

Rebel-Military Integration and Civil War Termination*

Katherine Glassmyer and Nicholas Sambanis
Yale University
Political Science

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Abstract

Civil wars are far less likely to end in peace agreements than are international wars, and more than a third of civil wars restart within a few years. This may be due to the time inconsistency of peace settlements in civil wars: once the rebels demobilize, they lose bargaining power and the government can renege on its promises. This makes rebels reluctant to stop fighting and quick to remobilize for a fight. A self-enforcing agreement could prevent this, but it is difficult to create such agreements. Recent efforts to structure self-enforcing agreements after civil wars have involved the integration of former rebels in a new national army. This solution should make unilateral defection from peace settlements more costly. This is an increasingly popular mechanism used in peace settlements, but it is not yet well-understood. We do not know if it works or under what conditions it is likely to be used. This article provides the first systematic study of rebel-military integration agreements and considers if and how such agreements can help build peace. It also analyzes the conditions under which such agreements will be reached and implemented. The analysis suggests that rebel-military integration has not been an effective peacebuilding mechanism, but this is often due to poor implementation of the agreements.

1. Introduction

Out of 140 civil wars since 1945, just 18% ended with a settlement by the end of 1999. Even with a settlement, the risk that countries emerging from a civil war will go back to war is high. This might be because of the time-inconsistency of peace agreements: once the rebels demobilize, they lose bargaining power and the government can renege on its promises. In the presence of such a problem, rational actors will hesitate to agree to or implement a peace agreement unless they are given credible security guarantees. Guarantees can be provided by third parties, or the rebels and the government can reach a self-enforcing agreement. Self-enforcement usually implies that each party retains some self-defense capability to discourage unilateral defections from the agreement. One way to structure such an agreement may be to integrate the rebels in a new national army. This should reduce the rebels' insecurity vis-à-vis the government.

We label this solution rebel-military integration (MI). MI is an increasingly popular peacebuilding strategy, having been used in almost a third of all peace processes in the 1990s. However we still do not know when MI is likely to occur or if it is effective. This article provides the first systematic analysis of MI.¹ We offer a mainly empirical perspective, looking at data that we collected on MI agreements and trying to distinguish between an economic and a security mechanism through which MI can affect the peace. We find that MI often fails to

¹ The only other relevant study is Hoddie and Hartzell (2003). They look at 16 cases of MI and argue that successful implementation of MI within 5 years of the war's end increases the prospects for lasting peace. They do not consider that peace failure may be the reason for failure to implement MI, or that the greatest risk of peace failure occurs in the first few years after the war, so the peace is likely to last five years after the war independent of MI. They look only at cases of negotiated settlement from 1980 to 1996, but MI has also taken place after military victories. They also do not analyze the impact of MI on peace by controlling for other relevant variables while comparing cases with and without MI. Walter (2002) briefly discusses 'military pacts', but does not analyze the effects of MI.

provide credible security guarantees and that it serves mostly as an economic strategy (providing employment to rebels). We also find that MI may be more effective if it is combined with political power-sharing, but this evidence is only suggestive.

The article is organized in four sections beyond the introduction. In the next section, we discuss three hypotheses on the relationship between MI and peacebuilding. The following section tests those hypotheses and identifies conditions leading to the signing and implementation of MI. We complement the statistical analysis with illustrative examples from actual cases of MI to gain insights into the mechanisms through which MI affects the peace. The last section concludes with a discussion of the theoretical and policy implications of this article.

Hypotheses about the Impact of MI on War Resumption

Civil wars are difficult to end partly because the parties have incompatible preferences, partly because war is profitable for some groups, partly because of imperfect information about the parties' relative resolve or capabilities, and partly because of the parties' inability to reach a credible commitment to end the violence. We focus on the commitment problem and draw on literature on the commitment problem to think about the ways in which MI can affect the peace (see Fearon, 2004; Fearon, 1995; Walter, 2002).

The commitment problem is due to the fact that the government, which retains control of the military after the war, can renege on its promises and punish the rebels after they disarm. MI could in principle mitigate this problem by giving rebels some control over the state's coercive capacity. By joining a new national military, former combatants can mutually deter violations of the peace. This security mechanism is based on the higher costs of mutual defection with MI.

MI, however, can also work through an economic mechanism: by offering rebels employment, it creates economic disincentives for war (see United Nations, 2000).

The security and economic mechanisms should have the same effect:

H1: Signing and implementing an MI agreement should reduce the risk of peace failure.

MI is a critical component of security sector reform (SIDDR, 2005) and MI provides a costly signal of the parties' commitment to the peace. Implemented MI is a costlier signal than simply declaring intent to integrate rebels in the military. But even signing an MI agreement may send a positive signal, compared to cases where the parties cannot agree to stop fighting. But failure to implement MI could be both a cause and a symptom of broader peace failure: if the parties intend to return to war, then the need for MI is greater, but so is the risk of failure to implement MI agreements.² Thus, we argue that:

H2: Successful implementation of MI should be less likely in difficult peacebuilding ecologies (where postwar hostility is high and local capacities for development are low).

Hypothesis two prompts us to consider the interactive effects of MI. We expect MI to be more effective where there is assistance from consent-based UN peace missions, if a peace treaty has been signed, or if other conditions are conducive to peace. We also consider the interaction between MI and political power-sharing, which is another peacebuilding strategy that is often used to resolve distributional problems in peace negotiations. While MI may be needed less if other solutions are possible, it may be more effective if used together with power-sharing, since the two strategies together can offer mutually reinforcing assurances.³ There is no consensus on the usefulness of power-sharing in the literature and no large-N quantitative study has been able

² This implies a selection effect that we consider in the empirical section.

³ On power-sharing, see Roeder and Rothchild (2005).

to estimate the effects of power-sharing while accounting for the obvious selection problems. Thus we do not focus on power-sharing, but use it only as a control.

So far we have argued that an implication of the security mechanism is that MI may occur less frequently in challenging cases. We can test this empirically by looking at the correlation between MI and other determinants of the peace. In doing so, we can explore the selection effect (that MI may be seen more frequently in ‘easy’ cases) and we can also learn more about the mechanisms through which MI affects the peace.

We can sort out the security from the economic mechanisms by looking at the link between per capita income as a measure of economic opportunity (Collier & Hoeffler, 2004) and MI. If MI occurs more frequently in richer countries, this would be inconsistent with an economic mechanism and more consistent with the security mechanism. Higher income may proxy for greater state capacity (Fearon & Laitin, 2003). If stronger states are better at counter-insurgency and can more easily repress their citizens, then demand for MI in richer countries would be more consistent with the security mechanism.⁴ Thus, we look at the correlation between per capita income and MI to assess the plausibility of the economic mechanism and we also discuss a number of cases to sort out the security and economic mechanisms by evaluating their observable implications:

H3a: If MI works by offering a security guarantee, then it should not be used more frequently in countries with fewer economic opportunities; and it should be used more often in countries with high levels of hostility.

⁴ If states with greater capacity are better able to control actors, then state promises to implement MI should also be more credible, which would make it more likely to see MI in countries with higher income. In low-income countries, the credibility argument would work together with the economic argument: since MI should be less credible in low-income, low-capacity countries, we should see less demand for MI for security reasons. More MI in those cases is more likely to be associated with the economic benefits it provides than with credible security guarantees.

H3b: If MI works by offering economic incentives, it should be used more frequently in countries with lower per capita income.

Empirical Analysis

To analyze the effects of MI agreements, we use the Doyle and Sambanis (2006) dataset that includes all civil wars from 1945 to 1999, adding our data on MI agreements. MI is a binary variable (*milint*), coded 1 if there was an agreement to integrate all or some of the rebels into the national military. Although only 27 wars ended with a negotiated settlement, there have been 34 cases of MI since 1945. Most cases are in Sub-Saharan Africa and 23 cases took place in the 1990s. In 22 cases, MI was ‘strict’ and was done on the basis of a formal agreement on how many combatants will be integrated and how; in the other cases (‘lenient’) MI was done with no clear plan and rebels were simply invited to join or the government restructured the military to include previously excluded minority groups. Out of 34 MI agreements, 23 were implemented.

Estimates of the Short-Term Effects of MI on the Risk of Peace Failure

A quick glance at the data suggests that MI does not lead to peace. Our dependent variable is *no war recurrence*: it is a binary variable coded 1 if there was no war resumption within two years of the previous war and 0 otherwise.⁵ We regress it on MI and other variables using logistic regression (Table I). A bivariate regression of *no war recurrence* on *milint*

⁵ Distinguishing between war recurrence and the start of a new, different, war is difficult and requires judgment calls. Moreover, there is often uncertainty about the precise timing of war starts and ends. We therefore check the robustness of our results by using a different list of ‘recurrent’ wars in the supplement, where we also report results for war recurrence up to a 5-year period (there are no significant differences). See our dataset notes for information on how we handle the coding of some ambiguous cases. We conduct a similar robustness test in our longer-term duration analysis (see supplement and dataset notes for details).

(Regression 1) shows a weakly significant *negative* relationship: peace is more likely to fail where there was MI.⁶ This result is driven by incomplete MI agreements (see supplement) and by lenient MI (Regression 2), since strict MI is negative though non-significant with a much smaller coefficient (Regression 3). Controlling for both forms of MI (Regression 4) confirms this result. Poorly structuring MI agreements increases the risks of peace failure (though this result may also be due to a selection effect that we discuss later). Implemented MI agreements are positively associated with peace, but the estimate is not significant (Regression 5).

[Insert Table I about here]

Based on these results, we could reject Hypothesis 1. But to fully evaluate the effects of MI, we also need to control for other factors that are likely to influence the risk of war recurrence since some of them may also be correlated with MI. We use as controls all the variables in the Doyle and Sambanis (2006) model of peacebuilding. We focus only on the results with respect to MI and briefly describe the controls below.

The Doyle and Sambanis model is based on an argument that the space for peace is determined by the level of post-war hostility, local capacities for reconstruction, and international capacities – i.e. external assistance to design and implement a peace settlement. They measure international capacities by the presence and mandate of *UN peace operations* and *non-UN peacekeeping*. Both should make a return to war less likely. They also control for *international economic transfers per capita* to the war-affected state. Higher transfers should reduce the risk of war recurrence.

⁶ Since our dependent variable (*warend2*) is coded 1 if *no war recurrence*, a negative coefficient for MI indicates a negative correlation with peacebuilding success.

Hostility is measured by the *deaths and displacements* (in logs) due to the war; the *number of factions*, the signing of a *peace treaty*, the *type of war* (ethno-religious or not?), and the level of *ethnic fractionalization*. Higher hostility should make reconciliation harder. More factions, no treaty, bloodier wars with many displaced persons, ethno-religious conflicts and high levels of fractionalization should increase hostility levels (Doyle & Sambanis, 2006). We also control for *War duration*, which has an ambiguous effect: long wars can make signing a peace agreement more likely as victory seems unlikely, but they can also make peacebuilding harder if longer wars also result in greater hostility and more damage.

Local capacities can be measured with several variables: the *level of per capita GDP* or by the *rate of growth of per capita GDP* immediately after the war's end.⁷ Greater capacities (in terms of wealth or infrastructure or poverty levels) should facilitate the process of rebuilding after civil war. We also measure countries' resource-dependence: less diversified economies are less well-prepared to respond to the challenges of reconstruction and an abundance of natural resources may create predatory incentives that can increase the risk of a fallback to war (see Ross, 2005). We measure resource dependence with *primary commodity exports as a percent of GDP*.

In regression 6, we estimate the effects of MI (all cases) adding these controls from the Doyle and Sambanis model and see that MI still has a negative, non-significant effect (the coefficient drops dramatically once we control for other factors).⁸ The model as a whole does not explain short-term war recurrence well and only local capacities seem to matter. These results lead us to reject hypothesis 1.

⁷ Doyle and Sambanis (2000, 2006) also use *energy consumption* to measure of local capacities. Income is more directly related to the economic mechanisms underlying our discussion of MI.

⁸ Interacting MI with all the variables in the model does not change the results and the interactions are jointly non-significant (see supplement).

We have argued that MI might be more effective if it is used in the context of a treaty, since treaties signal an interest in peace and enable international involvement. MI interacted with treaties does not have a significant effect (Regression 7, Table 1), nor is the interaction between MI and UN missions significant (Regression 8, Table 1). There is some indication that MI may have a more positive impact if there are treaties, since if regress our *no war recurrence* variable on MI on the sub-sample without a treaty (81 cases, see supplement), MI has a negative coefficient, whereas it is positive (yet non-significant) if there was a treaty. Similarly, in the 93 cases with no UN mission, MI has a negative and borderline significant coefficient, whereas the relationship is not significant if there was a UN mission (see supplement).

The case evidence is also consistent with an argument that impartial peacekeeping can help improve MI by reducing mutual suspicion during implementation. The best example is Mozambique, where the UN mission (UNOMOZ) managed demobilization and integration, often despite claims by both sides that the other was cheating (Synge, 1997). External involvement takes many forms. In Mali, the UNDP managed demobilization and set up the ‘Trust Fund for North Mali.’⁹ In Namibia, Zimbabwe, and South Africa, third parties helped the transition away from white rule by integrating non-whites in the military (Mills, 1992; Rupiah, 1996). In Tajikistan, the UN together with the ‘Collective Peacekeeping Forces in Tajikistan’ (‘CIS/PKF’) accompanied rebel units from their posts in Afghanistan to the assembly areas where they would await demobilization.

MI is often used in conjunction with political power-sharing agreements. By power-sharing, we mean any formal arrangement for the distribution of political positions (cabinet posts, legislative seats, etc.), but also sharp departures from previously exclusionary systems if

⁹ The Government contributed \$1 million to the Fund—a huge amount for that country—thereby affirming its commitment to the peace (UNDP, 1998; van Tongeren, 1999).

the new system formally includes minority groups and allows groups associated with the rebels to participate in elections if previously this was not allowed. In Regression 9, we interact MI with a binary variable identifying power-sharing cases. The interaction term is positive and weakly significant. But all the work is done by power-sharing, which is individually significant with a large effect and adding it to the model improves the effect of other hostility variables, slightly reducing the effect of income (Regression 10).

These estimates, however, cannot be interpreted as indicative of a causal connection since we argued earlier that we cannot consider power-sharing as an exogenous variable. So, we looked at some cases for illustrative evidence on the ways in which power-sharing might work together with MI. In some peacebuilding success stories, the rebel army transformed itself into a political party and engaged in non-violent political competition, while also being integrated into the military or police (El Salvador, Mozambique, Djibouti) (Cañas & Dada, 1999; Rupiya, 1998; Alden, 2001; Keesing's, 1995 & 1996). But in other cases, political competition led to more instability (Angola, Rwanda, and Chad) (Anstee, 1996; Synge, 1997: 30-31; Jones, 1999; Amnesty International, 2001). Power-sharing and MI seemed to be mutually reinforcing in several cases, as in Tajikistan and Uganda, where commanding officers from both sides were heavily involved in the design of the peace process (Abdullo, 2001; Lamwaka, 2002). In Mali, high-ranking Tuareg officers were integrated both in the high command of the army, but were also appointed to key non-military government positions (Van Tongeren, 1999). In cases like Bangladesh or Papua New Guinea, where power-sharing amounted to increased regional autonomy for the rebels, there should be less of a need for MI, if the autonomous regions are given the authority to self-police (Keesing's, 1997). In most of the cases that we reviewed, MI preceded and supported political reforms, including power-sharing.

Effects of MI on Long-Term Peace

We can use the same model to look at the longer-term effects of MI on the peace. The dependent variable is now months at peace from the end of the war until peace fails or until the end of December 1999. Each observation represents a single record of a peace spell (a peace process).¹⁰ Out of 138 peace processes, we have 73 failures and the last observed failure occurs at 634 months. We do not allow failures at time $t = 0$, but there are several cases where the peace fails within a month of the end of the previous war (e.g. Afghanistan, Colombia, Georgia). The median peace duration is 61 months.¹¹ We analyze these data using a Weibull model (there is evidence that the risk of peace failure declines over time, which justifies the use of a Weibull model). We present results in Table II.¹²

We find that MI agreements do not have a significant effect on the risk of peace failure (Regression 1). Thus, hypothesis 1 is also rejected for the long term. Local capacity variables play a major role in sustaining the peace after civil war – *per capita income* and *income growth* both lower the risk of peace failure, while large levels of natural resource dependence (proxied by *primary commodity exports*) increase that risk.¹³ *Deaths and displacements* also increase the

¹⁰ A ‘spell’ is a period in which all the covariates are assumed to be fixed. Some variables do change values over time and we have multiple events in some countries. So we re-estimate the model with interactions with (log of) duration time (supplement). Results on MI do not change.

¹¹ MI does not make a difference for peace duration. Among wars that ended in the 1990s, the average peace duration with MI is 42 months (22 observations) and without MI it is 41 months (37 observations). (These figures do not correct for censoring.)

¹² Survival analysis such as Weibull regression accounts for right-censoring (the problem that the peace may fail after the end of analysis time, in our case December 1999).

¹³ Table II presents estimates of hazard rates. An estimate above 1 indicates a positive coefficient whereas an estimate under 1 indicates a negative coefficient, decreasing the risk of peace failure (equivalent to a positive coefficient in the logit regressions in Table I).

risk of peace failure as does a high level of *ethnic fractionalization*, while consent-based UN missions have a weakly significant positive effect on the peace.¹⁴

The results on MI are qualitatively the same if we control only for ‘strict’ MI (Regression 2, though now the hazard rate is above one) or only for implemented MI (Regression 3). In Regression 4, we also control for cases that ended in a military victory since MI may be less likely in those cases and the outcome of the war could affect peace duration. Adding this control does not change the results on MI and does not diminish the strong effects of local capacities.

Interacting MI with the signing of a peace treaty, we find a weakly significant effect, reducing the risk of peace failure (Regression 5). Results on other variables are qualitatively unaffected, except for the UN variable.¹⁵ In one out of every two cases of MI with a treaty, there was a UN mission that frequently helped sign and implement the MI agreement. But the interaction between consent-based UN missions and MI is not significant (Regression 6) so even if we drop MI cases that happened outside the context of a UN mission, MI still does not have a significant effect. Power-sharing and MI interacted have a strong effect (Regression 7), but again power-sharing does the work and MI alone has a negative effect.

This analysis suggests that we can reject our first hypothesis even in the long-run. But there is weak support for the hypothesis that MI can have a positive effect if it is combined with a peace treaty or, in the short run, if it takes place in the context of a UN peace mission.

[Insert Table II about here]

¹⁴ We use UN operations as controls. We do not focus on their effects since some of that effect on peace outcomes may occur through the MI variable as UN missions often help design and implement MI agreements. The same applies to all covariates that may affect the dependent variable partly through their influence on MI.

¹⁵ MI with treaty and UN missions are jointly significant ($p = 0.012$).

This quantitative analysis can tell us that MI agreements usually do not work, but we still do not know exactly why. A look at the cases suggests some possible explanations. One thing that becomes immediately clear is that MI is a messy process. Only in a few cases (Mozambique, Angola, Rwanda) were there detailed guidelines on how many government and rebel soldiers to integrate in a new national army and how. Often MI amounts to the government simply allowing or encouraging the rebels to join the national army. In some cases (e.g., South Africa, Uganda in 1992), the government could absorb as many rebels as were willing and able to join, but in other cases, the army was down-sized and this created limits on the scope of MI.¹⁶ Consistent with the statistical evidence from Table I, we found clear case evidence that poorly structured and incomplete MI agreements are associated with peacebuilding failure. In Cambodia, restructuring the military took the form of ‘dualization’ rather than integration. An awkward government coalition emerged between Prince Ranariddh and Hun Sen (leader of the Vietnamese-installed party CCP). The two leaders each had their own bureaucracy, police, ministries, and military. Hun Sen’s power grew stronger and in 1997 he used ‘his’ half of the national army to depose his governing partner (Curtis, 1998).¹⁷ All but one case of MI that was not implemented coincided with a return to war.¹⁸ This may also be due to a selection effect that we explore in the next section. The cases also clearly show that there is wide variation in the

¹⁶ Lebanon is another example (Makdisi & Sadaka, 2005).

¹⁷ Other examples include Angola in 1991, where large numbers of rebels were not integrated (Anstee, 1996; Synge, 1997: 30-31); and Chad in the 1980s and 1990s, where only select rebel groups were integrated (Amnesty, 2001).

¹⁸ The exception is Laos, an unusual case of MI after military victory. Low-level violence persisted for years and, although MI was unimplemented, police integration was carried out and the two armies formed a joint command for the protection of the capital city (Fox Butterfield, “Laos Still Beset by International Rivalries” New York Times, December 8, 1975).

scope and design of MI and this implies that not all countries receive the same treatment. This makes it difficult to evaluate the effects of ‘properly’ designed and implemented MI.

Overall, the cases suggest that overcoming mistrust is very difficult and that constant reassurance is needed throughout a peace process. MI could help build trust, but integration must be deep and well-structured for this to happen. A good example is the case of Tajikistan, where the armies were integrated unit by unit into the national army, there was no within-unit integration, and planning for MI involved the military command from both sides, which helped erode the mistrust that had been built up during the war (Abdullo, 2001). If poorly structured or incomplete MI is associated with increased risk of peace failure, we should consider why some MI agreements are not implemented. We cannot sort this question out completely in our statistical analysis, so we looked at several cases to see if causality runs from MI failure to peace failure or the other way around. The policy debates surrounding the timing and implementation delays of MI agreements are relevant. Delays create suspicion and several authors have argued that implementation delays can cause a return to violence.¹⁹ But more frequently, implementation delays are a symptom of a failing peace process rather than a cause of it. In Sierra Leone, the MI specified under the Abidjan agreement of 1996 was never implemented, but the ceasefire leading up to Abidjan had broken down by the time the ink on the peace accord was dry. Angola in 1994 is another example. MI was an integral part of the peace process, with a plan to create a new national army split evenly between rebels and government troops. With an inflow of foreign assistance and 7,000 UN peacekeepers, the process could move fast, but at least fifteen thousand of UNITA’s troops were never demobilized and this undermined the entire demobilization process in December 1995. Savimbi stalled at each turn of the peace process and

¹⁹ The most frequently cited example is the peace failure in Angola after the Bicesse accords of 1991. This example motivated rapid DDR in Mozambique, as explained by Synge (1997: 30).

by mid-1997 only a small percentage of the FALA had been integrated. Fighting recommenced in 1998. UNITA demonstrated a general lack of commitment to the peace and this was not due to the failure of MI. The cases that we reviewed do not point to a clear causal chain: sometimes failure to implement MI can lead to peace failure, but other times it is a general deterioration to the peacebuilding environment that explains MI failure.

Where Are We More Likely to See MI?

The question of the determinants of MI implementation is directly related to the selection problem that we mentioned earlier: if MI is needed more in difficult peacebuilding ecologies and MI agreements are less likely to be implemented in difficult cases, then we will observe implemented MI in cases with an *ex ante* higher chance of peacebuilding success. This means that empirical estimates of the effects of implemented MI could be higher than the true effects.

We tried to account for possible selection effects in various ways, though it may not be possible to deal with this problem completely satisfactorily using our data. So we can think about the likely direction of any bias in our estimates and then look at cases for clues on the nature of the selection problem. Our second hypothesis speaks directly to this problem, since we argue that implemented MI will be less likely in relatively easier peacebuilding ecologies. But the selection effect may also apply to the signing of MI agreement (not just to implementation). If MI agreements are likely to be signed in tougher peacebuilding environments, then the direction of bias could work in the opposite direction, leading us to underestimate the effects of MI.

One way to account for the selection effect is to estimate a bivariate probit model of war recurrence with *signed peace treaty* as the dependent variable in the selection equation (cases

with no treaty indicate a harder peacebuilding environment).²⁰ The results on MI do not change substantively and we cannot reject the null hypothesis that the *no war recurrence* equation and the *signed peace treaty* equation are independent (see supplement).²¹ Since unobserved determinants of MI may also affect the risk of a return to war and we may have omitted those from the war recurrence equation, the proper way to estimate the effects of MI on war recurrence would be through instrumental variables regression. But here our data limit our ability to resolve this problem as no instrumental variable that we could think of from our dataset clearly satisfies the exclusion restrictions required for IV regression. We therefore leave this question open for further analysis.²²

So far we have been addressing the problem of selection on omitted variables. But our discussion of the selection effect suggests that there may be selection on observables—that is, other variables in the model may influence the likelihood of signing or implementing an MI agreement or the effect of MI agreements may be different in different ranges of the other covariates. Thus, we estimated a propensity score matching model, which accounts for selection on observables and estimates treatment effects on common support in the distributions of the treatment and control group. The average treatment effects of MI were slightly negative or near zero and never statistically significant (see supplement).²³

²⁰ We also estimated this dropping coups and partitions since they have lower ‘exposure risk’ to MI. In coups, the ‘rebels’ are typically already part of the military, so MI does not apply, as is the case with partitions (each side has its military). The results did not change.

²¹ We also estimated a model selecting on war outcome since MI agreements are less likely after military victory, but this did not affect the results for MI.

²² In the supplement, we estimate an instrumental variables model using the country’s geographical region and the decade the peace started as instruments for MI since there are regional and time differences in the use of MI but not in war recurrence patterns. But these are imperfect instruments, so there is still possible bias in the estimates we have presented.

²³ Propensity score matching was only used as a robustness check. There are possible problems with the application of that method to our data. One issue is that this method assumes that there

The process of dealing with the selection issue involves estimating models in which MI is placed on the left-hand-side of the war recurrence model and this allows us to see if other explanatory variables in the model are significantly correlated with MI. In Table III, we present regressions of MI on the variables included in the peacebuilding model. Income, military victories, and, especially, peace treaties have a significant association with MI (Regression 1).²⁴ About half of all treaties have included provisions for some form of MI, and fewer than 20% of civil wars with no treaty have led to MI. The fact that MI is more likely to occur in countries with lower per capita incomes helps us assess the economic opportunity mechanism that we discussed earlier and it lends support to hypothesis 3b. In Regression 2, we add a dummy variable for Sub-Saharan countries because that is where most cases of MI have taken place. Adding this control makes income non-significant and, since average per capita income is lower in African wars as compared to the rest, this is additional evidence that MI serves an economic function primarily and partly explains why MI is most often used in African cases.

[Insert Table III about here]

Postwar hostility measures are not significant determinants of MI (see results on deaths, displacements, factions, and war type), which would lead us to reject hypothesis 2 about the

are no significant omitted variables from the model. A related issue is that matching on the propensity score assumes that we have a good model of the assignment of the treatment (in this case MI), but this is rarely true in most practical applications. Thus, the equation used to estimate the propensity score could be mis-specified. There can be other sources of sensitivity of matching estimates. For a discussion with an application to the study of the effects of UN peacekeeping, see Sambanis and Doyle (2006: 23-36).

²⁴ The fact that income and treaties are correlated with MI should not affect our analysis substantively, since we control for them (see Tables I and II) and since the selection models that we estimated (propensity score matching and bivariate probit) should capture the fact that MI is more likely to occur in poor countries and after treaties.

association between peacebuilding ecology and the signing of MI. These results also weaken hypothesis 3a (that MI is designed to offer security guarantees) and are more consistent with the economic mechanism (hypotheses 3b). These results are not sensitive to dropping all coups and cases of partition, though we lose many cases and the effect of income drops and becomes more uncertain without them (see supplement). UN and other peacekeeping missions (see supplement for results on non-UN peace operations) are not significant determinants of the signing of MI agreements. Given that we expect that reassurances offered by peacekeepers should help the MI process, and given that we found earlier some weak evidence in support of this expectation, the fact that MI agreements are often signed outside the context of a UN peace mission might help explain why they are not effective.

Finally, in regression 3 we control only for implemented MI and see a slightly different picture as we find that MI is more likely to be implemented if *income growth* is high and it is less likely to be implemented if *resource-dependence* is high (the results are the same without coups and partitions, as we show in the supplement). What we find is not consistent with the security mechanism since MI seems to be implemented where the underlying risk of war is lower (higher growth and lower resource dependence reduce the risk of civil war).

Our results show that high hostility does not prevent the implementation of MI, but economic concerns are important and, consistent with hypothesis 2, we find that MI implementation less likely in difficult peacebuilding ecologies when those ecologies are defined in terms of local capacity variables (e.g. in cases with low per capita income). Hypothesis 2 can be rejected with respect to hostility variables, however, since we found no clear link between hostility variables and MI implementation.

Another way to gauge at the relationship between the underlying peacebuilding ecology and the likelihood of signing and implementing an MI agreement is to estimate the probability of peacebuilding success without the MI variable and see if cases where there was MI have on average a higher underlying chance of peacebuilding success. In fact, the probability of peace (no return to war) is significantly higher in cases where there was no signed MI agreement (see supplement).²⁵ This is consistent with the expectation that MI is used in overall harder cases.²⁶ But there is less support for hypothesis 2, that implementation is less likely in harder cases (see supplement).

We also turn to the cases to further explore the distinction between the security and economic mechanisms by analyzing different observable implications of each mechanism. A premise of the security mechanism is that MI security guarantees are credible. Thus, if parties sign MI agreements under conditions that violate that premise, this could be interpreted as evidence contrary to the security mechanism. But MI cannot offer credible security guarantees if there are severe imbalances in the numbers of rebels and government soldiers that are integrated in a new national army since large power asymmetries imply that the stronger side can easily unilaterally defect from the agreements. Thus, if the cases show evidence that MI is used despite large power asymmetries, then this would be evidence more consistent with the economic mechanism than the security mechanism.²⁷

²⁵ The mean probability of no war recurrence is also lower in cases with implemented MI, but the difference from cases with no MI is not statistically significant.

²⁶ Thus, the effects of MI might be higher than those in Table 1 if MI agreements were randomly assigned. Matching estimates should be able to account at least partially for this selection effect (they offer no evidence of a significant positive effect for signed MI). But the problem is not fully resolved if there is selection on unobserved variables and future research could explore this further based on a better model of the signing of MI agreements.

²⁷ Military balance should also take into account the relative sizes of the (ethnic) groups. In Rwanda, the Arusha Accords created a disproportionate distribution of power as the infantry was

In fact, MI rarely results in the integration of equal numbers of rebels and government soldiers, though in some cases, the army is expanded substantially and absorbs many rebels. In Uganda, Museveni's NRA expanded from 15,000 in the late 1980s to over 100,000 by 1992 as it absorbed fighters from other groups (Kiyaga-Nsubuga, 1999: 20). In many other cases, rebels agreed to integrate even when the government forces would far outnumber them, as in South Africa where the former government army constituted nearly 80% of the new army.

Cases of MI after military victory also do not fit the security mechanism. Perhaps the best example is the Chinese Red Army's integration of Tibetan rebels after the 1951 war, which was followed with an agreement to increase Tibet's regional autonomy. Tibetan units were kept ethnically homogeneous, but the Chinese later expended them on the frontlines of the Korean war. In Nigeria after the Biafran war (1967-1970), Biafran rebels were reintegrated in the army from which they had broken away to start their rebellion. Their military defeat clearly implied that integration would not provide them with a security guarantee and could only be seen as a political or economic strategy by the government to foster stability by restoring those men to their jobs. After military mutinies in the Central African Republic, rebel leader Lt. Parfait Mbaye insisted that his men be permitted to return to barracks rather than be demobilized. The two hundred defeated mutineers did not pose a significant security threat to the government, so reintegrating them in the army was not intended to resolve a security dilemma, but was rather a low-cost way to decrease the rebels' incentives for continued fighting. In most cases that we reviewed, economic incentives seem to drive the processes. This was evident even in cases

to be drawn 40% from the rebel Tutsi army (RPF) and 60% from the governing Hutu army (FAR) and the chain of command was to be drawn evenly from the two groups. The Hutu FAR outnumbered the Tutsi RPF two to one and this made the deal difficult for the Hutu to accept (Jones, 1999). In Angola, the 50-50 split between MPLA and UNITA after the Bicesse agreement reflected a pro-UNITA bias, given that the MPLA army outnumbered UNITA four to one (Brittain, 1998).

where the military was carefully balanced between government and rebel soldiers, as in Mozambique (Hume, 1994: 59). Likewise, the 1999 peace accord in Congo-Brazzaville describes the parties' concerns over 'the reconstitution of [their] careers' through re-integration in the military. In Angola, Jonas Savimbi highlighted the importance of economic concerns, saying that warfare had become the rebels' 'raison d'être': these men had no homes and families, let alone jobs, to which they could return (Anstee, 1996: 55). The critical economic function of MI can explain why it is often pursued even when it cannot offer security guarantees.

Another way to get at the distinction between the security and economic mechanisms is to consider how MI interacts with other peacebuilding strategies, especially programs to reintegrate ex combatants in society and restore them to jobs outside of the military. If MI functions as an economic incentive against rebel remobilization in poor countries, then civilian reintegration ('CR') programs should act as a substitute for MI.

CR programs take many forms. Ideally, CR should involve vocational training and other programs to facilitate the transition to civilian life, but reality usually falls short of the ideal. We define CR as any official program by the government or third party making some sort of non-salary transfer to former combatants for the purpose of easing their transition out of the military. A paradigmatic case is Mozambique. Here, a fairly generous civilian reintegration program made it difficult to staff the new, integrated national army. Intended to be 30,000 strong, drawn equally from FRELIMO and RENAMO, the actual post-war integrated Mozambican army was less than half that size. The CR program ended up with twenty thousand more participants than anticipated (Synge, 1997: 108-109). Most observers agree that the rebels preferred CR to MI.

Civilian reintegration had two components: the IRS which provided demobilized soldiers with information about job training and reintegration services, and more important, the RSS,

which aimed to reintegrate former combatants into civilian life by giving soldiers a total of two years of cash payments and a stock of agricultural supplies. The program has been wildly successful in the eyes of the demobilized rebels; surveys conducted by the UNDP report that over 90% of the program's beneficiaries were satisfied with the program because it 'allowed them an opportunity to start income-generating activity' (Venâncio, 1998). RSS paid the former combatants considerably more than army salaries (and left them free to pursue other employment), so it is not surprising that MI was unpopular by comparison. Most government and rebel officers, who were paid higher salaries than the infantry, decided to join the integrated national army (Alden, 1998).

Mali's successful peace process similarly offered both civilian and military integration opportunities. While fewer than 2,000 ex-combatants ended up integrating into the military, more than 9,000 participated in CR, which involved a cash payment and either a small monthly payment or enrollment in a UNDP credit program, depending on whether they had turned in their weapon or not, plus vocational training and educational scholarships. Of those who took the CR option, about a third opted for the plan that paid more generous benefits yet required turning in a weapon, while 6000 took the less lucrative plan that did not require handing over a weapon. Like the program in Mozambique, Mali's CR program reduced the demand for MI and offered tangible economic benefits to keep the peace.

Several of the cases that we reviewed suggest that CR and MI are substitutable and CR is often used to accommodate an 'excess supply' of ex-combatants. In Angola's most recent peace process, UNITA combatants who had been formerly integrated into the government army and demobilized were paid five months of back pay, given an integration allowance and a reintegration kit of household and farming items (Parsons, 2004). Likewise, in Cambodia in

1998 and Iraq in 1972, former rebel combatants who could not be integrated in the national army were provided with cash payments. This also happened informally in Namibia, where most of the 21,000 or so demobilized Namibian soldiers from SWATF (the largest Namibian force to fight for the South African side) continued to take their pay well after the peace process had concluded.

A factor that sometimes affects the degree to which CR can substitute for MI is the availability of funding. In Angola, vocational training programs for CR were not funded as foreign donors were much more interested in funding election organization and supervision (Anstee, 1996: 54-55). In Cambodia, lack of funding for CR did not allow the government to downsize sufficiently the army which had been radically oversized after the integration of thousands of Khmer Rouge troops (Keesing's, 2001). In the Southern Philippines, the government's failure to implement development projects in the Muslim separatist region is often seen as a cause of resumed conflict (Bacongco, 2000). Liberia's flawed demobilization and civilian reintegration program in 1997 has been called 'an important part of the lost chance in 1997, facilitating the re-recruitment of fighters for the armed groups that tore the country apart between 1999 and 2003' (International Crisis Group, 2004).

This case evidence, though merely illustrative, supports an interpretation of MI that is more consistent with the economic mechanism than the security mechanism.

Conclusion

Military integration agreements are increasingly popular strategies to end civil wars. The assumption underlying their use is that MI provides a credible security guarantee that reassures the parties. Yet, we found no significant effect of MI on peace duration in the short or long term.

Part of the problem is that MI agreements are often poorly structured and not fully implemented, so they cannot offer *credible* security guarantees. The label ‘military integration’ is often meaningless. It is a catch-all phrase that describes a wide array of policies, which work in very different ways and therefore can be expected to have different effects under different conditions. But, in principle, MI could address security and economic concerns, *depending on how it is structured and executed*. Often, the economic function of MI takes precedence and MI agreements may do better if there is external assistance. But the role of third parties in military and civilian reintegration should be further problematized. Third parties often can do nothing in the face of remobilizing combatants, but they sometimes can provide financing and monitoring that helps build trust while peace agreements are being implemented. Third-party intervention that is less muscular and more consent-based and builds economic capacities may be more effective in building trust and supporting different mechanisms for peacebuilding, including MI.

These insights are supported by our statistical analysis and by a review of several cases. Our article aims to start a dialogue on ways to structure peace agreements and create self-enforcing mechanisms for the transition to peace after civil war. We identified the need to study the interactive effect of MI agreements, exploring how they work in conjunction with peacekeeping, power-sharing, or other elements of peace agreements and we have argued that MI operates more through an economic mechanism rather than a security mechanism. We need to consider further if MI is a complement or a substitute for other economic interventions, such as demobilization and civilian reintegration programs.

The messy reality of the design and implementation of military integration makes it hard to draw firm conclusions about the potential efficacy of this strategy except inasmuch as we should always expect implementation and design problems. Our review of the cases and the

quantitative data revealed that MI is preferred by ex-combatants because of the economic opportunities that it offers rather than its security guarantees. We also argued that pursuing military integration outside of the context of a political settlement is unlikely to work and that military integration should only be promoted as a peacekeeping strategy as a part of a multi-dimensional approach to peacebuilding.

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KATHERINE GLASSMYER, b. 1976, doctoral candidate in Political Science at Yale University.

NICHOLAS SAMBANIS, b. 1967, Associate Professor of Political Science at Yale University since 2004, co-author of *Making War and Building Peace: United Nations Peace Operations* (Princeton, NJ: Princeton University, 2006) and co-editor of *Understanding Civil War: Evidence and Analysis* (Washington, DC: World Bank, 2005).

Table I. Effects of Military Integration on War Recurrence

Dependent variable is war recurrence

Standard errors in parentheses

| <i>Type of MI:</i> | Model 1 <i>All MI Agreements</i> | Model 2 <i>Lenient MI</i> | Model 3 <i>Strict MI</i> | Model 4 <i>Both forms of MI</i> | Model 5 <i>Implemented MI</i> | Model 6 <i>All MI Full Model</i> | Model 7 <i>Interaction Treaty x MI</i> | Model 8 <i>Interaction UN x MI</i> | Model 9 <i>Interaction PPS x MI</i> | Model 10 <i>Including PPS and MI</i> |
|---|---|------------------------------|-----------------------------|--|--------------------------------------|---|---|---|--|---|
| Military Integration (all) | -0.75# (0.42) | | | | 0.30 (0.58) | -0.05 (0.53) | | | | -1.27 (0.87) |
| Military Integration (Lenient Only) | | -1.17# (0.65) | | -1.26* (0.64) | | | | | | |
| Military Integration (Strict Only) | | | -0.38 (0.55) | -0.50 (0.55) | | | | | | |
| Ethnic War (<i>War Type</i>) | | | | | | 0.00 (0.44) | -0.01 (0.44) | 0.02 (0.44) | 0.00 (0.44) | 0.07 (0.45) |
| Deaths and Displaced Log (<i>logcost</i>) | | | | | | -0.23# (0.13) | -0.23# (0.13) | -0.23# (0.13) | -0.23# (0.13) | -0.22# (0.13) |
| Number of Factions (<i>Factnum</i>) | | | | | | -0.23# (0.13) | -0.24# (0.13) | -0.23# (0.13) | -0.29# (0.16) | -0.34# (0.18) |
| Net Transfers per Capita (<i>Transpop</i>) | | | | | | 0.00 (0.00) | 0.00 (0.00) | 0.00 (0.00) | 0.00 (0.00) | 0.00# (0.00) |
| UN intervention (<i>Unintrvn</i>) | | | | | | 0.53 (0.63) | 0.55 (0.63) | 0.39 (0.72) | 0.59 (0.65) | 0.56 (0.69) |
| Signed Peace Treaty (<i>Treaty</i>) | | | | | | -0.81 (0.55) | -0.99 (0.62) | -0.88# (0.49) | -1.23* (0.57) | -1.49* (0.64) |
| Level of GDP Log (<i>lnGDP</i>) | | | | | | 0.49* (0.22) | 0.51* (0.22) | 0.51* (0.22) | 0.58* (0.23) | 0.47# (0.27) |
| Primary Commodity Exports/GDP (<i>Isxp2</i>) | | | | | | -3.28* (1.37) | -3.23* (1.32) | -3.34* (1.32) | -3.26* (1.33) | -3.43* (1.41) |
| Treaty x MI (<i>trim</i>) | | | | | | | 0.26 (0.63) | | | |
| UN x MI (<i>unmi</i>) | | | | | | | | 0.48 (1.03) | | |
| Political Power-Sharing x MI (<i>MIPPS</i>) | | | | | | | | | 0.94# (0.55) | |
| Political Power-Sharing (<i>political power-sharing</i>) | | | | | | | | | | 2.38* (0.95) |
| Constant | 1.04** (0.24) | 0.95** (0.22) | 0.92** (0.24) | 1.04** (0.24) | 0.80** (0.23) | 4.95** (1.65) | 4.92** (1.66) | 4.97** (1.69) | 5.03** (1.62) | 5.16** (1.59) |
| Observations: | 120 | 120 | 121 | 120 | 120 | 119 | 119 | 119 | 119 | 119 |
| Wald Chi2 | 3.15 | 3.27 | 0.49 | 4.57 | 0.27 | 21.48 | 22.48 | 22.1 | 23.58 | 25.23 |
| P-value | 0.076 | 0.0704 | 0.4826 | 0.1015 | 0.6066 | 0.0107 | 0.0075 | 0.0086 | 0.005 | 0.0049 |

Note: **significant at 0.01; *significant at 0.05; # significant at 0.05 for a 1-tailed test. All data analysis performed in STATA.

Table II. Survival Model of Peace Duration

Weibull regression -- log relative-hazard form

Standard errors in parentheses

| <i>Type of MI:</i> | Model 1 <i>All MI Agreements</i> | Model 2 <i>Strict MI Defintion</i> | Model 3 <i>Implemented MI Agreements</i> | Model 4 <i>All MI Agreements</i> | Model 5 <i>Interaction MI x Treaty</i> | Model 6 <i>All MI Agreements</i> | Model 7 <i>All MI Agreements</i> |
|--|---|---|---|---|---|---|---|
| Military Integration | 0.80 (0.37) | 1.15 (0.47) | 0.79 (0.36) | 0.77 (0.35) | | 0.91 (0.46) | 3.30* (1.58) |
| Ethnic War (<i>War Type</i>) | 1.02 (0.30) | 1.03 (0.30) | 1.05 (0.31) | 0.99 (0.27) | 1.06 (0.31) | 1.01 (0.31) | 1.08 (0.33) |
| Deaths and Displaced in logs (<i>logcost</i>) | 1.15* (0.07) | 1.15* (0.07) | 1.15* (0.07) | 1.12 (0.08) | 1.14* (0.07) | 1.15* (0.07) | 1.14* (0.06) |
| Number of Factions (<i>Factnum</i>) | 1.06 (0.11) | 1.05 (0.10) | 1.06 (0.11) | 1.07 (0.10) | 1.06 (0.10) | 1.04 (0.12) | 1.14 (0.13) |
| Ethnic Fragmentation (<i>ef</i>) | 3.78** (1.87) | 3.95** (1.90) | 3.84** (1.98) | 3.38* (1.72) | 3.58** (1.72) | 3.95** (2.01) | 3.24* (1.65) |
| Net Transfers per Capita (<i>Transpop</i>) | 1.00 (0.00) | 1.00 (0.00) | 1.00 (0.00) | 1.00 (0.00) | 1.00 (0.00) | 1.00 (0.00) | 1.00 (0.00) |
| GDP Growth (<i>gdpprpf</i>) | 0.96** (0.02) | 0.96** (0.01) | 0.96* (0.01) | 0.96** (0.02) | 0.96** (0.02) | 0.96* (0.02) | 0.95** (0.02) |
| Level of GDP in logs (<i>lnGDP</i>) | 0.77* (0.09) | 0.80* (0.09) | 0.79* (0.09) | 0.73* (0.10) | 0.75* (0.09) | 0.77* (0.09) | 0.74* (0.09) |
| UN Peace Mission (<i>ch6</i>) | 0.51# (0.19) | 0.49# (0.18) | 0.50# (0.18) | 0.41* (0.16) | 0.55 (0.22) | 0.67 (0.34) | 0.45* (0.18) |
| Signed Peace Treaty (<i>Treaty</i>) | 1.13 (0.42) | 0.94 (0.32) | 1.08 (0.41) | 1.01 (0.37) | 1.63 (0.60) | 1.10 (0.40) | 1.53 (0.52) |
| Primary Commodity Exports/GDP (<i>Isxp2</i>) | 3.72* (2.17) | 3.71* (1.98) | 3.39* (2.07) | 4.25* (2.68) | 3.80* (2.49) | 4.48* (3.17) | 4.79* (3.71) |
| Military Victory (<i>milout</i>) | | | | 0.62 (0.19) | | | |
| MI x Treaty (<i>milintr</i>) | | | | | 0.45# (0.20) | | |
| Chapter VI UN x MI (<i>ch6mi</i>) | | | | | | 0.53 (0.42) | |
| Power sharing x MI (<i>mipps</i>) | | | | | | | 0.14** (0.08) |
| Observations: | 130 | 131 | 129 | 130 | 130 | 130 | 130 |
| Wald Chi2 | 88.27 | 101.23 | 85.17 | 84.44 | 73.23 | 83.23 | 77.81 |
| P-value | 0 | 0 | 0 | 0 | 0 | 0 | 0 |

Note: **significant at 0.01; *significant at 0.05; # significant at 0.05 for a 1-tailed test.

Table III. Determinants of Military Integration

Logit Regression
Standard errors in parentheses

| | Model 1 | Model 2 | Model 3 |
|--|---------------------|---------------------|---------------------|
| <i>Dependent Variable:</i> | <i>MI Agreement</i> | <i>MI Agreement</i> | <i>MI Agreement</i> |
| Ethnic War (<i>War Type</i>) | -0.08 (0.51) | -0.32 (0.48) | 0.33 (0.80) |
| Deaths and Displaced in logs (<i>logcost</i>) | -0.22 (0.19) | -0.14 (0.16) | -0.16 (0.14) |
| Number of Factions (<i>Factnum</i>) | 0.11 (0.18) | 0.04 (0.18) | -0.07 (0.22) |
| Ethnic Fragmentation (<i>ef</i>) | 0.58 (1.40) | -0.99 (1.34) | 0.16 (1.83) |
| Net Transfers per Capita (<i>Transpop</i>) | 0.00 (0.00) | 0.00 (0.00) | 0.00 (0.00) |
| GDP Growth (<i>gdpgrpfl</i>) | -0.01 (0.03) | 0.01 (0.03) | 0.09** (0.03) |
| Level of GDP in logs (<i>lnGDP</i>) | -0.65# (0.34) | -0.30 (0.42) | 0.05 (0.48) |
| UN Peace Mission (<i>ch6</i>) | -0.09 (0.64) | -0.19 (0.69) | 0.29 (0.82) |
| Signed Peace Treaty (<i>Treaty</i>) | 2.37** (0.62) | 2.11** (0.61) | 2.86** (0.72) |
| Primary Commodity Exports/GDP (<i>Iexp2</i>) | -0.25 (1.25) | -0.69 (1.09) | -5.24* (2.09) |
| Military Victory (<i>milout</i>) | -1.25* (0.55) | -1.11* (0.55) | |
| War Duration, logged (<i>lnwardur</i>) | 0.28 (0.21) | 0.39# (0.22) | 0.53# (0.30) |
| Sub-Saharan Africa (<i>geo5</i>) | | 1.85** (0.72) | 1.94** (0.74) |
| Non-UN Intervention (<i>nonUN</i>) | | | |
| Constant | -0.71 (1.85) | -1.46 (1.95) | -3.60* (1.81) |
| Observations: | 131 | 131 | 130 |
| Wald Chi2 | 42.01 | 47.83 | 40.42 |
| P-value | 0.0000 | 0.0000 | 0.0001 |

Note: **significant at 0.01; *significant at 0.05; # significant at 0.05 for a 1-tailed test.

