

Transitional Justice: A Case for Accountability

Maximilian Aviles

MAIR Capstone

22 May 2019

Abstract:

Transitional Justice Mechanisms (TJMs) seek to help some of the world's most vulnerable states make an effective transition to democracy and help increase the respect for human rights for the people living in those states. Despite the hopes for TJMs, many academics continue to question whether TJMs are actually effective, and if they are effective, then which types of TJMs work best? This paper seeks to fill an important gap in the literature by quantitatively assessing the impact of TJMs on a country's level of democracy and respect for human rights. The results presented in this paper do, in general, lend support to the claim that TJMs are likely to help increase a country's level of democracy and its respect for human rights. Specifically, this paper finds that justice mechanisms such as tribunals are likely to have more of a positive impact than truth mechanisms such as a truth commission. These results are in no way intended to be the final say on this topic, but rather they should serve as call for increased debate by policymakers and as a leaping off point for further research about the use of TJMs.

Introduction

The world watched in awe when regime after regime began falling during the Arab Spring, but once it passed, many people began asking questions about if/how these states would be able to move on. Should all the security personnel who decided to follow orders and open fire on protestors be punished? Or, should people be content with the end result and agree to just move on? These types of questions are not unique to the Arab Spring. We asked how Hutus and Tutsis would ever live as neighbors again. We asked how South Africa could heal the systemic problems created by apartheid. We asked how mothers could find justice for their sons who were forcibly disappeared during the Dirty War in Argentina. The field of transitional justice was created because of a need to find answers for these questions, and one of the great debates within the field continues to be whether people should seek retributive justice for previous crimes, or whether states should pursue a restorative type of justice through the use of truth commissions.

The research presented in this paper seeks to add to this debate by quantitatively assessing whether the use of tribunals is an effective transitional justice mechanism (TJM), and if it is, then if it is more effective than other TJMs such as truth commissions. By empirically looking at the relationship between specific types of TJMs and the levels of both democracy and human rights in post-authoritarian states, this paper provides evidence to support the claim that tribunals are more effective at cultivating democracy and human rights than truth commissions or other mechanisms. Although the results of this paper should not be interpreted as an end to the truth vs. justice debate, it does suggest that states in transition will enjoy higher levels of both democracy and human rights if they choose to seek justice and prioritize accountability.

Literature Review

Before delving into the methodology and results of this paper, it is important to understand what the wider literature of transitional justice has to offer. As Arthur (2009) points out, the field of transitional justice gathered steam in the wake of the Truth and Reconciliation Commission in South Africa and after the fall of the military junta in Argentina. For the most part, Arthur (2009) concedes that the majority of this debate is centered on the effectiveness of “naming and shaming” versus ensuring accountability through tribunals. However, this is not to say that there is consensus within the field that transitional justice is even effective in the first place. Gready and Robins (2014) note that while Western academics and NGOs tend to advocate for the use of transitional justice, the record for transitional justice is largely “ambiguous and disappointing.” Mendeloff (2004) expresses a similar sentiment by suggesting that academics and policymakers should “curb the enthusiasm” for transitional justice because there is simply not enough evidence to advocate for its benefits yet. As these authors suggest, it is not that there is a plethora of evidence showing that TJMs have adverse effects, it is simply the fact that we do not have sufficient evidence to conclusively argue the benefits of transitional justice.

Some academics, however, do go so far as to say that the use of transitional justice can be both unnecessary and even harmful. Loyle and Davenport (2016) point out that while TJMs can be useful, the literature fails to address how these mechanisms can actually be used to make a situation worse. In Rwanda, for example, transitional justice has been manipulated to cultivate both a degree of denial and an excuse for legitimizing forms of authoritarianism and repression (Loyle and Davenport 2016). Another reason commonly put forward by those against the use of transitional justice is the idea that these mechanisms are often imposed by Western governments and NGOs even though the local people may not want them. In Burundi, for example, the

majority of local people were against using TJMs because they felt as though the political gains made after the conflict had sufficiently compensated for the previous injustices they suffered (Samii 2013). Similarly, Rebecca (2017) notes that the majority of Colombians were actually opposed to the use of TJMs, and that efforts to utilize transitional justice were largely seen as a Western imposed necessity rather than a locally driven process. For many locals, as well as academics, the effects of transitional justice are still widely unknown, and it is likely this will continue to be the case until research can more definitively explain what those effects are.

Contrary to the work just described, there are those who take a more optimistic outlook on the use of transitional justice and find it can produce positive results. For example, Frederking (2015) quantitatively shows that there is a significant difference in levels of democracy and human rights between countries that used a TJM and countries that did not. Similar studies have also shown that although not all types of TJMs are linked to increases in democracy and human rights, there is certainly evidence to support the idea that transitional justice can be used to increase levels of democracy and respect for human rights if mechanisms are used correctly (Olsen et al. 2010). For example, Gibson (2006) concludes that it was South Africa's Truth and Reconciliation Commission that played a significant and independent role in promoting democracy within the country after the end of apartheid. For many, it is not whether transitional justice can be beneficial, but rather what types of transitional justice would be most beneficial.

So, where does the literature stand on the truth vs. justice debate? Not surprisingly, it remains fairly divided. Those who stress the need for truth often make the argument that progress can only be made by finding out what led to the conflict in the first place. Lambourne (2009), for example, argues that we need to move away from prioritizing the Western notion of

retributive justice and instead move to the promotion of restorative justice, which promotes truth commissions as a mean for identifying the systemic causes that led to the initial conflict. Advocates of this type of restorative justice often argue that its use can effectively provide accountability, reparations, and reintegration within an affected society (Valinas and Vanspauwen 2009). Effective reintegration is, perhaps, one of the most powerful claims made by those who advocate for the importance of truth. Often times, the supporters of the previous regime are the most likely to be sources of crime in a new democracy; reintegration can help diminish that link. Trejo et al. (2018) show that states that used a truth commission had lower rates of crime and violence than states that did not, and this is largely because of the reintegration power of the truth commission. And, along the lines of reintegration, truth commissions can help foster personal forgiveness between people, which Inazu (2009) notes is necessary for allowing a country to effectively move forward. Finally, an important point to note is that not all truth commissions are created equally. Taylor and Dukalskis (2012) show that “publicness” is an important piece of truth commissions, and that while truth commissions that are public do indeed have positive impacts on democracy, truth commissions that are not as public fail to be effective. Despite the wide body of work advocating for the use of truth commissions, there are some who maintain the justice is more important for a state in transition.

Advocates of the “justice” approach to transitional justice give a variety of both qualitative and quantitative reasons for why states in transition should utilize mechanisms like tribunals, lustration, or reparations. In response to the notion that transitional justice should promote restorative rather than retributive justice (the argument made by “truth” advocates), Wilson (2001) argues that restorative justice is, in reality, a political myth used as a “moral justification for amnesty,” and that retributive justice offers the most promising prospects for

individual reconciliation. Andersen (2015) similarly claims that retributive justice should be utilized because of its effectiveness in bringing about a respect for rule of law, which should be prioritized in any transitioning state. Additionally, there is empirical research indicating that countries that use a justice mechanism, especially tribunals, tend to have higher levels of democracy and a greater respect for human rights than countries that simply use a truth commission or no TJM at all (Frederking 2015; Frederking and Aviles 2019). Minow (2008) similarly advocates that if tribunals are able to be implemented within a country, then they offer the best prospects for bringing about respect for human rights within that country. Similar to the literature advocating for truth commissions, there are advocates of the justice approach who argue that the type of justice is important. For example, both Gibson (2006) and Andersen (2015) note that “evenhandedness” is likely to be important in determining whether the tribunals will be effective. In addition to characteristics like evenhandedness, it is also important that prosecutions remain apolitical. Guercio (2015) points out that the benefits of justice in Libya were squandered as soon as its referral to the ICC became politicized. The literature surrounding justice mechanisms seems to then carry a similar narrative to the literature about truth mechanisms; they can be helpful, but only if effectively implemented.

Interestingly, while there are several studies linking the positive effects of democracy and transitional justice, there are those who contend that, in the fragile beginning stages of democracies, political stability should be prized over seeking out justice. Although one would expect a significant number of new democracies to seek out some type of victor’s justice, Olsen et al. (2010) point out that the majority of states actually choose to use amnesty policies more than anything else. This phenomenon can likely be explained by looking at the strength of the outgoing regime rather than an inherent preference for amnesty policies. For example, if the

outgoing regime still has significant power, states would be more likely to choose amnesty over justice (Skaar 1999). The logic behind the choice to use amnesty is made clear by David (2010) who suggests that amnesties help provide the necessary breathing room to governments that are trying to transition to democracy. Other scholars suggest that while amnesty may not be an effective tool by itself, it can be effective if paired with other types of TJMs. Some, for example, suggest that amnesties can be useful when implemented in conjunction with tribunals (Andersen 2015; Olsen et al. 2010). And, as if the literature was not divided enough, still others suggest that amnesty seems to have relatively zero impact on levels of democracy and human rights and can even have adverse effects (Frederking and Aviles 2019). Clearly, the literature on amnesty seems to be as divided as the literature on transitional justice at large, with no easily discernible consensus on the topic.

Perhaps one of the most interesting takeaways from the literature is not so much the subject as it is the manner in which research on the subject is conducted. Gready and Robins (2014) note that a large majority of the transitional justice field continues to be rather normatively driven and that there is a desperate need for more empirical research. Similar observations are common, and there are even some who go as far as to say that the current transitional justice field is “inherently flawed” because of the common use of biased case selections and a heavy reliance on “normative conviction” and anecdotal evidence (Brahm 2007). This is not to say that the field is completely devoid of any quantitative research. However, due to the fact that quantitative research still constitutes such a small percentage of the field, definitive evidence is hard to find. Thoms et al. (2010) point out that the relative lack of empirical research within the field means that much of the empirical evidence suggests contradictory findings. This problem also stems from the fact that, because of the relatively new

introduction of quantitative research within the field, authors continue to use different measurements to study the same effect, which is likely to cause mixed results (David 2017). The current lack of sufficient quantitative evidence in regard to transitional justice is problematic, and the need for additional quantitative research should be embraced in order to come to more conclusive results about the effects of transitional justice (Mendeloff 2004, 2009; Frederking 2015; Frederking and Aviles 2019). Although there is some pushback to the usefulness of empirical data for transitional justice, there is clearly a realization among many that its current lack of quantifiable data is a weak point of the literature.

Hypothesis

In Frederking and Aviles (2019), the authors found that states that used tribunals in a democratic transition exhibited higher levels of both democracy and human rights than states that did not. Additionally, the authors found that “justice” mechanisms, like tribunals or reparations, were significantly correlated with higher levels of democracy and human rights compared to “truth” mechanisms, which seemed to only have a marginal impact on a country’s level of democracy or human rights. Countries that chose to forgive and forget through amnesty laws saw relatively little to no improvement in levels of democracy and human rights, and in some cases, actually saw those levels worsen. This paper seeks to both elaborate on these aforementioned conclusions and pick up where Frederking and Aviles (2019) left off. By running more sophisticated regression models and controlling for outside factors, both of which Frederking and Aviles (2019) acknowledged as necessary for further research, this paper provides some important data to both support and challenge their original conclusions. In analyzing the impact of TJMs on levels of democracy and human rights, it is important to note that a critical idea underlying this paper is the argument that countries should strive for higher

levels of democracy and increased respect for human rights. By understanding that democratization and increasing respect for human rights are the ultimate goals for states undergoing a democratic transition, we can now try and figure out whether TJMs are significantly effective means for achieving those goals.

Democracy and human rights are, undoubtedly, huge themes to try and focus on in the limited scope of a single paper; however, by testing for a few, specific hypotheses, it is possible to make these expansive themes more manageable for the purposes of this paper. So, before delving into the methodology and operationalization of these ideas, I will begin by first specifying the hypotheses that I will be testing in the pages that follow. These hypotheses are developed based on both the results of Frederking and Aviles (2019) and the larger literature surrounding TJMs, democracy, and human rights. The two main hypotheses for this paper are as follows:

H₁ - States that use tribunals will be more likely to exhibit higher levels of democracy and human rights than states that did not.

H₂ - States that use tribunals will be more likely to exhibit higher levels of democracy and human rights than states that used other types of TJMs

Although these two hypotheses seem rather simple, they are appropriate for this paper for two main reasons. First, as the literature suggests, the topic of transitional justice can often turn into a rabbit hole in which it remains difficult to focus on a single causal mechanism. By narrowing the scope of this paper to focus on the two stated hypotheses, I aim to avoid the temptation faced by many in the field to try and explain democratization and the cultivation of human rights in post-conflict countries writ large. Second, the narrow scope of these hypotheses is meant to increase the accuracy in operationalizing these concepts for the purposes of quantitative research.

Rather than simply throwing a plethora of variables at the wall and hoping something sticks, I aim to hone in on these two main hypotheses and dig in to the results presented in this paper.

Methodology

The cases used in this paper are taken from the Transitional Justice Database Project (TJDP), which was compiled by Olsen et al (2010). Although the original TJDP identifies 255 cases of countries that underwent a democratic transition, data presented in this paper uses just 184 of those total cases. The reason for excluding the other 71 cases is primarily because of the way the TJDP records civil wars. In the TJDP, a recurrent civil war is counted as several separate cases. For example, Olsen et al (2010) identify 164 cases of civil war; however, for the purposes of this paper, I consolidate those separate cases into a single case. This is a similar method used by Frederking and Aviles (2019), which notes that the consolidation of those cases leaves us with 91 cases of post-authoritarian societies and 93 cases of civil wars.

Trusting that the methodology behind the selection of cases for this paper is clear, I will now move on to explaining the different variables I use in this paper to test for the two main hypotheses. The majority of independent variables are also taken using information compiled in the TJDP. They are as follows:

- Tribunals – this is a dummy variable that is coded as 0 if the country did not use any form of tribunals or 1 if the country did use tribunals.
- Truth Commissions – again, this is a dummy variable where 0 represents a country that did not use a truth commission, and 1 represents a country that did issue a truth commission report.
- Amnesty – a dummy variable where 1 indicates the state issued amnesty for perpetrators of human rights abuses, and 0 represents a state that did not issue amnesty.

- Reparations – a dummy variable coded so that 1 represents a state that issued monetary compensation for victims of human rights abuses, and 0 represents states that did not.

The dependent variables used in this paper, which aim to measure both how democratic a state is and the respect for human rights within that state, are taken from two main sources: the Polity IV Project and the CIRI Human Rights Dataset. Measuring how democratic or autocratic a state is can merit an entire paper to itself, but for the purposes of this research, I chose to operationalize this variable by using data provided by the Polity IV Project, which is compiled by the Center for Systemic Peace. Polity IV is commonly used in quantitative studies that analyze democracy/autocracy as variables, so I see no reason not to use it in this paper as well. And, if measuring democracy can be seen as tricky, then measuring the respect for human rights is even more complicated. Thankfully, the CIRI Human Rights Dataset is widely recognized as a respectable way to measure the respect for human rights within a country. The CIRI Human Rights Dataset is, by no means, a perfect indicator for the respect of all types of human rights within a given state, but it is deemed accurate in measuring a person's right not to be imprisoned, tortured, or executed based on one's political beliefs. This paper, like Frederking and Aviles (2019), chooses to measure the dependent variables at both five and ten years after the given state went through a democratic transition. This is done in order to take into account that different TJMs may have different time periods in which their effectiveness would be most evident. Based on this description, the dependent variables used in this paper are as follows:

- Polity IV +5 – this is taken five years after the given state's transition and is scored as a number from 10 to -10 (10 being perfectly democratic and -10 being perfectly autocratic).
- Polity IV +10 – this is taken ten years after the given state's transition and is scored as a number from 10 to -10 (10 being perfectly democratic and -10 being perfectly autocratic).

- CIRI +5 – this number is taken five years after the given state's transition and is coded as a number from 0 to 14 (0 signifying no respect for human rights and 14 signifying full respect for human rights).
- CIRI +10 – this number is taken ten years after the given state's transition and is coded as a number from 0 to 14 (0 signifying no respect for human rights and 14 signifying full respect for human rights).

I also decide to also measure a country's GDP per capita to try and make sure that any perceived relationship between TJMs and democracy/human rights was not because of an unaccounted for outside factor. Although the decision to include an economic variable may, at the surface, seem random, there is ample literature to defend its use as a control for democracy. Huber et al. (1993) explicitly argues that economic development and democracy are causally linked. And, while some may counter by saying that democracy causes development, Lewis-Beck and Burkhart (1994) provide empirical evidence suggesting that the reverse is true: economic development causes democracy, not the other way around. In order to control for the possibility that economic development may be more of a predictor than TJMs, I decided to use data provided by the World Bank to create to variables:

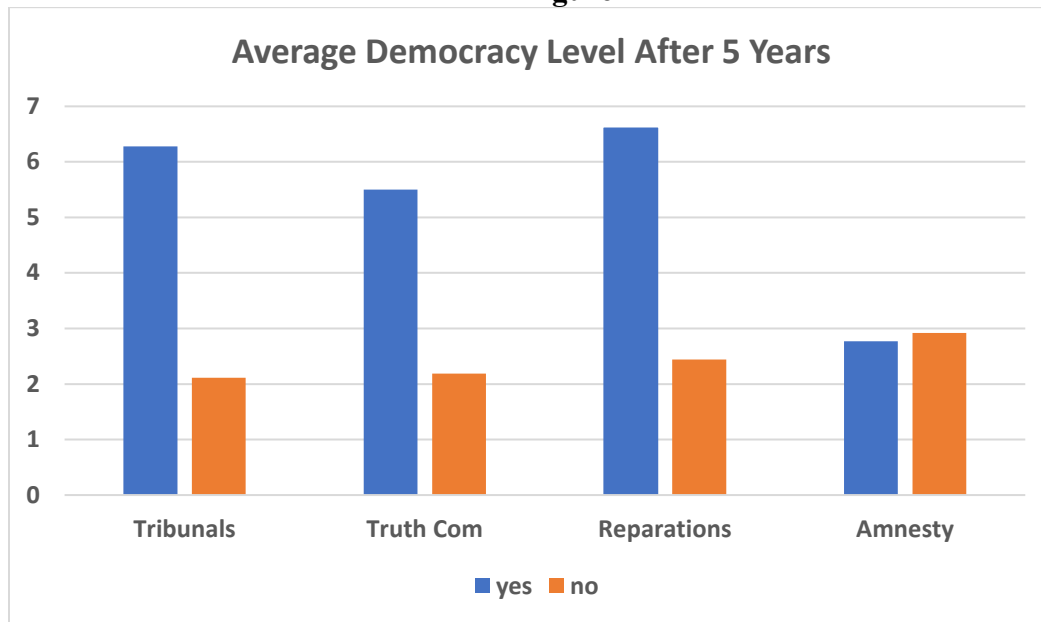
- GDP P/C +5 – this is the given country's GDP per capita five years after the state's transition.
- GDP P/C +10 – this is the given country's GDP per capita ten years after the state's democratic transition.

Let me be the first to say that I am not suggesting that GDP per capita is the most accurate way to measure a country's level of economic development; however, I do think it is sufficiently accurate for the purposes of this paper. Other measurements, which might be theoretically more

appropriate, tend to have larger gaps of data, especially for the majority of the cases used in this paper, which date back to 1970. By using GDP per capita, I am able to have data for nearly every case used in this paper.

Results

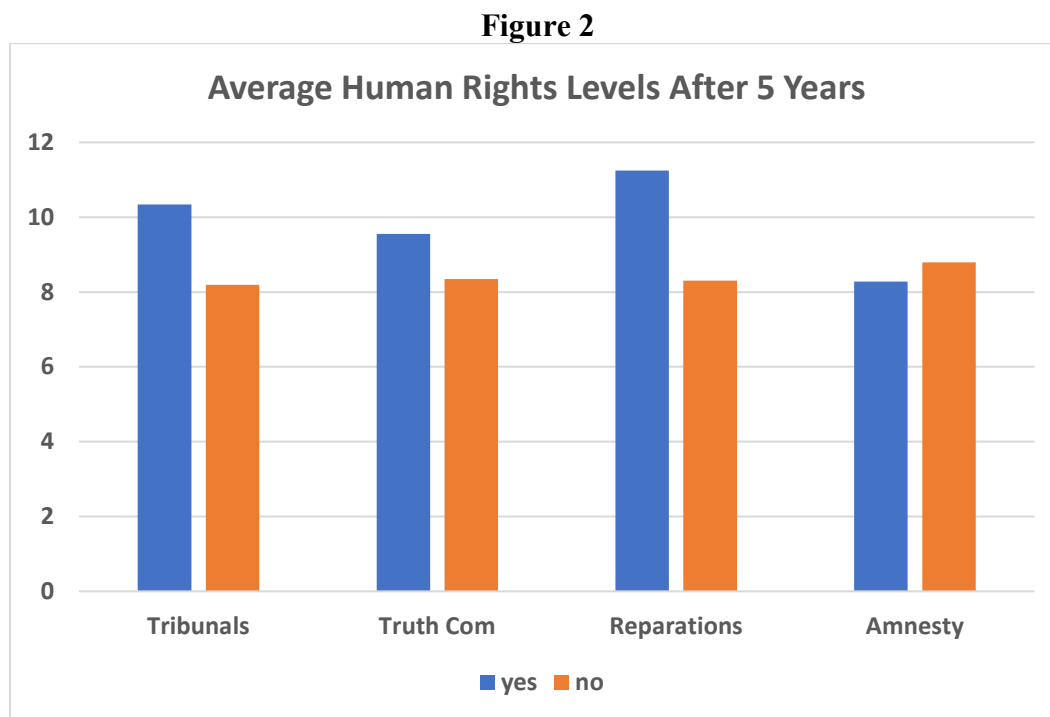
Figure 1



**Democracy as measured by Polity IV democracy index*

Before delving into correlations and regression analysis, I think it is helpful to begin by taking a cursory look at some of the descriptive statistics found within the data. Figure 1, which looks at the mean democracy score five years after a transition, is particularly useful in helping us visualize how stark of a contrast there is between states that used a TJM (especially a justice TJM) and those that did not. According to Figure 1, we see that states that used tribunals after a democratic transition or civil war have a mean Polity IV score of 6.28; however, states that did not use tribunals have a mean score of only 2.11. We see a similar disparity between states that used reparations and states that did not, and Figure 1 also shows a significant increase in democracy levels for states that used a truth commission. States that used a truth commission have an average democracy level of 5.5, while states that did not use a TJM average only 2.19.

What about states that simply granted amnesty? As Figure 1 depicts, there is relatively no difference in average levels of democracy between states that used amnesty and states that did not; in fact, closer inspection shows that states that used amnesty actually averaged slightly more autocratic than states that did not use amnesty.



**Level of human rights is measured by CIRI Human Rights Index*

Similar to a state's average level of democracy five years after a transition, Figure 2 shows that there does, at face value, appear to be a difference in respect for human rights between states that chose to use a TJM and states that did not. Figure 2 shows that regardless of whether a state chose to use tribunals, a truth commission, or reparations, states that used these mechanisms have higher human rights scores than states that did not. And, similar to Figure 1, Figure 2 shows that states that used amnesty programs actually exhibit slightly less respect for human rights than states that did not use amnesty. Strikingly, reparations actually show the largest difference in human rights scores, with states that used reparations scoring 11.25 while states that did not use reparations scored only 8.3.

Table 1
TJMs and Democracy: Bivariate Correlation

		Polity +5	Polity +10
Tribunals	Pearson Correlation	.270	.285
	Sig. (2-tailed)	.000	.000
Truth Com.	Pearson Correlation	.224	.157
	Sig. (2-tailed)	.003	.050
Reparations	Pearson Correlation	.212	.182
	Sig. (2-tailed)	.005	.022
Amnesty	Pearson Correlation	-.012	-.016
	Sig. (2-tailed)	.874	.846

Table 2
TJMs and Human Rights: Bivariate Correlation

		CIRI +5	CIRI +10
Tribunals	Pearson Correlation	.246	.251
	Sig. (2-tailed)	.003	.002
Truth Com.	Pearson Correlation	.144	.131
	Sig. (2-tailed)	.081	.116
Reparations	Pearson Correlation	.265	.233
	Sig. (2-tailed)	.001	.005
Amnesty	Pearson Correlation	-.069	-.027
	Sig. (2-tailed)	.404	.745

Taking a step up in the sophistication level from the previously described descriptive statistics, Tables 1 and 2 show the results from various bivariate correlation tests. Starting with the relationship between transitional justice and democracy, Table 1 shows that all three TJMs (tribunals, truth commissions, and reparations) enjoy statistically significant correlations with a country's level of democracy at both the five and ten year marks. However, it is interesting to note that the significance and strength of correlation marginally increases for tribunals between the five and ten year marks, while the significance of both truth commissions and reparations actually decrease from five to ten years. The fact that tribunals maintain a .000 significance for democracy at both five and ten years after a transition is promising evidence in regard to the paper's initial hypotheses.

Table 2, which shows the results for the correlation tests between the various TJMs and respect for human rights, suggests a somewhat similar story to the results presented in Table 1. Like the correlation for democracy, the bivariate correlation for human rights shows that tribunals are the only TJM that actually increase in strength and significance from five to ten years. Additionally, Table 2, like Table 1, shows that all correlations are in the positive direction, which means that if a country uses a TJM, then it is correlated with both higher levels of democracy and increased respect for human rights. One key difference between the correlations for democracy vs. the correlations for human rights is the fact that truth commissions, while significant for democracy levels, fail to meet the .05 threshold for statistical significance for human rights. Like the results of Table 1, the results of Table 2 also lend themselves to supporting the hypothesis that tribunals will be the most effective mechanism for increasing respect for human rights and democracy.

Table 3
TJMs and Democracy: Control for Economic Development

		Polity +5	Polity +10
Tribunals	Pearson Correlation	0.237	0.238
	Sig. (2-tailed)	0.003	0.003

Table 4
TJMs and Human Rights: Control for Economic Development

		CIRI +5	CIRI +10
Tribunals	Pearson Correlation	0.223	0.221
	Sig. (2-tailed)	0.007	0.008

Next, in order to make sure that these correlations are not spurious, I decide to run a partial correlation to control for a country's level of economic development. The results are presented in Table 3, which shows the results in regard to democracy, and Table 4, which shows the results with respect to human rights. Even when controlling for economic development, Table 3 shows that tribunals are still highly correlated with democracy at the .01 level, which

helps ensure that the initial correlation between tribunals and democracy is not spurious in nature. There is, however, at least some change in the significance and correlation strength between tribunals and human rights when we control for economic development. For example, the correlation significance for human rights after ten years decreases to .008 (the bivariate correlation showed an initial significance of .003). These changes in significance are far from indicating a spurious correlation between the use of tribunals and respect for human rights; however, the slight decrease in significance may indicate that economic development will show up as a significant variable in the regression models. In either case, these results show that there continues to be more evidence supporting, rather than refuting, the claim that tribunals are causally related to democracy and human rights.

[See Appendix for Table 5]

After finding that tribunals continued to remain significantly correlated despite controlling for economic development, I then moved on to analyzing the data using multiple regression models. Table 5 through Table 8 all show the results of these multiple regressions. It is important to note that all multiple regression analyses were conducted using a forward entry method, which helped to narrow down which variables could best explain the variance in the dependent variables. Table 5 shows the results from the multiple regression with democracy scores after five years as the dependent variable. Despite the numerous variables entered into the regression, only tribunals and GDP per capita met the threshold to be included in the final regression models. Table 5 shows that the use of tribunals was the first variable selected to fit the model, and this single variable has an Adjusted R Square of .067, which translates to an understanding that the tribunals variable helps explain roughly 7 percent of the variance of the Polity IV variable after five years. Once the measurement for economic development is included

in the regression, the model accurately explains roughly 10 percent of variance in a country's level of democracy five years after a transition or conflict. In addition to promising significance and Beta values presented in Table 5, both the tolerance and VIF values suggest that there is little to no multicollinearity between tribunals and GDP per capita.¹ And, just as telling as the variables that made it into the regression models are the variables that did not make the cut: truth commissions, amnesty, reparations, a combination of tribunals and truth commissions, and a combination of tribunals and amnesty. In terms of what these results mean for the hypothesized effects of tribunals, the combination of a statistically significant tribunals variable plus the exclusion of all other types of TJMs certainly lends itself to supporting the claim that tribunals are the most effective mechanism at supporting democracy within a country.

[See Appendix for Table 6]

Table 6 shows the results from the multiple regression model for democracy after ten years as the dependent variable. Again, we see that only tribunals and economic development met the significance threshold to be included into the regression model using the forward entry method. However, the results in this regression analysis are not exactly similar to the regression analysis presented for democracy after five years. For starters, it is interesting to note that while tribunals were the stronger indicator for democracy after five years, economic development is the stronger predictor for democracy after ten years. Additionally, the Adjusted R Square values for this regression model are stronger than the ones presented in Table 5, which means that this model is a more accurate predictor for democracy at ten years rather than five. In Table 6, we see that GDP per capita alone accounts for roughly 9 percent of the variance for democracy after ten years, and the addition of the tribunals variable increases that predictability to roughly 14

¹ This test for multicollinearity is performed for all regression models in this paper, and in all cases the results suggest that there are no inherent validity problems associated with multicollinearity among the given variables.

percent. Despite the increased significance of economic development, the fact that tribunals were the only TJM included in the regression model means that out of the various types of TJMs, tribunals continue to appear most likely to increase a country's level of democracy.

[See Appendix for Table 7]

Table 7 shows the multiple regression results with human rights measurements taken five years after a transition as the dependent variable. These results are rather surprising compared to the results in Tables 5 and 6. Whereas economic development merits inclusion in the regression models for democracy, it fails to meet the statistical criteria to merit inclusion in the regression models for any of the human rights tests. In finding out which variables can best explain the variations in levels of human rights five years after a transition, only tribunals and reparations meet the criteria for inclusion. Reparations actually show the strongest relationship with the human rights variable; reparations alone accounts for nearly 7 percent of the variance in the dependent variable according to the Adjusted R Square value. And, once we add tribunals to that mix, the predictability increases to roughly 10 percent. The results presented in this regression analysis offer two takeaways. First, there appears to be a highly significant relationship between “justice” mechanisms and respect for human rights. Second, this is largely the first piece of evidence that would contradict the hypothesis that tribunals are the most effective TJMs for increasing respect for human rights; the results of this correlation would suggest the importance of reparations as opposed to the use of tribunals.

[See Appendix for Table 8]

The results from the multiple regression for the respect of human rights ten years after a transition are presented in Table 8. Again, we see that economic development fails to meet the criteria for being included in the final regression model and, therefore, is not considered a

significant predictor of respect for human rights. Similar to Table 7, the only two variables included are reparations and tribunals. However, unlike the regression model for human rights after five years, the model for ten years has tribunals as being the more significant variable. Tribunals alone account for roughly 6 percent of the variance in the dependent variable according to the Adjusted R Square, and the inclusion of reparations increases that to roughly 9 percent. The tribunals dummy variable was noticeably more significant than the reparations variable and also enjoyed a slightly stronger correlation strength as given by the Beta coefficient. Despite the relatively low Adjusted R Square values, the results do present a statistically significant regression model for measuring the respect for human rights ten years after a transition or conflict. In terms of what this means for this paper's hypothesis, it does lend itself to supporting the claim that the use of tribunals will lead to increased respect for human rights.

Discussion

So how do we make sense of this litany of results, and more importantly, what do they mean for the two hypotheses being addressed in this paper? The first hypothesis, which states that countries that use tribunals as a form of transitional justice will have higher levels of democracy and human rights than states that do not, does seem to be strongly supported by the results of this paper. In addition to the strong correlation between tribunals and democracy/human rights, tribunals were the only variable that was significant enough to be included in all four multiple regression models. This means that the use of tribunals is statistically significant and can be considered an accurate predictor of both democracy and respect for human rights in a country after a democratic transition. The caveat to this claim, which I have already noted, is the fact that the R square numbers for these regression models remain fairly low. So, while tribunals may be highly correlated with democracy and human

rights, it clearly cannot explain the entirety of variance for a country's level of either democracy or human rights.

As for the second hypothesis, which predicts that tribunals will be more effective than other types of TJMs, the results are slightly mixed. While tribunals remained significant throughout the regression models, it is important to note that reparations were, at times, a more significant predictor for levels of human rights. Additionally, reparations enjoyed similar levels of significance and strength at nearly all levels of the correlation tests. So, while it may be fair to say that there is general support for the second hypothesis, it may be appropriate to give a slightly more nuanced understanding of the hypothesis. For example, the evidence presented in this paper seems to support the idea that while tribunals may be the most effective TJM for strengthening democracy, reparations appear to be at least as effective, if not more, than tribunals at cultivating a respect for human rights. Regardless, it is helpful to note that the results presented in this paper do lend general support for the conclusions reached in Frederking and Aviles (2019) by supporting the idea that "justice" mechanisms are more effective than "truth" mechanisms. In fact, no "truth" mechanisms were statistically significant enough to be included in any of the multiple regression models.

Another interesting finding taken from the results presented in this paper is the importance of the economic development for democracy. The significance of GDP per capita in the democracy regressions does seem to lend limited support to the literature that links economic development with democracy. The relationship is not strong enough to preclude other factors, like the use of tribunals, but the results do show that economic development may likely be an important piece of the puzzle. It would be interesting to see whether the role of economic development would increase or decrease if different indicators of the economy were used rather

than a simple GDP per capita number. Despite the significance of economic development in the democracy regressions, the fact that GDP per capita does not merit inclusion in the regressions for human rights is notable since its exclusion helps lend additional support for the effectiveness of TJMs.

Before moving on to the conclusion and implications of this research, I do think it is important to take a moment to clearly address some of the limitations of this paper. First and foremost, the limited number of cases available presents inherent problems in using the quantitative methods employed in this paper. For example, while reparations show a strong significance in the correlation and regression models, this could be due to the fact that there were less than 20 states that used reparations; such a limited number of cases is likely to show a variable as statistically significant when the opposite may be true. Second, I already noted in this paper that the importance of economic development might change if one were to use a measurement other than GDP per capita. While the use of GDP per capita was necessary for the purposes of this paper, I would encourage the use of more sophisticated indicators for economic development in future research. Additionally, the dependent variables, democracy and human rights, are measured using indices that largely focus on the characteristics of the government, not the people. Polity IV, for example, is measured primarily by the level of checks on the executive branch of a government, and the CIRI Human Rights Index solely focuses on a person's right not to be imprisoned, tortured or executed by the government because of one's political beliefs. It is entirely fair to say that these two measurements alone do not wholly reflect the spectrum of what democracy and human rights are defined as, and they certainly fail to capture how TJMs affect the healing of individuals. Consequently, I think it would be more appropriate for future research to study democracy and human rights in separate endeavors in order to allow for a more

nuanced understanding of the effects that TJMs have. While I do not think these three main limitations are significant enough to inherently threaten the results of this paper, I do think that accounting for them in future research will yield more conclusive results.

Ultimately, the results in this paper seem to yield themselves as natural jumping off points for future research rather than as conclusions in and of themselves. Future research, while taking into account the limitations previously mentioned, should continue to develop in two directions. First, while this paper treats all tribunals as a single category, it may be helpful to begin digging into these results to determine which types of tribunals work best. For example, differentiating between international tribunals, domestic tribunals, or hybrids will likely yield important distinctions to be made within the category of tribunals. Second, it would be interesting for future research to address what enables or pushes a country to choose tribunals in the first place. This paper specifically looks at the effects of employing a tribunal mechanism, but it would be interesting to see what conditions are likely to push a country to seek justice in the first place. Regardless of what the results in this paper suggest, it is clear that future research is vital to increasing our understanding of these mechanisms.

Conclusion

Do transitional justice mechanisms help build democracy and cultivate an increased respect for human rights? Do people have to answer for the crimes committed under an old regime or during a civil war? The results presented in this paper allow us to give a tempered “yes” to answer both of those questions. This paper empirically shows that there is at least some evidence supporting the idea that tribunals are not only effective at increasing levels of democracy and human rights, but that tribunals are more effective than states that choose to use amnesty policies or truth commissions. These results help to further the truth vs justice debate

by supporting the claim that justice mechanisms, like tribunals or reparations, will be more effective than truth mechanisms, which are often more widely used. However, it is important to note that the results of this paper do not connote an end to the truth vs justice debate, and more empirical studies are needed in order to confirm the results presented in this paper. By confirming these results, scholars and policymakers will be more informed about these mechanisms and effectively be able to use them to build democracy and respect for human rights in states that have recently ended a civil war or toppled an authoritarian regime. These mechanisms have, up until now, largely been confined to South/Central America and Africa; however, it does not take an active imagination to see the future importance of transitional justice in places like the Middle East, especially in the wake of the Arab Spring. In the process of writing this paper, Algeria's President Bouteflika was ousted by pro-democracy protests and Sudan's President Bashir was kicked out by similar calls for democracy, and we continue to wonder how long Maduro can hold on in Venezuela. Getting through a civil war or toppling an authoritarian regime is only half of the battle; it is how states choose to move on that can impact where they will end up. This paper, like others in its field, seeks to make the latter choice easier by identifying the transitional justice mechanisms that can both ensure progress and help avoid a return of conflict or authoritarian regime.

Works Cited

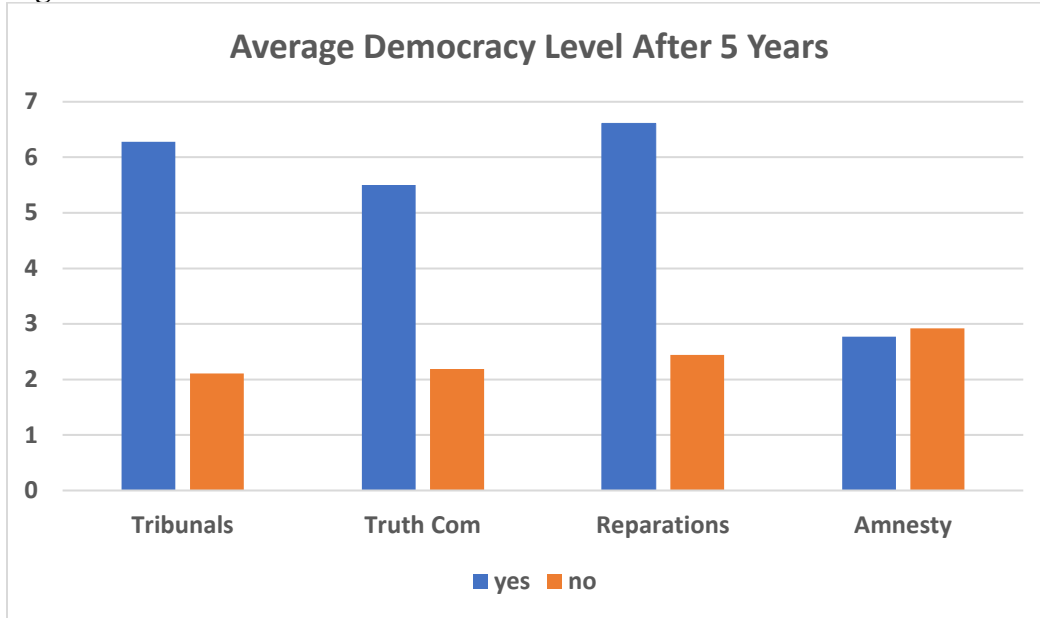
- Andersen, Elizabeth. "Transitional Justice and the Rule of Law: Lessons from the Field." *Case W. Res. J. Int'l L.* 47 (2015): 305.
- Arthur, Paige. "How Transitions Reshaped Human Rights: A Conceptual History of Transitional Justice." *Hum. Rts. Q.* 31 (2009): 321.
- Brahm, Eric. "Uncovering the Truth: Examining Truth Commission Success and Impact." *International Studies Perspectives* 8.1 (2007): 16-35.
- Burkhart, Ross E., and Michael S. Lewis-Beck. "Comparative Democracy: The Economic Development Thesis." *American Political Science Review* 88.4 (1994): 903-910.
- David, Roman. "Transitions to Clean Government: Amnesty as an Anticorruption Measure." *Australian Journal of Political Science* 45.3 (2010): 391-406.
- David, Roman. "What We Know About Transitional Justice: Survey and Experimental Evidence." *Political Psychology* 38 (2017): 151-177.
- Frederking, Brian K. "Putting Transitional Justice on Trial: Democracy and Human Rights in Post-Civil War Societies." *International Social Science Review* 91.1 (2015): 3.
- Frederking, Brian K and Max Aviles. "Human Rights After Civil Wars: The Record of Transitional Justice Mechanisms." *Emerging Threats to Human Rights: Resources, Violence and Deprivation of Citizenship*, edited by Heather Smith-Cannoy. Temple University Press. 2019.
- Gibson, James L. "The Contributions of Truth to Reconciliation: Lessons from South Africa." *Journal of conflict resolution* 50.3 (2006): 409-432.
- Gready, Paul, and Simon Robins. "From Transitional to Transformative Justice: A New Agenda for Practice." *International Journal of Transitional Justice* 8.3 (2014): 339-361.

- Guercio, Laura. "International Justice and Politics in Libya: A Lost Opportunity for the International Criminal Court and for a Lasting Peace in Libya." *Juridical Current* 18.1 (2015).
- Huber, Evelyne, Dietrich Rueschemeyer, and John D. Stephens. "The Impact of Economic Development on Democracy." *Journal of economic perspectives* 7.3 (1993): 71-86.
- Inazu, John D. "No Future Without (Personal) Forgiveness: Reexamining the Role of Forgiveness in Transitional Justice." *Human Rights Review* 10.3 (2009): 309-326.
- Lambourne, Wendy. "Transitional Justice and Peacebuilding After Mass Violence." *International Journal of Transitional Justice* 3.1 (2009): 28-48.
- Loyle, Cyanne E., and Christian Davenport. "Transitional injustice: Subverting Justice in Transition and Post-Conflict Societies." *Journal of Human Rights* 15.1 (2016): 126-149.
- Mendeloff, David. "Trauma and Vengeance: Assessing the Psychological and Emotional Effects of Post-Conflict Justice." *Hum. Rts. Q.* 31 (2009): 592.
- Mendeloff, David. "Truth-Seeking, Truth-Telling, and Post-Conflict Peacebuilding: Curb the Enthusiasm?" *International studies review* 6.3 (2004): 355-380.
- Mihai, Mihaela. "Transitional Justice and the Quest for Democracy: A Contribution to a Political Theory of Democratic Transformations." *Ratio Juris* 23.2 (2010): 183-204.
- Minow, Martha. "Making History or Making Peace: When Prosecutions Should Give Way to Truth Commissions and Peace Negotiations." *Journal of Human Rights* 7.2 (2008): 174-185.
- Olsen, Tricia D., Leigh A. Payne, and Andrew G. Reiter. "The Justice Balance: When Transitional Justice Improves Human Rights and Democracy." *HuM. RTs. Q.* 32 (2010): 980.

- Olsen, Tricia D., Leigh A. Payne, and Andrew G. Reiter. "Transitional Justice in the World, 1970-2007: Insights from a New Dataset." *Journal of Peace Research* 47.6 (2010): 803-809.
- Rebecca, Rowen Jamie. "'We Don't Believe in Transitional Justice:' Peace and the Politics of Legal Ideas in Colombia." *Law & Social Inquiry* 42.3 (2017): 622-647.
- Samii, Cyrus. "Who Wants to Forgive and Forget? Transitional Justice Preferences in Postwar Burundi." *Journal of Peace Research* 50.2 (2013): 219-233.
- Skaar, Elin. "Truth Commissions, Trials-or Nothing? Policy Options in Democratic Transitions." *Third world quarterly* 20.6 (1999): 1109-1128.
- Taylor, Laura K., and Alexander Dukalskis. "Old Truths and New Politics: Does Truth Commission 'Publicness' Impact Democratization?" *Journal of Peace Research* 49.5 (2012): 671-684.
- Thoms, Oskar NT, James Ron, and Roland Paris. "State-Level Effects of Transitional Justice: What Do We Know?." *International Journal of Transitional Justice* 4.3 (2010): 329-354.
- Trejo, Guillermo, Juan Albarracín, and Lucía Tiscornia. "Breaking State Impunity in Post-Authoritarian Regimes: Why Transitional Justice Processes Deter Criminal Violence in New Democracies." *Journal of Peace Research* 55.6 (2018): 787-809.
- Valiñas, Marta, and Kris Vanspauwen. "Truth-Seeking After Violent Conflict: Experiences from South Africa and Bosnia and Herzegovina." *Contemporary Justice Review* 12.3 (2009): 269-287.
- Wilson, Stuart. "The Myth of Restorative Justice: Truth, Reconciliation and the Ethics of Amnesty." *South African Journal on Human Rights* 17.4 (2001): 531-562.

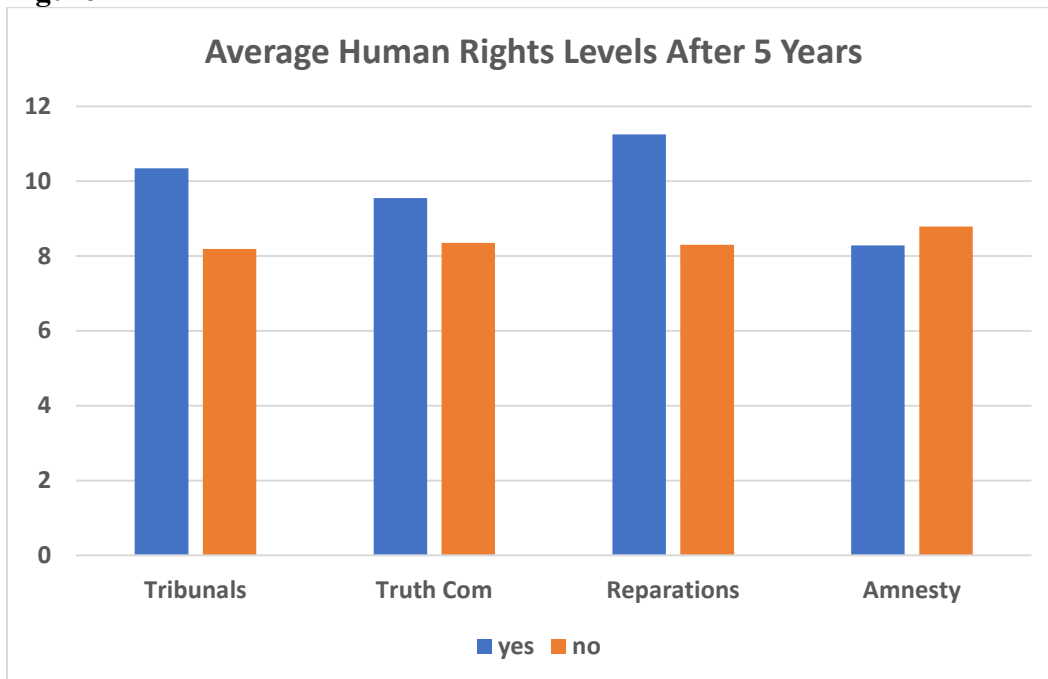
Appendix

Figure 1



**Democracy as measured by Polity IV democracy index*

Figure 2



**Level of human rights is measured by CIRI Human Rights Index*

Table 1**TJMs and Democracy: Bivariate Correlation**

		Polity +5	Polity +10
Tribunals	Pearson Correlation	.270	.285
	Sig. (2-tailed)	.000	.000
Truth Com.	Pearson Correlation	.224	.157
	Sig. (2-tailed)	.003	.050
Reparations	Pearson Correlation	.212	.182
	Sig. (2-tailed)	.005	.022
Amnesty	Pearson Correlation	-.012	-.016
	Sig. (2-tailed)	.874	.846

Table 2**TJMs and Human Rights: Bivariate Correlation**

		CIRI +5	CIRI +10
Tribunals	Pearson Correlation	.246	.251
	Sig. (2-tailed)	.003	.002
Truth Com.	Pearson Correlation	.144	.131
	Sig. (2-tailed)	.081	.116
Reparations	Pearson Correlation	.265	.233
	Sig. (2-tailed)	.001	.005
Amnesty	Pearson Correlation	-.069	-.027
	Sig. (2-tailed)	.404	.745

Table 3**TJMs and Democracy: Control for Economic Development**

		Polity +5	Polity +10
Tribunals	Pearson Correlation	0.237	0.238
	Sig. (2-tailed)	0.003	0.003

Table 4**TJMs and Human Rights: Control for Economic Development**

		CIRI +5	CIRI +10
Tribunals	Pearson Correlation	0.223	0.221
	Sig. (2-tailed)	0.007	0.008

Table 5
Multiple Linear Regression for Polity IV After 5 Years

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.270 ^a	.073	.067	5.703
2	.322 ^b	.103	.093	5.624

a. Predictors: (Constant), did country use tribunals

b. Predictors: (Constant), did country use tribunals, gdp_five

ANOVA^a

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	438.596	1	438.596	13.486	.000 ^p
	Residual	5593.708	172	32.522		
	Total	6032.305	173			
2	Regression	624.036	2	312.018	9.865	.000 ^c
	Residual	5408.269	171	31.627		
	Total	6032.305	173			

a. Dependent Variable: Polity IV democracy scale five years after transition

b. Predictors: (Constant), did country use tribunals

c. Predictors: (Constant), did country use tribunals, gdp_five

Coefficients^a

Model		Unstandardized Coefficients		Standardized Coefficients	T	Sig.	Collinearity Statistics	
		B	Std. Error	Beta			Tolerance	VIF
1	(Constant)	2.183	.479		4.562	.000		
	did country use tribunals	4.098	1.116	.270	3.672	.000	1.000	1.000
2	(Constant)	1.648	.521		3.162	.002		
	did country use tribunals	3.573	1.122	.235	3.185	.002	.963	1.039
	gdp_five	.000	.000	.179	2.421	.017	.963	1.039

a. Dependent Variable: Polity IV democracy scale five years after transition

Table 6
Multiple Linear Regression for Polity IV After 10 Years

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.310 ^a	.096	.090	5.615
2	.384 ^b	.147	.136	5.471

a. Predictors: (Constant), gdp_ten

b. Predictors: (Constant), gdp_ten, did country use tribunals

ANOVA^a

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	512.919	1	512.919	16.269	.000 ^b
	Residual	4823.558	153	31.527		
	Total	5336.477	154			
2	Regression	786.761	2	393.381	13.142	.000 ^c
	Residual	4549.716	152	29.932		
	Total	5336.477	154			

a. Dependent Variable: Polity IV democracy scale ten years after transition

b. Predictors: (Constant), gdp_ten

c. Predictors: (Constant), gdp_ten, did country use tribunals

Coefficients^a

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.	Collinearity Statistics	
		B	Std. Error	Beta			Tolerance	VIF
1	(Constant)	2.225	.545		4.084	.000		
	gdp_ten	.000	.000	.310	4.034	.000	1.000	1.000
2	(Constant)	1.724	.556		3.100	.002		
	gdp_ten	.000	.000	.262	3.418	.001	.957	1.045
	did country use tribunals	3.358	1.110	.232	3.025	.003	.957	1.045

a. Dependent Variable: Polity IV democracy scale ten years after transition

Table 7
Multiple Linear Regression for CIRI After 5 Years

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.267 ^a	.071	.065	3.301
2	.330 ^b	.109	.096	3.245

a. Predictors: (Constant), did country use reparation

b. Predictors: (Constant), did country use reparation, did country use tribunals

ANOVA^a

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	119.169	1	119.169	10.934	.001 ^b
	Residual	1558.597	143	10.899		
	Total	1677.766	144			
2	Regression	182.234	2	91.117	8.652	.000 ^c
	Residual	1495.532	142	10.532		
	Total	1677.766	144			

a. Dependent Variable: CIRI human rights scale five years after transition

b. Predictors: (Constant), did country use reparation

c. Predictors: (Constant), did country use reparation, did country use tribunals

Coefficients^a

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.	Collinearity Statistics	
		B	Std. Error	Beta			Tolerance	VIF
1	(Constant)	8.357	.291		28.749	.000		
	did country use reparation	2.893	.875	.267	3.307	.001	1.000	1.000
2	(Constant)	8.069	.309		26.118	.000		
	did country use reparation	2.443	.880	.225	2.778	.006	.956	1.046
	did country use tribunals	1.686	.689	.198	2.447	.016	.956	1.046

a. Dependent Variable: CIRI human rights scale five years after transition

Table 8
Multiple Linear Regression for CIRI After 10 Years

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.260 ^a	.067	.061	3.124
2	.322 ^b	.104	.091	3.073

a. Predictors: (Constant), did country use tribunals

b. Predictors: (Constant), did country use tribunals, did country use reparation

ANOVA^a

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	100.969	1	100.969	10.346	.002 ^b
	Residual	1395.624	143	9.760		
	Total	1496.593	144			
2	Regression	155.284	2	77.642	8.220	.000 ^c
	Residual	1341.309	142	9.446		
	Total	1496.593	144			

a. Dependent Variable: CIRI human rights scale ten years after transition

b. Predictors: (Constant), did country use tribunals

c. Predictors: (Constant), did country use tribunals, did country use reparation

Coefficients^a

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.	Collinearity Statistics	
		B	Std. Error	Beta			Tolerance	VIF
1	(Constant)	8.363	.294		28.456	.000		
	did country use tribunals	2.012	.626	.260	3.216	.002	1.000	1.000
2	(Constant)	8.218	.295		27.819	.000		
	did country use tribunals	1.709	.628	.221	2.719	.007	.959	1.042
	did country use reparation	2.052	.856	.194	2.398	.018	.959	1.042

a. Dependent Variable: CIRI human rights scale ten years after transition