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Thinking politically about corruption as problem-solving: a reply to Persson, Rothstein and Teorell

Heather Marquette & Caryn Peiffer

There has been a surge of interest in collective action approaches to fighting corruption. In 2003, for example, the Basel Institute on Governance established the International Centre for Collective Action (Pieth, 2012).¹ Development agencies have designed anti-corruption interventions - such as the Strengthening Action Against Corruption programme in Ghana - with an embedded collective action approach (Kantelberg & Ibrahim-Tanko, 2019). In civil society, innovators have emerged, like the Accountability Lab, with its ‘Integrity Icon’, designed to ‘name and fame’ public officials discharging their duties with integrity in order to generate collective action for better government.² And, of course, there have been a number of academic papers on the subject, including, among others, Persson et al. (2013) and our paper (Marquette & Peiffer, 2018). Persson, Rothstein and Teorell have recently written a response to our paper (Persson et al., 2019), and we reply in turn here.

Writing originally in 2010 (Persson et al. 2010), the authors made a significant contribution to the field by arguing that anti-corruption interventions often fail due to a ‘theoretical mischaracterisation’ of the problem. In place of the dominant principal-agent theory informing anticorruption interventions, they argued instead that corruption can best be explained as a collective action problem (Persson et al. 2010, 2013).

In our 2018 article (and a previous working paper [Marquette & Peiffer 2015]), we reviewed the literature on corruption and collective action - including, but not limited to Persson et al. (2013). Contrary to our original expectations, we found that while the arguments put forward by Persson et al. had ‘intuitive plausibility’, to quote Sparling (2018), unpacking the theory and its application revealed important challenges with regard to both the theory as posited and its potential application in practice. While far from dismissing the ideas put forward by Persson et al. and others, in our

¹ <https://www.collective-action.com>

² <http://www.accountabilitylab.org>

paper we nonetheless found that a) the case against principal-agent theory had not been proven and that there was a risk of throwing the baby out with the bathwater; b) that the application of collective action theory was incomplete (which Persson et al. [2019: 3] acknowledge, though they do not explain why or offer a solution); and c) that when ‘the benefits of passively or actively engaging in corrupt activities seem to outweigh the costs’ (Persson et al. 2013: 463), the most important question to follow, surely, is *why*? The answer the collective action approach to corruption emphasised in response to this question was that people engaged in corruption because they could not trust that others in society were refraining from it. In contrast, we looked at both historical and contemporary research where corruption is described as being ‘functional’, which is to say that it provides solutions to the genuine problems that people face in dysfunctional political systems and in contexts where scarcity of resources leads to significant challenges for ordinary citizens. This is not itself an original observation, as we say in our 2018 paper, but one that we argued was being overlooked by some contemporary scholarship (including the literature on ‘corruption as a collective action problem’) and is essential for understanding why corruption persists.

In their recent response to our paper, Persson et al. (2019) offered three critiques that we appreciate being given the opportunity to respond to: that our 2018 article contributed nothing new to thinking around anticorruption, because a collective action approach already explains that corruption is used to solve problems; that talking about corruption as ‘problem-solving’ means that we are making an argument for corruption as ‘good’ at the aggregate level; and that we are averse to theory. In this reply, we offer three responses to these critiques: 1) perceptions have been overemphasised in the literature on corruption and collective action; 2) thinking about the functions that corruption may help to fill is not the same as arguing that corruption is beneficial to society in the aggregate; and 3) without taking context into consideration, theories can act as problematic ‘magic keys’ too.

No, the collective action literature had not done functions justice

Perhaps the most significant critique Persson et al. (2019) put forward against our 2018 article is that the functionality argument we highlight, which asserts that corruption persists because it

solves problems for the individuals involved, brings nothing new to the table. This is because collective action theory, they argue, inherently recognises the functionality of corruption. They write, ‘the collective action-based approach entails that it is exactly because corruption functions to provide solutions to problems...that systemic corruption is so very difficult to get rid of’ (Persson et al. 2019: 5).

While we agree that a collective action-based approach to anticorruption can (and should) consider any role that corruption’s functionality plays in systemic corruption, something that we in fact argued in 2018 (Marquette & Peiffer 2018: 508), we strongly disagree with their assessment that this had already been done by themselves in their 2013 article or by most of the scholars writing under the ‘corruption as a collective action problem’ umbrella. We maintain that this literature had not given enough consideration to the role that corruption’s functionality plays in fostering collective action problems around corruption.

Instead, what we observed in the literature on a collective action-based approach to corruption was a strong tendency to emphasise the role that a lack of intra-group trust plays in engendering collective action problems around tackling corruption. We described this observation in 2018: ‘...collective action theory has largely been used to emphasise how perceptions of others’ likely actions factor into decisions to engage in corruption’ (Marquette & Peiffer 2018: 508). The main argument emanating from most of this literature was that ‘agents may well understand that they would stand to gain from erasing corruption, but because they cannot trust that most other agents will refrain from corrupt practices, they have no reason to refrain from paying or demanding bribes’ (Rothstein 2011: 231).

While we believe that this emphasis exists in Persson et al. (2013), which is the article that Persson et al. (2019) specifically defend, one can also find it in a wider collection of works within this literature (especially in Bauhr & Nasirtousi 2011; Rothstein 2011; Rothstein & Teorell 2015; Teorell & Rothstein 2015; Uslaner & Rothstein 2012; Rothstein & Tannenberg 2015). As an example, Uslaner and Rothstein (2012:5) argue that a lack of intra-group trust is the main reason why systemic corruption persists: ‘people in systemically corrupt settings participate in corrupt practices mostly because they perceive that most other agents play this game and that it therefore

makes little sense to be the only agent that acts honestly if one cannot trust others to be honest.’ As another example, Rothstein (2011: 231) argues that the only reason people would refrain from corruption is if they had institutions that ‘would make them trust that most other agents would refrain from taking part in corrupt behaviour.’

We are not alone in this reading of this as the main argument in the ‘corruption as a collective action problem’ literature; others also describe the literature as primarily highlighting the role that societal trust plays in sustaining systemic corruption. In Ledeneva, Bratu, and Köker’s (2017: 2) words, for example: ‘The collective action perspective holds that people will choose to act corruptly as long as they expect that most other people will also act corruptly’. Heywood (2017: 24) writes that the collective action perspective understands that corruption is ‘critically dependent on citizens’ assessments of how others will behave in any given situation’.

We want to restate our original position here which remains unchanged: by emphasising the role that social trust can play in shaping corruption patterns, the ‘corruption as a collective action problem’ literature offered an important contribution to thinking about anticorruption. As we wrote in 2018 ‘viewing corruption in this way makes an important contribution to the field by highlighting the collective, rather than individual, nature of corruption and the very difficult challenge that anticorruption efforts face in changing levels of distrust in society’ (Marquette & Peiffer 2018: 502). Indeed, our own work has tested this, finding considerable support for the notion that when individuals believe that corruption is commonly practiced they become less willing to engage in anticorruption action (e.g., Peiffer & Alvarez 2016).

It seems appropriate here to (re)note that in the wider literature on collective action dozens of variables have been identified as likely affecting whether a group will be able to overcome a collective action problem (Agrawal, 2002; Marquette & Peiffer 2018: 502). Our 2018 article thus asserts that in framing corruption as a collective action problem, the field should fully explore how anticorruption efforts are impacted by more than just the one variable—generalised trust—that we (and others) believe has been spotlighted. Without doing this the application of collective action theory to corruption remains incomplete. By putting so much emphasis on the importance of societal trust, the literature naturally has sacrificed a focus on other potentially important variables

that can help to explain why corruption persists and why anticorruption efforts are failing. In our reading of it, most of this literature has either not acknowledged the functionality of corruption or, if corruption's functionality was acknowledged, it was done so in passing and/or without fully exploring what it may mean for addressing systemic corruption.

It is worth also restating that the crux of our argument was that what may be of primary importance is that corruption can be reached for by individuals to solve real political, economic and social problems. As we argue (Marquette & Peiffer 2018: 508), such a recognition has important implications for policy thinking, because '...changing peoples' mentalities or perceptions of their colleagues or wider society's behaviour will not necessarily change the structural necessity for elites to use corruption as a mechanism to ensure political survival, for example, or for ordinary citizens to depend upon corrupt networks in order to access services'. Focusing primarily on intra-group trust over the many other factors involved in shaping collective action calculations risks shifting blame onto society, rather than corrupt elites, effectively depoliticising anticorruption.

No, we do not say that corruption is a 'good thing'

According to Persson et al. (2019; 6), we wrongfully assume that because corruption can be used by individuals to solve problems, it is also 'a socially efficient or "good" institution at the aggregate level.' In response, they point to a body of quantitative and qualitative research that lends support to the argument that corruption is almost always detrimental for development and wellbeing (Persson et al. 2019: 6). The authors even go as far as warning that research, like ours, that suggests that corruption is not always unequivocally 'bad' can help to fuel and enable corruption. This is because, as they argue, '...to even remotely suggest that corruption serves a positive function at the aggregate level...' may '...give political elites—who are by all available measures the ones benefiting the most from corruption—an official excuse for maintaining the [corrupt] status quo...' (Persson et al. 2019: 6-7). We think it is important to address this critique directly as we believe it represents a fundamental misreading of our research. This concern is also worth addressing

because it brings to the fore a discussion about censorship within what can be a highly politicised research field. Three points are worth making here.

Firstly, contrary to their account of our 2018 article, we do not make claims about corruption's potential aggregate (or net) harms or benefits. In fact, we offer no new insights on the supposed effects of corruption on societal wellbeing. The purpose of our article was to contribute to a wider discussion about *why corruption persists*. The functionality argument that we highlight is the notion that corruption persists because it is used by actors to solve real and persistent problems. While we reference examples in the literature where corruption has been used as a problem-solving mechanism, we never assert that corruption must therefore also be beneficial to society as a whole. Instead, we make a relatively simple point: because corruption is used as a problem-solving mechanism, it is especially difficult to eradicate.³

Of course, it is also worth noting that the field is not at all as settled on what corruption's aggregate impacts are, as Persson et al. (2019) suggest it is. While it is certainly foolhardy to ignore the volume of literature that demonstrates corruption's likely net harm to societies, it is important to recognise other research that argues differently. Space does not allow here for an exhaustive review of the literature, but it is worth highlighting that there is now an impressive body of research that has discredited the most often deployed measures of aggregate corruption, 'implying that we have no reliable way of measuring precisely the extent of the problem' (Heywood 2017a: 21; Heywood & Rose: 2014). The measurement limitation alone has severely undermined the field's ability to assess corruption's impacts on development.

³ Perhaps this interpretation of our paper lies in our citations of others' work. For example, we reference a history of research that recognised the functionality of corruption. Some of this research *also* argued that corruption can have beneficial knock-on effects for wider society, a view that almost certainly needs to be read within the historical context – post-colonial, pre-globalised financial system – in which it was written (e.g. Huntington, 1968; Leff, 1964; Leys 1965). This body of work was cited by way of providing a literature review on the functionality argument; these authors' arguments about the impacts of corruption are not what we draw on to make the case that corruption's functionality is important.

Moreover, other research has shown that some countries have achieved impressive developmental progress while having seemingly high levels of corruption (e.g. Khan 2006; note that Rothstein does this himself in Rothstein [2015]), and that certain types of corruption may coexist or even encourage certain types of positive developmental change (as just one example see Kelsall, 2013). Research along these lines suggests that not all types of corruption hinder development as well as that corruption may not hinder development everywhere. One of our favourite books, for example, is Merilee Grindle's *Job for the Boys* (2012); she convincingly shows that patronage, usually equated with corruption and incompetence, has historically permitted the pursuit of diverse political objectives, some of which are associated with positive development outcomes, such as state power consolidation (i.e. state building). Grindle's study is not only a good reminder that some types of corruption can be used to solve political problems, but also that some of the solutions captured through corruption may have wide-scale, long-term benefits that are not easily quantified in the immediate term.

Finally, though our article does not make any claims about corruption's potential wide scale benefits, Persson et al. (2019) warn us that research that highlights the potential beneficial impacts of some corruption in certain contexts may be used as an excuse for corrupt elites to pursue corrupt business as usual.⁴ It is not clear to us whether or not the authors have thought through the potential implications of this warning. It implies that - as a field - we should not be transparent in reporting our research findings, censoring ourselves when it comes to publishing research that may suggest that some corrupt patterns could offer net benefits to individuals or to society.

Back in the 1960s, Colin Leys argued that corruption research was hampered by an overly moralistic approach. He noted that, 'Similar phenomena, such as suicide, crime, or religious fanaticism, have intrigued sociologists greatly. However, the question of corruption in the contemporary world has so far been taken up almost solely by moralists' (Leys 1965: 216). The implication of this, we have argued elsewhere, is a policy-oriented literature that's *implicitly* moralistic in a way that hampers *realistic* policy interventions (Marquette 2012: 19). While political positions on corruption are often, understandably, morally black and white, the most

⁴ The reaction to this idea from our colleague Doron Navot is worth sharing: 'Well, I am not sure they (elites) read our papers, and they (elites) do not need them to excuse their behavior.' Pers. comm., D. Navot, 11 May 2019.

operationally useful new research is often more nuanced and avoids moral judgement. Understanding and shifting incentives and easing people towards non-corrupt behaviour is showing more promise in research in knotty areas such as reducing corruption in the massive skills training investments in Bangladesh (Khan 2019) and in getting the rich to pay more tax in Uganda (Kangave et al. 2018). If we - as scholars - self-censor our findings in research on corruption, we risk losing important insights such as these and, perhaps, even delegitimising the field.

There is no single (anti-)corruption theory magic bullet and, yes, context does matter

Finally, Persson et al. subtitle their final critique as ‘If context is everything maybe it is nothing’ (p. 7), referencing Wildavsky’s famous 1973 article on planning. In this section, they accuse us of being ‘anti-theoretical’. They write that:

If we, *perhaps somewhat unfairly*, push Marquette and Peiffer's apparent taste for context—and their corresponding underplaying of theory-based research—one step further than what they *perhaps explicitly state in their article*, in practice, this would imply that we would need one theory/ model for how to tackle corruption for each continent, country, or perhaps even village. Such an antitheoretical approach would be similar to if doctors had one theory of a specific form of cancer for each patient (2019: 8, emphasis added).

The highlighted caveats suggest that the authors know that this is an unfair critique, and yet they persist with it anyway. By saying that context matters, we are clearly not calling for theory to be abandoned. Instead, in our 2018 paper we say that all three theoretical lenses - principal-agent, collective action and functionality - are likely to be important, and none of them should be abandoned. There are many other theory-driven papers on corruption that also provide valuable insights: examples include Lessig (2013) on ‘institutional corruption’, Hoffmann & Patel (2017) on norms/behaviour, Abraham et al. (2018) on the psychology of corruption, Johnson et al. (2013) on a feminist theory of corruption and many more. Twenty years ago, Williams (1999: 511) argued

that ‘instead of putting all of our eggs in one conceptual basket [on corruption], there is a need to examine a range of related concepts’. Twenty years on, the field is finally starting to take up this challenge, and it is all the richer for it.

There is a tremendous amount of exciting new research energising the field. Since Persson et al.’s (2013) article, there have been a number of new anticorruption research initiatives. An important one is the large DFID-funded Anti-Corruption Evidence (ACE) programme, partly based at SOAS (led by Mushtaq Khan) and partly based at Global Integrity (led by Paul Heywood). While each part of ACE is structured differently, both focus on theoretically grounded, practically-oriented, problem-focused, granular research. This research is already helping us to better understand procurement risks (Dávid-Barrett & Fazekas 2019) and meritocracy in civil service management (Meyer-Sahling et al. 2018), among other things, with forty-one new research projects underway on issues as diverse as illicit financial flows, medicine theft, corruption in urban planning, beneficial ownership checks and so on. There is the Interdisciplinary Corruption Research Network (ICRN) whose members come from a diverse range of perspectives and have launched one of two exciting new podcasts dedicated to corruption (*Kickback: The Global Anticorruption Podcast*; the other being Brian Klass’s excellent *Power Corrupts*). There are a number of new methodological advances; a few examples include measuring cross-border informal trade and bribery (Bensassi & Jarreau 2019), measuring illicit financial flows (Khan et al. 2019), contract-level data in aid procurement (Dávid-Barrett et al. 2017) and our own bribery positive outliers research (Peiffer & Armytage 2019; Marquette & Peiffer 2018) and on corruption messaging experimental research (Peiffer 2018).

As Rothstein says, policymakers often search for an anticorruption ‘magic key’: ‘if only we could find the magic key (the “entry point”) and change this institutional device, we would be able to advise policy makers on [anti-corruption]’ (Rothstein, 2011: 107–8). The ‘corruption as a collective action problem’ has the potential to have its own ‘magic key’ properties, an insight that can apply to theory as well as to practice in anticorruption. The corruption field has certainly suffered from the search for one overarching theory to try to explain what is not really one concept but rather a ‘basket’ of complex political, economic and social phenomena playing out differently in diverse settings. As Heywood (2017: 39) has persuasively argued, researchers and policy makers

should pay more attention to where corruption happens and what type it is ('focus and locus'): 'if we are to address corruption for the purposes of effective policy, we need to disaggregate into different types, as well as different levels and locations in which it occurs'.

Indeed, we could lose sight of the move towards more targeted anticorruption interventions because of the confusion between corruption as a concept and corruption as manifested in specific problems. The same is as true for our research on functionality as anyone else's which is why, for example, we have recently looked at what the functionality lens brings to our empirical research on Uganda's health sector as a 'hidden' positive outlier on bribery (Peiffer et al. 2018),⁵ but why we were open to using a different theoretical lens - a 'sector characteristics' lens (Batley & McLoughlin 2015) - to help explain a very different case in South Africa's police (Peiffer et al. 2019).

Just as the multiple theories for the origin of cancer do not mean that doctors develop a new theory for every single type and every single patient (Paduch 2015), the logic in the quote above from Persson et al. (2019) does not hold. And even if the same theory is applied, it will be adapted to suit the particular patient's context (e.g., age, lifestyle, family history, genetic factors etc.). Current developments in oncological research are actually moving precisely in the direction of individualised treatment plans, based on DNA coding. As Heywood explains, 'recent advances in cancer treatment have started to look at the use of DNA sequencing to develop personalised therapies targeted at the individual level – precisely the kind of highly specific and tailored focus often dismissed as impractical or unrealistic by those working on corruption' (Heywood 2017b). It is not being 'antitheoretical' to say that context should drive interventions rather than theories,

⁵ Although we do not have space here to go into this, one of the findings from our research in Uganda was that the principal-agent based approach taken by the Ugandan government *was effective* in the short-term, but the reason why this effectiveness is unlikely to be sustained was because the intervention did not deal with functionality question, the underlying problems leading to bribery in the first place. Perhaps more worrying than the likely lack of sustainability, the *effective* principal-agent intervention led to dangerous unintended consequences that undermined the already weak provision of public health services (Peiffer et al. 2018). We mention this here because it is not the case that the principal-agent model for anticorruption interventions has been proven not to work, and there are many potential unanswered questions for researchers to grapple with; hence, the 'throwing the baby out with the bathwater' warning in our 2018 paper (p. 503).

but is, to our mind, a (potential) natural outcome of a diverse body of research and practice unfolding in entirely different political and economic contexts, in different types and in different sectors.

Conclusion

Understanding that anticorruption interventions face collective action challenges is important, though that is different to saying that corruption is a collective action problem; knowing what to do with this improved understanding is another thing entirely. It is easier to see how interventions in specific sectors and contexts (Heywood 2017a) may face collective action challenges, and then how to intervene to overcome these, such as with the work that the Basel Institute are doing with private sector actors around business integrity or that Mushtaq Khan and others in the SOAS ACE team are doing on strategies for aligning incentives in specific sectors. It is harder, however, to see this in a more generalised sense.

In our 2018 paper, we give Persson et al. (and others) a lot of credit for helping to shift the debate. What they said mattered - and it still does, but it does not help to explain everything they have claimed to. We had hoped for new theoretical insights or a new empirical contribution from Persson et al. (2019); given that it is almost ten years from when Persson et al. (2010) was published, one would hope for more guidance from the authors at this point. They promise us, after all, a ‘more fruitful and conclusive approach to how corruption can be more effectively thought of and fought in the future’ (Persson et al. 2019: 3). As readers, we still are unsure what a collective action approach to fighting corruption actually even looks like, and being told that it is (now) about ‘universalism’ - or ‘an “impartial” system of rule’ - is not particularly helpful (Persson et al. 2019: 10). This is a vision, rather than an ‘approach’, and it does not help anticorruption practitioners decide what to do differently, or how.⁶

⁶ While we do not have the space to address this in full here, there are recent critiques regarding this vision of universalism/impartiality. For example, Sparling (2018: 378) argues that: ‘it is, indeed, a dangerous ideal if transformed into the touchstone of good government precisely because it attempts to depoliticise debates about

Be that as it may, we come back in the end to the added value of a functionality lens. Our focus on ‘real world politics’ was not chosen thoughtlessly (see Navot 2015); rather, it reflects our belief that more of the field’s time should be spent in this space. Following his participation in a predominantly academic anticorruption workshop, Oxfam’s Duncan Green asked ‘how can the anticorruption movement sharpen up its act?’ He put forward some suggestions, including: ‘Let’s see corruption specialists working with those trying to crack real world problems – like bad roads, or poor quality health and education services – rather than sitting with other anti-corruption experts talking about corruption and so risking being pulled into generic and abstract debates about indicators. Corruption is a part of many big problems in development, but it is rarely the only part’ (Green 2017). A functionality lens can help us get to the ‘focus and locus’ level of granularity that we need in order to tackle some of the underlying problems and challenges that ordinary people face in dysfunctional, poorly resourced systems, and we look forward to unpacking this further in future research.

corruption’. Heywood (2018: 86) says that this vision of impartiality ‘has little to say about how to address genuine ethical dilemmas or challenges that, for some, represent the only true test of whether individuals act with integrity’.

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