

Economic statecraft, geoeconomics and regional political economies

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ABSTRACT

In this introduction to the special issue, we establish the overarching objective for the collection; to investigate the salience and efficacy of conceptions of Economic Statecraft (ES) and Geoeconomics for understanding and explaining shifts in state-market relationships in a number of regional political economies. After a very short overview of different generations of ES research, we establish the set of common questions that each of the papers address, and how we arrived at them as the research project evolved. We point to the importance of ensuring that ES is not just thought of as something that the more powerful regional states engage in, and the need to adopt a three-part analytical distinction between different components of ES: motivations and objectives; actions and tools; and outcomes and consequences. This allows us to trace the relationship between goals and effects, provides a basis for comparative studies, and makes it easier to make a distinction between ES and other forms of state involvement in the economy.

KEYWORDS Developmental states; economic security; economic statecraft; geoeconomics

The starting point for this special issue is to assess the salience and efficacy of 'new' (or more correctly, reformulated) ways of theorising and explaining the relationship between regional states and 'their' economic actors. To ask if there really has been a fundamental shift in basic starting points that drive policy changes, or simply a shift in emphasis and focus within existing paradigms. And if there has been a change—either a fundamental or incremental one - why? And why now?

There is of course a long history of scholarship on the relationship between states and markets in and of the region (however you define the 'region'). This is not to say it is a specific regional phenomenon. Far from it. As Ha-Joon Chang (2002) reminded us, if you take a long historical sweep, then it has been varieties of (strong) state directed development that have tended to be the norm in early development phases, rather

than the aberration that they were often argued to be during the years of neoliberal dominance. That said, it's probably fair to say that it is a region where the role of the state has been particularly prominent in research on both domestic political economies, and broader regional dynamics too.¹ Indeed, given how much has been written—not least in the pages of this journal—then it might seem a bit unnecessary to revisit the issue again. But times change, and so do the political and economic environments that policy makers try and navigate through. And in the process new concepts and approaches are developed and/or deployed to try and understand what is going on, and predict future action.

And this is where the concepts of Economic Statecraft (ES) and geo-economics come into the equation. While both have relatively long histories, both have also evolved and emerged (relatively) recently in their current incarnations as (relatively) prominent ways of explaining the methods and goals of state direction of economic activity. And the key word in the previous sentence is 'goals'. Because it is in identifying what that policy is meant to achieve that proponents of the utility of ES see a key difference with previous forms of state-market interactions (and the study of them).

Economic statecraft as evolving analytical tool

While the basic idea of ES is not new at all, its meaning and usage have both evolved over time. And it is what we might call the third generation of ES scholarship that is the focus of this special issue. Fundamentally, ES is a very simple concept, and can be understood as the use of economic means to gain foreign policy outcomes in line with a state's strategic objectives. It was propelled into common usage by Baldwin (1985) nearly four decades ago in a book that traced the effectiveness of the use of economic instruments through historical case studies going back as far as the Peloponnesian War. These examples showed, he argued, that economic means had been much more successful in getting others to change and do things they otherwise would not have done—a very simple and classic definition of power—than dominant political discourse at the time suggested.

Yet in many respects, it is the very simplicity of this understanding that makes it a problematic term. The type of economic instruments that Baldwin identifies as typical tools of ES - the facilitation or restriction of trade, aid and other financial flows - have long been (and continue to be) both deployed by states and studied by academics without recourse to the specific concept itself. This then results in questioning how much utility it has as a concept. Perhaps more correctly, how much value added it provides given the other conceptual tools that exist to study how states act and interact with economic actors at home and overseas; for example, economic diplomacy, commercial diplomacy, economic security, and even some understandings of soft power.

While the study of ES never fully went away, it witnessed something of a resurgence around the turn of the millennium in a second generation of scholarship as ‘the particularly widespread use of economic tools of statecraft since the end of the cold war ... prompted rising interest in the effectiveness of these instruments’ (Blanchard, Mansfield, & Ripsman, 1999, p. 1). Mastanduno (1999, p.288) might have been a little premature or optimistic (if that’s the right word) when he argued that ‘the dividing line between international political economy and security studies has all but disappeared’. But the apparent decline in the risk of a catastrophic global conflict after the end of the Cold War, combined with the ‘globalisation’ of economic activity, created incentives for thinking again about what was meant by security and what needed to be secured (Cha, 2000). Given the nature and extent of global flows of goods and money, how could states seek to manage vulnerabilities and guarantee economic security?

This key question remains important today. As the papers in this issue all show to different degrees, attempting to reduce potential vulnerabilities is an important consideration for all regional states. But there is a key difference in the understanding of what ES is meant to achieve. It is not just a case of getting others to change their behaviour, or of achieving greater economic autonomy and security and managing security externalities. ES is now perceived as a means of attaining (often grand) geopolitical objectives. Indeed, for Weiss and Thurbon (2021), it is exactly these (geo) strategic considerations and objectives that establishes ES as being different from other ways in which states interact with markets; for example, those state interventions designed to deliver developmental objectives.

ES, geoeconomics, and asymmetries

In this latest definition, the conception of ES often sits alongside its close relative, geoeconomics. As with ES, at a basic and fundamental level the idea behind geoeconomics, at least as proposed by Luttwak (1990, 1993), is also very simple; power competition will be based on economics rather than military conflict. Indeed, in some of its more simple definitions such as ‘the use of economic tools to advance *geopolitical* objectives’ [emphasis added] (Schneider-Petsinger 2016), it is not always clear where ES ends and geoeconomics begins as both share the emphasis on the geopolitical consequences and goals of state directed international economic activity.

Realistically, only a few countries probably have the ability to generate consequences that have truly global consequences (as the geo in geoeconomics and geopolitical suggests); to shift the distribution of power across the globe, and/or result in changes in the way that the world is ordered. So if ES and geoeconomics are only what the most powerful states can do, then they are concepts with rather limited utility. However, Mattlin and Wigell (2016) show that there are disagreements over what the spatial dimension of any particular action has to be for it to be

'elevated' to the level of **geo**economics. This creates room for international consequences to be included as being geoeconomic and geopolitical ones even if they aren't whole earth ones, but have more limited (and not always clearly defined) scopes. Moreover, Blackwill and Harris (2016, p. 9) include in their definition of geoeconomics the importance of 'the effects of other nations' economic actions on a country's geopolitical goals' and also defending 'national interests'. This perhaps leaves the thorny question of whether simply trying to enhance national economic autonomy in response to the geostrategic objectives of others counts as a geopolitical goal or not? Nevertheless, it is a definition that opens up the study of ES and geoeconomics to defensive and reactive objectives, and maybe domestic ones as well. And this idea of a defensive ES is particularly apposite for a number of the case studies in this special issue.

Scholvin and Wigell (2019, p. 4) argue that a key component of the power dynamic of geoeconomics is asymmetric interdependence, as it is this asymmetry that creates risks and dependencies; the ability of one side to exploit their position to get the other to do things that they would not otherwise have done. On its own, asymmetry is not a source of insecurity. If you have confidence and faith in your partner's ambitions and trustworthiness, then there is nothing to fear. Insecurity is only there if you do not trust the other actor to exploit these asymmetries for their gain and your loss. Such a fear that others might 'weaponize [asymmetric] independence' (Farrell & Newman, 2019) can create insecurity even if nothing has yet been done, as insecurity is often based on perceptions (and lack of trust of) future actions rather than a direct response to concrete past action. The material relationship in itself at any one time is arguably less important in establishing insecurity than perceptions of the long-term objectives of others.

And one key difference between early scholarship and more recent studies of ES is that the most often studied case study has changed. Given the role of the US in the global order, it is not surprising that it became the major focus of earlier ES scholarship. And as it remains the predominant global power, it is also not surprising that what the US government says and does (not least to maintain its position in the face of emerging challengers) is still important today. That said, there has been a much firmer focus than before on the 'new kids on the block' (Armijo & Katada, 2014); on how emerging/rising powers can use economic statecraft to try and increase their global influence and power in a world where the US remains the major power (and not just militarily).

But of all the rising powers, it is China that has become the main focus of attention. And it is China that of all the regional (and other emerging) states is assumed to have the capabilities and readiness to first deliberately build asymmetries, and then use them for geopolitical purposes. Although not the first work to highlight China by some distance, Norris's (2016) identification of the use of Chinese commercial actors as tools of Chinese grand strategy seemed to catch the moment. While there remains

considerable debate (and lack of clarity) over whether there really is geo-strategic intent behind what is being done by Chinese overseas (Kastner & Pearson, 2021), it rang true to those who were not just concerned about what China had done so far, but also what it would do in the future.

Notwithstanding the questions noted above over what it means to have a global impact, collectively (if not individually) for those who have identified Chinese statecraft in action, it truly is everywhere (Li, 2017); in Asia and the Indo-Pacific (Gong, 2020; Reilly, 2021), Europe (Reilly, 2017, 2021) Africa (Morgan, 2019), Latin America (Wei, 2019), Australia (Wong, 2021), the Middle East (Altin, 2022), across the Belt and Road (Kostecka-Tomaszewska & Krukowska, 2021) and in the US (Norris, 2017). And it is not just what China is doing itself that is important here. While the father of geoeconomics was predicting a ‘geo-economic resistance’ to China’s own strategies back in 2012 (Luttwak, 2012), more recently the ‘pushback’ against China has become more widespread and global too, with countries across the world adopting their own defensive strategies to ward off unwanted Chinese attention (Roberts, 2021, pp. 276–280).

For a journal like the Pacific Review, this emphasis on China is in many ways a great opportunity as not just China itself, but many of the countries most immediately affected by the expansion of Chinese overseas economic activity, fall within our geographical area of interest. Indeed, it was this focus on China—and in particular, discussions on whether the Chinese state always has the ability to direct economic action to achieve state goals as per ES assumptions—that lead to the decision to develop this special issue in the first place.² But as we will see, it has also generated some problems too when it comes to definitional precision, and also the ability to undertake cross country comparative analyses that go beyond just the ability of the biggest regional powers to get what they want.

Tools of ES and geoeconomics

Studies of the tools of ES typically make a dichotomised distinction between coercive (or punitive) and persuasive strategies (Blanchard & Ripsman, 2013, p. 5). The use and effectiveness of sanctions has probably been the single biggest focus of ES scholarship in general over the years (Drezner, 1999; Chan & Drury, 2000). Given the response to the Ukraine crisis, it is likely that existing scholarship on the impact of sanctions on Russia (Connolly, 2018) is likely to expand. The use of sanctions has specific resonance for the study of the region too. The imposition or relative lack of them (depending on your point of view) on China in 1989 in response to Tiananmen not only generated much debate at the time, but retains salience today in the form of the arms embargos that are still in place (by the EU, the UK independently as a now non-EU member state, and the US). The extension of controls over some exports to Hong Kong after the implementation of the National Security Law provides a more recent

example. Sanctions have also been used in response to the nuclear ambitions of North Korea, in disputes over Human Rights abuses in Xinjiang (both external sanctions on China and counter-sanctions by China), and in reaction to the 14 political grievances that China outlined that it had with Australia. We can also add to this list 'informal economic sanctions' where no formal action is announced or legal action taken, but where companies and individuals are incentivised or directed to pause or change their relations with specific countries. See, for example, the Chinese response to South Korea's deployment of the Terminal High-Altitude Area Defense (THAAD) missile system (Lim & Ferguson, 2022).

Actions can be both punitive and persuasive if differentially applied to friends and foes. Restricting exports, for example, might also be considered to be a punitive exercise in ES if those restrictions do not apply across the board to all markets. Conversely differentially controlling access to lucrative domestic markets is also seen as an ES tool and can be both punitive and persuasive. Historically, the maintenance of a list of countries that did not automatically receive Most Favoured Nation status from the US, and the market access that went with it, might provide a good example. Certainly, there was an argument that forcing China to apply for Most Favored Nation status every year (before it was granted Permanent Normal Trade Relations in 1999) could act as a way of socialising it into the liberal international order. The speed at which some companies have apologised to China when they have been accused of implying that one or more of Taiwan, Hong Kong and Macao are not integral parts of the People's Republic also points to the way that a market can have political consequences, if the state does not allow full and open access to it.³ Alternatively (or simultaneously in many cases), states can provide preferential access to some; for example through signing preferential trading agreements with a small number of states to either reward them for past action, or encourage them to act in certain ways in the future (Brummer, 2014).

China's promotion of overseas Special Economic Zones to facilitate its trade and investment relations with others has also been conceived of as a form of ES (Tang & Brautigam, 2012). It is an example that combines both trade and financial relations. And while financial flows - lending and foreign assistance—is probably the second biggest focus of the ES in general after sanctions over the years, it is the single biggest focus on the more recent literature on Chinese ES. Given how much China's global financial profile has changed since the turn of the millennium, this really is no surprise. Working out the boundaries between what would normally count as aid, what is state development lending, and what is (often state supported) overseas commercial activities is not easy given the way that such activities are both reported and executed. So we need to be a little wary of claims that China probably became the world's single biggest source of development finance in the 2010s surpassing both the IMF and the World Bank (Horn et al, 2019, p. 3). But even if we can argue about the exact figures and categorisations, it is clear that China evolved from

being a minor source of outward financial flows of different sorts (comparatively speaking) to become a major global actor (however defined) in not much more than a decade.

And if the speed and magnitude of this change wasn't enough in itself, the way that China's leaders actively promoted China as a force for change and reform of the way that the world is organised has only served to focus even more attention on Chinese goals and ambitions.⁴ You don't introduce 'China-centered multilateral institutions' like the Asian Infrastructure Investment Bank without the world taking notice (Chin, 2015, p. 217). But even creating new financial institutions in some ways pales into insignificance when compared with the time, energy, and money that the Chinese leadership has devoted to promoting the Belt and Road Initiative. The Schuman Declaration of 1950 argued that creating levels of economic interdependence between France and Germany (through cooperation initially only on coal and steel production), would make war between them 'not merely unthinkable, but materially impossible'. Establishing economic dependencies of different types on China is similarly thought of by at least some observers as an attempt to make challenging China—for example, on its territorial claims—at the very least economically damaging (if not unthinkable). And others assume even greater nefarious intent.

The problematic focus on China

This focus on Chinese finance, though, does create something of a problem in generating a comparative study on ES. This is in part because the conception of actors and actorness - who is acting as an agent of the state - can shift depending on the country that is being studied. Or put another way, state intent behind commercial action for geopolitical reasons is more often assumed when that investment comes from China than when it comes from other regional states. And this can make it difficult to do a truly comparative analysis.

We can illustrate this problem by asking a simple question. If the state doesn't do something directly, then is it still statecraft? Is providing, for example, a macroeconomic environment that incentivises economic actors to invest overseas still statecraft? Most definitions of ES would suggest that it only is ES if there is a direct geostrategic objective behind what the state is doing. But how do you prove this? How do you find a causal link between the state clearly wanting something, an action occurring, and an outcome resulting? In the Chinese case, this causal link is often assumed rather than found and proven, and the outcome itself can be simply taken as proof of the pre-assumed state goal without any further investigation.

Following on from this, a further question could ask if a Chinese company engages in a project overseas for commercial reasons, is this also

part of—or a result of - Chinese ES? For those who see statecraft and would answer in the affirmative, the explanation seems to rest on three interrelated observations and understandings. The first relates to the extent of the support provided by the state to help companies go global, combined with the state's ability to step in to prevent firms doing things that it doesn't want them to do. In both the way that the Chinese state facilitated outward flows of money in the 2010s, and also the way it reintroduced some restrictions in 2016-17 (to prevent acquisitions in overseas sporting and entertainment industries), we see clear state action in guiding the overall scope and direction of overseas activity. We might not be able to see the specific geostrategic objective behind each and every individual overseas transaction, but the guiding hand of the state to direct financial flows in certain preferred directions (and to prevent others) can be clearly seen. There is, so the argument goes, observable evidence that the state wants commercial actors to do some things, and not to do other things.

The second, is the nature of the overseas actors. Many of them are state owned, and even private sector actors are perceived as being potentially subject to future state control in the way that companies from other countries aren't. The reinforcement of the role of Communist Party committees even within private companies is a case in point, and Pearson, Rithmire, and Tsai (2022, 137) point to the introduction of a number of laws under Xi Jinping that mandate companies to act in the national interest that have created a 'blurred boundary between the Chinese state and firms'. So even when companies might be acting for commercial reasons now, in the future if the state asks them to do certain things with their commercial assets, then the assumption is that they will. The strategic objective is not clear now, but it will become so in the future.

The third is consequences. If a private Chinese company does things for commercial reasons that make it more effective, bigger, stronger and more competitive, then that in itself is seen as problematic by some. And that is because it will contribute to creating a bigger, stronger and more competitive China; and a bigger, stronger and more competitive China will be able to do things that have significant consequences for the global order. The immediate commercial objectives of the individual companies involved will aggregate up to attaining the state's strategic goals that will have geopolitical consequences. In both the second and third arguments, it is the lack of trust in future ambitions combined with a perception of the Chinese state's ability to make them happen that results in commercial projects being parsed as part of Chinese ES

So it seemed to us that a couple of things were happening. The first was that a rather different understanding of what was or wasn't ES seemed to apply to the study of China than to other countries. What might be a simple commercial activity if the firm was from somewhere else is deemed to be inherently political and strategic when that firm comes from China. To be sure, there can be suspicion of the motivations behind investments that originate from other countries too. Investments from companies that

are owned by other states or from non-Chinese sovereign wealth funds are indeed sometimes viewed with suspicion and even blocked. The would-be expansion of some Singapore government owned or linked companies in the late 1990s and early 2000s is a case in point (Carney, 2018). But the scope of what is considered to be either state related or potentially state related is much greater in many observations of Chinese actions. The purchase of the Kuka Robotics company by the Midea Group in 2016-17 did much to raise concern in Germany about the consequences of losing control of key technologies to China, even though Midea is a private Chinese company. Huawei, which has been the subject of more debate over a longer period in more countries than any other Chinese company, is employee owned. But ownership simply doesn't matter to those who believe that if and when it needs and wants to, the Chinese state can easily make these companies do its bidding; or those that think that they already are doing so, irrespective of who is the formal direct owner.

Trying to gain a competitive advantage in key economic sectors has been identified as being part and parcel of ES by Weiss and Thurbon (2021, p. 477). But their argument that ES can entail such commercial/ industrial objectives is qualified by the understanding of a geopolitical intent behind it; it is ES if it is an attempt to gain a commercial or technological advantage over a 'rival' power. So presumably, the same sort of investment from a country that has not already been identified as being a rival would not be considered to be ES. The key, then, is identifying a clear state and political intent behind individual commercial actions, which is not always easy when that action is being conducted by a non-state actor. Which brings us back to the importance of the assumption of state influence discussed above. And if you have that assumption, then rather than looking for evidence to prove a strategic objective behind any action, the action in itself is simply taken as evidence of the consequence of that pre-assumed intent. Causal strategic intent is assumed rather than proven, or assumed to be something that will only become evident in the future when the Chinese state decides to impose itself and its interests on Chinese commercial actors.

If our reading and understanding of the way Chinese ES was being studied is correct, then what are the consequences for the intersubjective meaning of ES? Has it become so differently understood and defined when it comes to the study of China that it is hard to build a truly comparative case study-based study of ES in different settings? Moreover, while the ES of other regional states might not have been totally ignored, they have certainly not been studied to the same extent as China's. Of all the other regional states, Japanese ES has probably been most studied, and most often framed as a response to China.⁵ South Korea's (and to a lesser extent Australia's) strategic thinking and actions have also been identified as proactively responding to Chinese statecraft by developing their own ES to avoid falling behind, and diminishing China-based economic insecurities (Thurbon & Weiss, 2021; Weiss & Thurbon, 2021).

With other regional states, though, while there is very much a recognition of the impact of China's (and Japan's) ES on individual countries and the region as a whole, there is much less focus on the ES of these states. Instead, other intellectual agendas, issues and debates tend to dominate, with regional states often studied as subjects and recipients of the ES of others, rather than as practitioners of ES themselves (Liao & Dang, 2020). As just one example, this journal published a Special Issue on 'the agency of weak states' in Southeast Asia (Eun & na Thalang, 2022) that only mentioned the term ES once (Chheang, 2022, p.343). And even then, there was no detailed engagement with the concept, with the focus instead on the importance and efficacy of balancing, bandwagoning and hedging. For many regional states, then, state action still seems to be most often studied within the context of studies of varieties of capitalism and developmental states.

Defensive-reactive understandings of geoeconomics and ES are present in both Blackwill and Harris's definition (2016) and Weiss and Thurbon's (2021) 'repurposing' of ES with a specific eye to responding to China's rise. Even if they do not have geostrategic consequences or intentions, state actions are considered to be ES if they are in response to the ES of others, and if they designed clearly and '*specifically to fend off, outflank, or move in step with clearly defined rival powers*' in key sectors [original emphasis] (Weiss & Thurbon, 2021, p. 477). Yet this understanding does not seem to have fully filtered through to scholarship of state action in much of the region; or indeed, to the study of less powerful states in other regions too.

So a plan was formed. We would fill this gap by undertaking a comparative study of ES across the region, and ask what was being done in various places and why? If we could find wider regional evidence of ES, was this just about a response to China, or were there other broader concerns too? And while accepting that economic interests are in themselves highly political, were these concerns that went beyond the broadly defined economic realm into something that could fit an even broadly more defined understanding of being 'strategic' in some way?

Changing focus

What became clear, though, in the workshop was that trying to pin the project around comparative ES methods and goals was rather problematic.⁶ While the paper authors had all found considerable action by states, it was not always easy to identify the strategic objectives of what was being done. We also faced the above noted problem of identifying what actions had geopolitical intentions, rather than seeking to enhance the economic competitiveness of economic sectors or countries (or even of a region in the shape of ASEAN). While concerns about China were never too far away, it was not always clear what the challenge was that needed a

response. Moreover, identifying the instruments of statecraft being used and the actions adopted were another problematic factor; it was evident that something resembling ES was clearly at work in some but not all instances. And to add an extra and final layer of complication, it really was not clear that many regional states had the ability to control economic affairs in ways that actually could result in identifiable strategic or foreign policy consequences. And here we should recognise that not only does state capacity differ across case study countries, but potentially also within them with a greater capacity to control or direct economic actors and bring about change in some sectors than in others (more of which shortly). Even in the Chinese case, the capacity to generate change even in the regional neighbourhood (let alone truly globally) is perhaps more often assumed than proven. In keeping with previous studies that have questioned the real extent of Chinese influence (for example, Goh, 2016) Ferchen and Mattlin show that Chinese ES did not always succeed as intended, and even generated unexpected dysfunctional outcomes.

Identifying these problems, however, helped us outline the solution. It seemed to make sense to make a three-part analytical distinction between different components of ES: motivations and objectives; actions and tools; and outcomes and consequences. This allows us to study the effectiveness of policy from goal through action to outcome. It also makes it easier to make a distinction between ES and other forms of state involvement in the economy. Many of the tools and actions might overlap to a large degree. Some of the outcomes might too. But there should be a difference when it comes to identifying the geostrategic motivations and objectives; and also potentially some of the geostrategic consequences too.

Making it easier, though, is not the same as making it easy. And the definitional devil is in the detail. Hence the importance of empirically rich case studies to test concepts against. If the defensive actions of regional states were a result of a rather general feeling of insecurity, and resulting policies designed to try to provide a greater level of economic sovereignty, does that re-blur the dividing lines between ES and Developmental State action that Thurbon and Weiss (2021) tried to clarify? Perhaps not. But what if it is not easy to ascertain a specific strategic political objective rather than a more general desire to increase national economic competitiveness? And what if the constraints that Carroll identifies in his contribution to this Special Issue make it difficult (or impossible) for states to control or incentivise economic actors to act in certain ways?

So rather than simply asking how ES was being carried out in each case study, we made a slight adjustment and decided instead to focus on the efficacy of the concept of ES in explaining and/or understanding what was going on. The new task, then, was to still ask what states are doing and why. But then to ask whether this action is best understood and/or explained by deploying conceptions of ES, or whether other concepts and ideas are perfectly adequate or more effective in framing state action in the region.

The task

With all collections, there is a balance between giving people with knowledge the freedom to say what they think needs to be said, and providing some form of common approach that glues the papers together and results in the whole being more than just the sum of its parts. With this in mind, we set five key questions for each paper to address (each containing a number of sub questions), recognising that the specifics of each case study might mean a greater focus on some of the questions than others.

First, what is the perceived problem that needs a response: what is the threat/where is the insecurity? Whilst this might seem an obvious question given the concerns with the rise of China (already noted above), can we identify something very specific and general that policy makers are concerned about; is there a specific and clearly articulated vulnerability? Or are states instead responding to these generalised and unspecific concerns about being intertwined with China based on a number of ‘what if in the future...’ type questions?

Or perhaps it isn’t anything specifically to do with China at all—or China alone at least. It is not easy to pin down a shared understanding of what the term ‘globalisation’ actually means, despite the many millions of words devoted to providing explanations. In its most vague form, it might just boil down to a feeling that domestic political economies are subject to forces that are beyond the control of the national government. And with the election of Trump and the Brexit vote in the UK, even the previous promoters of a neoliberal global economy seemed happy to champion the idea of a national economy and restoring some national state control. So maybe the imperative is an even more general feeling that it would be good to increase (where possible) autonomy and sovereignty and decrease technological and/or trade dependence on others.

Second, and very much related, what is the objective? In particular, given the above comments about the geostrategic foreign policy component of ES, is it just about increasing general economic security, or is it about gaining some other form of foreign policy/security objective too? Asking the broader question is especially salient for the study of a diverse Southeast Asia where out of several conceptions of security, two tend to dominate: regime security that is especially relevant in more authoritarian states where ruling elites are intent on staying in power; and comprehensive security that links development and growth in pathways to attaining traditional and non-traditional security (Foot & Goh, 2019). And if there is merit in posing the question in broader terms about securing autonomy and sovereignty, does it conform with the idea of an offensive or defensive type of ES as briefly discussed above; an attempt to influence others, or to defend the national interest from perceived insecurities? The defensive motivation, moreover, may not be wholly in relation to China, though that often forms part of unspoken motivations (which raises the question of

how we know this empirically). Rather, the struggle for economic security, and by extension economic autonomy and sovereignty, now occurs in a vastly different 'global governance' world (Zurn, 2018) where the de facto scope of the original Westphalian sovereignty principles has narrowed. In such a context, defensive types of ES may emerge in response to two key structural shifts. One a material power shift (such as given by the spectacular rise of China and other rising powers that have changed global and regional power distributions), and the second, the global governance shifts that Zurn highlights that are likely to constrain materially weaker states more than others. Whether such concerns have been turned into ES actions is a different issue; hence the emphasis on differentiating where possible motivations/objectives, actions/tools and outcomes.

Third, what is the ability? What economic policy tools might be available for states to engage in ES (both offensive and defensive) and/or developmentalism? In his contribution to this collection, Carroll points to the ongoing importance of non-state actors, including transnational ones, in shaping the way that national political economies function, and restricting their ability to do as they wish. With this in mind, how far do different states have the ability to incentivise, guide, direct, or control economic actors as they seek to attain their goals? And does this capacity differ across economic sectors: are financial flows easier to control than the trade in goods for example. And what factors shape differential levels of state capacity in these changed structural conditions? Ultimately, one aim here is to ask if ES as currently understood in the region is really just about what China and other bigger regional powers can do, or if it has salience beyond a handful of more powerful states too. Taking the Southeast Asian example described above, both security objectives—regime and comprehensive security—have led the materially weaker Southeast Asian states and the regional organisation (ASEAN) to engage in the more ambivalent 'hedging' action as threats remain diffuse and vague (Haacke, 2019; Kuik, 2008; Ruland, 2011). Yet, even when facing a more assertive China in the past few years, these states have chosen to hedge even more; importantly, these states have quietly begun to diversify their economic and political engagements from a concentration on China towards the US and other key powers (Kuik, 2021). Should such tools—diversification away from putting all eggs in one basket—count then as ES?

Fourth, what are the outcomes? Is there evidence of the state achieving what it set out to do? Or building on the consideration of state capacity in an answer to the third question, does the state seem unable to shape commercial activities in the way that it wants to? Or perhaps it is still too early to tell?

Finally, does the concept of ES provide a useful frame for thinking about the answer to the first four questions? Does it only make sense if you interpret it in a specific way (and if so, which way)? Or are there other (pre-existing) conceptions and frameworks that do a good enough job without thinking about ES in explaining what is going on? In short, what is the value added of using an ES frame?

The findings

We will let the individual papers speak for themselves. Collectively, they reinforce our belief that what states want and say is not necessarily what they do. Or indeed, what they *can* do. Hence, our suggestion that there is a need to separate out the study of ES into three separate components; intentions/objectives, actions/tools/capabilities and outcomes/consequences. Such a tripartite definition both allows us to consider in a more nuanced way the relationship between intentions and outcomes within individual country case studies, and also to compare differential abilities to exercise ES across different cases. Of course, the three are linked. It would be rather odd if any governments set goals and ambitions without first considering whether it had the capabilities required to attain these ends. That said, in at least some of the cases in this special issue (in Japan and South Korea for example) it seems that too much faith was placed on the ability to get buy-in from the business community. So perhaps even states—and in this case, two states with a track record of having strong and effective industrial policies in the past - at times overestimate their capacity to influence the actions of their national firms.

Intentions and capabilities

If we start with the focus on intentions/objectives, then we do indeed see the idea of ES directed against rivals and threats in a game of geo-strategic competition as the existing literature has emphasised and would expect. But by extending the scope of analysis beyond a narrower focus on bigger powers, we also see a whole set of other concerns too. In some instances, ES is aimed at reducing security externalities (Ferguson et al) while in others, it is aimed at enhancing economic and comprehensive security (Guild, Pitakdumrongkit).

The sort of globalization related concerns identified in the second wave of ES literature discussed above is also evident in this collection. Across the region, there is a sense of insecurity that results from trying to manage economies that are deeply embedded in globalised and interdependent networks of production, finance and trade. Even in China—the source of considerable insecurity in itself as we shall discuss shortly—there is a desire to make economic growth less reliant on external supplies of goods and resources and external demand for Chinese exports; hence the emphasis on ‘dual circulation’ and strengthening the national economy’s resilience and dependence on an unstable and potentially untrustworthy outside world. And of course, this is not unique to the region. There is, then, an idea that globalisation has undermined states’ abilities to control their own economic fate, and a resulting desire to enhance state autonomy. However, as the second wave of ES scholarship also argued, and reinforced by Carrol’s contribution to this collection, the very nature of the global value chains that are often the cause of this insecurity (and who has the

power within them) make it very difficult for states to simply and quickly change them. And perhaps this lack of ability to reduce insecurities even serves to increase a sense of a loss of control.

Global value and supply chains can be disrupted in a number of ways, and not always intentionally so. Natural disasters and the inability to navigate the Panama Canal have, for example, lead to at least temporary problems in recent years. The Coronavirus pandemic also resulted in rapid and significant shifts in the sort of goods that were in high demand, and the ability to supply what was available as a result of lockdowns in logistic hubs. And it is not just when there is a crisis that shifts in consumption habits and commercial objectives in other parts of the world can affect a national economy in ways that the government is unable to directly control. This was a lesson learnt by some regional states during the Asian Financial Crisis, and by pretty much all of them in the global crisis a decade later.

So regional anxieties about vulnerabilities and state capacities to dictate economic futures are not just about responding to China, but also have roots in longer standing perceptions of the nature of the global and regional economic order; these perceived threats in part stem from non-China systemic risks emanating from global financial flows or from security externalities arising from normal market-driven economic exchange. But as noted in the above discussion of the changing nature of more recent ES scholarship, there is a very clear China component to regional insecurities too. Is it going too far to suggest that perceptions of what Chinese ES is all about and designed to attain is the key driver of the ES of other regional actors? Maybe. But in the Taiwan case probably not. Moreover, fears of falling behind China and in some ways 'losing out' to it, being vulnerable to Chinese influence as a result of economic entanglements, or already being subject to Chinese economic coercion (or any combination of these three) are shown in this collection to be important drivers of Australian, Japanese, South Korean, and Indonesian ES thinking and practice. It has also been a key consideration in the search for collective economic security in ASEAN, in the face of rival conceptions of economic regionalism (that also threatened the loss of ASEAN's centrality). At the very least, it seems safe to say that one conclusion from this collection is that China does indeed loom large in thinking about economic insecurities in the region. And while some of this is informed by what has already been done, what *might* be done in the future is probably even more important.

The concept of ES appears to be relevant for all the case studies in highlighting the economic tools that states use to attain different strategically related goals. On the broadest conceptual level, the actions and tools adopted in the service of ES were by and large similar—the use of both market-conforming and interventionist policies. While these instruments are also the tools used in industrial policy and to achieve developmental goals, our emphasis on motivations/objectives saw clear security and strategic concerns as driving ES in all states. Once you dig down into the details, though, then there are also not just rather big differences in

the strategic goals of different states, but also the means that they have at their disposal to attain these objectives. As such it seems to make sense to try to identify and classify different types of ES based on differential objectives and capacities.

A simple way of doing this is to distinguish between those actions designed to bring about change in others on the one hand, and those that are primarily designed to defend and protect the national economy from potential influence by others on the other hand. However, there are two problems with such a simple and neat distinction. First, even assertive attempts to bring about change in others can be driven by perceptions of insecurity to some extent, as the Chinese example shows. Second, as clearly shown in a number of papers in this collection, defensively driven ES can also be designed to influence others. Examples include engaging others to compete with what China is doing (and thus undermine Chinese influence), and trying to cultivate new relationships and partnerships to diminish direct Chinese influence and asymmetric interdependence. Notably, this defensive but international strategy is not just limited to the more powerful. Goddard and Nexon (2016, 5) propose a '*post-realist*' global power politics in which materially weaker states and non-state actors are also likely to engage in power politics, only they make use of use of liberal and constructivist tools such as international organizations, laws, norms, rules as well as communicative/diplomatic tools, to try and influence others. These states' capacities to act in response to threats and problems, whether offensively or defensively, thus depends in part on their mobilising capacity to acting collectively with others similarly positioned and the use of a range of instruments (beyond realist ways of collective action through alliances or sanctions). And as is hopefully very clear by now, we think that the utility of ES as an analytical concept—or any analytical concept come to that—is enhanced if it can be applied to more than just a small set of states.

So while we suggest a distinction between three types of ES based on aims and capabilities, we accept that it is a very rough and imperfect categorisation that others might improve on. We also note that individual ES strategies very likely contain more than one of these types of action. But we also think that it provides at least some basis for thinking about differential goals and abilities based on the findings in our case studies, and thus might provide a starting point for other comparative studies.

First then we have those actions primarily designed to attain geostrategic objectives by bringing about change in others; international and proactive. Second, those strategies primarily designed in response to the perceived ES of others that also have the aim of influencing other global actors; international and reactive. And third, those strategies that are primarily focussed on doing things at home (including the regional 'home') to provide protection from the perceived ES objectives of others; reactive, domestic, and defensive.

Ultimately, though, perhaps even greater disaggregation is in order based on a more fine-grained distinction between different types of economic power. There is a difference, for example, between productive power (including

controlling key parts of the supply chain), market power, financial power and technological or innovative power. And one of the collective conclusions of this special issue is that the ability to control can vary quite significantly across different parts of the economy. Even these sub-categories of ES might still be too big in themselves. Chinese financial power, for example, might include holdings of various forms of foreign currency denominated debt, aid and development finance, foreign direct investment, and renminbi internationalisation (including the creation of a digital currency). All of them are important, and some of them overlapping. But important to different degrees in different cases in different ways. We perhaps need more nuance still, then, in facilitating an understanding of what works, and understanding which parts of the E in ES can be used most effectively by different states.

Outcomes

So what does work? Has the ES of regional states done what state elites wanted it to do and produced real-world effects? Although we have repeatedly stated the importance of our tripartite approach already in this introduction, we still think it is worth repeating one more time. That's because, as Ferchen and Mattlin note in their China case study, identifying an action and an outcome does not necessarily mean that either are a result of deliberate intent. Or alternatively, they might be the result of intent, but not the intent of the Chinese state as part of its ES. An outcome could be, for example, the result of the commercial objectives of Chinese international actors. Or the consequence of states responding to what they *think* China wants based on pre-existing understandings (and maybe even mis-perception). Most of the time, they argue, scholars of China's ES fail to analyse Chinese international economic interactions comprehensively along all three analytical components, thus overstating the Chinese state's ability to successfully prosecute a carefully designed ES strategy.

Seeing as we are repeating core claims, we will take the opportunity to repeat Carrol's emphasis on recognising the significant of Global Value Chains too, and what that means for an understanding of power, interest, and actorness in the global political economy beyond the power and interests of states. Even where there is a relatively high degree of state control and/or ownership and agency, and a relatively high success rate in attaining objectives, Liao uses the example of overseas coal power financing to show that outcomes are still seen to be influenced by global market dynamics to at least some extent. Moreover, these states' capacities to pursue their original intentions was also influenced by climate politics following the Paris Agreement. This resulted in a rapid scaling down of Japan's and South Korea's ES strategy of financing coal-fired plants in the Global South in their bid to compete with China for economic leadership and to build strategic relationships with states deemed important allies. We have already noted Katada, Lim and Wan's evaluation of the relative lack of success that the Japanese and South Korean governments have

had in encouraging reshoring to reduce the perceived risks of perceived dependence on China. Lai's discussion of Taiwanese policy points to at best a mixed result, and notes that it is in many ways still too early to tell because this certainly is not the end of the story of Taiwan's Southbound strategy. Or the end of any of the stories covered in this collection.

That said, there are some cases where the authors identify some success in attaining the outputs aimed for. The building of local critical mineral production capacity in Australia (Ferguson et al), diversifying domestic financial risk in Indonesia (Guild), and being able to direct elements of the Regional Comprehensive Economic Partnership (Pitakdumrongkit), all exhibit some elements of states succeeding in achieving what they set out to do. But as with the case of coal power financing, in all these cases there were limits to the extent to which these targeted outcomes were attained. In a globally interdependent world, outcomes—in the form of reduced security externalities or enhanced economic and comprehensive security or sustained strategic economic leadership—may not always be attainable given that much is outside the control of state actors engaged in ES in a globally interconnected world.

Conclusion: the utility of ES

It is tempting to just finish this introduction here and ask the reader to make their own mind up on the utility of the concept after having read all of the individual papers. Not least because there is no clear consensus of how useful it actually is. This is perhaps in part a result of using this special issue to go beyond just the study of the ES of the more powerful states in the region. It is in the defensive strategies of these less powerful states that the distinction between ES and economic security can become a bit blurry. Or put another way, if the geostrategic intent is to mitigate the security externalities of key economic interactions, and secure the prevailing political regime and the state's economic security, then where does ES end and other conceptions of security start?

This blurriness, though, leads us to three final thoughts that we offer as a conclusion of sorts. The first, building on Weiss and Thurbon, is that it makes sense to treat strategic and security motivations as rather different from other forms of state actions; most obviously, those state actions primarily devised to attain developmental goals. Even if there might be some overlap, the key is identifying the primary driver. In the Australian case, for example, some of the policy and regulatory shifts look very much like economic diplomacy or industrial policies. Yet the end goals of those policies were not developmental, but instead avowedly about outflanking a China that had posed a threat to Australia's security through its dominance in key sectors of Australia's economy.

Once that is done, though, the second is that ES does not have to exist on its own to have utility. Ferchen and Mattlin, for example, suggest

adopting a broader concept of economic influence, which includes ES as one subset of economic influence rather than the single overarching focus. And thinking about how ES sits (and fits) *alongside* other understandings of security and strategy rather than how it works *instead* of them might make some sense; one that allows for the sort of systematic tracing of the links between intentions, actions/tools and outcomes that is at the heart of this introduction.

Finally, in many of the analyses in this collection there is an implicit or explicit unease about the realist assumptions that can inform some understandings of ES and geoeconomics (including some of the foundational ones). We suggest that this (realist assumptions) doesn't have to be the case. Our focus on both the importance of perceptions and also the role of non-state actors in this introduction implies that studies from other theoretical starting points might have something to say about ES too. If we return once again to our core position, ES might actually be used to explore why state objectives do not materialise; or why they sometimes do and sometimes don't. Or perhaps working from the other way round, why observable changes are not always the consequences of state directed actors doing things to attain state defined goals. Whatever the case, the key is finding evidence of cause and effect rather than simply assuming that what the state wants, the state get. Or assuming that what happens is always a result of what the state wanted and did in the first place.

Notes

1. For a very good overview of the "ideas and debates" on Asian developmental states, see [Chu \(2016\)](#).
2. As part of a research project funded by the Academy of Finland, "Foreign acquisitions and political retaliation as threats to supply security in an era of strategic decoupling (ForAc)."
3. Though we should note that it is often Chinese consumers that raise the alarm and threaten action against offending companies (typically online), rather than direct agents of the Chinese state itself.
4. For an annotated bibliography of the major events and speeches that mark China's transition to a self-declared leading force for global governance reform, see the appendix in [Breslin \(2021\)](#).
5. For an overview of this literature see [Suzuki \(2022\)](#). Notably, he rejects the utility of ES approaches, and even questions the extent to which China and Japan are inherent and inevitable rivals.
6. We are very grateful to the S Rajaratnam School of International Studies at Nanyang Technological University in Singapore for hosting our workshop at a time when some COVID restrictions were still in place and subject to short notice change.

Disclosure statement

No potential conflict of interest was reported by the authors.

Funding

Academy of Finland.

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