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Reply to Signorino

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We thank *Political Analysis* for the opportunity to engage Professor Signorino's response to our article. Although we believe that Signorino has mischaracterized our argument in a number of ways, we also feel that—rather than engaging in a point-by-point rebuttal—the most productive use of this forum is to briefly underscore our points of agreement and disagreement with Signorino on how one conducts good theoretical and empirical social science.

As we indicate in our article, we share Signorino's conviction that formal models can be an invaluable tool for ensuring that one has a clear understanding of the behavioral implications of one's theory, particularly in complex strategic environments. We also agree with what we see as Signorino's central contribution: "that, in the presence of strategic behavior, researchers must work hard to ensure that their empirical model accurately tests the predictions generated by the theory" (Carrubba, Yuen, and Zorn, 2007 [16]). In sum, we find little basic disagreement: write down a model that accurately represents one's theory, use that model to deduce equilibrium behavior, generate a set of behavioral predictions, and ensure that your empirical estimator correctly operationalizes those predictions.

Beyond this, however, we diverge somewhat from Signorino's prescriptions. Simply put, we believe that Signorino has not effectively made the case that, in the majority of circumstances, executing this agenda requires any innovation beyond the standard empirical tools already widely employed in the discipline and that his approach can make theory building and testing more complex than necessary. Our article makes that case vis-a-vis his existing body of work, and we do not see anything in his response that prompts us to reconsider that position.

Signorino's response contains four main critiques of our argument: that our proposed approach to theory building and testing is in fact no less complex than his approach; that although we emphasize the role of comparative statics, we do not rely upon comparative statics when deriving our estimators; that, contrary to what we argue, uncertainty will often do more than smooth equilibrium relationships; and that none of our proposed estimators are new.

To clarify these points, consider first our two general approaches. Signorino's preferred approach entails deriving a stochastic model and then writing an estimator with which one directly estimates the parameters of that model. As an example, in an earlier work, appendix A in Signorino (1999) provides proofs for a simple stochastic model, equations

(4–7) provide the derived choice probabilities, and equations (18–20) provide the likelihood function for the empirical model one would estimate. Since quantal response uncertainty is employed primarily for estimation purposes, we would instead use a deterministic version of the model to identify equilibrium behavior (Carrubba, Yuen, and Zorn, 2007 [7–8]). Although there is admittedly one less node in the deterministic game, we believe its relative simplicity is self-evident. Once the solutions to the model are derived, scholars identify how the equilibrium behavior of interest changes with a change in the model's components (e.g., using algebra, plots, and so forth). This is all we mean by employing "comparative statics." From such an analysis, we can know exactly what the predicted relationships are and therefore what covariates must be included in the empirical analysis and how they should be included (e.g., interacted or not). As we discuss (Carrubba, Yuen, and Zorn, 2007 [11–12], in the proofs), the choice to use the simple interaction model (Carrubba, Yuen, and Zorn, 2007, equation [15]) or one of the others is then a matter of taste and data availability.

Seen in this light, comparative statics provide the researcher with the information necessary to know how to operationalize the functional form of the estimator. We provide equations (5) and (6) to motivate the Monte Carlo analyses in equations (7–17) not because we need to rely upon them to generate the estimators, but rather to demonstrate that we lose nothing by not relying upon these equations and instead relying upon the comparative statics provided in the previous section. Put differently, Section 3 of our article can be viewed as a proof that relying upon comparative statics from Section 2 is sufficient for correctly specifying the empirical estimator; these parts are not two separate papers, but rather two parts of a common proof. Evidently, we were insufficiently clear on this point.

Note also that the difference in the complexity of these two approaches is relatively small with such simple models. However, most theories are far more complex than those analyzed in these articles. The more choices available to each actor and the greater the number of actors, the more difficult writing a random utility or QRE version of the model is going to be. The same holds for the empirical estimator: a full structural estimator that simultaneously models all the choices will be far more challenging to estimate as the theoretical model's complexity increases. However, using a simple comparative statics analysis, it is a comparatively trivial exercise to identify a subset of important predictions to test. A similar dynamic occurs when (as is often the case) the researcher wishes to compare two or more competing models empirically. In such instances, full structural estimation of all the models examined will necessarily be far more complex than careful selection and testing of specific predictions where the models in question yield differing expectations at the margin.

Signorino (10–14) also asserts that we incorrectly dismiss the importance of modeling uncertainty. This point is important: if he is correct, our claim that one can intuit a probabilistic relationship from a deterministic model is fundamentally incorrect, and much of the gain in simplicity from our approach is undermined. Of course, we certainly agree that uncertainty can critically affect equilibrium behavior in a model; any signaling game adequately demonstrates this point. The issue is how uncertainty in the form of random utility and quantal response modeling affects equilibrium behavior.

To start, it is critical to distinguish between point predictions and comparative statics predictions. Adding these types of uncertainty can certainly affect the former, as Signorino's example demonstrates: if the expected utility of player 1 playing A is less than the payoff from playing $\sim A$, but the fixed payoff from playing A anticipating that player 2 will play R with certainty is larger than the payoff from playing A, one derives different equilibrium predictions. As Signorino demonstrates, for large enough uncertainty, this situation will arise. However, it does not necessarily follow that a test of the model's equilibrium predictions will differ as well. In our article, we focus on a comparison of the graphical solution space and identify the difference in smoothing for ease of comparison to Signorino's previous work. We can also characterize the deterministic solution algebraically:

Player 2 plays R if $u_2(\text{war}) \geq u_2(\text{cap})$,

Player 1 plays A if $u_1(\text{cap}) \geq u_1(\text{sq})$ when $u_2(\text{war}) \geq u_2(\text{cap})$, or if $u_1(\text{cap}) \geq u_1(\text{sq})$ when $u_2(\text{war}) < u_2(\text{cap})$,

Player 1 plays $\sim A$ otherwise.

Any researcher who treats their model as a simplification of reality will think probabilistically about the expected relationships generated by this model: The larger u_2(war) and the smaller u_2(cap), the more likely player 2 retaliates; the larger u_1(sq), the more likely player 1 is to attack; and, most important, the larger u_1(war), given a larger u_2(war) and smaller u_2(cap), and the larger u_1(cap), given a smaller u_2(war) and larger u_2(cap), the more likely player 1 attacks (i.e., the effects are interactive). These expectations lead the researcher to employ any of the estimators in Section 3 of our paper, exactly as the stochastic version of the model does. Signorino's special case with fixed payoffs does not counter our argument. By fixing all the parameters and only allowing the uncertainty to vary, Signorino does not allow expectations over variation in the parameter payoffs. Without such variation, there is no way to generate the expectations in the deterministic model we describe above.

Of course, this is not a general proof. It simply demonstrates that Signorino has not established that a deterministic model, properly empirically operationalized, necessarily leads to different predictions, and thereby model estimation. That said, we believe a more general claim certainly follows and a universal one is likely true as well. Why? This class of stochastic models simply makes every decision an expected utility calculation. By thinking about predicted behavior in a deterministic model probabilistically, the researcher induces the same calculations.

Finally, Signorino goes to some length to point out that none of the estimators in Section 3 of our article are new. Here again, we completely agree; in fact, a central point—arguably, the central point—of our article is that existing empirical techniques are often more than sufficient for testing models of strategic behavior. As we note in our article (Carrubba, Yuen, and Zorn, 2007, n. 12), such approaches have been widely (and appropriately) used to model strategic behavior in fields as diverse as political science, economics, and geography. Moreover, to the extent that our "minimalist" characterization of Signorino's existing work is correct—that is, that he takes issue only with linear-additive (mis)specifications of strategic behavior—we see even less disagreement on our most basic point: that carefully identifying and operationalizing a theoretical model's predictions are sufficient to ensure such a mistake is not made.

References

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