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ABSTRACT

Some two decades after an earlier review essay of mine, the time seems ripe to revisit the international political economy (IPE) of money. How has the study of money evolved in more recent years, and what is the understanding of international monetary politics today? The main message of this essay is – to be blunt – disappointment. Research has become increasingly insular and introspective, largely detached from what goes on in the real world. Two parallel developments are responsible, both reflective of wider trends in mainstream IPE. First is a steep decline of interest in broader systemic issues, as the pendulum has swung sharply toward the domestic level of analysis. And second is a marked loss of interest in practical policy solutions. In lieu of worries about *problems to be solved*, scholarship today tends to be driven more by curiosity about *puzzles to be explained*. The roots of these twin developments trace back, above all, to the framing effect of epistemology – the demanding methodological standards that mainstream IPE has set for itself. The fault lies, first and foremost, with the excessive priority given to formal scientific method.

KEYWORDS

money; global finance; international monetary politics; open economy politics; financial crisis; balance of payments.

INTRODUCTION

Monetary analysis in international political economy (IPE) is framed largely as the international extension of domestic macroeconomics, focusing centrally on the movement and management of money beyond the borders of a single sovereign state. Some two decades ago, I published a review essay surveying what then seemed to be a consensus understanding of the IPE of money (Cohen 1996). Of the many changes that had occurred in previous years, I suggested, none was so dramatic as the resurrection of global finance. Like a phoenix risen from the ashes, financial markets had taken flight and soared to new heights of power and influence in the affairs of nations. My aim was to highlight what the scholarly literature had to say about how the massive increase of capital mobility had come about and what might be its economic and political implications.

A generation later, as the globalization of finance has come to be taken for granted, the time seems ripe to revisit the IPE of money. How has the study of money evolved over the last two decades, and what is the understanding of international monetary politics today? The purpose of this essay is to survey developments in mainstream scholarship in more recent years, as illustrated by what appears in leading professional journals and in books published by the most widely respected university presses, particularly in the United States. These venues may not capture the full *breadth* of contemporary monetary studies, but arguably they do succeed in highlighting what is done at the *peak* of the field, where standards are established and ambitions are defined. They indicate what work has come to be considered most worthy of respect. My coverage of the literature is necessarily selective but may, I believe, be considered representative.

The main message of this essay is – to be blunt – disappointment. Of course, there is much to praise in the way the study of money has developed. The technical quality of most published scholarship today is undeniable. The level of refinement has never been higher. But, sadly, there is also much to regret. For the most part, the literature has become increasingly insular and introspective, largely detached from what goes on in the real world. Two parallel developments are responsible, both reflective of wider trends in mainstream IPE.

First is a steep decline of interest in broader systemic issues. At the dawn of the modern field of IPE, back in the 1960s and 1970s, structural analysis predominated in monetary studies. Attention was directed most to the overall system – how money flows were structured and managed on a global basis (e.g. Cohen 1977; Kindleberger 1973; Strange 1971b). Even as late as the 1980s and 1990s, systemic concerns were evident in responses to the revival of global finance. But more recently the pendulum has swung sharply toward the domestic level of analysis. Particularly striking is the dearth of response by mainstream scholars to the global financial crisis (GFC) of 2008–2009, which nearly landed the world in another Great Depression. More than ever, the broad structures within which monetary relations are governed would appear to be under attack, whether from the competitive behavior of private market actors or the rival interests of national governments. Yet, with only the rare exception (Drezner and McNamara 2013), remarkably little attention is now paid to the system as a whole. Referring to conventional IPE's failure to anticipate something like the GFC, I have spoken elsewhere (Cohen 2009) of the field's 'grave case of myopia'. If anything, in the years since the crisis, the myopia of monetary scholarship has become even worse. Researchers, it seems, would rather talk more about what happens nearer to home, *within* states, than about what goes on *between* nations.

And second, paralleling the first, is a marked loss of interest in practical policy solutions – a shift of attention from the management of outcomes to the analysis of inputs in the policy process. Where scholarship once was concerned with complex issues of strategy and governance, research now tends to concentrate more on narrow explorations of actor preferences or behavior, with particular emphasis on apparent anomalies. In lieu of worries about *problems to be resolved*, research tends to be driven by curiosity about *puzzles to be explained*. Analysis of the microfoundations of policy may be intellectually gratifying, but it fails to do much to improve the world around us. A good amount of work on money today labors mightily to produce, at best, banal results. Some efforts, regrettably, fail to rise much above the level of the trivial.

These twin developments are not unrelated. The roots of both trace back, above all, to the framing effect of epistemology – the demanding methodological standards that mainstream IPE has set for itself. The fault lies, first and foremost, with the high priority that has come to be given to formal scientific method – what is often described as a hard science model. This is an affliction that plagues all of IPE today, not just studies of money. To be considered truly respectable, it seems, scholarship in the field must be based firmly on the twin principles of positivism and empiricism, which hold that knowledge is best accumulated through an appeal to objective observation and systematic evaluation of evidence. Excessive emphasis is placed on technical sophistication, the elegance of theoretical reasoning, and the rigor of empirical testing. The result, sadly, is a literature that strays far from the challenges and anxieties of messy everyday life. In the IPE of money, neither structural analysis nor policy studies receive as much attention as they deserve.

The organization of this essay is as follows. I start with some essentials about international money flows and policy options that are needed to frame monetary analysis. The next section surveys principal developments in the mainstream monetary literature, highlighting the sharp swing of the pendulum toward the domestic level of analysis. Trends in scholarship on the international politics of money are then reviewed, followed by a discussion of critical approaches to monetary analysis and how they relate to more conventional studies of money. A final section concludes with some brief thoughts on the pernicious role of formal scientific method in the IPE of money today.

ESSENTIALS

Monetary analysis starts with the *balance of payments* – the accounting of all monetary transactions between the residents of a nation and the rest of the world. Every nation, by definition, has a balance of payments. The problem is that the balance of payments does not always balance. The economy may run either a surplus or a deficit. That is what we mean by external imbalance (disequilibrium). The question then is: What can the country do about it? For an economy in deficit, there are two broad choices: *financing* or *adjustment*. Either the imbalance must be paid for, or it must be eliminated.

Financing means finding the wherewithal to pay for the excess of external spending over revenues, either by running down accumulated foreign assets or by borrowing abroad in some form. Either way, the state's balance of international indebtedness – its net worth – worsens. And therein lies the rub, because the deterioration of net worth cannot go on forever. Sooner or later, foreign assets and borrowing limits will be exhausted, which means that sooner or later the deficit will have to be eliminated. That is what is meant by adjustment.

In principle, adjustment can be achieved by using any of three classes of policy instrument. These are what may be called the three D's – depreciation, deflation, or direct controls. Depreciation (or devaluation) means lowering the exchange rate of the home currency, reducing the price of domestic production relative to foreign goods and services, and thus encouraging an improvement of the trade balance. Deflation (also known as internal devaluation or austerity) means acting to reduce the overall level of spending in the economy, thus lowering imports. Macroeconomic restraint may be achieved through either monetary policy (the central bank's control of money supply and interest rates) or fiscal policy (the government's own spending and revenues). And direct controls mean making use of available policy instruments to limit import volumes (tariffs and non-tariff barriers) or outward flows of capital (capital controls and exchange restrictions).

In practice, however, policy choices are tricky, for two reasons. First, trade-offs are required. The challenge is encapsulated in what has been labeled the Unholy Trinity (Cohen 1993) – the intrinsic incompatibility of exchange-rate stability, capital mobility, and monetary policy autonomy, first identified by economist Robert Mundell (1968). In the early decades after World War II, governments could afford to pay little attention to the Unholy Trinity, since international capital markets had not yet recovered from the effects of prolonged depression and military strife. But then global finance staged its dramatic comeback, leading to a degree of monetary integration not seen since the end of the nineteenth century. More and more, it was felt, capital mobility was becoming something akin to a structural feature of world politics – an exogenous attribute effectively limiting governments to a binary trade-off between currency stability and monetary autonomy (Andrews 1994; Goodman and Pauly 1993). The central question became: What could be done to cope with the constraint imposed by increasingly massive flows of money?

Second, externalities are involved. One country's deficit is, by definition, another's surplus. In the absence of inter-planetary trade, the global balance of payments is a zero-sum game. Whatever one nation does to manage its balance of payments, there will inevitably be impacts on external balances elsewhere. The system is interdependent and can easily degenerate into policy conflict or worse if not effectively governed through some form of collective action.

It is these tricky trade-offs and externalities that make the subject of money so fascinating to students of IPE.

DOMESTIC POLITICS

Strikingly, the largest part of mainstream scholarship over the last 20 years has come to focus primarily on the *domestic* politics of monetary or exchange-rate choices, in a manner that is actually more akin to *comparative* political economy than to *international* political economy. The standard approach is 'inside–out', comparing and contrasting selected attributes of states at home that may be assumed to help shape their economic policies abroad. The approach reflects the popularity of the so-called Open Economies Politics (OEP) paradigm as codified not long ago by David Lake (2009). If the resurrection of global finance was the dominant influence in monetary studies two decades ago, the OEP paradigm has come to rule the IPE of money in more recent years.

For analytical purposes, OEP decomposes the policy process into three successive steps – starting at home with the formation of constituency interests; turning next to how interests are aggregated and mediated through domestic political institutions; and only then, finally, moving outward to the international stage where states maneuver or bargain to influence outcomes. In principle, all three stages matter. But that is hardly the way it works in practice, judging from what actually gets published, particularly in the leading journals. The first two stages, manifestly, receive the lion's share of attention. For mainstream scholars, especially in the United States, nothing seems more important than to understand the role of domestic interests and institutions in public policy. Systemic considerations are downplayed or ignored in what Thomas Oatley (2011), in a stinging criticism, calls the 'reductionist gamble' – the bet that results attained by reducing analysis to the driving force of domestic politics will not be moderated or distorted by developments at the international level. Stephen Chaudoin et al. (2015) confirm that in many cases the reductionist gamble does indeed miss important interactions between the domestic and international levels of analysis.

Interests

OEP begins with groups of individual actors – e.g. enterprises, industries, or factors of production – who might reasonably be expected to share more or less the same interests, defined as preferences over alternative outcomes. The assumption is that preferences can be derived from established economic theories highlighting the distributional implications of different national policies. With respect to monetary or exchange-rate issues, most research in this vein may be said to trace its lineage back to a seminal article by Jeffry Frieden (1991), which focused on how growing levels of financial integration might be expected to influence the policy preferences of influential constituencies. For Frieden, the key distinction was between 'integrationist' and 'anti-integrationist' forces – internationally oriented sectors, on the one hand, and more domestically oriented groups on the other. Integrationist forces would be expected to favor stability of the exchange rate over monetary autonomy; anti-integrationists, the reverse.

In the years since, much effort has gone into building on Frieden's original insight. The basic question is: Which actors matter? For some scholars, the relevant range of actors may encompass, more or less, the entire electorate. A notable recent example is provided by Stefanie Walter (2013), who emphasizes the role of 'voters' in determining adjustment policies. In her words: 'Voters who overall are less vulnerable to depreciation than monetary and fiscal tightening will prefer external adjustment to internal adjustment, and vice versa.... [Such] distributional issues strongly influence the politics of macroeconomic adjustment' (Walter 2013: 17). But electoral models of this sort belie a faith in political pluralism that would hardly seem to correspond to reality, where particularly powerful constituents often exercise a degree of influence far out of proportion to their actual numbers. Most scholars working on these questions are inclined to assume that, in practice, some domestic actors matter more than others. Sectoral models are preferred.

What sectors? For some, the dominant influences are to be found in the financial-services industry (Hefeker 1997; Henning 1994), whereas for many others it is the producers of material goods that deserve the most attention. Among goods producers, broad political cleavages would be expected to develop naturally depending on whether an industry's output is export-oriented, import-competing, or non-tradable. Much appears to rest, however, on context. Typical of recent work is the 'conditional preference theory' of David Steinberg (2015), highlighting the key role of the manufacturing sector, which he describes as a 'major driving force behind exchange rate policy' (2015: 8). However, the preferences of manufacturers, Steinberg argues, will be conditional, depending on a variety of political, economic, and institutional circumstances. Likewise, Frieden (2015) himself acknowledges the contingency of producer preferences once account is taken of such considerations as product differentiation, the share of foreign inputs in domestic output, and each firm's net foreign-currency liabilities.

Overall, sectoral models provide useful insight into the process of interest formation. But as a practical matter, they present a serious problem: how to convert the abstract categories of economic theory into real, live humans. Actors may be torn by conflicting distributional implications. What if a firm produces largely tradable goods, which would dictate a preference for depreciation, but also has large net foreign liabilities, which would suggest a preference for appreciation? Or what if an investor holds a large volume of foreign assets, which might dictate a preference for exchange-rate stability, but also invests heavily in domestic nontradable production, which suggests the reverse? How do we know which material interest will prevail?

Worse, how do we know that any of these material interests will necessarily count more than other non-egocentric considerations? Societal actors may have more on their mind than personal gain alone. Survey evidence analyzed by Edward Mansfield and Diana Mutz (2009) suggests that in many cases, preferences reflect not material self-interest but rather have a 'sociotropic' basis, defined as being determined by whether policy choices are felt to be good for the nation as a whole. Attitudes toward various out-groups also appear to play a role.

In reality, it is extremely difficult to line up socioeconomic groups clearly on any monetary or exchange-rate issue. A fair amount of empirical work has sought to test for the role of sectoral preferences (Bearce and Tuxhorn 2015; Broz et al. 2008; Frieden 2015; Hobolt and Leblond 2009), with mixed results at best. As Frieden concedes in a co-authored review essay (Broz and Frieden 2001: 327): 'Measuring group preferences and political influence is never easy, and data limitations leave analysts with crude proxies.' Attitudes tend to be opaque and fluid. The actors, it turns out, are complex beings with heterogeneous and often confusing mixtures of motives that cannot be as easily categorized as parsimonious theoretical models would have us believe. One wonders why so much effort has gone into such seemingly inconsequential endeavors.

Institutions

Even if we could specify interests precisely, another challenge remains: How are preferences translated into policy? Except in the most extreme pluralist model, the path to policy is no simple matter. As Frieden (1991: 450) cautioned from the outset, analyses of preferences 'have to do with the interests in play, not necessarily with the outcome of political conflict among them'. Much depends on how interests are aggregated and mediated through domestic political institutions – the second stage of the OEP paradigm. Much effort has gone into this issue, too, with only modestly better results.

For some scholars, such as Beth Simmons (1994) or David Bearce (2003), the place to look is in the structure of political parties, which might be thought to mobilize group preferences in reasonably consistent fashion. Rightist parties, for instance, historically have been more closely tied to big internationally oriented interests and so might naturally be expected to favor exchange-rate stability over monetary autonomy; whereas the reverse might be characteristic of leftist parties, which are traditionally allied more with labor groups that may bear the brunt of adjusting the domestic economy to external conditions. But here too there is a problem of translating theory into practice. Most political parties have multiple policy objectives, blurring classic right–left distinctions, and few elections are fought on monetary issues alone. As a result, partisan preferences are often quite contrary to what an exclusively class-based model might predict. Hence here too, not surprisingly, empirical research has tended to produce mixed results (Broz and Frieden 2001: 328–329).

Somewhat more successful are studies that look at the political regime itself – specifically, whether governments are democratic or authoritarian. Here, there is more consistency in the empirical evidence, suggesting a reasonably strong correlation between political regime type and monetary choices. Such scholars as David Leblang (1999), Lawrence Broz (2003), and David Bearce and Mark Hallerberg (2011) all find evidence that democracies are significantly less likely to adopt a fixed exchange rate than are non-democratic systems. Confusingly, though, their explanations differ. For Leblang (1999), the reason lies in the paucity of domestic opposition in autocracies. Authoritarian governments are more likely to peg because they are more insulated from domestic audiences and thus bear lower political costs for allowing the domestic economy to adjust to the exchange rate. For Broz (2003), the reason lies in the opacity of the policy-making process in autocracies. The transparency of a peg substitutes for political system transparency. And for Bearce and Hallerberg (2011), the reason lies in the absence of voting in autocracies. In democracies, electoral pressures can be expected to push the government in the direction of a more flexible exchange rate, since the median voter is likely to be a domestically oriented producer with a bias in favor of monetary autonomy.

Despite these differences, the empirical finding seems robust – indeed, perhaps the most consistent result in all of the extensive literature on the domestic politics of monetary relations. But it is also a rather crude observation, given how much variation there is among both democracies and autocracies. Can the result be refined any further?

Some scholars have tried. David Steinberg and Krishan Malhotra (2014), for instance, see value in distinguishing between three different kinds of authoritarian regime – civilian dictatorships, monarchies, and military regimes. They do not dispute the view that authoritarian regimes *in general* are more inclined to peg. However, drawing on selectorate theory, which focuses on the size of the population that is able to determine a government's leadership, they argue that some autocracies are more likely to maintain a fixed exchange rate than others. Their evidence suggests that among authoritarian governments, military and monarchic dictatorships are more likely to peg, since such regimes tend to have smaller selectorates. By contrast, fixed exchange rates are less likely in civilian dictatorships, where selectorates are larger. Interestingly, as Steinberg and Malhotra point out, this makes civilian dictatorships more like democracies, which also have larger selectorates.

Along similar lines, William Bernhard and David Leblang (1999) have highlighted a key distinction between two types of democratic systems – single-party *majoritarian* electoral systems, which are essentially winner-take-all, and *proportional representation* systems, where bargaining among parties determines the composition of a government. Electoral stakes are higher in majoritarian systems than in proportional representation systems, since in the latter a party may lose votes in an election but still retain the possibility of participating in a coalition government. Politicians in a majoritarian system, therefore, might be expected to have a stronger preference for domestic policy autonomy, to preserve the option of using monetary policy to influence an election's outcome. The constraint of a fixed exchange rate will impose lower electoral costs in a proportional representation system.

All this assumes, however, that monetary policy is under the thumb of the elected government. But what if the central bank is formally independent of political control? There was a time when it might have seemed counter-intuitive to suppose that politicians would willingly abandon their grip on such a powerful tool of economic management. Yet in practice, many governments have done so, primarily in order to enhance the credibility of monetary policy commitments. As William Bernhard et al. (2003) suggest in their introduction to an influential collection of essays, central bank independence (CBI) and a pegged exchange rate can be regarded as substitutes for one another – alternative forms of monetary commitment. Hence, the two options should be analyzed as a joint policy choice in which governments simultaneously weigh the costs and benefits of both institutional arrangements. Later analysis by Bearce (2008) appears to confirm that most mature economies treat CBI and a currency peg more as substitutes than as complements.

Studies like these demonstrate that there may well be room for greater refinement of our understanding of the role of domestic institutions in such matters. But here too, as in the analysis of interests, it is evident that no simple parsimonious results can ever be expected. Institutions too tend to be opaque and fluid, with effects that cannot be easily categorized. Hence, here too one may wonder whether all these efforts represent the wisest investment of intellectual capital.

INTERNATIONAL POLITICS

What about *international* politics? This brings us to OEP's third stage, where comparative political economy is supposed to give way to more strictly international analysis. In principle, this stage of the paradigm is no less salient than the first two. Certainly, the questions are as fundamental. How are monetary relations structured, how do states manage the externalities inherent in the balance of payments, and what can be done to improve overall governance of the monetary system? Yet, by comparison with the abundance of attention paid to domestic interests and institutions, questions like these now attract far less formal study. In practice, OEP's third stage is the mainstream's sad little orphan – unclaimed and relatively neglected.

As indicated, that was not always true. In the early years of modern IPE, much more emphasis was placed on structural analysis. But that focus has tended over time to dim, as mainstream scholarship has turned more and more to the domestic politics of money. The trend is particularly evident in the leading journals, where the most prestigious work is very much in the 'inside–out' tradition. Today, like the late American comedian Rodney Dangerfield, systemic considerations just 'don't get no respect'.

That does not mean that interest in the overarching global system has wholly disappeared. The pendulum has not swung that far. But it does mean that structural analysis has tended to become less ambitious than was once the case. The international politics of money today is pursued most often in a more narrowly drawn selective fashion, deconstructing the system into more easily managed research topics. The global structure is unbundled and addressed on a piecemeal basis. Many issues are studied – everything from reserve management or capital controls to regional monetary integration and the role of international organizations. Impressive books have been produced by some of the most familiar names in the field, including Rawi Abdelal (2007), Jeffrey Chwieroth (2010), Daniel Drezner (2014), Eric Helleiner (2014), Jonathan Kirshner

(2003), and Layna Mosley (2003). For the most part, however, connections between issue areas tend to be disregarded and holistic approaches are rare. The result is a body of scholarship that is fragmented and uneven, lacking any kind of common vision.

Representative are three issues that have stood out in the recent literature: collective action, monetary power, and currency internationalization.

Collective action

The fundamental challenge of international monetary politics is to achieve and maintain some degree of consistency among the competing priorities of nearly 200 sovereign states. Given the externalities that are integral to the payments adjustment process, the risk of serious divergences is ever present. In the absence of a formal world government, some form of collective action would seem called for to avert the threat of policy conflict or worse. A proper issue for scholars is to illuminate how collective governance could work in monetary affairs and what might be done to ensure its effectiveness.

Earliest discussions naturally looked to the possibility of overt policy coordination by major governments and central banks. But that is easier said than done. Why? Perhaps the best explanation was offered by Michael Webb (1991), pointing to financial globalization. In the first decades after World War II, monetary cooperation was a relatively uncomplicated affair limited essentially to coordination of payments financing. The idea was manage imbalances caused by inconsistent national macroeconomic policies. But with the resurrection of global finance, that was no longer enough. Now collective action would have to shift to coordination of monetary and fiscal policies themselves, which is inherently more difficult to achieve. The risk that politicians might renege on policy commitments that over time prove to be inconvenient – the so-called 'time-inconsistency' problem – was ever present.

Could the time-inconsistency problem be contained? The perennial challenge has attracted much attention in the recent literature, but little agreement. For some scholars, a practical solution might lie in *legalization* of commitments, defined as a form of institutionalization binding states to formal rules subject to procedures of international law (Goldstein et al. 2001). Beth Simmons (2001), for instance, shows how legalization in international monetary relations can help governments make credible policy commitments to market actors. For others, a possibility might lie in *delegation*, granting authority to less politicized public agencies for implementation of commitments. A prime example is provided by David Andrew Singer (2007), who explores the role of regulators in the banking, securities, and insurance industries – all ostensibly autonomous members of the governing establishment. Experience, he notes, is variegated. Though sometimes motivated by genuine systemic concerns, financial regulators are often subject to threats of political intervention that constrain their ability to effectively manage monetary relations. And yet others find inspiration in *evolution* – historical processes of learning and adaptation that have, at times, worked to resolve governance issues in the past (Andrews et al. 2002).

But why should collective action be restricted to state actors alone? Starting around the turn of the century (Cutler et al. 1999; Hall and Biersteker 2002), attention increasingly has shifted to the role of the private sector in financial and monetary governance. In some cases, market actors appear to operate more or less on their own to set the rules in specific issue areas. Prominently, these include US bond-rating agencies which, with their power to establish who may be considered creditworthy, have in the opinion of Timothy Sinclair (2005) effectively become the 'new masters of capital'. More often, market actors and other elements of civil society are incorporated into more complex arrangements together with representatives of the public sector, as explored in a number of recent studies (Abbott et al. 2015; Büthe and Mattli 2011; Mattli and Woods 2009).

To date, however, no coherent picture emerges to resolve the fundamental challenge of monetary governance. In a decentralized system of world politics, where territorial states cling to as much of their traditional sovereignty as possible, incoherence may be unavoidable. But the piecemeal approach of the formal literature surely does not help.

Monetary power

Curiously, until recently mainstream scholarship had little to say about the role of power in international monetary politics. This is certainly striking given how much emphasis was placed on such considerations in earlier structural analyses. For instance, Charles Kindleberger's (1973) masterful statement of what became known as the theory of hegemonic stability was largely about money and power. But then a hiatus set in that lasted more than two decades, perhaps because of the widespread perception at the time that the geopolitical dominance of the United States seemed to be fast fading into history. Seemingly, there was no longer a hegemon to talk about. Monetary power, in the words of Jonathan Kirshner (1995: 3) became 'a neglected area of study'.

Interest in power revived, however, with the appearance of Kirshner's seminal *Currency and Coercion* (Kirshner 1995), which reminded us that power matters even if it is not concentrated in a single dominant actor. And Kirshner's seminal book in turn triggered an influential collection of essays edited by David Andrews (2006), which emphasized the critical distinction between monetary power as a *theoretical concept*, focusing on its meaning and sources, and money as an *instrument of statecraft*, concerned with the deliberate exploitation of monetary relations to influence others. These two sources together have become the fountainhead for subsequent research on monetary power.

In the Andrews volume, exploring monetary power as a theoretical concept, I located the principal sources of monetary power – its macro-foundations – in the ability to minimize costs of payments adjustment (Cohen 2006). Monetary power, I argued, has two 'hands', a power to *delay* adjustment costs and a power to *deflect* adjustment costs, each with its own distinct roots in a country's financial or structural attributes. This dual approach has caught on in discussions of international monetary politics and is being increasingly applied in analysis (e.g. Hardie and Maxfield 2016; Otero-Iglesias 2014; Vermeiren 2014).

Kirshner's book, by contrast, was mostly about monetary statecraft, which he argued could take any one of three forms: currency manipulation, enforcement of monetary dependence, or systemic disruption. Some sources have questioned the underlying logic of Kirshner's framework, suggesting that his three forms of statecraft are neither mutually exclusive nor, in analytical terms, fully comparable (Andrews 1996). Nonetheless, this approach too has caught on, with subsequent work seeking to build on Kirshner's insights to explore the purposive use of monetary power in greater depth. Illustrative is a recent paper by Leslie Armijo and Saori Katada (2015) on the financial statecraft of emerging economies. Relevant strategies, they suggest, can be sorted in terms of three dichotomies: defensive or offensive, bilateral or systemic, and financial (involving credit and investment flows) or monetary (involving exchange rates and currency). Depending on circumstances, different countries can be expected to combine these elements in different ways in pursuit of national policy objectives.

Currency internationalization

Considerations of monetary power, in turn, have naturally led as well to a renewed interest in international currencies – national monies that come to play international roles. For an earlier generation of scholars (Cohen 1977; Strange 1971b), writing decades ago, currency internationalization clearly connoted a measure of power. Strange (1971a) even sought to develop a formal taxonomy of different types of international currency, each with its own implications for monetary politics. Today, it is understood that an international currency increases a country's power to delay adjustment costs, since foreign deficits can be financed with one's own domestic currency – a benefit that is often referred to as the issuer's 'exorbitant privilege'.

But then here too there was an extended hiatus until the 1990s, when preparations for the birth of Europe's new euro stimulated a return to the subject. For the first time, the global dominance of the US dollar seemed seriously threatened – and with it, US power. More and more, it became clear that currency internationalization has a geopolitical dimension. Since the start of the new millennium, prospects for the greenback, euro, and most recently China's renminbi have been hotly debated (Cohen 2011; Eichengreen and Kawai 2015; Helleiner and Kirshner 2009).

In a book published in 1998 (Cohen 1998), I stressed the rising level of overt competition among currencies – another consequence of financial globalization. The erosion of barriers to money flows had effectively 'deterritorialized' currency. Market actors were now freer to make choices about what moneys to use for domestic or international transactions, significantly altering the distribution of authority in monetary affairs both between governments and between the public and private sectors. Building on Strange's taxonomy, I also introduced the image of a Currency Pyramid to more fully represent the hierarchy of currencies around the world. A new vision of monetary geography was needed, I contended, in order to more fully comprehend the changes occurring in international monetary politics. Subsequently, I explored in detail the mutually endogenous relationship between currency internationalization and state power (Cohen 2015). An international currency, it turns out, is a double-edged sword, either augmenting or compromising a government's authority depending on the uses to which it is put.

Others have taken up selected dimensions of the topic. Some focus on the ways in which politics can influence a money's international standing. Helleiner (2008), for instance, sees two channels of influence – one operating indirectly through impacts on the key economic determinants of internationalization; the other operating more directly through deliberate government initiatives to promote use. Another notable example is an insightful paper by Kathleen McNamara (2008) stressing the interaction of material factors and the role of ideas in determining a money's international standing. Other commentaries are more concerned with the reverse: how a money's standing can affect a government's ability to project power. A good example is provided by Paul Viotti (2014), who describes an international currency as the 'monetary component of hard power'. Others who have written along similar lines include Carla Norrloff (2010), Jonathan Kirshner (2014), and Thomas Oatley (2015). From all these contributions, much insight has been gained, but here too the literature remains fragmented and uneven.

CRITICAL APPROACHES

The OEP paradigm can be credited for providing a handy framework for work at both the domestic and international levels of analysis (even if the international level is relatively neglected). But the approach can also be faulted for its relatively narrow focus on actor behavior set within a broader structure whose attributes, typically, are assumed to be given and relatively stable. Even scattered studies at OEP's third stage, unbundling international monetary politics, largely avoid thinking about the system as a whole. The time horizon for research tends to be relatively short, and the sovereign state occupies center stage as the principal unit of analysis. Larger issues of system governance or long-term systemic transformation are effectively swept under the rug.

In none of these respects, of course, is monetary scholarship an anomaly. Quite the contrary, in fact. As indicated, the swing of the pendulum in the study of money is part and parcel of much wider trends in the field of IPE – particularly in the pre-eminent version of the field that I call the 'American school' of IPE. Over the last decade, since two interventions of my own (Cohen 2007, 2008), a global conversation has been triggered over the proper nature and direction of the discipline (Blyth 2009; Leander 2015; Phillips and Weaver 2011); and broad surveys of IPE scholarship have heightened awareness of the remarkable diversity of the field (Cohen 2014; Paquin 2016). Yet, in the mainstream literature, the narrow focus of the OEP paradigm has sustained its stultifying grip. As Robert Keohane has written, 'The "new IPE", as embodied in the open economy politics approach… is remarkably reluctant to focus on major changes taking place in world politics… . I would urge scholars now active in the IPE field to spend more of their time pondering the big questions about change' (Keohane 2009: 34, 42).

Among mainstream scholars, to date, Keohane's advice has largely fallen on deaf ears. But that is not true everywhere. Beyond the American school, among theorists of a more 'critical' persuasion – often labeled 'heterodox' or 'radical' – the admonition could not be more welcome. Critical theorists include not only those that I have labeled the 'British school' but also many of the other clusters of IPE scholars that can be found around the world (Cohen 2014; Paquin 2016). Critical theorists reject the tendency to unbundle international monetary politics into neatly separable topics for research. For them, priority must be given to bigger questions having to do with the evolution of the system as a whole, understood in terms of vast and complex social structures. The core 'problematique' is systemic transformation. The time horizon is epochal and the state is treated as just one actor among many with an influential role to play.

The roots of critical approaches to the study of money go far back. The model was set by Susan Strange in a series of memorable volumes (Strange 1986, 1994, 1998) before her untimely death in 1998. For Strange, history was being fundamentally rewritten by the resurrection of global finance. Governments were not just facing a more difficult trade-off. They were being completely neutered, losing every last shred of their traditional monetary authority. As early as the mid-1980s, she was already expressing concern about ‘an international financial system in which the gamblers in the casino have got out of hand, almost beyond, it sometimes seems, the control of governments’ (Strange 1986: 21). A decade later, she was convinced that the casino had gone crazy. ‘The financial markets’, she declared, have ‘run beyond the control of state and international authorities’ (Strange 1998: 1).

Numerous variations have followed from others, emphasizing two themes above all – power and crisis. For critical theorists, the distribution of power in the global system and the risks and consequences of systemic crisis are the central questions for monetary analysis in IPE.

First, who is in charge? Heterodox theorists have no doubt that power matters in monetary relations – but not in the instrumental sense emphasized by Kirshner and other mainstream scholars. In critical analysis, power is understood more in the ‘structural’ sense suggested by Strange: ‘the power to shape and determine the structures of the global political economy… . the power to decide how things will be done, the power to shape frameworks within which [actors] relate to each other’ (Strange 1994: 24–25). In other words, power is the capacity to set the agenda that defines the choices available to others. Earlier work tended to look for a single source of emerging structural power in the monetary system, variously locating authority in ‘American empire’ (Panitch and Konings 2008), private financial institutions (Germain 1997), or even more vaguely, ‘global capital’ (Gill and Law 1989). But with the passage of time, it has come to be recognized that structural power is undoubtedly more complex and dispersed than previously thought – ‘transnational pluralism’, in Philip Cerny’s (2010) catchy phrase. Fresh insights have been provided by, *inter alia*, Helleiner (2006), Paul Langley (2009), and a recent collection of essays from the likes of Craig Murphy, Diana Tussie, Herman Schwartz, and Ronen Palan (Germain 2016). By no means, however, have opportunities for research on the continuing reconfiguration of authority in monetary affairs been fully exhausted.

Second, is the system in crisis? For many critical scholars, the GFC confirmed what they had believed all along – namely, that the global system was highly vulnerable and bound, sooner or later, to crash (Nesvetailova 2007). Strange, they feel, was right – casino capitalism had indeed gone mad. Sanity could not be restored without a fundamental transformation of monetary institutions. Admittedly, not everyone agrees. Informed observers have noted how, in practical terms, not all that much has actually changed as a result of the worldwide meltdown in 2008. In Helleiner's words: 'The crisis turned out to be much more of a status quo event … than a transformative one' (Helleiner 2014: xvii). But that does not stop commentators of a more critical persuasion from thinking seriously about what one calls the 'spectre of capitalist crisis' (Gamble 2009). Representative is an innovative analysis of global financial governance by Langley (2014), relying heavily on insights from cultural economy. Also noteworthy are two recent collections of essays by younger scholars with new perspectives to offer (Cafruny and Schwartz 2012; *New Political Economy* 2015).

The question is: How much do these discussions of power and crisis connect with more conventional American-style research? The answer, sadly, is: Hardly at all. Little work of a critical persuasion is cited in the mainstream literature. Indeed, apart from Strange, few heterodox scholars are even known to the mainstream community. Analysts who work in the American-school tradition have little tolerance for grand theories about social transformation, which they view as excessively prone to speculation and unsubstantiated generalization. But of course communication in the opposite direction is scarcely better. To their credit, many heterodox scholars do seem to try to be at least minimally informed about trends in more orthodox scholarship, if only to know what they dislike. But little of the broad range of conventional work, with its piecemeal approach to domestic or international politics, makes an impact in substantive terms. The gap between the two styles of monetary analysis, regrettably, remains as wide today as it was when I first started writing about it a decade ago.

The result is a loss for both. Arguably, mainstream scholarship could benefit from the insights of critical research on issues of systemic change. Though the time horizon for conventional analysis may be relatively abbreviated, it is important to remember that a succession of short-term decisions can have significant longer-term consequences. There is simply no excuse, other than analytical convenience, for assuming that the broader structure of monetary relations necessarily remains stable over time. Conversely, critical scholarship could benefit from the insights of more orthodox research on the details of each stage of the OEP paradigm. It is not enough to talk about broad social transformation from one historical epoch to another. It is also important to have an appreciation of the finer details of the process of transition – how we get from Here to There. I have long maintained that each side has much to learn from the other (Cohen 2008). I still believe that.

INSULARITY

What accounts for the insularity of conventional studies of money? Why is the mainstream literature so timid about taking on larger systemic issues? Why the fascination with *puzzles* rather than *problems*? Though many causes might be cited, I would argue that above all we should blame mainstream IPE's misguided preoccupation with methodology. Put simply, too much emphasis is placed on formal scientific method at the expense of substantive content.

In a hard science model, conjectures in some form are specified, based largely on deductive reasoning, and then systematically tested for accuracy – a process known formally as 'hypthetico-deductivism'. A premium is placed on parsimonious theoretical models that pare messy reality down to its bare essentials. The style is reductionist. The aim is to uncover core relationships – 'to predict something large from something small,' as a prominent economist once put it (Johnson 1971: 9). The assumption is that social phenomena are amenable to scientific explanation in essentially the same manner as are natural phenomena. Hence, the same principles of positivism and empiricism that are employed to isolate causal mechanisms in the physical sciences can be applied to the study of social relations as well. Universal truths are out there, just waiting to be discovered.

The advantages of a hard science model are many. One can hardly quarrel with its emphasis on rational, empirical inquiry. The approach is intended to avoid ambiguity and allows for precise, carefully calibrated observations. And through repeated testing of falsifiable hypotheses, it helps to promote a general cumulation of knowledge. Most prized are purely quantitative methods based on large sets of detailed statistical data – e.g. regression analysis or large-scale survey research. But respectability is accorded even to more qualitative approaches, such as structured case studies, textual analysis, or social experiments, so long as they appear to meet strict scientific standards.

But the approach also has a great disadvantage – an unfortunate tendency to shrink the horizons of scholarship. Attention is naturally diverted from the complex to the narrow by the practical requirements of empiricism. By definition, a hard science model depends on the availability of reliable data. Research, accordingly, tends to become data-driven, drawn away from issues that lack the requisite base of information. In effect, methodology plays a key role in defining *what* can be studied, automatically marginalizing questions that cannot be reduced to a manageable set of regressions or structured qualitative analysis. And among the most marginalized are questions about policy or the global system, which are inherently difficult to quantify. Policy studies and structural analysis are particularly resistant to a reductionist style of scholarship.

The detachment of mainstream IPE, therefore, is no accident. For some, the trend represents progress – all part of the 'maturing' of the field, as David Lake (2006) has put it. The more scholars limit themselves to a hard science model, the more the field approaches the respectability of 'normal' science. But that assessment seems altogether too kind, since it discounts the severe costs involved. The price of this kind of 'progress' is measured by how much gets left out. Little room remains for the study of complex policy issues or long-term systemic transformation. The swing of the pendulum in monetary studies, from the international to the domestic and from problem-solving to puzzles, is a direct reflection of the gradual 'hardening' of prevailing methodologies in mainstream scholarship.

Can the swing of the pendulum be reversed? That the epistemology of mainstream IPE may have become unduly narrow is by now widely recognized. A decade of debate has sensitized many to the limitations of the mainstream's reductionist style. Yet, resistance to a broader perspective remains strong, particularly in the United States. The case for change, therefore, bears repeating.

Reconsideration of how we do IPE does not require sacrificing rigor. But it does mean according greater respectability to work that is not so highly dependent on the demands of a highly refined methodology. Technical sophistication is by no means the only measure of professionalism. Equally valid are the thoughtful insights of studies that, like many critical approaches, are more historical or institutional or interpretive in tone. The key, it would seem, lies in what Peter Katzenstein (2009) calls 'analytical eclecticism' – a pragmatic research style that is willing to borrow concepts, theories, and methods from a variety of scholarly traditions, heterodox as well as orthodox, as needed. We need to reward scholarship that is driven by questions, not data.

Above all, we need to shed our reluctance to take on major issues of the day – the kind of 'big' socially important questions that, as Keohane (2009) notes, were once a central part of the field's agenda. How are monetary relations to be governed in a world of globalized finance? How are the interests of rising powers like China to be accommodated? Can finance play a role in coping with the challenge of climate change? Can social justice be promoted or inequality ameliorated through the monetary system? Eyes must be raised from the minutia of domestic preferences and institutions, as dictated by the OEP paradigm. The study of money must once again dare to expand its horizons.

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