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The old IPE and the new

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The old IPE and the new

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ABSTRACT

The 'old' IPE of the 1960s and 1970s explored the political implications of economic interdependence, in an analytically loose but creative way. The 'new IPE', as embodied in the open economy politics approach, is more rigorous and has the virtue of integrating comparative and international political economy into a common framework. But it pays too little attention both to how interests are constructed and how policies are subject to processes of international diffusion, and it is remarkably reluctant to focus on major changes taking place in world politics. IPE should come to grips with the fact that genuine economic development is taking place on a global scale; on the role of China, on volatility in financial and energy markets; on the role of actor other than states, and on the implications of the Internet for the analysis of power.

KEYWORDS

International political economy; asymmetrical interdependence; comparative political economy; change in world politics.

The fascinating documentation of changes in American IPE (international political economy) by Maliniak and Tierney, and Benjamin Cohen's work on the transatlantic divide in the field – published in *RIPE* in 2007 and as part of his 2008 book (Cohen, 2007, 2008) – provide an appropriate occasion for reflection on the history of the IPE field and where it stands now. My reflections are highly personal, since I was 'present at the creation' of the field in its present form, and since in recent years my work has addressed broader questions of international relations and has diverged from mainstream IPE. In comparing the old and the new IPE, I comment on what seems to be missing from contemporary work and on what I regard as promising directions for future scholarship.

THE OLD IPE

When Susan Strange, Joseph Nye, Peter Katzenstein, Stephen Krasner, John Ruggie, and I started to explore IPE – Susan in the 1960s, Joe and Steve and I around 1970, Peter and John a few years later – there was no field. Very little research was being done. Most economists were ignoring politics, and international relations specialists saw political economy as ‘low politics’, minor, boring, and incomprehensible. So our task – and opportunity – was first one of identification, then of broad interpretation. In his RIPE paper and his book, Professor Cohen provides an account that closely corresponds to my memory of these early days.¹ Identification of IPE as a proper subject of study was inaugurated in the United Kingdom by Strange’s 1970 article in *International Affairs*, ‘International Economics and International Relations: A Case of Mutual Neglect’ (Strange, 1970), followed by her book, *Sterling and British Policy* (Strange, 1971), which traced connections between politics and economically historically. The key markers in the United States were three special issues of *International Organization* during the 1970s – on transnational relations (Keohane and Nye, 1972), politics and economics (Bergsten and Krause, 1975), and foreign economic policies of advanced industrialized states (Katzenstein, 1978).²

It would be misleading to give the impression that these new formulations were entirely original. Indeed, we responded in part to what Richard Cooper, in *The Economics of Interdependence*, defined as the central problem of international economic cooperation: how to maintain openness while enabling states to retain sufficient autonomy to pursue legitimate objectives (Cooper, 1968: 5). In 1970 Charles Kindleberger edited a volume on the multinational corporation that contained an essay by Kenneth Waltz on the ‘myth’ of interdependence that presented an important challenge for Nye and me, and in 1973 Kindleberger published *The World in Depression*, which made a great impact on all of us (Kindleberger, 1970, 1973). In 1971 Raymond Vernon, who was at the Center for International Affairs at Harvard, published his book on multinational corporations, *Sovereignty at Bay*, as the culmination of years of work on this subject (Vernon, 1971). Finally, Nye’s work, and mine, were influenced by the contributions of Ernst B. Haas, particularly to the study of regional integration (Haas, 1958).

Nye and I contrasted what we called the ‘politics of interdependence’ with then-dominant statist and security-oriented conceptions of international relations. We sketched out broad concepts – such as transnational relations, transgovernmental relations, and connections between asymmetrical interdependence and power. We characterized what Realists viewed as ‘reality’ in world politics as an ideal type, and contrasted it with another ideal type, that of ‘complex interdependence’. In situations of complex interdependence, we argued, there were multiple actors (rather than just states), multiple issues that were not necessarily hierarchically ordered,

and force and the threat of force were not valuable tools of policy. In our view, neither the Realist model nor complex interdependence fully described world politics; but regional politics, and issue areas, varied in how close they were to one pole or the other.

Nye and I also elaborated a concept first brought to the IPE literature in another special issue of *International Organization* by Ruggie (Ruggie, 1975: 570–3): that of *international regimes*. In *Power and Interdependence* (Keohane and Nye, 1977) we sought to describe and explain regime change, not merely theoretically but also with systematic empirical work on the politics of money and oceans, and on US relations with Australia and Canada.³ In his edited volume on foreign economic policies, which also appeared in 1977, Katzenstein demonstrated that our international relations analysis of what was now called ‘international political economy’, or IPE, was too one-dimensional. Genuinely comparative analysis was required, since there was no single template of state responses to interdependence. Peter Gourevitch brilliantly pursued a related theme in his notable 1978 article on ‘the second image reversed’ (Gourevitch, 1978), and Ruggie followed several years later with his creative concept of ‘embedded liberalism’ (Ruggie, 1982). During the 1970s this cluster of challenges to Realism was itself challenged most cogently by Robert Gilpin, most notably in his 1975 book on *U.S. Power and the Multinational Corporation* (Gilpin, 1975); and by Krasner, in a major article on ‘State Power and the Structure of International Trade’ (Krasner, 1976).

We were young, exuberant, and friends with one another, neither expecting nor wanting general agreement.⁴ In her brilliant paper in this symposium, Kathleen McNamara captures well the richness – I would say, ‘joyous contestation’ – of these debates. To us, the under-explored area of political economy offered irresistible territory for intellectual adventure and, one might say, conquest. To paraphrase the words of a famous Tammany Hall boss, ‘we seen our chance and we ‘tuk’ it’. But in the context of contemporary political science, our empiricism was loose and sketchy, and we did not engage in sophisticated causal inference to support our grand theories.

IPE AND OEP: INCREASING RIGOR AND THE PRICE THAT WAS PAID

As Cohen (2008) argues, the dominant norms and incentive structures of American social science then took over, bringing with them reductionism, epistemic rigor, and the models of economics. In many ways, the results are impressive. We have much more comprehensive and precise descriptive mappings of how the world political economy works, in multiple dimensions. In some areas scholars have made some progress toward valid causal inferences, although as Martha Finnemore and Henry Farrell, and David Lake argue, such inferences are very difficult to make. Certainly the

quantity of data brought to bear on IPE problems has expanded, and the field is much more sophisticated about methodological problems such as selection bias and endogeneity. Lake's paper lucidly describes this progress, from the 'inside', so to speak, as the elaboration of an open economy politics (OEP) frameworks model. As he says, 'OEP begins with sets of individuals that can be reasonably assumed to share (nearly) identical interests', defined as preferences over alternative outcomes, measurable in material terms. Since interests are deduced from economic theory, OEP can build on the edifice of contemporary economics. The resulting structure has an impressive coherence, quite different from the 'cacophony' of the early years. This is the intellectual foundation on which the International Political Economy Society, co-founded by Lake and Helen Milner, has been constructed. As Maliniak and Tierney show, its commitment to liberalism, and to quantification, have swept into dominance in the field, as measured by publications in high-prestige journals.

I was not one of the people who formulated OEP, although Helen Milner, one of its leading advocates, was both an undergraduate student and PhD advisee of mine, and is one of my closest friends. Much of the OEP work has direct links to my rationalist argument in *After Hegemony* (1984), and it is also consistent with the methodological program laid out in *Designing Social Inquiry* (King *et al.*, 1994), works that almost bracket my own years at Harvard (1985–1996). It would be understandable, therefore, for me to identify with Randall Germain's social construction of the 'Harvard School', and to see OEP as a natural continuation of my earlier work that should properly dominate the IPE field. Yet despite my sympathy with many aspects of the OEP model, and my great respect for its leading proponents, I view it with a gnawing sense of dissatisfaction.⁵

On the positive side of the ledger, many of the disputes of the 1970s and 1980s are no longer salient because they have actually been resolved. Politics and economics are inextricably linked – and the politics of international and transnational economic relations are not 'low politics', unimportant and inferior. Transnational relations are important aspects of world politics, linked in multiple ways to states and interstate politics. What we called complex interdependence is now conceptualized in much more sophisticated ways in terms of networks. There is general acceptance that both differential state and non-state power, and multilateral institutions, are important factors affecting policy outcomes. Under different conditions, states may seek to promote other states' interests, hinder them, or be indifferent to their fate. In other words, many of the earlier controversies have faded because consensus on the answers to them developed, and they became mere background characteristics for more focused and rigorous analysis. To me, this shift represents progress, and is gratifying.

Also on the positive side of the ledger, on the whole, is that fact that during the last 20 years, IPE has been increasingly difficult to distinguish from

comparative political economy, or CPE. The original and challenging insights of Gourevitch and Katzenstein have become conventional wisdom. The brightest students in graduate school in the 1980s also recognized that it was not adequate to assume 'state interests', and then to vary systemic conditions such as power structures and institutions. In their own work, they vigorously unpacked the black box of the state (Milner, 1988, 1997; Moravcsik, 1998; Simmons, 1994). A cohort of scholars has demonstrated, in a variety of areas, the importance of domestic conditions. Recent work on coordinated and liberal market economies by Peter Hall and David Soskice (2001) represents a continuation of this line of thought – with a direct line, in my view, back to Katzenstein's special issue of *International Organization* on 'power and plenty' in 1977.

One of the great virtues of the open economy politics model described by Lake is that it integrates IPE and CPE into a common framework. In some respects, therefore, the OEP model is an advance over the old IPE. It highlights the fact that the most important explanatory variables in the field often reflect variation in domestic politics, reflecting different factor endowments and structures of industrial production, which as Albert Hirschman and Peter Gourevitch have emphasized, are partly endogenous to the world political economy itself.

Yet as Peter Katzenstein argues in more detail, a price has been paid. In particular, there is too little emphasis on how 'interests' are constructed – how the ideas that people have in their heads, and that they share collectively, affect their preferences. OEP is, to my mind, too materialistic and much of it is too inclined to identify rationality with egoism – an analytical mistake, since altruists, and suicide bombers, can act quite rationally.⁶ As long ago as 1988 Judith Goldstein and I began discussions of the role of ideas leading to our edited 1993 volume, *Ideas and Foreign Policy*. We cast our argument in a rationalist framework, but in many ways it was closer to what has come to be called Constructivism than to the materialistic rationalism of the OEP orthodoxy. In our more individualistic formulation, interests are in a symbiotic relationship with ideas: material interests (as Marx argued) indeed affect the ideas people hold, but ideas are not just 'hooks' for interests: they have independent impact. That volume contains major papers by, among other scholars now known as Constructivists, Peter Katzenstein and Kathryn Sikkink; and an important paper on sovereignty by Stephen Krasner, whose work is unclassifiable in paradigmatic terms since it represents an original synthesis of Realist, Institutional, and Constructivist elements.

One price not emphasized by Katzenstein is the assumption in much work that, as Beth Simmons and her colleagues say in a recent issue of *IO*, 'processes of policy change can adequately be understood by conceiving of national governments as making decisions independently of each other' (Simmons *et al.*, 2006: 787). If this null hypothesis were correct, IPE, as

opposed to CPE, would essentially be relegated to the study of contextual background factors. We could dismiss IPE in the words Trotsky used in discussing a socialist foreign ministry: we could 'issue a few proclamations and shut up the joint'.

I have been disheartened by this suppression of the 'T' in IPE, but I take heart not just from the symposium in the fall 2006 issue of *IO* but from the emphasis on networks, diffusion, convergence, and strategic interaction in a number of recent papers. As Simmons and increasing numbers of other scholars now argue, the null hypothesis that national governments make decisions independently is not sufficient to explain the spread of liberalism or of convergence in certain sectors. Competitive pressures seem to play a major role, and there is some evidence, less clear, that coercion, learning and emulation also are significant factors. In other words, the distinctively international and transnational processes studied by IPE have to be brought back into the picture.

This 2006 *IO* symposium seems to me to present some of the best of the new IPE. Yet there is still something missing. One thing that is missing, as McNamara and other contributors to this symposium argue, is sustained attention to issues of structural power, as they affect the processes of international political-economic interaction and negotiation. In the 1970s, Marxists and Realists raised these issues and forced the rest of us to grapple with them. Surely similar issues are now being raised by the major shifts in power – toward Asia – taking place in the world political economy, and exemplified by the July 2008 breakdown of the WTO Doha Round negotiations. As structural changes occur, it will become apparent that we have taken for granted certain power structures that are in fact changeable, and the theoretically rich and historically comprehensive style of work, best represented in the previous generation by Gilpin, and now by Katzenstein – discussed so well in this symposium by McNamara – will become increasingly important.

The discussion of causal realism and pragmatism by Finnemore and Farrell, in this issue, provides a way for me to clarify my epistemological stance, which may suggest some differences with some of the ways in which some practitioners of OEP have recently taken the field. I am a very American Mid-Westerner, pragmatic to the core. I have always hesitated to label myself in terms of the philosophy of science, since I am not trained in that field; but my sympathies are much closer to scientific realism, or what Finnemore and Farrell, following Daniel Little, call 'causal realism', than to strict positivism. Finnemore and Farrell contrast these two views by saying that 'causation is important, but also . . . causal mechanisms exist independently of directly measurable relationships between variables'. In *Designing Social Inquiry*, King, Verba and I cite Little's 1991 book in saying much the same thing: 'any coherent account of causality needs to specify how the effects are exerted' (King *et al.*, 1994: 85). My commitment to

qualitative analysis as an intrinsic part of good social science does not reject sophisticated quantitative analysis in the least, but does reflect a view that causal mechanisms are best elucidated with case studies and narratives, conducted in an analytically rigorous way.⁷

Substantively, what is missing for me in contemporary IPE is the *synthetic interpretation of change*. Nye, Strange, and I saw huge changes taking place before our eyes and we tried – in intuitive and not very scientific ways – to interpret them. Scholars now have a much harder task because the volume of work is so much greater and our knowledge of the complexity of these systems is so much richer. Nevertheless, those of us who are not in the field of IPE – scholars, policy-makers, or citizens – need to know what the best research says about the big questions. One of them, the diffusion question, is now getting the attention it deserves. But there are others.

FIVE BIG QUESTIONS ABOUT CHANGE

Suppose we asked, as Nye, Strange and I did in 1970: what are the big changes going on in the world political economy? Surely one could make a longer list, including such issues as outsourcing and migration, but I will mention five major changes:

- (1) *For the first time in human history, genuine economic development is taking place for much of the world's population.* East Asia (excluding China) led the way with four decades of high sustained growth, and growth has for over two decades been rapid in India and extremely fast in China. In more recent years, there have been indications that sustained rates of high growth may be emerging in parts of Latin America, although the situation in most of sub-Saharan Africa remains bleak (Rodrik, 2007: 14). Our theories of IPE were constructed in a very different world: of 'developed' and 'developing' countries – the latter identified mostly by the fact that they were *not* developing. Indeed, for this reason perhaps, Gourevitch, Katzenstein, Nye, and I paid most of our attention to developed countries (Krasner, 1985 is a notable exception). Dependency theory, of course, emphasized – and it now seems, exaggerated – the structural differences between countries in the center and in the periphery. It is perhaps less obvious that the assumption of political as well as economic hierarchy between rich and poor is also deeply embedded in the theories of asymmetrical interdependence that Nye and I, and others developed, and in realist or quasi-realist theories of American hegemony and western dominance such as those of Krasner. I expect that our implicit hegemonic assumptions will continue to hamper our vision until scholars from Brazil, India and China, and other emerging great powers, become more prominent in the field.⁸

- (2) *China in particular has become a huge player in international trade and finance*, as the manufacturing center of the world for a huge number of products in ordinary life. China was the largest economic power for centuries, before the industrial revolution, although in those times economic interdependence was much lower than now. China's re-entry into the world political economy since 1978 has been a huge shock to the system – it would seem to be at least comparable to the opening up of the North Atlantic grain trade in the 1870s so memorably studied by Gourevitch. An appropriate metaphor is that of a elephant jumping into a small pond. The effects are already evident in trade, and will increasingly be apparent on financial issues, since China's foreign reserves of well over \$1 trillion constitute a political as well as an economic resource.⁹ In the West, scholars have been slow to take China sufficiently into account, although there are signs of change. Without China, we would be staging *Hamlet* without the Prince.
- (3) *Volatility in financial and energy markets has been extreme*. Kindleberger (1978) long ago emphasized the tendency of capitalism toward 'manias, panics, and crashes', and his insight was borne out again in 2008. At the same time, oil price shifts have again been driving shifts in power and asymmetrical interdependence. The sharp rise in oil prices up until mid-2008 contributed to policy changes such as the resistance of oil producers, from Venezuela to Russia and even to Saudi Arabia, to American demands; Brazil's leadership in ethanol production and technologies to use ethanol in automobiles; and China's drive for energy resources in Africa, which is undercutting 'good governance' initiatives, and the influence of the international organizations like the World Bank, on that continent. In the 1970s, rising oil prices, and greater leverage for producers, had major effects on world politics, reflected in the creation and persistence of OPEC and the Arab oil embargo of 1973, which led to the creation of the International Energy Agency, associated with the OECD. But OPEC turned out to be ineffective and prices declined sharply in the mid-1980s and stayed well below late 1970s prices, in real terms, throughout the twentieth century. What will happen to them now is a great unknown. There is some discussion now of how oil affects democracy (Ross, 2001), civil conflict, and aggressive foreign policy, but the demand for analysis of the causes and consequences of oil price fluctuations surely exceeds by far the supply of serious scholarship on the subject.
- (4) *Truly global actors are now important in world politics*. Global corporations and NGOs such as Human Rights Watch, Oxfam, or Greenpeace are exemplary. As John Ruggie comments, 'This isn't "IPE" any more, and it certainly isn't "CPE"'. Global actors demand global rules.¹⁰ There is some work on this aspect of globalization, but we still have relatively little systematic knowledge about the implications of global civil

society for political outcomes. Law faculties have paid attention: one of the most important research programs on this topic focuses on the new field of 'global administrative law' (Kingsbury and Krisch, 2006; Kingsbury *et al.*, 2005). Anne-Marie Slaughter (2004) has written about a 'new world order', in which the state is disaggregated and networks of governmental sub-units, NGOs, private corporations, and a variety of regulatory and coordinating bodies become prominent participants in rule-making. John Ruggie has played an important policy role in this emerging global society and has also commented on the politics that ensues (Ruggie, 2004). It would be good if some IPE scholars could turn their attention, and their analytical tools, toward what might be called 'GPE', or 'Global Political Economy'.

- (5) *Electronic technologies have become the basis for global communications.* We are aware of how such technologies have affected commerce, finance, and investment, but what about their effects on political power? To exercise influence, sets of individuals with common values or interests need to be able to communicate with one another, to form groups, and to act collectively. Indeed, Hannah Arendt once defined power as 'the ability to act in common'. Historically, such communication has been very difficult except through formal organizations, including the state, and all but impossible across state boundaries except with the aid of states. This formerly constant reality has been changing with incredible speed during the last two decades, but we have hardly begun to understand the implications of this momentous fact. One implication may be that collective action on a global scale, for good or ill, is easier than it has ever been before. In this sense, there is *more power* in the system than in the past. Since variations in power are crucial to world politics, the changes in electronic technology have to be important, but I have not seen recent work addressing these issues of communication and power.¹¹

In discussing these big questions, innovative scholars may discover rigorous and quantitative methods that can illuminate them. If so, more power to them. But we cannot afford to wait to address these questions for such methods, if they are not available now. As Finnemore and Farrell, and McNamara argue, more attention needs to be paid in graduate programs to rigorous qualitative methods, which have undergone a renaissance during the last 15 years; and as Katzenstein argues, more problem-oriented research is needed to maintain a focus on really important questions. I would urge scholars now active in the IPE field to spend more of their time pondering the big questions about change, and asking not only what the best existing research tells us about them, but what interpretive leaps may be necessary to point the way to more profound and relevant scholarship. I offer this admonition particularly to those scholars who have attained

reputations for science and can therefore afford to let the wings of imagination spread.

Study major changes in world politics using a diverse portfolio of methods: this is the message of my brief essay. The best insights of the British school and contemporary American IPE are both valuable – so are contributions from historical and economic sociology, and from the ‘ideational turn’ in much international relations scholarship. Our standards should be high; but a monoculture, as McNamara implies, depletes the soil from which it grows.

Normatively, I value the critical spirit of British IPE, and of Susan Strange and Robert Cox, because, like them, I am unwilling to accept the contemporary political-economic system as either natural or good. Injustice and inequality are endemic to IPE. But I also value the discipline of social science, as reflected in American IPE, which seeks to separate value judgments from positive analysis. I believe that in the long run, social scientists can have a more positive impact on the human condition through rigorous, persuasive analysis than through subjective criticism. But for us to help improve the human condition, we need to reflect on the big questions.

ACKNOWLEDGEMENTS

I wish to thank Michael Tierney for urging me to contribute to this symposium at a point when I was reluctant to do so, and Peter Gourevitch, Peter J. Katzenstein, Stephen D. Krasner, Joseph S. Nye, and John Gerard Ruggie for comments on an earlier version of this paper. I am especially grateful to Peter Katzenstein for pressing me to keep going further in explicating my themes. Kate Weaver provided especially thorough and perceptive comments on the penultimate version of this paper, helping me to link it to other papers in this symposium.

NOTES

- 1 Some of the arguments in this paper were originally put forward in a commentary on an earlier version of Professor Cohen’s paper, ‘The Transatlantic Divide: Why are American and British IPE so Different’, at the first meeting of the International Political Economy Society, Princeton University, 17 November 2006. In my view, Professor Cohen’s original paper on the transatlantic divide, as published in *RIPE* in 2007, is excellent – well-researched, thoughtful, sensitive to historical context, and respectful of both American and British international political economy, or IPE. Perhaps it is not surprising that I think so, since we are of the same generation – indeed, we were both on the Board of Editors of *IO* in 1970, along with Susan Strange.
- 2 Citations are to the published volumes. The transnational relations issue of *International Organization* appeared in the summer of 1971 and the foreign economic policies issue in the fall of 1977. It should be noted that C. Fred Bergsten, among young policy-oriented economists, was a leader in fostering

connections between economics and politics, and that all of this work was indebted to Shonfield (1965).

- 3 It is interesting, and sobering, to note that in our field, people keep reading the conceptual and theoretical 'promissory notes', but the empirical work that follows – in many ways harder to do and certainly more time-consuming – disappears from citations and reading lists. I see many references to Chapters 2 and 3 of *Power and Interdependence*, and still receive requests to reprint them; but I almost never see any references to the empirical material in Chapters 4–7.
- 4 Robert Gilpin was a referee for Princeton University Press of *After Hegemony*, and I was a referee for the same press of *The Political Economy of International Relations*. Both reviews were enthusiastic, although as is well known, Gilpin and I differed on many issues.
- 5 As Peter Katzenstein notes, although many of the founders of IPE received their PhDs from Harvard and started thinking about political economy issues around the lunch-table at the Center for International Affairs, few of us really identified with Harvard (for example, I was on leave at Harvard in 1972 from Swarthmore). The Harvard label also obscures the great influence of Ernst Haas, at Berkeley (the teacher of John Ruggie), and Robert Gilpin, at Princeton. And several of us who received our PhDs at Harvard were based elsewhere while doing our most important work: this is true of Gourevitch, Katzenstein, Krasner, and myself. But the most serious problem with the 'Harvard School' label, as Katzenstein points out, is that few of us who launched contemporary IPE are in sympathy with the 'monocultural turn' in IPE documented by Maliniak and Tierney. Indeed, Randall Germain may be surprised to learn that I agree with much of what he says about the dangers of 'acute scholarly autism' in contemporary IPE. If contemporary IPE were really dominated by the 'Harvard School' as he characterizes it, I would also count myself an 'outsider'.
- 6 For my most recent and careful discussion of egoism and rationality, see Keohane (2001: 1–13). This essay is reprinted in my *Power and Governance in a Partially Globalized World* (Routledge, 2002), where the relevant passages appear on pp. 255–7 in a section entitled, 'The Limits of Rational Egotism'.
- 7 The 'state of the art' in this vein is, in my judgment, by an economist. See Greif (2006).
- 8 The reader may note, in this passage (written for the first draft of this paper in May 2008), another point of agreement between myself and Randall Germain.
- 9 Clearly, this interdependence is not entirely one-sided, since China would also lose from a collapse of the dollar. It would be good to see the complexities of Chinese–US economic interdependence more thoroughly explored by scholars with intimate knowledge of China, the United States, and IPE.
- 10 Personal communication, May 2008.
- 11 One could speculate further. In particular, *our conceptions of time may be changing*, prompted by the internet and the cell-phone. Blogs and the 24-hour news cycle have had a major impact on American politics. Economically, as the saying goes in the internet era, 'if you stop to eat lunch, you are lunch'. In personal relationships, many people are available by cell-phone virtually continuously, and people commonly apologize for not replying even to routine emails within a day or two. But most people in the world still want to stop to eat lunch, to spend time with their families and friends, to relax. It would be interesting to inquire about the implications for world politics of the speeding up of time, and perhaps its fragmentation as continual interruptions and multi-tasking become accepted aspects of modern life.

NOTES ON CONTRIBUTOR

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