Why Do IR Scholars Study Fragile States and Not Comparativists?

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Abstract

Ever since its inception, the study of failed and fragile states has been a topic pursued mainly within the International Relations community. Looking at the journals in which the relevant research is published, IR outlets strongly outweigh journals and edited collections from other subdisciplines of political science. This fact, however, is surprising on at least two counts. First of all, Comparative Politics is much more closely concerned with the subject matter of fragile states, i.e. domestic political institutions, than IR to whom the intrastate level is often only of passing interest. Second, Comparative Politics already had developed a substantial body of research of the 'weak state' with relevant works going back to the 1960s. We can explain this lack of intellectual overlap as a result of path dependence yet we still have to explain how IR came to take up the study of fragile states in the first place. This paper argues that political practice played three different roles in launching fragile states as an International Relations research topic in the early to mid-1990s. First, practitioners and former practitioners were among the earliest authors writing on the subject. Second, the earliest articles on the topic were often published not in traditional academic journals but in outlets that catered to practitioners. Third, research efforts were sponsored by governmental agencies. These early efforts shaped an approach to fragile states which framed them not as political systems with particular features but as problems of, and threats to the international community. IR specialists adopted this outsider's perspective in keeping with their discipline's usual approach to domestic politics. The path thus set shapes research to the present day – that's why we still don't know much about the *causes* of state fragility while its *consequences* have been elaborately, sometimes exhaustively discussed.

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"Whoever fights with monsters should see to it that he does not become a monster in the process. And when you gaze long into an abyss the abyss also gazes into you." (Friedrich Nietzsche, Beyond Good and Evil, Aphorism #146)

1. Introduction

The concept of failed and fragile states (FS)[1] has gained widespread acceptance and use in academic and political discourse ever since its inception in the early 1990s. As the bibliometric study of Büger and Bethke (2011: 20, Fig. 1) shows, the number of publications on the subject – both academic and non-academic – has risen dramatically from a mere handful in the first half of the 1990s to almost 50 in 2008 alone.

The majority of these publications have been published by scholars who self-identify as International Relations (IR, including Security/Strategic Studies) specialists while a minority comes from Comparative Politics specialists (CP, including Area Studies), in addition to contributions from International Law, Anthropology and other disciplines.[2] On the face of it, this is quite puzzling since CP seems much better equipped than IR to deal with the phenomenon of states with a decreasing ability "to implement [their] rules, collect taxes and enforce [their] monopolies of violence" (Lambach 2007: 33).[3]

An introductory textbook on CP stresses that "comparative politics […] involves both a method of study and a subject of study. As a method of study comparative politics is – not surprisingly – premised on comparison. As a subject of study, comparative politics focuses on understanding and explaining political phenomena that take place within state, society, country, or political system" (Lim 2006: 5). Another one adds that CP "deals with the very essence of politics where sovereignty resides – i.e. in the state: questions of power between groups, the institutional organization of political systems, and authoritative decisions that affect the whole of a community" (Caramani 2008: 3).

IR, on the other hand, was classically defined as the study of the politics between states and, in particular, the causes of interstate war and peace (see, e.g., Jackson/Sørensen 2007: 2, Smith 2010: 3; on the origins of IR also see Vitalis 2010). It is generally acknowledged that the scope of IR has

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1   I use "failed and fragile states" as an umbrella term for a variety of labels that have been used in this particular field of research: failed states, fragile states, failing states, collapsed states, inverted states etc. However, I distinguish it from related concepts like the "weak state" and the "quasi-state" – see below.

2   I consider IR and CP to be two subdisciplines of the larger discipline of Political Science. In this approach, Security Studies (or Strategic Studies) are particular research fields (or sub-sub-disciplines, if you prefer) within IR.

3   Such a Weberian approach has been the subject of criticism, some of it more valid than others (see, e.g., Eriksen 2011). However, this paper will not address the merits of this discussion since it focuses on the politics and sociology of FS research rather than its content.

broadened as a consequence of the growing complexity of the international system and now has to include additional actors (international organizations, non-state actors, networks), levels of interaction (supranational, international, transnational) and subject areas beyond diplomacy and war. This has somewhat muddled the definition of what IR means but there is a general sense that for a topic to fall into the remit of IR some of its aspects (e.g., its causes, its processes, its consequences, the actors involved) have to cross national borders in some substantial fashion.

While a precise delimitation of these subdisciplines of Political Science is therefore not possible, we can still discern a certain division of labor: While CP focuses on political phenomena that primarily exist or take place *within* countries, IR addresses those issue who primarily exist or take place *between, above, or beyond* countries (Lim 2006: 5, Caramani 2008: 3). It has long been argued that this separation of research fields is unhelpful and myopic (see, e.g., Jacobsen 1996), a charge which is becoming even more pointed in an age of globalization, transnationalization and the ensuing changes in state sovereignty. But even though the criticism is valid, this division of labour continues to be upheld through academic practices: IR scholars predominantly read works by other IR scholars, present at IR conferences, and publish in IR outlets while CP scholars predominantly read works by other CP scholars, present at CP conferences, and publish in CP outlets. Universities sustain this division by hiring political scientists mainly as specialists in IR or CP in order to fill departmental needs (someone has to teach undergrad classes and advanced research seminars in those particular sub-disciplines, after all).

State failure, whether understood as a process or a feature of political systems, undoubtedly is an issue of domestic politics. It has obvious international features, e.g., the question inhowfar external factors cause or contribute to fragility, but the domestic aspects clearly outweigh the international ones. The puzzle is then why the majority of current FS scholarship comes from IR even though its "outside-in" perspective on issues of domestic politics seems ill-suited to the subject at hand. This is all the more surprising as the inquiry into FS has strong antecedents in CP. In his seminal work on political modernization, Huntington stated that "(t)he most important political distinction among countries concerns not their form of government but their degree of government" (1969: 1). Similar concerns led much of the literature on political development in the 1960s and 1970s (e.g., Almond/Coleman 1960, Almond/Powell 1978) even though it employed a systems approach rather than the institutionalist language that is back in vogue today (Skocpol/Evans/Rueschemeyer 1985).

In this paper I argue that current FS research developed out of a different strand of literature within IR that took little notice of earlier work from CP. I further argue that this particular strand was characterized by a particularly strong desire to inform and advise policy. To this end, I first present some of the literature on policy advice before presenting a historiography of the FS research field. The paper then shows different ways in which concerns over practical applicability has influenced the development of theory. In the final part, I explain this development as a path dependent process and discuss some of the pathologies that FS research has inherited as a result.

2. Policy relevance in Political Science research

Within the substantial literature on how Social Science research can be used to advise and inform policy-making the prevailing view is that of science as "suppliers" while there is a "demand" for knowledge from the policy community (see, e.g., Jentleson 2002, Wilson 2007). The task of science, then, is to generate knowledge and transmit it (either directly or through translators like think tanks or specialized research institutes) to political actors, whether these are governmental agents, NGOs or IO staff.

Some authors argue that scientists have a duty to lend their expertise to their policy community, either because of their comparative advantage as a class of people that is free to analyse political phenomena (Zelikow 1994) or because of some higher, moral obligation as Wallace argues when he compares the funtions of contemporary intellectuals with those of prophets and priests in pre-modern societies: "of interpreting signs and symbols, of communing with the infinite, of looking beyond the immediate concerns of day-to-day life, of providing frameworks to reduce the chaos of experience to understandable shape" (Wallace 1996: 306). In a similar fashion, Jentleson (2002) has argued forcefully for a greater engagement of theory with practical issues in the wake of September 11. An additional argument is that as science is largely funded by public tax money, its research findings should be applicable towards the betterment of society.

Studies of policy advice have identified several different kinds of knowledge that scientists can provide. In a seminal contribution, George (1993: 115-134) differentiates three kinds of knowledge that are of value to policymakers: 1) conceptual frameworks for strategies and instruments, 2) generic knowledge (a theory) about the effectiveness of each strategy, and 3) actor-specific behavioral models. George argued that academics are best suited to provide types 1 and 2, while the provision of type 3 knowledge was better left to intelligence agencies. Taking a slightly different approach, Jentleson (2002) identifies three distinct utilities that theory can offer for policy: diagnosis, prescription for conceptualizing strategies, and lesson-drawing. Walt (2005) revises this typology and comes up with four purposes of scholarship: diagnosis, prediction, prescription and evaluation. According to Nye (2008), academics can provide three different kinds of knowledge: immediate advice on particular issues, middle-level theories on general policy areas or world regions, but also the framing and mapping of new questions without giving ready-made answers. Lepgold (1998) argues that different types of research have different values for policy advice. He presents a continuum of knowledge generation and application that spans from 1) pury theory, via 2) issue-oriented puzzles and 3) case-oriented scholarship, to 4) policy making. Ideally, scholars and practitioners working on group 3 and 4 puzzles will draw on results from the first two groups thus helping to translate more abstract knowledge into concrete diagnoses and proposals.

Lepgold's approach already hints at an issue that is frequently decried: the so-called "gap" (sometimes also called a "chasm") between academic research and the policy community. While by no means the first to make this claim, George's 1993 monograph is frequently referred to in current

discussions of the topic.[4] According to the literature (see, e.g., Jentleson/Ratner 2011, Lepgold 1998, Walt 2005), there are several reasons contributing to the existence of this gap. The first is that scholars face unfavorable professional incentives that deters them from striving for practical applications to their work. In short, conducting policy-relevant research is not rewarded in academia which places a higher premium on publications in scholarly publications and bringing in grant money. Secondly, practitioners are said to display little interest in the kinds of questions that scholars ask, instead focusing on those variables which they can directly manipulate. And finally, academic debates are seldom resolved with sufficient clarity to make them easily accessible to practitioners.

Some academics are slightly less sanguine about the issue. They argue that while the gap does exist, it does not represent an insurmountable problem, especially given the proliferation of think tanks and other actors who can serve as "translators" or "bridge-builders" between the separate worlds of academia and practice (Jentleson/Ratner 2011, Wilson 2007). In this vein, Walt speaks of a "trickle-down model linking theory and policy" (2005: 25) whereby general theoretical knowledge gets translated into middle-range theories or regionally specific analysis which is then transmitted to practitioners.

A majority of contributors to the debate explicitly urge scholars to close the gap by reaching out to practitioners and by asking policy-relevant questions.[5] Lepgold and Nincic argue that the pursuit of such knowledge could be beneficial for scholars, in that "a deep and continuing concern for the substance and stakes involved in real-world issues can help prevent theorists' research agendas from becoming arid or trivial" (Lepgold/Nincic 2001: 4). They claim that striving for policy relevance does not have to come at a cost of losing scientific rigor and that policy-relevant theory can be just as true, complete and significant as basic research. Nye (2008), while acknowledging that research institutions are different from think-tank and public policy settings and that these characteristics should not be unduly blurred, argues that scholars should not be afraid that their work might become "tainted" or "corrupted" in some way by a too-close relationship with the policy community. He exhorts university departments to balance their output by hiring a mix of academics with different degrees of contact with practitioners: not all scholars should engage in dialogue with policymakers, but neither should all refrain from it. This would entail a greater openness towards "scholar-pracititioners" (Wasserman/Kram 2009) who are committed to generating knowledge that is useful for policy.[6]

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4   Lepgold (1998) notes for the United States that this gap has only developed in the latter half of the 20th century, whereas in earlier decades, scholar-pracitiners had frequently moved between political and academic positions (also see Wallace 1996 for the United Kingdom). According to Walt (2005), worries about the divide between theory and practice had been growing since the 1970s and had substantially increased in the 1990s. Nye (2008) also claims that the gap has continued to widen since George's initial publication in 1993.

5   Jentleson and Ratner define "policy-relevant scholarship as *research, analysis, writing and related activities that advance knowledge with an explicit priority of addressing policy questions*. Policy-relevant scholarship does not in any way mean atheoretical work. It does, though, orient more toward theories that are middle range in their level of abstraction in contrast to efforts at general theory and –isms. It identifies policy challenges, and only then turning to theories and methods to understand and manage them" (Jentleson/Ratner 2011: 8, emphasis in the original).

6   Paris (2011) criticizes that the existence of a gap between theory and practice seems to be an article of faith that has not been subject to rigorous empirical testing. Indeed, most of the contributions decrying the gap are based on

While this represents a mainstream view of policy-relevant research, there are also two different critical positions, according to a typology proposed by Eriksson and Sundelius (2005). One of them, which they term the Critical Perspective, contends that academia should side with the powerless against the systems and actors that oppress them. Hence, scholars who subscribe to this view have no qualms advising most NGOs even as they reject offers from governmental agencies. The other, the Independence Perspective, warns against engaging in any kind of policy advice on three grounds: 1) The demand for expert explanation of current events leads to a sacrifice of theoretical and historical depth, 2) Policy advice endangers the independence, and thus the integrity of research, and 3) Scholars should have complete autonomy in setting their research agenda.[7] For the remainder of this section, I shall focus on the latter position and on the issue on agenda-setting in particular, since it is especially pertinent to the case of FS research.

The most strident critic of policy relevance is Hill (1994) who warns not only of a too-cozy relationship between scholars and practitioners but more generally of devoting too much attention to current political affairs. He acknowledges that theorists tends to "follow an agenda derived from the movement of events - and indeed that there is an inevitability about doing so in the '*longue durée*'" (Hill 1994: 7).[8] This is partly driven by the availability of funding from governmental agencies and foundations which are primarily interested in contemporary issues (Hill 1994: 7-8, Lepgold 1998: 46, Eriksson/Sundelius 2005: 56) However, a preoccupation with current affairs carries opportunity costs: When the next fad comes along, there will be less scholarship available on it become many scholars had devoted their work to the previous day's topic.

But the biggest risk, according to Hill, is that the academic research agenda will be determined by others – most likely the government – if academics "become, almost without noticing it, reactive to the initiatives of others, rather than pursuing their own professional concerns, which would otherwise intersect with policy issues only occasionally. Creativity is thus attenuated, while the

personal observations from scholars with substantial professional experience on both sides of the divide (e.g., Joseph Nye, Alexander George and Bruce Jentleson). However, Paris rightly argues that theory can exert influence on policy-making in less direct ways than is usually assumed by straightforward models of policy advice. Using research on fragile states as an example, he claims that "academic ideas may also play a role in influencing practitioners' understandings of what is possible or desirable in a particular policy field or set of circumstances, thus 'ordering the world' in which officials identify options and implement policies. If policy influence is defined in this broader manner—capturing the 'gradual seepages into organizations of new ideas, metaphors and rationales'—then the scholarship on fragile states seems to have been somewhat more influential. Specifically, academic research has helped to define and refine understandings of state fragility as a policy problem and it has informed the development of operational frameworks for responding to this problem. Put another way, scholarly ideas have helped to 'order' the conceptual world for policymakers who face the difficult task of responding to most disorderly parts of the physical world" (Paris 2011: 59-60). Borrowing terminology from literature on research utilization in other disciplines, he later identifies these two understandings as the "problem-solving" and the "enlightenment" functions of scholarly ideas (Paris 2011: 61). Eriksson and Sundelius (2005) also emphasize the opportunities in the training of students (future practitioners) and mid-career officials.

7 Even though he comes out in favor of policy-relevant research, Nye (2008) echoes some of these criticisms: When dispensing advice, there is a danger of becoming a "technician" for government, academics might be tempted to follow dominant political opinions and the quest for short-term relevance might lead the analyst to forgo abstraction and general knowledge. Also see Lepgold 1998.

8 Hill cites the development of integration theory during the early days of the European Economic Community. Lepgold (1998: 46) adds that Liberal Institutionalist IR theories emerged in the 1970s as a reaction to a growing economic interdependence as evidenced by the oil crises while the rise of Neorealism took place during a new period of tension between the major powers in the 1980s.

longer time-frame so essential to balanced judgment is squeezed out by the pre-occupations of the day" (Hill 1994: 8). IR, as all Social Sciences, has to preserve its historical, theoretical and normative depth as its comparative advantage lest it degenerate into quasi-journalistic chronicling of day-to-day affairs.

What the critical approach taken by Hill (1994) illustrates is that the unidirectional model of scholars delivering knowledge to those policymakers who demand it is too simplistic. As Walt put it, "(t)he relationship between theory and policy is not a one-way street" (2005: 29). On the one hand, ideas and knowledge might travel from practitioners to academics and thus help spark theoretical innovation. On the other hand, demands and incentives from the policy side might affect the scholarly agenda in less beneficial ways. There are several ways in which a close exchange between theory and practice can impede scholarly progress: First, scholars might engage in outright self-censorship if they fear that their results would endanger their relationship with practitioners. Second, nuance frequently is lost when preparing key findings for an audience of practitioners. Thirdly, research is aligned to official interests and the policy relevance of an issue determines which questions are asked at all (and, by extension, which ones are not). Finally, scholars can be induced to take up political concerns and official terminologies without critically examining them.[9] It is these latter two kinds of influence that are particularly worrisome because they are generally not the product of deliberate choices by academics but more frequently result from subconscious decisions that are made without adequately considering the trade-offs that a scholar engages in. I would further hypothesize that these two types of influence, due to their subconscious nature, are much more frequent than the first two.

Much of the debate about policy relevance is really about how one defines the role and the duties of a scholar. This is, at its heart, a normative question that cannot be answered in a clear-cut manner that would be satisfying to both camps. However, whether the downsides of political relevance outweigh the benefits is a question that can be answered empirically, at least for a given field of research. FS research is a case that brings several features of the debate into sharp relief: The early contours of the debate were sketched by scholar-pracitioners and these shape the field unto this day. The generation of knowledge was never confined to the academic side but took place in a continuous interplay between theory and practice. In many ways, the introduction of the concept of FS was extraordinarily successful, as Paris highlights: "Rarely has a concept traveled so quickly from the periphery to the core of both international relations (IR) scholarship and policymaking" (2011: 58). At the same time, the concept has been subject to manifold scholarly critiques who criticize the concept as shallow, misleading, politically problematic and simplistic.

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9   Examples of this are the burgeoning literatures on issues like aid harmonization, statebuilding or transnational terrorism. Rubin has neatly encapsulated the problem of these kinds of contributions as follows: "Studies of statebuilding operations often try to identify 'best practices' without asking for whom they are best" (Rubin 2006: 184).

3. The Development of Failed States Research

While the body of literature of FS has grown substantially since the early 1990s, there has been little systematic study of FS research itself.[10] While many contributions offer some description of how the field has developed (e.g., Call 2008), only two explicitly analyze the history of the research field.

The first one is Paris' (2011) account of how FS research has been able to influence policy. Therein, Paris basically follows the unidirectional model of policy advice outlined in the previous section. He presents an overview of those aspects of FS literature which have been picked up by practitioners. Early works, he suggests, became influential because they came at the right time, offering a conceptual lens through which to make sense of the post-Cold War disorder: "it was the elaboration of state failure as an organizing concept, more than the findings of the nascent empirical literature, which seemed to successfully penetrate into the policy domain in the early-and-mid 1990s. The failed state concept offered a way of thinking about the new international security environment at a moment when venerable Cold War policy frameworks—including bipolarity, containment, and deterrence—seemed suddenly obsolete" (Paris 2011: 62)

Beyond providing a conceptual vocabulary, the findings from research into the causes of FS have also had some impact by shaping discourses and highlighting particular aspects of fragility. Paris points to the literature on conflict economies, on identity-based conflict and on the impact of regime type on conflict risk. FS research has also helped shape operational frameworks for responding to failed states and post-conflict situations, particularly with the rediscovery of the "statebuilding" paradigm. These discussions have influenced operational strategies within the UN, the OECD-DAC and the World Bank. Finally, state agents have also taken note of recent discussions about the role of legitimacy and state-society relations in fragile environments.

In short, Paris presents FS research as having a substantial, if indirect impact on political practice by shaping understandings of the problem and providing conceptual frameworks for responding to it. Moreover, consistent with his unidirectional model of policy advice he asserts that it was scholars who "played a leading role in articulating the concept of state failure" (Paris 2011: 62). However, he is quite aware of the limitations of his account and thus enumerates several issues that warrant further inquiry: What is the relative importance of academic ideas in explaining policy shifts with respect to fragile states? By what channels did academic ideas about failed states enter the policy community? What problems exist at the researcher-practitioners interface and how might these be addressed? And finally, how has political discourse influenced academic research on FS? (Paris 2011: 65-67)

The second closer study of FS research is an unpublished paper by Büger and Bethke (2011) who view the development of the concept through the lens of Actor-Network Theory.[11] They identify

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10 For an analysis of how the *political* discourse about FS has developed see Lambach 2004.

11 The paper was originally presented at the 2010 ISA Annual Conference. I quote from a revised version of the paper which Felix Bethke has kindly made available to me.

four stages in the development of the network of actors who study FS: "Only loosely mentioned in academia of the late 1980s (phase one), the concept was extended to numerous disciplines and foreign policy makers in 1990s (phase two), it was securitized and globalized in the early 2000s (phase three), and in a contemporary phase (phase four) there has been a double trend of homogenization through quantification and heterogenization through criticism. In all of these phases, circulation has intensified and further actors became enrolled" (Büger/Bethke 2011: 14).

The first phase mainly consisted of contributions from Africanist scholars, frequently in the form of country case studies, who problematized the nature of the post-colonial African state. In the second phase, IR scholars identified FS as a challenge for defence and foreign policies of Western states. Additional themes are studied: changes in sovereign statehood, how to respond to state failure, the role of the state in internal conflict. Furthermore, International Law discovers the topic in reponse to UN interventions in the 1990s. The third phase is characterized by the securitization of FS in the wake of September 11. IR specialists now cast FS as threats to global security by linking them to terrorism and other items of the new security agenda like organized crime, migration and the drug trade. In this phase, international organizations, particularly those concerned with development issues, enter the network. The fourth and current phase is characterized by two innovations: some actors depoliticize the notion of FS through quantification while other contributions question the validity of the concept and criticize its role in legitimizing intervention.

Using co-citation analysis, they compile a list of the most frequently cited works among a corpus of 213 scholarly articles published between 1990 and 2010. The ensuing list "illustrates that the network at least, in terms of citation, is dominated by IR scholars and to some extent by area specialists. Non-academic actors like IOs, foreign policymakers, development agencies and think tanks are cited to a lesser extent" (Büger/Bethke 2011: 29). They describe the development of FS research as "a story of struggle over homogenizing the scope, aim and meaning of the concept" (Büger/Bethke 2011: 31)

What the papers by Paris and Büger and Bethke share is the assumption that FS was developed by scholars first and foremost. In contrast, Jones, who is highly critical of the FS concept as such, opines that it is really a political category that was taken up by academics: "The use of such a category by politicians is not surprising; what is remarkable is the way this notion has been so readily absorbed in academic analysis with little concern or critical reflection. The discourse has been embraced by scholars of International Relations, Political Science and Development Studies. The majority of academic works about 'failed states' take a general acceptance of the category as their point of departure, and proceed to offer explanations of state failure, or discuss appropriate forms of policy towards fragile or failing states. The manner in which the notion has been unquestioningly accepted is illustrated not only by the many specific studies of 'state failure' but by the incorporation of the term in general works." (Jones 2008: 181)

In addition, Newman highlights the role that scholar-practitioners and think tanks, working across the divide between academia and practice, have played a major role in "pushing" the topic: "The

fact that there are so many major programmes which seek to do the same thing – to understand and measure state weakness – indicates the interest which exists in the topic, and the availability of funds to pursue such research. It also raises concerns about the failed state industry, which clearly has an interest in ongoing worries about the international hazards of failed and weak states, which might in turn raise questions about the objectivity and results of some of these analyses" (Newman 2009: 426).

This paper takes up the claims from Jones and Newman in order to add further depth to the accounts of Paris and Büger and Bethke. Specifically, I wish to amend the narrative by focusing on the role of scholar-practitioners and political institutions on the development of the research field. I further challenge Büger and Bethke's assumption that there always has been a unified network of FS researchers. Instead, I argue that for a long time, there have been different strands of research in IR and CP which still have not been integrated.[12] To this end, I will first show that current FS research is strongly foreshadowed in CP research into "weak states" from the 1960s to the 1980s. After that, I discuss why IR was so receptive to the topic and how it developed a distinct approach to FS which remained largely isolated from the CP literature. Finally, I show that the early development of FS as an analytical concept was strongly influenced by practical concerns: policy-oriented scholars ("scholar-practitioners") and (former) practitioners were among the most prominent authors, findings were published in outlets that catered to practitioners and research was funded by governmental agencies.

3.1 Failed States in Comparative Politics and International Relations

The starting point of FS research is that stateness is not dichotomous, as International Law insists, but that it exists along a continuum. This idea was first clearly enunciated by several publications from the 1960s which focused on the postcolonial state in the newly independent countries in Asia and Africa tackled similar questions.[13] For instance, Myrdal spoke of the "soft state" in South Asia which "is handicapped not only by the attitudes and institutions in the villages, but also by inhibitions of the rulers. Moreover, no South Asian country has an administration prepared to enforce new rules, even when these rules are not very revolutionary. Corruption, rampant at least on the lower levels even in colonial times, is generally increasing and takes the edge off commands from the central government" (Myrdal 1968: 898-899). Huntington put this more generally: "The most important political distinction among countries concerns not their form of government but their degree of government" (1969: 1). Similar concerns led much of the literature on political development in the 1960s and 1970s (e.g., Almond/Coleman 1960, Almond/Powell 1978) even

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12 Somewhat confusingly, the term "state failure" – which was very popular in the first decade of FS research before it was supplanted by "fragile states" – already had been used by economists to describe situations where state intervention causes a less efficient allocation of goods than that which would have occurred without intervention.

13 Another early contribution by Nettl (1968) was the first to clearly discuss stateness as a variable. Unfortunately, his paper did not receive much attention at the time.

though it employed a systems approach rather than the institutionalist language of Huntington and others.

After a brief lull, the 1980s saw a range of publications analysing state institutions in developing countries. This was sparked, on the one hand, by a marked disappointment that the newly independent states had failed to develop according to the expectations of modernization theory and, on the other, a rekindling of interest in the state in the Social Sciences (Skocpol/Evans/Rueschemeyer 1985). These contributions were motivated by the puzzle that some postcolonial states, which disposed of an enormous military apparatus and huge bureaucracies, seemed unable to implement even the simplest political measures. Among the first to address this question were Robert Jackson and Carl Rosberg who, in their seminal 1982 article "Why Africa's Weak States Persist" made an observation that foreshadowed the main conclusion of later FS research: "(T)here have been times when Angola, Chad, Ethiopia, Nigeria, Sudan, Uganda, and Zaire have ceased to be 'states' in the empirical sense – that is, their governments lost control over important areas in their jurisdiction during struggles with rival political organizations" (Jackson/Rosberg 1982: 1).[14]

Other works from this period focused on state-society relations.[15] Eric Nordlinger (1987) developed a fourfold typology of states depending on the autonomy of the state and societal support for the state, with weak states those who have both low support and low autonomy. Gören Hyden (1983) described the soft states of Africa as "the inevitable product of a situation where no class is really in control and dominant enough to ensure the reproduction of a given macro-economic system" (Hyden 1983: 63). This results in a state apparatus which is unable to overcome societal kinship ties in its attempt to exercise control. In what is probably the most important and lasting contribution of this strand of research, Joel Migdal (1988) proposed a theory of how weak states interact with their societies. He portrayed state-society relations as a strategic interaction between agents of government and a variety of societal authority figures ("strongmen"). According to Migdal, the effectiveness of the state is determined by the degree to which he can co-opt or overpower strongmen. These strongmen, in turn, attempt to preserve their power bases while trying to get access to rents which are controlled by the state.[16]

In contrast, IR had long been oblivious to the issue of state fragility. "Weak states" were generally thought to be states lacking in material capabilities to properly defend themselves from external aggression (see, e.g. Handel 1990). There were some early works that attempted to introduce questions of fragility into IR, most notably Buzan (whose "People, States, and Fear" was first published in 1983) who considered weak states to be subject to an ongoing state-formation process:

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14 Robert Jackson's early specialization was in Comparative Politics and African Politics. It was only in this 1982 article, and particular in later explorations of this topic (Jackson/Rosberg 1986) that Jackson began to develop an IR approach to the topic. Thus, to a degree, Jackson contradicts my argument that IR did not address problems of stateness until the 1990s.

15 Beyond these theoretical contributions, there are also a number of single-country case studies like Callaghy (1984) which take a similar approach.

16 For a nuanced discussion of the types of power that states employ, see Mann 1986.

"Because they are still in the early stages of the attempt to consolidate themselves as state-nations, domestic violence is endemic in such states. Under these circumstances, violence is as likely to be a sign of the accumulation of central state power as it is to be a symptom of political decay" (Buzan 1991: 99).[17]

Robert Jackson's monograph "Quasi-States" (1990) provided a foundation for future IR engagement with FS. Therein, he consolidated his earlier work on the sovereignty of postcolonial states (Jackson 1986, 1987, Jackson/Rosberg 1985, 1986) into a coherent theory about the development of a new "sovereignty regime" that granted "negative sovereignty" to those postcolonial states which were incapable of displaying "positive sovereignty", i.e., the capability to exercise effective control over their territories and populations.

It was not until a few years later that the term "failed state" was first used in a scholarly publication. when Gerald Helman and Steven Ratner, in a contribution for *Foreign Policy*, described it as "utterly incabale of sustaining itself as a member of the international community" (1992/93: 3). In retrospect, it does not seem surprising that IR should appropriate the concept at this particular time in history after having successfully ignored the literature that had already existed for decades. After the end of the Cold War, IR was in a strange, new land for which it had no maps. Within a few short years, the major challenges of the day had turned completely around, from deterring nuclear aggression by a major power to containing civil wars through multilateral peacekeeping. The number of internal conflicts rose sharply, increasing pressures on the international community to intervene. Against the backdrop of debates about the meanings of sovereignty (Walker 1990, Weber 1992) and security (Buzan 1991) in an increasingly interconnected world as well as calls for global governance (Rosenau 1992), practitioners and scholars alike were casting about for concepts that would help them understand the new challenges the world was faced with.[18] The notion of FS was just such a concept "defining a phenomenon that helped practitioners make sense of a new, ambiguous international environment, which was open to many possible interpretations" (Paris 2011: 62).

Table 1 about here

Helman and Ratner's essay achieved widespread recognition. A citation analysis shows that only a few years after its initial publication, it had already been widely cited, mostly in International Law and in Political Science papers discussing humanitarian intervention, peacekeeping and reconstruction of war-torn countries. It was not until 1995 that other publications started to scrutinize the concept as such, giving it more analytical depth. Table 1 lists all publications from

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17 Job (1992) built on Buzan's work to propose that the security dilemma worked differently for weak states. Also see Holsti 1996.

18 We should not underestimate the fear and uncertainty that this post-Cold War disorder had provoked. Apocalyptic predictions by journalists and intellectuals (Kaplan 1994, Rufin 1991, Enzensberger 1993) received mainstream attention while even some scholars penned doom-and-gloom visions of the future (Huntington 1996).

1992 to 1998 that make a direct contribution to the study of FS instead of employing the concept to speak of a different issue. The list was compiled through a qualitative assessment using rather narrow criteria to determine whether a particular article were about FS *per se* or whether it was really speaking about a different but related issue. This disqualified a number of publications which are highly relevant to FS research but which are really about the African state (Bayart 1993, Englebert 1997), corruption (Reno 1995), state sovereignty (Buzan 1991), the international relations of postcolonial states (Clapham 1996), the state and war (Holsti 1996) or individual cases of state failure (Reno 1995). It also disqualified contributions to the CP literature on weak states (e.g., Forrest 1994) as well as discussions from other disciplines like International Law (e.g., Türk 1995), Geography (Christopher 1997) or Philosophy (Cavallar/Reinisch 1998) as well as journalistic accounts (Kaplan 1994).[19]

This early literature provided diverse attempts at a definition and delimitation of the FS concept. Büger and Bethke (2011) argue that several actors attempted to establish themselves as central points of the network by providing conceptual accounts which would have enticed other actors to cite their works. Noticeably, the clear majority of this early literature – while spurring substantial progress in the conceptual development of FS – also took a very explicit IR approach to the issue. First of all, the authors frequently took an external point of view on failed states, refraining from delving too deeply into the domestic politics of a country or engaging in a detailed analysis of its society. Second, almost all publications provided some discussion of the implications of FS for Western countries and presented policy recommendations for dealing with these troublesome countries. These two points show that the authors – in the best tradition of policy-relevant research – operate from the vantage point of their respective governments and, more or less overtly, identify themselves with the interests of these governments. The result is that, third, the role of external actors (or the regional and international environment in general) in causing state failure is downplayed or ignored outright in favor of variables like neopatrimonialism, rentierism and ethnic politics.

This particular approach is no surprise given that the large majority of works in Table 1 were published in journals or edited volumes that are clearly aimed at an IR audience. The only exceptions were the works by Widner, Zartman, Gros and Forrest who spoke to a CP/Development Studies/African Studies audience.[20] Noticeably, not one of the remaining 14 IR texts cites a single

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19 Also, the first and second phase reports from the State Failure Task Force (SFTF) which are dated 1995 and 1998, respectively (Esty et al. 1995, 1998b), have not been included since these are only the dates when they were finished and presented to the CIA who had funded the SFTF's activities. I understand that they were only released to the public with considerable delay – in their 1998 article, Esty et al. describe the First Phase Report from 1995 as "for official U.S. government use" (Esty et al. 1998a: 38, Fn. 11). While I cannot give an exact date when the reports were eventually released to the public, a citation analysis for the First Phase report using Google Scholar shows that – except for two or three cases – virtually all citations have been from 1998 or later. This shows that the SFTF's results had little impact on the initial development of the research field – which is just as well since what the SFTF was studying was not state failure at all (for a detailed critique see Lambach/Gamberger 2008).

20 A majority of authors also self-identify as scholars of IR or Security Studies although this criterion is less clear-cut than the venue of publication. For instance, Ali Mazrui or William Zartman have substantial publication records in both CP and IR. In addition, Steven Ratner is a lawyer, Gearoid O Tuathail a geographer and Timothy Luke a political theorist.

work from the earlier CP literature, thus betraying a distinct lack of interest in understanding the domestic dynamics of failure.[21] This is consistent with the findings of Büger and Bethke (2011: 28-29) who show that four of the five most cited works in the FS network (Rotberg 2004, Kaplan 1994, Fukuyama 2004, Jackson 1990) take an IR perspective (the exception is Zartman 1995).

Comparative Politics, meanwhile, did not continue its research into "weak states" using this particular term. However, other strands of literature were built on top of the foundations established by the earlier works of Huntington, Migdal and others. In African Studies alone, this entails the literature on the African State and the Postcolonial State (Bayart 1993, Mbembe 2001, Young 2004, Zolberg 1992), on State-Society Relations (Rothchild/Chazan 1988, Migdal 1994), and on neopatrimonialism and corruption (Reno 1995, 1998). This research continued separately from the burgeoning IR literature on FS, a state which was reinforced through subdisciplinary boundary practices as expressed in publication venues and professional meetings and organizations. It was only in 1999 that CP articles started to appear which took notice of both bodies of literature (Allen 1999, Baker 1999).[22]

3.2 The Political Origins of Failed States Research

Research into FS was not only strongly IR-centric in its formative years, it was also heavily influenced by concerns of policy relevance. The influence of the policy community and of scholar-practitioners is visible in three ways which I will discuss in turn: 1) (Former) practitioners and scholar-practitioners as authors of relevant articles, 2) the publication of these articles in outlets that are primarily addressed to practitioners, and 3) research funding by governmental agencies.

The 18 publications from Table 1 have a combined total of 26 different authors. If we exclude article no. 15 which alone has seven authors (Daniel C. Esty, Jack Goldstone, Ted Robert Gurr, Barbara Harff, Pamela T. Surko, Alan N. Unger and Robert S. Chen) this leaves 19 authors who have contributed to the early FS literature. A review of publicly available biographies shows that a substantial number of these authors have close ties to practice.

Eight of these 19 authors have spent the majority of their professional lives in the policy community, usually as staff members of state agencies: John A. Ausink has worked for the U.S. Department of Defence, the RAND Corporation and several other policy-oriented think tanks. Pauline H. Baker is President of the Fund for Peace and has prior work experience in several positions at the U.S. Congress. Douglas H. Dearth has held various posts in the U.S. intelligence community. Gerald B. Helman is a retired United States Ambassador who now works for a private policy consulting business. James F. Miskel has worked for the U.S. National Security Council and

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21 A partial exception are Luke and O'Tuathail (1998) who cite more recent monographs – Bayart (1993) and Bayart/Ellis/Hibou (1999) – that follow in the footsteps of the CP "classics" from the 1960s to 1980s. Another is Rondos (1994) who also cites various Africanist publications.

22 In Germany, a similar process played out in a much smaller disciplinary community with the CP tradition kept alive by Tetzlaff (1992, 1993, 1995) and Molt (1995) while Mair (1996, 2000) and Schneckener (2003, 2006) took an IR approach.

other government agencies. Richard J. Norton is a Commander in the U.S. Navy. Alex Rondos has spent his career in a variety of positions with NGOs, the World Bank and the Greek government. Karin von Hippel has recently joined the U.S. State Department after having worked for several UN and EU agencies.

Several of the remaining authors are also tied to educational institutions of the U.S. military. In addition to Miskel and Norton, Robert H. Dorff and I. William Zartman have held positions at the Naval War College, the Naval Academy or the Army War College. Another four authors have worked as consultants for political bodies: Mohammed Ayoob has worked for the United Nations and the International Commission on Intervention and State Sovereignty, Jeffrey Herbst has consulted for the UN, the World Bank and the U.S. State Department, Ali A. Mazrui has worked for the Organization of African Unity, the World Bank and the UN and Steven B. Ratner has been advisor to the UN and the State Department. Only five (Joshua B. Forrest, Jean-German Gros, Timothy W. Luke, Gearoid O Tuathail and Jennifer A. Widner) do not seem to have had a professional relationship with the policy community at some point in their career. It is telling that three of the four authors identified as CP scholars in the previous section (Forrest, Gros and Widner) turn up as "pure" scholars while the majority of IR scholars display varying degrees of proximity to practice.[23] It is exactly this proximity that has led scholars to uncritically accept the prevailing notion that "failed states" exist and that this particular syndrome is best categorized as "state failure". That this is no mere coincidence is underscored by the fact that Luke and O Tuathail, the only IR scholars without any connection to the policy community, have produced the articles that are the most critical of the concept, i.e., exploring the use of "failed states" discourse in popular media.

Scholar-practitioners, beyond simply accepting the concept of FS at face value, also tend to approach the subject from the perspective of Western policy-makers. The articles surveyed above are replete with the question "What should be done about failed states?" Many feature explicit policy recommendations aimed at the policy community. "As those states descend into violence and anarchy […] it is becoming clear that something must be done", write Helman and Ratner (1992: 3). In his contribution, Herbst "suggests some alternative strategies to deal with failure in Africa, and elsewhere, that would involve significant changes in international legal and diplomatic practices" (Herbst 1996: 120). It is quite obvious that striving for policy relevance inevitably leads to a normative approach, yet this normativity is not critical but rather supports and affirms official positions.

The role that policy relevance played in the early development of the literature is also quite evident when looking at where the respective pieces were published. Of the 18 articles, eleven appeared in journals or edited volumes that were clearly aimed at a more practically minded audience. Helman and Ratner's article was published in *Foreign Policy*, a "general audience" periodical which in 1992

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23 It should be noted that William Zartman's time at the U.S. Naval Academy was the only government-related post that he occupied during his long and distinguished career. If we take this into account, the theory-practice divide between IR and CP becomes even more stark.

was published by the Carnegie Endowment for International Peace. Nowadays, it is part of a media conglomerate and boasts that "(t)he magazine's readers include some of the most influential leaders in business, government, and other professional arenas throughout the United States and more than 160 other countries"[24]. Two articles were published in *Parameters*, a Strategic Studies journal from the US Army War College which features "topics of significant and current interest to the US Army and the Department of Defense. It serves as a vehicle for continuing the education and professional development of graduates of the US Army War College (USAWC) and other senior military officers, as well as members of government and academia concerned with national security affairs"[25]. Other journal articles were published in the *World Policy Journal*, the *Defence Intelligence Journal*, the *Naval War College Review* and *Brassey's Defence Yearbook,* all of whom explicitly cater to a practical readership.

Of the papers in edited collections, four can be classified as policy-relevant. The article by Ayoob appeared in the volume "Managing Global Chaos", edited by Chester Crocker, a former U.S. Assistant Secretary of State, and Fen Osler Hampson and published by the United States Institute of Peace Press. The book was designed as both a textbook on methods of conflict management and a resource for diplomats, military officers and other practitioners. The Esty et al. paper was published in an edited collection by John Davies and Ted Robert Gurr entitled "Preventive Measures: Building Risk Assessment and Crisis Early Warning Systems" which was published by Rowman and Littlefield, a core concern of which was to help in the development of better early warning systems for practical use. Alex Rondos' article appeared in the volume "Global Engagement", edited by Janne Nolan and published by the Brookings Institutions that featured several serving government officials among its contributors. Finally, Ali Mazrui's 1998 paper was published in "Peacemaking and Peacekeeping for the New Century" by Olara Otunnu and Michael Doyle, also from Rowman and Littlefield, a book that featured several high-profile practitioners like Kofi Annan, Boutros Boutros-Ghali, Nelson Mandela or Sadako Ogata among its contributors.

The remaining publications, which again include all four CP contributions, appeared in more traditional scholarly venues, such as the journals *Third World Quarterly*, *Daedalus*, *International Security*, *Review of International Political Economy*, and *Geopolitics*. The volume edited by Zartman was published by Lynne Rienner while Forrest's article appeared in another Lynne Rienner publication, "The African State at a Critical Juncture", edited by Leonardo Villalón and Philip Huxtable. It is also quite clear that policy relevance did not come at the cost of academic impact. Of the six most-cited works (Zartman, Helman/Ratner, Herbst, Ayoob, Gros, Esty et al., in that order), three (Helman/Ratner, Ayoob, Esty et al.) clearly strive for policy relevance while the other three (Zartman, Herbst, Gros) seem to have more scholarly goals.[26]

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24 http://www.foreignpolicy.com/about_us, retrieved 30 August 2011.
25 http://www.carlisle.army.mil/usawc/Parameters/Mission.cfm, retrieved 30 August 2011.
26 These six works all have 95+ citations according to Google Scholar while the remaining eleven all have 50 citations or fewer.

Finally, the policy community directly influenced the early development of the field through financial incentives. The theoretical literature surveyed above agreed that the availability of grant money can influence the topics that scholars choose to study which sometimes results in academic "fads", and the FS field is no different in this regard. The most obvious, and arguably the most prominent example is the State Failure Task Force (SFTF, since 2003: Political Instability Task Force, PITF), a team of eminent researchers that was formed in 1994 when senior policymakers, reportedly led by U.S. Vice-President Al Gore himself, demanded tools to "assess and explain the vulnerability of states around the world to political instability and state failure"[27]. With generous funding from the CIA, the SFTF undertook a massive data-mining effort to extract the correlates of state failure – which they defined broadly as any revolutionary or ethnic war, genocide or adverse regime transition, thus limiting the project's value to FS research in a more narrow sense – to develop a model that was able to predict these kinds of political crises two years in advance with some degree of certainty. The SFTF/PITF has since refined this global model, developed regional models and addressed questions that are related to political instability (the most recent results are published in Goldstone et al. 2010).

In the United Kingdom, the Crisis States Research Centre at the LSE had received a ten-year grant from the Department for International Development to look into the causes of state failure and identify factors that affect the successes for post-conflict reconstruction (Crisis States Programme 2001). Furthermore, the work that led to the publication of Robert Rotberg's two influential edited volume (Rotberg 2003, 2004) had been sponsored by the World Peace Foundation which intended to "provide both practical and conceptual understanding to practitioners and scholars"[28].

The policy community also sponsored key academic events. Between 1998 and 2002, a series of five conferences on failed states was held, first at Purdue University, later in Florence, Italy and in Santa Barbara, CA, which were financed by the Strategic Outreach Program of the Army War College.[29] These conferences featured more than 40 paper presentations by well-known scholars such as Chadwick Alger, Mohammed Ayoob, Lothar Brock, Christopher Clapham, Robert Dorff, Ted Robert Gurr, Hans-Henrik Holm, Robert Jackson, Michael Nicholson, Georg Sørensen and Peter Wallensteen. While not all of these paper were subsequently published in traditional academic outlets, they were made available on the World Wide Web, giving them great visibility and ensuring their impact on the field.

These examples show that the early development of FS as a concept was strongly influenced by practical concerns, in some cases even by practitioners and political institutions themselves. The educational institutions of the U.S. military – the Army War College and the Naval War College in particular – were deeply involved in establishing failed states as a viable field for academic research. Some authors assert that analytical utility was never the primary aim of FS research but

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27 http://globalpolicy.gmu.edu/pitf/, retrieved 30 August 2011.
28 http://www.worldpeacefoundation.org/collapsed.html, retrieved 30 August 2011.
29 http://web.archive.org/web/20030621174704/http://www.ippu.purdue.edu/failed_states/index.cfm and
   http://www.comm.ucsb.edu/research/mstohl/failed_states/2002/failedstates.php#paper_index, both retrieved 30
   August 2011.

the production of a legitimizing discourse for external intervention in poor countries (Bilgin/Morton 2002, Hill 2005, Sogge 2007). I would not go as far – there did not seem to be a concerted effort by governmental agents to create such a legitimazing discourse. Rather, these agents were seeking an understanding of the complex new challenges in the post-Cold War world and thus asked questions (or supported those who asked these questions) in a particular way. In the end, the notion of FS strongly resonated with practitioners' concerns in the immediate post-Cold War period (Halvorson 2010).

Did any of the scholars involved in the early period of FS research violate their academic integrity in the process? How one answers this question comes down to the roles that actors assign themselves and the meanings they attach to terms as "scholar" and "practitioner". The evidence presented above shows that for the majority of authors, there was no inherent conflict between their academic and their practical duties, particularly for those who were practitioners first and foremost. And yet, Hill was right to assert that too close a contact between the worlds of theory and practice – as is clearly the case here – has its costs. In the field of FS research there were strong pressures and incentives to follow an approach to the topic that takes an external point of view, that glosses over the role of external actors in fostering fragility and that casts fragility as a problem of, or worse, a security threat to the international community. Thus, certain avenues of research were closed off and critical questions were often left unasked, at least by adherents to the mainstream that was established in those days. In sum, it is fair to say that the more insidious dangers I mentioned above – research aligning with official interests, the policy relevance of an issue determining the questions asked, and political concerns being taken up by scholars – have clearly come to pass in this case.

4. Conclusion: Path dependence in FS Research

This paper has provided evidence that early research into failed and fragile states has been strongly shaped by concerns over policy relevance. While it cannot be denied that later on, the evolving academic discourse has been able to inform and influence policymaking (Büger/Bethke 2011, Paris 2011), the autonomy of this discourse from practical concerns should not be overstated. Very likely, it is exactly this proximity of theory and practice that has facilitated the adoption of the FS concepts by all kinds of political actors.

Depending on where one stands in the debate about the roles and duties of academics, this might seem like a desirable outcome. However, it has clearly taken its toll on academic research, not just on its integrity – the meaning of which is contested – but also on its quality. As Kuhn (1970) has pointed out in his classic study, research is path-dependent: The mainstream creates a dominant paradigm that shapes future research until it is eventually overturned in favor of a revolutionary new paradigm that promises greater explanatory power. In the case of FS research, the early research had the same kind of influence on later works, by establishing a mainstream which future

contributions adhered to. As Büger and Bethke (2011) show, the field is still strongly dominated by an IR perspective.

More recent literature has inherited the problems of earlier contributions: First, the research takes an outsider's perspective with only a limited understanding of domestic political processes thus ignoring the richness and complexity of a society. This opens it to the – quite justified – charge that state failure is too easily equated with anarchy, a Hobbesian state of nature which glosses over the variety of non-state forms of political order that have been shown to exist under such conditions. Second, external factors or actions by external actors that might contribute to failure are often downplayed (Nuruzzaman 2009). Third, fragile states are understood first and foremost as "our" problems to solve (or worse, as a threat to our interests and well-being) not as features of the political organization of a given society. Taken together, these three problems also explain why there is no comprehensive theory of the causes of fragility whereas entire library shelves are filled with suggestions how to deal with its consequences. We do not know much about what makes a state "fragile", if that is indeed an appropriate descriptor, but we sure know a lot about the international consequences arising therefrom.

This is not to deny that some very critical papers have been published even within IR (e.g., Bilgin/Morton 2002, Call 2008, Eriksen 2011, Jones 2008). There are also several innovative contributions that offer better analytical leverage than the relatively limited FS paradigm, e.g. by situating the issue within ongoing processes of state formation (Schlichte 2005) or looking at hybrid forms of governance that produce political order in the absence of a Weberian state (Clements et al. 2007). But these are distinct minorities to what is still a very uncritical mainstream. Anthropologists as well as CP and Area Studies scholars frequently display an aversion towards the term, even incredulity at its widespread popularity. They – rightly – criticize the notion of FS for its empirical shallowness and its lack of a theory of state-society relations (see, e.g., Andersen/Möller/Stepputat 2007, Bøås/Jennings 2005, Hagmann/Hoehne 2009). Thus, the two separate academic discourses from CP and IR have begun to meet, but only in a very limited way that has been anything but fruitful for everyone concerned.

Does this mean that FS should be abandoned as an analytical concept, as Call (2008, 2011) argues? Not necessarily. I believe that the possibilities of the term have not been fully explored and that a new approach to the study of fragile states could lead to interesting new avenues for research. First of all, state fragility has to be contextualized historically. As the archeologist Joseph Tainter put it, "we today are familiar mainly with political forms that are an oddity of history, we think of these as normal, and we view as alien the majority of the human experience. It is little surprise that collapse is viewed so fearfully" (1988: 24). But by approaching fragility and failure of state institutions not as a terrible calamity but as one possible, and entirely temporary outcome of the contingent processes of state formation and decay would go a long way towards appreciating the complexity of historical developments. Second, FS research would do well to learn from anthropological accounts of state functioning in developing countries (for a synthesis of African cases, see Bierschenk 2010).

These accounts provide a vivid picture of how the state works even under less-than-ideal conditions and would help dispel the "deficit-oriented" mindset that underpins the current mainstream. Related to that, a theory of FS would also have to have a theory of political order that explains how men govern themselves in the absence of a centralized, hierarchical polity – which, contrary to the popular myth of statehood, is (still) an everyday reality in many parts of the world. Third, FS research should concentrate on its absolute core: the ability and capacity of state institutions to regulate society and uphold public order. This entails a rejection of overly broad characterizations that are expressed, e.g., in popular statistics like the Failed States Index (Fund for Peace 2005) which is little more than an amalgamation of various indicators of political and social crisis rather than an accurate measurement of state fragility. By ridding the literature of its normative and teleological burden, FS can become a valuable analytical concept that allows one to speak about the problems and results of state institutions which do not conform to the Weberian ideal-type.

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Table 1: Publications on Failed and Fragile States, 1992-1998

No.	Year	Title	# Citations
1	1992	Helman, Gerald B. and Steven B. Ratner (1992): Saving Failed States. In: Foreign Policy(89): 3-20.	303
2	1994	Rondos, Alex (1994): The Collapsing State and International Security. In: Nolan, Janne E. (Ed.): Global Engagement: Cooperation and Security in the 21st Century. Washington: Brookings Institution: 481-503.	2
3	1995	Mazrui, Ali A. (1995): The Blood of Experience: The Failed State and Political Collapse in Africa. In: World Policy Journal 12(1): 28-34.	24
4	---	Widner, Jennifer A. (1995): States and Statelessness in Late Twentieth-Century Africa. In: Daedalus, Vol. 124, No. 3, p. 129-153.	32
5	---	Zartman, I. William (Ed.) (1995): Collapsed States: The Disintegration and Restoration of Legitimate Authority. Boulder: Lynne Rienner.	558[30]
6	1996	Ayoob, Mohammed (1996): State Making, State Breaking, and State Failure. In: Chester Crocker and Fen O. Hampson (Ed.): Managing Global Chaos: Sources of and Responses to International Conflict. Washington D.C.: Institute of Peace Press: 37-51.	104
7	---	Baker, Pauline H.; Ausink, John A. (1996): State Collapse and Ethnic Violence: Toward a predictive model. In: Parameters, Vol. 26, No. 1, p. 19-31.	49
8	---	Dearth, Douglas H. (1996): Failed States: An International Conundrum. In: Defense Intelligence Journal 5(2): 119-130.	11
9	---	Dorff, Robert H. (1996): Democratization and Failed States: The challenge of ungovernability. In: Parameters, Vol. 26, No. 2, p. 17-31.	38
10	---	Gros, Jean-Germain (1996): Towards a Taxonomy of Failed States in the New World Order: Decaying Somalia, Liberia, Rwanda and Haiti. In: Third World Quarterly 17(3): 455-471.	100
11	---	Herbst, Jeffrey (1996): Responding to State Failure in Africa. In: International Security 21(3): 120-144.	183
12	1997	Luke, Timothy W. and Gearoid O Tuathail (1997): On Videocameralistics: The Geopolitics of Failed States, the CNN International and (UN)Governmentality. In: Review of International Political Economy 4(4): 709-733.	16
13	---	Norton, Richard J. and James F. Miskel (1997): Spotting Trouble: Identifying Faltering and Failing States. In: Naval War College Review 50(2): 79-91.	10
14	---	von Hippel, Karin (1997): The Proliferation of Collapsed States in the Post-Cold War World. In: Brassey's Defence Yearbook, Vol. 107, p. 193-209.	2
15	1998	Esty, Daniel C. et al. (1998): The State Failure Project: Early Warning Research for U.S. Foreign Policy Planning. In: John L. Davies and	97

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[30] Citations for the edited volume as a whole. Individual chapters have different citation counts in addition to this figure.

		Ted R. Gurr (Ed.): Preventive Measures: Building Risk Assessment and Crisis Early Warning Systems. Lanham, Oxford: Rowman and Littlefield: 27-38.	
16	---	Forrest, Joshua B. (1998): State Inversion and Nonstate Politics. In: Villalón, Leonardo A.; Huxtable, Phillip A. (eds.): The African State at a Critical Juncture: Between disintegration and reconfiguration. Boulder: Lynne Rienner, p. 45-56.	32
17	---	Luke, Timothy W. and Gearoid O Tuathail (1998): The Fraying Modern Map: Failed States and Contraband Capitalism. In: Geopolitics 3(3): 14-33.	7
18	---	Mazrui, Ali A. (1998): The Failed State and Political Collapse in Africa. In: Olara A. Otunnu and Michael W. Doyle (Ed.): Peacemaking and Peacekeeping for the New Century. Lanham: Rowman & Littlefield: 233-243.	n/a

Note: "# Citations" are the number of citations according to Google Scholar as of 26 August 2011.