Comments on Prat and Strömberg, and Robinson and Torvik[1]

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This session of the 2010 Econometric Society World Congress is an opportunity to look at the state of the field of political economy. The two papers presented are quite different in terms of purpose and style, but they are representative of two of the most exciting ideas developed in the field in this decade. The paper by Andrea Prat and David Strömberg provides an overview of the literature on the political economy of mass media: that is, the study of how mass media affects political outcomes and economic allocations and how the political system and the economy determine the structure of mass media. The paper by James Robinson and Ragnar Torvik presents a new theoretical attempt to explain why the ability of societies to adapt to changing environments depends on the quality of their institutions. The study of mass media and political institutions is not new to political economy. In the last 10 years, however, we have seen a renewed interest in these issues. New questions have been asked and, perhaps even more important, new techniques have been introduced to answer them. For this reason, these papers are illuminating not only on the specific topics that they develop but also for what they tell us about how political economy in general is changing.

The paper by Prat and Strömberg provides an excellent explanation of the renewed interest in the economics of the media: innovations on both the empirical and theoretical fronts. Let us consider empirical work first. Two questions are key to this field and, until recently, remained unsatisfactorily answered: Does the mass media change a sufficient number of votes to affect the outcome of elections? Does the media affect policies? Moreover, if the answer to these questions is "yes," then does the media have an agenda, and who or what determines it? What types of biases are imposed or heightened by the media? Although the answer to these questions often is assumed to be obvious, a rigorous empirical analysis has remained elusive until recently. The challenges in answering the first two questions can be noted in the following observation: The fact that political action on a given issue may follow coverage by the mass media on that issue does not imply that the coverage causes the policies. It may well be that coverage follows the attention generated by the political action, or they both may be determined by the interest of the public simultaneously. Ideally, the only way to quantify the effect of media on policies is to run a randomized experiment. Strömberg (1999) was one of the first to observe that exposure to mass media often is randomly determined by exogenous factors (e.g., the conditions for the diffusion of radio signals depends on geography). This random variation in media exposure therefore can be used as a "natural experiment" to assess how policy making is affected by the presence of mass media. This idea has proven to be a genuine methodological innovation in the study of mass media and it has been applied creatively in many other papers to answer a variety of questions including the impact of media on citizens' information (Gentzkow 2006, Snyder and Strömberg 2010), and on voters' behavior (Della Vigna and Kaplan 2007, Enikolopov et al. 2009). Innovations also have been substantial in terms of new data available for analysis and new tools

that can be utilized.   Methods from computational linguistics have been used to study politicians' language (Diermeier et al. 2007), and compare it to the language used by the media (Gentzkow and Shapiro 2010).[2]   This allows for the construction of more objective measures of media bias.

These creative empirical approaches, however, would not amount to so much without a sufficiently rich theoretical framework to interpret and guide them.   As Prat and Strömberg clearly explain in their survey, the media industry should be seen as a three-way strategic relationship among politicians who are interested in influencing their coverage, voters who use the media, and (unsurprisingly) media outlets seeking to maximize profit or promote an agenda.   This requires a sufficiently rich game-theoretic framework.   To see the challenges for research here, we note that a newspaper article or a television program provides three different types of services: It serves as a vehicle of information, a coordination device that makes information common knowledge, and a a form of entertainment.   Considering all of these factors is important in determining its scope and for welfare analysis.

All of this research clearly has given us a deeper understanding of the mass media that we have experienced so far.   In reading about this great work, however, a question came to my mind, and I pose it deliberately in the most provocative way: Is this literature about the present and the future or about the past?   An implicit assumption in most of the works presented in the survey is that the mass media is the gatekeeper of information with either monopolistic or oligopolistic power; that it maximizes profits or the interests of its owners; and that it may be influenced by the government of the states in which it operates.   Let us consider now the media event that characterized the summer of 2010: the online publication of secret documents of the U.S. Defense Department by WikiLeaks.org.   WikiLeaks is a nonprofit organization; it is not based in any state, so it is difficult to control by any government.   Perhaps more important, it is organized as a "wiki", that is, a website in which anyone can edit any page, therefore it cannot (at least, in principle) reflect a given point of view and it is open to anyone.   The case of WikiLeaks.org is only one example that illustrates how technology is radically changing information collection and distribution: this will have a significant impact on mass media in general.   Alternative media outlets (e.g., Internet forums, social networks such as Facebook, and search engines) are especially important sources of information in nondemocratic countries (which typically have a tight grip on traditional mass media).   The controversies between Google and the Chinese government in the fall of 2010 and between Saudi Arabia and Blackberry in the summer of 2010 are examples of this phenomenon.

The changes in technology that we are experiencing today should have and definitely will have an effect on academic research.   It is difficult to predict how to proceed exactly, but we already have the conceptual tools to address some of these changes, I provide one example.   In one of the most influential and insightful papers in the literature, Mullainathan and Shleifer (2005) modeled newspapers as monopolists or oligopolists who have the power to decide their ideological position, and they do it to maximize their profit.   A citizen who does not like the ideology of Paper A, can affect it only indirectly, with his own feet, by buying Paper B (if present) or not buying anything at all.   A citizen's choice is limited, however, because in this world, only a few players can make

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[2]Gentzkow and Shapiro (2010) used this to measure local newspapers' ideological positions and, by comparing it with local representatives, to conclude that their choices are consistent with   profit maximization.   By contrast, Puglisi (2006), using similar techniques, found a bias in *The New York Times* in favor of democratic candidates during elections.

their point of view public. As in a Downsian model, alternatives are exogenously limited (i.e. political alternatives in Downs; here, alternatives of opinions). What about letting citizens voice their own dissent directly? Ten years ago, this probably was prohibitively costly, but now the cost of voicing an opinion is only as high as the cost of maintaining a blog. Today, therefore, we are closer to a model of *citizen journalists* in which entry is cheap, in which there could be many more than just two alternatives, or in which alternatives are exogenous. In such a model, there would not be an ironclad distinction between media and public: Citizens imitate the media (by providing information through blogs) and the media imitates citizens, by allowing journalists to air their opinions through blogs. To continue the comparison with models of political competition, such a model would be similar to the citizen candidate models, as in Besley and Coate (1997) and Osborne and Slivinski (1996). If anything, the case for citizen journalists is stronger than the case for citizen candidates, given that it is easier to blog than to run for office. In general, technology is making the media industry flat and resembling more a network of connections (e.g., Facebook) than a unilateral channel of transmission, as it has been until recently. This will require new ideas in both theoretical and empirical work.

I now comment on the Robinson and Torvik paper. What struck me about this paper is its ambition. It starts with a discussion of a number of historical episodes that are used to argue that the ability of a society to cope with changes in the environment depends on its institutions. The examples range widely over time and location: from Europe after the plague of the 14th century to colonialism to postrevolutionary North America. In all of these examples, the authors identify a common theme: The same shock can induce opposite effects in different societies. Let us consider the example of Europe after the plague of the 14th century. throughout Europe, the reduction in population led to an increase in the bargaining power of workers. In Western Europe society adapted through a radical change in institutions that, amongst other things, led to the end of serfdom, thereby making an important first step toward a modern society. By contrast, the increase in bargaining power was resisted vigorously by landlords in Eastern Europe, who responded by tightening their control on peasants. This led to institutions that implemented what has been called the "second serfdom." What can explain this divergence? Robinson and Torvik believe that countries in which there are institutions with well-defined property rights or civil liberties can adapt in a more productive way to changes in the environment. There even may be situations in which the same shock leads to higher production (and welfare) in one country and to lower production (and welfare) in another. This observation is not completely new. In a slightly different form, it was made in the political-economy literature by Tornell and Lane (1999) who, in the context of a similar two-sector model, attempted to show that an increase in natural resources may induce either an increase or a decrease in output depending on the power of special-interest groups. They dubbed this phenomenon "the voracity effect." In their paper, Robinson and Torvik laid out a more ambitious framework that can be used to study a wider range of historical phenomena and institutional environments. Perhaps the closest antecedent to this line of work is Diamond (2005), who studied how society reacts to changes in the environment (and why some survive and others do not).

To make this point the authors consider a model with two sectors: a productive sector consisting of a mass l of producers (p), and a rent-seeking sector consisting of a mass $1-l$ of political entrepreneurs (e). The income of a worker in the first sector is $y = f(l, r^p)$: It depends on both the mass of people working in this sector, and the per-capita resources that can be used in this sector

(r^p), and it is increasing in both terms. Similarly, the income of a political entrepreneur is $x = f(l, r^e)$, where r^e are the per-capita resources dedicated to this sector and it again is assumed to be increasing in both terms because (in the authors' interpretation) the larger is the fraction of the population engaged in production, the smaller is competition in the rent-seeking sector. Total resources are fixed at an exogenous lever r. Producers and political entrepreneurs share resources according to a fixed exogenous rule $q \in [0,1]$, so that $r^p = \dfrac{\theta}{l} r, r^e = \dfrac{1-\theta}{1-l} r$. The variable θ is interpreted as the "institution": A high θ corresponds to an institution in which the productive sector receives a high level of per-capita resources. Citizens can choose freely the "sector" in which they operate, so an equilibrium is an allocation l^*, x^*, y^*, where no agents has an incentive to change sector. This requires:

$$x^* = f(l^*, r^e) = f(l^*, r^p) = y^*$$

Equilibrium aggregate per-capita income therefore is $y^* = l^* y^* + (1-l^*) x^*$. Using (basically) only this condition, the authors are able to show that following an increase in r, aggregate income may either go up or down depending on θ: it goes down when θ is sufficiently small.   An appealing feature of this model is its simplicity and the fact that it attempts to formalize an apparently counterintuitive phenomenon: that an increase in resources results in a reduction in income when there is a strong "rent-seeking sector."

I have three comments: (1) on the way institutions are modeled; (2) on the need to explicitly model the market for resources; and (3), on the scope of the model.

The authors model institutions in a simple way through the parameter θ. This may be seen as an asset by some readers because it does not limit the model to a particular institution thereby allowing a wider application.   However, there are a number of problems with this approach.   The first concerns the "empirical testability" of the model. The parameter θ is crucial to the analysis but it is not observable in the real world. What we observe are the institutions themselves: for example, the type of judicial system, the presence of separation of powers, the role of the parliament, the role of elections (if any), and so on.   The question then becomes one of identifying a mapping between institutions and power shares (i.e., the values of θ).   The advantage of developing a theory that microfounds the political process is that it naturally provides direct predictions of the effects of institutions on economic outcomes.   The second problem generated by a "black-box" approach to institutions is more theoretical.   Without an explicit microfoundation for the "rules of the game" (i.e., how the two sectors interact) it is difficult to evaluate how realistic the assumptions of the model are or to understand what determines the division of surplus between the two groups. I provide two examples. First, in the model, it is assumed that institutions (θ) determine the distribution of resources (i.e. the allocation of r) *before* production takes place. In any political system, however, politicians (i.e., the political entrepreneurs in Robinson and Torvik's terminology) are able to tax output *after* production takes place.   Why is this not allowed in the model?   It is difficult to evaluate this point because there is no explicit taxation in the model: all the interaction is captured by the functional forms described herein.   This is an important point because if politicians can appropriate at least part of the output in the productive sector, then they will try to maximize it (or, at least, to distort it less) resulting in significantly different qualitative predictions.   In light of this observation, it is unclear how robust the results of this

paper might be. Another issue emerges when we consider how the division of total output changes as we change the allocation of citizens in the two sectors (l). The authors assume that when most workers are in the productive sector, then the "political sector" generates higher per-capita income (i.e., Assumption 1). This condition yields an interior solution for l, but it is difficult to interpret. I expect that the ability of politicians to extract resources from the private sector depends on the aggregate resources that they control (e.g., the size of the autocracy), and this depends on l and $r^e(\theta)$. If all citizens are in the productive sector, then who would support these politicians? Who would give them resources? If not, would any worker even seek to be a political entrepreneur? The model fails to account adequately for these interactions and concerns.

My second observation is that the framework presented does not model market interactions. This is a significant limitation because even in less-developed countries, it is presumable that factor markets will determine the allocation of resources which may have significant effects on the results. A formal modeling of markets for resources also seems important to understand how an economy transitions to a new steady state after a change in the fundamentals.

My final observation concerns the scope of social phenomena that the model attempts to explain. Let us consider the example mentioned herein regarding the impact on the Atlantic trade on institutions in Europe. The rise of the Atlantic trade was not a sudden event; it took place slowly in a time span of at least decades. As trade increased, institutions changed with it. The authors are modeling such an event with a static model in which institutions are taken as given. I would have found more convincing a dynamic model in which the economy and the institutions evolve simultaneously. Developing a model with this characteristic is difficult, but probably worthwhile.

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