

JAD-PbP WORKING PAPER SERIES NO. 4 | AUGUST 2009



Justice or Peace?

A Statistical Study of the Relationship
between Amnesties and Durable Peace

By Erik Melander

Justice or Peace?

A Statistical Study of the Relationship between
Amnesties and Durable Peace

By Erik Melander

JAD-PbP Working Paper No. 4, August 2009.

Previous working papers in JAD-PbP working paper series

Evaluating and Comparing Strategies of Peacebuilding and Transitional Justice, No. 1 May 2009

What will jus post bellum mean? Of new wine and old bottles, No. 2 May 2009.

The EU Peacebuilding Framework: Potentials and Pitfalls in the Western Balkans and the Middle East, No. 3. June 2009.

Editorial Team

Karin Aggestam and Annika Björkdahl,
Lund University

Table of Content

1. Introduction	4
1.1. Background	4
1.2. Previous Research	5
2. Time inconsistency, Regime type and the Credibility of Amnesties.	7
2.1 Hypotheses	8
2.2 Other Determinants of Peace Agreement Success	8
3. Data, Research Design and Methods	10
4. Results	13
5. Discussion and Conclusion	17
Bibliography	18

1. Introduction

This paper examines the statistical relationship between amnesty provisions in peace agreements and the likelihood that a peace agreement will last at least two years.¹ The record of peace agreements since the end of the Cold War shows that the first two years are crucial for the achievement of durable peace in that a peace agreement that holds over the first two years has a 94 per cent chance to avert renewed fighting in later years. I find that the inclusion of amnesty provisions in peace agreements significantly reduces the risk that a peace agreement will fail in the sense that there is a return to fighting within the first two years, but only if the political institutions are authoritarian. In democracies, and in regimes in flux, amnesty provisions have no pacifying effect. Instead peace agreements with amnesty provisions are less likely to last over the crucial two year period if the institutional setting is democratic or in flux than if the setting is an authoritarian regime. The policy implications are clear: if the rationale for amnesty provisions is to strengthen the peace there is no reason to include such provisions in peace agreements negotiated in democracies, although amnesty provisions may benefit peace in authoritarian societies.

In this paper I also report that a widely used measure of political gender equality, the percentage of women in parliament, is positively related to the chance that a peace agreement will survive. Thus, in a setting with more equality between women and men peace agreements are more likely to usher in durable peace. These novel findings are robust to numerous control variables and different estimation techniques.

1.1. Background

The benefits or drawbacks of amnesty provisions in peace agreements negotiated in civil wars is an intensively debated issue. The debate is often framed as peace versus justice, in that amnesties, if they increase the chance for durable peace at all, may do so at the expense of justice. Amnesty often entails that perpetrators of very serious crimes go unpunished, and it is feared that amnesties may contribute to a climate of impunity for such crimes.

Typically the different sides in this debate invoke a small number of prominent cases for empirical support. Careful analysis and comparison of a limited number of cases is undoubtedly very valuable as a heuristic for coming up with new theoretical propositions, for examining causal mechanisms in detail, and for probing limiting conditions to general expectations. In order to generate generalizable findings, however, it is also necessary to examine the overall pattern of association between amnesty provisions and durable peace. To date, no study has been published that uses methods of statistical inference to gauge the relationship between amnesty provisions in peace agreements in civil war and the durability of peace. This paper aims to fill this lacuna.

In what follows I first review the state of the art in the research on the effects of amnesties in the context of civil war. Next I introduce my theoretical arguments and hypotheses. I discuss controls for possibly confounding factors. The subsequent section deals with data, research design and methods. Then I report my results and finally I offer my conclusions.

Acknowledgements: The author would like to thank Ralph Sundberg for excellent assistance with data issues.

¹ About the author: Erik Melander, Associate Professor at the Department of Peace and Conflict Research, Uppsala University, Sweden.

1.2. Previous Research

As far as I am aware, no published study uses methods of statistical inference to assess the effect of amnesties in peace agreements on agreement durability. Snyder and Vinjamuri (Snyder and Vinjamuri 2003) compare thirty-two cases of civil wars that ended between 1989 and 2003, focusing on how trials, truth commissions and amnesties influence durable peace, human rights and democracy. They discuss eleven ended wars in which amnesties were granted either formally or de facto. They do not report statistical tests or stringent controls for alternative explanations but infer that amnesties can benefit both peace and human rights if “accompanied by political reforms that curtail the power of rights abusers, and if they can be effectively enforced.” (Ibid: 34) Amnesties can be particularly useful, they suggest, when dealing with perpetrators that remain strong and thus are potential spoilers. At the same time, they go on to state that “in implementing an amnesty, however, it is important to make sure that perpetrators are removed from office or that the institutional setting of politics is so fundamentally altered that a return to the ways of the past is unfeasible”. (Ibid.: 39)

Snyder and Vinjamuri’s qualification that amnesties require changing the setting so that a return to war and gross human rights abuses becomes unfeasible risks making their thesis about the effectiveness of amnesties tautological. If the institutional setting in a post-conflict society has changed so fundamentally that a return to “the ways of the past” indeed is infeasible, the presence or not of amnesties becomes superfluous.

The need to remove perpetrators from positions of power, on the other hand, seems to contradict their earlier statement that amnesties are particularly useful when perpetrators are undefeated and strong. If the successful implementation of amnesties requires that undefeated and strong perpetrators are deprived of their power amnesties would seem to pave the way to intensified conflict rather than to stabilization.

Whereas Snyder and Vinjamuri criticize arguments against amnesties for lacking “strong empirical foundations” it is hard to discern any strong empirical support for their own propositions about the conditions under which amnesties are effective.

In an unpublished statistical study on the effects of transitional justice measures on peace duration Lie, Binningsbø, and Gates (Lie *et al.* 2007) report preliminary findings indicating that amnesties tend to be associated with shorter peace spells in democracies, but they do not provide a theoretical rationale for why this is so. The authors also underline that their findings are not very robust and to be viewed only as a first cut.

There are also several statistical studies, both published and unpublished, that deal with other aspects of transitional justice than amnesties, such as truth commissions and human rights trials; with other dependent variables than durable peace, typically respect for human rights and level of democracy; and that study other samples of countries, e.g., countries transitioning from authoritarian rule toward democracy. There is a wealth of related in-depth case studies as well as rich descriptive reports and advocacy pieces that raise awareness about the debates and developments concerning transitional justice, and that advance important arguments (Thoms *et al.* 2008). While acknowledging these contributions, Thoms *et al.* in their overview of the field nevertheless identify several points for improvement:

There is little evidence that TJ [transitional justice] produces either beneficial or harmful effects. [...] It is in fact striking that so many commentators have expressed such strong positions on the basis of so little reliable evidence (Ibid.: 4).

Until recently, the field was driven by principles rather than data, concerning itself chiefly with asserting the need for TJ and listing the purported strengths and weaknesses of different TJ mechanisms, rather than with gathering and analyzing detailed impact evidence. [...] With some important exceptions, much of the empirical TJ research to date has been analytically weak, relying largely on impressionistic descriptions of a small number of well-known cases, rather than systematically comparing impacts across a broad range of cases, including societies in which TJ has not been pursued (Ibid.: 5).

The literature has largely avoided structured cross-case comparisons, preferring instead to focus on individual countries. As a result, most of the TJ debate extrapolates inappropriately from a handful of well-documented cases, including Argentina, Chile and South Africa. The TJ knowledge base, in other words, relies heavily on a biased sample (Ibid.: 27).

Proper research design – including careful attention to case election, standardized measurements, and inter-study comparability – is very much needed (Ibid.: 42).

Against this background the present study will make a contribution specifically to the debate about the role of amnesties in the transition from civil war to peace, and to the broader field of transitional justice research.

2. Time inconsistency, Regime type and the Credibility of Amnesties.

In this study I will examine whether peace agreements are durable in the sense that they result in the absence of organized fighting in the two calendar years following the year of the signing of the agreement. I argue that the effect of amnesty provisions in this regard should differ depending on the institutional context. Specifically, amnesties can be expected to work best when a peace agreement is signed in a setting in which the political institutions are authoritarian, whereas amnesty provisions should not have a pacifying effect in democracies and in regimes in flux (regimes in flux encompass regimes in which the political institutions are undergoing a transition and regimes where the political institutions have collapsed). This means that in order to test these expectations I have to test for interaction effects between amnesty provisions and the type of regime.

Coexistence of former enemies within one state typically entails new and increased vulnerabilities for the combatants and faction leaders. Individuals who have survived a conflict by hiding from and fighting their enemy must in a peace process typically disarm, disband units, and leave the relative protection of defensive positions or inaccessible hiding places. If this is to happen the combatants must be really desperate for peace, or trust that their position of increased vulnerability in the future will not be taken advantage of (Walter 1999; Hartzell 1999). The function of amnesties is to contribute to that crucial trust, but the question inevitably arises whether the protection promised in amnesties is credible.

The strongest argument for why amnesties would build trust can be adapted from the theory on the role of costly signalling in the settlement of conflict. Following Fearon (1995; 1992), who in turn draws on bargaining theory developed in economics (Akerlof 1970), Hoddie and Hartzell (2003; 2005) argue that combatants can overcome mutual mistrust, without relying on external third party security guarantees, by exchanging costly signals. Costly signals refers to acts that are costly to the sender in specific ways and thus cannot be lightly dismissed by sceptical observers as cheap talk only. In the process of negotiating and implementing peace, the conflict parties can try to demonstrate their goodwill and trustworthiness by acting in ways that they would be less likely to be willing to do if their commitment to the peace process was not genuine. By incurring on themselves costs associated with moving the peace forward they can strengthen their credibility. The act of signing a peace agreement with amnesty provisions is costly because important parts of the signatories' support bases can be expected to loathe the affording of amnesties to enemies. Amnesties are likely to be viewed as rewarding enemies and as indicating weakness. Although bluff and deception is always possible, an insincere signatory would thus be less likely to agree to amnesty.

While the theory of costly signalling as applied to peace processes has a lot of merit important complications remain to be addressed before a comprehensive theoretical account can be arrived at. A key complication is that although costly signalling may help convince adversaries of present preferences for peace, the willingness to adhere to the terms of peace may change in the future, especially if the balance of power shifts. This is the time inconsistency problem, also often referred to as the commitment problem (2002; Walter 1997; Collier *et al.* 2003). The gist of this problem is that actors often face radically different incentives over time, so that it becomes difficult or impossible to credibly commit to a future course of action, e.g., to honor an amnesty. The leaders of a warring party may be genuinely interested in granting their adversaries a permanent amnesty as part of a peace deal, but may be unable to credibly do so because the adversary will have to take into account that this willingness to respect an amnesty, even if presently genuine, may weaken in the future.

Time inconsistency in this sense is a general problem in peace processes. However, I argue that peace agreements with amnesty provisions are especially vulnerable to time inconsistency. More specifically, because of time inconsistency amnesties should be inherently less credible in democracies, and in societies in which the political institutions are undergoing transition or have collapsed.

I propose two mechanisms that undermine the credibility of amnesties in particular. First, the leadership in authoritarian systems are inherently more stable than the leadership in the other types of regimes (i.e., democracies, political systems in transition, and collapsed political systems). According to Paul Collier, incumbents win only 45% of elections in democracies (Collier 2009). In comparison, authoritarian leaders tend to stay in power the longest time as evidenced by, e.g., Fidel Castro and

Robert Mugabe (Bueno de Mesquita et al. 2003). A change of leadership in the government, or in the former rebel organization, may undermine an agreement involving amnesty because the new leadership may differ in its assessment of the necessity or rightfulness of the amnesty, and may therefore set out to renegotiate or revoke the peace deal, using force if necessary.

Uncertainty and changeability should in general make everything agreed in a peace accord less credible. According to the bargaining theory of war, peace agreements in civil war are likely to hold if the terms of the agreements reflect the balance of power and resolve among former and potential warring parties. An actor perceiving that its relative strength is increasing in a post-conflict setting may demand more favorable terms, something which may threaten the peace if other actors view the balance of power differently and resist the demands. This is why in general changes affecting the balance of power should increase the risk that a peace process breaks down (Werner and Yuen 2005).

There is, however, reason to believe that this is a greater concern when amnesty provisions are involved than when most other aspects of peace agreements are at issue. This is so because amnesties directly concern the personal freedom or even physical survival of the former combatants. The very inclusion of amnesty provisions in a peace agreement indicates that those who negotiated the agreement saw the possibility that justice or revenge would be exacted as a particularly serious prospect.

Peace agreements negotiated in societies in which the regime is in the process of transitioning from one type of political system to another, or in which the government structures have collapsed (e.g., Somalia), should be even more fraught with uncertainty about the future. Amnesties agreed under conditions of regime transition or collapse should therefore entail serious credibility problems.

The second mechanism that undermines the credibility of amnesty provisions in non-authoritarian settings is that amnesties are more likely to be questioned because of a fundamental clash between impunity for war crimes on the one hand and the principles of human rights and the rule of law that undergird democracy on the other. In authoritarian regimes in contrast this type of clash of principles is much less likely to generate doubts about the credibility of amnesties, the reason being that unaccountability and disregard for human rights are part and parcel of most authoritarian systems. Amnesties negotiated with actors who use force and blackmail to further their aspirations is contrary to the working logic of democracy, but in line with the dirty deals through which authoritarian systems typically work.

2.1 Hypotheses

I thus form two hypotheses. The first hypothesis does not specify any interaction with regime type, but the first and the second hypotheses together capture the expected relationship between amnesty, democracy and peace agreement success.

H1: Amnesty provisions increase the chance that a peace agreement will last at least two years.

H2: The pacifying effect of amnesty provisions is weakened in democracies and regimes in flux.

2.2 Other Determinants of Peace Agreement Success

As part of robustness checks, I will include a large number of control variables so as to minimize the risk that any significant associations that turn up in the tests are spurious.

Among the control variables one, namely the percentage of women in parliament as a rough measure of the level of equality between women and men, stands out for several reasons. In the last decade a series of statistical studies have found that higher levels of gender equality in a society are associated with less political violence. This recurring result has been reported for such important phenomena as militarized interstate disputes, (Caprioli 2000; Caprioli and Trumbore 2003; Regan and Paskeviciute 2003), international crisis behavior, intrastate armed conflicts and civil wars (Melander 2005a; Caprioli 2005) and human rights abuse (Melander 2005b). The accumulated, consistent evidence that gender equality, measured in different ways, is negatively related to so many forms of political violence suggests that gender equality may be of fundamental importance for our understanding of political violence in general. Nevertheless these findings have been largely ignored judging from the

number of citations by other scholars using quantitative methods to study these and related phenomena. I am not aware of any study of the durability of peace agreements that includes any controls for the level of gender equality.

If gender equality is related to the types of peace agreements that are negotiated in a society, and to the likelihood that peace will last, our results may be biased. My preliminary tests show that amnesty provisions are less likely to be included in peace agreements in societies with higher levels of gender equality.

Because I suspect that gender equality influences both a key explanatory factor – amnesties – and the outcome of interest, I will pay special attention to gender equality when controlling for spuriousness. To test for the expected pacifying effect of gender equality thereby becomes a secondary purpose of this study, and I thus formulate the secondary hypothesis:

SH1: The greater the female representation in parliament, the more likely it is that a peace agreement will last at least two years.

As far as I know, this study is the first that takes into account the potentially pacifying effect of gender equality in post-conflict societies using methods of statistical inference.

3. Data, Research Design and Methods

When testing these and similar hypotheses great care has to be taken when designing the study and choosing which data to use. First it must be underlined that despite some new datasets on peace agreements and their included provisions there is still a scarcity of data that is sufficiently detailed and contain a large number of observations. In this study I use the UCDP Peace Agreement Dataset from the Uppsala Conflict Data Program, which covers all peace agreements in all intrastate armed conflicts in the period 1989-2005. An important strength of this dataset is that it includes peace agreements also in armed conflicts of a lower intensity and not only agreements in full-scale civil wars. The intensity threshold for inclusion in the UCDP data is that at least 25 deaths are inflicted in battle in one calendar year between a government and a rebel organization challenging the government over issues related to governance or the status of territory. Many previous studies only consider full-scale wars, which produced at least 1000 battle deaths in a year. There are at least three reasons why it is better to include also the conflicts of lower intensity. First, these conflicts are often very destructive although they never result in 1000 battle deaths in any year. It is thus very important to try to understand under which conditions minor armed conflicts can be ended. Second, if durable peace agreements can be negotiated in minor armed conflicts it is likely that the escalation to a full-scale civil war, that would then be included in the other datasets, is prevented in at least some cases. In other words, had it not been for these peace agreements some of the minor conflicts would in all likelihood have grown into very large wars sooner or later. Third, by only studying peace agreements in full-scale civil wars we risk introducing a selection bias. For example, suppose for the sake of argument that power sharing provisions are more likely to be included in peace agreements that are negotiated in very large wars, and that peace agreements in very large wars are generally more likely to fail. If this would be the case we would risk underestimating the potential peace-promoting effect of power-sharing if we do not take into account how effective power-sharing is a means of ending conflicts of a lower intensity.

The other great advantage of the UCDP conflict data is that they are dyadic, which means that if several different rebel outfits are fighting the government over an issue, e.g., how power in the country is to be divided, UCDP tracks the level of activity for each rebel group separately. For example, following the Lomé Agreement in the civil war in Liberia in 1991, the Independent National Patriotic Front of Liberia (INPFL) – a splinter faction from the main rebel group National Patriotic Front of Liberia (NPFL) – largely stuck to the agreement and later dissolved in the following year whereas NPFL under Charles Taylor continued to wage war against the government, his warlord rivals and the West African peacekeeping force ECOMOG. This kind of nuanced information would be lost if the conflict were to be aggregated into, e.g., “Liberia vs. NPFL & ULIMO” as in the Correlates of War dataset. For this paper I am using a version of UCDP Peace Agreement Dataset that has been adapted to the dyadic structure of the UCDP conflict list.

Studying the effects of peace agreements with statistical methods requires several tricky research design choices. First it must be decided what the unit of analysis is. It is common that several different peace agreements are signed over some time in a sequence before a lasting cessation of fighting is obtained. Sometimes such sequential peace agreements build on each other; sometimes a later peace agreement replaces previous agreements. When assessing the effect of agreement provisions it thus becomes necessary to take into account which provisions are in force at any given moment in time. The solution I have chosen is to consider a peace agreement to be ended when a new agreement is signed by the government and the same rebel group. In statistical terms, the peace spell for each agreement is censored when the subsequent agreement is signed. If provision from the prior agreement remain in force and thus carry over to the subsequent agreement I take this into account by adding information about this to the later agreement. It is common that subsequent agreements reaffirm parts of all of the provisions of a previous agreement, but sometimes new agreements reverse what has been agreed previously.

Another reason why agreements should be censored when a new agreement is signed is that the number of observations, or spells of peace following the signing of a peace agreement, would otherwise be inflated over time. Consider for example the set of seven agreements that were signed in the civil war in Guatemala in 1996, from 1996/05/06 to 1996/12/29. The last year of activity in this conflict was 1995 and Guatemala is still at peace, but would it be correct to consider at least seven spells of non-

failed peace agreements to be still in existence today? Given that we are interested in understanding how peace agreements may succeed in ending armed conflict it is more reasonable to view the Guatemalan peace agreements as steps in a peace process, and that there is thus one non-failed spell of peace following the signing of (the latest of) these agreements. The subsequent agreements replace the previous agreements signed by the same set of warring parties, the Government of Guatemala and the URNG in this case, and this is why I censor all but the latest peace agreements in Guatemala in 1996 (but provisions from the earlier agreement carry over to the latest agreement).

Another complication is that the UCDP conflict data (like all other conflict data sets with a global scope) records the level of conflict activity (measured in the number of battle deaths) on the basis of the calendar year; either a conflict dyad is active in a calendar year, i.e., if there were at least 25 battle-related deaths, or it is not active. Thus, we don't know when within the calendar year that the fighting takes place, and thus we do not know how many, if any, deaths occurred before respectively after the signing of an agreement. To assess the success of a peace agreement in bringing about a lasting cessation of fighting we can check whether the following calendar year was active (i.e., the agreement failed) or not (i.e., the peace lasted at least during the first calendar year following the year in which the agreement was signed). We can of course also take into account conflict activity in later years.

This implies that if an agreement is signed in an active year and the conflict dyad is active also in the next calendar year the agreement must be left out of the analysis because we do not know if 25 battle related deaths were sustained in the period from the signing of the first agreement to the signing of the second agreement. That is to say, the first agreement may have been a perfect success in terms of ending all killing, with all the battle activity taking place only after the signing of the second agreement (which may have very different provisions), or the period between the signing of the first and second agreement may in fact include fighting resulting in at least 25 battle related deaths (within one and the same calendar year), thus implying that the first agreement was unsuccessful in warding off renewed warfare.

Thus, since the conflict data in terms of the timing of events is no more fine-grained than the calendar year we cannot distinguish in practice between a peace agreement that holds for one day, and one that lasts almost two years (i.e., if the first agreement is signed early in year 1 and the second agreement is signed late in year 2, and both years are active). However, this is less of a problem than it might seem at first. If we want to understand the causes of durable peace it is hardly relevant whether a peace agreement holds for one month or several months. As pointed out above, any agreement that is followed by renewed fighting between the same set of adversaries within two years is for most practical purposes a failure.

Thus it matters little for the prospects of durable peace in a country whether a peace agreement lasts for a few months more if war will return soon anyway has further implications for the choice of statistical method for analyzing the strength of different types of peace agreement. In my data, 45 eligible agreements ended in renewed fighting within the first or second calendar year after the year in which the agreement was signed. Out of a total of 113 eligible signatory dyads, 68 were not followed by renewed fighting at any point in time. Only 4 dyads returned to fighting at a later stage than the first two years. This means that if a government-rebel group dyad that has signed a peace agreement manages to avoid a resumption of hostilities for the first two years the risk that they will take up arms against each other again at a later point is very small (i.e., 64 out of 68, or 94 percent, of the dyads that kept the peace the first two years were also able to continue to keep the peace in the subsequent years). Conversely, the overwhelming majority of the failures occur within the first two years.

This also means that when using these data most of the variation in the length of the period of time up to an agreement failure is not practically meaningful from the perspective of understanding the conditions for durable peace. What is important is above all if the agreement survives the first two years or not, not if the time to failure is three months or 15. It is precisely this unimportant variation, however, that is likely to drive much of the results when event history or survival analysis methods, such as Cox regression, are used to examine the determinants of the duration of peace agreements. Suppose, for the sake of argument that power-sharing agreements that fail can be expected to do so after some limited amount of time, say between 12 to 18 months from the signing. Perhaps this type of agreement tends to break down in connection with preparations for elections, which typically do not get up to speed for some time. Suppose further that agreements that stipulate separation of forces and disarmament if they fail tend to do so quite soon after the signing, perhaps because insincere signatories who plan to resume fighting cannot afford to go along with such measures. In this constructed example we may find that power-sharing agreements are much more likely to hold than agreements stipulating disarmament. But if both types of agreements are equally likely to fail within a two year period those

conclusions would be less important from a practical perspective, since the results would reflect a variation in the dependent variable which is of little real-world interest.

Against the background of the discussion above I will in this study examine the factors that determine whether a government-rebel group dyad that has signed a peace agreement returns to active conflict or not in the two calendar years that follow the year in which the agreement was signed. The dependent variable is thus a dummy variable taking the value 0 if the peace holds over the first two years and the value 1 if the fighting restarts at any time during the first two years. To analyze the determinants of peace agreement success in this sense I will use cross-tabulations and multiple probit regression.

4. Results

A good way to begin the analysis is with a simple two-by-two cross tabulation between the inclusion of amnesty provisions and peace agreement success (Table 1). This method gives a good overview over how these variables relate to each other and provides results in terms of percentages that are intuitively easy to interpret. Table 1 shows that amnesty provisions in general appear to be positively associated with peace agreement success. Only 26 per cent of the peace agreements with amnesty provisions ended in renewed fighting within two years, whereas the failure rate for the peace agreements without amnesty provisions is 48 per cent. As indicated by the Chi-square value of 5.18, this difference is statistically significant at the .023 level. Table 1 thus provides preliminary support for Hypothesis 1. We can also see that more than a third of all peace agreements include amnesty provisions in some form. It would thus seem that amnesty provisions are a quite important means for promoting durable peace.

Table 1. Amnesty Provisions and Peace Agreement Success

	No Amnesty	Amnesty	
Peace holds at least two years	37 (52.1%)	31 (73.8%)	68 (60.2%)
Renewed fighting	34 (47.9%)	11 (26.2%)	45 (39.8%)
	71 (100%)	42 (100%)	113 (100%)

$$Chi2(1)=5.18 \ Pr=.023$$

However, Table 1 does not speak to the argument that the effect of amnesty provisions can be expected to differ between authoritarian and non-authoritarian settings. Table 2 is a cross-tabulation between amnesty and success that controls for the authoritarian regime type. The left-most sub-table shows the relationship between amnesty and success in authoritarian regimes only. The right-most sub-table, in contrast, illustrates how amnesty provisions relate to peace agreement success if we look at democracies and regimes in flux only. There appears to be a very strong effect of amnesty in the authoritarian settings, but no effect at all in the democracies and regimes in flux. The pacifying effect of amnesty provisions in authoritarian regimes is quite striking; only a single peace agreement that included amnesty failed, which corresponds to just 5 per cent of the peace agreements with this combination of variable values. The presence of a strong, statistically significant pacifying effect of amnesty in authoritarian regimes, and the absence of any significant relationship between amnesty and peace agreement success in democracies and regimes in flux, constitute preliminary support for Hypothesis 2.

Table 2. Amnesty and Peace Agreement Success Given Democracy

		Authoritarian Regimes				Democracies & Regimes in Flux					
		<i>No Amn</i>	<i>Amn</i>			<i>No Amn</i>	<i>Amn</i>				
<i>Peace holds</i>		14 (50.0%)	20 (95.2%)	34 (69.4%)		23 (53.5%)	11 (52.4%)	34 (53.1%)			
<i>Renewed fighting</i>		14 (50.0%)	1 (4.8%)	15 (30.6%)		20 (46.5%)	10 (47.6%)	30 (46.9%)			
		30 (100%)	21 (100%)	49 (100%)			43 (100%)	21 (100%)	64 (100%)		
					<i>Chi2(1)=11.6</i>						
					<i>Pr=.001</i>						
						<i>Chi2(1)=.0069</i>					
						<i>Pr=.934</i>					

Cross-Tabulations become unwieldy as soon as we need to control for more than one variable in addition to the main explanatory variable. Table 3 therefore presents the results of a series of multiple probit regressions with peace agreement success as the dependent variable. The first column headed “Additive Model” reports the results of a model in which there is no interaction term, and the included variables are thus supposed to have an additive effect on the risk of renewed fighting within two years. The Additive Model corresponds to a test of H1 (*Amnesty provisions increase the chance that a peace agreement will last at least two years*) alone, with regime type entered as a control only. The excluded reference category for the regime types is democracies together with regimes in flux. Just like in Table 1, the results in the Additive Model indicate that amnesties have a significant pacifying effect. Also, there is a weakly significant pacifying effect of the authoritarian regime type. In other words, this model suggests that both amnesties and an authoritarian setting are beneficial to durable peace. However, if the model is incorrectly specified that could be misleading conclusions. In the theoretical section I argued that there are reasons to expect that the effect of amnesties is very different in authoritarian institutional settings on the one hand and in democratic regimes and regimes in flux on the other.

Indeed, just as when we moved from Table 1 to Table 2, the interplay between amnesty provisions and regime type comes out when we shift from the Additive Model to the Baseline Model. The Baseline model adds an interaction term between amnesty and the authoritarian regime type, *Amnesty in Authoritarian Regime*, plus two control variables, *Inactive Year* and *Outstanding Issues*. These two control variables are statistically significant and have the expected signs.

Table 3. Probit analysis of determinants of peace agreement success, 1989-2006

	<i>Additive Model</i>	<i>Baseline Model</i>	<i>Gender Model</i>	<i>Pruned Model</i>
Amnesty	-0.571* (0.262)	0.140 (0.390)	-0.514 (0.379)	0.028 (0.329)
Authoritarian Regime	-0.407(*) (0.225)	-0.020 (0.393)	-0.321 (0.432)	0.088 (0.316)
Amnesty in Authoritarian Regime		-1.858* (0.882)	-1.405(*) (0.812)	-1.696* (0.711)
Signed in Active Year		1.432** (0.310)	1.478** (0.356)	
Outstanding Issues		0.725* (0.319)	0.857* (0.342)	
Women in Parliament			-0.301* (0.141)	
Constant	0.107 (0.162)	-1.453** (0.373)	-0.575 (0.418)	-0.088 (0.154)
Observations	113	112	91	113

Robust standard errors in parentheses

(*) significant at 10%; * significant at 5%; ** significant at 1%

Interpreting the results for regressions involving interaction terms is somewhat intricate, especially when the dependent variable is binary, as is the case in this study. The sign, magnitude and significance of the individual coefficients that together capture the interaction effect (*Amnesty*, *Authoritarian Regime*, and *Amnesty in Authoritarian Regime* in this case) tell us nothing in and of themselves about the existence and statistical significance of substantially meaningful interaction effects on the probabilities which we are interested in (the risk of the recurrence of fighting in this study).

I examined the effects in the Baseline Model closer and found that the inclusion of amnesty provisions in peace agreements indeed significantly reduces the risk that a peace agreement will fail if the political institutions are authoritarian, whereas amnesties have no effect in democracies and regimes in flux. Table 4 illustrates the difference that amnesty provisions do for the risk of peace agreement failure in the context of democracies and regimes in flux (the column to the left) and in authoritarian regimes (the column to the right). The predicted probabilities of agreement failure were computed using the program Clarify (King *et al.* 2000; Tomz *et al.* 2003). The predicted risk that a peace agreement will fail goes down from about 74 per cent to about 19 percent if amnesty provisions are included in peace agreements in an authoritarian setting. This very large difference is statistically significant. In contrast, there is no statistically significant difference between the predicted risks whether amnesties are included (0.76) or not (.75) in democracies and regimes in flux. Table 4 also illustrates another noteworthy result. In the Additive Model it appeared that peace agreements negotiated in authoritarian regimes were in general less likely to fail than other peace agreements, but when the interaction between amnesty and regime type is taken into account we see that this pacifying effect of autocracy only exists when amnesties are included. In the absence of amnesties there is no significant difference between the failure risks of authoritarian regimes (.74) and other regimes (0.75).

Table 4. The Effect of Amnesty Provisions Given Regime Type

	<i>Not Authoritarian Regime</i>	<i>Authoritarian Regime</i>
No Amnesty	0.75	0.74
Amnesty	0.76	0.19
Difference	Not significant	-0.55

The shaded cells report predicted probabilities of recurrence of active conflict. Probabilities calculated using Clarify, assuming a peace agreement with outstanding issues signed during a year of active conflict.

When there are no amnesty provisions, peace agreements are equally likely to fail regardless of the institutional setting. But when amnesty provisions are present the authoritarian regimes are much more likely to keep the peace than democracies and regimes in flux. H2 (*The pacifying effect of amnesty provisions is weakened in democracies*) is thus corroborated.

In addition to the statistically significant control variables included in the Baseline Model I added as robustness checks 18 other control variables, one at a time, to the Baseline model, but none of these 18 controls became statistically significant (results on request).

The Gender Model includes a nineteenth additional control variable, the natural log of the percentage women in parliament, and this variable is statistically significant with a pacifying effect, as expected. Thus, the secondary hypothesis (*The greater the female representation in parliament, the more likely it is that a peace agreement will last at least two years.*) is supported as well. This result that peace agreements in a context of higher levels of gender equality are more likely to survive is an important novel finding. The number of observations goes down somewhat in the Gender Model because of missing data, but the results for the other independent variables are largely similar to the Baseline Model. It is important to underline that also the results in the Gender Model are robust to the addition, one at a time, of all the other 18 control variables that were used to gauge the robustness of the Baseline Model.

The Pruned Model, finally, is a bare-bones model that only includes the variables representing amnesty provisions, regime type, and the interaction between amnesty and the authoritarian regime type. The interplay between these variables stands out to most clearly in the Pruned Model. In sum, there is robust support for my second hypothesis across the three models that test for the interplay of amnesties and regime type.

5. Discussion and Conclusion

The statistical results presented above show that the effect of amnesty provisions in peace agreements on agreement durability differ importantly depending on the setting in which the agreement is negotiated, so that amnesties substantially reduce the risk that a peace agreement will end in renewed fighting in authoritarian regimes, but that there is no pacifying impact of amnesties in democracies and regimes in transition or collapse. In the many authoritarian states with peace agreements including amnesty provisions the amnesties seem to work in the sense that the risk that fighting will resume is substantially reduced. This potential benefit must then be weighed against other detrimental effects that amnesty provisions may or may not have.

This study thus contradicts the conclusions that Snyder and Vinjamuri arrived at in their comparative study (2003). Snyder and Vinjamuri hold that amnesties are effective in promoting durable peace only when accompanied by political reforms that effectively rein in powerful human rights abusers. Thus, Snyder and Vinjamuri imply that amnesties should work best in more democratic settings where such reforms and curbing of perpetrator power are the most likely. My findings suggest the opposite, namely that amnesties are the least likely to benefit peace in the more democratic settings. Amnesties are best suited for the most authoritarian states, where perpetrators within government and among rebels can freely enter dirty deals that reflect the balance of power, as necessity and expediency dictate. With the help of amnesties, warlords within and outside governments are more likely to be able to stay at peace, thereby saving them the costs of having to spend their troops and money in confrontation with other armed factions. Whether amnesty provisions in peace agreements are beneficial for the prospects for democracy and human rights in these often severely misruled countries is an important question for future research.

Bibliography

- Akerlof, George A. 1970. "The Market for "Lemons": Quality Uncertainty and the Market Mechanism." *The Quarterly Journal of Economics* 84 (3):488-500.
- Bueno de Mesquita, Bruce, Alastair Smith, Randolph M. Siverson, and James D. Morrow. 2003. *The Logic of Political Survival*. Cambridge, Mass.: MIT Press.
- Caprioli, Mary. 2000. "Gendered Conflict." *Journal of Peace Research* 37 (1):51-68.
- . 2005. "Primed for Violence: The Role of Gender Inequality in Predicting Internal Conflict." *International Studies Quarterly* 49 (2):161-78.
- Caprioli, Mary, and Peter F. Trumbore. 2003. "Identifying 'Rouge' States and Testing their Interstate Conflict Behavior." *European Journal of International Relations* 9 (3):377-406.
- Collier, Paul. 2009. *Wars, Guns, and Votes: Democracy in Dangerous Places*: Harper.
- Collier, Paul, Lani Elliot, Håvard Hegre, Anke Hoefler, Marta Reynal-Querol, and Nicholas Sambanis. 2003. *Breaking the Conflict Trap: Civil War and Development Policy*. Washington: The World Bank and Oxford University Press.
- Fearon, James D. 1992. Threats to Use Force: Costly Signals and Bargaining in International Crises. Ph.D. Dissertation, University of California, Berkeley.
- . 1995. "Rationalist Explanations for War." *International Organization* 49 (3):379-414.
- Hartzell, Caroline. 1999. "Explaining the Stability of Negotiated Settlements to Intrastate Wars." *Journal of Conflict Resolution* 43 (3):3-22.
- Hoddie, Matthew, and Caroline Hartzell. 2003. "Civil War Settlements and the Implementation of Military Power-Sharing Arrangements." *Journal of Peace Research* 40 (3):303-20.
- . 2005. "Signals of Reconciliation: Institution-Building and the Resolution of Civil Wars." *International Studies Review* 7:21-40.
- King, Gary, Michael Tomz, and Jason Wittenberg. 2000. "Making the Most of Statistical Analyses: Improving Interpretation and Presentation." *American Journal of Political Science* 44 (2):347-61.
- Lie, Tove Grete, Helga Malmin Binningsbø, and Scott Gates. 2007. "Post-Conflict Justice and Sustainable Peace." In *Post-Conflict Transitions Working Paper*: World Bank.
- Melander, Erik. 2005a. "Gender Equality and Intrastate Armed Conflict." *International Studies Quarterly* 49 (4):695-714.
- . 2005b. "Political Gender Equality and State Human Rights Abuse." *Journal of Peace Research* 42 (2):149-66.
- Regan, Patrick M., and Aida Paskeviciute. 2003. "Women's Access to Politics and Peaceful States." *Journal of Peace Research* 40 (3):287-302.
- Snyder, Jack, and Leslie Vinjamuri. 2003. "Trials and Errors: Principle and Pragmatism in Strategies of International Justice." *International Security* 28 (3):5-44.
- Thoms, Oskar N., James Ron, and Roland Paris. 2008. "The Effects of Transitional Justice Mechanisms: A Summary of Empirical Research Findings and Implications for Analysts and Practitioners." In *Center for International Policy Studies Working Paper*.
- Tomz, Michael, Jason Wittenberg, and Gary King. 2003. "CLARIFY: Software for Interpreting and Presenting Statistical Results. Version 2.1." Stanford University, University of Wisconsin, and Harvard University.
- Walter, Barbara F. 1997. "The Critical Barrier to Civil War Settlement." *International Organization* 51 (3):335-64.
- . 1999. "Designing Transitions from Civil War: Demobilization, Democratization, and Commitments to Peace." *International Security* 24 (1 (Summer)):127-55.
- . 2002. *Committing to Peace. The Successful Settlement of Civil Wars*. Princeton: Princeton University Press.
- Werner, Suzanne, and Amy Yuen. 2005. "Making and Keeping Peace." *International Organization* 59 (2):261-92.

What is Just and Durable Peace?

The research project Just and Durable Peace by Piece (JAD-PbP 217488) is funded by the Seventh Framework Programme of the European Commission. It aims to shed new theoretical and conceptual light on the *problematique* of building just and durable peace. It examines the effectiveness of general peacebuilding strategies and evaluates to what extent they enhance self-sustainable peace. In addition, it analyses and compares EU's peacebuilding strategies in the Western Balkans and the Middle East. JAD-PbP applies an interdisciplinary approach, drawing on insights in peace and conflict research, international law, political science and international relations in order to make contributions to science, policy-making and the causes of just and durable peace.

The project comprises seven partners: Lund University (coordinator), Bath University, Hebrew University, Jordan Institute of Diplomacy, University of St Andrews, University of East London, Uppsala University.

For more information:

www.justpeace.se

Contact:

Department of Political Science, Lund University, Sweden.

Karin.Aggestam@svet.lu.se

Annika.Bjorkdahl@svet.lu.se

Phone: +46 46 2224923, alt. +46 46 2220162

