

The External Validity of Laboratory Experiments:  
Qualitative rather than Quantitative Effects<sup>1</sup>  
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1: Introduction

Laboratory experiments are used to address a wide variety of questions within economics, including whether behavior is consistent with the predictions and assumptions of theory and how various mechanisms and institutions affect the behavior of economic agents (see Roth 1987 for an overview). The experimental laboratory has become an integral part of the field of economics and a productive dialog now exists between theory, laboratory experiments, and field studies. Results from laboratory experiments are being published in the leading economics journals and many top departments now have experimental laboratories.

Recently, however, a set of papers by Levitt and List (2006, 2007a, 2007b, 2008) has questioned what we can learn from laboratory studies. At the center of their critique is the statement that “the critical assumption underlying the interpretation of data from lab experiments is that the insights gained can be extrapolated to the world beyond” (Levitt and List 2007a, p. 153) and the subsequent argument that there are “many reasons to suspect that these laboratory findings might fail to generalize to real markets” (Levitt and List 2008, p. 909), suggesting that the critical assumption about extrapolation may not hold. Specifically, the authors highlight five factors that differ between the laboratory and economic environments of interest.<sup>2</sup> They recognize that external validity also is a concern for field experiments and for naturally occurring data, however their statement that “field experiments avoid many of the important obstacles to generalizability faced by lab experiments” (Levitt and List 2008, p. 910) has caused many to interpret their papers as discrediting laboratory experiments and ranking field experiments as a superior methodology.

The papers by Levitt and List have caused quite a stir both inside and outside of the field of experimental economics. A common response in defense of laboratory experiments has been to counter attack field experiments, arguing that field experiments suffer from the same charges levied at laboratory experiments, namely a lack of external validity.<sup>3</sup>

In his reply to Levitt and List, Camerer (2010) moves beyond the generalizability of field experiments and systematically addresses the five factors that Levitt and List (2007a) argue reduce the generalizability of laboratory studies. Camerer (2010) notes that the features of the lab that differ from the field make less of a difference on behavior than Levitt and List (2007a)

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<sup>1</sup> The authors thank George Lowenstein, Jack Ochs, Alvin Roth and Tim Salmon for their helpful and thoughtful comments, and we thank Guillaume Frechette and Andrew Schotter for inviting us to write this comment.

<sup>2</sup> The five factors they discuss are: the level of scrutiny, the lack of anonymity, the context, the stakes, and the population.

<sup>3</sup> In an echo of the attacks on laboratory experiments, critics have argued that certain markets studied in the field may differ substantially, and thus provide limited insights about, other markets of interest (not coincidentally, a common example has been the sports-card market studied by List in List (2006)). In addition, proponents of laboratory studies have argued that field experiments also lack internal validity as limitations on control in the field make it more difficult to identify causal relationships. Finally, some have raised concerns about the difficulty of replicating field experiments.

suggest. He comments that when concerns about one of these factors arise, lab studies can be altered to better mirror an external environment of interest.<sup>4</sup> Camerer argues that there is no evidence that lab experiments specifically designed to mirror a particular environment lack external validity. In addition, he compares the strengths and weaknesses of data collected in the lab and field, essentially arguing that laboratory experiments are more easily replicated and have greater internal validity, whereas field experiments have greater external validity. However, Camerer also makes the striking argument that external validity is irrelevant for a large class of laboratory studies. While he states that external validity is crucial for studies that aim to inform policy, he argues that it is not necessary for studies aiming to understand general principles. Referring to the former as the policy view and the latter as the science view, Camerer (2010) argues that since experiments conducted under the science view do not aim to forecast behavior in a particular external target setting, it is irrelevant whether these laboratory results generalize to the field.

The papers by Levitt and List and the reply by Camerer (2010) contribute to what many view as an overdue debate on the contribution of laboratory experiments to economics. Unfortunately, much of the debate has been aimed at a straw-man version of external validity. While the debate has centered on the extent to which the *quantitative* results are externally valid, we will argue that for most laboratory studies it is only relevant to ask whether the *qualitative* results are externally valid. Interestingly, among the authors on both sides of the debate there is significantly less (and possibly no) disagreement on the extent to which the qualitative results of a laboratory study are externally valid.

In Section 2, we explain why for most laboratory studies it is only relevant whether the *qualitative* results of the study are externally valid. In Section 3, we argue that laboratory studies are conducted to identify general principles of behavior and therefore promise to generalize. In Section 4, we examine whether laboratory experiments live up to this promise. We discuss the extent to which qualitative results persist outside of the lab, and how we should respond when they do not. We will avoid the debate on whether the concerns about external validity are more or less warranted in laboratory or field environments. We do not see this debate as being productive as it presupposes that the methodologies are in competition. We conclude the paper by arguing, as many others do, that the lab and field methodologies are highly complementary and that both provide important insights to our understanding of economics.<sup>5</sup>

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<sup>4</sup> Camerer (2010, p..) notes “Except for obtrusive observation in the lab (which is required by most human subjects protection practices), most of the features of lab experiments can be relaxed, if necessary, to improve generalizability if that is an important goal.”

<sup>5</sup> Roth (2008) notes “Lab and field experiments are complements not only with each other, but also with other kinds of empirical and theoretical work.” Falk and Heckman (2009) write “Field data, survey data, and experiments, both lab and field, as well as standard econometric methods, can all improve the state of knowledge in the social sciences.” In their Palgrave entry on field experiments, Reiley and List (2007) write “the various empirical approaches should be thought of as strong complements, and combining insights from each of the methodologies will permit economists to develop a deeper understanding of our science.” Levitt and List (2007b, p. 364) point to the complementarities in stating “we believe that the sharp dichotomy sometimes drawn between lab experiments and data generated in natural settings is a false one.... Each approach has a different set of strengths and weaknesses, and thus a combination of the two is likely to provide more insight than either in isolation.” As discussed below, Kessler (2010) highlights a specific way in which laboratory and field results are complements in the production of knowledge.

## 2: Quantitative versus qualitative external validity

In the debate about whether laboratory studies are “generalizable” or “externally valid,” these terms are often not explicitly defined. Indeed, formal definitions of external validity vary substantially. Some definitions of external validity simply require that the *qualitative* relationship between two variables hold across similar environments. For example, Guala (2002, p. 262) states: “an experimental result is internally valid, if the experimenter attributes the production of an effect B to the factor ... A, and A really is the ... cause of B in the experimental set-up E ... [The experimental result] is externally valid ... if A causes B not only in E, but also in a set of other circumstances of interest F, G, H, etc.” More demanding definitions of external validity additionally require that the *quantitative* relationship between A and B identified in one set-up hold in other comparable settings.

Levitt and List (2007a) describe concerns about laboratory experiments meeting the higher standard. In their conclusion, they accept that laboratory experiments meet the first definition of external validity, and argue that (emphasis added) “lab experiments that focus on *qualitative* insights can provide a crucial first understanding and suggest underlying mechanisms that might be at work when certain data patterns are observed” (p. 170-171).<sup>6</sup> However they argue that laboratory experiments fail to meet the higher standard, questioning whether (emphasis added) “the experimental findings are *equally* descriptive of the world at large” (p. 158). More directly, Levitt and List (2007b, p. 351) write “even for those experiments that are affected by our concerns, it is likely that the qualitative findings of the lab are generalizable, even when the quantitative magnitudes are not.” In responding to Levitt and List, the subsequent debate has centered on the extent to which *quantitative* lab findings are externally valid.

This focus on quantitative external validity is misplaced for many (if not most) experimental studies, however, as the emphasis in these studies is to identify the direction rather than the magnitude of an effect. Indeed, the non-parametric statistical methods commonly used to infer significance rely solely on qualitative differences. Few experimental economists would argue that the magnitude of the difference between two laboratory treatments is indicative of the magnitude one would expect to see in the field or even in other laboratory studies in which important characteristics of the environment have changed.<sup>7</sup> For example, the revenue difference between an English auction and a first-price sealed bid auction in the lab is not thought to be indicative of the quantitative difference one would find between any other set of English and first-price sealed bid auctions. Similarly, despite the clear objective of finding externally valid results, the experiments that tested various designs of FCC spectrum auctions were not aiming to identify magnitudes that would generalize to the field. Instead, they were run with the

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<sup>6</sup> Levitt and List also note that “lab experiments can suggest underlying mechanisms that might be at work when certain data patterns are observed and provide insights into what can happen in other related settings” (2007b, p. 363).

<sup>7</sup> While many field experiments are written up to emphasize the magnitude of an estimated effect, it is presumably not the intention of the authors that the level of this magnitude is expected to generalize to other environments. For example, List and Lucking-Reiley (2002) identify a nearly six-fold increase in contributions when they increase seed money for a fundraising goal from 10% to 67%. Few would expect this result to generalize to a six-fold increase in all other charitable campaigns. Presumably the authors do not report this result in their abstract to suggest that it is quantitatively generalizable, but instead report the result: to demonstrate the strength of the effect, to compare it to the strength of the other results in their paper, and to suggest that it is of economic significance.

expectation that the general principles of behavior identified in lab auctions also would be present in the field (e.g. Ledyard, Porter and Rangel 1997; Plott 1997).

The emphasis on qualitative results is in part explained by the fact that all theoretical and empirical models require simplifying assumptions. In constructing these models, we eliminate any factors that we think are not central. A consequence of abstracting away from environments of interest is that we likely fail to capture the precise magnitude of the effect we expect to see in those environments.<sup>8</sup>

Since most experimental studies focus on directional effects, the debate about external validity should center on qualitative rather than quantitative predictions. Falk and Heckman (2009) introduce a framework that we can use to conceptualize the difference between the two types of external validity. Considering a relationship between an outcome  $Y$  and a number of variables as defined by  $Y=f(X_1, X_2, \dots X_N)$ , which they refer to as an all-causes model since it “captures all possible causes of  $Y$ ”, they note that the causal effect of  $X_1$  on  $Y$  is the effect of varying  $X_1$  holding fixed  $X^* = (X_2, \dots X_N)$ .<sup>9</sup> Following on Levitt and List, Falk and Heckman also focus on the conditions under which the quantitative findings of the laboratory are externally valid. They show that the substantial requirements for quantitative external validity are that  $f$  is separable in  $X_1$  and  $Y$  is linear in  $X_1$ . Notice, however, that the requirements securing external validity of the qualitative effects are weaker. For the qualitative results to be externally valid, we simply require  $Y$  to be monotonic in  $X_1$  and for changes in  $X^*$  to not reverse the relationship of  $X_1$  on  $Y$ .

In the all-causes model, the concerns about external validity raised by Levitt and List are concerns that in the laboratory the magnitude of  $X_1$  and the level at which  $X^*$  is held fixed do not correspond to environments outside of the lab. If, in contrast to the current debate, the concern is merely whether the qualitative effect generalizes, then the differences between the laboratory and the field are only relevant if they are thought to reverse the relationship of  $X_1$  on  $Y$ .

Take, for example, the winner’s curse. Early experimental demonstrations of the winner’s curse, using student subjects, found that increasing the number of bidders increased seller revenue while providing public information about the value of the item for auction decreased seller revenue (Kagel and Levin 1986). Since bidders with different levels of experience may differ in their understanding of the incomplete information problem at the core of the winner’s curse, the effect of increasing the number of bidders or the effect of increasing public information may differ across subject pools. However, independent of the subject pool, we expect that increasing the number of bidders will increase the number of individuals who fail to understand the winner’s curse. And we expect that providing public information will mitigate the effect of incomplete information on the bids of anyone who had previously failed to recognize the winner’s curse. The magnitude of these comparative statics will very likely be different between students and oil company executives, but we expect the qualitative results to be the same.<sup>10</sup>

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<sup>8</sup> We would only describe quantitative relationships with our models if all the factors we assumed away were irrelevant for the magnitude of the examined effect.

<sup>9</sup> Note that in many experimental studies  $X_1$  is a binary variable indicating different market mechanisms or institutions.

<sup>10</sup> Dyer, Kagel and Levin (1989) find that professionals also are subject to the winner’s curse. See Frechette (2011) for a review of studies comparing the behavior of students and professionals. Out of 13 studies that allow

### 3: Do laboratory studies promise generalizability?

In the debate about the external validity of laboratory experiments there has been disagreement about when external validity is important. Levitt and List state that “the critical assumption underlying the interpretation of data from lab experiments is that the insights gained can be extrapolated to the world beyond.” Schram (2005, abstract) has a more moderate statement noting that “External validity is relatively more important for experiments searching for empirical regularities than for theory-testing experiments.” Camerer (2010) takes this argument one step further and argues that external validity is important for experiments conducted from a policy-evaluation perspective, but not for experiments conducted from a scientific perspective.<sup>11</sup> Camerer (2010) states that “if the goal is to understand general principles, whether the ‘lab generalizes to the field’ ... is both irrelevant and often unknowable and distracting” (p.\_\_). In fact, a central point of his paper is that “lab experiments can generalize well when they intend to (and promise to), but need not when they don’t intend to (and don’t promise to)” (p.\_\_).<sup>12</sup>

While there may be disagreement on whether there is a promise for quantitative results of an experiment to be externally valid, we do not think there can be much disagreement on the extent to which the qualitative results promise external validity. Since laboratory experiments are meant to uncover general principles of behavior, it is difficult to see how a concern for external validity is not warranted. Camerer argues that “in the scientific perspective the purpose of an experiment is not to provide results that necessarily generalize to a particular target setting.” However, even without a particular external target in mind, the general rules that govern behavior in the experimental environment must nonetheless apply in other environments with similar characteristics.<sup>13</sup> Surely it is a minority of experimental studies that examine environments that have no counterpart outside of the study and for which we would not expect that the “insights gained can be extrapolated to the world beyond.” While laboratory studies may not promise quantitative external validity they do promise qualitative external validity. The question of interest is whether they live up to this promise.

### 4. Do laboratory results inform us about the world outside the lab?

Over the course of the debate, authors have suggested two conditions under which we can extrapolate from the laboratory to other environments of interest. Falk and Heckman (2009) summarize the two conditions: “When the exact question being addressed and the population being studied are mirrored in an experiment, the information from it can be clear and informative. Otherwise, to transport experimental findings to new populations or new environments requires a model.” Camerer (2010) also highlights that extrapolation is warranted either when the population and environment examined in the laboratory mirrors an environment of interest or

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comparison of professionals and students in standard laboratory games, he finds only one example where the behavior by professionals is closer to what is predicted by economic theory.

<sup>11</sup> Camerer (2010, p.\_\_) states that in the policy evaluation perspective “ideal data are those that are likely to produce a very good guess about how a policy will actually work in a target setting.”

<sup>12</sup> The many experimental studies on various FCC auction mechanisms demonstrate that policy makers and practitioners are often deeply interested in qualitative (as well as quantitative) effects.

<sup>13</sup> For example, Plott (1982) argues that the markets examined in the lab also are real markets and therefore that the general principles of economics demonstrated in the lab should also hold in other markets.

when one uses previous studies to account for differences between the lab and field (implying one has an underlying model in mind). Camerer (2010) states that “parallelism does *not* require that students in a lab setting designed to resemble foreign exchange behave in the same way as professional foreign exchange traders on trading floors. It simply requires that any dependence of lab behavior on student-professional status be comparable to the degree of dependence in field settings.”<sup>14</sup> Levitt and List (2007b, p. 364) also stress the value of a model in extrapolating experimental results when they write that “even in cases where lab results are believed to have little generalizability, some number [a laboratory estimate] is better than no number, provided the proper theoretical model is used to make inference.”<sup>15</sup>

While mirroring a particular environment of interest or using a model for inference are both appealing, it is important to recognize that these conditions are very stringent. It is difficult to envision a laboratory study that fully mirrors the circumstances of the external environment of interest and it is unrealistic to think that we can find a model that allows us to predict how differences between the lab and the field will interact with any comparative static. If these conditions were needed for external validity, then laboratory results would at best provide very limited insight about behavior outside the lab. Fortunately, neither of these conditions is necessary for the qualitative results to extrapolate. As noted earlier, the qualitative effects will be externally valid if the observed relationship is monotonic and does not change direction when changing the level of variables seen in the field relative to those in the lab.

In a laboratory experiment, subjects are presented with incentives that are meant to capture the central features of the environment in which the economic decisions are usually made. The experimenter has in mind a model that assumes that the laboratory environment does not differ from a comparable field environment on a dimension that would change the sign of the comparative static.<sup>16</sup> Provided the experimental model is correct, the qualitative results should generalize. What does that mean in practice? What can we conclude about behavior outside the laboratory when we reject, or when we fail to reject, a directional hypothesis in the laboratory?

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<sup>14</sup> Camerer (2010) similarly suggests that threats to external validity arise when features of the “common design” (Camerer 2010, p. \_\_) of laboratory studies differ in important ways from the environments of interest outside of the lab. Camerer suggests that a number of features of an environment will impact behavior: the incentives, the decision context, the rules, the endowments, and the characteristics of the actors. He argues that external validity will be at risk if two environments differ significantly on any of those dimensions or if decisions are very sensitive to small changes in those features.

<sup>15</sup> Levitt and List (2007b) argue that a model is required to predict outside of the laboratory. “Our approach to assess the properties of the situation is to explore, both theoretically and empirically, how individual behavior changes across judiciously chosen levels of these factors, as moderated by both the task and the agent type. Until this bridge is built between the lab and the field, any argument concerning behavioral consistency might be considered premature” (p. 363). They also note that the demands on this model are rather substantial, “unless considerable changes are made in the manner in which we conduct lab experiments, our model highlights that the relevant factors will rarely converge across the lab and many field settings....what is necessary are a model and a set of empirical estimates to inform us of when and where we should expect lab behavior to be similar to a particular field environment and, alternatively, when we should expect large differences” (p. 364).

<sup>16</sup> If a laboratory experiment were expected to generate a result that was specific to the lab (i.e. rather than a result that identified a general principle) such that the sign of the result might change outside the lab, we contend that the experimenter should not have bothered to run the experiment in the first place.

Suppose that laboratory results reject the hypothesis that a variable affects behavior in a certain way. To what extent does this finding allow us to draw inference on the role the manipulated variable will have outside of the laboratory? If in the very controlled laboratory setting we reject our hypothesis, then it is unlikely that the manipulated variable will affect behavior in a more complicated external environment.<sup>17</sup>

What if instead we fail to reject a hypothesis in the lab? Does that imply that the hypothesis is likely to find support in field settings with similar characteristics? Schram (2005, p. 232) argues that “After a theoretical design, a test [of a new airplane] in a wind tunnel is the stage of laboratory experimentation. If it does not ‘crash’ in this experiment, the plane is not immediately used for the transport of passengers, however. One will typically conduct further tests in the wind tunnel under extreme circumstances. In addition, further testing including ‘real’ flights without passengers will be conducted.” Thus finding lab evidence consistent with a theory will typically lead to repeated investigations of the result, and ideally these will be done under various stress-test conditions in the lab and in the field. Absent these stress tests, however, is it reasonable to expect the documented comparative static in the lab to also hold in the field? The answer may depend on the strength of our prior, but identifying a comparative static in the lab certainly increases our posterior belief that the comparative static will be found in the field. Since the lab is thought to investigate general principles of behavior, we expect these principles to hold both inside and outside of the laboratory.

As with any finding, however, caution is needed to generate predictions in different settings. For example, in documenting statistical discrimination against women in the sports-card market, List (2004) does not claim that women always will be charged a price that is a specific magnitude greater than that for men, or that women always will be charged a higher price, or that there always will be statistical discrimination, but rather that when there are grounds for statistical discrimination against a particular group the market is likely to respond in a predictable way. In fact, in a study on taxi fare negotiations in Lima, Peru, Castillo et al. (2009) document that statistical discrimination leads to inferior outcomes for men since they have a greater willingness to pay for taxi rides than do women.

So what should be done if we identify a comparative static in the lab but fail to find evidence of the comparative static outside of the lab?<sup>18</sup> When designing an experiment, the experimenter assumes the lab setting captures the important characteristics of environments of interest and that the qualitative result will hold outside the lab. Failure to replicate a lab finding in the field may result from the experimenter’s model failing to capture central features of the decision environment outside the lab. This is akin to when a result that holds true in a model is not observed in the world. In these cases, we infer that the model has an assumption that does not hold or that the model has abstracted away from something important. Consequently, failure to replicate an experimental finding should cause us to revisit the question at hand, as it may be an

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<sup>17</sup> For example, Schram (2005, p. 231) writes: “The bottom line is that there is no reason to believe that a general theory that is rejected in the laboratory would work well in the world outside of the laboratory.” Of course this does not mean that the theory being tested is wrong, it just means is that it is not a good approximation of actual behavior.

<sup>18</sup> Of course, some studies are conducted in the laboratory since they cannot be conducted in the field. For example, it is difficult to see how a signaling experiment along the lines of Cooper et al. (1997a,b) could be conducted in the field.

indication that the laboratory and field environments were different on a dimension that plays an important role in driving the comparative static results.

For example, theoretical studies by Goeree et al. (2005) and Engers and McManus (2007) along with lab studies by Orzen (2008) and Schram and Onderstal (2009) all demonstrated that all-pay charity auctions generated higher revenue than other fundraising mechanisms, while subsequent field studies contradicted this comparative static. Carpenter et al. (2006) and Onderstal et al. (2010) both found that contributions fell under the all-pay auction. Interestingly, the field studies also demonstrated why this discrepancy may have occurred. While the theory and laboratory experiments had assumed full participation, the field studies found that potential donors will opt out of participating in the all-pay auction. Thus the inconsistencies between the lab and field resulted from an incorrect (and restrictive) assumption of full participation in the auction.<sup>19</sup> By ignoring the importance of participation, the initial laboratory model was misspecified.

When results of a laboratory study are not observed in certain field settings, it is of interest to determine which assumption in the laboratory has failed to hold true. The fact that certain laboratory environments may fail to capture the central features of the decision environment outside the lab is raised in Kessler (2010), which highlights a distinction between *methodological differences* and *strategic and informational differences*. *Methodological differences* are differences between environments inside and outside the lab that result from laboratory methodology. Factors highlighted by Levitt and List (2007a) like scrutiny and the voluntary participation of the actors are methodological differences, since they systematically differ inside and outside the lab. *Strategic and informational differences* are differences in information, incentives, actions, etc. that might vary from one environment to another and can be manipulated by an experimenter.

While it is tempting to conclude that inconsistencies between lab and field studies result from methodological differences, care should be given to determine whether instead strategic and informational differences are driving the results. Kessler (2010) aims to explain why gift exchange is more commonly seen in laboratory than field experiments. Using laboratory experiments, he shows that differences in the relative wealth of the firm, the efficiency of worker effort, and the action space available to the worker (strategic and informational differences, not methodological ones) contribute significantly to the differences in results between the laboratory and the field. Another example is the lab and field differences of Dutch and sealed bid auctions. While laboratory studies by Cox, Roberson and Smith (1982, 1983) find that the revenue in sealed bid auctions dominates that in Dutch auctions, a field study by Lucking-Reiley (1999) finds the reverse revenue ranking. While these results initially were ascribed to methodological differences between lab and field, a subsequent study by Katok and Kwasnica (2008) shows that *strategic and informational differences* can explain the divergent results. Specifically, they note that the clock speed in Lucking-Reiley was much slower than that in Cox et al., and they show in a laboratory study that revenue in the Dutch auction is significantly lower than in the sealed bid

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<sup>19</sup> See also Ivanova-Stenzel and Salmon (2008) for a further illustration that endogenous entry may influence the revenue rankings in auctions. Interestingly, Corazzini et al. (2010) show a similar decrease in participation in the lab when participants in the all-pay public good auction are given heterogeneous endowments.

auction at fast clock speeds, whereas the reverse holds at slow clock speeds.<sup>20</sup> As the initial study failed to account for the effect of clock speed on the revenue ranking, the model was misspecified and the results seen at fast clock speeds did not generalize to environments with slow clock speeds.

Notice that in these examples the failure to generalize was not a failure of laboratory methodology but rather evidence that the initial laboratory experiment did not capture an important feature of the decision environment. By identifying which features of the decision environment are causing the differential results (and which are not) we hone our model of behavior.<sup>21</sup>

## 5: Conclusion

Economic research aims to inform us of how markets work and how economic agents interact. Principles of economic behavior are expected to apply outside of the unique environment in which they are identified. The expectation and promise of economic research is that the uncovered principles of behavior are general and therefore externally valid. However that promise does not imply that the magnitude of an estimated effect applies generally. In many cases, including many experimental economics studies, the expectation is simply that the qualitative results are generalizable. The simplifying assumptions used to secure internal validity imply that the magnitude of the observed effect will likely differ from the magnitudes in other environments. Interestingly, there appears to be broad agreement that the qualitative results seen in the laboratory are externally valid. To our knowledge, there is no evidence suggesting that the lab-field differences discussed in the ongoing debate reverse directional effects identified in the lab.

In emphasizing the importance of qualitative results, we have ignored the studies that appear to estimate preference parameters in the laboratory. The objective of some of these studies is to derive comparative statics, whereas others emphasize the parameter estimates themselves – and while some of these parameter estimates may be thought to be scale free and generalizable, others are context dependent and therefore unlikely to generalize.

When authors use preference parameters to generate comparative statics, they often do so with the expectation that the comparative statics, rather than the estimated preference parameters, will generalize. For example, while Andreoni and Vesterlund (2001) estimate male and female demand functions for giving in the laboratory, they solely emphasize the surprising comparative static result that women are less sensitive than men to the price of giving, and it is this

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<sup>20</sup> Katok and Kwasnica (2008, p. 346) note: “The Cox et al. (1982) study used clocks that descended between 0.75% and 2% of their maximum value every *second*; the Lucking-Reiley (1999) field study used a clock that decreased approximately 5% per *day*... Since slower auctions impose higher monitoring and opportunity costs on bidders and are generally less exciting, the slow clock may cause the bidders to end the auction early.”

<sup>21</sup> In fact, if factors like scrutiny, decision context, or characteristics of the actors interact importantly with a comparative static in a way that we do not expect, the fact that we did not expect the interaction means our model is misspecified. In particular, it means we have left out an important interaction that will be important to include in the model to make predictions. For example, if only women (or only students) were to respond to the incentive of lowered prices, then a model of demand that does not account for gender (or student status) would fail to explain or predict the effect of prices on behavior.

comparative static that they subsequently try to extrapolate. They first note that Andreoni, Brown and Rischall (2003) find the same gender difference in price sensitivity when examining how annual giving responds to an individual's marginal tax rate. Then, using data on tipping by Conlin, O'Donoghue, and Lynn (2003), they find that tipping by men is more sensitive to the cost of tipping than it is for women. Thus, despite generating demand estimates for giving, Andreoni and Vesterlund (2001) do not examine whether the quantitative results generalize, instead they use the qualitative results to predict behavior outside the laboratory. This emphasis on comparative statics is also seen in some studies on individual risk preferences, time preferences, and preferences over payoffs to others, which aim to identify general principles such as loss aversion, probability weighting, present bias, and inequality aversion.

While we may expect the comparative statics derived from preference parameter estimates to generalize, it is questionable whether the estimates themselves will generalize. Camerer (2010) compares estimates of loss aversion and immediacy preference and demonstrates that the lab-lab and field-field differences are approximately as big as lab-field differences. He concludes that some preference parameters are scale-free and that their estimates are likely to be externally valid. That being said, the external validity of context-dependent preference parameter estimates is more questionable. For example, while lab and field studies on other-regarding preferences help identify the general characteristics of behavior that result from such preferences, they are unlikely to identify the magnitude of such effects across domains. Considering the amount of work professional fundraisers put into soliciting funds, it is clear that other-regarding behavior depends greatly on context. One act of charity by an individual cannot predict all his other charitable acts; instead, each charitable act has specific characteristics. Hoping an estimated preference for giving can be extrapolated to all other environments is similar to hoping that we can predict a consumer's demand for all goods from an estimate on demand for one good.

Perhaps because other-regarding preferences are so complex, however, it would be particularly costly to dismiss a research methodology from shedding light on the phenomenon. Indeed, research from both lab and field experiments have played a significant role in improving our understanding of what triggers giving. As noted earlier, field experiments helped us understand behavior in all-pay charity auctions. Lab experiments have also played an important role in helping us understand charitable giving by providing a controlled environment that enables us to identify which mechanisms may be driving behavior.

For example, field studies have repeatedly shown that contributions in many settings can be impacted by information about the contributions made by previous donors, see for example, List and Lucking-Reiley (2002), Croson and Shang (2008), Frey and Meier (2004) and Soetevent (2005). While these studies demonstrate that individuals respond to the contributions of others, they provide little information on which mechanisms may be driving the result. One hypothesis is that information about the contributions of others may provide guidance when there is uncertainty about the quality of the product provided by the organization (e.g. Vesterlund 2003; Andreoni 2006). While one easily can show theoretically that sequential giving can generate an increase in donation, signaling is a difficult behavioral task and it may be questioned whether donors will be able to exploit their ability to signal quality. Unfortunately, the signaling model is not easily tested in the field, as it is hard to isolate changes in charity quality. However it is not difficult to conduct such a study in the laboratory, and indeed a substantial attraction of the lab is

that one can easily contrast competing hypotheses.<sup>22</sup> Potters, Sefton and Vesterlund (2005, 2007) investigate sequential giving both with and without uncertainty about the quality of the public good. They find that sequential contributions increase giving when there is uncertainty about the quality of a public good, but not when the quality of the public good is known. Thus, behavior is consistent with individuals seeing large initial contributions as a signal of the charity's quality. This result corresponds with field evidence that new donors are more sensitive to information on past contributions than are continuing donors.

Lab and field experiments each add unique and complementary insights to our understanding of economic behavior. Discussions aiming to secure a relative ranking of the two methodologies are both unwarranted and unproductive. Instead, methodological discussions should highlight the ways in which laboratory and field experiments are complements. And ideally, those discussions will spark new research that takes advantage of their combined strengths.

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<sup>22</sup> For example, reciprocity may by itself generate a response to information about the contributions of others.

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