and thereby warrant publication.

In summary, empirical findings are interesting and important to the extent that they change our collective knowledge about (or confidence in) one or more causal relationships or processes that enhance our ability to predict or control outcomes we care about. The more an empirical study changes our knowledge about (or confidence in) a causal relationship or process, the greater the ability to predict or control an outcome provided by that causal relationship or process, and the more we care about that outcome, the greater the contribution that study makes. Authors of empirical papers targeted at *CQ* are encouraged to conduct studies that make meaningful contributions and to clearly and explicitly build a case for each of these aspects of their studies' contributions.

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## Reference

Platt, J. R. (1964). Strong inference. *Science*, *146*, 347-353.