1 Amnesty Operationalization

Some scholars have questioned whether it is useful to observe cumulative amnesty laws, rather than a binary measure of the presence or absence of any amnesty law. Theoretically, there are merits in using the measure we are using, which does capture iterative legislative processes in quite a few countries (of the 587 country-years with at least one amnesty, 179 country-years have more than one amnesty, and 408 have only one). But there are also merits in using a binary measure. On the side of the binary measure, one important amnesty (like Brazil 1979 self-amnesty law) can have the same effect as many amnesties in another case.

The concern with using only a binary measure is methodological. When the binary variable is included in fixed-effects regression, nearly all of the variation goes away. Fixed-effects models are looking for changes within countries, and then comparing those to similar changes in other countries. Because so many amnesties happen before the transition, or soon following the transition, most of the data panels we are studying do not have any variation on a binary amnesty variable. For this reason, the fixed effects model does not tell us much about what happens to human rights outcomes after amnesties are passed. (587 country-years in our dataset have at least one amnesty. But there are only 80 observations in 15 countries that go from no amnesty to amnesty. In the fix effects model, those 80 observations serve as the baseline, against 240 observations that follow passage of an amnesty. In short, using the binary, we are comparing a very small subset of our total cases.)

2 Democratic Transitions

Table 1: Democratic Transitions and Reversions.

Country	COWID	Region	Spell	Start	End	War	Nego
Albania	339	Euro	democratic transition	1990	1995	0	1
Albania	339	Euro	autocratic reversion	1996	1996	-	-
Albania	339	Euro	democratic transition	1997	2010	0	1
Algeria	615	MENA	democratic transition	2004	2010	1	1
Argentina	160	Amer	democratic transition	1973	1975	1	1
Argentina	160	Amer	autocratic reversion	1976	1982	-	-
Argentina	160	Amer	democratic transition	1983	2010	0	0
Armenia	371	Euro	democratic transition	1991	1995	0	0
Armenia	371	Euro	autocratic reversion	1996	1997	-	-
Armenia	371	Euro	democratic transition	1998	2010	0	0
Azerbaijan	373	Euro	democratic transition	1992	1992	1	0
Azerbaijan	373	Euro	autocratic reversion	1993	2010	-	-
Bangladesh	771	Asia	democratic transition	1972	1973	1	0
Bangladesh	771	Asia	autocratic reversion	1974	2008	-	-
Bangladesh	771	Asia	democratic transition	1991	2006	1	0
Bangladesh	771	Asia	autocratic reversion	2007	2008	-	-
Bangladesh	771	Asia	democratic transition	2009	2010	0	0
Belarus	370	Euro	democratic transition	1991	1995	0	0
Belarus	370	Euro	autocratic reversion	1996	2010	-	-
Benin	434	Africa	democratic transition	1990	2010	0	1
Bolivia	145	Amer	democratic transition	1982	2010	0	1
Brazil	140	Amer	democratic transition	1985	2010	0	1
Bulgaria	355	Euro	democratic transition	1990	2010	0	1
Burkina Faso	439	Africa	democratic transition	1977	1979	1	1
Burkina Faso	439	Africa	autocratic reversion	1980	2010	-	-
Burundi	516	Africa	democratic transition	2005	2010	1	1
C Af Republic	482	Africa	democratic transition	1991	2002	1	1

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Table 1 – continued from previous page

Country	COWID	Region	Spell	Start	End	War	Nego
C Af Republic	482	Africa	autocratic reversion	2003	2010	-	-
Cambodia	811	Asia	democratic transition	1988	1996	1	1
Cambodia	811	Asia	autocratic reversion	1997	1997	-	-
Cambodia	811	Asia	democratic transition	1998	2010	1	0
Chile	155	Amer	democratic transition	1989	2010	0	1
Congo (Brazzaville)	484	Africa	democratic transition	1991	1996	1	1
Congo (Brazzaville)	484	Africa	autocratic reversion	1997	2010	-	-
Cote d'Ivoire	437	Africa	democratic transition	1999	2002	1	0
Cote d'Ivoire	437	Africa	autocratic reversion	2003	2010	-	-
Croatia	344	Euro	democratic transition	1999	2010	0	0
Czechoslovakia	315	Euro	democratic transition	1989	1992	0	1
Czech Republic	315	Euro	democratic transition	1993	2010	0	0
Dominican Rep	42	Amer	democratic transition	1978	2010	0	0
DR Congo	490	Africa	democratic transition	2004	2010	1	1
Ecuador	130	Amer	democratic transition	1979	2010	0	1
El Salvador	92	Amer	democratic transition	1982	2010	1	1
Estonia	366	Euro	democratic transition	1991	2010	0	0
Ethiopia	530	Africa	democratic transition	1994	2010	1	0
Gabon	481	Africa	democratic transition	2009	2010	0	1
Georgia	372	Euro	democratic transition	1991	2010	1	0
Germany	255	Euro	democratic transition	1990	2010	0	0
Ghana	452	Africa	democratic transition	1970	1971	0	0
Ghana	452	Africa	autocratic reversion	1972	1977	-	-
Ghana	452	Africa	democratic transition	1978	1980	1	1
Ghana	452	Africa	autocratic reversion	1981	1995	-	-
Ghana	452	Africa	democratic transition	1996	2010	0	1
Greece	350	Euro	democratic transition	1974	2010	0	0
Guatemala	90	Amer	democratic transition	1986	2010	1	1

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Table 1 – continued from previous page

Country	COWID	Region	Spell	Start	End	War	Nego
Guinea-Bissau	404	Africa	democratic transition	1991	2002	1	1
Guinea-Bissau	404	Africa	autocratic reversion	2003	2004	-	-
Guinea-Bissau	404	Africa	democratic transition	2005	2010	0	1
Haiti	41	Amer	democratic transition	1990	1990	1	1
Haiti	41	Amer	autocratic reversion	1991	1993	-	-
Haiti	41	Amer	democratic transition	1994	1999	0	0
Haiti	41	Amer	autocratic reversion	2000	2003	-	-
Haiti	41	Amer	democratic transition	2004	2010	1	0
Honduras	91	Amer	democratic transition	1980	2010	0	1
Hungary	310	Euro	democratic transition	1989	2010	0	1
Indonesia	850	Asia	democratic transition	1999	2010	1	1
Iran	630	MENA	democratic transition	1997	2003	1	1
Iran	630	MENA	autocratic reversion	2004	2010	-	-
Kenya	501	Africa	democratic transition	2002	2010	0	1
Kosovo	347	Euro	democratic transition	2008	2010	0	0
Kyrgyzstan	703	Asia	democratic transition	2005	2010	0	0
Latvia	367	Euro	democratic transition	1991	2010	0	0
Lebanon	660	MENA	democratic transition	2005	2010	0	0
Lesotho	570	Africa	democratic transition	1993	2010	1	1
Liberia	450	Africa	democratic transition	2003	2010	1	1
Lithuania	368	Euro	democratic transition	1991	2010	0	0
Macedonia	343	Euro	democratic transition	1991	2010	1	0
Madagascar	580	Africa	democratic transition	1991	2010	0	1
Malawi	553	Africa	democratic transition	1994	2010	0	1
Mali	432	Africa	democratic transition	1991	2010	1	0
Mauritania	435	Africa	democratic transition	2007	2007	1	0
Mauritania	435	Africa	autocratic reversion	2008	2010	-	-
Mexico	70	Amer	democratic transition	1994	2010	1	1

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Table 1 – continued from previous page

Country	COWID	Region	Spell	Start	End	War	Nego
Moldova	359	Euro	democratic transition	1991	2010	1	0
Mongolia	712	Asia	democratic transition	1990	2010	0	1
Mozambique	541	Africa	democratic transition	1994	2010	0	1
Nepal	790	Asia	democratic transition	1990	2001	1	1
Nepal	790	Asia	autocratic reversion	2002	2005	-	-
Nepal	790	Asia	democratic transition	2006	2010	1	1
Nicaragua	93	Amer	democratic transition	1990	2010	1	1
Niger	436	Africa	democratic transition	1991	1995	1	1
Niger	436	Africa	autocratic reversion	1996	1998	-	-
Niger	436	Africa	democratic transition	1999	2008	1	0
Niger	436	Africa	autocratic reversion	2009	2009	-	-
Niger	436	Africa	democratic transition	2010	2010	0	0
Nigeria	475	Africa	democratic transition	1978	1983	0	1
Nigeria	475	Africa	autocratic reversion	1984	1998	-	-
Nigeria	475	Africa	democratic transition	1999	2010	1	0
Pakistan	770	Asia	democratic transition	1973	1976	1	0
Pakistan	770	Asia	autocratic reversion	1977	1987	-	-
Pakistan	770	Asia	democratic transition	1988	1998	1	1
Pakistan	770	Asia	autocratic reversion	1999	2006	-	-
Pakistan	770	Asia	democratic transition	2007	2010	1	1
Panama	95	Amer	democratic transition	1989	2010	1	0
Paraguay	150	Amer	democratic transition	1989	1991	1	0
Paraguay	151	Amer	democratic transition	1992	2010	1	0
Peru	135	Amer	democratic transition	1979	1991	1	1
Peru	135	Amer	autocratic reversion	1993	1999	-	-
Peru	135	Amer	democratic transition	2000	2010	1	0
Philippines	840	Asia	democratic transition	1986	2010	1	0
Poland	290	Euro	democratic transition	1989	2010	0	1

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Table 1 – continued from previous page

Country	COWID	Region	Spell	Start	End	War	Nego
Portugal	235	Euro	democratic transition	1974	2010	0	0
Romania	360	Euro	democratic transition	1989	2010	1	0
Russia	365	Euro	democratic transition	1992	2010	1	1
Senegal	433	Africa	democratic transition	2000	2010	1	1
Serbia	345	Euro	democratic transition	2000	2010	0	0
Sierra Leone	451	Africa	democratic transition	2001	2010	0	0
Slovakia	317	Euro	democratic transition	1993	2010	0	0
Slovenia	349	Euro	democratic transition	1992	2010	0	0
South Africa	560	Africa	democratic transition	1992	2010	0	1
South Korea	732	Asia	democratic transition	1987	2010	0	1
Spain	230	Euro	democratic transition	1975	2010	1	1
Sudan	625	MENA	democratic transition	1985	1988	1	1
Sudan	625	MENA	autocratic reversion	1989	2010	-	-
Taiwan	713	Asia	democratic transition	1992	2010	0	1
Thailand	800	Asia	democratic transition	1974	1975	1	1
Thailand	800	Asia	autocratic reversion	1976	1977	-	-
Thailand	800	Asia	democratic transition	1978	1990	1	1
Thailand	800	Asia	autocratic reversion	1991	1991	-	-
Thailand	801	Asia	democratic transition	1992	2005	1	1
Thailand	802	Asia	autocratic reversion	2006	2007	-	-
Thailand	800	Asia	democratic transition	2008	2010	1	1
Timor-Leste	860	Asia	democratic transition	2002	2010	0	0
Turkey	640	MENA	democratic transition	1973	1979	0	1
Turkey	640	MENA	autocratic reversion	1980	1982	-	-
Turkey	640	MENA	democratic transition	1983	2010	1	0
Uganda	500	Africa	democratic transition	1980	1985	1	0
Uganda	500	Africa	autocratic reversion	1986	2010	-	-
Ukraine	369	Euro	democratic transition	1991	2010	0	1

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Table 1 – continued from previous page

Country	COWID	Region	Spell	Start	End	War	Nego
Uruguay	165	Amer	democratic transition	1985	2010	0	1
Zambia	551	Africa	democratic transition	1991	2010	0	1
Zimbabwe	552	Africa	democratic transition	2009	2010	0	1

3 Variable Description

3.1 CIRI Scales

Differences in the relationship between transitional justice mechanisms and the outcome variables would mean that the non-overlapping spaces between the CIRI PHYSINT and EMPINX scales are driving the findings. This is an ideal test for our hypotheses. The non-overlapping space for EMPINX measures the degree to which elected legislators are willing to legally restrict open participation, competition, and representation. If amnesties strengthen democratic bargains between political opponents, and create the space for governments to welcome competition, then they should be associated with higher EMPINX scores. The non-overlapping space for PHYSINT represents arbitrary violence and abuse by state agents. If prosecutions deter arbitrary violence, we should observe a correlation between more prosecutions and cost truth commissions, and greater PHYSINT scores.

3.2 Cumulative Coding of Transitional Justice Mechanisms

To capture the difference in transitional justice policy effects between the immediate and the long-term, this paper employs a strategy of cumulative counting in the construction of variables. For each count variable in the dataset measuring the number of transitional justice policy events that happened in any given country-year, there is a cumulative count. For example, if the data indicates that the first two guilty verdicts were handed down in Argentina in 2005, and then two more in 2006, then the cumulative count (Sum Guilty) would be 0 in 2004, 2 in 2005, and 4 in 2006. If no more guilty verdicts were handed down after 2006, the Sum Guilty variable would retain a score of 4 through the remainder of the transition panel. See Table 2 for an illustration. Table 3 provides a description of all count and summed variables used in this paper.

Table 2: Cumulative Counting Illustration

	Year	Dom Pros	Dom Pros Cum	Amnesties	Am Cum
Transition X	1990	0	0	1	1
Transition X	1991	1	1	0	1
Transition X	1991	3	4	0	1
Transition X	1992	5	9	0	1
Transition X	1993	1	10	0	1
Transition X	1994	1	11	0	1
Transition X	1995	0	11	0	1
Transition X	1996	0	11	2	3
Transition X	1997	0	11	0	3
Transition X	1998	0	11	0	3
Transition X	1999	0	11	0	3
Transition X	2000	0	11	0	3

Table 3: Summary of Variables

	count	mean	sd	min	max
PHYSINT	1706	4.593787	2.106197	0	8
EMPINX	1708	5.294078	1.871518	0	8
One-Sided Violence	1761	.1862578	.6164909	0	9
Prosecutions	1667	.3077385	.9766574	0	10
Amnesty	1667	.0257948	.1798539	0	3
Prosecutions	1667	3.44751	6.087604	0	54
Guilty Verdicts	1667	1.634673	3.115862	0	25
Amnesties (Sum)	1667	.6094781	1.250875	0	8
Prosecutions * Amnesties	1667	6.25135	28.58434	0	432
Guilty * Amnesties	1667	2.80024	12.23672	0	192
Previous HR Protections	1740	-.6422784	.7972093	-2.701097	1.814949
Regional Prosecutions	1740	.6315166	–	0	1
Rupture	1761	.4900625	–	0	1
Civil War	1736	.2010369	–	0	1
Judicial Ind (LinzStaton)	1758	.4860037	.2317029	.0437	.9788
Judicial Ind (LC Keith)	1640	.845122	.796829	0	2
Years Since Trans	1761	11.26065	7.525463	1	37
Polity II	1761	5.202158	4.888764	-9	10
GDP pc (ln)	1697	7.111239	1.32884	4.480213	10.15113
Population (ln)	1716	16.40218	1.267823	13.70945	19.29896
Observations	1761				

4 Model Choice

Using fixed-effects regression allows us to explore the relationship between predictor and outcome variables within each transition, based on the assumption that each transition has its own individual characteristics that may influence its use of justice policies and its protection of human rights. Because the dependent variable is an interval measure, a fixed-effects ordered logit would be an ideal model. We choose regressions instead because they are easier to interpret and visualize based on conventional methods. As a robustness check, we compared our results to those produced by fixed-effects ordered logits. None of the findings are altered. The equation for our general fixed-effects model assumes the following form:

$$Y_{it} = \beta_1 Y_{it-1} + \beta_2 X_{it-1} + \beta_3 Z_{it-1} + \alpha_{i=1\ldots n} + \mu_{it} \tag{1}$$

where: Y_{it} is the dependent variable (i=transition and t=time); Y_{it-1} is the lagged dependent variable; X_{it} is a matrix of control variables; $Z_{1,it}$ is a matrix of transitional justice variables of interest; β_{1-3} are the coefficients for the controls and independent variables of interest; $\alpha_{i=1\ldots n}$ is the unknown intercept for each transition (n transition-specific intercepts); and μ_{it} is the error term.

One assumption of a fixed-effects model is that the individual transition's characteristics are unique and uncorrelated with the other transitions' error terms. If they are correlated, then one should use a random-effects model. We employ a Hausman test between fixed-effects and random-effects models and find a systematic difference between the coefficients produced; following convention, we therefore choose a fixed-effects model.

5 Matching

To achieve a valid estimate of a variable's effects, it's effects should be observed between treatment and control groups that are "balanced." This means that the groups possess the same mean values of all covariates. In a situation of perfect balance, one can simply perform a difference in means test to assess the effects of treatment. However, in observational data, perfect balance of this kind rarely exists. CEM can be used to create more balance between groups by (1) generating matched strata that share the same levels of different pre-treatment characteristics (2) "pruning" observations that are not matched and (3) using the number of treated and untreated observations in each strata to create weights for further analysis (Blackwell et al. 2009).

CEM is an alternative strategy to two-stage selection models, which not only require a set of assumptions about variable distributions that are unwarranted in this case, but also introduce a series of identification issues that are difficult to resolve (Heckman 1976; Simmons and Hopkins 2005). For instance, it might be that an adequate instrumental variable for human rights prosecutions or amnesties is not available.

Table 4 reports diagnostics on the matching procedure for the models considering physical integrity violations. Matching factors are chosen because they might feasibly affect both the choice to engage in prosecutions, and ultimately, the repressive practices that governments engage in. The treatment is the existence of human rights prosecution in any given country-transition-year. One can see that prior to matching, there is a good deal of imbalance between treatment and control groups on mean values of variables predicting selection into treatment. The CEM matching procedure works well. Imbalance is almost entirely zero after the matching procedure is conducted. Based on these four variables, the data are grouped into 40 strata, 34 of which are matched. Only 119 of 1,834 observations are unmatched.

Overal, matching models predicting physical integrity protections perform very well. In each, around 75% of variation in the dependent variable is explained by independent variables. A sizeable portion of the R-squared is explained by the inclusion of the lagged dependent variable, but the inclusion of additional variables adds explanatory power to the models. Many of the control variables—like Judicial Ind, Polity II, and Civil War are associated with human rights outcomes, as is commonly discovered in the literature on repression.

Table 4: Matching Diagnostics for PHYSINT Models

Standardized Differences in Covariate Means

Covariate	**Before Matching**	**After Matching**
Previous HR Protection	-0.28	0.00
Regional Prosecutions	0.14	0.00
Rupture	0.03	0.00
Judicial Independence	0.07	0.00
Strata = 40	Matched Strata = 34	
Matched Obs = 1715	Unmatched Obs = 119	

Table 5 reports the results of the matching procedure for those models considering civil and political rights. Matching factors are chosen because they might feasibly affect the choice to pass amnesty laws. Here, Civil War is substituted in for Judicial Independence, used above, because we discovered that Judicial Independence is in no way statistically correlated with the use of amnesties. Civil war is. The matching procedure again works well. It generates 21 strata, 18 of which are matched. Only 29 observations are unmatched, and thus pruned from the analysis. The imbalance between the strata is reduced to a near-zero value.

Models 8-12, which predict changes in the protection in civil and political rights measured by CIRI's EMPINX index, perform very well. For these models, we repeat the same steps as the previous five, and achieve R-squared measures over .80.

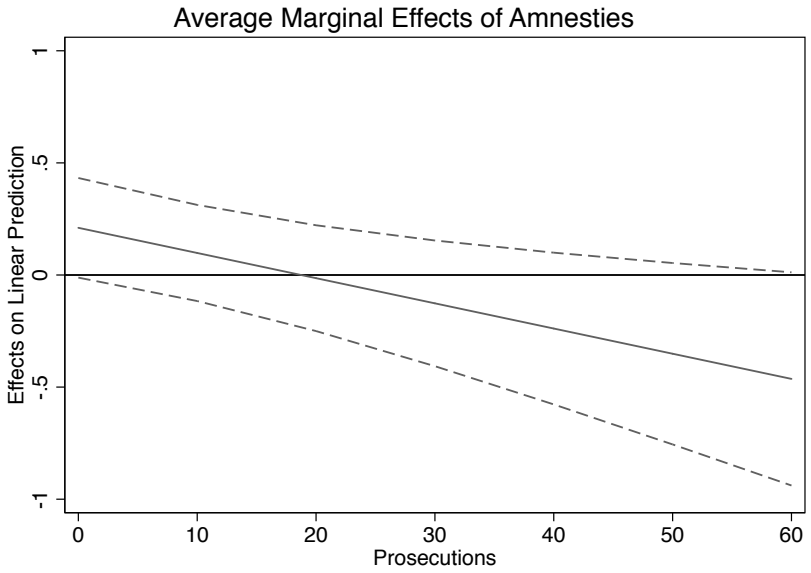
Table 5: Matching Diagnostics for EMPINX Models

Standardized Differences in Covariate Means

Covariate	**Before Matching**	**After Matching**
Previous HR Protection	-0.23	0.00
Regional Prosecutions	0.08	0.00
Rupture	-0.15	0.00
Civil War	-0.03	0.00
Strata = 21	Matched Strata = 18	
Matched Obs = 1805	Unmatched Obs = 29	

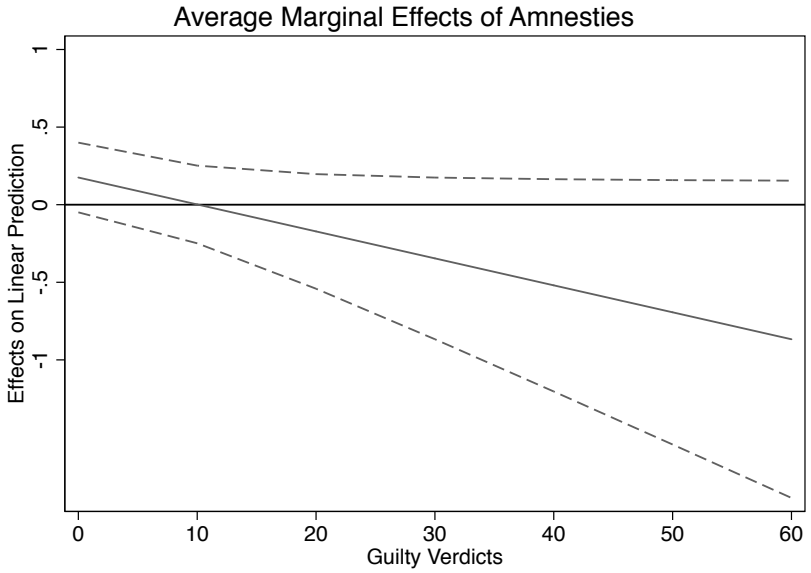
6 Interactions and Marginal Effects

Figure 1: Effects of Model 5 Interaction Term between Amnesties and Prosecution on PHYSINT



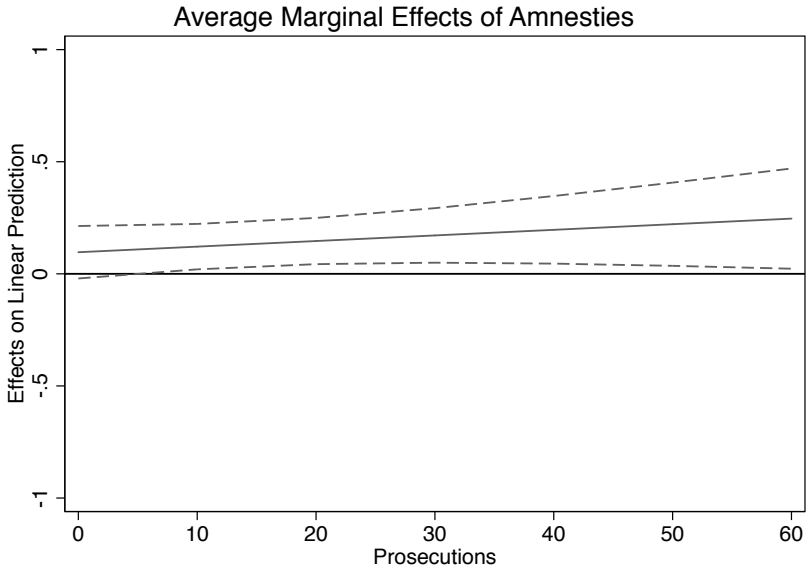
Note: Depicted are the average marginal effects of each term, conditional on its interacting variable, with 95% confidence intervals.

Figure 2: Effects of Model 7 Interaction Term between Amnesties and Guilty Verdicts on PHYSINT



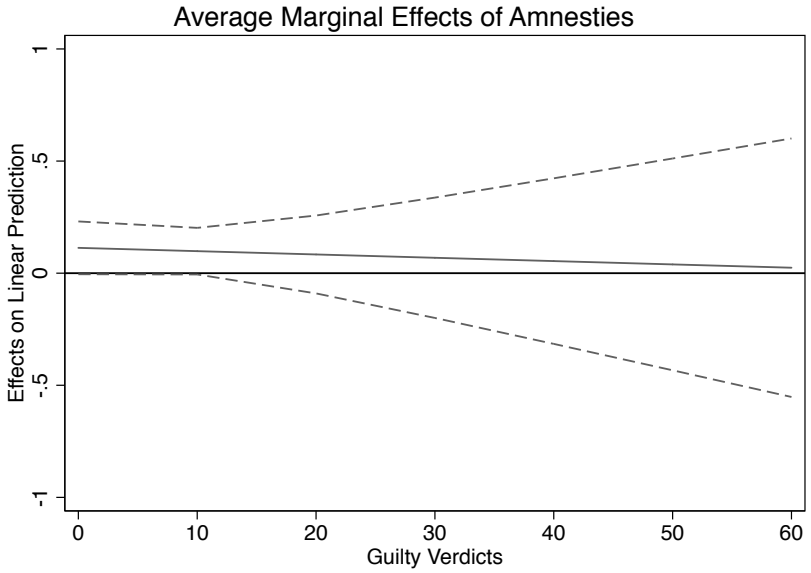
Note: Depicted are the average marginal effects of each term, conditional on its interacting variable, with 95% confidence intervals.

Figure 3: Effects of Model 10 Interaction Term between Amnesties and Prosecution on PHYSINT



Note: Depicted are the average marginal effects of each term, conditional on its interacting variable, with 95% confidence intervals.

Figure 4: Effects of Model 12 Interaction Term between Amnesties and Prosecution on PHYSINT



Note: Depicted are the average marginal effects of each term, conditional on its interacting variable, with 95% confidence intervals.

7 Omitted Variable Bias

Unobserved heterogeneity could bias our results, and explain away some of the findings. For example, if we omit a number of important explanatory variables that explain the presence of transitional justice mechanisms, as well as improved human rights outcomes, it would mean that we are seeing relationships where none exist.

In order to address this problem, we implement a post-estimation test developed by Emily Oster (2017). Oster's test reports a delta term that indications indicates what proportion of selection would have to be explained by unobservables, compared to the observables included in the model, for the treatment effect to be zero. In Model 4, which predicts the effect of prosecutions on PHYSINT, that delta term is 4.5, meaning that omitted variables would have to explain 450% more selection than the variables included in the model for the effect of Prosecutions to be zero. For Model 9, unobservables would have to account 300% more selection than the observed variables for the effect of Amnesties to be zero. In either case, it is unlikely that omitted variables account for this much variation.