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## **The Method Makes the Manuscript: Key Texts in the Theoretical and Methodological Advancement of the Study of Civil War**

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**The Logic of Violence in Civil War** by Stathis N. Kalyvas Cambridge: Cambridge University Press, 2006. Pp. 508+ xvii+ appendix+ bibliography +index. £16.99 (pbk). ISBN 978-0-521-67004-3.

**Insurgent Collective Action and Civil War in El Salvador** by Elisabeth J. Wood. New York: Cambridge University Press, 2003. Pp. 257 +xvii +epilogue + appendix + notes + bibliography + index. £22.99 (pbk). ISBN 978-0-521-01050-4.

**Inside Rebellion: The Politics of Insurgent Violence** by Jeremy M. Weinstein Cambridge: Cambridge University Press, 2007. Pp. 375 + xv + appendix + bibliography + index. £17.99(pbk). ISBN 978-0-521-86077-2.

The last ten years have been an exciting and stimulating period for scholars focused upon the study of civil war. Publications have grown exponentially, academic communities have continually expanded and civil war scholars have assumed a position at heart of methodological advancement within the social sciences. The three texts under review have been at the centre of these developments, becoming essential reads within the literature on violence and conflict. Undoubtedly each represents an ambitious and comprehensive project, constituting notable advancements to the field. Broadly speaking they each seek to develop our understanding of the variance in motivations, intensity and patterns of violence adopted by insurgent movements. Going beyond correlation analysis, each study represents a return to favor of case based work within conflict studies, combining a range of methodologies to present compelling accounts of the micro-foundations of violence.

The important contribution made by each of these texts is perhaps best reflected in the notable number of reviews already undertaken<sup>ii</sup>. A wide variety of authors within a range of different journals have published papers assessing the achievements of each of the works. Indeed, Kalyvas himself wrote a lengthy, and at times pointed review of Weinstein's book.<sup>iii</sup> Yet to date most reviews have focused on the undoubted accomplishments of the works, failing to discuss in detail some of the areas in which improvement remains possible. The authors of this paper first observed this deficiency when we met at workshop based upon qualitative methods and the study of civil war, at the Peace Research Institute Oslo (PRIO)<sup>iv</sup>. In particular we noted that

existing methodological assessments remained far too abstract, notably failing to assess if the data that informed the studies was put to use accurately, or if the conclusions drawn from new bodies of data were as valid as the authors assumed. Moreover issues regarding case selection, the cause-symptom problem and tensions between rational choice and other modes of explanation were too often excluded from focus. As a result of both the discussions undertaken on the course and the subsequent dialogue that the authors continued, we have produced this review that we believe is more “micro” in its methodological critique, allowing for a more holistic evaluation of the strengths and weakness of the approach taken in each of these three seminal works.

The paper opens with a brief section recounting those areas in which the texts have most notably progressed theoretical and methodological understanding within the discipline, before the substantive section of the paper addresses methodological issues within these works that have too often been overlooked.

### **Civil War Theory from the Inside Out**

As the title of his text suggests, Weinstein’s work seeks to build a more substantial theory of civil war by going ‘inside rebellion’ to illuminate the origins and structures of insurgent movements. In doing so the author presents an acutely rational theory that views initial endowments to which rebel leaders have access as the key determinant of the form of organization that emerges and the strategies of violence they employ.

For when a rebel force has access to lootable or otherwise easily accessible resources, Weinstein predicts the formation of a group characterized by low levels of commitment, taking fewer risks for immediate short-term gains. These groups of rebel ‘consumers’ are for Weinstein more likely to enjoy a flood of new opportunistic recruits, who are more likely to undertake indiscriminate and ill disciplined forms of violence. Alternatively it is argued that those groups that originate in resource-poor environments must instead rely on their social endowments for recruitment. Thus resource poor groups must appeal to long-term interests, draw on norms, networks, and values that produce a selective pool of activist recruits. These rebel ‘investors’ will endure the costly investment required within warfare for the promise of future

payoffs. As a result a greater discipline is expected, meaning better relations with non-combatants should be the norm.

On the theoretical side, by creating two mutually exclusive categorizations Weinstein's theory lacks an element of dynamic appreciation. For based solely on the presence or absence of resource endowments, groups are categorized into mutually exclusive categorizations (consumers or investors). This disregards the quite probable eventuality that a rebel group shifts from one group to the other in the course of a conflict. Moreover it ignores the possibility of groups in resource poor regions recruiting on the basis of economic benefits, an occurrence that for example in the presence of external sources of income may in reality be quite likely. The use of coercion and abduction in recruitment is also excluded from the analysis, save a minor reference in a footnote. Hence the theory fails to truly appreciate the multiple incentives that are likely to be utilized by all rebel groups.

In addition to this lack of dynamism, the theory attempts to hold constant both the state and civilian actors. In this sense the relative power of actors, or the level of control, is essentially ignored. Such exclusion is illogical as the collaboration or hostile nature of non-combatant population, and the capacity of the state to challenge the rebels are each likely to have some bearing on the form of violence a movement adopts.

### **A Dynamic Analysis of Civil War**

It is this dynamic element of relative power or the variance in levels of control that assumes the central place in the work of Kalyvas. In this sense the author goes beyond the simplistic dichotomy of active combatants and passive civilians and shows how patterns of violence in civil war results from the interaction between combatants and non-combatants. The author connects three levels of analysis – interactions between unitary political actors; interactions between political actors and the populations under their control; and interactions within groups and among individuals.

Kalyvas focuses on developing a theory of what he describes as the “deep structure” of violence in human conflict, one that links local experiences of violence to strategic goals of combatants and civilians alike, using degree of control as the key intervening variable. Variation in the level of violence is a non-linear function of patterns of

control of territory by combatants, and the shifting patterns of incentives that result for civilians to either defect or denounce to one warring side or another. He provides a sophisticated and detailed account of the mechanisms by which control and collaboration interact, focusing in particular on the “micro-foundational” logic linking violence to belligerents’ territorial control and civilians’ decisions to collaborate (or not) in conflict situations. In this sense Kalyvas operates from a position of “thick” rationality, assuming actors to act strategically in pursuit of a range of preferences including the acquisition of status or wealth, the increased likelihood of survival of selves and family members, and other similar incentives.

However, despite the clarity of the author’s argument there are still open questions regarding the scope conditions of his theory. In claiming that his argument applies to any “armed combat within the boundaries of a recognized sovereign entity between parties subject to a common authority at the outset of the hostilities” (p. 17) Kalyvas may have overstated its range. For instance, as Weinstein argues, economically and politically motivated actors may differ in the way they behave, as a result of the different incentive structures upon which recruitment is based. In particular, individual fighters do not always comply with the commands of their immediate superiors in civil wars in which the organizational structures are too weak to enforce these rules coherently. This is a point that Kalyvas recognizes in theory, but does not explicitly include in his formal model and hypotheses regarding selective violence (p. 195-204). Rather he assumes organizations employ violence against civilians rationally, whether discriminately or indiscriminately, in an attempt to deter defection. Moreover he makes the strong assumption that command and control issues are insignificant in the deployment of violence against civilian populations. Clearly, accounts provided by Weinstein, among others, illustrates that such is not always the case. This points to a potential limitation on the utility of Kalyvas’ theory, namely that it is less applicable to forms of conflict such as Weinstein’s “opportunistic rebellions”, where short-term economic gains take precedence over strategic objectives for combatants (p. 10).

### **Collective Emotional Action**

Wood’s research is guided by the question of why, despite the extraordinarily high risks of doing so, non-combatants support opposition organizations (including guerilla

armies) when material rewards are low. This is grounded in the case of El Salvador, in which despite minimal material incentives, insurgents overcame ‘free rider’ problems to generate widespread and largely voluntary participation. Wood contends that the traditional (Olsonian) explanation of collective action is an inadequate approach through which to explain the conflict in El Salvador, for neither coercion nor selective material incentives can account for the behavior of insurgent campesinos.

Instead Wood grounds her explanation of non-combatant participation upon moral and emotional justifications (p. 251). Rejecting the traditional confinement of such features to the irrational sphere, Wood argues that they were “cogent and enduring” reasons for rational action. Wood thereby reconceptualises the nature of incentives that may motivate collective action, while accepting the rational choice tenet that “social outcomes are the by-products of choices made by individuals.”<sup>v</sup>

Wood’s interpretation of the collective action puzzle centers upon three main elements, participation, defiance, and pleasure in agency. Participation justifies non-combatant involvement in relation to religious obligation and the struggle against injustice. Defiance denotes a refusal to acquiesce in the face of the state’s brutal reaction to the insurgency. And finally pleasure in agency refers to “the positive affect associated with self-determination, autonomy, self-esteem, efficacy, and pride that come from the successful assertion of intention” (p. 235). Pleasure in agency was a collective experience, a just pleasure, because the existing societal structure was seen as unjust.

The pleasure in agency, it appears, is where material and non-material rewards intersect: the expected structural change would mean not only political recognition and empowerment, but also an enhanced position from which to negotiate politically with elites – about very material issues such as access to land. Wood readily admits that the demarcation line between the two is a blurred one, “...despite my emphasis on the emotional and moral reasons for acting, aspirations for land did play an important role” (p. 236). But she goes on to stress that land occupation was “a moral and political claim” and not a productive or legal one (p. 236).

While Wood, like Kalyvas, acknowledges the validity and efficacy of the other’s approach, neither attempt to incorporate the others insights in their own theoretical account. In doing so, their work represents a turn away from efforts in the previous

decade to achieve what one might call a “grand unified theory” of conflict research. Rather than evaluating the relative explanatory power of theoretically distinct explanations such as “greed” and “grievance”, both prefer to bracket out elements inconsistent with their analytical framework, maintaining analytical consistency at the expense of explanatory completeness. In different ways both authors provide compelling examples of question-driven research.

Despite the strength of argument within Wood’s text, a number of aspects of the social context of the civil war have been excluded. Wood argues that there was widespread support for the insurgency in her case-study areas, yet she leaves the relationship between insurgent and non-insurgent campesinos largely unexplored. Given the widespread grassroots support, it is likely that non-material rewards could be significant to those who did participate and their kin. While it is a daunting task to disentangle intention and effect, the social rewards for participation are not explored sufficiently in Wood’s rational actor perspective. As such, the account does not connect the micro-foundations of civil war with the macro-level: There is no meso-level analysis of the social contexts provided by neighborhoods, villages, families, etc. Given Wood’s extensive fieldwork, one may assume that a great deal of rich, empirical data has not been incorporated into the analysis.

Furthermore Wood finds, through her interview data, that “the growth of political organizations before the war depended significantly on the ideological work of liberation theology” (p. 119). Liberation theology is accordingly quite central to the motivational forces of participation and defiance, and thus appears to be an important ideational antecedent variable. Like liberation theology, moral and emotional reasons are difficult to understand independent of their wider social context. Wood analyses the historical and the material context, mainly with regard to land issues, but her interpretation of the social universe is incomplete.

This feeds into a related point, the absence of regional context. Would it not be fruitful to conceptualize the Salvadorean uprising against the backdrop of regional ideological currents? These ran through large parts of Central America in the 1970s and 1980s, affecting several of El Salvador’s neighboring countries. An analysis of such factors would have strengthened Wood’s analytic narrative, but regional, political context is almost fully neglected in Wood’s analytic narrative.



## **The Method of Entering Rebellion**

In terms of methodology, Weinstein's book can be divided in two parts: one qualitative small-N and the other a quantitative large-N study. Chapters two through seven investigate the structure and strategies of four rebel movements in three countries, the National Resistance Army (NRA) in Uganda, the Mozambican National Resistance (RENAMO) in Mozambique, and the Shining Path National and the Shining Path Huallaga in Peru, through in-depth case analyses. After first theorizing on five aspects of rebel groups' structure and strategies, Weinstein then underpins his theory by selected empirical examples from the abovementioned cases. The theoretical elements of the book draw inspiration from a myriad of prominent scholars. The empirical evidence is based on Weinstein's own collection of nearly 200 interviews with combatants and civilians related to the four insurgent movements mentioned above. Access to these informants was made possible through national elites and local leaders.

In the latter part of the investigation, chapters eight and nine attempt to expand the argument to a larger set of cases, mainly by using regression analyses. By employing a dependent variable measuring combat related deaths, Weinstein finds that rebels' access to material resources greatly increases the bloodiness of the rebellion. Through a second regression, Weinstein finds that resource wealth increases the number of rebel groups engaged in civil wars, and that the number of rebel groups is negatively correlated to strong states.

By focusing upon the organizational structure of rebel groups, essentially going 'inside rebellion', Weinstein has undoubtedly produced a largely original argument and a fine body of new interview data. Yet in search of methodological variety, the author has failed to overcome a number of notable shortcomings.

More serious than the issues associated with the overly parsimonious theory discussed above, the methodological inadequacies unfortunately detract from the text. Most serious of these is the significant ground attempted to cover by the book. Theorizing on the micro-foundations of rebellion while at the same time trying to create macro-evidence for his argument, Weinstein leaves the reader short of breath in trying to follow the massive implications of his methods. Instead of aiming to uncover the multiple mechanisms linking micro incentives to macro patterns, Weinstein should

have stayed inside rebellion. Addressing the lack of theory on causal links in civil war research is the book's best feature, but rather than actually building a theory, the author goes beyond the micro incentives of rebellion when presenting underdeveloped regression tables. Large-N correlations with causal value can only be potentially identified in regressions posing several potential explanations in one model. The lack of sufficient statistical controls, the use of questionable proxy measures<sup>vi</sup>, and unclear causal direction does not move the argument beyond the four case studies.

With regards to the qualitative research the author's choice of cases further damages the methodological rigor of the text. While it appears the author based selection upon Mill's method of difference, matching cases that were similar in most respects but differing on the dependent variable, little explanation as to why these states are similarly matched is provided. Given the huge variety of state capacity measures that could have been considered this seriously detracts from the transparency of the work. In this sense the reader is left with a lingering suspicion that selection bias is at play, a concern not reduced by the impressive theoretical fit and convenient linguistic overlap between Weinstein and the cases.

A final confusing feature of this book is Weinstein's own labeling of his methods. In the first chapter the author sets out an impressive research design, promising both "micro-comparative" and "ethnographic" work, in addition to application of process tracing. Though there are elements of these techniques and methods in this book, *Inside Rebellion* rests first and foremost on the method of classical in-depth case investigation. Given the relatively short period spent within the field, it is unlikely that Weinstein and his assistants were able to generate the depth of analysis required for the ethnographic method.<sup>vii</sup> Moreover Weinstein appears to have viewed interviewees as observations requiring coding, rather than as informants. Thus rather than seek to uncover a wider focus on the construction of the social reality in which these cases exist, subjects' complete stories were searched for commonalities and differences, a more rigid style of data collection which seems to fall short of the ethnographic threshold of immersion<sup>viii</sup>. Disappointingly, by adopting this approach the author missed an opportunity to generate a large body of meta-data, that may have proved useful in overcoming response bias resulting from lies, omissions and alike<sup>ix</sup>.

Similarly while there are elements of process tracing in Weinstein's book, the lack of sufficient exploration into the existence of alternative intervening causal mechanisms, suggests that it cannot truly be considered to be process tracing at the core.<sup>x</sup> Moreover the ethnographic claims that the author presents do not sit well with process tracing method unless one attempts to bridge different philosophies of social science that underpin these divergent methods. This confusion seems to highlight the great need for a definitive or instructional text on the topic of process tracing, so as to generate a consistent appreciation as to how this method should best be adopted.

All in all *Inside Rebellion* is an excellent example of large-scale field research. As such, it could have been a book presenting brand new theories on the micro-incentives of rebellion – theories that could have paved the way for groundbreaking studies attempting to open the “black box” of causal mechanisms in armed conflict. Yet while methodologically the book retains some impressive features, it eventually comes up short on account of the scale of the task that was adopted. Had Weinstein chosen to focus upon the book's strengths and remove inapplicable labels and underdeveloped statistics, its weaknesses could have been effectively addressed without losing any of the core contributions to knowledge that the author effectively makes.

### **Capturing Dynamics**

In comparison Kalyvas maintains a close match between his analytical assumptions and his methodological choices, with the former consistently driving the latter. In this regard, his work provides an interesting – and highly complementary – counterpoint to that of Wood, who remains similarly disciplined in her alignment of theory and methodology. The multiple methods that Kalyvas adopts – from large-N quantitative, to geospatial representations, comparative historical analysis, archival study and interviews in the field – are each brought to bear on a single theoretically consistent project. The choice and application of methods follow from the questions asked and the epistemological and ontological assumptions employed, and not vice versa.

In addition, Kalyvas frankly acknowledges the methodological difficulties entailed in studying certain phenomena (e.g. civilian support by looking at allegiance). By directly addressing, rather than concealing, the difficulties in linking theory to empirics, Kalyvas opens a dialogue with other scholars and allows readers to consider ways of overcoming such research obstacles. Towards that end, in presenting the

empirical support for his theoretical account, Kalyvas spends a significant amount of time on mis-predictions, villages in which the level and type of violence did not accord with his expectations. The field would be well served if all academics adopted such an openly self-critical approach.

The text is not without issue, however. Methodologically the biggest challenge for Kalyvas is how to conceptualize and measure control. He defines it as the extent to which forces of either side can move freely in a given area, and rates each village along a five-point scale, from sole control by incumbents (1), to sole control by insurgents (5) (p. 421). His primary sources of information on the military movements and patrol schemes include regional court records and military records from Greek, British and German authorities (p. 393). This creates problems on two fronts. First, these sources of data would tend have a great deal of information regarding the incumbents, but presumably do not have as much relevant information on insurgent movements given that the latter are almost by definition clandestine.<sup>xi</sup> Second, the data have been collected by one of the parties to the conflict. Due to the strategic nature of human interactions in a civil war context, it is likely that the insurgents and other social actors worked to conceal their true actions, and tried to feed the incumbent side false information to obtain an advantage. Given the strong constraints on the collection and verification of information on insurgents for the authors of such sources, there is a positive likelihood that the written record includes false and/or misinterpreted data. The use of British records in addition to German and official Greek sources mitigates, but does not fully rectify the problem.

Accordingly, to address such issues Kalyvas goes one step further and employs a method of triangulation, cross-checking the official written sources against local written and oral histories, including an extensive series of interviews he conducted in the relevant regions of Greece. Naturally, there are numerous potential pitfalls in using such data as well – particularly with the passage of considerable time since the events took place – but Kalyvas is admirably sensitive to such considerations (p. 395-411). His 215 interviewees can be classified by political affiliation, which allowed Kalyvas to oversample partisans of the insurgency.<sup>xii</sup> The distribution of interviewees seems to provide Kalyvas with some leverage to correct for biases in his written sources through a stronger representation of insurgent activists among respondents. This presumably provides him with better information on the insurgent's side and

their capacity to exercise military control. Taken alone, the insurgent interviews could have introduced a bias as well, but in combination with the written sources and the small number of interviews with people from the incumbent's side, Kalyvas has found a good way of triangulating his data sources. Admittedly, to the extent that oral and written histories (and the qualitative and quantitative databases constructed from them) incorporate the same sources of bias – whether as a result of shared culture or some other difficult-to-control source of bias – corroboration of the findings of one method by another may actually reinforce or even magnify the ensuing distortion. Kalyvas' solution to this latter problem is to provide numerous collaborative anecdotes from the broader literature illustrating similar results in other contexts. Such evidence is obviously not definitive, but it is likely the only approach possible within the context of a single in-depth case study.

### **Encapsulating Emotions**

Unlike Kalyvas, Wood's single case study does not include comparative material although she believes that her findings are relevant to other cases with the same scope conditions: "where poor people are excluded from social and political participation, where an emerging social movement challenges that exclusion and makes claims on the state and the well-to-do, and where the response of the state is repression rather than accommodation" (xii-xiii). As these conditions were present in many Latin American countries, comparative material from the region may have added causal leverage. If it is true that participation, defiance, and pleasure in agency were the main drivers behind the insurgency, Wood's findings might have been corroborated by including the regional grassroots resistance movements that played such a major part in Central America at the time, presumably creating a sense of collective purpose that was not only larger than the individual, but larger than the individual state as well.

Wood's research is based chiefly on her interview data and maps of the case study areas drawn collaboratively by 12 teams of campesinos; both are triangulated by means of a variety of written sources, from human rights report to household surveys. Of the 200 campesinos interviewed by Wood, only 24 were non-participants in the insurgency. This might be an effect of her 'snowballing' sampling method, which was imperative to obtain access to her interviewees in the context of civil war. But it does introduce an element of selection bias into her findings, which is reflected in her focus

on reasons for participation rather than non-participation. In this sense perhaps her research would have benefited from theoretical sampling for the reasons for non-participation. It provides for a neat argument that non-participants held the old political culture, while participants developed a new political culture, expressed as participation, defiance and pleasure in agency. Yet interview data that shows the absence of such sentiments among non-participants would have strengthened the argument.

In addition to interviews, another important source of data in Wood's book stems from the map-making research. This method involved Wood asking insurgent campesinos to take part in map-making workshops in order to draw maps of their cooperative's property boundaries during the course of the conflict. Both highly innovative and a very apt methodological tool, this provided important insights into the social and ideological aspect of the insurgency. The maps, proud testaments to collective agency and achievements, became "ideological constructions, acts of critical remembrance and redemption as well as an assertion of power to claim and hold land" (p.218).

Having read the formal model in the book's appendix, however, one may be forgiven for doubting that emotions of pride and empowerment, tantalizingly well captured in the map-making exercises, played a part in the insurgency at all. The formal model of the Salvadoran rural insurgency is based on a rational choice model which "...draws on the rational actor approach in that individuals decide whether to participate or not based on anticipated costs and benefits" (p. 267). The model closes the circle and takes the reader back to the beginning, to the collective action problem. But, as Green and Shapiro suggest, perhaps Olsonian collective action theory should be tested in terms of its "claim that collective incentives do not [matter]".<sup>xiii</sup> By focusing on individual (moral and emotional) reasons and cost-benefit considerations, Wood's account of the civil war refines collective action theory yet keeps one foot safely in the rationalist camp.

## **Conclusions**

The rationalist study of civil war has contributed insights into the structural determinants of war, chiefly by inferring causal relationships from statistical correlations between socio-demographic variables and civil war violence. We now

know that “[c]ivil war is more likely to occur in countries that are poor, are subject to negative income shocks, have weak state institutions, have sparsely populated peripheral regions, and those with mountainous terrain.”<sup>xiv</sup> This research method, however, has a number of limitations, which has been recognized by the civil war research community. As argued in this review, this method cannot assist us with distinguishing adequately between causes and symptoms, and tends to conceptualise civil war as “a series of dichotomized possibilities”, driven by external or internal factors, grievance or greed, and rational or irrational actors.”<sup>xv</sup> In the context of these limitations, each of the texts under review represents significant advancements in methodological rigor. The sheer scale and ambition underlying each of the projects is breathtaking, and while issues have been raised with each of the texts, together they deserve the prevalent position they have assumed in the literature. In this sense we can only hope that the methodological diversity and theoretical sophistication exhibited within these texts can be built upon to continue the quest for understanding civil war. In particular, we applaud the explicit and detailed treatment of theory and method found within these works, including the authors’ willingness to openly discuss weaknesses and limitations of their chosen approach. The field would be well-served if all civil war scholars were so candid. Their methodological self-awareness not only enables us to better assess the validity of their findings, it also fosters an open dialogue on ontological and epistemological limitations. If emulated, this approach may promote cross-fertilization between micro- and macro-level analyses of civil wars, creating new opportunities to advance our understanding of this complex social phenomenon.

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<sup>i</sup> G. Clayton paper editor, otherwise equal authorship assumed.

<sup>ii</sup> See in particular, S. Tarrow, 2007, “Inside Insurgencies: Politics and Violence in an Age of Civil War (Book Review Essay).” *Perspectives on Politics* 5(3): 587-600. Other reviews have been provided by M. Biggs, 2007, “Review: The logic of violence in civil war. By Stathis Kalyvas.” *American Journal of Sociology* 113(2): 558-560; J.K. Birnir, 2007. “Book Reviews: Kalyvas, S.N. (2006). *The Logic of Violence in Civil War*.”, *Comparative Political Studies* 40(11): 1395-1398; M.J. Boyle, 2006, “Review: The logic of violence in civil war. By Stathis Kalyvas.” *International Affairs* 82(6): 1167-1168; P. Rozic and P.J. Verovsek, 2008, “Reviews: Stathis Kalyvas: *The Logic of Violence in Civil War*”, *Perspectives* 16(2): 114-116. P. Staniland, 2008 “Explaining Rebel Strategies: A review of *Inside Rebellion: The Politics of Insurgent Violence*” *Review of Politics* 70(1): 129-131; M.E. Allison (2004) “*Insurgent Collective Action and Civil War in El Salvador* (review)” *Latin American Politics and Society*, 46(3): 144-149.

<sup>iii</sup> S. Kalyvas, “Book Reviews: Weinstein, J. M. (2007). *Inside Rebellion: The Politics of Insurgent Violence*,” *Comparative Political Studies* 40 (9): 1146-1151. In his review of Weinstein’s work, Kalyvas flags a number of issues that we also develop in the following pages; however, we differentiate our work from his and other reviews both through our more extensive and detailed

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examination of selected elements of Weinstein's methodology, as well as through our use of a critical comparative approach in evaluating the three books.

<sup>iv</sup> The Course was organized by the Peace Research Institute Oslo and the Technical University of Trondheim (NTNU), For more details on this course see the course webpage, <http://www.prio.no/CSCW/Events/Event/?oid=62214090> accessed 1.2.2011.

<sup>v</sup> D.P. Green and I. Shapiro, 1994. *The Pathologies of Rational Choice Theory: A Critique of Applications in Political Science*. New Haven: Yale University Press, p. 20.

<sup>vi</sup> For example recently a rather solid consensus has emerged that GDP per capita is not a sufficient proxy for state strength, thus this should have been combined with additional measures.

<sup>vii</sup> In addition the choice and method of selection used for the interviews was left rather silent, leaving another source of potential selection bias hanging over the study.

<sup>viii</sup> H. Gusterson, "Ethnographic Research" in A. Klotz and D. Prakash (eds.), 2008. *Qualitative Methods in International Relations: A Pluralist Guide*. New York: Palgrave Macmillan.

<sup>ix</sup> See for example, L. A. Fujii, 2010. "Shades of Truth and Lies: Interpreting Testimonies of War and Violence," *Journal of Peace Research* 47 (March): 231-241.

<sup>x</sup> J. T. Checkel, "Process Tracing" in A. Klotz and D. Prakash (eds.), 2008. *Qualitative Methods in International Relations: A Pluralist Guide*. New York: Palgrave Macmillan.

<sup>xi</sup> Although Kalyvas includes the Archive of the Communist Party of Greece in his sources, he does not mention it among his sources for the evaluation of the military situation.

<sup>xii</sup> Slightly less than one third (69 persons) of them belonged to left-wing organizations, about 10% (21) were associated with the rightists, and more than half seemed to have been without political affiliation (121) (see appendix A). Additionally, four interviewees have changed their position during the conflict and switched sides from the left to the right. Given the course of the Greek Civil War, this last fact alone already provides some indication for his hypothesis that control spawns allegiance and not vice versa, though Kalyvas does not elaborate on these four interviewees in detail.

<sup>xiii</sup> D.P. Green and I. Shapiro, *The Pathologies*, p. 84, emphasis added.

<sup>xiv</sup> C. Blattman and E. Miguel, 2009. 'Civil War', National Bureau of Economic Research Working Paper no. 14801, p. 31.

<sup>xv</sup> C. Dolan, 2006. *Social Torture: The Case of Northern Uganda 1986-2006*. New York, Berghahn Books, p. 4.