Political Analysis Advance Access published October 15, 2007

Political Analysis (2007) 15:1–19
doi:10.1093/pan/mpm027

On Formal Theory and Statistical Methods: A Response to Carrubba, Yuen, and Zorn

Curtis S. Signorino
Department of Political Science
303 Harkness Hall, University of Rochester, Rochester, NY 14627
e-mail: sign@mail.rochester.edu

The recent article by Carrubba, Yuen, and Zorn (2007) (CYZ) attempts to relate the strategic random utility models in Signorino (1999, 2002, 2003) and in Signorino and Yilmaz (2003) to existing game theory practice and to existing statistical techniques. It contributes to this literature by reminding us that comparative statics analysis can be applied to the equilibria of these models. There are a number of claims in CYZ, however, that require clarification. In particular, the article's primary claim is that comparative statics analysis, in combination with one of three proposed statistical estimators, provides a simpler alternative to methods previously advocated. This claim (or combination of claims) is incorrect. When one examines the procedure CYZ recommends, it is no simpler for substantive researchers than anything previously recommended. Moreover, none of the proposed estimators are new: they are exactly the same methods introduced in Signorino (1999, 2003), in Signorino and Yilmaz (2003), in Signorino, Walker, and Bas (2002), and in Bas, Signorino, and Walker (2007).

1 Introduction

Given the prevalence of strategic behavior in politics, developing appropriate theoretical and methodological tools for analyzing such behavior is an important task for political scientists. For various reasons, the two subfields—formal theory and statistical methods—have evolved independently of each other, without much reflection on or regard for the other. Fortunately, this seems to be changing. The last decade has seen an increased focus on methods that take formal theory seriously. In general, the goals have been (1) more rigorous, empirically relevant theory and (2) a closer connection of statistical models and methods to that theory.

Because this area is relatively new, questions naturally arise concerning how various "strategic" statistical techniques relate both (1) to existing "standard" statistical methods and (2) to existing game theoretic models. Assumptions are made in any modeling enterprise, whether game theoretic or statistical. Simplifying assumptions made by game theorists often do not simplify matters from a statistical (or econometric) perspective. Similarly, simplifying assumptions made by methodologists may not line up exactly with standard game theory practice. As we iterate through this process of comparing theory and methods, areas of each are illuminated, hopefully clarified, and when necessary, modified.

Authors' note: I am indebted to David Carter, Justin Esarey, Gretchen Helmke, Yukari Iwanami, Jeremy Kedziora, Gary King, Shawn Ramirez, Arthur Spirling, and Robert Walker for their valuable comments.

The recent article by Carrubba, Yuen, and Zorn forthcoming (hereafter referred to as "CYZ") serves a useful purpose in this enterprise. The article attempts to relate the strategic random utility models in Signorino (1999, 2002, 2003) and in Signorino and Yilmaz (2003) to existing game theory practice and to existing statistical techniques. It contributes to this literature by reminding us that comparative statics analysis can be applied to the quantal response equilibria (QRE) and Nash equilibria of these models. There are a number of claims in CYZ, however, that require clarification. In particular, the article's primary claim is that comparative statics analysis, in combination with one of three proposed statistical estimators, provides a simpler alternative to methods previously advocated. This claim (or combination of claims) is incorrect.

CYZ is really two separate articles: one focusing on formal theory and comparative statics analysis and the other focusing on statistical estimators. I will address in Section 3 the main theoretical claim concerning comparative statics. There, I demonstrate that, although CYZ makes a contribution in linking comparative statics analysis to strategic random utility models, comparative statics analysis is much less relevant to the broader argument than portrayed in CYZ. Indeed, to the extent that comparative statics plays a role in CYZ, it is really a secondary role: comparative statics analysis is secondary to model specification and derivation of equilibrium conditions. Because of that, comparative statics are actually unnecessary for demonstrating many of CYZ's theoretical points. Moreover, when one examines the procedure CYZ recommends, it is no simpler for substantive researchers than anything recommended in Signorino (1999, 2002, 2003), Signorino and Yilmaz (2003), or Signorino and Tarar (2006). A number of issues concerning nonlinearity and uncertainty are also clarified in this section.

In Section 4, I turn to the proposed alternative estimators. Interestingly, despite CYZ's theoretical emphasis on comparative statics, none of the statistical estimators proposed in CYZ require comparative statics analysis or are directly linked to comparative statics by the authors. Moreover, none of the proposed estimators are actually new: they are the same methods introduced in Signorino (1999, 2003), in Signorino and Yilmaz (2003), in Signorino, Walker, and Bas (2002), and in Bas, Signorino, and Walker (forthcoming).

2 Referent Model

In the following analysis, it will be useful to refer to the simple game employed in CYZ and in Signorino and Yilmaz (2003). Figure 1 displays two versions of this game. In both,

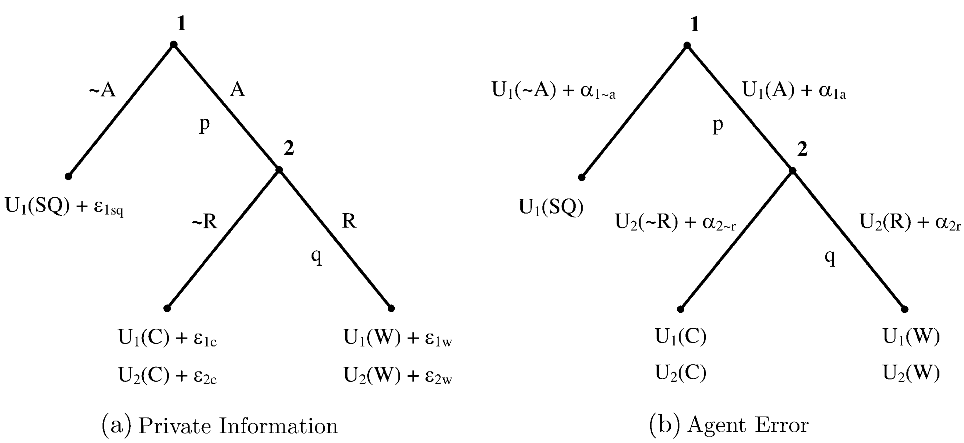


Fig. 1 Simple games with assumptions of private information and agent error.

player 1 must decide whether to attack (A) or not (∼A). If attacked, player 2 must then decide whether to resist (R) or not (∼R). The game has three outcomes: status quo (SQ), capitulation by player 2 (C), and war (W).

The games in Fig. 1a and 1b differ in their information assumptions. The game in Fig. 1a is one with private information (the ε's) and has a Nash equilibrium. Player j's true utility for an outcome is denoted by $U_j^*(\cdot) = U_j(\cdot) + \varepsilon_j$, where $U_j(\cdot)$ is the part of the utility that is observable by the other player and by the analyst. We assume that the players do not observe each other's ε's—the private information. We also assume that the analyst does not observe the ε's. Although it is not necessary, for simplicity we will also assume that the players and analyst share the same (i.i.d.) assumption concerning the distribution of the ε's. Given those assumptions, we can derive the equilibrium probabilities p_ε and q_ε. From the analyst's perspective, the probability that player 1 attacks is

$$p_\varepsilon = \Pr[U_1^*(A) > U_1^*(\sim A)] \tag{1}$$

$$= \Pr[q_\varepsilon(U_1(W) + \varepsilon_{1w}) + (1 - q_\varepsilon)(U_1(C) + \varepsilon_{1c}) > U_1(\mathrm{SQ}) + \varepsilon_{1\mathrm{sq}}] \tag{2}$$

$$= \Pr[\varepsilon_{1\mathrm{sq}} - q_\varepsilon\varepsilon_{1w} - (1 - q_\varepsilon)\varepsilon_{1c} < q_\varepsilon U_1(W) + (1 - q_\varepsilon)U_1(C) - U_1(\mathrm{SQ})] \tag{3}$$

$$= F_\varepsilon[q_\varepsilon U_1(W) + (1 - q_\varepsilon)U_1(C) - U_1(\mathrm{SQ})] \tag{4}$$

$$= F_\varepsilon[U_1(C) - q_\varepsilon(U_1(C) - U_1(W)) - U_1(\mathrm{SQ})], \tag{5}$$

where F_ε is the c.d.f. of $\varepsilon_{1\mathrm{sq}} - q_\varepsilon\, \varepsilon_{1w} - (1 - q_\varepsilon)\, \varepsilon_{1c}$. From the analyst's and player 1's perspective, the probability that player 2 will resist is

$$q_\varepsilon = \Pr[U_2^*(W) > U_2^*(C)] \tag{6}$$

$$= \Pr[U_2(W) + \varepsilon_{2w} > U_2(C) + \varepsilon_{2c}] \tag{7}$$

$$= \Pr[\varepsilon_{2c} - \varepsilon_{2w} < U_2(W) - U_2(C)] \tag{8}$$

$$= G_\varepsilon[U_2(W) - U_2(C)], \tag{9}$$

where G_ε is the c.d.f. of $\varepsilon_{2c} - \varepsilon_{2w}$.

Figure 1b is very similar. However, rather than private information concerning outcomes, Fig. 1b is one with "agent error" (the α's), resulting in a QRE (McKelvey and Palfrey 1998). Here, the outcome payoffs are assumed to be observed. It is the expected utilities for actions that are perturbed. We assume that player j's true utility for an action $U_j^*(\cdot) = U_j(\cdot) + \alpha_j$ can be decomposed into an observable component ($U_j(\cdot)$) and a component that is unobserved by the other player (α_j) and by the analyst. Based on those assumptions, we can derive the equilibrium probabilities p_α and q_α. The probability that player 1 attacks is

$$p_\alpha = \Pr[U_1^*(A) > U_1^*(\sim A)] \tag{10}$$

$$= \Pr[q_\alpha U_1(W) + (1 - q_\alpha)U_1(C) + \alpha_{1a} > U_1(\mathrm{SQ}) + \alpha_{1\sim a}] \tag{11}$$

$$= \Pr[\alpha_{1\sim a} - \alpha_{1a} < q_\alpha U_1(W) + (1 - q_\alpha)U_1(C) - U_1(\mathrm{SQ})] \tag{12}$$

$$= F_\alpha[q_\alpha U_1(W) + (1 - q_\alpha)U_1(C) - U_1(\mathrm{SQ})] \tag{13}$$

$$= F_\alpha[U_1(C) - q_\alpha(U_1(C) - U_1(W)) - U_1(\mathrm{SQ})], \tag{14}$$

where F_α is the c.d.f. of $\alpha_{1\sim a} - \alpha_{1a}$. The probability that player 2 resists is

$$\begin{aligned} q_\alpha &= \Pr[U_2^*(R) > U_2^*(\sim R)] && (15)\\ &= \Pr[U_2(R) + \alpha_{2r} > U_2(\sim R) + \alpha_{2\sim r}] && (16)\\ &= \Pr[U_2(W) + \alpha_{2r} > U_2(C) + \alpha_{2\sim r}] && (17)\\ &= \Pr[\alpha_{2\sim r} - \alpha_{2r} < U_2(W) - U_2(C)] && (18)\\ &= G_\alpha[U_2(W) - U_2(C)], && (19) \end{aligned}$$

where G_α is the c.d.f. of $\alpha_{2\sim r} - \alpha_{2r}$.

Although Signorino (2003) already discusses these two games in much more detail, it is important to note at least two points before we proceed. First, it is common to assume that the stochastic components (the ε's and α's) are distributed either normal or logistic. Although this will result in different equations for the equilibrium probabilities (e.g., probit versus logit), the relationships expressed in those equilibrium probabilities will be the same. In regression analysis, it is generally immaterial whether one estimates a logit versus probit regression. Similarly, it substantively makes no difference whether one specifies the equilibrium probabilities as logit or probit probabilities. Although the analysis in CYZ is conducted using logit probabilities, I will use probit probabilities in this article for pedagogical purposes.

Second, as Signorino (2003) demonstrates, the equilibrium probabilities for the private information and agent error models will be almost identical for simple games like that in Fig. 1. Let us assume that the ε's and α's are independent and identically distributed. As one can see from comparing the private information equations (1–9) to the agent error equations (10–19), the equilibrium probabilities for player 2 will be identical and the probabilities for player 1 will be very similar, differing only in a variance term. The systematic relationships between the equilibrium behavior and the utilities are essentially identical.

The upshot of this is that, for the models in Fig. 1, it is of no practical consequence (1) whether we employ the (Nash) private information model or the (QRE) agent error model and (2) whether we assume the stochastic terms are distributed normal or logistic. The main substantive points will be the same. For a number of examples in later sections, I will therefore reference the private information model (Fig. 1a) and assume the stochastic terms are i.i.d. $N(0, \sigma^2)$, resulting in equilibrium probabilities

$$p = \Phi\left[\frac{q\, U_1(W) + (1-q)U_1(C) - U_1(\mathrm{SQ})}{\sqrt{\sigma^2(q^2 + (1-q)^2 + 1)}}\right] \tag{20}$$

and

$$q = \Phi\left[\frac{U_2(W) - U_2(C)}{\sqrt{2\sigma^2}}\right]. \tag{21}$$

The outcome probabilities are the product of the probabilities along the path: $p_{\mathrm{sq}} = 1 - p$, $p_c = p\,(1-q)$, and $p_w = pq$.

Assuming one has data for the players' decisions and regressors for the utilities, then one can estimate parameters via maximum likelihood estimation (MLE).[1] In this case, all utilities and probabilities are indexed by observation i. Let the dummy variables $y_{i,\text{sq}}$, $y_{i,c}$, and $y_{i,w}$ denote which outcome (SQ, C, or W) occurred in observation i. As shown in Signorino (1999, 285; 2002, 101; 2003, 322), the likelihood function to be maximized would take the form

$$L = \prod_{i=1}^{N} p_{i,\text{sq}}^{y_{i,\text{sq}}} p_{i,c}^{y_{i,c}} p_{i,w}^{y_{i,w}}. \tag{22}$$

Suppose instead that one only had data on player 1's actions but could still specify player 2's utilities with regressors—and therefore estimate q_i for each observation i. Denote $y_{i,a} = 1$ if player 1 attacks. As shown in Signorino (1999, 285; 2003, 323), the likelihood equation to be maximized in this case is

$$L = \prod_{i=1}^{N} p_i^{y_{i,a}} (1 - p_i)^{1-y_{i,a}}. \tag{23}$$

3 Formal Theory, Comparative Statics Analysis, and Uncertainty

With the model basics in hand, we can now proceed to the primary claims made in CYZ. In this section, I demonstrate that the comparative statics analysis proposed in CYZ is neither simpler nor qualitatively different than anything recommended in Signorino (1999, 2002, 2003), Signorino and Yilmaz (2003), or Signorino and Tarar (2006). CYZ makes a number of other claims concerning functional form, nonlinearity, and the role of uncertainty. Some of these are incorrect. Others are correct but not new.

3.1 *Minding Our P's and Q's: Same Formal Model, Different Ways of Viewing It*

CYZ claims to demonstrate that the nonlinear equilibrium relationships in the models in Signorino (1999) can be derived through comparative statics analysis. Technically, that is true. However, as we will see, it is not surprising.

CYZ begins with an analysis of the simple game in Signorino and Yilmaz (2003), which was reproduced in Fig. 1. In Comparative Statics Analysis and Deterministic Modeling, CYZ derives the equilibrium conditions for the model. It is important to note that these are the same as those shown in equations (5) and (9), having normalized $U_1(\text{SQ}) = 0$ and $U_2(C) = 0$:

$$p = F\{U_1(C) - q[U_1(C) - U_1(W)]\}. \tag{24}$$

$$q = G[U_2(W)]. \tag{25}$$

At this point in the analysis, CYZ has not yet assigned specific densities to the stochastic terms. As before, F and G are the c.d.f.'s associated with the left-hand side functions of the stochastic terms in equations (3) and (8).

[1] It is also relatively simple to estimate the Bayesian posteriors for the parameters using Markov Chain Monte Carlo simulation—e.g., using WinBUGS.

Comparative statics analysis is the analysis of how equilibria change with respect to changes in model parameters. Suppose the equilibrium choice is a continuous and differentiable function of the model parameters, then comparative statics analysis is typically conducted by taking the first derivative of the equilibrium choice with respect to each model parameter. By doing so, one can characterize the effects of parameters on the equilibrium. It is analogous to analyzing marginal effects in a regression equation.

CYZ provides the first derivatives of p and q with respect to the players' utilities. For reference, I reproduce these equations here:

$$\frac{\partial q}{\partial U_2(W)} = g[U_2(W)] \tag{26}$$

$$\frac{\partial p}{\partial U_1(W)} = f\{U_1(C) - G[U_2(W)][U_1(C) - U_1(W)]\} \times G[U_2(W)] \tag{27}$$

$$\frac{\partial p}{\partial U_1(C)} = f\{U_1(C) - G[U_2(W)][U_1(C) - U_1(W)]\} \times \{1 - G[U_2(W)]\} \tag{28}$$

$$\begin{aligned}\frac{\partial p}{\partial U_2(W)} &= f\{U_1(C) - G[U_2(W)][U_1(C) - U_1(W)]\} \\ &\quad \times [U_1(W) - U_1(C)] \times g[U_2(W)],\end{aligned} \tag{29}$$

where f and g are the first derivatives of F and G, respectively. CYZ then conducts comparative statics analysis, assessing how changes in players' utilities will affect the equilibrium probabilities. CYZ concludes that "these relationships yield exactly the relationships that Signorino and Yilmaz derive in their MLE. In particular, note that we have derived, through standard comparative statics, both the nonlinear and the 'conditionally monotonic' relationships illustrated in Fig. 5 of Signorino & Yilmaz (2003, 562) and reproduced below as Fig. 3." For reference, Fig. 2 reproduces the equilibrium probability of attacking shown in Fig. 5b of Signorino and Yilmaz (2003, 562) and in CYZ's Fig. 3. The equilibrium probability of attacking is plotted as a function of $U_2(W)$ and $U_1(C)$, holding $U_1(W)$ constant at zero. CYZ's replication of Fig. 2 and analysis of comparative statics are instructive for a number of reasons.

First, we should note that the relationships shown in the Fig. 2 version of Signorino and Yilmaz (2003, 562) are not "derived in an MLE." In fact, it is quite the opposite. The formal model specified in Signorino and Yilmaz (2003, 554–5) is used to derive the equilibrium probabilities p and q, which are then employed in a likelihood model. The Monte Carlo analysis of Signorino and Yilmaz uses MLE to demonstrate that the strategic estimator correctly recovers the parameters of the model on average, whereas the misspecified logit model does not. Given correct parameters, however, the MLE is actually irrelevant to plotting the relationship shown in the Fig. 2 version of Signorino and Yilmaz. Whether we simply plotted the true equilibrium probability surface for p—as in Fig. 2 above—or the estimated probability surface based on the average results of the Monte Carlo experiments does not matter, since the strategic model recovered the true parameters on average.

Given that, what has CYZ demonstrated with its version of Fig. 2? CYZ's plot is based on the private information model's equilibrium probabilities p and q in equations (5) and (9), "using a type 1 extreme value distribution." The plot of Signorino and Yilmaz (2003,

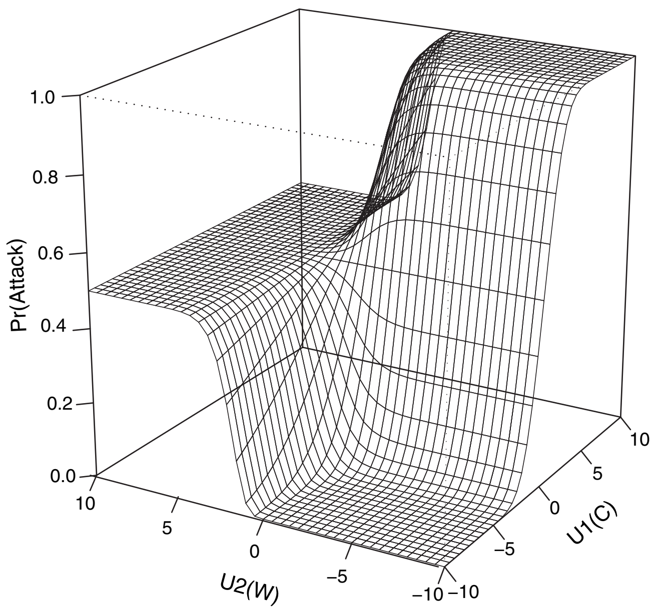


Fig. 2 Effect of $U_2(W)$ and $U_1(C)$ on the equilibrium probability of attacking. The figure reproduces the relationship shown in Fig. 5(b) of Signorino and Yilmaz (2003, 562) and Fig. 3 of CYZ.

562) was based on the agent error model's equilibrium probabilities in equations (14) and (19). As I noted in Section 2 and explain in more detail in Signorino (2003), these are essentially identical probability models. It should not be surprising that CYZ replicates a plot of the relationships in Signorino and Yilmaz when it has used essentially the same probability model. The versions of Fig. 2 of Signorino and Yilmaz and CYZ simply demonstrate the similarity of the two probability models. Moreover, notice that in plotting its version of Fig. 2, CYZ did not require or make use of the comparative static equations (26–29) in any way.

Second, and really the key point here, is that the underlying formal model is the center of this exercise, not comparative statics analysis. After specifying the model, the most important step is to derive the equilibrium—here, represented by the probabilities p and q. Once that is done, there are a number of ways one might analyze or interpret the relationships embodied in the equilibrium. Comparative statics analysis is just one tool for analyzing the equilibrium conditions. As the Fig. 2 versions of Signorino and Yilmaz (2003) and CYZ show, plotting the equilibrium relationships is another. Comparative statics analysis and plots of equilibrium probabilities should be consistent with each other when they are both based on the same equilibrium model. The analog, again, to regression is analyzing the conditional expectation of the dependent variable. We can analyze $E(Y|X)$ in a number of ways: via an analysis of marginal effects, using first differences, or by calculating or plotting fitted values—each of which should be consistent with the other, since they are all based on the same $E(Y|X)$.

If there is a contribution in CYZ, it is the demonstration of comparative statics analysis with respect to the strategic equilibrium probabilities. To my knowledge, no one has yet taken the derivatives of the equilibrium probabilities p and q and interpreted them as CYZ does. That said, comparative statics analysis plays a secondary role here and its overemphasis only obfuscates the main point: the need for a formal model, derivation of the equilibrium, and closely linking the statistical model to the equilibrium relationships. Indeed, in much of CYZ, if one simply replaced the phrase "comparative statics" with "formal model" or "equilibrium conditions," the argument is clearer—but it has been made before. For example, in discussing the results from the Monte Carlo analysis in Signorino (1999), suppose we rephrased CYZ's sentence as "We certainly agree that Signorino has demonstrated the failure of these typical logits. [N]one of the three logits estimated by Signorino rely upon correctly derived [equilibrium conditions]." Or suppose, when in concluding the section on comparative statics, CYZ instead wrote that "much of the failure of traditional logit analysis can be resolved with more careful [derivation of equilibrium conditions]." Not only are these more accurate statements but also they are precisely the points in Signorino (1999) and in Signorino and Yilmaz (2003).

Finally, CYZ repeatedly makes the claim that it offers simpler alternatives. That of course begs the question: what is actually simpler here? and for whom? CYZ's demonstration of comparative statics required that the researcher (1) specify a formal model, (2) derive the equilibrium, and then (3) analyze the first derivatives. As conducted in CYZ, this is actually no simpler than anything proposed in Signorino (1999, 2002, 2003), Signorino and Yilmaz (2003), or Signorino and Tarar (2006). As I have just demonstrated, comparative statics analysis is an alternative technique to plotting the relationships. However, both should produce the same results, given the same underlying model. Although comparative statics analysis may feel more natural for some game theorists, it will almost certainly not be simpler for the broader array of substantive researchers. Returning to the regression effects analogy, one might ask: how often do substantive researchers take derivatives and analyze those, versus plotting fitted values or calculating first differences? For most political scientists, taking derivatives is not the simpler option.[2]

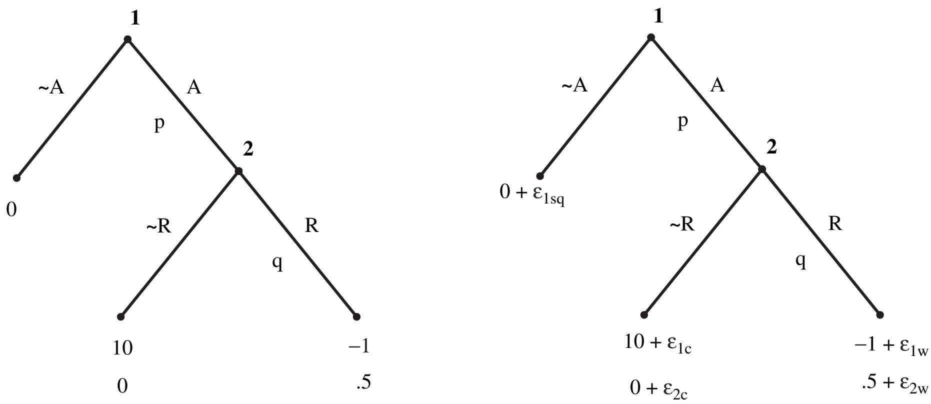
3.2 *Functional Form, Nonlinearity, and the Role of Uncertainty*

The preceding section addresses CYZ's claim that comparative statics analysis is somehow qualitatively different and simpler than techniques previously proposed. Before turning to the statistical estimators, we should clarify a few other theory-related claims.

In its section demonstrating comparative statics, CYZ reaches a number of conclusions concerning functional form, nonlinearity, and the role of uncertainty. The overarching point is that the nonlinear relationships in strategic random utility models are due to the structure of the game and the incentives, not due to the uncertainty on the part of the analyst or players. At times, it is not clear in this section (Nonlinearities, Stochastic Modeling, and Comparative Statics Analysis) whether these claims are assumed to be new or whether CYZ is attempting to illustrate results established in previous published work. The statements are generally true concerning how the structure of the game, including decision sequence and players' utilities, can create nonlinear relationships between parameters of the model and players' actions. However, these are already well documented in Signorino (1999, 279, 280, 282–3; 2003, 319–20), Signorino and Yilmaz (2003, 557), and Signorino and Tarar (2006, 589).

Some of CYZ's claims concerning the role of uncertainty are also true. For example, uncertainty is not required for nonlinearity. CYZ demonstrates this in its Fig. 6, where the

[2]To be clear, I am not arguing that plotting relationships is intrinsically superior to comparative statics analysis. For the researcher who is comfortable with taking and interpreting derivatives, analyzing comparative statics may be a more general (and powerful) tool. There is certainly more information summarized in the derivatives than in one plot. The same can be said for the analysis of marginal effects versus fitted values. A more general tool is not, however, always a simpler tool for substantive political scientists.



(a) Deterministic, SPE={~A,R}

(b) Private Information

Fig. 3 Example with specific payoffs.

logit quantal response equilibrium (LQRE) and subgame perfect equilibrium are plotted for the crisis model in Signorino (1999). However, it is already well known (see, for example, McKelvey and Palfrey [1998]) and Signorino [1999, 283; 2003, 338]) that the SPE is a special case of the LQRE and of the other strategic models in Signorino (2003). For recursive models, it is the special case where there is no uncertainty—i.e., the limiting case as the variance of the error terms goes to zero. Uncertainty was employed in Signorino (1999) and in Signorino (2003) not to induce nonlinearities but, in the former, to avoid the zero-likelihood problem (1999, 281) and, in the latter, as the subject of theoretical and econometric interest (in particular, to examine how the different sources of uncertainty interacted with the rest of the game structure to produce different equilibria).

Although uncertainty is not necessary for nonlinearities to exist, CYZ gives the impression that uncertainty does not matter or can be tacked on at the end, prior to estimation. CYZ states that "Writing down a deterministic model, carefully deriving predictions from that model, and recognizing that the world is not deterministic when generating the empirical estimator is enough to ensure consistency between one's theory and test" (Conclusion; see also footnote 8). This statement is true only when uncertainty does not exist on the part of players in the theoretical model—e.g., only when the players have perfect and complete information and it is only the analyst who is uncertain about, say, regressor specification. Otherwise, given what we know about the role of uncertainty in game theory, this claim is false. The critical difference between models with private information versus those without is that players in the private information setting must take the uncertainty into account in determining best responses.[3] Adding a modest amount of private information to a model without uncertainty can greatly change equilibrium predictions, not to mention result in multiple equilibria.

The preceding claim results in part from CYZ's attempt to demonstrate the related claim that games with uncertainty are simply smoothed versions of deterministic models. CYZ's Figs. 4 and 6 certainly give this impression. Although games with theoretical uncertainty (including LQRE or private information models in Signorino [2003]) can

[3]Note that this is also one of the defining features of the QRE: in formulating best responses, players take into account that their opponents may make mistakes.

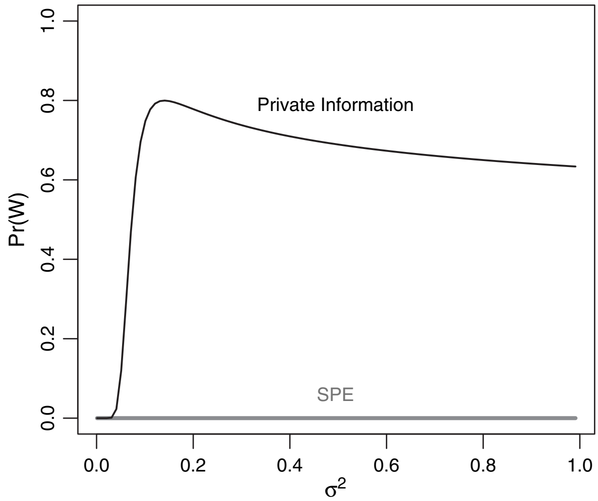


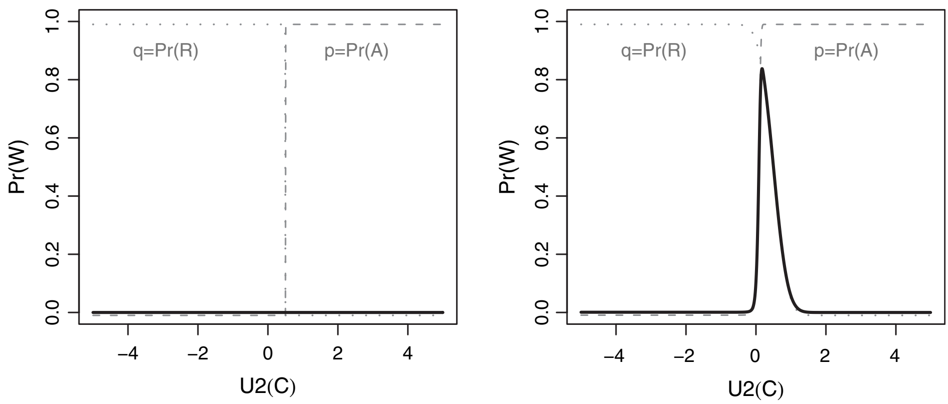
Fig. 4 Effect of uncertainty on equilibrium probability of war. The black line displays the equilibrium probability of war for the private information model. The gray line shows the SPE prediction for the deterministic model.

under certain specifications of utilities appear to be no more than smoothed versions of the complete information versions, this is not always the case. Consider the examples shown in Fig. 3, where player 1's observable payoffs have been set to $U_1(\mathrm{SQ}) = 0$, $U_1(C) = 10$, and $U_1(W) = -1$ and player 2's observable payoffs are $U_2(C) = 0$ and $U_2(W) = .5$. Figure 3a displays the game with no uncertainty. In this case, player 2 prefers war (W) to capitulation (C). Player 1 prefers most of all that player 2 capitulates but otherwise prefers the status quo (SQ) to war. In this game, the subgame perfect equilibrium is (∼A,R), leading to the SQ outcome.

Now consider Fig. 3b, which depicts the same case but with private information (and equivalent analyst uncertainty). Here, player 1 is uncertain about player 2's true preferences. For simplicity, we also assume that the analyst shares the same uncertainty and is similarly uncertain about player 1's true preferences. As before, we will assume that the stochastic terms are i.i.d. $N(0, \sigma^2)$. In this case, the equilibrium probabilities (equations 20 and 21) depend not just on the preference ordering but also on the relative size of the observable utilities and on the variance of the uncertainty. When player 1 factors this uncertainty into her expected utility calculation for attacking (A), this can greatly change her optimal response, relative to the subgame perfect equilibrium under complete information.

Figure 4 demonstrates the effect of the uncertainty on the equilibrium probability of war and compares that to the subgame perfect equilibrium under complete information. When $\sigma^2 = 0$, there is no uncertainty—we have a complete information game and the equilibrium probability is exactly the subgame perfect equilibrium: SQ, resulting in Pr $(W) = 0$. However, as the uncertainty increases (as denoted by its variance), the likely outcome is very different from the SPE. Notice that for $\sigma^2 > .1$, war is highly likely in the private information model.

The intuition for the equilibrium behavior in Fig. 4 is fairly straightforward. When σ^2 is extremely small, say $\sigma^2 = .01$, player 1 knows that player 2 will almost certainly go to war if attacked: $q \approx .9998$. Given q, player 1's utility for attacking is $U_1^*(A) = q(-1 + \varepsilon_{1w}) + (1 - q)(10 + \varepsilon_{1c}) = -.9978 + .9998\varepsilon_{1w} + .0002\varepsilon_{1c}$. In comparison, player 1's utility for not attacking is $U_1^*(\mathrm{SQ}) = 0 + \varepsilon_{1\mathrm{sq}}$. Since in this numerical example



(a) Deterministic SPE (b) Private Information, $\sigma^2 = .05$

Fig. 5 Comparison of equilibrium probabilities of war.

$\varepsilon \sim N(0, .01)$, it is highly unlikely that $U_1^*(\mathrm{SQ}) < U_1^*(A)$. In fact, the probability that player 1 attacks is $p < .0001$ and the probability of war is $\Pr(W) < .0001$.

In contrast, when σ^2 is only a little larger, say $\sigma^2 = .2$, player 1 is no longer as certain about player 2's response, especially since player 2's observable utilities are relatively close in comparison to the variance of the stochastic term. For $\sigma^2 = .2$, the probability that player 2 resists is $q = .78$. Although this still seems fairly high, the change has a large effect on player 1's expected utility calculation. The chance that player 2 capitulates and the benefits from it now likely outweigh the cost of war. In this case, $U_1^*(A) = 1.36 + .785\varepsilon_{1w} + .215\varepsilon_{1c}$. Given that $\varepsilon \sim N(0, .2)$, it is unlikely that $U_1^*(\mathrm{SQ}) > U_1^*(A)$. Now, the probability that player 1 attacks is $p \approx .99$ and the probability of war is $\Pr(W) \approx .778$. Thus, although it is true that the stochastic model's equilibrium probabilities are continuous (and differentiable) in σ^2, Fig. 4 demonstrates that models with uncertainty are not just smoothed versions of deterministic models. The deterministic versus private information equilibrium predictions may be very different for a given level of σ^2.[4]

One might argue that CYZ's "smoothness" claim focuses on changes in equilibria as a function of utilities, not on uncertainty variance. However, from a regression perspective, this is immaterial, since we cannot estimate the regression parameters and σ^2 individually—as Signorino (1999, 2003) notes, they are unidentified, just as in any standard logit or probit regression. In MLE, as the optimization routine searches over values of the regression parameters, it is actually searching over values representing the ratio of the parameters over σ^2 (at least in the probit case). Therefore, one cannot separate the effects of the variance and utilities in analyzing their effects on the equilibrium probabilities.[5]

We can, on the other hand, separate the effects from a theoretical perspective. As an example, let us consider how changes in player 2's utility for capitulation affects the equilibrium. Figure 5a displays the subgame perfect equilibrium predictions for the deterministic model in Fig. 3a when varying $U_2(C)$. Figure 5b displays the equilibrium probabilities for the private information model in Fig. 3b as a function of $U_2(C)$, while

[4] Some readers may be uncomfortable with this example since player 1 is essentially "tempted" into war by increased uncertainty. Signorino (2003, 338, Fig. 3) provides a similar example, but one where player 1 makes a different choice under uncertainty (than she would have made under certainty) in order to "avoid" a potentially very bad outcome. This is actually worked through for a set of (observable) utility values and is not "conjecture" as CYZ claims in footnote 8.

[5] The exception to this is when one specifies σ^2 as a function of regressors. See, for example, Bas (2007).

holding $\sigma^2 = .05$. The solid black line in each figure is the equilibrium prediction for war. The gray lines are the equilibrium predictions for attacking and resisting.

If one were to take the advice in CYZ and consider only the deterministic model, what would one conclude? With one caveat, the deterministic model predicts war will never occur. Why? When $U_2(C) < .5$, player 2 prefers war. Knowing that, player 1 will choose not to attack, resulting in a status quo equilibrium. When $U_2(C) > .5$, player 2 prefers to capitulate rather than go to war. Player 1 will attack in those situations, resulting in a capitulation equilibrium.

The caveat is the cutpoint $U_2(C) = .5$, where player 2 is indifferent between capitulation and war. Although cutpoints are important in determining where best responses change, the predicted behavior at these points of indifference is often treated as a nuisance by game theorists. What occurs at the cutpoint generally depends on how one defines away the nuisance. One can simply assume that a particular action is taken in this case (e.g., indifference leads to capitulation). Or, one can assume that player 2 plays a mixed strategy. The main reason it is often considered a nuisance is because the cutpoint has Lebesgue measure zero—i.e., it is infinitesimally small. Given any reasonable probability density, its probability of occurring is zero. Indeed, because of this, the behavior at cutpoints is often completely ignored—as it is in CYZ.

Turning to Fig. 5b, we see that the equilibrium predictions in the private information model are at times very different from those in the deterministic model. Although the equilibrium probabilities for attacking and resisting do appear to be slightly smoothed versions of the SPE counterparts, this is example specific. They will not always be so. More to the point, the interaction of these probabilities produces the probability of war $\Pr(W) = pq$. As Fig. 5b shows, the private information model predicts a fairly high probability of war for a nonnegligible region of $U_2(C)$. Again, the intuition is straightforward: in the region around $U_2(C) = .5$, player 1 is uncertain about what player 2 will do. This creates a window for war, where player 1 is more likely to attack but player 2 is still likely to defend.

In sum, although deterministic models may under certain conditions approximate the relationships in models with uncertainty, in many other situations the predictions will be very different. If one's theoretical model includes uncertainty (e.g., private information or agent error), then the equilibrium conditions should be derived based on the assumed uncertainty. That was actually one of the points of Signorino (2003). If one wants to conduct comparative statics analysis, one should then do so based on the equilibrium conditions for the theoretical model with uncertainty. Similarly, derivation of an estimator, observable implications, or insights for model specification should be based on the equilibrium conditions of the model with uncertainty.[6]

4 Alternative Statistical Methods

The second component of CYZ's main claim is that it offers (presumably new) alternative statistical estimators that are simpler than anything recommended previously. In particular, CYZ asks, "Can we use existing logit and probit models to test strategic theories of politics?" (Estimation Techniques). It argues that we can and proposes to demonstrate this with three alternative methods: a structural "attack-only" model, an "interactive probit" model, and a "two-stage" model. In this section, I demonstrate that these methods are no simpler than, nor even to methods recommended in previous research.

[6] Readers interested in models with signaling should also see the perfect Bayesian equilibrium models in Lewis and Schultz (2003), Wand (2006), Signorino and Whang (2007), and Whang (2007).

4.1 *Missing in Action: Comparative Statics*

The data-generating process used for CYZ's Monte Carlo analysis is based on the model in Fig. 1a. CYZ normalizes $U_2(C)$ to zero and specifies a very simple set of regressors in the remaining utilities: one regressor for each of the four utilities. CYZ proceeds by restating the equilibrium probabilities for the attacker's decision to attack and the defender's decision to resist. Aside from a minor error, CYZ's equations (5) and (6) are the same as equations (8) and (3) respectively, here, which were the equilibrium probabilities used in the theoretical analysis in Section 3.

Of particular importance is CYZ's statement that "Equations (5) and (6) provide the basis for three possible estimation techniques" (Some Estimators for Strategic Models). Given the vigorous defense of comparative statics, how does CYZ link comparative statics analysis to the proposed statistical estimators? It does not. Comparative statics are nowhere to be found in the section on statistical estimators. CYZ does not even discuss how the comparative statics provide insight into the derivation and specification of the estimators. As stated in CYZ, the three alternative techniques are motivated instead by the equilibrium probabilities p and q, which are precisely the equations one would derive (see equations 1–9) if one followed the recommendations in Signorino (1999), Signorino and Yilmaz (2003), and, especially, Signorino (2003).

Still, let us ignore this disconnect and, instead, ask: Is there anything new in the three proposed methods? And, will these techniques, at least as demonstrated in CYZ, be of any practical help to researchers?

4.2 *The Attack-Only Model*

In the attack-only model, the researcher does not have data on whether the defender resists (R) or not. The dependent variable consists solely of whether or not the attacker attacked (A). Explanatory variables are assumed to exist for both the attacker's and defender's utilities. Because explanatory variables exist for the defender's potential actions, q can be estimated and then used in estimating player 1's probability of attacking. The likelihood for this model was shown previously in equation (23).

Despite CYZ's claim that the attack-only model is a new alternative to existing techniques, it is neither new nor simpler than estimating the full structural model. Indeed, it is not even new to Achen (2006), as cited by the authors. Estimating the attack-only model via MLE is exactly what was recommended in Signorino (1999, 285, see equation 20), in Signorino (2003, 323, see equation 3), and in Signorino and Yilmaz (2003, 555). We spend an entire section in Signorino and Yilmaz (2003, 557–61) discussing misspecification of the attack-only model.

Moreover, despite its critique of the "complex machinery" of strategic random utility models and its argument for simpler methods, CYZ admits that the attack-only model "cannot be estimated using standard logit/probit software, requiring instead that the (relatively simple) likelihood be programmed and estimated" (Monte Carlo Analysis). CYZ gives the impression that somehow the attack-only model is easier to derive and to estimate than the full equilibrium model. This is incorrect. First, notice that in order to derive the attacker's probability of attacking p (equation 5), one needs to derive the entire strategic random utility model—here including player 2's probability of resisting q (equation 9)—since the expected utilities "higher" in the tree depend on equilibrium choice probabilities further down the tree. Therefore, from a mathematical perspective, the machinery for the attack-only model is no less complex than for the full model.

Secondly, the likelihood is not that much simpler to program than for the full model—it is just based on one of the two equations in the full structural model, and both of the equilibrium probabilities are required for that equation. If researchers can program the attack-only model, they can surely program the full structural model. Similarly, if researchers cannot program the full model, they almost certainly cannot program the attack-only model. From a machinery perspective, the full structural model versus the attack-only model is really a wash. What will determine whether someone uses the full structural model versus the attack-only model is whether he/she has data on the defender's actions. In sum, although the attack-only model may be useful when choice data further down the tree are not available, it is generally no simpler to implement than the fully structural model, and it is a technique that was previously introduced in a number of articles.

4.3 *The Interactive Probit Model*

The second method CYZ proposes is an "interactive probit" model (see CYZ equation 15). The dependent variable is, again, the attacker's decision to attack. The main insight for this method is that the probability of attacking p is a function not just of player 1's utilities but also of player 2's utilities via q, player 2's probability of resisting. In CYZ's specification, the variables that affect the defender's utility for war are therefore interacted with the variables that affect the attacker's utilities. Although this is not a full structural specification of the underlying model, as in the attack-only model, it is an approximation that might be worth investigating. A major (practical) upside of this approach is that the model may be estimated using existing statistical software for probit (or logit). CYZ finds that for their simple Monte Carlo example, the interactive probit is able to approximate relatively well the strategic relationship between the probability of attacking and the utilities for war and for capitulation (see CYZ's Fig. 7).

Once again, however, this is not an alternative to methods previously introduced. The characterization of functional form misspecification of Signorino and Yilmaz (2003) uses Taylor series approximations, which allows one to express strategic functional forms in terms of polynomials of and interactions between regressors (see Figs. 3 and 4 of Signorino and Yilmaz, pp. 558–60). Indeed, we state, "Our use of Taylor series expansions as approximations for the strategic regressions suggests one possible solution to the functional form problem: run polynomial regressions with interaction terms" (Signorino and Yilmaz 2003, 565).

Based on that, it would seem that I would be in agreement with CYZ concerning the interactive probit approach. As CYZ state: "To the extent that the interactive model requires nothing more than incorporation of multiplicative interaction terms into a standard binary-response model, this represents a significantly more tractable approach than derivation and estimation of the model's full set of structural parameters." I believe we should be a bit more cautious about this for at least two reasons.

First, substantive research is never simple. In contrast, in Signorino and Yilmaz (2003), we constructed the very simplest strategic model we could think of to demonstrate strategic misspecification. It was simple not just in terms of complexity of the game but also in the specification of the regressors. To provide a "negative" result—e.g., a demonstration of bias, inconsistency, etc.—it is often useful to start as simple as possible. Bias and inconsistency rarely improve when we make things more complicated.

CYZ, on the other hand, is attempting to provide a positive (or constructive) technique for researchers to use in a wide variety of situations. Yet, all that is demonstrated with its interactive probit analysis is that CYZ has been able to approximate the strategic relationship in "the simplest model possible" (Signorino and Yilmaz 2003, 551). The reason CYZ is able to approximate that relationship using only two interacted regressors is precisely because the data-generating process was constructed to be so simple.

A direct implication of Signorino and Yilmaz (2003) is that if reality is even a little more complicated than "the simplest model possible," then CYZ's purely interactive recommendation (i.e., without higher order polynomials or interactions) will lead to functional form misspecification. It is not difficult to construct models (or imagine data-generating processes) with nonmonotonicities. Often countervailing effects (e.g., balance of power) in competing players' utilities (e.g., for war) will suffice. Similarly, data aggregation—for example, analyzing war versus "no war" in the deterrence model—may result in more complicated relationships. In such cases, a second-order polynomial expansion will be the minimum required to approximate the nonmonotonic relationships. CYZ provides no guidance concerning what the researcher should do if a more complicated relationship is thought to exist. Nor does it provide any sense of the robustness of the single interactive model employed.

My second concern relates to what substantive researchers, especially those without a formal model, will take away from CYZ's recommendation. How will they use it in practice? CYZ's statement that the interactive approach is an alternative to "derivation and estimation of the model's full set of structural parameters" could give the impression that researchers need not even construct a theoretical model and/or think about how that model relates to the specification of the interactive regression model. Many such researchers are likely to include as many interactions (and perhaps polynomials) as possible. The model, its estimation, and its interpretation then become not quite as tractable as CYZ claims.

After suggesting that researchers might run polynomial regressions with interaction terms, Signorino and Yilmaz (2003, 565) also noted the practical difficulties of doing so: "Most problematic is that increasing the order (and, therefore, the extent to which the polynomial approximates the true functional form) combinatorially increases the number of parameters that must be estimated." Suppose a researcher wanted to conduct a polynomial/interactive regression based on six explanatory variables. To estimate a second-order model, she would need to estimate 28 parameters. To estimate a third-order model, she would need to estimate 84 parameters. How do researchers interpret all the coefficients, or determine which ones are "significant," warranting inclusion in estimating marginal effects? This will only be complicated by the likely multicollinearity issues, resulting from the numerous polynomials and interactive terms. And that is with only six explanatory variables.

Despite those reservations, Signorino and Yilmaz (2003, 565) remain optimistic that structural models and "less structural" (i.e., polynomial regression, semiparametric, nonparametric) methods might be used iteratively or in combination to inform each other. It seems to me, however, that formal theory is critically important here, not just for interpretation of the polynomial/interactive results but also for providing insights concerning the statistical model specification—e.g., the degree of polynomials as well as reducing the set of variables that need to be interacted.

In summary, the interactive probit approach is neither new nor an "alternative" to methods previously recommended—it is a method Signorino and Yilmaz (2003) proposed, albeit with some caution. It certainly merits more research. However, CYZ's demonstration of the technique based on a model that was designed to be as simple as possible provides no new insight into how these methods might be applied in practice, especially by researchers without a formal theoretical model to begin with.

4.4 *The Two-Stage Method*

The third alternative CYZ proposes is a "two-stage" method. This method estimates the full structural model but does so in stages, equation by equation. In the first stage, the researcher conducts a probit regression, using data only on player 2's decision to resist. Given the parameter estimates from that regression, the probability of resisting q_i is calculated for each observation. Those estimates of q_i are then plugged into a second-stage probit regression using player 1's attack data. CYZ finds that this method performs relatively well in recovering the parameter estimates on average.

Yet again, this is not an alternative to previously proposed methods. Signorino, Walker, and Bas (2002, 15–16) developed exactly this estimator and applied it to triadic interstate conflict (i.e., it was a three-stage estimator). That paper was greatly revised. Bas, Signorino, and Walker (forthcoming) now provides a detailed analysis of a more general estimator, which we refer to as a "statistical backwards induction" estimator. We show that the parameter estimates are consistent but that the standard errors associated with them are biased. Because of that, we provide methods for correcting the standard errors—R and Stata programs are provided for estimating these models and their standard errors. The technique is demonstrated both with Monte Carlo analysis and via a replication of Leblang's (2003) analysis of speculative currency attacks.

4.5 *On the Role of Logit and Probit*

Having addressed CYZ's main claims, I end this section by clarifying two related claims concerning logit and probit. First, CYZ at times gives the incorrect impression that Signorino (1999, 2002, 2003) and Signorino and Yilmaz (2003) are critiques of logit and probit broadly—not just the traditional linear utility specification but the very use of logit or probit in regression analysis. Much of CYZ's explicit argument on this is detailed in footnotes 1 and 2. Footnote 1 states: "it is unclear whether Signorino uses the term 'traditional' to refer to the use of logits and probits in general or whether he is referring to just standard functional forms of these estimation techniques (specifically, the linear link). Thus we leave the term intentionally undefined here as well." CYZ then refers to a "minimalist" versus "maximalist" reading of my critique (see footnote 2). According to CYZ, "In the minimalist reading of the critique, Signorino simply is critiquing the existing applications of logit and probit. Scholars generally operationalize linear logits and probits, and these typical logits and probits fail because relationships . . . are often non-linear" (footnote 2). In contrast, "In a maximalist reading of this critique, Signorino is critiquing the entire enterprise of using logits and probits to test strategic models period." It continues, "Although nowhere in his published work does Signorino unambiguously state that he is making the maximalist critique, it is not an implausible conclusion" (footnote 2).

Although Signorino (1999, 2002, 2003) and Signorino and Yilmaz (2003) frequently use shorthand terms like "typical," "common," "traditional," and "standard" in reference to applications of logit and probit, there is some clarification as to what is meant by this:

- "To be precise, by 'traditional logit' I mean binomial logit with linear latent utilities." (Signorino 1999, 285, footnote 18)
- "we analyze the misspecification of using logit or probit with the ubiquitous linear **XB** specification of the latent variable equation." (Signorino and Yilmaz 2003, 551)
- "Hereafter when we refer to the 'typical,' 'common,' or 'traditional' binary specification, it is precisely to this first-order linear latent variable specification to which we refer" (Signorino and Yilmaz 2003, 553)
- "we have characterized the strategic misspecification that arises from using the typical logit or probit specification—one where the latent dependent variable is

a linear function of the parameters and a first-order function of the explanatory variables" (Signorino and Yilmaz 2003, 565; see also similar statements on pages 552 [especially footnote 3], 554, and 557).

- "I will refer to the model just presented as the 'bivariate probit model'—and the reader should interpret this as the bivariate probit model with linear latent variables. In other words, the typical selection model for binary data." (Signorino 2002, 102)

To be clear, the critique has concerned the default linear **XB** specification of a regression equation, when the researcher is analyzing strategic behavior. The critique is neither of logit or probit as probability models nor of MLE in general. As Signorino and Yilmaz (2003) note, the critique is not even specific to binary or other discrete data. It is a very general critique of functional form misspecification. Moreover, as we have already seen, Signorino, Walker, and Bas (2002) and Bas, Signorino, and Walker (forthcoming) actually proposed using variants of standard logit and probit regression.

On that last note, and finally, CYZ claims to show that the estimators in many of the examples in Signorino (1999, 2003) and in Signorino and Yilmaz (2003) are variants of logit and probit. This is again true but not new. Notice that the strategic examples in Signorino (2003) are referred to as "strategic probit" models. More to the point, notice that equations (4–7) in Signorino (1999, 283) are all logit probabilities. Referring to the action probabilities of the simple crisis model in Signorino and Yilmaz (2003), we state that "If they are i.i.d. Type 1 Extreme Value, then the resulting action probabilities will be logistic" (p. 555). In fact, Signorino and Yilmaz (2003) conduct the entire analysis of misspecification in the context of binary response models (see, for example, the "strategic" and "Taylor" regression models in our Tables 3 and 4). As yet another example, notice that equations (1) and (2) in Signorino and Tarar (2006, 589) are probit probabilities. These references to logit and probit also suggest that CYZ's "maximalist" characterization is perhaps not so plausible of a conclusion after all.

5 Concluding Remarks

So, did comparative statics really need to be "defended" in the first place? Advocated, certainly. Defended, certainly not—at least not from these quarters. Comparative statics analysis is one method for analyzing the equilibrium conditions of a formal model. Just as it is important to connect formal theory to statistical models and methods, it is similarly important to connect comparative statics analysis to statistical analysis. CYZ contributes to this literature by initiating a discussion of exactly this relationship. We should note, however, that some progress has already been made in this area. As Ashworth and Bueno de Mesquita (2006) observe, nonparametric methods can be fruitfully employed when comparative statics are monotone. Nevertheless, there is still much work that needs to be done in determining exactly how comparative statics analysis should be used in combination with statistical analysis in other situations, especially when (1) relationships are potentially nonmonotonic and (2) parametric methods (like MLE) are employed.

Fortunately, one major advantage of formal theory and statistical modeling is that we can be very (mathematically) precise in our analysis. As we probe the relationship between formal theory and statistical analysis, we may at times find that our assumptions (the "received wisdom") concerning that relationship do not hold up. Indeed, I would suggest that we already have. I conclude with two examples of this.

An early critique of Signorino (1999) was that estimating the full structural models required too much of researchers. Some conjectured that if comparative statics were monotone, standard logit and probit analysis would be perfectly reasonable. This also seemed reasonable to me. Indeed, that is why in Signorino and Yilmaz (2003) we state: “it is often conjectured that typical logit or probit techniques should be fine for testing models where one can show (e.g., via an analysis of comparative statics) that the relationship being analyzed is monotonic” (p. 564). Moreover, “In the interest of keeping the analysis as simple as possible and of creating ideal conditions for those interested in applying logit to comparative statics, we have constructed the ‘simplest model possible’ and have ensured that the relationship between all action probabilities and the regressors is monotonic” (p. 565). The purpose there was twofold: first, to create the simplest model possible for our characterization of strategic misspecification and, second, to create ideal conditions for the above conjecture.

Signorino and Yilmaz’s (2003) conclusions were not an all-out assault on comparative statics analysis but a cautionary note to those interested in applying logit or probit with a linear **XB** specification in such situations: “the received wisdom requires the important qualification that the relationship is not just monotonic, but unconditionally monotonic. When the data-generating process implies unconditionally monotonic relationships between the regressors and dependent variable of interest, then the first-order **XB** specification is appropriate. Nevertheless, it is incumbent upon the researcher to determine (i.e., prove) that the theory implies such a relationship before using that specification” (p. 564).[7] Notice that this is a critique of regression specification, not of comparative statics analysis. It actually encourages researchers to use techniques like comparative statics analysis to analyze the relationships embodied in their theoretical models, so they can then better specify their statistical models.

Finally, recent research by Ramsay and Signorino (2006) on bargaining behavior suggests that matters may be slightly more complicated than what the above implies. Ramsay and Signorino (2006) find that even in some cases where the relationships are unconditionally monotonic, commonly used estimators still lead to biased inferences due to strategic censoring. Moreover, traditional censoring estimators like tobit are unable to alleviate the bias because the censoring in those estimators does not correctly model the strategic decision making. Thus, although it is tempting to seek simple solutions for substantive researchers, political (and economic) behavior may at times be more complex than canned models and methods allow. All of this simply reinforces the importance of the goals previously identified: the need for formal theory and a closer connection of statistical models and methods to that theory.

Funding

National Science Foundation (SES-0413381).

References

Achen, Christopher. 2006. Expressive Bayesian voters, their turnout decisions, and double probit. Paper presented at the 2006 annual meeting of the Society for Political Methodology, Davis, CA.

Ashworth, Scott, and Ethan Bueno de Mesquita. 2006. Monotone comparative statics for models of politics. *American Journal of Political Science* 50:214–31.

Bas, Muhammet Ali. 2007. Democratic inefficiency: Regime type and the tendency to make foreign policy mistakes. Working paper. University of Rochester.

[7]See also Ashworth and Bueno de Mesquita (2006, 230).

Bas, Muhammet Ali, Curtis S. Signorino, and Robert W. Walker. Forthcoming. Statistical backwards induction: A simple method for estimating recursive strategic models. *Political Analysis* doi:10.1093/pan/mpm029.

Carrubba, Clifford J., Amy Yuen, and Christopher Zorn. 2007. In defense of comparative statics: Specifying empirical tests of models of strategic interaction. *Political Analysis* doi:10.1093/pan/mpm008.

Leblang, David A. 2003. To defend or to devalue: The political economy of exchange rate policy. *International Studies Quarterly* 47:533–59.

Lewis, Jeffrey B., and Kenneth A. Schultz. 2003. Revealing preferences: Empirical estimation of a crisis bargaining game with incomplete information. *Political Analysis* 11:345–67.

McKelvey, Richard D., and Thomas R. Palfrey. 1998. Quantal response equilibria for extensive form games. *Experimental Economics* 1:9–41.

Ramsay, Kristopher W., and Curtis S. Signorino. 2006. Bargaining and society: A statistical model of the ultimatum game. Working paper. University of Rochester.

Signorino, Curtis S. 1999. Strategic interaction and the statistical analysis of international conflict. *American Political Science Review* 93:279–97.

Signorino, Curtis S. 2002. Strategy and selection in international relations. *International Interactions* 28:93–115.

Signorino, Curtis S. 2003. Structure and uncertainty in discrete choice models. *Political Analysis* 11:316–44.

Signorino, Curtis S., and Ahmer Tarar. 2006. A unified theory and test of extended immediate deterrence. *American Journal of Political Science* 50:586–605.

Signorino, Curtis S., Robert W. Walker, and Muhammet Ali Bas. 2002. Picking a fight: Disputes, deterrence, and war-time alliances. Paper presented at the 2002 annual meeting of the Society for Political Methodology, Washington, DC.

Signorino, Curtis S., and Taehee Whang. 2007. Uncertainty and learning in statistical strategic models. Working paper. University of Rochester.

Signorino, Curtis S., and Kuzey Yilmaz. 2003. Strategic misspecification in regression models. *American Journal of Political Science* 47:551–66.

Wand, Jonathan. 2006. Comparing models of strategic choice: The role of uncertainty and signaling. *Political Analysis* 14:101–20.

Whang, Taehee. 2007. Seeing is believing? International signaling use of economic sanctions. Working Paper.